Not Even Wrong:
The Failure of String Theory and the Continuing Challenge to Unify the Laws of Physics
Peter Woit
Jonathan Cape, London 2006.

Review by Aaron Bergman

I.

String theory, the enormously ambitious and speculative endeavor that has, for the past thirty years, attempted to unify our understanding of quantum mechanics and gravity has failed to live up to its initial promise. Its relative domination of the field of fundamental theoretical physics has long led to criticism within the scientific community. In the last few years, however, a number of popular books and television shows have made the case for string theory to the public. There is certainly a place, then, for these criticisms to also be presented to the public. More so, the sociology of modern theoretical physics could provide a fascinating context in which to present a reasonably disinterested discussion of the pros and cons of both string theory as a research program and the way in which modern theoretical physics is pursued. Dr. Woit has instead chosen to write a tendentious account providing little guidance as to why, even in the face of such criticism, so many have chosen to work on string theory. After reading this book and some of the unfortunate innuendo it contains, one might conclude not that string theorists are honest researchers doing the best they can to understand the nature of the universe, but rather are misguided devotees of a failed cult mired in self-delusion.

It would be difficult to entertainingly sustain such a polemic for the full length of a book, so the first half of *Not Even Wrong* is devoted to a rather concise history of modern physics. I will make no attempt to review this section as I am past the point of being able to judge how such a presentation will come across to the general public. A fuller accounting of the material can be found in other references many of which Dr. Woit helpfully cites. The main distinguishing feature here is more of a focus on how mathematics enters the story with a particular emphasis on the work of Hermann Weyl. I am not an expert on this history, so I will leave the question of its accuracy for others. I had a few problems[1] with the science as presented in this half of the book, but it is generally entertaining. A unique contribution as best I can tell, is chapter 10, “New Insights in Quantum Field Theory and Mathematics”. This chapter, on a subject close to my heart, is both an introduction to the tremendously fruitful interaction of physics and mathematics that has occurred in the last twenty years and a paean to one Edward Witten.

Dr. Witten, now at the Institute for Advanced Study in Princeton, NJ, is widely thought of as the most intelligent theoretical physicist of the latter part of the twentieth century. His contributions to the modern understanding of string theory, field theory and a fair chunk of mathematics are almost mind boggling. Born earlier, it is hard to believe that he would not be part of the pantheon that created our current understanding of reality. If string theory turns out to be on the right path, he will be remembered as possibly its most significant practitioner, and if it turns out to be wrong, his place in history is still assured by his other work in both physics and mathematics. I can only hope that the beauty of much of the material presented in this chapter and the contributions by Dr. Witten will be able to be appreciated by the lay-reader.

In the interest of full disclosure, I should mention that Dr. Woit has a ‘blog’ on the internet entitled “Not Even Wrong”. As he describes in his book, it is largely devoted to his critique of string theory. I have frequently posted material in the comments section of his blog, and much of the discussion here has also been discussed there.

---

[1] For the technically minded, I found the presentation of anomalies to be somewhat misleading, and there is too much a focus on the removal of infinities, the significance of which has changed in our modern understanding of field theory.

The remainder of this review will be devoted to those polemical parts of *Not Even Wrong*. I will assume that the reader believes that, as speculative and abstract as it may come across, the pursuit of fundamental theoretical physics is worthwhile. Dr. Woit clearly agrees that this is a valuable project, even if we disagree to various extents on the methods and direction in which it is being pursued. I hope to answer many of scientific critiques he proffers, and I will spend some time on those aspects of the book I found distasteful. More importantly, I will try in the final section to describe why those of us who choose to work on string theory continue on despite the criticism offered by Dr. Woit and others before him. It is a strange pastime we are engaged in as researchers, a product of our hopes and dreams, but it is an activity wrought with beauty and some small measure of progress which I hope to convey.

II.

It is worthwhile at this point to provide a brief overview of what string theory is. As alluded to above, the biggest issue bedeviling fundamental theoretical physics is the incompatibility of quantum mechanics and gravity. Such a unification is called a theory of quantum gravity. The problem, however, is that this incompatibility has proven to be almost completely impenetrable to experiment. This is fairly unique in the history of physics. In this field, there have been almost no unexpected experimental results coming for three decades. Particularly in the realm of particle physics, the 'standard model', our current quantum mechanical theory of subatomic particles and the forces that act upon them, explains every experimental result available.[2] There are some reasons to believe that the Large Hadron Collider (LHC), being built near Geneva in order to explore the further reaches of the standard model, may also provide something unexpected in the next few years, but it seems mostly a fond hope that it will be able to tell us about quantum gravity. The sad fact is that it may be impossible to probe quantum gravity with our current experimental techniques. We are left, then, to theorize in a vacuum, a far cry from the era when the standard model was developed and unexplained experimental results littered the landscape.

