20
The End of Physics

THE LUCASIAN PROFESSORSHIP OF MATHEMATICS OCCUPIES A CURIOUS POSItion at Cambridge University, for it has traditionally been occupied by a great theoretical physicist. Newton and Dirac were both Lucasian Professors, and its current occupant is Stephen Hawking, easily the most well-known cosmologist in the world. Unfortunately, Hawking is famous less for his considerable accomplishments than for his long struggle with a degenerative neural disease that has kept him in a wheelchair for many years, impeded his ability to speak and write, and may eventually kill him. On April 29, 1980, Hawking assumed the chair with an inaugural lecture entitled, "Is the End in Sight for Theoretical Physics?" The answer, he said, is probably yes. A student read the speech for him:

In this lecture [Hawking's text began] I want to discuss the possibility that the goal of theoretical physics might be achieved in the not-too-distant future, say, by the end of the century. By this I mean that we might have a complete, consistent, and unified theory of the physical interactions which would describe all possible observations. Of course one has to be very cautious about making such predictions: We have thought that we were on the brink of the final synthesis at least twice before. At the beginning of the century it was believed that everything could be understood in terms of [classical] mechanics. All that was needed was to measure a certain number of coefficients of elasticity, viscosity, conductivity, etc. This hope was shattered by the discovery of atomic structure and quantum mechanics. Again, in the late 1920s Max Born told a group of scientists visiting Göttingen that "physics, as we know it, will be over in six months." This was shortly after the discovery by Paul Dirac . . . of the Dirac equation, which governs the behavior of the electron. It was expected that a similar equation would govern the proton, the only other supposedly elementary particle known at that time. However, the discovery of the neutron and of nuclear forces disappointed these hopes. We now know in fact that neither the proton nor the neutron is elementary but that they are made up of smaller particles. Nevertheless, we have made a lot of progress in recent years and, as I shall describe, there are some grounds for cautious optimism that we may see a complete theory within the lifetime of some of those present here.[1]

Hawking has been but one among many physicists who have hoped that unification is at hand, although few have been willing to state their hopes so boldly. Today, unification is the research topic for an entire new generation of theorists. The subject has split into branches and subdivisions, and there are unification conferences and unification journals and even unification text-books. It is the rare theorist who has not tried his hand at putting things together.

Physics has always progressed by drawing together seemingly disparate phenomena into one framework. Newton's recognition that the force that made apples fall was identical to the force that kept the earth in its orbit was a unification, as was the approximately contemporaneous realization that lightning, static electricity, and Saint Elmo's fire were all manifestations of one phenomenon: electricity. In the nineteenth century, Maxwell synthesized electricity, magnetism, and optics into his theory of electromagnetism.

These successes in turn elicited more grandiose but less happily conceived unification schemes. As Freeman Dyson has pointed out, "The ground of physics is littered with the corpses of unified theories."[2] The early nineteenth-century chemist Claude Berthollet spent years trying to demonstrate that gravity was a sort of chemical attraction. Michael Faraday, Maxwell's great predecessor, anticipated Einstein by eighty years when he tried to establish a relation between electromagnetism and gravity in 1850. The beginning of gauge theory was a mistaken stab at unity by Hermann Weyl. Einstein's long failure with unified field theories is well known; in retrospect, a principal stumbling block was his refusal to treat the forces in atomic nuclei as fundamental, a folly that reached its height when he published a paper unsuccessfully attempting to prove "that the elementary formations [i.e., particles] that make up the atom are held together by gravitational forces."[3] Schrödinger, too, spent the last years of his life obsessively pursuing the chimera of a unified field theory. In the 1930s, Yukawa tried to unify the strong and weak forces, but the eventual success of his work owed nothing to unification. Heisenberg launched, two decades later, a unified field theory that started as a collaboration with Pauli. When Pauli withdrew, Heisenberg pressed on. To Pauli's fury, Heisenberg claimed during a radio broadcast in February 1958 that a unified Heisenberg-Pauli theory was imminent, and only a few small technicalities remained to be worked out. Rumors swept the press. Pauli responded by mailing his friends a letter consisting of a blank rectangle, drawn in pencil, with the caption: "This is to show the world I can paint like Titian. Only technical details are missing."[4]

Mindful of the recent record of failure, unification aficionadoes today practice their craft with a measure of irony, for they are as embarrassed by the loose speculation required as they are entranced by the sweetness of the problem. Almost every practicing physicist has been approached by eager cranks with unified theories, sweaty would-be Einsteins with equations of the world that sew up all loose ends; now look at the theoretical legions invading the territory of the nuts. (The presence of the crackpots is in its own way a measure of the attractiveness of the idea.) A complete unified theory would mean the end of physics. Which is not to say that physics equations would vanish, physics experiments would stop, and all physicists would be out of a job. Many millions of loose ends would need tying up; connections would still need to be drawn; effects, to be understood; applications, to be devised and exploited. Science would continue, but all of the fundamental questions that physics can pose would have been answered, and our knowledge of force and matter would henceforth change only in particulars and not in outline.

