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Theory Bound and Unbound: Superstrings and Experiment[1]

1. Introduction: Theory Unbound

In mid-July 1986, Steven Weinberg stood before the International Conference on High Energy Physics, then meeting in Berkeley, and delivered what should have been welcome news: theory matched experiment to a T. In virtually every sector of experiment the collocation of theories known as the Standard Model matched the results of a generation of accelerator work from the Stanford Linear Accelerator Center in Palo Alto, Fermilab outside Chicago and the Brookhaven National Laboratory in New York; all the way through CERN bordering Geneva and the Deutsches Elektronen Synchrotron (DESY) near Hamburg to Novosibirsk in Siberia. One quark (of six) still was missing, but the search had by no means been exhausted. Challenges had come and gone: monojets (isolated pencil-thin bursts of particles) "shouldn't" have existed but they appeared at CERN, then disappeared back into the noise; massive neutrinos with no place in the model had been detected, then dropped; a new ultra-weak force might have been seen, but it looked increasingly dubious. Despite these "peculiarities," or perhaps because of their number and short half-life, theorists were growing increasingly inured to oddities in the data, less willing to plunge into new speculation. And while the strongly-interacting sector of the Standard Model could

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not be used to derive quantitatively the complexities of nuclear physics – for example – the theoretical community chalked that circumstance up to the known calculational tribulations of a theory poorly suited to the perturbative expansions used so successfully in quantum electrodynamics. If the goal of theoretical physics was the prediction, retrodiction, and quantitative account of stabilized, recognizably "relevant" phenomena, the Standard Model was a spectacular, indeed unrivalled success. Everyone "should" have been content. After decades of unexplained tracks in cloud chambers, bubble chambers, and nuclear emulsions, after countless thousands of nights staring at the photographic remnants of the subvisible and the data tapes from electronic experiments, theory, at last, had caught up with experiment.

Yet (Weinberg continued) in the years since the Standard Model's consolidation in 1974-75, a deep unease had settled over the theoretical community. But this was not the unease of being unable to account for experimental anomalies, new particles, interactions, or effects. No, what Weinberg reported was dissatisfaction of another sort: in what we might call *the intratheoretical* domain. Why, he and other colleagues had long demanded, were there so many, ("embarrassingly many") free parameters in the theory, such as the strength by which quarks and leptons were bound to the Higgs particle (the entity responsible for separating the electromagnetic from the weak force). Why do the particle masses have the values they do? Why is the symmetry of the theory just SU(3) x SU(2) x U(1) and none other? Why are there three versions of each of the exclusion principle-obeying particles – an electron, for example, is accompanied by exactly two heavier versions, the muon and an even heavier partner, the tau? After a decade of furious work on these and other intratheoretical lacunae, the list of proposed solutions had grown ever longer.

One proposed solution to some of these issues was known as technicolor (a scheme to extend, analogically, the dynamics of the newly articulated color force that explained nuclear particles as composites made of quarks). Technicolor modeled a composite Higgs on just this sort of scheme. Another explanatory scheme involved attributing a substructure to the quarks and leptons themselves. Yet another theoretical reassessment had been dubbed supersymmetry; it paired every known whole-spin particle with a postulated half-spin partner, and every known half-spin particle with a postulated whole-spin particle. An electron (half-spin) got a hypothesized whole-spin mate, the *selectron*; the whole spin photon got a half-spin partner, the *photino*. Grand Unified Theories (GUTs) were yet another scheme that would explain much of what the standard model had to postulate. In it the symmetries that had brought the weak and electromagnetic forces together would be extended to embrace the strong (color) force as well. "You know," Weinberg lamented, "just listening to myself reciting that list, I feel a sense of tremendous frustration. We've been working on these ideas [the theoretical "loose ends"] for more than a decade, since the mid-1970s, and we have almost nothing to show for it in terms of hard agreement between predictions and experiments." (Weinberg, [1987a], p. 271). No theoretical mechanism, that is, that would first explain why the parameters of the Standard Model were what they were, why there were precisely three generations, and so on, then go further to make a concrete, confirmed, and novel experimental prediction. (The one exception to this being one parameter predicted by grand unified theories.) Despite this gloomy assessment, Weinberg went on to fly the banner of a theory that offered new hope – even while it lacked any confirmed experimental test: "superstrings."

Indeed, in the months preceding Weinberg's summary talk, and almost exactly a decade after the events of November 1974 that heralded the "gauge revolution" that had ushered in the Standard Model, theoretical physicists had begun speaking of a "string revolution." Launched in August 1984 (Gross, [1989], p. 311), this was not a revolution that took hold by predicting a raft of new entities; it was not remotely like the heady days of the mid-1970s in which theorists hung on each latest experimental development at the Stanford Linear Accelerator Center, the Brookhaven National Laboratory or the Deutsches Elektronensynchrotron to interpret data and suggest new regions in which to search. Perhaps just because the events of 1984 and what followed were so *unlike* the recent experience of gauge physics, advocates and detractors alike began to invoke history to ground their claims.

According to some, the introduction of strings was unlike Dirac's introduction of his relativistic equation, others contended that string theory was similar to Einstein's discovery of general relativity, and yet others likened string theory to the fruitless quest Einstein had undertaken for a Unified Field Theory – and these were but a few of the analogies and counter-analogies drawn on each side. Never before had the history of physics been invoked so liberally in defense of current research. While to some, theory had come unbound from its necessary and historical moorings in the details of experiment, to others theory had finally arrived at its telos. At long last, advocates asserted, theorists could derive the foundation of physics largely on the basis of mathematical self-consistency alone.

My concern here is to explore how it was possible for Weinberg, at the end of his 1986 Berkeley address, to embrace string theory as a fundamental account of all known physics, a "final theory," *and yet* in the same breath, aver that "the phenomenological success of superstring theory is not part of its justification so far." (Weinberg [1987a], p. 280). Baldly put, the superstring theorists had a final theory of nature that had little traffic with experiment beyond the existence of gravity and the handedness of certain particles. It is this combination of ambition and detachment that would have been so unimaginable in almost any other time in the nineteenth or twentieth century, and that commands our attention. For what the late twentieth-century is witnessing in the string controversy is a profound and contested shift in the position of theory in physics.

For decades, perhaps centuries, theorists and experimentalists have viewed theory as irreducibly constrained by quite immediate features of experimental results – it was always possible to make vastly more theories than were consistent with laboratory results, and news from the benchtop or accelerator floor halted, guided, and confined theoretical work. That constraint structure has now shifted. String theorists are not, in the first instance, looking to the daily produce of laboratories for the elimination, suggestion, or constraint of theories. Instead, string theory has turned to mathematics, not simply to the long-established domains of Lie algebras, Fourier transforms, and the linear algebra of quantum mechanics and gauge theory, but to a vast array of mathematical techniques and disciplines some of which are being worked out simultaneously by physicists and mathematicians. In this process the disciplinary and social location of theoretical physics has been, often painfully, displaced.

Closely tied to this shift in disciplinary location is a philosophical displacement in the character of realism as defended by practitioners. In the past a standard defense of the realistic character of physical law invoked the sense that experiment matched theory in ways and to a degree that lay outside the theorist's control. Now, with string theory, that effect behind realism, the sense that a selection takes place outside the domain of theory, has been shifted away from the specifics of masses and lifetimes. Instead, the broad features of physical theory serve to provide certain constraints on any theory, but even more importantly is the role of the much more abstract notion of consistency. Weinberg: "the real reason so far for being interested in superstring theories is that they are mathematically consistent[,] finite ... relativistic quantum theories, of which there are pretty few, and these are the only ones we know that contain gravity." (Weinberg, [1987a], p. 280).

Weinberg may have been a forceful spokesperson for the superstring revolution, but he was certainly not alone. In theoretical physics groups across the United States, Europe, and the fragmenting Soviet Union, the race for string theory shot forward after 1984. Behind the enthusiasm lay a view of nature as a unified whole with underlying principles that we can grasp largely through mathematics. Opposing this view lay another image of nature: nature as always surprising, nature marked by pockets of order hard won from the intense study of experimental work. On the pro-string view experiment, while needed, could wait; the order of the world would reveal itself through that single theory that will pass through the narrow gates of consistency. On the antistring view, experiment was necessary not down the road but right now; without experiment, theory would wander aimlessly.