Such a situation is not entirely without precedence, however. At the turn of the twentieth century, Einstein was presented with the incompatibility of Newton's theory of gravitation and his newly developed theory of special relativity. Almost without experimental input, and with a little help from mathematicians, Einstein was able to reconcile these two theories into his theory of general relativity, a profound new understanding of the nature of space and time.

The hope then is that we could, as a field, be like Einstein and solve our current conundrum by thought alone. Ironically, however, the origin of string theory has nothing to do with the attempt to bring quantum mechanics and Einstein together. String theory instead began as a single mathematical expression that contained many desirable properties relating to the force that holds the nucleus of the atom together, the 'strong' nuclear force. It was soon realized that the origin of this expression could be understood if one replaced the particles of quantum mechanics with miniscule vibrating strings. While intriguingly matching certain experimental facts, the resulting theory had many problems and was ultimately supplanted by the theory of quantum chromodynamics, a part of the aforementioned standard model.[3] One of the problems was that the strings could behave like a massless particle which was not observed. In the mid-seventies, it was realized that this particle could describe gravitation, and string theory was reimagined as a theory of quantum gravity. For this reason, it is often said that string theory 'predicts' gravity. This means that,

---

[2] One new, but not unexpected result, is that a subatomic particle called the 'neutrino' has a mass. The smallness of this mass represents somewhat of a puzzle, but the extension to the standard model that allowed for such a mass had been worked out well before the presence of the mass was discovered. Still, it is not completely impossible that neutrinos may ultimately be able to tell us something about quantum gravity.

[3] In many ways, the AdS/CFT conjecture described below represents both a revival and improvement of this old style of string theory.

when one writes down the quantum theory of strings (as opposed to particles), gravity appears *ex nihilo*. As recounted in *Not Even Wrong*, by 1984 the number of needed dimensions for the theory to make sense had been reduced to ten (still more than the four we observe), and the resolution to a particular technical problem caused Dr. Witten to devote himself to the study of strings. Many others soon followed his example. This is often called the 'first superstring revolution'.

Although I was not around then, it is my understanding that a number of grandiose claims were made during the enthusiasm of that era. It was hoped that string theory would uniquely distinguish the standard model from the infinite array of possible quantum theories. After the initial burst of excitement, these hopes were not realized, and string theory went through a fallow period in the late eighties. In the mid-nineties, however, the subject was revived with new techniques for attacking problems that had previously seemed insuperable. These past years have led to a deep understanding of black holes and uncovered unsuspected relations between the different string theories that were known to exist. This has reinvigorated the field and convinced many that string theory is on the right track. This is often called the 'second superstring revolution'.

Such rapid progress proved to be unsustainable, however, and string theory has not had a revolution since. This has led to a sense of the doldrums among many, but with the lack of an obvious bandwagon to climb on, it has also led a wider variety of research and a renewal of interests in aspects of the theory that had fallen by the wayside in the nineties. One of these developments, described not entirely accurately in chapter 17 of *Not Even Wrong,* is the discovery that there appear to be an unimaginably large number of possible states of string theory each of which leads to a different set of particles and forces. The dream of uniqueness, if not dead, is certainly breathing its last breaths. After the halcyon days of the nineties, I find it now to be a time of diminished but more realistic expectations with the prospect for a complete understanding seeming farther in the future. But revolutions are always unexpected, and who can say what is around the corner.

The best argument against string theory is that it has been thirty years now, and string theory has yet to predict a single thing about our world. No one has proven string theory incorrect either. In its current state, it would be difficult to do so without also undermining much of quantum mechanics in the process. The question, then, is one of pessimism or optimism. For good reasons, many string theorists still believe that string theory represents the best hope we have for uniting quantum mechanics and gravity. It is, importantly, if not the only game in town, in the judgment of many, the best game in town.

III.

The critique of string theory in *Not Even Wrong* is given in chapters 12-17. The critique ranges from the technical (chapter 12), to the aesthetic (13), the philosophical (14,17) and the sociological (16). Chapter 15, a recounting of the so-called "Bogdanov affair" seems out of place here. As I will discuss later, its inclusion is representative of a distasteful sort of innuendo that is interspersed within these chapters. I will spend this section discussing the science, leaving the innuendo, my major problem with this book, for the next section.