In the *Critique of Pure Reason*, Immanuel Kant argued that some aspects of nature will remain forever unknown to us, because our minds must impose a structure on our sensations for us to have any experience of the world at all. As a consequence, we are led inevitably to make certain suppositions about nature that are, in actuality, by-products of the organizing activity of our own brains. Such presuppositions—Kant showed their number, and said they apply to more than science—are unprovable in theory but indispensable in practice. He called them "regulative ideas," and listed the unity of nature as a cardinal example. Because of it, the structure of science itself draws physicists toward unification. Given that the standard model contained the answers to all ordinary physical questions, it is then little wonder that an orgy of unifying came after its completion, as theorists followed the impulse built into the science, and no surprise that the sudden explosion of unified theories in the 1970s brought explosive and diverse reactions.

"To my mind," Gerard 't Hooft said, "the most successful programs of unification always came about when there was some urgent question to be answered. In most cases, the urgent question was that something seemed to be wrong in the present understanding, and the correct answer then turned out to be that the only way to get it right was to say that this effect and that effect cancel, and the only way to make them cancel was to put them into one big theory together and show that they were the same force. Then you get something like unification. That's the way I view, say, the unification of the gravitational forces with special relativity which gave general relativity. That was an enormously far-reaching theory, but it arose because there was an obvious question to be answered: How do you reconcile the notion that the effect of two bodies on each other only depends on their masses, nothing else, and on the other hand keep relativistic invariance for the whole set of equations? If you try to answer it, you run into all sorts of difficulties unless you assume, precisely as Einstein did, curved space-time and all that. There was no other solution. So it *had* to be right. That's the way, to me, you should derive a theory. What Einstein did later was try to put electromagnetism together with gravity. However, this time, he didn't have a good motive. All he wanted was unification. That is, he didn't have as a motive that something would be wrong with physics if he didn't do it. And that's why I think he did not succeed."

We asked if this applied to the current unification efforts.

"I'd make the same objection," 't Hooft said. He had been working on putting forces and particles together for a decade, and felt that in many respects he did not have that much to show for it. "There is no obvious physical need. There is an aesthetic need, but not one of purely mathematical logic. And my conviction is that as long as that need is not there, it's unlikely that it will work in this simple way. It may work, so people should continue trying it, but it may well be just like the fate that struck Einstein—that although it looks from an aesthetic point of view to be an obvious thing to try, nature is more subtle than this. The trick is not to try to put things together which do not really belong together, but rather to search for places in the world where there are discrepancies, where different ideas are clashing that ought to be described in one and only one way."

He was asked what kind of discrepancy he had in mind.

"Well, a very important discrepancy I'm interested in, like many other people, is quantum gravity. Because we still don't have a good way of reconciling gravitation with quantum mechanics. There still isn't. I'm sure that whenever somebody finds a way to do that, he'll solve millions of problems in ordinary physics." We remarked the long-standing difficulties in the theory of quantum gravity. "Well, there simply *isn't* a theory. A theory is completely lacking. People claim that they have ideas of theories abut it—Stephen Hawking is doing a lot on it. He has made, you know, some brilliant contributions. But the most fundamental theory, Lagrangian or Hamiltonian, or a proper description of Hilbert space, is missing. And so we just switch on gravity, which is a pain in the neck."

The difference between the domains of gravitation and the elementary particle forces is the difference between the proton and the plenum. The queen of the interactions, gravitation is easily ignored when looking into the microscope of a particle accelerator; at any other time, its gentle, firm, omnipresent pull dominates the firmament. Gravitation takes place on great, epic terrain—the bald ridges and smooth valleys of space-time itself—whereas quantum theory deals in billionths of millimeters, microscopic ferment, entities beneath visibility and beyond visualization. Big/small, classical/quantum, geometry/algebra, the sole obvious point of contact of these most opposite of physical constructs is that their arena of play is the same Universe.

