

Adventures in Physics and Math

Edward Witten

From a young age, I was fascinated by astronomy. This was not exceptional because the late 1950's were the start of the space race and everyone was excited about space. I really cannot remember how much my interest in astronomy predated the space race. At the age of about nine or ten, I was given a small telescope (a 3 inch reflector) and one of the highlights of my growing up was to see the rings of Saturn. Something that puzzles me in hindsight is that in those days I thought Saturn was hard to find. By now, having assisted several children with their own small telescopes, I am quite aware that Saturn is one of the most obvious objects in the sky and easy to find from almost anywhere on Earth in any small telescope on any clear night when it is above the horizon. It will not look like it does in a professional photo, but it is a striking sight through any telescope (Fig. 1).

As a youngster, I dreamed of growing up to be an astronomer, but I was also very much afraid that by the time I was an adult, astronomers would have to live and work in space. This sounded dangerous to me. Looking at how things turned out half a century or so later, we see that space satellites play a very important role, but the astronomers who develop them and use them stay safely on the ground. I suppose that the repair of the Hubble space telescope by astronauts is one of the very few cases in which work related to astronomy has been done by humans actually working in space. Incidentally, though space telescopes play a substantial role, ground-based astronomical observations are certainly still very important.

At about age 11, I was presented with some relatively advanced math books. My father is a theoretical physicist and he introduced me to calculus. For a while, math was my passion. My parents, however, were reluctant to push me too far, too fast with math (as they saw it) and so it was a long time after that before I was exposed to any math that was really more advanced than basic calculus. I am not sure in hindsight whether their attitude was best or not. However, the result was that for a number of years the math I was exposed to did not seem fundamentally new and challenging. It is hard to know to what extent this was a factor, but at any rate for a number of years my interest in math flagged.

Eventually, however, I understood that math and theoretical physics were the fields in

which I had the most talent, and that I would really only be satisfied with a career in those fields. I was about 21 years old when I made the decision between mathematics and theoretical physics, and I made this decision based on very limited knowledge about either field. My choice was theoretical physics, in large part because I was fascinated by the elementary particles.

This was in the early 1970's. For a 20 year period, beginning roughly when I was born, there had been an amazing succession of discoveries about elementary particles. At the beginning of this period, the proton and neutron and the atomic nucleus had been the smallest things known. The modern concept of elementary particles barely existed. But beginning around 1950, there had been an explosion of discoveries. This had resulted mostly from new technologies, especially but not only the ability to make particle accelerators in which elementary particles are accelerated artificially to very high energies.

In short, when I started graduate school at Princeton University in the fall of 1973, the study of elementary particles had been in a state of constant tumult going back at least two decades. But beneath the surface there was the potential for change. What we now know as the Standard Model of particle physics had been written down, in essentially its modern form, in a long process that had been essentially completed, just a few months before I started graduate school, by David Gross, Frank Wilczek, and David Politzer. (David Gross was later to be my graduate advisor.)

The period of perpetual revolution in the world of elementary particles actually continued during my graduate days. One of the biggest discoveries of all was announced on November 11, 1974. This was the discovery of the J/ψ particle. Though its lifetime is far less than a nanosecond, it was astonishingly long-lived for a particle of its mass and type. It was such a dramatic discovery that it led to a remarkably speedy Nobel Prize for the heads of the teams that made the discovery, and people used to talk about the November revolution in physics. To those of you who are too young to remember the Cold War or who might want to brush up on your history books, let me just say that there is another event that used to be called the November revolution.

Anyway, by November, 1974, I had just about learned enough about elementary particles that I could understand what the excitement was all about and what people were saying, but not quite enough to participate prominently. After what looked to me like some

initial confusion, it was realized in a few days that the J/ψ particle was made from a new kind of quark. As I say, to me it looked like this realization followed a few days of initial confusion, but it may be that things were clearer even sooner to the senior professors, and their understanding just took a while to filter down to us students.