To understand this divide is to glimpse the changing role of theory in the physics of the late twentieth century.

2. Superstrings: The Final Theory

Behind the idea of string theory is this conviction: to unify gravitational, nuclear, and electromagnetic physics, it is necessary to move beyond a world of point particles and quantum field theory. Instead of structureless point particles as basic, string theory has one-dimensional objects (strings) held under enormous tension – some 10^{39} tons – with vibratory states that correspond to the different elementary particles. These tiny strings are to be on the order of 10^{-33} cm long (some 10^{20} smaller than a proton). Just as point particles arc out one-dimensional "worldlines" in space-time, so these strings carve out two-dimensional "worldsheets." A closed string, for example, would trace the surface of a distorted cylinder in spacetime; the coming together in spacetime of two such objects would construct a surface something like that of a pair of pants. These and related geometrical objects open onto vast areas of modern mathematics – Riemann surfaces, algebraic topology, and even number theory.

The string debates revolve around an agreement and a profound disagreement. By consensus, theorists agree that theory must operate under a series of constraints, and that those constraints must confine theoretical work to a very few, if not a single representation of some sector of the world. The dispute is over the source and appropriateness of those constraints. To particle theorists like Howard Georgi, Sheldon Glashow and others, the constraints must come, in large measure, from the nitty-gritty of experiment. To string theorists like David Gross, Edward Witten, John Schwarz and Michael Green, such experimental constraints might be desirable, even possible in the long run, but in the here and now, theory must look to its own set of constraints, those provided by the most basic features of the world combined with powerful mathematical and aesthetic constraints. These, string advocates insist, can or at least might isolate a single "theory of everything."

String theory had its origin far from unified theories. For during the 1950s and 1960s literally millions of bubble chamber photographs had flooded theorists with new particles, resonances, and lifetimes. Among the many theoretical responses to the deluge was a series of empirical formulae and conjectures that led in the late 1960s and early 1970s to a picture of the hadrons (strongly interacting particles) predicated on quarks bound together by one-dimensional strings, elastic bands on the order of 10^{-13}cm. Vibrations of these strings became the different resonance of, for example, the pion. Philosophically, the early string program issued from the attempt to discuss the strong interactions in the light of the general properties of scattering processes – rather than the detailed mechanisms modeled on quantum electrodynamics sought by quantum field theorists. These early string results, designed to account for even-half-spin particles (bosons) were then expanded in 1971 to include a string account of odd-half-spin (fermion) particles like the proton.[2] This program came to a screeching halt in 1973-74, with the extraordinary success of gauge physics, both in theory (in particular the discovery that under certain conditions quarks inside nucleons acted free of one another) and experimental success (the discovery of neutral currents and shortly thereafter the J/psi and other charmed particles).[3] In addition to the success of the competition, string theories of nuclear particles had their own, internal failings: inter alia, they predicted the existence of a new massless spin-2 particle that was nowhere to be seen in the hadronic world.

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2 String models of hadrons are reviewed in Scherk [1975]; many of the original string papers are reprinted in Schwarz [1985].

3 On the discovery of neutral currents, see Galison, 1987, chapters 4-6.

Unwilling to part with the remarkable mathematical-physical structure of string theories, Schwarz and the young French physicist, Joël Scherk, brought their creation back to life later in 1974 by changing the scale of the theory, and by reinterpreting the misbegotten spin-2 particle as the quantum of gravitational force, the graviton. Quite independently, T. Yoneya came to the same conclusion. (Yoneya [1974]). This quite radically reformulated the meaning of the formalism – overnight strings went from lying at the interior of hadrons and stretching across a proton diameter (10^{-13}cm) to being objects at the Planck length: a scale one gets by combining the fundamental constants of relativity, quantum mechanics, and gravity, $L_P = (hG/c^2)^{1/2} = 10^{-33}$cm. By moving their physics up in energy by a factor of twenty powers of ten, Schwarz and Scherk advanced on two goals: 1) they hoped to save their hadron theory by finding work for the otherwise unemployed spin-2 massless particle, and 2) they hoped to save quantum gravity by employing string theory to make a finite theory of quantum gravity. Strings were no longer a form of intrahadronic glue, they were now the defining fabric of spacetime itself. (Scherk and Schwarz [1974]). Few listened. For many of the next ten years, the development of string theory was slow, involved only a handful of people, and had a highly restricted audience, even among theoretical particle physicists. First, a few authors began exploring the possibility that string theories might be made supersymmetric, that is, that the theories might contain a symmetry that postulated a fermion for every boson. Such theoretical objects were known as superstrings. By itself, the constraint of supersymmetry does not tightly limit the construction of string theories, but there were other considerations as well. (See figure 1.)

In 1983, at the Fourth Conference on Grand Unification, Edward Witten [1983] spoke persuasively about the promise of the new theories, and highlighted a further set of constraints: the mixed gauge and gravitational superstring anomalies had to be eliminated. Quantum field theorists had, of course, long known about other kinds of anomalies: terms generated when the classical version of a theory has a certain symmetry, but where that

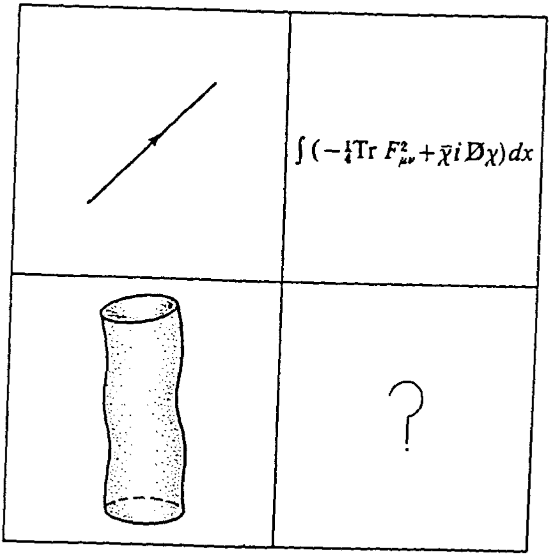

Figure 1. Superstring's Magic Square. In the upper left hand corner, Green, Schwarz, and Witten have placed the worldline (in spacetime) of a point particle. Made supersymmetric (every exclusion-principle-obeying particle has a non-exclusion-principle- obeying partner), the equation for a point particle sits in the upper right. In the bottom left is the world sheet of a closed string – a deformed cylinder in spacetime. Needed, Green, Schwarz, and Witten insisted in 1987, was a full theory, grounded on a cleanly stated physical principle that would combine string theory, supersymmetry and Einstein's general theory of relativity. These physical principles would then ground the mathematical successes of string theory as it was known now.
Source: Green, Schwarz, and Witten [1987], p. 27.

symmetry was violated by quantum corrections. The danger of anomalies lies in the symmetries they destroy. Gauge theories, for example, have certain symmetries: in the simplest case, if one takes every quantum field and modifies each wave function by an arbitrary phase at each point in spacetime, the theory remains unchanged. More generally, the quantum fields can be altered in more complex ways, for example by mathematical operators that form a group, but where the order of application matters (simple phase changes, by contrast, can be made in arbitrary order).[4] Such non-commuting symmetries can be vitally important; gauge symmetries assure the renormalizability of the theory, and without renormalizability, a theory is hardly a theory at all. (In a nonrenormalizabile theory, quantum calculations lead to infinite quantities, and the theory loses its predictive capacity.) In the case of the standard model, anomalies break the gauge symmetry of the theory, spoil its renormalizability, and so render it inconsistent.

Cancelling the gauge anomalies in the Standard Model became an absolute necessity to salvage the consistency of the theory. It turned out that for the anomalies to cancel, there had to be an equal number of types of leptons (such as electrons, neutrinos, or muons) and types of quarks (such as the up quark, the down quark, the strange quark, and the bottom quark). In this way, the constraint of renormalizability led historically to a powerful prediction: given the six known leptons, there had to be sixth quark.