Chapter 12 presents a host of technical issues with both supersymmetry, a variety of quantum theory I have not yet discussed, and superstring theory. It would be possible to go through this chapter and discuss various misunderstandings and caveats[4], but the majority of the issues are both largely correct and well-known. At times it does feels petty, though, such as when, at the end of the chapter, Dr. Woit, unable to begrudge string theorists much of anything, uses the fact that quantum field theory techniques are

---

[4] One particularly egregious example is in the discussion of the cosmological constant on pp. 178-9. It is true, as Dr. Woit states, that the cosmological constant as predicted by supersymmetric models is much too large. What he neglects to mention, however, is that without supersymmetry (such as is the case in the standard model), the problem persists and is, in fact, many, many times worse.

used to do computations in string theory to deny us even our successes in mathematics. In lieu of a tedious point by point discussion of this section, I hope that the final section of this review will serve as a counterpoint.

It is true, as Dr. Woit states in this chapter, that there really is no such thing as 'string theory'. It is, rather, a collection of partial theories and calculational techniques bound by physical intuition and conjecture. The amazing and beautiful thing, for those of us who study it, is that this skein is remarkably robust. Calculations that have the possibility of destroying this structure invariably turn out to reinforce it. One cannot help believe that, while we may not know what it is, there is a theory out there waiting for us. It is in this way that string theory is a labor of hope. It is respectable to not share these dreams, and Dr. Woit clearly does not, but often has it been that, when presented with this fascinating and beautiful web, even some skeptics have begun to believe. This perception of beauty is the subject of the next chapter in *Not Even Wrong*, and while it is true that many of the recent constructions reek of *ad hocery* and Rube Goldberg contraptions, I hope in this review to communicate some of what string theorists find so beautiful about string theory. Beauty ultimately being a personal judgment, however, Dr. Woit is entitled to his opinion.

It is not worth spending much time on whether or not string theory is 'science'. String theory is a hope to do science and, as Dr. Woit agrees, much of what goes on in theoretical physics is the same. The question as to when to give up on one's hopes is a difficult one, and I am not sure the philosophers have much to add on the subject. The one philosophical issue of import here is the subject of chapter 17, the 'landscape' of vacua. These vacua are the countless states of the theory mentioned above. In this context, our universe would correspond to a particular vacuum which determines the sort of physics we would observe. As is described in the chapter, while there were inklings of a multitude of vacua in the eighties, it was possible to believe that there was some mechanism which would choose a unique vacuum which would, hopefully, correspond to our world. Recently, that belief seems increasingly less plausible. There is a seemingly robust description of zillions and zillions of vacua, each of which seems to describe consistent physics completely unlike our world.

Faced with this proliferation, one immediately begins to worry about the predictivity of the theory. With so many vacua it might be possible to explain any experimental result one can imagine. If this were true, it would not mean that string theory was wrong, but it would mean that it would be completely useless as it could never make a prediction. To avoid this unhappy state of affairs, a fair number of senior people have decided to take a radically new approach to predictivity. The idea is that one should determine all the possible vacua that are consistent with the existence of intelligent life and imagine that it is equally likely[5] that we could be in any of them. Then, while we could not precisely predict anything about our world, we could assign probabilities to the results of future experiments. These are often called 'anthropic explanations' although they depend both on the anthropic principle, the statement that we definitely exist, and a principle of mediocrity, the idea that we are not unlikely. This latter principle allows one to translate the counting of vacua into a statement about probabilities.

I have significant philosophical questions about this endeavor and believe that it is misguided. In fact, at a recent conference in string theory, eighty per cent of the participants, somewhat to the chagrin of many senior figures, voted against such an 'explanation' for a particular constant of nature, the cosmological constant. Beyond the philosophical and implementation issues with this program, I also feel that it is horribly premature. The first point is that, as has often been joked in the field, having produced precisely zero vacua that look like our world, it was immediately postulated that there are countlessly many. We simply do not know, when we begin to be able to input what we already know about the world into string theory, that this proliferation of vacua may not ultimately winnow away to something manageable. Even if it does not, it is helpful to remember that there are uncountably many quantum field theories, and yet, after doing a finite number of experiments, we can still use them to make predictions. It seems possible that such a

---

[5] More generally, one might relax the assumption of the vacua being equally likely. This is a significant issue in this process, but not one I wish to dwell on here.

situation will appear in string theory. Finally, a number of string theorists have begun to conjecture that certain types of quantum theories never occur in string theory. Thus, the discovery that our world was one of these disallowed theories would at least be able to tell us that string theory was incorrect.

