7 AUSTIN PART 2, 1988–1992

7.1 THE BOOK

So in the summer of 1988, having realized that I would never be a great scientist, I decided to write a book.

This may come as a surprise. Weren't things going so well? Certainly, the problems that I was working on were fun, and occasionally I got positive feedback from others I respected. But I did not have a feeling that I was moving science forward. The great excitement of the day was connecting the heterotic string to the observed standard model, and I did not seem to have the particular tools for this. In fact, when I look back, I seemed to have worked almost entirely on what looked like oddities as compared to the real problem. The only string paper that was fairly well cited at the time was my first, on the Polyakov path integral, and that was almost all pedagogical. Meanwhile, many others were making what looked like major progress.

Certainly, the most notable of these was Edward Witten. For nearly ten years he had driven high energy theory forward with new ideas, the way that Feynman, Gell-Mann, Weinberg, Polyakov, and ’t Hooft had done earlier. I recall the pleasure, even before string theory came along, of reading each new paper by Witten and learning unexpected new aspects of quantum field theory. But at the same time, it was overwhelming.

In Feynman’s Nobel speech, he tells the story of poor Slotnick, whose just-finished PhD dissertation Feynman had reproduced, and more, in a single night. Not surprisingly, Slotnick never wrote another paper. And stories had it that Feynman affected others the same way. I have earlier mentioned that first meeting with Witten, which was a little bit like Slotnick’s meeting with Feynman. But I don’t think that Edward has ever shown the highly competitive streak of Feynman; instead, he is competing with history. But each new paper from him gave me the joy of reading, and the question, “why am I needed?”

On a smaller scale, I must have had some of this effect on my classmates at Caltech. But science is large, and they found their own directions. Happily, theoretical physics also turned out to be large, but I didn’t know it then. Before going on with the book, a few more bits of schadenfreude for you. I had recently seen the movie *Amadeus*, which (a bit inaccurately) described Salieri’s torment at being unable to match Mozart’s genius. So I empathized with Salieri. I also put a picture of Witten on the back of my office door, to desensitize myself for when we met. (Yes, I was such a goof.)

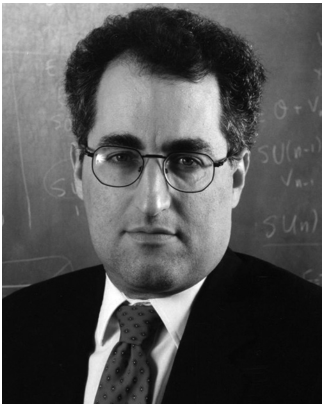


Figure 7.1
Edward Witten, 1996. The picture of Edward Witten posted on Joe's office door probably looked something like this. It's unlikely that it served its intended purpose. Courtesy of Randall Hagadorn.

The reason for my book was that I had just taught a one-year string course based on the Polyakov path integral. Green, Schwarz, and Witten (GSW) had just written a two-volume book on string theory but it did not include the Polyakov path integral, using mainly the older light-cone methods. I thought that in a year I could transcribe my course notes, avoiding too much repetition with GSW. People seemed to enjoy my writing, and I enjoyed it, though I did not account for how the effort would scale between a paper and a book. And I kept wanting to improve things, and string theory kept moving, so it ended up taking nine years. During this time I spent about 30 percent of each year on it, mostly in the summer. There was a break of a year when D-branes hit, but the next year I knew that I had to finish, and spent almost the whole year on it.

Figure 7.2
Edward Witten presenting Joseph Polchinski with Fundamental Physics Prize in Geneva, 2013. Courtesy of Stephen Parke.

Having admitted to channeling Salieri, I can also tell you about channeling Michelangelo. In the Michelangelo story, I cringed at the years that he spent on his commitment to making Pope Julius's tomb. How could he have wasted so much of his creative life? It was only long after finishing my book that I realized that I had done exactly the same thing.