Similarly, superstring theory can be constrained by the condition that the dreaded anomalies must be cancelled. For example, for the theory to be consistent, it must be possible to relabel the points on the world sheet of a string without changing the physics. This symmetry, known as conformal invariance, has an anomaly – that is, even if the theory itself has confor-

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4 Symmetry groups can be classified exhaustively. A simple phase symmetry is dubbed the unitary group U(1) – it is this symmetry that characterizes electromagnetism. Rotations in two dimensions are known as the orthogonal group, O(2), and higher-dimensional extensions of the rotation group follow the nomenclature O(3), O(4), ... O(n). Another infinite series of order-dependent (non-commuting) groups find a two-dimensional representation in the complex two-by-two Pauli spin matrices of nonrelativistic quantum mechanics. Known as the special unitary group, these are designated SU(2), SU(3), ... SU(n). Combined symmetries such as the SU(2)xU(1) symmetry of the electroweak interaction play a central role in the last 25 years of unified field theories. In addition to these infinite series of groups, there are particular symmetry groups which include the exceptional group, E_6 that play an important role in grand unified theories joining the strong and electroweak forces, and E_8, which has particular importance in the string theories.

mal symmetry, quantum mechanical corrections threaten to destroy it. In the nonsupersymmetric string theory, cancellation of this conformal anomaly meant that the background space (in which the strings live) has to have precisely 26 dimensions; in the supersymmetric case, a similar cancellation of the conformal anomaly forced the background spacetime to have 10 dimensions (nine spatial, one temporal). Contemplating these "constraints on possible string theories," Michael Green remarked, "It is in this sense that superstring theory is immensely elegant." (Green [1986], p. 55).

Consider the aesthetic virtue of the conformal symmetry constraint a bit further by contrasting strings with point particles. In the quantum field theory of point particles, as quintessentially exhibited by quantum electrodynamics, the theory is typically organized into a perturbation expansion in powers of the electric charge, e. For any given process – such as the scattering of a positron from an electron, certain terms in the perturbation expansion are relevant; corresponding to each such term is a Feynman diagram which (in this case) has a positron worldline and an electron line coming in and one of each coming out. Between the arrival and departure of the electron and the positron, anything that can happen (is allowed by conservation laws to happen) will happen. The electron and proton can annihilate and form a virtual photon, the virtual photon can then disintegrate into a positron and an electron. Or the same thing might happen except the virtual photon might spend part of its ephemeral life as another positron and electron along the way. As the number of particles involved increases, or as the order of perturbation theory increases, the number and complexity of these diagrams rapidly increases. Nowhere is the challenge of such diagrammatic analysis so well illustrated as in the work of T. Kinoshita, the acknowledged master of higher-order corrections to the muon and electron magnetic moments. In these calculations, which lead to the ten-significant figure predictions that have been matched to experiment, twentieth-century science, finds one of its most astonishing results. But the cost in complexity is astonishing. In 1982,

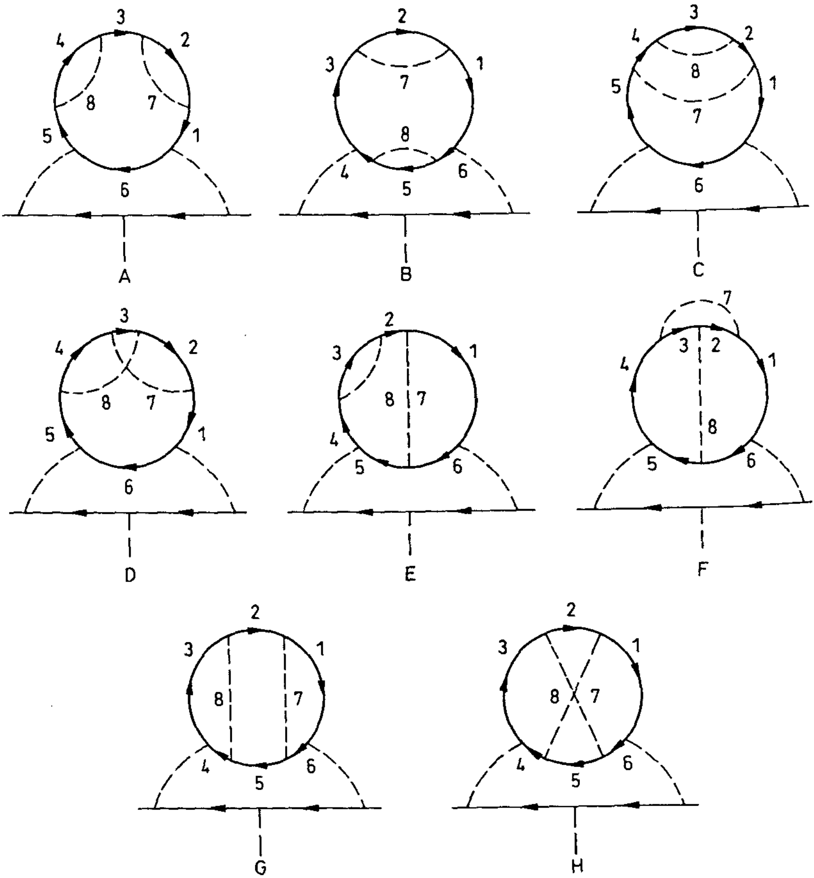
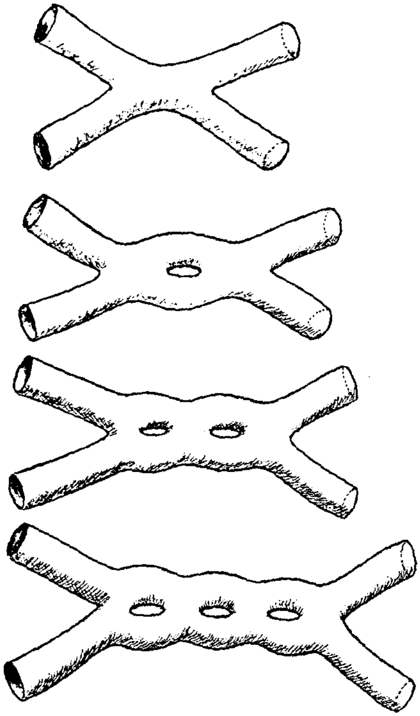


Figure 2. Kinoshita's Feynman diagrams. Kinoshita and Lindquist [1983], p. 868.

Kinoshita published eighth-order corrections to the muon magnetic moment – and had to face 891 diagrams, each distinct from the other. (See figure 2).

In string theories, the world line of a point particle is, as noted above, replaced by a worldsheet. For closed strings, the worldsheet forms a cylinder, and two strings in interaction form something like figure 3a. Conformal symmetry, the mapping of a complex point z into f(z), can take any topologically equivalent shape into the shape of figure 3a. Only surfaces with (for ex-



Figures 3a-3d. Feynman diagrams for two closed strings scattering into two closed strings

ample) a hole in the middle of the shape, such as figure 3b, cannot be so altered. As it turns out, for important classes of string theories the perturbation expansion of the string theory corresponds precisely to the series of such doughnuts with ever more holes, figures 3a-3d. Kinoshita's 891 diagrams become one. So it was that mathematics – as here with the topological and algebraic properties of two-dimensional Riemann surfaces – radically reshaped the constraints, simplicity, and as Green put it, the "elegance" of physics.

Witten in particular wanted to know whether the combined symmetries of gauge theory and gravitational interaction would lead to anomalies.

Soon, Witten, working with a younger collaborator, Luis Alvarez-Gaumé, could demonstrate that the dangerous version of the anomalies, the "hexagon anomaly" canceled in one version of the theory. (Alvarez-Gaumé and Witten [1983]). Though this was a calculational tour-de-force, became widely known as one of the "miracles" of string theory, and provided a new theoretical technology for the string theorists, the particular model Witten and Alvarez-Gaumé explored was not phenomenologically compatible with the particles of the standard model.