Why do we continue on, needing so much hope? I reiterate my promise to describe later the successes of string theory, but now I want to discuss the most common justification for studying string theory: “it is the only game in town”, the subject of chapter 16. In fact, this chapter is more of the sort of investigation into the practice of physics and the pressures on physicists that I expressed my hope for in the introduction. It is not at all clear to me that our current system is the best for incubating talent and new ideas, and I have little positive to offer in that direction. Dr. Woit ascribes string theory’s preeminent position to “the social and financial structures within which people are working are an important part of this situation”[6] but this is not responsive to the question of whether string really is “the only game in town.” This statement needs some translation. As mentioned above, what is meant is that string theory is the best game in town. This sounds horribly arrogant, but it is an opinion based on considerable investigation. Quantum gravity has turned out to be an enormously difficult problem. There is a paucity of even plausible ideas, some of which are related in chapter 18. Most of them have been around for years and have progressed little beyond bare skeletons. One exception is the collection of ideas generally termed Loop Quantum Gravity (LQG). It is true, as Dr. Woit relates, that researchers in LQG are often aggrieved by the statement that string theory is the only game in town, but the fact of the matter is that most string theorists that have investigated LQG have found it wanting. It is a radically new class of theories that has as yet been unable to make any contact with the major results of the usual style of quantum theory. In contrast to string theory, the theory of quantum gravity so produced has not been able to demonstrate even the attraction known to Newton hundreds of years ago. The nontrivial consistency checks that the standard model obeys to be a viable quantum theory go unexplained in LQG. It is not impossible that these difficulties will be overcome, but the I think it is fair to say that the relative proportion of string theory versus LQG positions available reflects the collective assessment of the field as to the prospects of each theory.

And yet, given that I have painted a not entirely rosy picture of string theory, should we not be encouraging radically new ideas? I wish I knew how. It is true that the publish or perish environment discourages thinking about hard problems that will probably end in frustration. Given the relatively small number of positions available for work on quantum gravity, the obvious choice for a young researcher is to work in a direction where there are tangible results in sight. There are plausible improvements one could make to the current system that might make such thinking more viable. Tenure, of course, is meant to encourage independence, but it may not be enough given the widespread belief that these sort of new ideas are the mainly the province of the young. Ultimately, I hope and believe that the next young Einstein will be able to break through regardless of the system. Were such a thing to happen, the field would rapidly follow the new ideas whether they lead to string theory or to somewhere else completely unexpected.

IV.

What worries me about the prospects of a string theory backlash is the propensity for such things to quickly transform from attacks on the science (or lack thereof) to attacks on the scientists. Although there are few overt attacks in *Not Even Wrong*, there are number of disturbing juxtapositions. These stories add little to the scientific case and mainly serve to paint string theorists in a bad light through often anonymous quotations and to tar string theory by association with a number of unfortunate incidents.

The first example of this is in chapter 11, “String Theory: History”. At the end of his description of something called the ‘analytic S-matrix’ and Dr. Geoffrey Chew’s ‘bootstrap philosophy’, the discussion takes a surprising turn:

---

[6] p. 239

> The S-matrix programme continued to be pursued by Chew and others into the 1970s. Just as the political left in Berkeley fell apart, with many turning to Eastern and New Age religions, followers of the S-matrix also stopped talking about democracy and some began to look to the East.[7]

This particular subsection concludes with

> Even now, Capra's book [*The Tao of Physics*], with its denials of what has happened in particle theory, can be found selling well at every major bookstore. It has been joined by some other books in the same genre, most notable Gary Zukav's *The Dancing Wu-Li Masters*. The bootstrap philosophy, despite its complete failure as a physical theory, lives on as part of an embarrassing New Age cult, with Chew continuing to this day as guru, refusing to acknowledge what has happened.[8]

Taken alone, this is perhaps just an interesting anecdote to supplement the history being conveyed. However, given that the thesis of *Not Even Wrong* is the complete failure of string theory as a physical theory, it is hard to imagine that the description of this "embarrassing New Age cult" is not meant to rub off on the current practice of string theory. It is worth noting that, while historically string theory is connected with the analytic S-matrix philosophy, it has moved far beyond those origins, and as far as I know none of the people mentioned in this section of the book have anything to do with modern string theory.

