Not Even Wrong, ten years later: a view from mathematics on prospects for fundamental physics without experiment

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Columbia University

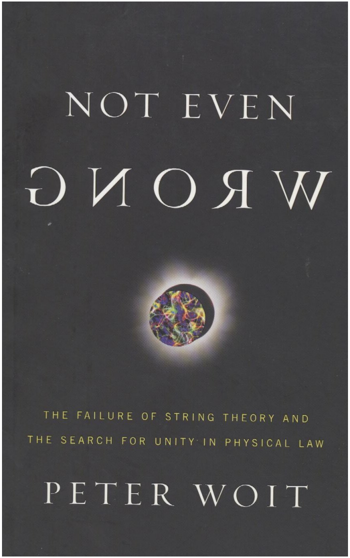
Rutgers Physics Colloquium, February 3, 2016

Outline

Some advertisements and some provocations:

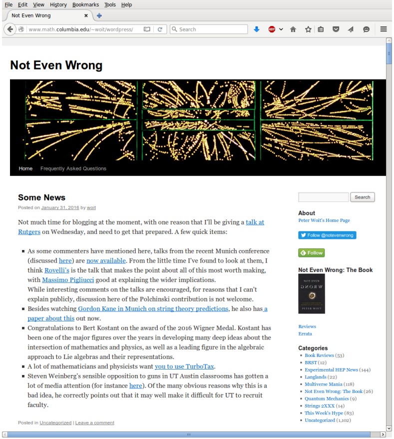
- Advertisements: an old book, an ongoing blog, and a forthcoming book.
- Review what has happened to the idea of string theory unification.
- Raise the issue of what happens absent new experimental guidance. Can the example of mathematics help?
- Evidence for deep connections between math and physics: quantum mechanics and representation theory
- Speculation about relevance of ideas from representation theory to better understanding the Standard Model.

Not Even Wrong: the book



Written largely in 2001-2
2003: under review at Cambridge University Press
Fall 2004: accepted for publication by British publisher (with help from Roger Penrose)
Mid-2006: published around same time as Lee Smolin's *The Trouble with Physics*. The "String Wars" kick off.

Not Even Wrong: the blog



Not Even Wrong

Home Frequently Asked Questions

Some News

Posted on January 31, 2016 by woit

Not much time for blogging at the moment, with one reason that I'll be giving a talk at Rutgers on Wednesday, and need to get that prepared. A few quick items:

- As some commenters have mentioned here, talks from the recent Munich conference (discussed here) are now available. From the little time I've found to look at them, I think Rovelli's is the talk that makes the point about all of this most worth making, with Massimo Pigliucci good at explaining the wider implications.
  While interesting comments on the talks are encouraged, for reasons that I can't explain publicly, discussion here of the Polchinski contribution is not welcome.
- Besides watching Gordon Kane in Munich on string theory predictions, he also has a paper about this out now.
- Congratulations to Bert Kostant on the award of the 2016 Wigner Medal. Kostant has been one of the major figures over the years in developing many deep ideas about the intersection of mathematics and physics, as well as a leading figure in the algebraic approach to Lie algebras and their representations.
- A lot of mathematicians and physicists want you to use TurboTax.
- Steven Weinberg's sensible opposition to guns in UT Austin classrooms has gotten a lot of media attention (for instance here). Of the many obvious reasons why this is a bad idea, he correctly points out that it may well make it difficult for UT to recruit faculty.

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- Book Reviews (53)
- BRST (12)
- Experimental HEP News (144)
- Langlands (22)
- Multiverse Mania (118)
- Not Even Wrong: The Book (26)
- Quantum Mechanics (9)
- Strings 2XXX (14)
- This Week's Hype (83)
- Uncategorized (1,102)

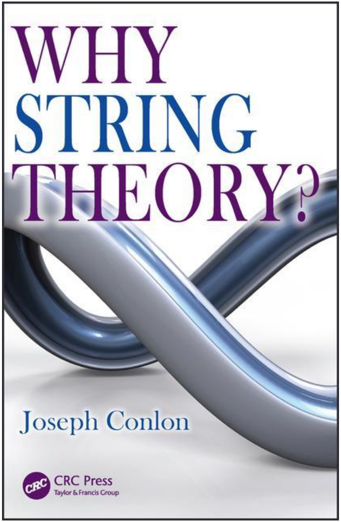
Started March 2004
In 2006, one of the battlefields of the string wars.

Currently

- 1512 postings
- 41,335 comments
- Around 20,000 page-views/day (mostly robots...)

