# Superstring Unification

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## Abstract

This speech is based on a short article entitled "Resuscitating Superstring Theory" that I wrote for the November 16, 1987 issue of *The Scientist*.

The ultimate goal of particle physics is to achieve a unified understanding of fundamental forces and particles in terms of beautiful and compelling mathematical principles. A closely related goal is to achieve a deeper understanding of the origin and evolution of the Universe. These overarching themes were pioneered by Einstein. It seems appropriate to reflect on them in this centennial year of his general theory of relativity.

With the wisdom of hindsight, we can say that his goals were on target, but that he did not have a realistic chance of success in his quest for a "unified field theory." Crucial experimental and observational facts were not known at the time. Nuclear forces were poorly understood, and so he did not consider them. Also, his lingering doubts about the validity of quantum mechanics led him to focus on classical considerations.

Let me now turn to the post-Einstein era, which is the one that I have witnessed in my career. The strong nuclear force that binds quarks together inside protons, neutrons, and the other hadrons was not yet understood in the 1960s. During that decade, theorists faced the challenge of finding a simple explanation for the wealth of new particles that the experimentalists were discovering. I was a student in Berkeley, where Professors

Chew, Mandelstam, and others were developing ideas such as "Regge pole theory" and the "bootstrap hypothesis." These approaches were not successful, but by a remarkable sequence of events they led to superstring theory.

In 1968-70 (when I was a junior faculty member in Princeton), Gabriele Veneziano, Yoichiro Nambu and others developed the "dual resonance model," which was interpreted a little later as the theory of a relativistic string. This model incorporates the bootstrap and Regge ideas in a specific mathematical framework, and thus it was able to account for many qualitative features of hadron physics. In 1971 a second dual model was discovered by Pierre Ramond, André Neveu, and me.

Both models shared certain defects: They required additional dimensions of space and they predicted the existence of massless particles, which do not exist in the hadron spectrum. In the period 1972-73, several of us tried very hard to modify the string theories so as to eliminate the extra dimensions and the massless particles. However, all such modifications destroyed the mathematical consistency of the theories.

The final nail was driven into the coffin of string theory in 1973-74, when quantum chromodynamics (QCD) emerged as a theory of the strong nuclear force. Its successes were immediate and convincing. String theory, a very active area of research for about five years, dried up practically overnight.

In 1972 I had moved to Caltech, and in 1974 I arranged a 6-month visit by Joël Scherk, a French physicist with whom I had worked earlier in Princeton. Both of us felt strongly that string theory was too beautiful a mathematical structure to be completely irrelevant to nature. We were convinced of the essential correctness of QCD, but we still thought that string theory deserved a last look before being abandoned. Soon we realized that its defects could be turned into virtues by using it for a purpose that is completely different from the one for which it was originally developed.

Massless particles do occur in nature: The quanta of light (called photons) and the quanta of gravity (called gravitons) are examples. These particles are not hadrons, however. Indeed all consistent versions of the string theories that we knew about contain a massless particle with exactly the properties of the graviton. By invoking prior results of Weinberg, we were able to show that the interactions of string theory gravitons at low energy agree precisely with those determined by general relativity. This result was obtained independently by Tamiaka Yoneya. Also, we drew attention to the work of Kaluza and Klein in the 1920s showing that extra dimensions of space can play a useful role in gravitational theories, where the geometry of spacetime is dynamical.

Since I was trained as a particle physicist, gravity was far from my mind in early 1974. Traditionally, particle physicists were taught to ignore the gravitational force, which is entirely negligible compared to nuclear and electromagnetic forces under ordinary circumstances. For example, the gravitational attraction between an electron and a proton in a hydrogen atom is about 38 orders of magnitude weaker than the electric attraction.

The physics world was quite different in those days. Physicists who specialized in the study of gravity, often called relativists, generally studied the largest things in the universe (including the universe itself), and they had no use for particle physics. They attended different meetings, read different journals, and had no need for serious communication with particle physicists, just as particle physicists felt they had no need for galaxies, black holes and the early universe in their quest to understand nuclear forces and elementary particles.

For these reasons, even when Scherk and I realized that string theory had mathematical features suggestive of gravity, we were not predisposed to interpret it as a physical theory of gravity. Also, there was no community that was eager to hear about it. However, after a few weeks of intense deliberations, we were ready to take the plunge.

Thus, Scherk and I proposed reinterpreting string theory as a framework for a unified quantum theory of gravity and the other fundamental forces. This was a radical change in viewpoint that required, among other things, supposing that the size of a string is roughly equal to the

Planck length ($10^{-33}$ cm) in order for the gravitational force to have the correct Newtonian strength. This is 20 orders of magnitude smaller that what was envisioned when strings were being used to describe hadrons, whose typical size is $10^{-13}$ cm.

In addition to incorporating gravity in a unified theory there was another bonus. All previous attempts to include gravity in the framework of quantum field theory had led to formulas plagued by meaningless infinities called "nonrenormalizable ultraviolet divergences." We knew that string theories have much "softer" shortdistance behavior than conventional quantum field theories, and we were confident that this problem would not occur. In short, we conjectured that string theory provides the correct framework for achieving a consistent quantum theory of gravity.

Scherk and I were very excited by the possibility that string theory could be the Holy Grail of unified field theory. In addition to publishing our work in scholarly journals, we gave numerous lectures at conferences and physics departments all over the world. We even submitted a paper to the 1975 essay competition of the Gravity Research Foundation, for which we received an honorable mention. For the most part our work was politely received – as far as I know, nobody accused us of being crackpots. Yet, for a decade, very few experts took the proposal seriously. Fortunately for me, Murray Gell-Mann believed that I deserved to be supported. Being an environmentalist, he was concerned about endangered species.

In 1979, Michael Green and I began collaborating on the further development of superstring theory. Each year we made discoveries that we felt would attract the interest of other physicists. This did not happen until after a discovery that we made in the summer of 1984 while attending the Aspen Center for Physics. We showed how certain apparent inconsistencies, called anomalies, are circumvented. The subject suddenly became very fashionable – one of the most active areas of research in theoretical physics.

In the 60 years since Einstein's quest for a unified theory, the physics community has learned to accept quantum mechanics, and it has developed very successful "standard models" of particle physics and cosmology. Before we can bridge the gap between these models, which are valid at low energy, and superstring theories, which are best understood at the Planck scale, we probably need to learn facts about nature at intermediate energy scales. I think it is unlikely that the correct solution will be found by pure thought. A more plausible scenario is that our experimental colleagues will discover supersymmetry at the LHC. Since supersymmetry arose out of string theory, this would be an enormously encouraging demonstration of its relevance. If that should happen, the development of a supersymmetric standard model would become a realistic goal. Whether or not such a new standard model would be sufficient to point the way to the Planck scale, it would at least constitute a big step in that direction.

In conclusion, the construction of the ultimate theory of all particles and forces based on superstring theory is still a distant dream. It can be frustrating when we are asked to make experimental predictions. Yet there are successes that we can point to. Our studies have led to very fruitful interactions with many areas of mathematics and physics. This is already compelling evidence that the string theory community is engaged in very important research. I feel very fortunate for having been able to participate in the development of this fascinating subject.