String Theory and the Crisis in Particle Physics

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Peter Woit
Columbia University

The twentieth century saw an historically unprecedented expansion of our understanding of the physical world at its most fundamental level, culminating in an extremely successful theory of elementary particles and their interactions, now known as the Standard Model. This theory was in place by the mid-1970s, and since then particle physics has become in some ways a victim of its own success, struggling to find a way to make further progress. For nearly a quarter century now, the field of particle theory has been dominated by the pursuit of one speculative idea known as "string theory", despite the continuing failure of this idea to lead to any testable predictions. In recent years many string theorists have begun to promote the idea that this lack of conventional scientific predictivity is a problem arising not from the theory, but from the nature of the universe itself.

In his introduction to this conference, George Steiner raises the question of whether this situation "entails an ontological crisis in the very concept of what constitutes a science". I will argue that this is not the case, that what has happened here is no more than a conventional example of a failure of a speculative research program. What is unusual about this failure is its occurrence in a new research environment that physicists are not used to, one in which the previous mechanisms that kept speculative excess in check have weakened or disappeared. The period of exponential increases in experimentally accessible energy scales that characterized the middle part of the twentieth century is now long past. The challenge for theoretical particle physics in this century will be whether it can find ways to continue to make progress, while facing new and much more difficult constraints.

Experimental Constraints

From the 1930s on, the rapid development of better technologies for accelerating and colliding particles has been the force driving progress in our understanding of the nature of fundamental particles and their interactions. Higher energies correspond to smaller distances, so each new generation of accelerators has allowed investigation of physics at a smaller distance scale. Just like Moore's law for semiconductors, which continues to be responsible for rapid technological change, for much of the twentieth century accessible energies increased exponentially. From 1 MeV (million electron Volts) in 1931, there was steady progress until 1987, when the so-called Tevatron at Fermilab began operating at nearly 1 TeV (trillion electron Volts). For the past twenty years this has come to a stop, with the Tevatron remaining the highest energy accelerator in the world. An ambitious plan to build yet a higher-energy accelerator in the US, the SSC, ran aground when the federal government canceled the project in 1993 as the likely cost of the machine grew to more than $10 billion.

For the forseeable future, hopes rest almost entirely on the LHC (Large Hadron Collider), which is nearing completion at CERN in Geneva and should begin accelerating particles sometime next year. The LHC uses superconducting magnets to store a beam in a ring of about 27 km in circumference, achieving in this way energies seven times higher than at the Tevatron. So, if all goes well, instead of the better than order of magnitude energy increases common in each earlier decade, it will have required 21 years to get a factor of seven. The future looks yet more difficult since accelerator energies scale only linearly in the size of the machine and strength of the magnets. It appears unlikely that any government will be willing to finance the construction of a device much larger than the LHC during the next few decades, and the LHC magnets already push the limits of current technology. There are no higher energy post-LHC accelerators even in the design stage at the current time. The most likely possibility may be the "DLHC", a possible upgrade of the LHC that would double its energy, by using new magnets of double the field strength. Such magnets capable of being mass produced and surviving in a high-radiation environment have yet to be designed, so the DLHC is likely to be at least two decades away.

As accelerator energy scale increases have dropped from a factor of 100-1000 each 20 years to a factor of seven during the past two decades, and maybe a factor of two over the next twenty years, the engine that has been driving progress has slowed down dramatically. The very near term future is bright, as the next few years will see the LHC finally start doing physics, but the past two decades have been difficult ones, and the post-LHC period will face the same problems.

The Power and the Glory of the Standard Model

At the same time that particle physics has begun to run up against limits of a technological nature, it has also run into a problem discussed by John Horgan in his 1996 book *The End of Science*. A highly successful scientific advance may open up new lines of inquiry, but it may also solve many of the outstanding problems in a field, leaving only the ones that are much more intractable. The story of the Standard Model may be the best example of this phenomenon.

The Standard Model is an example of a quantum theory involving an infinite number of degrees of freedom, called a quantum field theory. It is a very special sort of quantum field theory, possessing an infinite dimensional symmetry called gauge symmetry. Such quantum gauge field theories are formulated in terms of geometrical quantities that are at the center of the modern understanding of geometry: these go under the names of "connections" (called "gauge fields" by physicists), "spinors", and the Dirac operator. While quantum gauge field theories involve some concepts well-understood by mathematicians, they go well beyond these, requiring new mathematical ideas that are still incompletely understood. To this day these theories have resisted attempts to find a completely successful and rigorously well understood formulation. Their study, especially in the hands of physicist Edward Witten, has led to striking new ideas about pure mathematics which have revolutionized some subfields of the subject.