This idea of a cult returns in chapter 14 in the story of Dr. John Hagelin who some may remember as having run for president under the banner of the Natural Law party. Dr. Hagelin has written a fair number of perfectly legitimate scientific articles, mainly on the subject of a Grand Unified Theory (GUT), a theory which seeks to unite the three non-gravitational forces. He also has an interest in transcendental meditation which eventually led him to Maharishi International University. As Dr. Woit states:

> By 1995, Hagelin had written 73 scientific articles most of them published in very prestigious particle theory journals, many of them cited by more than a hundred later articles. If one examines the list of these articles in the SLAC database, a couple of titles stand out: 'Is Consciousness the Unified Field? (A Field Theorist's Perspective)' and 'Restructuring Physics from its Foundations in Light of Maharishi's Vedic Science'.[9]

Now, it is true that those particular titles stand out in a search of Dr. Hagelin's work, but it is because they are not at all representative of the other 71 articles. Neither article was published in a legitimate journal, and neither has a single citation. It appears, at least from a perusal of the titles of his papers, that Dr. Hagelin was able to mostly separate his interest in Vedic science from his more legitimate research pursuits. What does this have to do with string theory? Dr. Woit helpfully tells us, "Looking at these articles, one finds that from the mid-1980s on, Hagelin was identifying the 'unified field' of superstring theory with the Maharishi's 'unified field of consciousness'." It is easy to imagine a reader concluding from these quotations that Hagelin's rather non-mainstream beliefs were among those "published in very prestigious particle theory journals",[10] when such a thing clearly is not true.

In fact, little of Dr. Hagelin's work has anything to do with string theory, and Dr. Woit comments that "virtually every theoretical physicist in the world rejects all of this as nonsense and the work of a crackpot"[11].

---

[7] p. 150

[8] pp. 152-3

[9] p. 210

[10] p. 210

[11] p. 211

Unfortunately, the quote continues, “but Hagelin’s case shows that crackpots can have PhDs from the Harvard Physics Department and a large number of frequently cited papers published in the best peer-reviewed journals in theoretical physics.” This is ostensibly a segue into the question of what is and is not science, but the implication is hard to miss. This story does little to illustrate the subsequent philosophical discussion. Instead, it again juxtaposes string theory with a cult-like group and implies that those involved might just be crackpots.

The comparison to cults is made more explicit at the end of the chapter 14:

> I have heard another version of this worry expressed by several physicists, that string theory is becoming a ‘cult’, with Witten as its ‘guru’. ... Some string theorists do express their belief in string theory in religious terms. For instance, a string theorist on the faculty at Harvard used to end all his e-mail with the line ‘Superstring/M-theory is the language in which God wrote the world.’ String theorist and author Michio Kaku, when interviewed on a radio show, described the basic insight of string theory as ‘*The mind of God is music resonating through 11-dimensional hyperspace*’.[12]

This is an unpleasant array of innuendo. “Several physicists” believe these things. “Some string theorists” use religious language to describe their study. And look, here are two marginal examples. The inclusion of such statements might make one infer that Dr. Woit thinks of string theory as a cult, but not to worry,

> Personally, I don’t think the categories of cult or religion are especially appropriate in this circumstance since they refer to human activities with many quite different characteristics from what is going on in the physics community.[13]

It is strange, then, that Dr. Woit spends so much time describing cult-like behavior when he so clearly states that he does not believe that string theory has fallen victim to it. I guess it is all just meant as a warning: “Science thus has no grant of immunity from the dangers of cult-like behaviour to which human beings can fall prey.”[14] I can only hope that if string theory does ever show signs of becoming a cult, Dr. Woit will be kind enough to inform us.

The subject of the next chapter is the strange story of the Bogdanov brothers, something in which I happen to have been marginally involved. A brief summary of their story is that these two French brothers were able to obtain PhDs in physics after having published a number of papers each of which ranged from incomprehensible to nonsensical. When this was realized, there was a brief scandal in the field, making the pages of such publications as Nature and The New York Times. What is told in *Not Even Wrong* is generally true, and we are told the reason for its inclusion: “The Bogdanov affair convincingly shows that something is seriously broken in that part of the scientific community that pursues speculative research in quantum gravity.” This conclusion is undermined by a number of important elisions in the telling of the story, the most important of which is that the writings of the Bogdanovs, to the extent that anyone can make sense of them, *have almost nothing to do with string theory*. None of the journals they published in are journals that string theorists generally publish in, and none of their papers appeared on the online preprint archive from which the vast majority of physicists obtain papers. It is safe to say that, outside of the referees of the relevant journals, nobody in the field had ever seen these published papers. The question, then, is what does this story have to do with string theory? Dr. Woit quotes the following anonymous statement