As the standard model and the various grand unified theories drew together the elementary particle forces, the absence of gravitation became more and more conspicuous. In a sense, the very elegance of general relativity has impeded the urgent task of putting gravity into quantum terms.

Too successful to offer much scope for theoretical tinkering, Einstein's monument stood, a seamless wall of mathematics, in proud isolation from the rest of physics. If quantum mechanics and relativity were uneasy spouses in quantum field theory, how much more troubled would be a quantum theory of gravity itself.

In any quantum treatment of gravitation, the force is transmitted by a massless spin-two particle called the graviton. The graviton gives rise to infinities intractable enough to make veteran renormalizers conclude that straight quantum gravity simply is not finite. "Something is missing there," 't Hooft said. "We are all trying very hard to make it all work, but it turns out to be conceptually extremely difficult. One of the big difficulties is that we realize that some of our well-known concepts have to be abandoned, because nature isn't going to be as simple as it appears now. However, we cannot abandon everything at once, because then there is nothing left to work on."[5]

The problem faced by 't Hooft and his coevals is unprecedented in the history of physics. They believe that matter and energy were one just for an instant, at the dawn of time, in the ravening fire of the Big Bang. Thus the phenomena they seek to describe existed only at unimaginable energies that can never be reproduced in the laboratory. Experiment consequently seems almost helpless. Theorists have spent the two decades since SU(5) trying to bootstrap themselves to unification without benefit of data, working out the right theory by pure mathematical ingenuity and physical intuition. They court the risk of divorcing theory entirely from experiment, and turning physics, the prototype of an empirical science, into what Georgi has called "recreational mathematical theology."

Notwithstanding such fears, a dizzying variety of unification theories sprang up in the 1970s and 1980s. Each one bolder than the next but all unproven, they went by various overlapping names—quantum gravity, supersymmetry, Kaluza-Klein, supergravity—the list of permutations was as long as the list of theorists working on the subject. Of all available tries at unification, what is called "superstring theory" is the most remarkable because its predictions are more than usually bizarre, because its history is more than usually chaotic, because it is apparently renormalizable, because it has a tenacious body of adherents, and because, when asked about it, Murray Gell-Mann told us flatly that he believed that some version of string theory some day would be the theory of the whole world. "It's a fantastic thing," he said. "It's a candidate. It's *the* candidate."[6]

Superstring theory arose from the unexpected recent marriage of two wild ideas: (1) the Universe has extra, hidden dimensions; (2) subatomic particles ultimately are not little points but little strings. The idea that the Universe had hidden dimensions was first proposed by the German physicist

Theodor Kaluza in 1919.[7] Inspired by Einstein's four dimensional theory of relativity as well as the Unified Field Theory, Kaluza tried to incorporate electromagnetism into a *five* dimensional form of general relativity. Kaluza's work was brought into accord with quantum mechanics by a thirty-two-year-old Swedish physicist named Oskar Klein; in Klein's version, the fifth dimension was hidden, curled up into a minute circle and playing no real role in our world.[8]

For a while, theorists found the work of Kaluza and Klein exciting, but it didn't seem to go anywhere, and the idea languished for nearly half a century. Then, in the 1970s, it was revived in a strikingly different context, the multidimensional "string theories." Based on work by Gabriele Veneziano, an Israeli, and elaborated by Yoichiro Nambu and a dozen other theorists, the string model at first dealt just with the strong interactions.[9] Its adherents regarded hadrons as little one-dimensional strings rather than points. Mesons were strings with a quark at one end and an antiquark at the other; when the meson forcefully struck another particle, the string snapped, producing two new strings. Isolating a single quark was thus as impossible as creating a piece of string with just one end. The visualizability of the theory broke down for baryons, which had to be imagined as strings with *three* ends. Although the mathematical properties of the string model were fascinating and elegant, its equations seemed to contain a horrific panoply of ghosts, infinities, anomalies, unobserved spin-two particles, and impossible particles that travel faster than light. Many of these could be removed by artful equation-juggling, but only at the price of assuming that space-time has more than the usual number of dimensions—twenty-six, in fact. (*Twenty-six dimensions*!? The physicists who discovered this didn't even *try* to explain what on earth it could mean.)[10] In 1974, two theorists at Caltech, John Schwarz and the late Joël Scherk, who had worked on an alternative string model with only ten dimensions, realized that the unwanted spin-two particle might be the quantum of gravitation.[11] At a stroke, what had been a troubled theory of the strong force was converted into an excellent candidate for a unification theory.[12]