I will not make much mention of the book as we go along. It just did not intersect much with the rest of my life, even my research. That seems surprising, but the book lagged the research. Just picture me slaving away, 30 percent of my time. But there is one bit of missed physics, right at the beginning, which I have always regretted. I was thinking again about the monopole catalysis problem, trying to improve the theory. I had an unusual effective field theory, with the effective fields lighter than the monopole but heavier than the others. I realized that this would arise in many situations, like heavy-light quarks, and even proton-electron. So I asked Mark Wise if he had seen this before. He said it was very interesting, and we should work it out. But I had just started the book, and was not ready to pause. So I left this to Wise and Nathan Isgur. The resulting heavy quark theory was very useful. So Wise and I joke that he gave me some projects when I got to Harvard, and later I paid him back.

Finally, three influences. Steve Weinberg, for setting the bar with his beautiful gravitation book, which I hoped to match. Initially, we talked about collaborating on a book, but it would have been very difficult melding my nonhistoric approach with his. The second influence was Edward Witten, for the reasons given earlier. The third was Jan Haag, who was our live-in nanny when my first son was one year old. She was an interesting woman who had traveled the world, and planned to write her own autobiography. So I figured that if my nanny could write a book, then so could I.

7.2 FUN WITH DUALITY

Reflecting now on my work in that period, I was doing in string theory much of what I had done in QFT as a student, trying to understand what the theory really was. Point particles in quantum field theory had been studied for a century. Infinitely thin relativistic strings were new. What special properties might they have?

A striking phenomenon special to strings was *T*-duality. If you put a particle in a box and make the box smaller and smaller, all that happens is that the excited states get heavier and heavier due to the momentum quantization. But for strings, after the box gets small enough, lighter and lighter states appear in the spectrum, and there is a perfect symmetry between a very large and very small box. Apparently, there was a minimum length, something one might expect in the ultimate short-distance theory. It was also an example of duality, the equivalence between the quantum theories of the large box and the small, but one that was visible even at weak coupling. And in more current parlance, it was an example of emergent space.

Almost all of the attention to this subject had been for the closed bosonic string (as a warm-up) and the heterotic string, as the putative theory of the real world. But there were other string theories, the open and unoriented bosonic theories and the SUSY type I, type IIA, and type IIB theories. And I had two new students, Rob Leigh and Jin Dai, who needed problems. Initially, I divided the strings between them, but before long it became one big project.

The IIA/IIB case was quickly solved: they are *T*-dual to each other, meaning that they are the same theory in different limits. The duality transformation flips the sign of one fermion, which flipped the IIA and IIB strings. It was a nice result, though later we learned that Dine, Huet, and Seiberg (DHS) had found this a few months earlier. But the rest of our papers were orthogonal.

The other cases were harder. We found that the *T*-duality flipped the normal Neumann boundary condition with the fixed Dirichlet condition. This change in the boundary condition meant that on the open string, the endpoints were no longer free to move in some directions, while the interiors, and the closed strings, were still free. After some thought, we realized that the string endpoints had to be stuck to some lower-dimensional object, with any dimension obtainable depending on the number of *T*-duals.[1]

So this was rather remarkable: starting with a theory of open and closed strings, one finds in the limit of a very small space a new large space, with closed and open strings, but also these new objects, one for each Chan-Paton factor. Moreover, we reasoned that due to gravity, these objects could not be rigid, and we identified the excitations in the string spectrum. So they were not stuck in the form given by *T*-duality, but could take any shape and number. The term *p-branes* had just been coined by Achucarro, Evans, Townsend, and Wiltshire to describe the membranes of supergravity,[2] so we called our new branes Dirichlet branes, or D-branes for short, to distinguish them. The fact that a *T*-duality produced something (the D-brane) from nothing (empty space) was nicely resolved by realizing that empty space was actually full of space-filling D9-branes.

T-DUALITY AND D-BRANES

The incredible richness of string theory owes itself to the extended nature of strings. The one-dimensionality of strings allows them to "feel" the geometry of spacetime in ways inaccessible to point particles. For instance, if space has a periodic dimension, such as the circular dimension of a cylinder, then a closed string can wind around it while a point particle cannot.