While calculations in these theories are difficult, by 1971 ways were found to consistently do some of them, in work by Gerard 't Hooft and his thesis advisor Martin Veltman. Two years later these calculational methods were applied by David Gross, David Politzer and Frank Wilczek to show that a quantum gauge field theory could successfully explain the strong interactions between quarks that bind them into protons, neutrons and the other particles studied by experimenters. 't Hooft and Veltman were awarded the Nobel Prize in 1999, Gross, Politzer and Wilczek in 2004. By 1973 all of the ideas of the Standard Model were in place and people soon began to use that term to refer to it. Since 1973 there has been no theoretical development in particle theory that has been deemed worthy of a Nobel prize. With the success of the Standard Model, the field of particle theory became very much a victim of its own success.

Several generations of particle accelerators have gone into operation in the years after 1973, with the remarkable result that all experimental results found by them agree closely with the Standard Model. More precisely, whenever anything observable by experiments can be reliably computed to some accuracy within the Standard Model and thus a prediction can be made, the experimental results have turned out to coincide with the prediction to the expected accuracy. Some calculations remain difficult or impossible to do, but whenever anything can be calculated, it agrees with experiment. This utter lack of disagreement between theory and observation has made the lives of experimenters more boring, and the lives of theorists rather miserable.

The Standard Model as it exists today is different in only one respect from the original version of 1973. Since that time evidence has been found for neutrino masses, so the original model has been supplemented by terms for neutrino masses, but in all other respects the model remains unchanged. Also unchanged are the limitations of the model: it does not answer certain questions that we believe a fully successful theory should provide answers to.

One of the main questions left unanswered is how to incorporate gravity into the Standard Model, or into any quantum-theoretical framework. Standard techniques for extracting sensible calculational results seem to fail when we try and apply them to theories involving gravity (although recently evidence has appeared indicating that in certain special cases this may not actually be true). One can estimate the size of quantum effects due to gravity, and unfortunately these will be effectively unobservable, of sizes so small that no conceivable experimental set-up would be able to measure them. The problem of quantum gravity is thus one of principle, effectively decoupled from experimental accessibility.

The other main limitation of the Standard Model has to do with the way it handles a crucial part of the theory often called the "Higgs phenomenon." For the model to work correctly, the vacuum state must not be invariant under the gauge symmetry, and this is arranged by introducing a conjectural field called the "Higgs field". Doing this introduces twenty-some undetermined parameters into the model, and we would like to understand what governs the values of these parameters. Of these parameters all but one has already been fixed (often to high accuracy) by experiment. The one unknown parameter determines the mass of a still-unobserved particle called the "Higgs particle". For consistency, this mass cannot be too large, so if the Standard Model is correct the LHC should be able to observe this particle and fix the last parameter in the model. If the Standard Model is not correct, there must be some now-unknown physics going on, for which the Higgs field of the Standard Model provides an approximate description valid at energies low enough to have been studied to date. The best reason for optimism that the LHC will see something unexpected is that it is the first accelerator of high enough energy that has to either produce Higgs particles, or show deviations from the Standard Model if the Higgs does not exist.

String Theory: Hopes and Disappointments

By 1984, physicists had spent the previous decade looking without much success for a new theory that would transcend the limitations of the Standard Model. A very small number worked not on quantum field theory, but on something called "string theory". In string theory, the role of point-like elementary particles is replaced by one-dimensional elementary objects, the "strings". During the summer of 1984, Michael Green and John Schwarz were able to show that one possible source of inconsistency (known as an "anomaly") canceled out in two specific string theories. This encouraged Witten to start working in this area, and many others soon joined him. He and others quickly produced examples of string theories that they hoped would both contain gravity and reproduce the Standard Model at low energies, finally providing a truly unified theory, a so-called "Theory of Everything".