Because gravitation, unlike the strong force, is a manifestation of the structure of space-time, extra dimensions are not necessarily disastrous. Schwarz and Scherk could use a Kaluza-Klein-like device to ensure that the extra dimensions are perpetually hidden from view, squashed into tiny, unvisualizable balls at each point in space. Moreover, string theories naturally could be extended to "supersymmetry," a method of classifying together particles of force and particles of mass. (For this reason, the theory bears the name of *super*string theory.)[13] The ideas of Schwarz and Scherk were sufficiently off-beat that they attracted little interest until 1984 and 1985, when Schwarz and a colleague proved that superstrings were not only completely free of ghosts and anomalies—that is, they are mathematically consistent—but that they are consistent for just two versions of the theory.[14] These immediately became candidates for a Theory of Everything.[15] Physicists found the thought of deriving the Universe from the requirements of consistency alone to be irresistible, enchanting, marvelous; unlike Candide, who lived in the best of all possible worlds, we might live in the *only* possible world.[16] Theorists have descended upon superstrings in droves, despite its penchant for predictions that even physicists consider bizarre, such as the existence of "shadow matter" in the Universe, matter invisible to us, that can only be detected by gravitational effects and nothing else. Although there is as yet not a scrap of experimental evidence for superstring theory, it is completely renormalizable and does not appear to be in conflict with anything we know so far—no mean feat for a physical theory nowadays.

At the very least, superstring theory is a textbook example of a theoretical bandwagon, of how a clever mathematical conceit can suddenly become *démodé*, dominate discussion and conference proceedings for months and even years, ultimately withering for lack of contact with experiment. At the very most, it is, as Gell-Mann put it, "the theory of everything—gravity, weak, strong and electromagnetic interactions plus a lot of other things all together—a completely unified theory of nature." If Gell-Mann is right, the books of future historians of science may well treat the construction of the standard model as a lengthy parenthetical interlude between the first inklings of superstring theory after the First World War and its successful application to nature sixty years later.

Howard Georgi once remarked that there is little need for string theories and other unified theories because they only apply to phenomena like the Big Bang that can never be approximated by experiment. He advocated what are called "effective field theories," the suggestion that at different energy realms different field theories are applicable. Just as it would be foolish for engineers who build bridges and design cars to use quantum mechanics, it is nonsensical for particle physicists in an SU(3) × SU(2) × U(1) world to try to go much past SU(5). The phenomena beyond grand unification are so ephemeral, so distant in time, or so heavy that they play no role even in the subatomic domain probed by the largest particle accelerators. Despite his status as a godfather to the unification movement, he professed to find most unification theory unappetizing. "The most interesting question at the moment is what exactly breaks SU(2) × U(1)," he said. "We still don't know what's giving mass to the *W* and the *Z*. We just know that symmetry is broken. It's an absolutely open question whether it's a Higgs or a dynamical mechanism or something that we haven't thought of. I regard that as the only question that I can see at the moment that is both obviously fundamental and obviously physics. Unification is clearly fundamental, but it may not be physics if you can't see any of the effects."

In 1984, Steven Weinberg came to Harvard to give a lecture series on string theories. Georgi greeted him by writing a limerick on the blackboard before Weinberg's first talk.

Steve Weinberg, returning from Texas
Brings dimensions galore to perplex us.
But the extra ones all
Are rolled up in a ball
So tiny it never affects us.

"And," Georgi said, "it bothers me a little that it never affects us."[17] In his view, unification theories in general and string theories in particular may inherently be concerned with the hows and whys of phenomena seen only during the unreachable holocaust of the first instants of creation. If reaching the energy scale of grand unification requires an accelerator whose length is measured in light-years, reaching the energy of full unification could only be done in a machine the size of the galaxy. Because such machines are absent from any version of the future, Georgi has argued that despite their formal elegance, mathematical rigor, and beautiful complexity, unification theories may ultimately be no better than attempts to calibrate the end of the world by examining permutations of the number 666.

Contemptuous of idle philosophizing, practicing physicists tend to be uninterested in the metaphysical overtones of their craft. They define the end of physics operationally, as the day when no government will pay to test further a future unified theory, and resist speculating about why physicists keep trying to put such theories together. When we asked Glashow one day why he had immediately jumped to the idea of a larger, unified gauge group, he responded by reading a passage written in 1927 by one of his Harvard predecessors, Percy Bridgman.