I have gone into so much detail about this to try to explain what my interests were as a graduate student in the mid-1970's. In short, when I was a graduate student, the era of perpetual revolution in particle physics was still in full swing. I assumed it would go on and I was hoping to participate in it. But in hindsight, the quick success in understanding the J/ψ maybe should have been a hint that the scientific landscape was going to change. In fact, it turned out that the surprising properties of this new particle made perfect sense in the Standard Model, and even had been predicted before, though I don't know how well-known the papers making this prediction had been. Certainly I had not known about them.

Meanwhile, I developed another interest as a student, which in a way contained seeds of some of my later work. Here I should explain to those of you who are not physicists that one side of what theoretical physicists do is to try to understand the laws of nature, and the other side is to try to solve the equations in different situations and work out predictions for what will happen. The separation between these two sides of the subject is not always so clear. For example, there is no hope to understand what are the correct laws of nature without at least some ability to solve them and find their predictions. But in practice, much of what physicists do is to try to understand the behavior of matter in situations in which, at least in principle, the appropriate equations are known. This can be easier said than done; for instance, it is one thing to know the Schrodinger equation, which describes the behavior of electrons and atomic nuclei, and another thing to solve the equations and understand the behavior of a piece of copper wire.

As a particle physicist, my main goal in principle was to understand what are the fundamental equations. However, the emergence of the Standard Model created a novel situation. Some very new fundamental equations were in the process of being established just as I began my graduate studies, and some of them were really very hard to understand. In particular, the Standard Model said that protons, neutrons, pions, and other strongly interacting particles are made from quarks, but no quarks were to be seen. To reconcile the contradiction, one had to believe that quarks are "confined," meaning that no matter how much energy one pumps in, quarks can never be separated. The catch was that the Standard

Model equations that are supposed to describe quark confinement are opaque and difficult to solve. So it was hard to understand if quark confinement would really happen.

Understanding quark confinement became my passion as a student and for a number of years afterwards. But it was a very hard problem and I did not make much progress. In fact, in its pure form of demonstrating quark confinement using the equations of the Standard Model, the problem is unsolved to this day. To be more precise, from large scale computer simulations, we know that the result is true, but we do not really have a human understanding of why.

I gained a couple of things from this experience, even though I was not able to solve the problem I wanted. One was negative. I learned the hard way what I regard as one of the most important things about doing research. One needs to be pragmatic. One cannot have too much of a preconception of what problem one aims to solve. One has to be ready to take advantage of opportunities as they arise.

Reluctantly, I had to accept that the problem of quark confinement that I wanted to solve was too difficult. To make any progress at all, I had to lower my sights considerably and consider much more limited problems. (As I will explain later, I eventually made a small contribution to the problem, but this was almost 20 years later.)

On a more positive note, in accepting this and making what progress I could on more limited problems, I began to get some experience thinking about what physicists call the strong coupling behavior of relativistic quantum systems—the behavior of these systems when the equations are hard to solve by standard methods. This experience became important in my later work.

Here I should again explain to those of you who are not physicists that when the coupling is weak, everyone who goes to graduate school in physics learns what to do. When the coupling is strong, a large variety of questions and methods come into play. As a result, I am not sure that there is any such thing as being an expert on how quantum systems behave for strong coupling, and in any event certainly I myself never have become such an expert. I have learned quite a bit while always feeling like a beginner.

In 1976, I completed my Ph.D. at Princeton University, and moved to Harvard

University for what proved to be four years of postdoctoral work. It was also a very eventful time personally. Chiara Nappi, whom I married in 1979, arrived at Harvard as a postdoctoral fellow at the same time that I did. We had met at a physics summer school in the French Alps in 1975. She was invited to Harvard by a distinguished mathematical physicist, Arthur Jaffe. Our first child was born while we were still at Harvard.

At Harvard, I learned a lot from many of the senior professors, originally the physicists and then some of the mathematicians as well. I do not want to go into too much technical detail but I will try to give a flavor.