Anomalies played one final role in setting the constraint structure of any possible string theory. It was a long-established feature of "low-energy" particle physics (low compared with the energy of string theory) that many particles came either left- or right- handed. This "chirality" can be illustrated by the neutrino, which only occurs in the "left-handed" form. That is, the angular momentum of a neutrino is always left-handed with respect to the neutrino's linear momentum. If we were to look at the relation of the angular momentum of a neutrino to the momentum of that neutrino in a mirror, however, we would have a "right-handed" neutrino, an object not to be found in nature. So any theory of particles must be preferentially left- or right-handed in certain respects. Since even space dimensions (e.g. 2-dimensional space, 4-dimensional space, etc.) are always reflection invariant, the number of space dimensions had to be odd. But even in theories with odd spatial dimensions, quantum corrections frequently lead to anomalies in chiral theories, and these anomalies can destroy crucial symmetries. Disastrously for superstrings, some theorists believed in the early 1980s that it was impossible in 10 dimensions to have a chiral superstring theory. (Green, [1986], p. 56-57). Since superstrings had to live in 10-dimensional space, the constraints seemed to leave no exit. Then came the string revolution.

It was in August 1984 that Schwarz and Green, building on the work by Witten and Alvarez-Gaumé, showed how to get a chiral, anomaly-free theory using the specific gauge symmetries, SO(32) and $E_8 \times E_8$. For the former, they could actually produce a full string theory. (Green and Schwarz [1985a, 1985b]). Their efforts were followed shortly by those of the

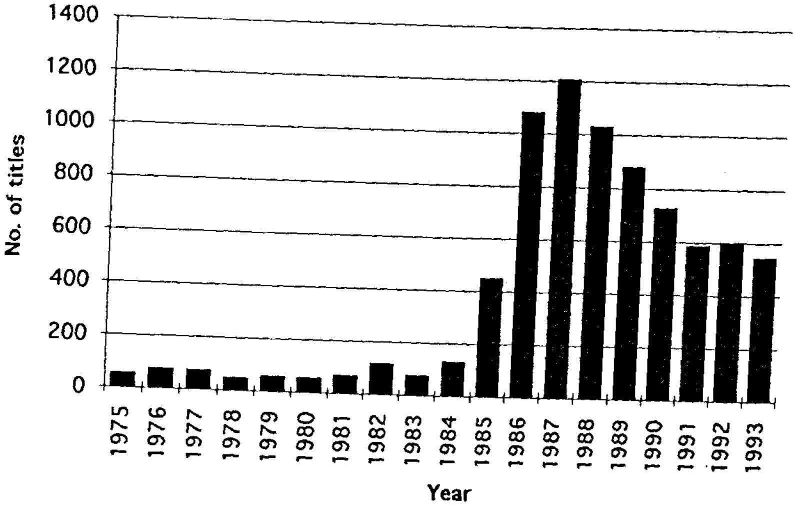


Figure 4. Number of articles on strings and superstrings, 1974-1993.

"Princeton string quartet," David Gross, Jeffrey Harvey, Emil Martinec, and Ryan Rohm who, (in [1985]), constructed a hybrid variant of string theory that went with E_8 x E_8. Theirs was a mixture of two closed string theories, one was the older bosonic string in 26 dimensions and the other was the newer superstring theory in ten dimensions. Dubbed "heterotic" after the strength of mixed biological species, the new string theory promised phenomenological success along with theoretical value. Hopes ran high as the group E_8 clearly contained as a subgroup the symmetry associated with one of the known grand unified theories. The quartet concluded that "Although much work remains to be done there seem to be no insuperable obstacles to deriving all of known physics from the E_8 x E_8 heterotic string." (Gross, Harvey, Martinec, and Rohm [1985], p. 283). Or again, as Schwarz [1985] put it a bit later that same year, "Superstring theory – or, more specifically, the E_8 x E_8 heterotic superstring – could be a 'theory of everything' (p. v)."

Reaction among theorists was extraordinary, as figure 4 indicates.

The steady stream of string and superstring articles from 1974 to 1984, was followed by an explosive growth – up to 1,200 articles in 1985 – followed by a gentle decline to a roughly steady-state number of 500-600 articles per year in the period following the self-proclaimed "revolution."

Now there were only a handful of possible string theories, but for some theorists even this handful was too much. One problem was that there might be a multitude of compactification schemes – theoretically driven compression mechanisms that would compress the 10 or 26 dimensions of the full superstring theory into the four spacetime dimensions that we observe. As Schwarz [1985] wrote, anomaly conditions, perhaps at the two-loop level, might winnow the race. "Additional conditions, such as these might provide, are hoped for since five theories, each with many possible compactifications, is more than we really desire. It is conceivable that the correct theory is the only one possible" (pp. 942-943). And elsewhere, in 1987: "Three theories are known, but it would be extremely nice if the number could be reduced to one. Then we could argue that there is a unique consistent theory that accounts for all of fundamental physics." (Schwarz, [1987b], p. 40).

This search for uniqueness and what Weinberg called "rigidity" is the attribute of a theory that constraints fully secure. Supersymmetry by itself would not do the job; indeed, the wide variety of mixed gravitational and particle theories had so far been available in many variants, none of which were uniquely determined, and none of which were completely consistent. Ordinary Lorentz invariance, taken by itself, provided Weinberg with an example of an incompletely rigid constraint: it is not difficult to combine vectors, axial vectors, and tensors to form a wide variety of relativistically invariant theories, an infinite number of which have strictly nothing to do with the physical world. Superstring theories, Weinberg insisted in October 1985, are different: they are "rigid." There are, he added, "almost no string theories at all." A free field string theory has interactions that only arise from the topology of two-dimensional manifolds – and these are specified uniquely by the simplest of properties: the number of handles that you can put on them. "[S]o there is nothing you can tinker with in these theories; they are either right or wrong as they stand." (Weinberg [1986], pp. 236-237). Superstring theory, he continued a few months later, "has the smell of inevitability about it." In superstrings, according to Weinberg, we find "a theory that cannot be altered without messing it up; that is, if you try to do anything to this theory, add additional terms, change any of the ways that you define things, then you find you lose these symmetries, and if you lose these symmetries the theory makes no sense. The sums over histories, the sums over surfaces break down and give nonsensical results. For this reason, quite apart from the fact, string theory incorporates gravitation, we think that we have more reason for optimism now about approaching the final laws of nature than we have had for some time." (Weinberg, [1987b], p. 105).

In the measure that these constraints – anomaly freedom, GUT compatibility, unitarity, chirality, and finiteness – exclude all but a single account of the world, superstring theorists were inclined to adopt that account, *in virtue of its unique consistency over and above phenomenological success.* As they compiled their two-volume textbook on strings in 1987, Green, Schwarz and Witten [1987] made this quite plain. "Quantum gravity," they argued,

> has always been a theorist's puzzle *par excellence*. Experiment offers little guidance except for the bare fact that both quantum mechanics and gravity do play a role in natural law. The characteristic mass scale in quantum gravity is the Planck mass [10^{19} GeV]. This is so far out of experimental reach that barring an unforeseeable stroke of good luck (like the discovery of a stable Planck particle left over from the big bang) we can hardly hope for direct experimental tests of a theory of quantum gravity. The real hope for testing quantum gravity has always been that in the course of learning how to make a consistent theory of quantum gravity one might learn how gravity must be unified with other forces. (p. 14).

Mathematics has a new role here. For the claim made for this mathematized physics was not simply that mathematics rendered physics more rigorous, as Laurent Schwarz's work on distributions had rigorously defined the Dirac delta function. Nor was the claim by string theorists that they had used mathematics to rid the discipline of an objectional concept the way Lagrange or Hamilton had cut "force" from Newton's mechanics. Reflecting in 1986 on the two years since the revolution, John Schwarz called this period "a new era in theoretical physics, quite unlike anything before ... the beginning of an era of unification – not only of forces and particles, but of concepts and disciplines." Mathematics and physics had converged in their interests, and only the "loss of contact with our experimental colleagues and the real world" had to be avoided. (Schwarz, [1987], p. 201). As string theorists exploited this new admixture of algebraic topology, quantum field theory, and general relativity, they could "test" theories, until a single, inalterable and fundamental account of physical reality remained.

This kind of rigidity, Weinberg contended, was not *just* a matter of aesthetic or philosophical satisfaction. Having read the critical articles and comments made about superstrings by other prominent theorists, Weinberg [1987a] responded in 1987:

> I don't really understand these attacks. At present superstring theory provides our only hope of understanding physics at the Planck scale, the scale where gravity is important. Furthermore it is beautiful, it has a kind of rigidity that you look for in a kind of physical theory that will in the end turn out to have something to do with the real world (p. 282).