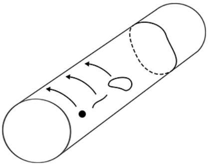


Figure 7.2a

The more the string winds around the cylinder, the “more string” there is and the heavier it gets. This contribution to the effective mass of the string is called *winding momentum*, and it comes in integer units of the cylinder’s radius divided by the square of the string length. If the closed string is also moving around the cylinder, then it has Kaluza-Klein (KK) momentum, which also contributes to its mass.

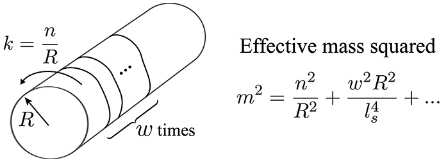


Figure 7.2b

This formula has a striking property: it can also be interpreted as the mass of a *different* closed string winding around a *different* cylinder!—the new string’s KK momentum is the original’s winding and vice versa, and the new cylinder’s radius is the inverse of the original’s in units of the string length. This is *T*-duality, the existence of equivalent descriptions of a closed string on the cylinder, uncovering a relation between physics in two distinct spacetimes! This duality relates different worldsheet QFTs, and hence extends to a duality between different superstring string theories. Another upshot of *T*-duality is that it sets up the string length as a minimum invariant length in the theory, since any smaller radius can be traded for a larger one.

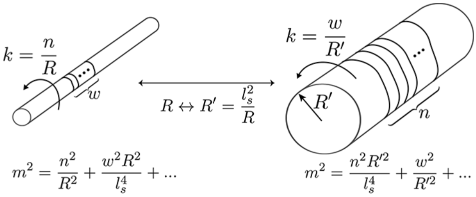


Figure 7.2c

T-duality also has surprising implications for the open string. A free open string on the cylinder can move around it with any amount of KK momentum, but necessarily has zero winding since it can always be unwound. After *T*-dualizing, however, the lack of winding is swapped with a lack of KK momentum, meaning that an object on the new cylinder is holding fixed the string's endpoints and stopping it from moving around the cylinder. With the endpoints restricted to move only along the new cylinder, the new open string will wind around with a winding equal to the KK momentum of the original open string moving around the original cylinder.

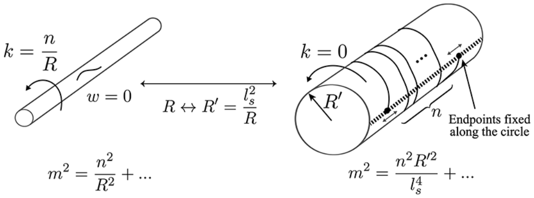


Figure 7.2d

That "object" holding on to the open string endpoints is nothing but Joe's D-brane! It is a dynamical membrane in spacetime on which open strings can end. Its shape is described by the number and configuration of the attached open strings. A D-brane interacts with its surroundings by exchanging strings, either by emitting closed strings when their open strings combine or by absorbing closed strings that break up into open strings on the brane. This interaction with closed strings, which includes the graviton, demonstrates that D-branes generate a gravitational field.

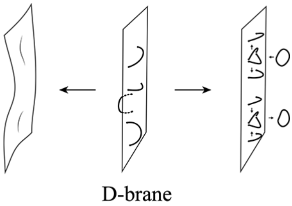


Figure 7.2e

D-branes have proven to be a crucial ingredient in the underlying structure of quantum gravity. Many insights have come from studying not a single D-brane in isolation, but rather a collection of them stacked on top of each other. The endpoints of an open string attached to a stack of D-branes are labeled by *Chan-Paton factors* indicating which members they're connected to.

Increasing the number of D-branes in a stack increases its mass and hence the strength of the gravitational field around it. This makes it harder for open strings to merge and escape the D-branes, and their low energy dynamics effectively decouple from the surrounding spacetime. Interestingly, the physics of these low energy open strings is described by a conformally invariant QFT, namely, a conformal field theory (CFT), with fields that are matrices instead of numbers. The rows and columns of these matrices are labeled by the Chan-Paton factors.