One senior colleague at Harvard was Steven Weinberg, who was a Standard Model pioneer (and 1979 Nobel Laureate). There were certain fundamental topics in physics that I had had trouble understanding as a graduate student. I think Steve thought that many of the physicists had some of the same confusions I did. Whenever one of these topics came up at a seminar, he would give a small speech explaining his understanding. After hearing these speeches a number of times, I myself gained a clearer picture.

I also learned a lot from Sheldon Glashow and Howard Georgi. Glashow was a senior professor and another Standard Model pioneer and 1979 Nobel Laureate. Georgi was a junior faculty member, just a few years older than I was. In fact, office space was scarce at Harvard and Georgi and I shared an office.

Glashow and Georgi, among other things, were experts at building models to explain the results coming from particle accelerators. I learned a lot from them, and had the age of perpetual revolution continued, I probably would have aimed to learn to do what they did. But as I have already hinted, the nature of experimental progress was changing at just this time. Great advances have continued, in areas ranging from neutrino physics—which is well-developed here in Japan, by the way—to cosmology. Important new particles have been discovered, most recently the Higgs particle. But rather than perpetual revolution, the surprise coming from particle accelerators during these decades has been the fantastic success of the Standard Model. It works much better, and at much higher energies, than its inventors must have anticipated.

Although I certainly did not realize this at the time, the changing landscape meant that I would find more opportunity in somewhat different directions. That is why my interaction

with yet another senior physicist at Harvard, Sidney Coleman, proved to be important. He was a legendary figure for his insights about quantum field theory, and was the only one of the physicists I have mentioned who was actively interested in strong coupling behavior of quantum fields. The others appeared to regard such questions as a black box, not worth thinking about.

On a number of occasions, Coleman drew my attention to significant insights that I think I would otherwise just not have heard about, or at least not until long after. Often these insights involved fundamental mathematical ideas about relativistic quantum physics, or its relationship with other areas of modern mathematics. There are many topics that were important in my later work that I had simply no inkling of until I learned about them from Coleman. At the time I could not make much sense of what I was hearing, but luckily I remembered enough that it was useful later. Just to give one example, I can remember Coleman explaining to me an insight, originally due to the Soviet mathematician Albert Schwarz, that certain surprising results of physicists working on the Standard Model actually had their roots in the “index theorem” of Michael Atiyah and Isadore Singer. This was actually a major theorem of 20th century mathematics, but I had never heard of it, or even of the concept of the index, or of the names Atiyah and Singer.

I should explain that although in the 17th, 18th, and even much of the 19th centuries, mathematicians and physicists tended to be the same people, by the 20th century the two subjects had appeared to go different ways. This happened because mathematics made advances that seemed to take it far away from physics, but also because physics after around 1930 had moved in directions—involving relativistic quantum field theory—that seemed too difficult to understand mathematically.

My graduate education in physics had occurred at a time when there was not much engagement between cutting edge mathematics and physics. Like the other physics graduate students I knew, I had not learned the sort of things one would want to know if one wished to grapple with contemporary questions in mathematics. It was typical of a physics graduate education of the time that I had never heard of the Atiyah-Singer index theorem or most of the other things that I heard about from Coleman.

Developments such as the role of the Atiyah-Singer theorem made some of the most prominent mathematicians curious about what physicists were doing. I started talking a lot

to some of the mathematics professors at Harvard, especially Raoul Bott and David Kazhdan. I also got to know Michael Atiyah and Isadore Singer. Atiyah invited me to visit in Oxford in the winter of 1977–8 for the first of what turned out to be many visits. Atiyah and Singer were important influences in my later work.

I was interested in what the mathematicians had to say and I certainly learned a lot of new things. At the same time, I was reasonably skeptical of whether the mathematicians could shed light on the physics problems that I was interested in—especially the problem of quark confinement, which I mentioned before.

In fact, I wasn't entirely wrong in this skepticism. The renewed interaction between mathematics and physics that was developing in the late 1970's has proved to be far more robust and significant than I anticipated at this time, and also far more important in my own work than I expected. However, it has remained difficult for mathematicians to grapple with quantum field theory, and the importance of modern mathematics for physicists has come mainly because of new problems that have emerged from the physics.