---

[12] pp. 215-6

[13] p. 216

[14] p. 216

> So no one in the string group at Harvard can tell if these papers are real or fraudulent. This morning told that they were frauds, everyone was laughing at how obvious it is, This afternoon, told they are real professors and that this is not a fraud, everyone here says, well, maybe it is real stuff.[15]

The implication here is that string theorists are incapable from telling legitimate research from nonsense. I cannot speak for the group at Harvard, but at the time I was a graduate student at Princeton, and I can speak for my own involvement. I first learned of the relevant papers in a posting on the internet by Dr. John Baez. Having found a copy of one of the papers available online, I posted that "the referee clearly didn't even glance at it."[16] While the papers were full of rather abstruse prose about a wide variety of quite technical areas, it was easy to identify outright nonsense in the areas about which I had some expertise. The above quotation about Harvard surfaced a week later in an e-mail that I and many others received from the Bogdanovs. I cannot speak to the authenticity of any of it, but I find it hard to believe that the faculty at Harvard would be unable to see the nonsense that I, as a graduate student, found easily. The fact remains, however, that a pair of non-string theorists were able to get nonsensical papers generally not about string theory published in journals generally not used by string theorists. This is surely an indictment of something, but its relevance to string theory is marginal at best.

There are assorted other shots at string theorists scattered through the book. Dr. Woit devotes a page to describing his failure to have his book published at a university press. Taking the story at face value, at worst it speaks poorly of a few anonymous referees, but it hardly supports the sweeping conclusion that "clearly the level of such dishonesty and the extent to which many string theorists were unwilling to acknowledge the problems of their subject was far beyond anything I had originally imagined."[17] Later, we have the following

> A superstring theorist looking for a pleasant place to spend a week or so at someone else's expense will in most years have a choice of thirty or so conferences to go to, many in exotic locations. In 2002 for, for example, among the most prestigious and difficult to arrange options would have been a summer workshop in Aspen but, during the year, other possible destinations would have been Santa Barbara, Chile, Trieste, Genova, the Black Sea, Corsica, Paris, Berlin, Vancouver, Seoul, China and many others, including Baku in Azerbaijan.[18]

It would seem that string theorists are recipients of great privilege on "someone else's expense". What Dr. Woit does not say is that this situation is hardly unique to string theory. Physicists (and many other fields of academia) like to hold conferences and schools in places that are pleasant to visit. And, while it is true that many of these trips are funded, much of this support will usually go to poorly paid young researchers who could not afford to attend on their own.

V.

All this unpleasantness aside, there are a number of scientific arguments in *Not Even Wrong*, and the reader who has not concluded that string theorists are spoiled acolytes of the cult of Witten will probably still be left wondering why anyone would ever want to study string theory. I hope now to answer that question. In fact, there are two somewhat linked but still quite independent issues here. Chapter 12 is devoted to attacking both supersymmetry and string theory. Neither theory necessitates the other, however, and,

---

[15] p. 218

[16] http://groups.google.com/group/sci.physics.research/msg/46af03e89db9bb59

[17] p. 228

[18] p. 232

while there is a substantial overlap, there are many who work on one subject but not the other. I will begin with the question of supersymmetry.

The standard model, for all its experimental successes, has at least one unfortunate aspect: it requires something to break the electroweak force into electromagnetism and the weak nuclear force. This is called electroweak symmetry breaking and is usually accomplished by a field named after Dr. Peter Higgs (although the idea occurred independently to many scientists). The Higgs field is unique in the standard model (it does not carry something called 'spin'), and this leads to a unique problem: quantum fluctuations will give a huge contribution to its mass. The 'natural' value for this contribution ends up being near the Planck mass, around one ten-thousandth of a gram. The actual mass of the electron is about 10^{17} times smaller than that which means that we need to remove by hand these fluctuations to an accuracy of seventeen decimal places. In physics this is called 'fine-tuning' or a 'naturalness' problem, and, while there is nothing wrong with it theoretically, it is rather distasteful. Supersymmetry is a way of canceling these quantum fluctuations by pairing each particle up with another as yet unobserved type of particle and having the fluctuations of those pairs of particles cancel each other out.

