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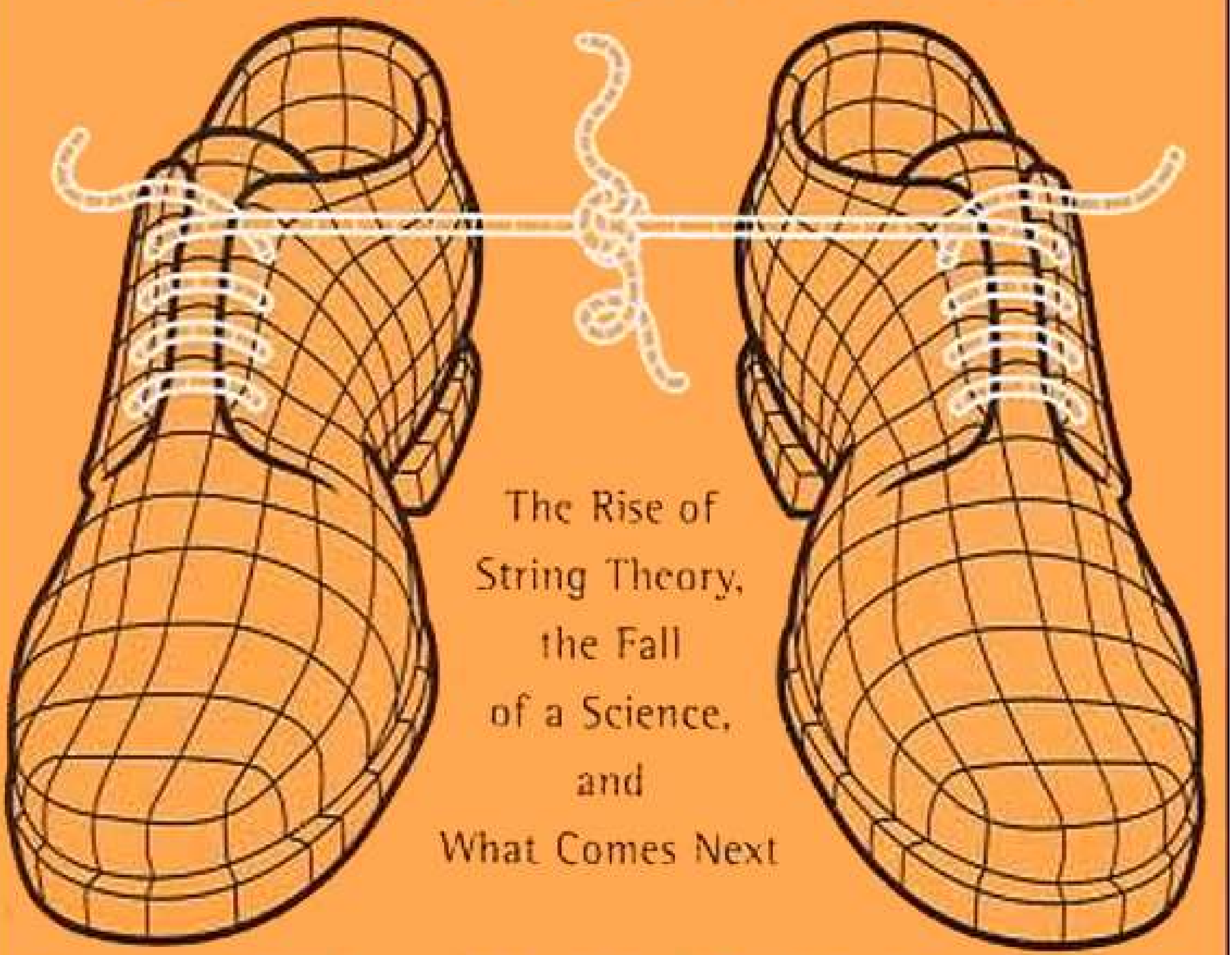
THE TROUBLE WITH PHYSICS



The Rise of
String Theory,
the Fall
of a Science,
and
What Comes Next

LEE SMOLIN

The TROUBLE with PHYSICS

The Rise of String Theory, the Fall of a Science, and What Comes Next

Lee Smolin

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Introduction

There may or may not be a God. Or gods. Yet there is something ennobling about our search for the divine. And also something humanizing, which is reflected in each of the paths people have discovered to take us to deeper levels of truth. Some seek transcendence in meditation or prayer; others seek it in service to their fellow human beings; still others, the ones lucky enough to have the talent, seek transcendence in the practice of an art.