Only gradually, I started to see a payoff from what I had been learning from the mathematicians. At first, each time this happened, it seemed like an exception. I will describe one of the first instances. This occurred soon after I had joined the faculty of Princeton University in 1980. I was interested in supersymmetric field theories, which seemed to have the potential to solve some problems left open by the Standard Model of particle physics. I was puzzled in trying to understand the nature of the vacuum in these theories. The behavior I found was simpler than seemed to be explained by standard ideas of physicists.

Trying to get to the bottom of things, I considered simpler and simpler models, each of which turned out to contain the same puzzle. After pondering this for a long time, I eventually remembered—I think while in a swimming pool in Aspen, Colorado in the summer of 1981—a lecture that I had heard by Raoul Bott about two years earlier. At a physics summer school in the French island of Corsica, Bott had given a group of physicists a basic introduction to a mathematical topic known as Morse theory. Morse theory had been fundamental in Bott's own work, but I am sure that just like me, most of the physicists at that school had never heard of it, and had no idea what it might be good for in physics. And I had probably not heard of Morse theory again until that day in 1981 when—dimly managing to remember part of what Bott had told us—I realized that Morse theory was behind what I

had been puzzling over.

A different way of saying much the same thing is that one could apply basic tools of quantum physicists to get a new understanding of Morse theory. My paper linking these two subjects is called “Supersymmetry and Morse Theory” (Fig. 2). My original motivation was to understand supersymmetry, but the paper has probably been more important for its influence on Morse theory. It is probably the first paper I wrote that is well-known in the mathematics world. Or at least it is better known than any of my other papers up to that point.

This is actually a typical example of how my work from that period—around 30 years ago or a little more—was related to mathematics. I was trying to answer physics questions; the possible mathematical interest was secondary. If the answer to the physics question shed some light on the mathematics, then this was a surprise. At first, the surprises seemed like isolated cases, not part of a pattern. It took a long time to really recognize that there was a pattern.

There is actually a human side to this that I would like to mention. Marston Morse, who had invented Morse theory starting in the 1920’s and 1930’s, was from 1935 on a professor at the Institute for Advanced Study in Princeton, where I work. He died at an advanced age in 1977 and I never met him. However, his widow Louise Morse remains well and active at the age of more than 100. I got to know her after my work on Morse theory. She has continued to host gracious receptions for the mathematical community in Princeton until very recently.

Before saying more about the relation of my work to mathematics, I should tell you about the major influence on my physics that I have not mentioned so far. This is String Theory.

The Standard Model of particle physics describes the interactions of nature that are important for elementary particles—what physicists call the strong, weak, and electromagnetic interactions. Left out is gravity. Gravity is important for stars, galaxies, and the Universe as a whole, but its influence for an individual atom or elementary particle is unmeasurably small (Fig. 3). The best description that we have of gravity is Albert Einstein’s theory of General Relativity. This was Einstein’s greatest creation, according to which gravity results from the curvature of spacetime. Einstein actually developed his theory of gravity

before Quantum Mechanics had emerged in its modern form. His theory is what we call a classical or pre quantum mechanical theory.

Updating Einstein's theory of gravity to take Quantum Mechanics into account has been a dream of physicists since the 1930's. But it is a tough challenge, and direct experimental clues are scarce or nonexistent, beyond the bare fact that General Relativity and Quantum Mechanics both exist and are important parts of the description of nature.

Both then and, in my opinion, to the present day, direct assaults on this problem have yielded little insight. This is actually an illustration of something that I explained before: the need to be pragmatic in research. One generally should not have too much of a preconception of what one is aiming to do. At a given moment, the time may simply not be ripe to solve a particular problem. Progress on something else may be needed first, and the key may eventually come from an unexpected direction.