"[S]omething to do with the real world," for Weinberg, meant the very real possibility that "fundamental physics" the theory in terms of which all the other entities of science were ultimately built, might soon come to an end. (Galison [1983]).

Precisely the separation of theoretical concepts from experimental analogues has, for some physicists, broken the sense of "interpretation," that held concepts to correspond to intuitions grounded in experience. Application, however, was not what Weinberg had in mind; it was not by deriving specific results from the lower energy theory that the concepts of string theory would find their place. In one interview, the physicist P. C. W. Davies asked Weinberg how superstring theory would describe the electron and the neutrino. Weinberg responded: "I find your question an awkward one. It's like asking 'how in general relativity do you work out the shape of a suspension bridge?" (Davies and Brown [1988], p. 216). It is not in girders that the curvature tensor of spacetime finds its sense. And it was not in reinterpreting experimentally accessible objects like the electron that Weinberg found the meaning of superstrings.

But beyond the question of applicability – the inability of a high-level theory to reproduce the results of a lower level phenomena – there is the question of how to interpret the concepts used. Could we give a physical interpretation to the variables beyond the four that correspond to ordinary spacetime? Weinberg's answer:

> I think not. The final theory is going to be what it is because it's mathematically consistent. Then the physical interpretation will come only when you solve the theory and see what it predicts for physics at accessible energies. This is physics in a realm which is not directly accessible to experiment, and the guiding principle can't be physical intuition because we don't have any intuition for dealing with that scale. The theory has to be conditioned by mathematical consistency. (Davies and Brown [1988], p. 221).

On Weinberg's view, the removal of the theory from proximity to experiment was therefore not simply a statement about how we ought proceed, strategically, in building theory. The energy chasm between the two branches of physics removed the intuition of experimental entities from our understanding of the basic entities: superstrings.

I end this brief excursion into string theory with some remarks by Princeton's David Gross, who chose an exploration on foot as the basis for his extended metaphor about the relation of theory to experiment. It was a relation far different from the close cooperation that had marked the mid-1970s. "One of the important tasks of theorists is to accompany our experimental friends down the road of discovery; walk hand in hand with them, trying to figure out new phenomena and suggesting new things that they might explore." (Zichichi [1990], p. 237). Burnt into Gross's memory, indeed into the collective memory both of pro- and antistring theorists, was the example of the J/psi and other "charm" particle discoveries of November 1974 and its aftermath. For during those frenetic days experimentalists tossed new particles into the ring and theorists worked furiously to explain them; theorists postulated new particles, new effects, and new theories – experimentalists responded with tests that could be prosecuted within months, sometimes in days. Now, in the late 1980s, this close relation had been broken – not very many new experimental results were coming out of the accelerators, and the discoveries that were being reported had a wickedly short life: the neutrino oscillations (indicating that the neutrino might have a mass) came and went, proton decays were reported and retracted, monojets spurted momentarily from CERN, then vanished, the fifth force grabbed attention for a while and then faded. Under these circumstances some theorists – Gross included – were less and less inclined to theorize furiously after each new sighting. These were no longer the days of new charmed particles and the myriad of discoveries that followed the "November Revolution" launched by SLAC and Brookhaven/MIT in 1974. Looking back in 1989, Gross continued his metaphorical trek:

> It used to be that as we were climbing the mountain of nature the experimentalists would lead the way. We lazy theorists would lag behind. Every once in a while they would kick down an experimental stone which would bounce off our heads. Eventually we would get the idea and we would follow the path that was broken by the experimentalists. Once we joined our friends we would explain to them what the view was and how they got there. That was the old and easy way (at least for theorists) to climb the mountain. We all long for the return of those days. But now we theorists might have to take the lead. This is a much more lonely enterprise. (p. 328).

Without knowing the location of the summit, or how far it was, the theorists could promise little by way of reassurance to themselves or to the experimentalists. In the meantime, experimentalists were not only left behind, they were left out altogether.

3. Antistrings: Keeping Physics Between Chemistry and Metaphysics

Not surprisingly, many experimentalists were shocked, not so much by the idea of theorists leading the way up an uncertain trek up an uncharted mountain, but because they did not see how they could even gain a toe-hold in the foothills. Carlo Rubbia, who only a few years before had taken home a Nobel Prize for his team's discovery of the intermediate vector bosons, the W and the Z, sadly reflected:

> I am afraid I am one of the few experimentalists here. In fact, I can see we are really getting fewer and fewer. I feel like an endangered species in the middle of this theoretical orgy. I am truly amazed. The theories are inventing particle after particle and now for every particle we have there is a particle we do not have, and of course we are supposed to find them. It is like living in a house where half the walls are missing and the floor only half finished ... (Zichichi [1990], p. 232).

After the bruising W and Z search, and a contentious struggle with the top quark, Rubbia had little appetite for an unknown zoo of particles as numerous as the known. Even one or two particles were terribly hard to find – Rubbia's UA1 collaboration had employed some 150 physicists for years at a cost of hundreds of millions of dollars to find the W and Z. Now the theorists were ordering a supersymmetric partner of every known entity: the selectron, partner to the electron, and so on all the way down the line.

Not only was this half-missing world overwhelming in its mandate for experimental discovery, the very motivations cited by the theorists had moved ever further from the accelerator floor. Gosta Ekspong, a senior European experimentalist who often worked at CERN, looked down on the podium itself on which was inscribed the Dirac equation, $(\delta + m)\psi = 0$. Reflecting on these spartan symbols he saw a line of poetry, "beautiful," "full of life and beauty," and above all open to "true predictions," including the existence of the positron, and to variances with experiment, in the subtleties of spectroscopy. Every theorist, he reflected, must aspire to the heights of Dirac's achievement, and in particular to infer truth from the presence of beauty. But cautioning his theorist colleagues, he argued that this poetic elision from beauty to truth often will not hold, and so hesitated before the purported aesthetic satisfactions of the superstring theorists:

> I would like to address the question of truth and beauty; truth being experiment, beauty being theory ... The problem is that the latest theories are so remote from experiment that they cannot even be tested. Therefore they don't play the same role as Dirac's equation ... I hope that this search for beauty does not drive theorists from experiments, because experiment has to be done at low energies, from one accelerator to the next and so on. Big jumps do not seem to be possible. (Zichichi [1990], p. 232).

For Ekspong, the path from superstring theory to experiment was too long, and if compactification multiplied its branches, the path was also too poorly defined to carry beauty the great distance to truth.

In the 1970s and 1980s, "theory" and "experiment" were categories (better: subcultures) with intermediate categories of phenomenologists straddling the fence. Ahmed Ali was one of these, having worked at both DESY (*Deutsches Elektronensynchrotron*) and CERN, and he invoked the idiom of the experimentalist, when he declaimed: "The present superstring theories are like letters of intent written by a lobby of theoretical physicists. They are very good in intent; but often what is said in the letter of intent and what is measured in the experiment are two very different things. The figure of merit of a theory is its predictive power which could be tested in an experiment in a laboratory." (Zichichi [1990], p. 232).

In a sense, the discomfiture of experimentalists, and those working hand in glove with them, could be expected. New techniques in theory had left experimentalists ill at ease with gauge theories in their early stages, though by 1974, experimentalists had found the gauge theories suggestive of a wide range of predictions, tests, and new directions for empirical work. Now the case of superstrings seemed much worse; there was no clear avenue for the accelerator laboratory to follow, and the theories themselves offered precious little to hold on to in the way of physically "intuitable" entities. Less expected, perhaps, at least to an outsider, was the vehement reaction against string theory from *within* the theoretical high-energy physics community.

I now turn to the most considered part of the opposition, an opposition grounded not on a hostility to wide-ranging claims about unification, nor on an opposition to theoretical particle physics. Quite the contrary, Georgi and Glashow were as central to the gauge revolution of the 1970s as anyone. No, the dispute centered on something much deeper, on a vision of what physics should be.