Another way of engaging life's deepest questions is science. Not that every scientist is a seeker; most are not. But within every scientific discipline, there are those driven by a passion to know what is most essentially true about their subject. If they are mathematicians, they want to know what numbers are, or what kind of truth mathematics describes. If they are biologists, they want to know what life is, and how it started. If they are physicists, they want to know about space and time, and what brought the world into existence. These fundamental questions are the hardest to answer and progress is seldom direct. Only a handful of scientists have the patience for this work. It is the riskiest kind of work, but the most rewarding: When someone answers a question about the foundations of a subject, it can change everything we know.

Because it is their job to add to our growing store of knowledge

And those scientists who work on the foundations of any given field are fully aware that the building blocks are never as solid as their colleagues tend to believe.

This is the story of a quest to understand nature at its deepest level. Its protagonists are the scientists who are laboring to extend our knowledge of the basic laws of physics. The period of time I will address — roughly since 1975 — is the span of my own professional career as a theoretical physicist. It may also be the strangest and most frustrating period in the history of physics since Kepler and Galileo began the practice of our craft four hundred years ago.

The story I will tell could be read by some as a tragedy. To put it bluntly — and to give away the punch line — we have failed. We inherited a science, physics, that had been progressing so fast for so long that it was often taken as the model for how other kinds of science should be done. For more than two centuries, until the present period, our understanding of the laws of nature expanded rapidly. But today, despite our best efforts, what we know for certain about these laws is no more than what we knew back in the 1970s.

How unusual is it for three decades to pass without major progress in fundamental physics? Even if we look back more than two hundred years, to a time when science was the concern mostly of wealthy amateurs, it is unprecedented. Since at least the late eighteenth century, significant progress has been made on crucial questions every quarter century.

By 1780, when Antoine Lavoisier's quantitative chemistry experiments were showing that matter is conserved, Isaac Newton's laws of motion and gravity had been in place for almost a hundred years. But while Newton gave us a framework for understanding all of nature, the frontier was wide open. People were just beginning to learn the basic facts about matter, light, and heat, and mysterious phenomena like electricity and magnetism were being elucidated.

Over the next twenty-five years, major discoveries were made in each of these areas. We began to understand that light is a wave. We discovered the law that governs the force between electrically charged particles. And we made huge leaps in our understanding of matter with John Dalton's atomic theory. The notion of energy was introduced; interference and diffraction were explained in terms of the wave theory of light; electrical resistance and the relationship between electricity and magnetism were explored.

Several basic concepts underlying modern physics emerged in the next quarter century, from 1830 to 1855. Michael Faraday introduced the notion that forces are conveyed by fields, an idea he used to greatly advance our understanding of electricity and magnetism. During the same period, the conservation of energy was proposed, as was the second law of thermodynamics.

In the quarter century following that, Faraday's pioneering ideas about fields were developed by James Clerk Maxwell into our modern theory of electromagnetism. Maxwell not only unified electricity and magnetism, he explained light as an electromagnetic wave. In 1867, he explained the behavior of gases in terms of the atomic theory. During the same period, Rudolf Clausius introduced the notion of entropy.

The period from 1880 to 1905 saw the discoveries of electrons and X rays. The study of heat radiation was developed in several steps, leading to Max Planck's discovery, in 1900, of the right formula to describe the thermal properties of radiation — a formula that would spark the quantum revolution.