In the case of Quantum Gravity, the unexpected direction was String Theory. The physicists who developed the roots of String Theory in the late 1960's and early 1970's were not thinking about Quantum Gravity. They were trying to understand the force that holds an atomic nucleus together. The theory that these physicists developed was incredibly rich. But it was not entirely correct as a theory of the strong interactions, and it went into eclipse following the emergence of the Standard Model. However, in the 1970's, a few physicists—among them Joel Scherk, David Olive, John Schwarz, and Tamiaki Yoneya—realized that String Theory might actually give the key to Quantum Gravity (Fig. 4).

The basic idea of String Theory, to state it naively, is that an elementary particle is not a point particle but a little loop of vibrating string (Fig. 5). If one asks why this simple idea leads to a deep theory, the best I can do by way of a non-technical answer is to remind you of the beauty of music. The many different ways that a string can vibrate lead to the richness of music and (if String Theory is correct) the unity of the elementary particles (Fig. 6).

By the early 1980's, Schwarz was developing this idea with Michael Green (and sometimes with Lars Brink) and they were obtaining very interesting ideas. I became aware of and fascinated by what they were doing. I was reluctant to be involved, in part because I thought that even if the theory is right, understanding it would be an incredibly long-term challenge. (In fact, among the criticisms of String Theory that are made by some prominent

physicists, I personally think this is the most cogent one, to the present day.) Still, I became fascinated, and for example I spent most of the summer of 1982 trying to learn the theory by studying a review article that Schwarz had written.

In 1982–3, though I remained reluctant to become wholeheartedly involved in String Theory, I did a few things that were relevant. The work of Green and Schwarz had the potential not just to combine General Relativity with Quantum Mechanics but to unify gravity with all the forces of the Standard Model. But I realized that there was a problem—a theory like they were building was going to have trouble accounting for the peculiar “handedness” of elementary particles. Technically, this was because of something called “anomalies.” They had to cancel to make the theory work.

I explored this question in a paper “Gravitational Anomalies,” with Luis Alvarez-Gaumé (Fig. 7). We were not able to solve the problem, but we did discover a related phenomenon—technically known as anomaly cancellation in Type IIB superstring theory. Without trying to explain this in detail, let me just say that, even before one tries to use it to make a theory of nature, the internal consistency of String Theory depends on a lot of miraculous-looking details that delicately hang together when one looks closely. This internal consistency is a remarkable story and it is one of the reasons for believing that String Theory should be taken seriously. By discovering anomaly cancellation in Type IIB superstring theory, Alvarez-Gaumé and I made our own small contribution to that story. However, the problem of handedness of elementary particles remained.

One day in the summer of 1984, I learned that Green and Schwarz had discovered a new way to cancel the anomalies. To me this was electrifying news. It had been clear to me for several years that the problem of anomalies was the main obstacle to make theories of physics derived from String Theory significantly more realistic. So it was obvious that one was going to be able to do much better, and this was certainly borne out by work in the following months.

Almost immediately, I wrote a short paper “Some Properties of $O(32)$ Superstrings,” explaining some of the most obvious points about why the new mechanism of anomaly cancellation made string theory potentially more realistic. But a few months later, with Phil Candelas, Gary Horowitz, and Andy Strominger, I wrote a more significant and satisfying paper “Vacuum Configurations For Superstrings” (Fig. 8).

This paper was interesting for physics because we showed how to get semi-realistic unified models of physics out of String Theory in a rather elegant way. It also had an impact on the relationship between physics and mathematics because the construction used rather modern ideas in differential geometry in an essential way.

In fact, once one took String Theory seriously, one soon found a lot of reasons that physicists would have to pay attention to previously unfamiliar topics in more or less modern mathematics. For example, at a basic level, a string moving in spacetime sweeps out a two-dimensional surface with the property of what mathematicians call a Riemann surface. Riemann surfaces are an important topic in the mathematics of the last century, and they became important for physicists primarily because of String Theory.

From my vantage point, all this made the interaction of physics with more or less contemporary mathematics far more robust and significant. Opportunities to apply physics-based insights to “purely mathematical” problems stopped seeming like exceptions.

In my own work in the years just after 1984, the development that is most worth mentioning here involves topological quantum field theory. This was partly motivated by hints and suggestions by the mathematician Michael Atiyah, who pointed to mathematical developments that he suggested should be better understood using physical insight. Other hints came from developments in physics.