Before 1984, the *annum mirabilis* of strings, Howard Georgi opened the 1983 Fourth Workshop on Grand Unification with a transparency of a recent advertisement he had spotted:

HELP WANTED
Young Particle Theorist
to work on
Lattice Gauge theories
Supergravity
and
Kaluza-Klein Theories

Here, Georgi asserted, was a telling sign of the times, a position caught between "chemistry" (particle physicist nomenclature for a calculational activity in which fundamental principles were no longer at stake) and "metaphysics and mathematics" (activities disconnected from any contact with experiment) (p. 3). Superstrings had not yet emerged but the problem had. These highly mathematized theories did not fix experimental consequences – by contrast, the SU(5) GUT had fixed the parameter relating the strength of electromagnetism to the weak nuclear force, $sin\theta_W$.

The next years polarized the situation further. In 1986, Paul Ginsparg (who had himself contributed a computer analysis to the Alvarez-Gaumé and Witten no-anomaly demonstration superstrings in 1983), collaborated with his Harvard colleague Sheldon Glashow to bemoan the loss in superstrings of a productive tension between experiment and theory:

In lieu of the traditional confrontation between theory and experiment, superstring theorists pursue an inner harmony where elegance, uniqueness and beauty define truth. The theory depends for its existence upon magical coincidences, miraculous cancellations and relations among seemingly unrelated (and possibly undiscovered) fields of mathematics. Are these properties reasons to accept the reality of superstrings? Do mathematics and aesthetics supplant and transcend mere experiment? Will the mundane phenomenological problems that we know as physics simply come out in the wash in some distant tomorrow? Is further experimental endeavor not only difficult and expensive but unnecessary and irrelevant? (Ginsparg [1986], p. 7).

This was an altogether different view than that which Glashow had taken in the euphoric moments after he and Georgi had produced the first grand unified theories. GUTs, at least the SU(5) and SO (10) versions, did have very high energies – only a few orders of magnitude less than the Planck scale. And like the string theories, GUTs too forecast a "desert" in which no new experimental results could be found. But differentiating GUTs and superstrings were several crucial items. GUTs forecast a crucial parameter in the electroweak theory – $sin\theta_W$; at least SU(5) and SO(10) predicted a decay of the proton that should be measurable in deep mine experiments; and by construction the new grand unified theories reproduced all of the known phenomenology of both the electroweak and quantum chromodynamic theories. Strings could do neither, that is they could not make new predictions (such as $sin\theta_W$) and they could not reproduce the known phenomenology of SU(2)xU(1) and SU(3). Finally, GUTs, at least in their original formulation, simply continued the gauge program as it had already been formulated. That is, grand unified theories were "conservative" in the sense that they operated under essentially the same theoretical constraints (those of renormalizable gauge theories) as the rules of the game for SU(2)x U(1) and color SU(3).

In fact, by 1989, for Georgi the proliferation of GUTs, especially their assimilation into even larger unification schemes of the superstring, was the source of a mighty ambivalence:

I feel about the present state of GUTs as I imagine that Richard Nixon's parents might have felt had they been around during the final days of the Nixon administration. I am very proud that the grand unification idea has become so important. [But] I cannot help being very disturbed by the things which GUTs are doing now.

GUTs, he insisted, had been motivated by the desire to complete the unification of forces by accounting for the weak mixing angle and explaining the quantization of charge.

> They [GUTs] were certainly not an attempt to emulate Einstein and produce an elegant geometrical unification of all interactions including gravity, despite the parallels which have been drawn in the semipopular press. Einstein's attempts at unification were rearguard actions which ignored the real physics of quantum mechanical interactions between particles in the name of philosophical and mathematical elegance. (p. 446).

Imitating Einstein in the late 20th century was, Georgi judged, a losing proposition.

Georgi argued that it was the nature of physics itself at stake, not an incidental question of style, but the defining quality of the discipline. "Theorists," Georgi [1989] insisted, "are, after all, parasites. Without our experimental friends to do the real work, we might as well be mathematicians or philosophers. When the science is healthy, theoretical and experimental particle physics track along together, each reinforcing the other. These are the exciting times." (p. 452). His argument was that when experimentalists get ahead, as the bubble chamber experimentalists did in the 1960s, the discipline becomes eclectic, overrun with results without order or explanation. At other times theory gets ahead and the path is strewn with the irrelevant speculations of rationalization out of touch with reality.

Georgi's assessment of the state of theoretical physics is reflected in the practice of his physics, and in particular from the understanding he has advocated of effective field theories. Effective field theories (EFTs) have their roots in work by Weinberg and others quite some time ago. But over the last twenty years they have evolved into more than a specific tool, a way of thinking about physics that characterizes theoretical work by a generation of physicists whether they follow a phenomenological or a string theoretical path through the world. The idea is this: the mass of a particle and its wavelength are interconnected through the deBroglie relationship: the heavier the mass, the shorter the wavelength. And just as a surfer is concerned about waves on the scale of meters (not on the scale of millimeters), a physicist working at an energy scale corresponding to a wavelength of 10^{-13} cm need not be concerned with physics that takes place at the energy of 10^{-19} cm. This observation is formalized within the language of quantum field theory: in a given account of a quantum process – such as the scattering of particle A off of particle B – there will always be terms that involve the exchange of much more massive particles. For example, an electron scattering from another electron does so (in largest measure) through the exchange of a photon. But it is possible, quantum dynamically, for the electrons to exchange the much heavier particle Z^0 postulated in the Weinberg-Salam-Glashow theory, and experimentally defended in work culminating at CERN in 1983. Effective field theorists say yes, it is true that quantum electrodynamics left out the Z^0 in its classic formulation, but these additional Z^0 terms Z^0 will be suppressed by a factor of 1/M, where M is the mass of the Z^0. Should we discover new particles using bigger accelerators (RIP, SSC), these new particles would give rise to even smaller corrections because 1/M would be that much smaller.

Just because new particles of higher mass have a decreasing effect on the calculations made at lower energies, effective field theories can be thought of as a theoretical means of vouchsafing existing knowledge, a reason to take seriously quantum electrodynamics in its domain, come what may. It is this feature of epistemic insulation that has drawn the attention of Cao and Schweber [1993].

But for most physicists the retrospective justification of theories such as QED is not the main function of effective field theories; instead, their interest is in using the method to think about future theories. Suppose one wants to design an accelerator. In the case of the superconducting supercollider, most theoretical physicists were primarily interested in exploring the way in which the weak and electrodynamic forces separated. The unbuilt accelerator might have produced the Higgs particle, a leading contender for the symmetry breaking mechanism. Effective field theories told them that the physics of much higher energies would not play any immediate role in those dynamics, even if ultimately the Higgs particle could itself be explained by reference to theories grounded at yet higher energies.

> If we adopt the effective field theory point of view, we must try to work our way down to short distances from what we know at longer distances, working whenever possible in the effective theory which is *appropriate* to the scale we are studying. We should not try to guess the ultimate theory at infinitely small distances. Even if we could do it, it would probably be about as useful as explaining biology in the language of particle physics. (Georgi, [1989], p. 456).

This is an antireductionist argument. It addresses (as I understand it) the possibility that a Planck-scale string theory might be found that satisfies the theoretical constraints imposed on it. Georgi's fear is that even with such a theory, the compactification (that takes the many- dimensional theory into our accessible four-dimensional one) might remain unsolved, and therefore the gap between the Planck scale and the scale at which our accelerator-based particles live might remain so large that we would be uninformed about the accessible physics. We might have some reason not to rule out the high-energy theory, but we could not ever feel confident that the gap could be closed.

Now suppose, optimistically, that a nonperturbative string theory is found, that this theory dynamically determines the compactification, and that the resulting low-energy theory does match known phenomenology. Under such assumptions, Georgi [1989] was still not persuaded that other theories were ruled out, perhaps an infinite class of such theories might also produce what we know:

> [I]f we allow ourselves to be beguiled by the siren call of the 'ultimate' unification at distances so small that our experimental friends cannot help us, then we are in trouble, because we will lose that crucial process of pruning of irrelevant ideas which distinguishes physics from so many other less interesting human activities. (p. 457).

This is an underdetermination argument, one that I take to be distinct from the antireductionist one.