In 1905, Albert Einstein was twenty-six. He had failed to find an academic job in spite of the fact that his early work on the physics of heat radiation alone would come to be seen as a major contribution to science. But that was just a warm-up. He soon zeroed in on the fundamental questions of physics: First, how could the relativity of motion be reconciled with Maxwell's laws of electricity and magnetism? He told us in his special theory of relativity. Should we think of the chemical elements as Newtonian atoms? Einstein proved we must. How can we reconcile the theories of light with the existence of atoms? Einstein told us how, and in the process showed that light is both a wave and a particle. All in the year 1905, in time stolen from his work as a patent examiner.

The working out of Einstein's insights took the next quarter century. By 1930, we had his general theory of relativity, which makes the revolutionary claim that the geometry of space is not fixed but evolves in time. The wave-particle duality uncovered by Einstein in detailed understanding of atoms, chemistry, matter, and radiation. By 1930 we also knew that the universe contained huge numbers of galaxies like our own, and we knew they were moving away from one another. The implications were not yet clear, but we knew we lived in an expanding universe.

With the establishment of quantum theory and general relativity as part of our understanding of the world, the first stage in the twentieth-century revolution in physics was over. Many physics professors, uncomfortable with revolutions in their areas of expertise, were relieved that we could go back to doing science the normal way, without having to question our basic assumptions at every turn. But their relief was premature.

Einstein died at the end of the next quarter century, in 1955. By then we had learned how to consistently combine quantum theory with the special theory of relativity; this was the great accomplishment of the generation of Freeman Dyson and Richard Feynman. We had discovered the neutron and the neutrino and hundreds of other apparently elementary particles. We had also understood that the myriad phenomena in nature are governed by just four forces: electromagnetism, gravity, the strong nuclear force (which holds atomic nuclei together), and the weak nuclear force (responsible for radioactive decay).

Another quarter century brings us to 1980. By then we had constructed a theory explaining the results of all our experiments on the elementary particles and forces to date — a theory called the standard model of elementary-particle physics. For example, the standard model told us precisely how protons and neutrons are made up of quarks, which are held together by gluons, the carriers of the strong nuclear force. For the first time in the history of fundamental physics, theory had caught up with experiment. No one has since done an experiment that was not consistent with this model or with general relativity.

Going from the very small to the very large, our knowledge of physics now extended to the new science of cosmology, where the Big Bang theory had become the consensus view. We realized that our universe contains not only stars and galaxies but exotic objects such as neutron stars, quasars, supernovas, and black holes. By that black holes radiate. Astronomers also had evidence that the universe contains a lot of dark matter — that is, matter in a form that neither emits nor reflects light.

In 1981, the cosmologist Alan Guth proposed a scenario for the very early history of the universe called inflation. Roughly speaking, his theory asserts that the universe went through a spurt of enormous growth extremely early in its life, and it explains why the universe looks pretty much the same in every direction. The theory of inflation made predictions that seemed dubious, until the evidence began to swing toward them a decade ago. As of this writing, a few puzzles remain, but the bulk of the evidence supports the predictions of inflation.

Thus, by 1981, physics had enjoyed two hundred years of explosive growth. Discovery after discovery deepened our understanding of nature, because in each case theory and experiment had marched hand in hand. New ideas were tested and confirmed and new experimental discoveries were explained in terms of theory. Then, in the early 1980s, things ground to a halt.

I am a member of the first generation of physicists educated since the standard model of particle physics was established. When I meet old friends from college and graduate school, we sometimes ask each other, "What have we discovered that our generation can be proud of?" If we mean new fundamental discoveries, established by experiment and explained by theory — discoveries on the scale of those just mentioned — the answer, we have to admit, is "*Nothing!*" Mark Wise is a leading theorist working on particle physics beyond the standard model. At a recent seminar at the Perimeter Institute of Theoretical Physics, in Waterloo, Ontario, where I work, he talked about the problem of where the masses of the elementary particles come from. "We've been remarkably unsuccessful at solving that problem," he said. "If I had to give a talk on the fermion-mass problem now, I'd probably end up talking about things I could have in the 1980s."[1] He went on to tell a story about when he and John Preskill, another leading theorist, arrived at Caltech in 1983, to join its faculty. "John Preskill and I were sitting together in his office, talking. . . . You know, the gods of physics were at Caltech, and

and lepton masses, and he wrote it on a yellow sheet of paper and stuck it on his bulletin board . . . so as not to forget to work on them. Fifteen years later, I come into his office . . . and we're talking about something, and I look up at his bulletin board and [notice that] that sheet of paper is still there but the sun has faded everything that was written on it. So the problems went away!"