String theories are quantum theories, and at low energies have a massless, spin-two particle that can play the role of the graviton and provide a gravitational force. Consistency of the string theory requires that it have a property called "supersymmetry", and that the strings move in a space-time of 10 dimensions. To connect string theory to the observed world, one has to find some way to make 6 of these dimensions invisible, and one has to account for the fact that observed physics does not have the property of supersymmetry. From 1984 to the present day, efforts to find a way around these problems have dominated efforts to find a way to use string theory to unify physics.

Witten and others were highly enthusiastic about this at the beginning, as they quickly identified a class of six-dimensional spaces called "Calabi-Yaus", which were consistent possible ways of wrapping up six of the dimensions and making them unobservably small. Observed physics in our four-dimensional space-time would depend on which Calabi-Yau one chose, but mathematicians only knew a limited number of these spaces. It appeared that all one had to do was to identify all the Calabi-Yaus, and check each of them to see if they led to Standard Model physics at low energies. Large numbers of theorists joined the chase, working on both the problem of getting the Standard Model, and the more abstract problem of better understanding string theory.

As time went on, it became apparent that the main problem was that not only were there a large number of Calabi-Yaus, but each Calabi-Yau came with a set of "moduli" parameters needed to specify its size and shape. One needed to find not just the right Calabi-Yau, but also some mechanism for fixing these parameters. The hope was that better understanding of string theory would identify some dynamical effect that would "stabilize the moduli", but the only known examples of such effects tended to cause moduli to run off to infinite or zero size, not allowing a stable, appropriately small Calabi-Yau.

"String theory" does not actually refer to a well-defined theory, rather to a set of approximate rules for calculating what happens with quantum strings interacting weakly with each other. The rules are only fully consistent in the limit of zero interaction between the strings, but in this limit one cannot get non-trivial physics. For weakly interacting strings, the hope is that the approximation is rather good, but for larger interaction strength not only will the approximation become inaccurate, but one doesn't even know what theory it is one is supposed to be approximating. String theory can be described as not really a theory, but a hope that a theory exists which in the weakly interacting approximation is described by rules that are understood.

The mid-nineties saw some progress towards going beyond the weakly coupled approximation. Witten conjectured the existence of a fully consistent formulation of string theory, to be called "M-theory", which would in various limiting cases give the known versions of string theory. This conjecture involved introducing new degrees of freedom into the theory, called "branes" (as in "membranes"). A brane is a higher dimensional object whose effects become important as strings become more strongly interacting, and can act as a location where strings begin and end. While M-theory in principle unified five different kinds of string theories, it did not solve the problem of how to find the right Calabi-Yau, but actually made it much worse. Now, it was no longer just a matter of finding the right small six-dimensional space, since at low energies there appeared to also be a huge number of possible consistent configurations for branes of various dimensions. This opened up all sorts of new possibilities in addition to the many ways of wrapping up some dimensions: one could try assuming that our universe was a brane, was the intersection of various branes, etc.

The latest period of string theory research began a few years ago with the discovery of possible methods for stabilizing the moduli parameters. While these methods promised a resolution of the moduli stabilization problem, this was very much a Pyrrhic victory. The stabilization of moduli required putting together quite complicated configurations of Calabi-Yaus, branes, and generalized magnetic fluxes. Even proponents referred to the constructions as "Rube Goldberg machines", after a series of well-known American cartoons of absurdly complicated mechanisms designed to do something simple. Most disastrously, each construction ended up not with one possibility, but with an unimaginably large number such as 10^{500} of them. Since 1984, each step towards better understanding string theory and finding a way to reproduce the Standard Model had led to greater complexity and more possibilities. While for many years researchers were trying to characterize the kind of physics string theory could lead to, now there are so many possibilities that the focus of some string theory research is to try and identify possibilities that string theory can't reproduce.

What has happened here reflects one of the two endpoints that can occur when one pursues a wrong speculative theoretical framework. One possible outcome is that the framework is quite rigid, and one sooner or later hits a definitive inconsistency or contradiction with experiment. The other possibility is that the framework is much more flexible, so one can keep adding features to it that allow one to evade contradiction with experiment. Sooner or later though, it becomes apparent that such a framework, while not inconsistent, is useless. One finds that one has to put in as much complexity as one is getting out, so no testable predictions can be made. Our successful physical theories have a well-defined property of beauty and elegance: a huge amount of information about the world flows from a small number of assumptions. Unsuccessful theories produce no more information than what one puts into them. This form of failure is what has afflicted string theory.