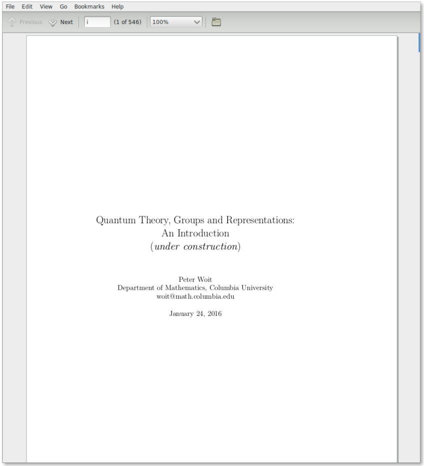
`http://www.math.columbia.edu/~woit/blog`

An advertisement for the competition



Finally in 2015, a book with the counter-argument I was expecting. Recommended if you want to hear a sensible opposite point of view.

A very different book



Quantum Theory, Groups and Representations:
An Introduction
(*under construction*)

Peter Woit
Department of Mathematics, Columbia University
woit@math.columbia.edu

January 24, 2016

About 540 pages, 90-95% complete
To be published by Springer, late 2016?
Based on a year-long course for advanced undergraduates and graduate students, taught 2012-3 and 2014-5.

`http://www.math.columbia.edu/~woit/QM/qmbook.pdf`

Physics departments

1975-79

B.A./M.A. in physics, Harvard
Very exciting time.
Summer job 1978 at SLAC, Crystal Ball experiment, SPEAR
Also took many mathematics classes including graduate courses

1979-84

Ph.D. in physics, Princeton
Thesis *Topological charge in lattice gauge theory* (Curt Callan)

1984-87

Postdoc, ITP Stony Brook

1987-88

Unpaid visitor, Harvard physics

Mathematics departments

1987-88

adjunct Calculus instructor, Tufts math department

1988-89

Postdoc, Mathematical Sciences Research Institute, Berkeley

1989-93

Assistant professor, Columbia math department (non-tenure track)

1993-current

various titles at Columbia. Teach one course/semester, manage computer system, online homework system.
Current title: Senior Lecturer (permanent position, 5-year renewable)

Some comments

String unification: the vision

The First Superstring Revolution

1984, Sept. 10 Anomaly cancellations, Green, Schwarz
1984, Sept. 28 *Some properties of O(32) superstrings* Witten
1985, Jan. 2 *Vacuum configurations for superstrings* Candelas, Horwitz, Strominger, Witten.

The vision

Use 10d superstring, nearly unique by anomaly cancellation.
Compactify 6 dimensions using a Calabi-Yau manifold.
Get effective supergravity theory in 4d at low energy, unified theory of SM + quantum gravity.

7 known families of Calabi-Yaus, each one parametrized by moduli spaces of various dimensions from 36 to 203.

The plan: pick family, find dynamics that fixes the moduli, get the SM.

String unification: the problem

- Collaboration with mathematicians: more and more families of Calabi-Yaus. Currently unknown if the number of families is finite.
- Better understanding of the theory: more and more possibilities for dealing with extra 6 dimensions (e.g. branes). More and more possible "string vacua".

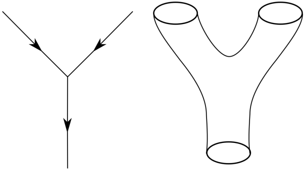
Research has steadily moved in the wrong direction, away from the vision

Better understanding the theory just keeps making the problem worse. More and more possible "approximate string vacua".

Fundamental problem

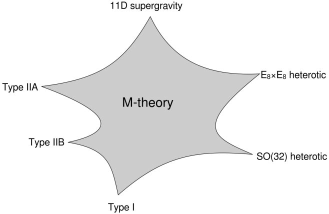
It appears that you can get just about any low energy physics you want, depending what you do with the extra dimensions. No predictions about observable physics.

String unification: the source of the problem



- String theory is a generalization of single-particle quantum theory, not QFT.
- Can get an analog of single-particle interactions from the geometry of the string. Can get an analog of a Feynman diagram expansion.
- Don't get the phenomena of QFT: non-trivial vacuum, non-perturbative behavior. Need a "non-perturbative string" or "string field" theory to get true, not approximate, "string vacua".

M-theory conjecture



1995: M-theory

Conjectural non-perturbative theory

1997: AdS/CFT

New ideas about non-perturbative string theory, but no help with the “too many string vacua” problem

Current situation: "string theory" is not a theory, but a conjecture there is a theory

Typical summary talk by David Gross, Strings 20XX. “The big open questions are: What is string theory? What are the underlying symmetries of string theory?”

