

String Theory: An Evaluation

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For nearly seventeen years now most speculative and mathematical work in particle theory has centered around the idea of replacing quantum field theory with something that used to be known as “Superstring Theory”, but now goes under the name “M-Theory”. We’ve been told that “string theory is a part of twenty-first-century physics that fell by chance into the twentieth century”, so this year the time has perhaps finally come to begin to evaluate the success or failure of this new way of thinking about particle physics. This article will attempt to do so from the perspective of a quantum field theorist now working in the mathematical community.

The theory has been spectacularly successful on one front, that of public relations. Best-selling books and web sites are devoted to explaining the subject to the widest possible audience. The NSF is funding a series of NOVA programs on string theory, and the ITP at Santa Barbara is organizing a conference to train high school teachers in string theory so that they can teach it to their students. The newspaper of record informs us that “Physicists Finally Find a Way to Test Superstring Theory” (NYT 4/4/00).

The strongest scientific argument in favor of string theory is that it appears to contain a theory of gravity embedded within it. It is not often mentioned that this is not yet a consistent quantum theory of gravity. All that exists at the moment is a divergent series that is conjectured to be an asymptotic perturbation series for some as yet undefined non-perturbative string theory (the terms in the series are conjectured to be finite, unlike the situation in the standard quantization of general relativity). String theorists actually consider the divergence of this series to be a virtue, since otherwise they would have an infinity (one for each compactification of six dimensions)

of consistent theories of gravity on their hands, with no principle for choosing amongst them.

String theory has led to many striking new mathematical results. The concept of “mirror symmetry” has been very fruitful in algebraic geometry, and conformal field theory has opened up a new, fascinating and very deep area of mathematics. Unfortunately the mathematically interesting parts of string theory have been pretty much orthogonal to those parts that attempt to connect with the real world.

The experimental situation is best described with Pauli’s phrase “it’s not even wrong”. No one has managed to extract any sort of experimental prediction out of the theory other than that the cosmological constant should probably be at least 55 orders of magnitude larger than experimental bounds. String theory not only makes no predictions about physical phenomena at experimentally accessible energies, it makes no predictions whatsoever. Even if someone were to figure out tomorrow how to build an accelerator capable of reaching Planck-scale energies, string theorists would be able to do no better than give qualitative guesses about what such a machine might see. This situation leads one to question whether string theory really is a scientific theory at all. At the moment it’s a theory that cannot be falsified by any conceivable experimental result. It’s not even clear that there is any possible theoretical development that would falsify the theory.

String theorists often attempt to make an aesthetic argument, a claim that the theory is strikingly “elegant” or “beautiful”. Since there is no well-defined theory, it’s hard to know what to make of these claims, and one is reminded of another quote from Pauli. Annoyed by Heisenberg’s claims that modulo some details he had a wonderful unified theory (he didn’t), Pauli sent his friends a postcard containing a blank rectangle and the text “This is to show the world I can paint like Titian. Only technical details are missing.” Since no one knows what “M-theory” is, its beauty is that of Pauli’s painting. Even if a consistent M-theory can be found, it may very well be a theory of great complexity and ugliness.

From a mathematician’s point of view, the idea that M-theory will replace the Standard Model with something aesthetically more impressive is rather suspicious. Two of the most important concepts of the Standard Model are that of a gauge field and that of the Dirac operator. Gauge fields are identical with connections, perhaps the most important objects in the modern formulation of geometry. Thinking seriously about the infinite dimensional space of all connections has been a very fruitful idea that mathematicians

have picked up from physicists. The importance of the Dirac operator is well known to physicists, what is less well known is that it is of similar importance in mathematics where it plays the role of "fundamental class" in K-theory. This is reflected in the central role the Dirac operator plays in the Atiyah-Singer index theorem, one of the great achievements of twentieth century mathematics.