Each problem here involved applying physics ideas to a problem that traditionally would have been viewed as a math problem, not a physics problem. These were all problems that I would not have seriously considered working on until String Theory broadened our horizons concerning the relations between mathematics and physics. In each case, the aim of my work was to try to show how a problem that naively is “purely mathematical” could be approached by methods of physicists.

I will just tell you about one of these problems. It involved knots in ordinary three-dimensional space. A tangled piece of string is a familiar thing in everyday life, but probably most of us are not aware that in the 1900's, mathematicians built a deep and subtle theory of knots. By the time I became involved, which was in 1987–8, there was a puzzle, which Atiyah helped me appreciate. The mathematician Vaughn Jones had discovered a marvelous new

way of studying knots—for which he later received the Fields Medal. Vaughn Jones had proved that his formulas worked, but “why” they worked was mysterious.

It may be hard for someone who does not work in mathematics or science to fully appreciate the difference between understanding “what” is true and understanding “why” it is true. But this difference is an important part of the fascination of physics and mathematics, and I guess all of science. I will say, however, that the difference between “what” and “why” depends on the level of understanding one has at a given level of time. One generation may be satisfied with the understanding of “why” something is true, and the next generation may take a closer look.

Anyway, getting back to knots, I was able to get a new explanation of Vaughn Jones’s formulas by thinking of a knot as the trajectory followed by an elementary particle in a three-dimensional spacetime. There were a few tricks involved, but many of the ideas were standard ideas of physicists. Much of the novelty was just to apply the techniques of physicists to a problem that physicists were not accustomed to thinking about.

This work became one of my best-known contributions, among both mathematicians and physicists. But it is also an excellent illustration of something I said in my acceptance speech the other night. No matter how clever we are, what we can accomplish depends on the achievements of our predecessors and our contemporaries and the input we get from our colleagues. My ability to do this work depended very much on clues I got from work of other scientists. In several cases, I knew of these clues because colleagues pointed out the right papers to me or because the work was being done right around the corner from me by colleagues at the Institute for Advanced Study in Princeton. It also helped at a certain point to remember some of what I had learned from Sidney Coleman back when I had been at Harvard, involving yet another insight of Albert Schwarz.

I have only had the chance to describe some highlights of my experiences in mathematics and physics until about 1990, but I think these highlights are reasonably illustrative. This was really my formative period. By this time, I had quite a bit of experience in doing research, and had developed something fairly close to my present outlook on many questions. I will not try to give a complete account of what has happened since then, partly because it would be too long and partly because we probably do not yet have enough perspective on some of the more recent developments. However, I want to say a little about

my work in the years 1994–5, which I regard as the most productive in my career.

By this time in my life, I had basically spent about two decades working on quantum field theory, but this work involved a sort of split personality. The early part of my career had involved conventional problems of particle physics, which in the Standard Model we understand in terms of quantum field theories in four-dimensional spacetime. Starting in the late 1980's, when I worked on problems like knot theory, I was basically studying somewhat similar field theories, though with rather different goals. The rest of my work was in string theory and largely revolved around a quite different and rather special class of quantum field theories, in two dimensions.

The bifurcation of my interests in two different classes of quantum field theories, studied with methods that are often quite different, felt a little strange. What happened in 1994–5 was that a larger perspective emerged in which all quantum field theories in all dimensions play a role. Many physicists contributed to this. My own contributions basically had two parts.

With Nathan Seiberg, I developed in 1994 what both mathematicians and physicists call Seiberg-Witten theory, though this phrase means rather different things to mathematicians and physicists. I want to explain this in some detail because it reflects the differences in outlook that still exist today between physicists and mathematicians.

To physicists, Seiberg-Witten theory was our new way of understanding how certain quantum field theories behave when quantum effects are big.