To develop his view that superstring theory is not necessarily unique, Georgi has often brought in different metaphors, one about the man who lost his keys one dark night and was seen hunting furiously beneath a street lamp. Asked whether he thought he lost them in just that circle of light, the despondent fellow said, no, but that was the only place he was ever to have a chance at finding them. Outside the circle of light cast, so to speak, by the energy regime around symmetry breaking of the weak and electromagnetic forces (around 1 TeV), Georgi saw physics as blundering in the dark – why search in one place, under one set of constraints, and not another? Especially in the energy region of the Planck mass (where we have no idea how physics will alter), why should anyone even suspect that string theory, developed for use in 1960s strong interaction physics, is relevant? For Georgi, the depth of our ignorance at the Planck scale is practically infinite. We know that at these energies ordinary relativistic quantum field theory breaks down; everyone agrees that is so. Under these circumstances, we have no grip on how to generalize the theory; according to Georgi, we do not even know how to formulate the problem, let alone the solution. It is his view that the uniqueness of superstring theory is an artifact of history; had the particular alternative of hadronic string theories not been available as the legacy of strong interaction theories, Georgi contended, superstring theory might well not exist in the 1980s. We see this theory as unique, but its uniqueness issues from our (perfectly understandable) delight in having *any* consistent extension of quantum field theory, not in the inevitability of this particular route beyond current knowledge. But (Georgi maintained) that was an historical accident deriving from a failed strong interaction theory resuscitated to fill a desperate need: string theory was unique within the frame of history, not *sub specie aeternitatis*.

To Weinberg, Georgi's history was flawed. Hadronic string theory ought not be seen merely as an abortive account of 1960s strong-interaction theory; it should rightly be thought of as an attempt to satisfy the constraints of analyticity, unitarity, Regge asymptotic behavior, and other symmetries demanded of any scattering process depicted by the S-matrix formalism. As such, string theory was (and remains) more abstract than a theory of matter; it is a characterization of features that must hold good for any theory. Perhaps, Weinberg suggested, superstring theory is best thought of as the unique way of satisfying the fundamental conditions imposed by scattering processes that include both gravity and familiar forces of particle interaction. On his view, these S-matrix constraints were still the relevant criteria for any scattering theory, including any theory embracing gravity. (Weinberg, [1986], p. 237).

While further exchange is not on the record, the question haunting the field remains: in the Planck limit, where we know "ordinary" quantum field theory breaks down, why choose to abandon just the constraint of theorizing with point particles? loosen this particular assumption and maintain intact the rest of the quantum field theoretical structure? Knowing that ordinary quantum field theories break down, how, in the absence of experiment, is one, among all the assumptions of field theory, to choose rightly?

Precisely this absence of connection to the "everyday accelerator physics" (if you will pardon this oxymoronic construction) set strings apart, and now in a more extreme form than ever before. Ginsparg and Glashow [1986] continued in their May 1986 manifesto:

> For the first time since the Dark Ages, we can see how our noble search may end, with faith replacing science once again. Superstring sentiments eerily recall "arguments from design" for the existence of a supreme being. Was it only in jest that a leading string theorist suggested that "superstrings may prove as successful as God, Who has after all lasted for millennia and is still invoked in some quarters as a Theory of Nature?" (p. 7).

Julian Schwinger, himself one of the creators of quantum electrodynamics (and Glashow's mentor in the early 1960s), was likewise appalled by the hubris of the theory, and the concomitant abandonment of experimentation. "[P]hysics has been, until recently, an experimental subject, the theories also ought to have more contact with experiment. Or, to put it another way, theories should be more modest. They should try to make contact with what we know and to extend it a modest amount. Let us see if we can't avoid theories that attempt to be theories of everything and thereby announce that we have in our hands the theory of the whole Universe." (Schwinger, [1990], p. 236).

For Witten, the accusation that string theory ignored experiment was premature and historically unreasonable. After all, he argued in September of 1986, relativity and relativistic astrophysics offered a striking three-fold instance of a theory confirmed by experience in ways utterly unavailable at the time Einstein sent his field equations to *Annalen der Physik*. Neutron stars, proposed in the 1930s, were well within the known physics of the day. Weak in their emission of light, small in cross section, it seemed hopeless to expect their discovery. Who could have divined the future invention of radioastronomy. "[O]ne can be very close to the truth and still be caught unawares by a sudden and almost accidental discovery. Wheeler around 1960 guessed that a spinning (and slowing down) neutron star was powering the Crab nebula, but no one realized that this meant that the neutron star could be seen as a pulsar." (Witten, [1987], p. 77). Gravitational lenses formed a second example of a powerful confirmatory phenomenon proposed by Einstein in the 1930s but in no way expected to be a significant astrophysical tool in the 1970s and beyond. Finally, gravitational waves, known in the 1910s but seemingly unobservable for decades, now loom on the horizon as nearly detectable. "[T]echnology develops faster and farther than we think." Experiment and observation, Witten [1987] is arguing, proceed down a twisted path, and we should not prejudge what may be observed down the road on the basis of what appears discoverable now: "So," he concluded, "we should be optimists." (p. 77).

At the *Superworld I* conference, which took place as Witten prepared these remarks for publication, his collaborator on the anomaly paper, Luis Alvarez-Gaumé, presented Witten's gravitational example of a "hidden experiment." Glashow was unmoved: "I am familiar with that example from your prophet. He was talking about 1930. Let us remember that in 1932 the positron and the neutron were discovered so there were a few other things happening aside from waiting around for gravitational lenses. That is not so obviously true today." (Zichichi [1990], p. 239).

Now it might be thought that Georgi and Glashow, authors of the very project of a grand unified theory that sat a million times above the presently observable mass scale, were hardly in a position to criticize strings on the grounds of scale alone. From their perspective, however, the two cases were hardly analogous. What marked off the superstring from GUTs was precisely the absence of experimentation; "Only a continued influx of experimental ideas and data can allow the paths from top [that is Planck-scale, superstring theories] and bottom [that is experimentally grounded theorizing] to meet." (Ginsparg and Glashow [1986], p. 9). By 1988, Glashow [1988] judged that

> the historical connection between experimental physics and theory has been lost. Until the string people can interpret perceived properties of the real world, they simply are not doing physics. Should they be paid by universities and be permitted to pervert impressionable students? Will young Ph.D.'s whose expertise is limited to superstring theory be employable if and when the string snaps? Are string thoughts more appropriate to departments of mathematics or even to schools of divinity than to physics departments?" (p. 25).

Reiterating his discomfiture with the string theory in 1986, Glashow added an historical argument.

> The downwards path [from rational explanation of theory to phenomenology] hadn't worked very well. However elegant it would have been had planets moved in circles, they simply don't. The revolution of special relativity emerged from observation and not from the power of positive thinking. Without the careful experimental work that led Maxwell to his equations, no philosopher could have discovered the real nature of space and time. Quantum mechanics, too, crept out of an experimental morass which was irreconcilable with classical theory. It was not an abstract mathematical creation. (Glashow [1990], p. 250).

By 1990, to his dismay, Glashow reported that Princeton piety had spread to Harvard. (Zichichi [1990], p. 236).

Using history rather differently, Weinberg claimed that superstrings in the late 1980s, was like general relativity in the 1920s: Eddington and Pauli must have found Einstein's theory compelling on similar grounds, even though, like string theory, general relativity had the barest of confirmatory evidence.

That, at present, physicists were a "long way" from being able to provide the details of the nonperturbative theory or a quantitative account of phenomenology is understandable. After all, he noted, we are still "a long way from being able to understand turbulence." (Weinberg, [1987a], p. 282). Historical examples flew back and forth across the divide. To the string theorists, patience was needed; Einstein's theory of general relativity had scant support in its original formulation, the precession of the perihelion of mercury and not much else. Arrayed against the Einstein analogy were the myriad of dead ends that theory had pursued without their experimental colleagues. As Gross acknowledged in [1989], "The arguments against success are easy – history teaches us that without direct experimental clues and tests theorists tend to go wrong." But in favor lay the extraordinarily successful theory of "low energy physics," (relative to the Planck scale) alongside gravity, which offered a window into Planck mass physics. "Finally," he added, "we can be lucky." (p. 327).