Such a proliferation of new particles, one for every particle already observed, seems like a high price to pay for solving a metaphysical problem, but it turns out that supersymmetry accomplishes much more. When one writes down the minimal extension of the standard model that has supersymmetry, it turns out that the quantum fluctuations automatically force the (now two) Higgs fields to break the electroweak symmetry; this breaking does not have to be put in by hand as in the standard model. Another nice feature is that the lightest of these supersymmetric partners is stable in realistic theories. This particle turns out to have precisely the right properties to be the dark matter that we think we need to understand the universe. Finally, it turns out that the strengths of the three nongravitational forces depend on how much energy is involved in one's experiment. If you graph these strengths against energy in the standard model, the three lines almost meet, but not quite. If you put in supersymmetry, however, the lines meet to within the error bars of the data. This is aesthetically quite pleasing and provides support for the previously mentioned GUTs where these forces unify at some high energy scale.

This is not a bad portfolio for one simple modification to the standard model. One might even consider it beautiful that the imposition of this one symmetry could solve so many problems. For these reasons, people flocked to supersymmetry. Still, as Dr. Woit relates, the problems come because one is forced to break supersymmetry to explain why we have not detected any of these partner particles. Many of the naturalness problems return, and there are a number of possible breaking mechanisms. A nice property of quantum field theory, however, is that we do not need to know everything about how the breaking happens; we can parametrize our ignorance. This parametrization is the 105 new parameters referred to in the chapter. What Dr. Woit sees as a problem, however, can be seen as an opportunity. No one expects all of these 105 parameters to be independent. Different mechanisms for supersymmetry breaking lead to relations among the parameters. If we see supersymmetry at the LHC, it should be possible, at the proposed next generation collider, the International Linear Collider (ILC), to measure many of these parameters which will allow us to probe the mechanism by which supersymmetry is broken.

The considerable enthusiasm for supersymmetry has ebbed somewhat in recent years. In the simplest model, supersymmetry predicts the mass of the aforementioned Higgs field to be on the light side. With each passing year and experiment, however, the lower bounds on said mass get larger and larger, pushing supersymmetry into a corner, needing more and more fine-tuning to survive. It is possible to avoid this by hiding the Higgs in various ways by adding extra particles to the model, but then the appeal of the original simple models is lost. All this theory aside, the nicest aspect of supersymmetry, at least of the sort that solves the fine-tuning problem mentioned above, is that we should be able to see it at the LHC, and this particular question should be resolved, if all goes well, by the end of the decade.

For quantum gravity, on the other hand, the experimental outlook is much bleaker. While there are various possible observations that, if made, could confirm a particular theory, there is almost nothing that would serve to rule out a given theory. Thus, we are flying blind. Inevitably, there exist people who will nonetheless theorize their best in such circumstances. Why do the vast majority of them choose to work on string theory?

There are, of course, non-scientific reasons, but I will focus on the major scientific reasons. The most important of these is that string theory really does appear to be a theory of quantum gravity, with the emphasis on the word 'gravity'. It is the only approach, to my knowledge, which includes quantum field theory and in which you can compute the gravitational attraction between two objects. While string theory does not, at this point, predict our world, it can at the very least plausibly encompass it. No other theory has been shown to do that.

Still, as mentioned above, there is really no such thing as string theory, but rather an assortment of connected principles and calculational techniques. That these all agree in regimes where they overlap makes everyone who works on strings believe that there is a some theory underlying all of it; we just don't know what it is yet. Nevertheless, we can still do a number of impressive things with what we have. The most famous and significant of these is the calculation of the entropy of a black hole by Drs. Andrew Strominger and Cumrun Vafa.

As is well known by now, a black hole is a region of space, predicted by Einstein's theory of general relativity, from which nothing can escape. What is surprising is that, as Dr. Stephen Hawking showed, black holes have a temperature. Eventually, a black hole left alone will radiate away all its mass as heat. Temperature, however, is a fundamentally disordered thing. A hot glass of water, for example, has countless molecules all zigging around in random directions at high speeds. What this means is that something that has a temperature should also have a lot of states that all look the same from a large scale point of view. It does not matter which particular direction each molecule in the glass of water is going; it still looks like a glass of water to us. The (logarithm of the) number of large states that look the same is called the entropy. Hawking's calculation shows that black holes seem to have an entropy.[19]

Finding the individual states that lead to this entropy has been one of the holy grails of quantum gravity research. While we cannot compare prospective theories with experiment, we can at least compare them with Hawking's calculation which only involves quantum field theory in the presence of a black hole. In the mid-nineties, it was realized that certain objects in string theory, called D-branes, precisely described black holes. The simplest examples all have zero entropy, but Strominger and Vafa were able to analyze a more complicated example and exactly count the possible states of the D-brane. The answer they obtained was precisely the entropy of the black hole as predicted by Hawking's formula. Subsequently, other quantities have been computed that again match up to expectations, and the calculations have been extended to a wide class of black holes (although, unfortunately, not to every possible black hole).