To be fair, we've made two experimental discoveries in the past few decades: that neutrinos have mass and that the universe is dominated by a mysterious dark energy that seems to be accelerating its expansion. But we have no idea *why* neutrinos (or any of the other particles) have mass or what explains their mass value. As for the dark energy, it's not explained in terms of any existing theory. Its discovery cannot then be counted as a success, for it suggests that there is some major fact we are all missing. And except for the dark energy, no new particle has been discovered, no new force found, no new phenomenon encountered that was not known and understood twenty-five years ago.

Don't get me wrong. For the past twenty-five years we have certainly been very busy. There has been enormous progress in applying established theories to diverse subjects: the properties of materials, the molecular physics underlying biology, the dynamics of vast clusters of stars. But when it comes to extending our knowledge of the laws of nature, we have made no real headway. Many beautiful ideas have been explored, and there have been remarkable particle-accelerator experiments and cosmological observations, but these have mainly served to confirm existing theory. There have been few leaps forward, and none as definitive or important as those of the previous two hundred years. When something like this happens in sports or business, it's called hitting the wall.

Why is physics suddenly in trouble? And what can we do about it? These are the central questions of my book.

I'm an optimist by nature, and for a long time I fought the conclusion that this period in physics — the period of my own career — has been an unusually fallow one. For me and many of my friends who entered science with the hope of making important contributions to what then was a rapidly moving field, there is a shocking we have not achieved anything that we can be confident will outlive us. This has given rise to personal crises. But, more important, it has produced a crisis in physics.

The main challenge for theoretical particle physics over the last three decades has been to explain the standard model more deeply. Here there has been a lot of activity. New theories have been posited and explored, some in great detail, but none has been confirmed experimentally. And here's the crux of the problem: In science, for a theory to be believed, it must make a new prediction — different from those made by previous theories — for an experiment not yet done. For the experiment to be meaningful, we must be able to get an answer that disagrees with that prediction. When this is the case, we say that a theory is *falsifiable* — vulnerable to being shown false. The theory also has to be *confirmable*; it must be possible to verify a new prediction that only this theory makes. Only when a theory has been tested and the results agree with the theory do we advance the theory to the ranks of true theories.

The current crisis in particle physics springs from the fact that the theories that have gone beyond the standard model in the last thirty years fall into two categories. Some were falsifiable, and they were falsified. The rest are untested — either because they make no clean predictions or because the predictions they do make are not testable with current technology.

Over the last three decades, theorists have proposed at least a dozen new approaches. Each approach is motivated by a compelling hypothesis, but none has so far succeeded. In the realm of particle physics, these include Technicolor, preon models, and supersymmetry. In the realm of spacetime, they include twistor theory, causal sets, supergravity, dynamical triangulations, and loop quantum gravity. Some of these ideas are as exotic as they sound.

One theory has attracted more attention than all the others combined: string theory. The reasons for its popularity are not hard to understand. It purports to correctly describe the big and the small — both gravity and the elementary particles — and to do so, it makes the boldest hypotheses of all the theories: It posits that the world contains as yet unseen dimensions and many more particles than string — that obeys simple and beautiful laws. It claims to be the one theory that unifies *all* the particles and *all* the forces in nature. As such, it promises to make clean and unambiguous predictions for any experiment that has ever been done or ever could be done. Much effort has been put into string theory in the last twenty years, but we still do not know whether it is true. Even after all this work, the theory makes no new predictions that are testable by current — or even currently conceivable — experiments. The few clean predictions it does make have already been made by other well-accepted theories.