4. Conclusion

For all too long, we physicists, philosophers, and historians have found it convenient to speak about the history of the relation of experiment, theory, and mathematics as if these categories of inquiry passed from premodern chaos to a permanent and quasi-stable "modern" science sometime in the 17th-century. But why should we assume such a static relationship? Why expect that there is a fixed model when so much else has changed in the practice of physics. Already in the mid-19th century, theoretical physics began to separate from experimental physics. By the time of H. A. Lorentz, Wilhelm Wien, Ludwig Boltzmann, and Einstein, recognizable sites of distinctly theoretical work had emerged in a form distinct from the much older tradition of mathematical physics and celestial mechanics. Quantum mechanics undoubtedly further separated experiment and theory as institutional settings for theory arose, perhaps none better known than Niels Bohr's in Copenhagen. Enrico Fermi, who established himself on paper and in the laboratory, is remembered precisely because his crossover was one of the last. String theory continues this separation, but has done so in sufficient measure that the difference in degree has been registered by theoretical physicists as a difference in kind: the practice of superstring theory is a practice far closer to mathematics than to experiment.

Sociologically, the separation of superstring from gauge physics done at accelerator energies has been remarkable. Much as graduate students entering their training quickly sort out into experimentalists and theorists, now theoretical students quickly divide into those destined for strings and those heading for point particles. And once in place, few make the transition. Theorists, even some string theorists, watch in wonder at virtuoso string theorists who move with ease in Calabi-Yau spaces, yet have little sense of pions, muons, and other entities whose hard-won understanding characterized so much of twentieth-century physics. At the other extreme from experiment, mathematicians have greeted the string theories with a mixture of wonder and horror at the productive speculations offered by string theorists about topics previously thought thoroughly mathematical. One much-discussed proposal by Arthur Jaffe and Frank Quinn [1993] sought to divide mathematics into "theoretical" and rigorous subfields on the model of theory and experiment in physics:

> [P]hysicists are not in fact isolated. They have found a new "experimental community": mathematicians. It is now mathematicians who provide them with reliable new information about the structures they study. Often it is to mathematicians that they address their speculations to stimulate new "experimental" work. And the great successes are new insights into mathematics, not into physics. What emerges is not a new particle, but a description of representations of the "monster" sporadic group using vertex operators in Kac-Moody algebras. What is produced is not a new physical theory, but a new view of polynomial invariants of knots and links in 3-manifolds using Feynman path integrals or representations of quantum groups ... (p. 3).

Jaffe and Quinn touch on the myriad of partly epistemic and partly sociological tensions that sometimes strain the border region between mathematics and physics. Physicists can, at times, disdain the mathematical work as rigorous, but ultimately a matter of "cleaning up" after the real work of understanding is done. Mathematicians, conversely occasionally view the physicists' speculations as nothing more than empty promises. By modeling the relation of speculation and proof on the theory/experiment division of labor, Jaffe and Quinn hope to make room for both. But however this cultural boundary area is negotiated, experiment – the activity of manipulating gases, magnets, and data tapes – has receded from view.

This shift in theory is the second shoe to fall; as I have argued elsewhere, high-energy experimentation too is barely recognizable to someone trained in an early modality of work. Boyle's efforts at Gresham House differ not merely in scale from CERN and Fermilab, but in kind. When a collaboration of two, three or five hundred physicists undertakes a five- to ten-year $500 million inquiry in particle physics, it is not a 17th-century air pump or 19th-century Hertzian coil writ large: the founding epistemic principles of experiment have altered. There are no gentlemanly "neutral" witnesses to experiment even in the 19th century, let alone in the late twentieth; there is no possibility of any direct "experimental repetition" when the device costs more than the net assets of most major universities; there is not even a very well-defined notion of an experimental author when no single individual can conceivably be fundamentally involved with all sectors of experimental construction, data taking, data analysis or interpretation. Even when the results are in, comparing data in a colliding beam experiment often must pass through triggers, filters, and phenomenological analysis before it faces the basic equations of quantum chromodynamics. Monte Carlo simulations, which stand at the threshold between experiment and theory, again challenge any fixed notion of what counts as an experiment. What would Maxwell have called a young particle physicist, who qualified professionally by doing a simulation of a drift chamber deep within a projected colliding beam detector? An experimentalist? A theorist? These categorical boundaries have shifted.

Though the large-scale experiment has been the focus of my concern for many years (Galison e.g. [1985], [1987], [1992]), I have here deliberately turned away from these matters and towards the practice of theory to explore the opposite end of twentieth-century physics: theory pushed maximally far from what Glashow called "the experimental morass."[5] As string theory (and string theorists) occupied an increasingly significant part of contemporary particle physics, this distance from the laboratory has raised deep questions about the very nature of physics as a discipline. To superstrings advocates, the new theories represent the best hope for a final theory of nature, the most important physics development since quantum mechanics. As these advocates see the world, hearkening back to the back-and-forth between experiment and theory is a nostalgic recollection not unlike the colliding beam experimentalist who wishes he was blowing glass cloud chambers at the Cavendish. To superstring detractors, string theory posed a threat to the very existence of an experimentally-based inquiry they call physics.

This is a debate about the nature of physical knowledge. It is a struggle within the community of particle theorists to understand the nature of the constraints that define the character of physical law, and make good its purchase on nature. The string vision takes quantum field theory and extends it in what advocates consider to be the minimal possible way: generalizing the point particle to the one-dimensional string. By doing so, while maintaining other constraints on quantum field theory, it became possible in the mid-1980s for physicists to explore a unified theory of the electromagnetic, weak, and strong forces along with gravity, and to do so without the disabling infinities that hamstrung all previous attempts at a quantum theory of

5 For the argument that theory is not a categorical opposite from practice, but rather that theoretical practices and constraints should be considered on a par with experimental practices and constraints, see Galison, 1987, esp. chapter 1 and 5; Galison, 1988a,b; and Warwick (1992).

gravity. The combination of theoretical virtues – anomaly freedom, chirality, unitarity, finitude – with at least the possibility of finding grand unified phenomenology as a low-energy limit – elevated string theory to a "theory of everything." For the superstring theorists, mathematical constraints (from algebraic topology to a host of related disciplines) served the role of delimitation the way experiment had at an earlier time. Mathematical self-consistency promised uniqueness and in that singularity the hope of access to a final, realistic picture of the foundation of nature.

This substitution of mathematical constraint for experimental constraint is precisely what stunned much of the particle physics world outside the cadre of superstring theorists. Absent the world of experiment, theorists dubious of the superstring program argued that the mathematically-generated intratheoretical constraints would be dangerously arbitrary. True, the string theorists conceived of their substitution of strings for point particles as the minimal modification of quantum field theory. But without experiment to adjudicate among starting assumptions, unbelievers did not see this as such a modest move.

Alongside the contending visions of nature came different strategies for the pursuit of physics. Top-down theory or bottom-up physics? Perhaps both will be productive. Faced with both sides' frustration at making new and experimentally confirmable results, Glashow sardonically remarked at one point that the choice was much crueler, either build castles in the air or chase ambulances on the street.

One among the many interesting features of the debate is the near-universal invocation of history by all participants. The ghost of Einstein hovers over the scene, but appears in many guises. To Green, Witten, Gross, and Weinberg, Einstein's apparition is that of the youthful physicist, struggling to develop general relativity. His success, with only the barest of observational data – really no more than the precession of the perihelion of mercury – offers hope for string theorists as they labor far from the laboratory. The promise of many future confirmations in yet undreamt-of accelerators or astrophysics girds against the charge of being outside of experience altogether. To Georgi and Glashow, the spectre of Einstein is not the creative 36-year old of 1915, but the struggling 75-year old Einstein, disconnected from experiment, cut off from the power of quantum mechanics, quantum field theory, and accelerator-based particle physics that animated so much of the physics of the early 1950s. Wounded by this disconnection, Georgi and Glashow's ghostly Einstein wanders without direction in the futile search for a unified field theory that would grasp all physics and bring the unruly discipline to an end.

As physics comes to the millenium, the path of physics divides. Which does the theorist follow? Theorists drove Einstein's ghost both ways.

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