Fallout from string unification failure: Hype

Science

The New York Times ON THE WEB

April 4, 2000

Physicists Finally Find a Way to Test Superstring Theory

Related Articles

- The New York Times on the Web: Science

Diagram

- A Far-Out Theory to Describe What's Out There

Forum

- Join a Discussion on Superstrings and Beyond

By GEORGE JOHNSON

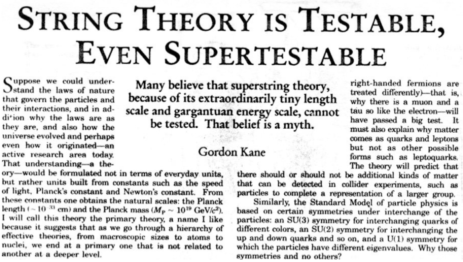
For a quarter of a century, superstring theory has promised that the universe could be understood more deeply than ever before, with all the forces unified into one, if it were seen in a startling new light -- as a kind of mathematical music played by an orchestra of

Keith Meyers/The New York Times

Dr. Lisa Randall speaking to Dr. Raman Sundrum, superstring theorists who portray the universe as one of many bubbles floating inside a four-dimensional megaverse.

Large amounts of hype

Documented on the blog. Typically, "test", "prediction" don't have the usual meaning. Implications for credibility of the subject.



STRING THEORY IS TESTABLE, EVEN SUPERTESTABLE

Many believe that superstring theory, because of its extraordinarily tiny length scale and gargantuan energy scale, cannot be tested. That belief is a myth.

Gordon Kane

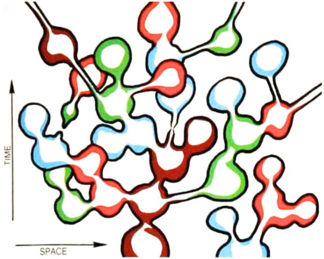
Suppose we could understand the laws of nature that govern the particles and their interactions, and in addition why the laws are as they are, and also how the universe evolved and perhaps even how it originated—an active research area today. That understanding—a theory—would be formulated not in terms of everyday units, but rather units built from constants such as the speed of light, Planck's constant and Newton's constant. From these constants one obtains the natural scales: the Planck length ($\sim 10^{-33}$ cm) and the Planck mass ($M_P \sim 10^{19}$ GeV/c^2). I will call this theory the primary theory, a name I like because it suggests that as we go through a hierarchy of effective theories, from macroscopic sizes to atoms to nuclei, we end at a primary one that is not related to another at a deeper level.

right-handed fermions are treated differently)—that is, why there is a muon and a tau so like the electron—will have passed a big test. It must also explain why matter comes as quarks and leptons but not as other possible forms such as leptoquarks. The theory will predict that there should or should not be additional kinds of matter that can be detected in collider experiments, such as particles to complete a representation of a larger group.

Similarly, the Standard Model of particle physics is based on certain symmetries under interchange of the particles: an SU(3) symmetry for interchanging quarks of different colors, an SU(2) symmetry for interchanging the up and down quarks and so on, and a U(1) symmetry for which the particles have different eigenvalues. Why those symmetries and no others?

String theory "predictions" of superpartner masses: 250 Gev (1997), 1.5 ± .2 TeV (arXiv:1601.07511)

Fallout from string unification failure: the Multiverse



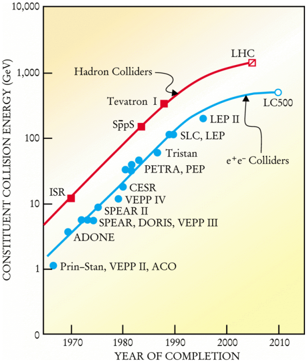
Where string theory unification vision has ended up

- Conjectural features of string theory imply an exponentially large number of consistent “string vacua”
- Can get any low energy physics by choice of string vacuum
- Only "anthropic" predictions possible

A real and present danger

This isn’t science.
String theory enters the textbooks, multiverse explains why it can’t be tested.
Students taught that we can’t ever do better than this, not worth trying.

The end of (a certain kind of) physics?



Next e^+e^- machine: 2 x LEPII, 2030s?

Next pp machine: 7 x LHC, 2040s?

Dangers

Impassable technological barrier to further experimental progress.

Sociological/psychological: people don't like to give up on their visions.

Is post-empirical physics possible?

December 2015, Munich conference *Why Trust a Theory? Reconsidering Scientific Methodology in Light of Modern Physics?*

Do we really need to change the philosophy of science? Susskind: the problem is the "Popperazi".