Part of the reason string theory makes no new predictions is that it appears to come in an infinite number of versions. Even if we restrict ourselves to theories that agree with some basic observed facts about our universe, such as its vast size and the existence of the dark energy, we are left with as many as 10^{500} distinct string theories — that's 1 with 500 zeros after it, more than all the atoms in the known universe. With such a vast number of theories, there is little hope that we can identify an outcome of an experiment that would not be encompassed by one of them. Thus, no matter what the experiments show, string theory cannot be disproved. But the reverse also holds: No experiment will ever be able to prove it true.

At the same time, we understand very little about most of these string theories. And of the small number we do understand in any detail, every single one disagrees with the present experimental data, usually in at least two ways.

So we face a paradox. Those string theories we know how to study are known to be wrong. Those we cannot study are thought to exist in such vast numbers that no conceivable experiment could ever disagree with all of them.

These are not the only problems. String theory rests on several key conjectures, for which there is some evidence but no proof. Even worse, after all the scientific labor expended in its study, we still do not know whether there is a complete and coherent theory that can even go by the name "string theory." What we have, in fact, is not a theory at all but a large collection of approximate calculations, together with a web of conjectures that, if true, point to the existence of a theory. But that theory has never actually been writdon't know what mathematical language it should be expressed in — perhaps a new one will have to be invented to describe it. Lacking both fundamental principles and the mathematical formulation, we cannot say that we even know what string theory asserts.

Here is how the string theorist Brian Greene puts it in his latest book, *The Fabric of the Cosmos:* "Even today, more than three decades after its initial articulation, most string practitioners believe we still don't have a comprehensive answer to the rudimentary question, What is string theory? . . . [M]ost researchers feel that our current formulation of string theory still lacks the kind of core principle we find at the heart of other major advances."[2]

Gerard 't Hooft, a Nobel Prize winner for his work in elementary-particle physics, has characterized the state of string theory this way: "Actually, I would not even be prepared to call string theory a 'theory,' rather a 'model,' or not even that: just a hunch. After all, a theory should come with instructions on how to deal with it to identify the things one wishes to describe, in our case the elementary particles, and one should, at least in principle, be able to formulate the rules for calculating the properties of these particles, and how to make new predictions for them. Imagine that I give you a chair, while explaining that the legs are still missing, and that the seat, back and armrest will perhaps be delivered soon. Whatever I did give you, can I still call it a chair?"[3]

David Gross, a Nobel laureate for his work on the standard model, has since become one of the most aggressive and formidable champions of string theory. Yet he closed a recent conference intended to celebrate the theory's progress by saying, "We don't know what we are talking about. . . . The state of physics today is like it was when we were mystified by radioactivity. . . . They were missing something absolutely fundamental. We are missing perhaps something as profound as they were back then."[4]

But though string theory is so incomplete that its very existence is an unproved conjecture, that does not keep many who work on it from believing that it is the only way forward for theoretical physics. One prominent string theorist, Joseph Polchinski, of the Kavli Institute for Theoretical Physics at UC Santa Barbara, was ternatives. . . . All good ideas are part of string theory."[5] Lubos Motl, an assistant professor at Harvard, recently asserted on his blog that "the most likely reason why no . . . person has convinced others about [an] alternative to string theory is that there probably exists no alternative to string theory."[6]

What is going on here? Usually in science one means something quite definite by the term *theory*. Lisa Randall, an influential particle theorist and Motl's colleague at Harvard, defines a theory as "a definite physical framework embodied in a set of fundamental assumptions about the world — and an economical framework that encompasses a wide variety of phenomena. A theory yields a specific set of equations and predictions — ones that are borne out by successful agreement with experimental results."[7]

String theory does not fit this description — at least not yet. How, then, are some experts sure there is no alternative to string theory, if they don't know precisely what it is? What exactly is it that they are sure has no alternative? These are some of the questions that led me to write this book.

Theoretical physics is hard. Very hard. Not because a certain amount of math is involved but because it involves great risks. As we will see over and over again as we examine the story of contemporary physics, science of this kind cannot be done without risk. If a large number of people have worked on a question for many years and the answer remains unknown, it may mean that the answer is not easy or obvious. Or this may be a question that has no answer.