Remarkably, the last few years have seen some string theorists decide not to abandon the theory in the face of this failure, but instead to turn the problems of the theory into an excuse for not being able to overcome the limitations of the Standard Model. In this new, heavily promoted point of view, the vast number of possibilities inherent in string theory is called the "Landscape", and the assumption is made that our universe is but one infinitesimal piece of a much larger "multiverse" realizing other possibilities. The values of some of the Standard Model parameters are "explained" by the "Anthropic Principle", that they must have more or less the observed values for intelligent life to exist. The promotion of this kind of explanation as a new way of doing science, inescapable because it is what string theory leads to, has led to a fierce controversy among physicists. Any one following this controversy would do well to remember what many of those involved in it often seem to forget. There is a simple, scientifically conventional explanation for how to deal with what has happened: string theory appears to have failed as a unified theory, and one has to try harder and look for something else.

It is important to recognize that this failure is not due to the limits of our experimental techniques. String theory as currently understood not only makes no predictions about physics at currently accessible energies, it makes no predictions about what will happen at LHC energies, or for that matter at any energy, no matter how high. Attempts to use string theory to make predictions about cosmology, using aspects of string theory that could be relevant to the energy scale of the Big Bang, run into exactly the same problems that keep one from making predictions about particle physics at much lower energies.

On Difficulty

The story of the development of string theory is a remarkable one, without any real parallel in the previous history of speculative ideas about physics. By now tens of thousands of papers have been written on the subject, and any student entering the field faces the daunting task of spending many years trying to master even a portion of this literature. Much of the work on string theory uses quite sophisticated mathematics, of a sort that even most mathematicians are unfamiliar with. For anyone who has not spent years learning about the issues involved, the task of evaluating whether progress has been made towards resolving the central difficulties is a daunting one.

Quantum field theory is already in many ways the most difficult subject in conventional theoretical physics. It also by now boasts a huge literature, and many questions remain open about even the simplest physically relevant examples of these theories. A wide range of highly sophisticated mathematical techniques have been used to study one aspect or more of the behavior of quantum fields. String theory in some ways is yet much more difficult, as the foundations of the subject are still unknown, but the subject of a vast array of conjectures based on a wide variety of different kinds of calculations. The constructions used to try and make contact with the Standard Model have become more and more complex as time goes on, adding yet another source of difficulty.

The study of either quantum field theory or string theory comes up continually against the limits of the human intellect. Some understanding of quantum field theory leads to an appreciation for just how powerful its conclusively tested insights into nature are. They may initially be difficult to assimilate, but ultimately one finds that the fundamental concepts of the subject are, properly understood, simple if not at all intuitively obvious. This sort of appreciation leads inherently to a sympathy for a certain form of elitism: this is not a subject truly accessible to large numbers of people, especially not to ones who don't have the time and energy to devote to long and serious study. My critique of string theory over the past few years has made me many fans who are convinced that sophisticated mathematics is not needed to understand fundamental physics correctly, and that this is why string theory must be wrong.   They're often unhappy to hear that I strongly disagree with them, that I think particle physics probably needs even more mathematics of a high level of abstraction and intellectual difficulty.

The impasse physics has reached in recent years raises the very real question of whether human beings have finally come up against the limits of their capacity for understanding the universe. Just as a dog cannot be trained to understand Einstein's theory of general relativity, perhaps we have as little hope as the dog of understanding the theory that explains those questions left unanswered by the Standard Model.

A Point of View From a Different Science: Mathematics

My own belief is that physicists still have a ways to go before running into the necessity of working with theories they are not capable of understanding, and it is based on my experience in a related science, that of mathematics. The intellectual difficulty arising from the use of high levels of abstraction is significantly greater in some subfields of mathematics than in any area of string theory. Huge progress based on deep insights into the fundamentals of the subject also characterized mathematics during the last century, often leaving the most difficult problems outstanding. During the past two decades mathematicians have continued to make very significant progress on such problems. In 1993 Andrew Wiles announced his proof of Fermat's Last Theorem, solving a problem that had remained intractable for more than three centuries. His proof used a wide variety of sophisticated modern methods, while developing some new ones. Just five years ago, Grigori Perelman posted on the arXiv preprint server a set of papers outlining a proof of the Poincaré Conjecture, the most famous unsolved problem in topology, one that dated back more than a century.   Both Wiles and Perelman worked intensively by themselves for seven years or more to come up with the advances needed in their work.   While some aspects of mathematics become ever more difficult as easier problems get solved and the harder ones remain, mathematicians continue to this day to come up with new ideas and to push forward their understanding into areas that centuries of previous work had left inaccessible.