When one is doing research, the trick is to find a problem simple enough that one can solve it but interesting enough that solving it is worthwhile. Seiberg and I managed to do this by finding quantum field theories that were simple enough that we could solve them and subtle enough so that we could learn useful lessons by solving them. Among other things, Seiberg-Witten theory enabled me to finally make a small contribution to the understanding of quark confinement, as I had dreamed to do as a student. It is instructive to look back and realize how far out of reach this contribution would have been in my student days, when I had first worked on this problem. As I told you before, another lesson about research is to not have too many preconceptions about what one might be able to accomplish at a given point in time.

My work with Seiberg also had mathematical implications to the study of four-dimensional spaces. These implications are what mathematicians usually refer to as Seiberg-Witten theory. This is actually an interesting illustration of the fact that although in some ways, mathematics and physics have grown much closer together during my career, in some ways they remain far apart. The goals of the two fields and the tools that they can reliably use are quite different. Mathematicians using what are called Seiberg-Witten equations (along with other tools) have made wonderful discoveries in geometry, but usually without much reference to the quantum side of Seiberg-Witten theory.

The following year, drawing among other things on the experience from Seiberg-Witten theory, I proposed a somewhat similar picture of the behavior of String Theory for strong coupling, when the standard methods of calculation are not adequate. In doing this, I built on and extended the work of quite a number of colleagues. It will not be practical here to explain all of the clues, but I want to mention that at the annual string theory conference that was held in Berkeley in 1993, John Schwarz had been very excited about work he was doing with Ashoke Sen which in hindsight was on the right track. I had never seen John so excited except at the beginning of 1984, a few months before he and Michael Green made their epochal discovery about anomaly cancellation. So I listened carefully and this was one of the things that pointed me in the right direction.

At any rate, the picture that emerged was that all the string theories as traditionally understood are different limiting cases of a single richer theory, now called M-theory (Fig. 9). This theory is the candidate for superunification of the laws of nature. The traditional string theories are alternative descriptions of a bigger reality; they each tell part of a richer story. All quantum field theories in all dimensions are part of this richer story.

I have often been asked why I used the name M-theory to describe the richer theory that has the traditional string theories as limiting cases. M-theory was meant as a temporary name pending a better understanding. Some colleagues thought that the theory should be understood as a membrane theory. Though I was skeptical, I decided to keep the letter “m” from “membrane” and call the theory M-theory, with time to tell whether the M stands for magic, mystery, or membrane. Later, the membranes were interpreted in terms of matrices. Purely by chance, the word “matrix” also starts with “m”, so for a while I would say that the M stands for magic, mystery, or matrix.

Perhaps I should conclude by briefly explaining my view of the significance of the mathematical and physical work that I have been involved in. It actually is more simple to explain my opinion on the mathematical side. Quantum field theory and String Theory contain many mathematical secrets. I believe that they will play an important role in mathematics for a long time. For various technical reasons, these subjects are difficult to grapple with mathematically. Until the mathematical world is able to overcome some of these technical difficulties and to grapple with quantum fields and strings *per se*, and not only with their implications for better-established areas of mathematics, physicists working in these areas will continue to be able to surprise the mathematical world with interesting and surprising insights. I have been lucky to be at the right place at the right time to contribute to part of this.

On the physical side, though there is no definitive answer, there are many circumstantial reasons to believe that String Theory and its elaboration in M-theory are closer to the truth about nature than our presently established theories. String/M-theory is too rich and its consistency is too delicate for its existence to be purely an accident. Another strong hint is the elegance with which unified theories of gravity and the particle forces can be derived from String/M-theory. Moreover, where critics of String Theory have had interesting ideas, these have tended to be absorbed as part of String Theory. Finally, String/M-theory has repeatedly proved its worth in generating new understanding of established physical theories, and for that matter in generating novel mathematical ideas. All this really only makes sense if the theory is on the right track.