Mathematics: a non-empirical science

There is one science that does not rely on empirical testing to make progress: mathematics.

Mathematics suffers from some of the same inherent difficulties as theoretical physics: great successes during the 20th century were based on the discovery of sophisticated and powerful new theoretical frameworks, hard and time-consuming to master. Increasingly difficult to do better, as the easier problems get solved (see John Horgan's "End of Science" argument).

Highly abstract mathematics is in a very healthy state, with recent solutions of long-standing problems:

1994: Fermat's Last Theorem (Taylor-Wiles)

2003: Poincaré Conjecture (Perelman)

Mathematics: some methodological lessons

In my experience, the cultures of mathematics and physics have significant differences. Some things valued highly in mathematics, less so in physics:

- Always be extremely clear about precise assumptions
- Always pay close attention to the logic of an argument: at each step, does the conclusion really follow?
- Where precisely is the boundary between what is understood and what isn't?

Physics has never really needed to pay close attention to these issues. Experiment could be relied upon to sooner or later sort things out. Paying close attention to them carries a big cost, danger of getting lost in technicalities. Best mathematics avoids this, less good mathematics doesn't.

Mathematics and physics: historical lessons

History

Historically, deep new ideas about mathematics and physics have turned out to be closely related

Mathematics	**Physics**
Riemannian geometry (1867 -)	General relativity (1915 -)
Lie group representations (1925 -)	Quantum mechanics (1925 -)
Index theorem (1960 -)	Dirac equation (1928 -)
Ehresmann connections (1950 -)	Yang-Mills theory (1954 -)

More recent examples

Topological quantum field theories

For history told from this point of view, see *Not Even Wrong*

A working hypothesis: radical Platonism

Deep new ideas about mathematics will continue to be fruitful in this kind of physics.
More conventionally: will continue to find new ways of exploiting symmetries, using new mathematics.

Warnings

- Many (most?) strongly disagree, seeing deep physical ideas as very different than deep mathematical ideas. Lee Smolin: ”mysticism”
- Without help from experiment, very easy to get lost in unfruitful mathematical directions.

Platonism

Mathematical objects exist (in some sense...)

Radical Platonism

Basic mathematical objects exist, are congruent with basic physics objects

A different vision

Conventional vision

Some very different physics (strings?) occurs at Planck scale.
Standard model is just an effective theory at large distances.

A different vision: is the SM close to a fundamental theory?

The lesson of experiment 1973 - today: extremely difficult to find a flaw in the Standard Model.
Maybe the Standard Model is not just a low energy approximation, but includes elements of a truly fundamental theory.
Some evidence: asymptotically free theories make perfectly good sense at arbitrarily short distances.
Note: this vision is simultaneously very radical and very conservative

But then how can one hope to make progress without experimental guidance?

Implications I

One should pay close attention to what we don't understand precisely about the SM, even if the standard prejudice is "that's a hard technical problem, and solving it won't change anything".
Some examples:

- QCD: can one do better than Monte-Carlo lattice simulations?
- Non-perturbative electroweak theory
- Non-perturbative treatment of gauge symmetry (BRST)

Implications II

Maybe quantum gravity is not so radically different than the SM. Similar geometric variables: spinors, connections and curvature.

Indirect test of quantum gravity?

Absent experimental input on quantization of space-time degrees of freedom, can one somehow unify, treating space-time degrees of freedom on same footing as SM degrees of freedom? Can one find a unified picture that convincingly explains something new about the SM degrees of freedom?

Note: multiverse advocates claim indirect tests are possible, just test string theory vacua with eternal inflation. True, but string theory is untestable due to multiverse. Circularity.

Implications III

One should try to better understand deepest links between SM and mathematics. Rest of the talk will cover

Quantum mechanics and representation theory

This is well understood (although not necessarily well-known). Topic of book-in-progress.
Related to fundamental ideas about number theory.

The Standard Model and representation theory

Some ideas from representation theory

- Dirac cohomology.
- Geometric representation theory and categorification.

Quantum mechanics

Two Basic Axioms of Quantum Mechanics

The states of a quantum system are given by vectors $\psi \in \mathcal{H}$ where $\mathcal{H}$ is a complex vector space with a Hermitian inner product.

Observables correspond to self-adjoint linear operators on $\mathcal{H}$.

Main examples generate symmetries

- Momentum $\mathbf{P}$: translations
- Angular momentum $\mathbf{L}$: rotations
- Charge Q: phase transformations

One example that doesn't: Position $\mathbf{X}$

The mysterious part: how does classical behavior emerge?