String theory, to the extent it is understood, posits that the world is fundamentally different from the world we know. If string theory is right, the world has more dimensions and many more particles and forces than we have so far observed. Many string theorists talk and write as if the existence of those extra dimensions and particles were an assured fact, one that no good scientist can doubt. More than once, a string theorist has said to me something like "But do you mean you think it's possible that there are *not* extra dimensions?" In fact, neither theory nor experiment offers any evidence at all that extra dimensions exist. One of the goals of this book is to demystify the claims of string theory. The ideas are beautiful and well motivated. But to understand why they have not led to greater progress, we have to be clear about exactly what the evidence supports and what is still missing.

Because string theory is such a high-risk venture — unsupported by experiment, though very generously supported by the academic and scientific communities — there are only two ways the story can end. If string theory turns out to be right, string theorists will turn out to be the greatest heroes in the history of science. On the basis of a handful of clues — none of which has an unambiguous reading — they will have discovered that reality is far more vast than previously imagined. Columbus discovered a new continent unknown to the king and queen of Spain (as the Spanish royals were unknown to the residents of the New World). Galileo discovered new stars and moons, and later astronomers discovered new planets. All this would pale in the face of the discovery of new dimensions. Moreover, many string theorists believe that the myriad worlds described by the huge number of string theories really do exist — as other universes impossible for us to see directly. If they are right, we see far less of reality than any group of cave dwellers saw of the earth. No one in human history has ever guessed correctly about such a large expansion of the known world.

On the other hand, if string theorists are wrong, they can't be just a little wrong. If the new dimensions and symmetries do not exist, then we will count string theorists among science's greatest failures, like those who continued to work on Ptolemaic epicycles while Kepler and Galileo forged ahead. Theirs will be a cautionary tale of how not to do science, how not to let theoretical conjecture get so far beyond the limits of what can rationally be argued that one starts engaging in fantasy.

One result of the rise of string theory is that the community of people who work on fundamental physics is split. Many scientists continue to work on string theory, and perhaps as many as fifty new PhDs are awarded each year for work in this field. But there are some physicists who are deeply skeptical — who either never saw the point or have by now given up waiting for a sign that the theory has a consistent formulation or makes a real experimental prediction. The split is not always friendly. Doubts are expressed on each other, and it is real work maintaining friendships across the divide.

According to the picture of science we all learned in school, situations like this are not supposed to develop. The whole point of modern science, we are taught, is that there is a method that leads to progress in our understanding of nature. Disagreement and controversy are of course necessary for science to progress, but there is always supposed to be a way to resolve a dispute by means of experiment or mathematics. In the case of string theory, however, this mechanism seems to have broken down. Many adherents and critics of string theory are so confirmed in their views that it is difficult to have a cordial discussion on the issue, even among friends. "How can you not see the beauty of the theory? How could a theory do all this and not be true?" say the string theorists. This provokes an equally heated response from skeptics: "Have you lost your mind? How can you believe so strongly in *any* theory in the complete absence of experimental test? Have you forgotten how science is supposed to work? How can you be so sure you are right when you do not even know what the theory is?"

I have written this book in the hope that it will contribute to an honest and useful discussion among experts and lay readers alike. In spite of what I have seen in the last few years, I believe in science. I believe in the ability of the scientific community to rise above acrimony and resolve controversy through rational argument based on the evidence in front of us. I am aware that just by raising these issues, I will anger some of my friends and colleagues who work on string theory. I can only insist that I am writing this book not to attack string theory or those who believe in it but out of admiration for them and, above all, as an expression of faith in the physics scientific community.

So this is not a book about "us" versus "them." During my career, I have worked on both string theory and on other approaches to quantum gravity (the reconciliation of Einstein's general theory of relativity with quantum theory). Even if most of my efforts have gone into these other approaches, there have been periods when I avidly believed in string theory and devoted myself to solving its key problems. While I didn't solve them, I wrote eighteen papers in the subject; thus, the mistakes I will discuss are my mistakes as believed to be true, in spite of never having been proved. But I was among the believers, and I made choices about my research based on those beliefs. I will speak of the pressures that young scientists feel to pursue topics sanctioned by the mainstream in order to have a decent career. I have felt those pressures myself, and there were times when I let my career be guided by them. The conflict between the need to make scientific judgments independently and make them in a way that doesn't alienate you from the mainstream is one that I, too, have experienced. I write this book not to criticize scientists who have made choices different from mine but to examine why scientists need to be confronted with such choices at all.