Whatever may be one's opinion as to the simplicity of either the laws or the material structures of Nature, there can be no question that the possessors of such a conviction have a real advantage in the race for physical discovery. Doubtless, there are many simple connections still to be discovered, and he who has a strong conviction of the existence of these simple convictions is much more likely to find them than he who is not at all sure that they are there.[18]

We asked why unification was the necessary outcome of physics.

"It's *not* necessary," Glashow said. "All I can say is that if you have the faith, you have an advantage. Physicists in the past who have looked for simplifying, unifying assumptions have done well. Better than physicists who haven't. But there's no *reason* that things get simpler. They could become more and more chaotic and more and more complicated. They may, at some point. But so far things are getting simpler. I can't say simple, but simpler."[19] Nonetheless, the difference between unification as a long-range goal and unification *now* was important to Glashow. Supersymmetry, supergravity—none of it was to his taste. String theory, he told us in 1985, "is sociologically interesting as an example of a theoretical bandwagon, and not much else." In 1995, his opinion had not changed.

He is not alone. Julian Schwinger, for one, told us that unification was a "fad," a "grand illusion" that is not "a theory in the usual sense but an aesthetic and emotional glow about how things would work if only we could compute them." He dismissed the current push toward unification as simple theoretical hubris. "It's nothing more than another symptom of the urge that afflicts every generation of physicist—the itch to have all the fundamental questions answered in their own lifetimes."[20]

Across the city at Caltech, Richard Feynman grimaced with annoyance when we brought up the subject of tying together the three theories of the standard model. He let it be known that he didn't like the group terminology and that he had doubts about the ambition. "There isn't any theory today that has SU(3), SU(2) x U(1)—whatever the hell it is—that has any experimental check," Feynman snapped. Biting off the words, he quickly listed several serious difficulties with existing unification theories. His voice boomed sarcasm, crowding his big office. "Now, these guys are trying to put it all together. They're *trying* to. But they haven't." He was asked if he felt there'd been any progress toward unification. "*No*," he said. "For the following reason." He stopped, frowned. "Wait a second. It's a crazy question! Because we now know that in Einstein's time he was nowhere near unification. So to say that we're nowhere closer than that time, that's ridiculous. We're certainly closer. We know more. And if there's a finite amount to be known, we obviously must be closer to having the knowledge, okay? I don't know how to make this into a sensible question. It always looks like you're close to unification. We're always trying to put stuff together, okay? The thing that's different between the present time and the time of Einstein is the enormous amount of new phenomena that Einstein knew nothing about." Irritation crept into his voice. He literally twisted with agitation; the discussion was veering into the philosophical. "Electricity and gravity in the 1920s—I'm talking about the 1920s rather than the 1930s—looked close to being unified. The Schrödinger equation, even when it came, was still a differential equation like gravity. So you could say, oh, they're just some sort of differential fields or equations of the world, and it looks like you might be able to unify them someday. But in the meantime, a whole lot of new phenomena came. *It's a dumb question*. Cancel everything I said." He slammed his hand down on the desk. "We know more than we did then. That's true." He abruptly stood up, muttered goodbye, and stalked out the door without another word.[21]

Astonished, we watched him walk with long, urgent strides down the long corridor, drumming his knuckles on the walls as he passed. He turned his head as he went, and glared back in our general direction. Graduate students dodged out of the way as Feynman careened down the hall. "It's goddamned useless to talk about these things!" he shouted back at us. Doors opened along the hallway; heads craned out. "It's a complete waste of time! The history of these things is nonsense!" Feynman paused before turning the corner, and took in a lungful of air. "You're trying to make something difficult and complicated out of something that's simple and beautiful," he said, loudly, vanishing around the corner.

In the corridor, a respectful moment of silence. Murray Gell-Mann poked his head out of his office. "I see you've met Dick," he said mildly.

"You know," Steven Weinberg said, "their wasn't that much of an intellectual discontinuity from SU(3) × SU(2) × U(1) to grand unification." We had just ordered lunch in the Harvard Faculty Club. Around us were open jackets and open wine bottles, loosened ties and clattering silver: the furniture of academic meals. Weinberg talked about going up fifteen orders of magnitude in his paper with Georgi and Quinn, the giddy audacity of cranking through the numbers across such an enormous range. The fire alarm rang. For a moment or two, everyone in the room looked about with the polite incomprehension customarily awarded to signals of disaster. Waiters shooed out the crowd. Weinberg brushed off his trousers and sat on the steps of a nearby building. We asked what he meant by the lack of intellectual discontinuity in grand unification. "I'm not saying this in any critical spirit. What you were really talking about was a new symmetry structure imposed on the good old dynamics of quantum field theory. But with strings, you really have a new dynamics. It's still within the framework of quantum mechanics. But that's almost the only thing that has remained. String theories *look* like field theories over an enormous range of energies, up to the fundamental scale, which is somewhere in the neighborhood of 10^{16}, 10^{17}, 10^{18} GeV. But if you really get up to the fundamental scale, then they stop looking like field theories altogether. They really are a new kind of dynamics."