Fig. 1

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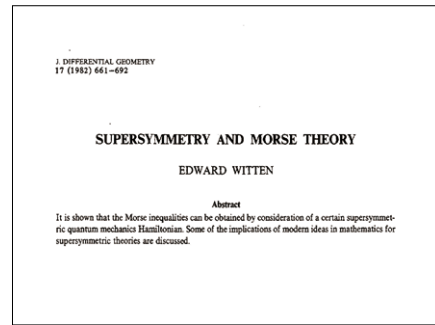


Fig. 2

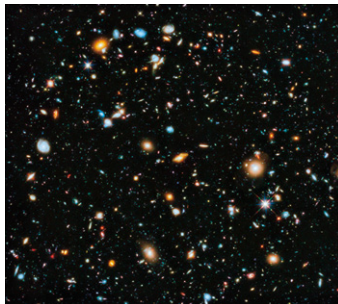


Fig. 3



Fig. 4

John Schwarz (left)

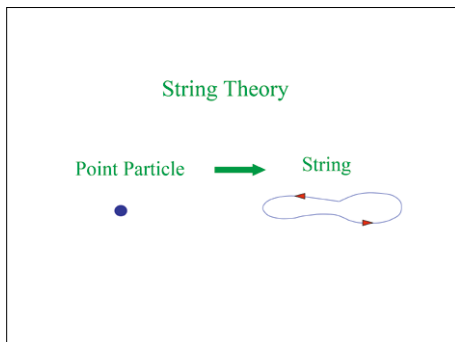



Fig. 5

Why does this lead to something deep?

The best answer I can give is to remind you of the beauty of music. Like a violin string, one of these strings can vibrate in many ways:



This leads to the richness of music and – if string theory is correct – the unity of the elementary particles.

Fig. 6

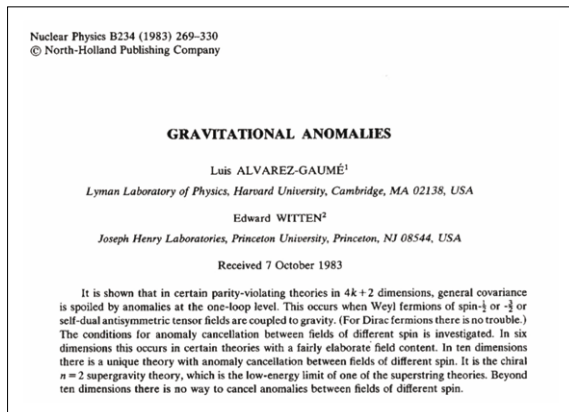


Fig. 7

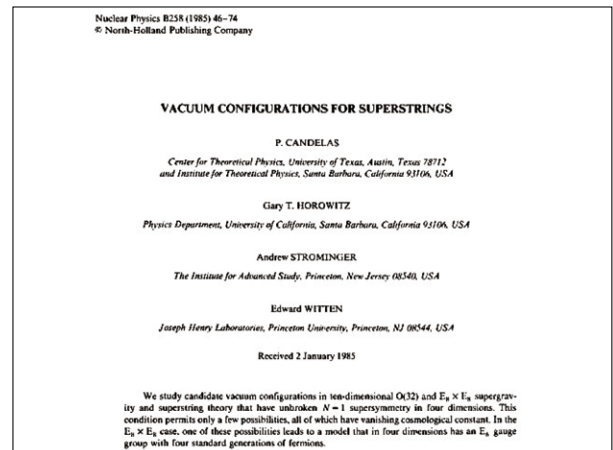


Fig. 8

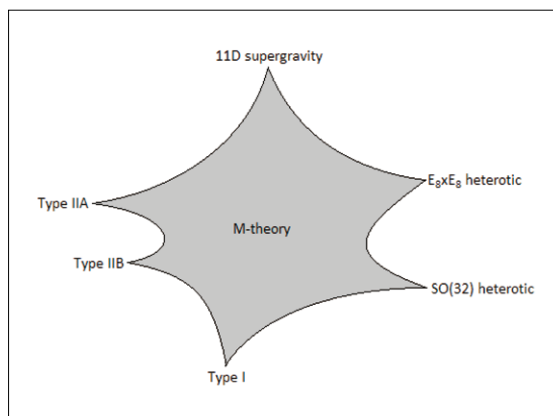


Fig. 9

wikipedia