In fact, it took me a long time to decide to write this book. I personally dislike conflict and confrontation. After all, in the kind of science we do, anything worth doing is a risk and all that really matters is what our students' students will think worthy of teaching their own students fifty years down the road. I kept hoping someone in the center of string-theory research would write an objective and detailed critique of exactly what has and has not been achieved by the theory. That hasn't happened.

One reason to take these issues public goes back to the debate that took place a few years ago between scientists and "social constructivists," a group of humanities and social science professors, over how science works. The social constructivists claimed that the scientific community is no more rational or objective than any other community of human beings. This is not how most scientists view science. We tell our students that belief in a scientific theory must always be based on an objective evaluation of the evidence. Our opponents in the debate argued that our claims about how science works were mainly propaganda designed to intimidate people into giving us power, and that the whole scientific enterprise was driven by the same political and sociological forces that drove people in other fields.

One of the main arguments we scientists used in that debate was that our community was different because we governed ourselves according to high standards — standards that prevented us from embracing any theory until it had been proved, by means of published the case in string theory. Despite the absence of experimental support and precise formulation, the theory is believed by some of its adherents with a certainty that seems emotional rather than rational.

The aggressive promotion of string theory has led to its becoming the primary avenue for exploring the big questions in physics. Nearly every particle theorist with a permanent position at the prestigious Institute for Advanced Study, including the director, is a string theorist; the exception is a person hired decades ago. The same is true of the Kavli Institute for Theoretical Physics. Eight of the nine MacArthur Fellowships awarded to particle physicists since the beginning of the program in 1981 have also gone to string theorists. And in the country's top physics departments (Berkeley, Caltech, Harvard, MIT, Princeton, and Stanford), twenty out of the twenty-two tenured professors in particle physics who received PhDs after 1981 made their reputation in string theory or related approaches.

String theory now has such a dominant position in the academy that it is practically career suicide for young theoretical physicists not to join the field. Even in areas where string theory makes no predictions, like cosmology and particle phenomenology, it is common for researchers to begin talks and papers by asserting a belief that their work will be derivable from string theory sometime in the future.

There are good reasons to take string theory seriously as a hypothesis about nature, but this is not the same as declaring its truth. I invested several years of work in string theory because I believed in it enough to want to try my hand at solving its key problems. I also believed that I had no right to an opinion until I knew it in detail, as only a practitioner could. At the same time, I have worked on other approaches that also promise to answer fundamental questions. As a result, I'm regarded with some suspicion by people on both sides of the debate. Some string theorists consider me "anti-string." This couldn't be less true. I would never have put so much time and effort into working on string theory, or written three books largely motivated by its problems, if I wasn't fascinated by it and didn't feel that it might turn out to be part of the truth. Nor am I *for* anything except science, or *against* anything except that which threatens science.

But there's more at stake than amity among colleagues. To do our work, we physicists require significant resources, which are provided largely by our fellow citizens — through taxes as well as foundation money. In exchange, they ask only for the chance to look over our shoulders as we forge ahead and deepen humanity's knowledge of the world we share. Those physicists who communicate with the public, whether through writing, public speaking, television, or the Internet, have a responsibility to tell the story straight. We must be careful to present the failures along with the successes. Indeed, being honest about failures is likely to help rather than hurt our cause. After all, the people who support us live in the real world. They know that progress in any endeavor requires that real risks be taken, that sometimes you will fail.