Mutterings of false alarm; people started to return to the faculty club, although the fire alarm was still ringing because no one could figure out how to turn it off. As we filed in, we asked Weinberg whether strings change our understanding of the birth of the Universe. Despite the hubbub and the jostling, he spoke readily and concisely. "It doesn't really answer any questions, because if you let the clock run backward and imagine what the Universe looks like as you go to earlier and earlier times, you still see a singular state." That is, properties like the energy density shoot up to infinity as you go back in time, and at the beginning is a white-hot point unexplained by current physics.

On another occasion, Weinberg remarked, "I'd say the period from the mid-sixties to the mid-seventies was enormously exciting, progressive, the best time we've had in physics since the late forties. Unfortunately, since then experiment and theory have gotten out of touch with each other. It's not really the fault of anybody, it's just the logic of the way the subject has developed. It's been the most frustrating decade, really, just awful!—in the sense that the thing that the brightest theorists are doing does not directly bear on any experiment that's about to be done or can be done in the foreseeable future. Supergravity, Kaluza-Klein, grand unification, all of that stuff, with a few little exceptions, can't be tested experimentally. And where it can, it hasn't been terribly impressive. Look what's happened with supergravity. The people who've been working on it for the past ten years are enormously bright. Some of them seem brighter than anyone I knew in my early years. They have elaborated these theories of supersymmetry, supergravity, and superstrings in a way that I think is unprecedented in the history of science for a theory that has *no experimental support whatsoever*.

"I've done it myself, I'm not badmouthing them. I think it was the right thing to do, because, as I say, you do what you can, and this was the best that could be done." He was discouraged by the implicit ironies: So many good theorists with so many good ideas who think they're so close to unification—only to find that proving the theories is utterly impossible with any foreseeable technology. "We just can't go on doing physics like this without support from experiment," he said. "The experimentalists do great things—discover the *W* and Z—and God bless them, it's wonderful. But the theory has moved to the point where these experiments are not helping. I hope that with the next generation of accelerators, we'll get out of this morass."[22]

Weinberg spoke in 1985. Today, a decade later, his hope is unrealized; to a daunting extent, particle physics is exactly where it was when he was interrupted by the faculty club fire alarm. With huge effort, experimenters have filled in a few missing pieces from the standard model. Notable among them is the top quark, its discovery finally announced at Fermilab in March 1995, after many false starts and early intimations. (The top turned out to be amazingly heavy—its mass is about that of a gold atom.) But neither that discovery nor any other provided decisive indications of how to move beyond the standard model. Indeed, some of the experiments have closed down possibilities. Studies of the Z^0 at CERN and Stanford in 1989 strongly suggested that three and only three families of elementary particles exist; the standard model's list of quarks and leptons seems exhaustive. (No new quark families; no more neutrinos.) Dashing hopes, nobody has turned up any of the welter of mirror particles predicted by supersymmetry. (No squarks, as the supersymmetric mirror quarks are called; no sneutrinos.) Experiments to determine whether neutrinos might have mass have been inconclusive. The Higgs has never been observed. And proton decay—which probably must exist if unified theories are to be constructed—has not been confirmed, though a second generation of experiments is under way.

With the standard model, high-energy physicists seem to have become victims of their own success. They have explained everything in reach yet are unsatisfied by the answers; worse, they have no way to go beyond them. To cite just one pressing question, they want to know why mass is scattered in such apparently random fashion. Why is the muon 200 times heavier than the electron? Why does the top quark have a mass almost *40,000* times greater than that of the up quark? Why not 4,000 times, or 400? If the Higgs field provides all particles with mass, why does it provide them with *those* particular masses? And what is the mass of the Higgs itself? For that matter, why do the coupling constants have their values? Surely nature has not chosen such a sloppy picture for its final statement. At higher and higher energies, physicists say, the standard model *must* break down somewhere. But where? How? Why? Despite thorough inspection of every corner in the subatomic realm, nothing so far has shown a road ahead. The prospect is of endless tantalization and stretches of tedium.