To the extent that the conceptual structure of string theory is understood, the Dirac operator and gauge fields are not fundamental, but are artifacts of the low energy limit. The Standard Model is dramatically more "elegant" and "beautiful" than string theory in that its crucial concepts are among the deepest and most powerful in modern mathematics. String theorists are asking mathematicians to believe in the existence of some wonderful new mathematics completely unknown to them involving concepts deeper than that of a connection or a Dirac operator. This may be the case, and one must take this argument seriously when it is made by a Fields medalist, but without experimental evidence or a serious proposal for what M-theory is, the argument is unconvincing.

Given the lack of experimental or aesthetic motivation, why do so many particle theorists work on string theory? Sheldon Glashow describes string theory as "the only game in town", but this begs the question. Why is it the only game in town?

During much of the twentieth century there were times when theoretical particle physics was conducted quite successfully in a somewhat faddish manner; there was often only one game in town. Experimentalists regularly discovered new unexpected phenomena, each time leading to a flurry of theoretical activity and sometimes to Nobel prizes for those quickest to correctly understand the significance of the new data. Since the discovery of the J/Psi in November 1974, there have been no solid experimental results that disagree with the Standard Model. It is likely that this situation will continue at least until 2006 when experiments at the LHC at CERN are scheduled to begin. To a large extent particle theory research has continued to be conducted in a faddish manner for the past quarter century, but now with little success.

Graduate students, post-docs and untenured junior faculty interested in physics beyond the Standard Model are under tremendous pressures in a brutal job market to work on the latest fad in string theory, especially if they are interested in speculative and mathematical research. For them, the idea of starting to work on an untested new idea that may very well fail looks a lot

like a quick route to professional suicide. Many physics researchers do not believe in string theory but work on it anyway. They are often intimidated intellectually by the fact that some leading string theorists are undeniably geniuses, and professionally by the desire to have a job, get grants, go to conferences and generally have an intellectual community in which to participate.

What can be done? Even granting that string theory is an idea that deserves to be pursued, how can theorists be encouraged to try and find more promising alternatives? Here are some modest proposals, aimed at encouraging researchers to strike out in new directions:

1. Until such time as a testable prediction (or even a consistent compelling definition) emerges from string theory, theorists should publicly acknowledge the problems theoretical particle physics is facing, and should cease and desist from activities designed to sell string theory to impressionable youths, popular science reporters and funding agencies.

2. Senior theorists doing string theory should seriously reevaluate their research programs, consider working on less popular ideas and encourage their graduate students and post-docs to do the same.

3. Instead of trying to hire post-docs and junior faculty working on the latest string theory fad, theory groups should try and identify young researchers who are working on original ideas and hire them to long enough term positions that they have a chance of making some progress.

4. Funding agencies should stop supporting theorists who propose to continue working on the same ideas as everyone. They should also question whether it is a good idea to fund a large number of conferences and workshops on the latest string theory fad. Research funds should be targeted at providing incentives for people to try something new and ambitious, even if it may take many years of work with a sizable risk of ending up with nothing.

Particle theorists should be exploring a wide range of alternatives to string theory, and looking for inspiration wherever it can potentially be found. The common centrality of gauge fields and the Dirac operator in the Standard Model and in mathematics is perhaps a clue that any fundamental physical model should directly incorporate them. Another powerful and unifying idea shared by physics and mathematics is that of a group representation. Some of the most beautiful mathematics to emerge from string theory involves the study of (projective) representations of the group of conformal transformations and of one-dimensional gauge groups (“loop groups”). This work is essentially identical with the study of two dimensional quantum field

theory. The analogous questions in four dimensions are terra incognita, and one of many potentially promising areas particle theorists could look to for inspiration.

During the 1960's and early 1970's, quantum field theory appeared to be doomed and string theory played a leading role as a theory of the strong interactions. Could it be that just as string theory was wrong then, it is wrong now, and in much the same way: perhaps the correct quantum theory of gravity is some form of asymptotically free gauge theory? As long as the best young minds of the field are encouraged to ignore quantum field theory and pursue the so far fruitless search for M-theory, we may never know.