Nothing to say about this

Where do these axioms come from? I

A unitary representation of a Lie group G gives exactly these mathematical structures:

- A complex vector space $V = \mathcal{H}$, the representation space.
- For each element of the Lie algebra of G (the tangent space at the identity of the group), one gets a linear operator on H (for mathematicians, skew-adjoint. Multiply by i to get physicist's self-adjoint operator).

Where do these axioms come from? II

Taking as Lie algebra the functions on phase space (the Poisson bracket makes these a Lie algebra), associating operators to functions by

$$f \to -iO_f$$

is a representation exactly when you satisfy Dirac's relation (setting $\hbar = 1$)

$$[-iO_f, -iO_g] = -iO_{\{f,g\}}$$

These are not all "symmetries"

One get a representation like this, even though all operators don't commute with the Hamiltonian. For example, just looking at functions $f = x, g = p, 1$, get "Heisenberg Lie algebra" and operators satisfying

$$[X, P] = i\mathbf{1}$$

But, never have $[X, H] = 0$ for Hamiltonian operator.

Group representations are not just symmetries

A group and its representation theory govern the basic structure of quantum mechanics, not just symmetries of a Hamiltonian

Much, much more detail in current book project

Relations to number theory

The representation of the Heisenberg commutation relations is sometimes known to mathematicians as the "Segal-Shale-Weil" representation.
Weil (1964): used this representation not for the field $\mathbf{R}$, but for number fields like the rationals $\mathbf{Q}$, and their local versions at each prime p, the p-adic numbers $\mathbf{Q}_p$.
Part of the story of modern number theory. The Langlands program and "automorphic representations". A kind of geometry, with prime numbers playing the role of points.
"Geometric Langlands", an analog with points the points on a surface, turns out to be related to 4d QFT, based on gauge theory.

Evidence for radical Platonism...

Structure of the Standard Model

Path integral expression of the problem. Want to compute, for certain functionals F

$$\int_{\mathcal{A}} \int e^{\int (-|F_A|^2 + \overline{\psi} D_A \psi) d^4x} F(\psi, A)[d\psi][dA]$$

Basic elements of the problem:

- Fermionic quantization of the space of solutions of a Dirac equation in a fixed background connection A
- Integrate over $\mathcal{A}$, the space of connections
- Deal with gauge symmetry group $\mathcal{G}$, really want to integrate over $\mathcal{A}/\mathcal{G}$.

What follows is rank speculation

Dirac Cohomology

A major new idea in representation theory, popular in recent years, is that of "Dirac cohomology".

Basic idea: irreducible representations of Lie algebras can be found by looking for solutions of the eigenvalue equation

$$C\psi = \lambda\psi$$

where C is a Casimir operator. Follow Dirac, introduce a Clifford algebra (γ-matrices) and spinors, get a square-root of C. This will be an algebraic analog of a Dirac operator. Then characterize representations by looking a solutions of a Dirac-like equation.

A reference: Huang and Pandzic (2006) *Dirac operators in representation theory*

Could the Dirac equation and spinors of the SM somehow play this sort of role in a new representation theory story?

Geometric representation theory and categorification

A major theme in mathematics in recent years is that one should construct representations not on a vector space, but on a "category" of geometric nature (set of geometric objects, with morphisms between them). The vector space representation is then derived from this.

One theme of this subject: construction of representations involve the "classifying space" BG of the group G. Intriguingly, $\mathcal{A}/\mathcal{G}$ is the classifying space $B\mathcal{G}$ of the gauge group $\mathcal{G}$.

Does the QFT operation of integrating over $\mathcal{A}/\mathcal{G}$ have an interpretation in terms of the representation theory of $\mathcal{G}$? Very little is understood mathematically about how to think about representations of this kind of infinite dimensional group.

Summary

Some intentionally provocative claims:

- Ten years after the “string wars”, the case that the vision of a unified theory based on string theory has failed is even stronger. The use of untestable multiverse scenarios to justify this failure is a significant danger to physics as a science.
- If technological barriers stop future experimentally-driven progress, physicists might want to look to mathematics for some guidance, both methodological and substantive.
- The concept of a representation of a group is both a unifying theme in mathematics, and at the basis of the axioms of quantum mechanics.
- The Standard Model may be closer to a true unified theory than people expect, but with new ideas about its underlying structure needed. Inspiration for such new ideas might be found in modern mathematics, in particular in advances in representation theory.

Thanks for your attention!