Even more dismaying, no projects now on the drawing board seem guaranteed to turn up something new. In the past, physicists have always forged ahead by building bigger accelerators, which have allowed them to explore terrain at ever higher energies. Faced with the dilemma of the standard model, their initial reaction was to propose the largest pure research project ever attempted: the Superconducting Supercollider. This vast accelerator—a fifty-mile ring in the drylands outside Waxahachie, Texas—was intended to smash together protons at 20 trillion electron volts, an energy theorists believed sufficient to find the Higgs boson, thus pinning down one of the greatest unknowns in the standard model. Yet the U.S. Congress stunned scientists the world over by killing the project in the fall of 1993. Years of effort and billions of dollars had already been spent; bulldozers had already excavated ten miles of tunnel. But as price estimates rose from $4.4 billion to $11 billion and doubts arose about the management of the project, the government balked. Its decision marked the end of the postwar era of partnership among science, industry, and government—the environment in which the standard model had been developed.

The only current project of similar scope is at CERN, which plans to spend $2 billion to expand its Large Electron Positron ring, opened in 1989, into a bigger ring called the Large Hadron Collider (though financing the machine has been the subject of contention among member states). The LHC, as it has been dubbed, is slated to reach 10 TeV in 2004 and 14 TeV in 2008—less powerful than the Superconducting Supercollider would have been but at least with some chance of existing. If completed, the LHC may well be the last large particle accelerator ever built. To be sure, a few more ambitious projects are on the drawing board: a muon collider, backed by scientists at Brookhaven, and a Next Linear Collider, which would shoot bundles of electrons from both ends of a long, straight accelerator, having them collide in the middle. The idea, which has champions at Stanford and KEK in Japan, would avoid, at least in theory, some of the complications necessary to bend electrons around a circle. But such projects are bound to be enormously expensive; even if funded, they are at least a decade away. And in an era of austerity there is no guarantee that any of them will be approved.

Recoiling from this dark prospect, some high-energy physicists wonder if their discipline has indeed come to an end. Perhaps this generation may see the end of physics, as Hawking suggested, not because it has accomplished its intellectual mission but because it has exhausted the interest and resources of the governments that have supported it. Other physicists worry that the vast teams of Ph.D.s needed to run the projected accelerators will be so unpleasant to work in that the best and the brightest will shun the field. Time and again, the lions of the last generation of physics, now leaders of huge experimental groups, told us that, were they young and beginning their careers, they would think twice about working in such a huge group.

In response, some physicists have attempted to devise clever, quick, inexpensive ways of peering beyond the standard model—conjuring up high-energy phenomena with low-energy equipment. One way is to hunt for rare events: processes that just maybe could occur, decays that might not be forbidden, particles that could be spotted by the lucky. All over the world, these experiments are now tucked into the corners of laboratories, small and untended apparata which hope to snare occurrences rare enough to have been overlooked in earlier experiments. Proton decay is the archetype—a phenomenon whose mere existence would be enough to shake the community. (No need to spot it twice. Just one gold-plated event and unification is in business!) By and large, though, these experiments are risky, as the failure to establish proton decay demonstrates; worse, they cannot rule out any subject of investigation, for the failure to find a phenomenon may only mean that the background was prohibitively high or the equipment insufficiently sensitive.

Other physicists have chosen a different route: high precision. Like art scholars who learn new insights about old masters from microscopic inspection of brushstrokes, the physicists hope to measure familiar parts of nature with such incredible accuracy as to shed new meaning on the whole. Many such projects are under construction, but we were drawn to one with both a fascinating chance to peek into the future and great historical resonance—the (g-2) experiment at Brookhaven National Laboratory.

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Workmen unbolted the wagon wheel from the big silver ring, and the ceiling crane lifted it away. Dangling from the hook, the spokes silhouetted against the ceiling, the assembly bobbed like a spider drifting on its line. Underneath, the ring rested on a system of metal shims that workers would use to nudge the ring into its proper position; several of them were already measuring its placement and tightening bolts. A team of engineers looked on from a computer room on a sort of mezzanine, a room whose floor was torn up in preparation for installation of computer cables. Satisfied that the morning's work was nearly complete, Gerry Bunce ordered pizzas from Alfredo's, a local joint celebrated at Brookhaven for its quick deliveries to the laboratory bench. He had to twist around pieces of apparatus that seemed to fill every available inch of floor space. Things were cramped, he said. "As you can see"—waving his hand toward the metallic curve of the outer magnet, nearly scraping the walls—"the experiment just barely squeaks inside."