It is difficult to overstate the impact of this calculation on the field. Unlike calculations in other theories, no assumptions had to be made to in order to obtain the correct result. It did not have to work. This is a striking confirmation that, for whatever its other flaws, string theory really is a theory of quantum gravity. No other theory has been able to match this success. The significance of this is that, even if string theory turns out to be the wrong theory of quantum gravity, how it solves the puzzles presented by the unification of quantum mechanics and gravity will aid us in understanding and formulating future theories.

The connection between D-branes and black holes also led to another significant advance. It turns out that the region near a black hole looks very much like something called anti-de Sitter (AdS) space. This is a particular solution of Einstein's equations that looks nothing like our world. Dr. Juan Maldacena realized that, if D-branes and black holes were the same thing, this particular spacetime would be exactly equiva-

---

[19] This inverts the history. Before Hawking found his temperature, Dr. Jacob Bekenstein postulated that a black hole should have an entropy for other reasons relating to the second law of thermodynamics. This was generally disregarded until Hawking's calculation showed that the corresponding temperature also exists.

lent to the theory that describes the D-brane, an ordinary quantum field theory. This equivalence is called the AdS/CFT[20] conjecture. It claims that string theory in anti-de Sitter space is the same thing as an ordinary quantum field theory in one fewer dimension. While anti-de Sitter space is not the universe in which we live, this is the first complete definition of any theory of quantum gravity in any universe[21]. As such, it is a significant and profound accomplishment.

This can also be turned around so we can attempt to use string theory to learn new things about quantum field theory. We hope to find the correct string theory that would completely describe quantum chromodynamics (QCD), but even lacking that, there is some recent hope that one can use generic aspects of string theory to help understand the results of the Relativistic Heavy Ion Collider (RHIC), a machine that collides gold nuclei at high energies to explore QCD in extreme environments. Thus, even if string theory fails to be our theory of quantum gravity, it will still be interesting as an alternate description of quantum field theory.

Finally, and ironically given Dr. Woit's interest in mathematics, string theory has led to a wide array of interesting mathematics. Many of the people who work on the subjects lauded in chapter 10 also spend much of their time on string theory. In fact, string theory has given new insight into and has extended many of the mathematical results from quantum field theory that Dr. Woit so admires. For a person with significant mathematical interests (like myself), this makes string theory exciting to study irrespective of its successes or failures as a theory of nature.

The upshot of this is that most people believe that string theory is a theory of quantum gravity, and that it is the only theory developed enough to deserve this label. For that reason alone, those of us who dream quantum gravitational dreams are drawn to study it. It is not a perfect dream, and many of the problems pointed out *Not Even Wrong* are legitimate critiques. I do not know how to tell when to give up on a dream. Is a quarter century of study a long time or a brief moment in history? It is a strange time in physics that has encouraged so many to proceed in such a speculative endeavor. The lack of experimental results has necessitated thinking that is not grounded in any unexplained observations of reality.[22] This era will soon come to an end, however. If all goes well, the LHC will turn on in 2008, with new experimental results hopefully following soon after that. The theoretical physics community is already gearing up for this event. In addition to cosmology, more jobs are going to phenomenologists, theoretical physicists that specialize in the sort of physics that is likely to be observed at accelerators. Everyone hopes that something beyond the standard model will be seen, and the usual practice of physics can continue after this brief interregnum. Even with this change in focus, however, there will still be a few positions for those willing to step out onto the speculative ledge and think about quantum gravity. With hubris, curiosity and stubbornness, scientists will always follow these dreams. Because of all that it has accomplished, and until an obviously better idea comes along, many of those foolhardy scientists will continue to dream of strings.

---

[20] CFT here stands for 'Conformal Field Theory', a particular type of quantum field theory.

[21] There are theories of 'quantum gravity' in universes with less than four dimension, but the theory simplifies in those dimensions to such an extent that it is far from clear that these theories have much to tell us about quantum gravity in four or more dimensions.

[22] There are deep and interesting puzzles from cosmology, but without the ability to create new universes in a lab, it is not comparable to the sort of experiment one can do in an accelerator.