The main thing that distinguishes mathematics from physics and other sciences is the different role of experiment. Mathematicians don't look to the real world to test their theories, instead they rely upon rigorously clear argumentation and logic. The Wiles proof neither can have nor needs an experimental test. While sometimes mathematicians draw inspiration from physics or patterns observed computationally, more often what drives mathematical progress is the internal logic of the subject. If one can understand a mathematical formalism well enough, at a deep enough level, one can see what it can do and what it can't do. From this one sometimes see ways to modify it or invent new ideas that will be capable of solving problems that the old formalism could not deal with. This kind of research requires one to pay close attention to the issue of exactly what it is that one understands, and what ideas still have not been thoroughly thought through.

Physicists have never had a much of a need for this sort of rigorous clarity of thought, since they have always had experiment to ultimately sort out what works and what doesn't.   String theory has been a subject pursued in the way that has always worked for theoretical physicists in the past: don't worry too much if things aren't quite making sense, just keep pushing on building conjecture on conjecture, trying to get to the point where a concrete and tested connection to experiment can be built, assuring one of solid ground to stand on. As particle theory enters a new environment in which experimental results are much harder to come by, it may need to learn from mathematicians how to work in a much more painstaking way, taking care to see what can be built solidly without reliance on validation by experiment.

A lesson can also be learned from Wiles and Perelman, who showed what kind of long-term effort is generally needed to make a fundamental advance against a difficult problem. The current organization of research in physics puts the best young people in a position of needing to quickly prove themselves, to produce results on time-scales of a year or two at most if they want to remain employed. At later stages of their career, even with tenure, the pressure of grant applications continues to discourage many from making the kind of commitment to an unpopular speculative research program that may be needed to make progress.

There is no necessary reason for the world to be this way, but experience has shown that fundamental ideas about mathematics and physics are deeply connected and intertwined. Lacking inspiration from experiment, physicists may find that searching for it in mathematics is one of the few avenues left open. Einstein's development of the theory of general relativity is one example where this sort of effort paid off, as his intensive involvement with

Riemannian geometry showed him the way to the correct field equations. At the same time, his failure later in life to get anywhere with geometrically inspired attempts at unification of gravity and electrodynamics shows that any particular effort along these lines may very well be misguided and not work out.

Closing Thoughts

It may be that relatively soon, perhaps in 2010, unexpected results from the LHC will appear and show particle physicists the way forward to overcome the limitations of the Standard Model and find new insights into the fundamental laws of the physical universe. If so, particle physics can happily revert, at least for a while, to making progress in the ways it has found fruitful in the past. But it is also all too possible that the LHC will teach us nothing more than the value of the remaining one unknown Standard Model parameter, by discovering just a Higgs particle with the expected properties.   One could argue that at this point in history, it is best to do nothing and just wait and see. It seems to me that a better use of the time would be to spend it facing up to the problems and failures that have plagued the subject for the last few decades, leading to a better preparation for absorbing whatever lessons there will be to learn in the coming years.

The last few years have seen the rise of something much worse for particle physics than a period of stasis. Instead of acknowledging the failure of string theory-based unification, prominent theorists have begun to heavily promote an excuse for this failure, arguing that fundamental physics needs to adopt a new paradigm that gives up on making conventional scientific predictions. It is certainly logically possible that our universe is a small part of a multiverse, and that some aspects of our fundamental physical laws are not universal, but contingent characteristics of our local environment. For such a conjecture to deserve to be called scientific, it must come with the standard sort of scientific evidence, or a plausible idea about how one might someday find such evidence. The proponents of the multiverse have provided neither, making their proposed new paradigm nothing less than an attack on science itself. Humanity may sooner or later reach the limits of what it can understand about the universe, but there is no evidence that we are there yet. Currently fashionable claims to the contrary reflect only a very different sort of human limitation, that of being unwilling to face up to the failure of a cherished idea.