The building, like much else in the experiment, was a matter of new wine in old bottles. It had once housed the bubble chamber that Nick Samios and company used to discover the omega minus, a big step en route to the construction of the standard model. Now, three decades later, Bunce and his colleagues were seeking evidence of forms of matter beyond the standard model, by precisely measuring the effects of the swarm of virtual particles that envelops the muon. Because a spinning muon continually emits and reabsorbs virtual particles, which are always themselves spinning, a magnetic field "sees" a muon as having a slightly different spin than it otherwise would have. The muon's spin direction in a magnetic field is thus fractionally different from what it would be if it were not releasing and taking in virtual particles. Measuring this deviation is also a reworking of the past. A kissing cousin to the Lamb shift, it is known formally as the anomalous magnetic moment and informally as (g-2). (The name, one recalls, came from the realization that the magnetic moment of an electron in a world without virtual particles would be exactly 2; the real magnetic moment, g, is slightly larger than 2, with the difference being g-2.) In 1947 John Nafe and Edward Nelson measured (g-2) for the electron, an experiment that spurred Julian Schwinger to renormalize quantum electrodynamics, thus proving the validity of the theory. Today, the Brookhaven team is hoping that measuring (g-2) for the muon would help them go beyond theory.

Schwinger worked out the value of (g-2) for the electron. That (g-2) is now the most precisely calculated number in the history of science. In the early 1980s, Toichiro Kinoshita, a theorist at Cornell, spent hundreds of hours on the world's biggest computers to evaluate the mind-bogglingly complex equations necessary to predict the contributions of every known type of particle. Eventually he arrived at a value for (g-2) of the electron—.001159652460, plus or minus a few trillionths.

Meanwhile, he was working on (g-2) for the muon. In addition to calculating the interactions when muons emitted and reabsorbed virtual photons, he had to take into account what happened when the photons split into virtual electrons and positrons, and what happened when the virtual electrons and positrons interacted with the magnetic field. Maddeningly, the virtual photons could also split into virtual quarks and antiquarks, so the contributions of the strong force had to be included. And through the weak force the muon could emit and reabsorb a virtual Z^{o}, emit and reabsorb a W and a virtual neutrino, emit a virtual neutrino, turning into an electron in the process while also emitting another neutrino—the possibilities were endless. Kinoshita found himself trucking with equations that had 20,000 terms; he gobbled up supercomputer time in three continents. Eventually he accounted for every known type of particle. His final prediction for the muon (*g*-2)—.00116591688, with the usual small margin of error.

That number—.00116591688—was the target at which the experimenters at Brookhaven were shooting. If their work was accurate enough, any deviation would signal something new. Frustratingly, though, even if Bunce and colleagues detect the presence of new forms of matter, they will be unable to pin down their identities; the experimenters would be like sea explorers forced to turn back with land visible on the horizon. Nevertheless, they would have made a vital contribution to the geography of the subatomic world.

"We're seeking a factor of twenty improvement in precision over the previous (*g*-2) measurement [made at CERN in 1979]," Bunce said. "That's not going to be easy—it was a great experiment! But it's a fantastic opportunity to see if there's something new out there." His glance came to rest at the empty pipe protruding from the wall, future conduit for the experimental muons. "This was the last magnet ring to be installed," he said. "But much remains to be done. Tomorrow, we start making measurements on the magnets. Then we have to hook up the refrigeration system and connect up the beam pipe that will lead the particles from the accelerator into the—"

Bunce stopped; the pizza had arrived. As the delivery driver waited, Bunce moved about the floor, gathering a collection to pay for the food. He escorted the driver to the frigid parking lot.

As it often does, the Long Island weather had taken a blustery turn. The wind had picked up, shaking the scrub oak and pitch pine. While the sun continued to stream confidently and unfalteringly through patches of blue, white clouds covered most of the sky, and thick black columns indicated the presence of snowstorms on other parts of the island. Anything could happen.

The pizza van rumbled off. Bunce stood outside, good hand jammed into his jacket pocket. It was possible to imagine that an air of stillness and expectation hung over the scene. Eventually, smiling, he turned back to work. Inside the recycled building, amid reused pieces of equipment in a half-completed room, we saw a dozen or so members of the (*g*-2) experiment gathered around boxes of pizza. Food steaming in one hand, writing implements in the other, they were honing their strategies to tease the secrets of matter from a measurement a decimal place or two more precise than ever before. "Hey!" Bunce said, laughing. "Save me some!"[23]