In recent years, many books and magazine articles for the general public have described the amazing new ideas that theoretical physicists have been working on. Some of these chronicles have been less than careful about explaining just how far the new ideas are from both experimental test and mathematical proof. Having benefited from the public's desire to know how the universe works, I feel a responsibility to make sure that the story told in this book sticks close to the facts. I hope to lay out the various problems we have been unable to solve, explain clearly what experiment supports and doesn't support, and distinguish fact from speculation and intellectual fad.

Above all, we physicists have a responsibility to the future of our craft. Science, as I shall argue later, is based on an ethic, and that ethic requires good faith on the part of its practitioners. It also requires that each scientist be the judge of what he or she believes, so that every unproved idea is met with a healthy dose of skepticism and criticism until it is proved. This, in turn, requires that a diversity of approaches to unsolved problems be supported and welcomed into the community of science. We do research because even the smartest among us doesn't know the answer. Often it lies in a direction other than the one pursued by the mainstream. In those cases, ence depends on healthy support for scientists who hold divergent views.

Science requires a delicate balance between conformity and variety. Because it is so easy to fool ourselves, because the answers are unknown, experts, no matter how well trained or smart, will disagree about which approach is most likely to yield fruit. Therefore, if science is to move forward, the scientific community must support a variety of approaches to any one problem.

There is ample evidence that these basic principles are no longer being followed in the case of fundamental physics. While few would disagree with the rhetoric of diverse views, it is being practiced less and less. Some young string theorists have told me that they feel constrained to work on string theory whether or not they believe in it, because it is perceived as the ticket to a professorship at a university. And they are right: In the United States, theorists who pursue approaches to fundamental physics other than string theory have almost no career opportunities. In the last fifteen years, there have been a total of three assistant professors appointed to American research universities who work on approaches to quantum gravity other than string theory, and these appointments were all to a single research group. Even as string theory struggles on the scientific side, it has triumphed within the academy.

This hurts science, because it chokes off the investigation of alternative directions, some of them very promising. Despite the inadequate investment in these approaches, a few have moved ahead of string theory to the point of suggesting definite predictions for experiments, which are now in progress.

How is it possible that string theory, which has been pursued by more than a thousand of the brightest and best-educated scientists, working in the best conditions, is in danger of failing? This has puzzled me for a long time, but now I think I know the answer. What I believe is failing is not so much a particular theory but a style of doing science that was well suited to the problems we faced in the middle part of the twentieth century but is ill suited to the kinds of fundamental problems we face now. The standard model of particle physics was the triumph of a particular way of doing science that came to dominate physics in the 1940s. This style is pragmatic on hard conceptual problems. This is profoundly different from the way that Albert Einstein, Niels Bohr, Werner Heisenberg, Erwin Schrödinger, and the other early-twentieth-century revolutionaries did science. Their work arose from deep thought on the most basic questions surrounding space, time, and matter, and they saw what they did as part of a broader philosophical tradition, in which they were at home.

In the approach to particle physics developed and taught by Richard Feynman, Freeman Dyson, and others, reflection on foundational problems had no place in research. This freed them from the debates over the meaning of quantum physics that their elders were embroiled in and led to thirty years of dramatic progress. This is as it should be: Different styles of research are needed to solve different kinds of problems. Working out the applications of established frameworks requires very different kinds of thinking — and thinkers — than inventing those frameworks in the first place.

However, as I will argue in detail in the pages to come, the lesson of the last thirty years is that the problems we're up against today cannot be solved by this pragmatic way of doing science. To continue the progress of science, we have to again confront deep questions about space and time, quantum theory, and cosmology. We again need the kinds of people who can invent new solutions to long-standing foundational problems. As we shall see, the directions in which progress is being made — which are taking theory back into contact with experiment — are led by people who have an easier time inventing new ideas than following popular trends and for the most part do science in the reflective and foundational style of the early-twentieth-century pioneers.

I want to emphasize that my concern is not with string theorists as individuals, some of whom are the most talented and accomplished physicists I know. I would be the first to defend their right to pursue the research they think is most promising. But I am extremely concerned about a trend in which only one direction of research is well supported while other promising approaches are starved.

It is a trend with tragic consequences if, as I will argue, the truth

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THE UNFINISHED REVOLUTION

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