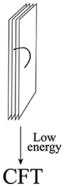


Figure 7.2f

The T-duality of the unoriented strings led to new puzzles, and a new object. We can think of *T*-duality as acting on the left- and right-movers as $(x_L, x_R) \to (-x_L, x_R)$, while the unoriented theory is defined by projecting on the orientation $(x_L, x_R) \to (x_R, x_L)$. Conjugating orientation reversal by *T*, we get $(x_L, x_R) \to (-x_R, -x_L)$. This new operation is equivalent to a worldsheet reflection times a spacetime reflection. The unoriented space is thus *T*-dual to an oriented string theory on a half-space. This new kind of boundary we christened an *orientifold*, because it was constructed from the product of an *orbifold* and an *orientation* reversal.

I seem to like simple compound constructions (Q-ball, D-brane, orientifold, and later *enhancon* and *discretuum*). I have a certain pride that other people would have discovered all these things, but by naming them I have put my stamp on them. But orientifold was almost a joke, such a clumsy word for something I thought that no one would ever find interesting. So today I give a private chuckle whenever I hear it.

The last *T*-duality we did not quite get right. The type I superstring had both D-branes and orientifolds, but we had mistakenly concluded that these were stuck together into a single object. This would be true for the minimal D-brane number (½ from the orientifolding), but with a higher number they had degrees of freedom that could move. But in all, this was pretty nice for a 10-page paper.

You might think that with this great set of insights I had made it, and I did not need to write that darn book (which was on hold anyway, because I was suffering from research-withdrawal after four months of writing). But I did not appreciate what I had done. I thought that the next step had to be finding the D-branes of the heterotic string, since this was assumed to be the real theory. A nice argument by Dixon, Kaplunovsky, and Vafa showed that the standard model could not be obtained from type IIA, B theories. But if I had any imagination, I would have realized that with the new possibilities from D-branes, the argument no longer held. But I persisted, fruitlessly, in trying to find heterotic D-branes.

I gave zero talks about the paper, my lack of confidence and common sense stopping me. I had forgotten Georgi's maxim, "Don't hide your light under a bushel basket." Had I given a few talks, someone in the audience, or even just the effort of writing the talk, might have led to the missing connections. I have mused over the fact that my paper with Yunhai had shown that the D9-brane sourced a 10-form RR potential, and the paper with Jin and Rob had shown that all the different D*p*-branes were connected by T-duality. But it took me six years to put these two together. Or more precisely, perhaps I knew the connection implicitly, but did not know what it meant; I needed someone to ask the right question.

As it was, the paper got two citations in its first five years. But there was one very nice and important paper, written by Leigh entirely on his own, working out the effective field theory for the D-branes. Leigh went on to some outstanding work, in strings, particle physics, and QFT, and is now a professor at Champaign-Urbana. Dai works in the IT industry in Shanghai, currently with a startup, and has over one hundred patents filed.

By the way, the title of this section was my intended title for the paper. But Rob is a serious guy and vetoed it, so we ended up with "New Connections between String Theories." Keeping score, of the five string theories, K. S. Narain had shown that the two heterotic theories were *T*-dual, we and DHS had found that the type II theories were *T*-dual, and we found that the type I theory was dual to type II, but with the ground state of type I dual to an excited state of type II, with D-branes. The fact that type I theory was dual to type II with D-branes meant that the D-branes were BPS states, something whose significance may not have been clear, but soon would be. By considering all the excitations of the D-branes, one could conclude that these were intrinsic excitations of the type II theory. The last connection, between heterotic and type I-II, would come not from a perturbative *T*-duality but from a nonperturbative one.

7.3 COSMOLOGICAL CONSTANT

I think I first heard about the cosmological constant (CC) problem during a lecture by Sidney Coleman on spontaneous symmetry breaking. Of course, the classic Einstein story was well known, but the full quantum problem, though known long ago to Pauli, had not penetrated into most discussions of QFT. But as Coleman explained, spontaneous symmetry breaking pointed up the reality of the vacuum, and that the ground state was not the most symmetric state. So it would naturally have an energy on the order of the spontaneous symmetry breaking scale. Moreover, even if canceled classically, it would get large quantum corrections.

I gave this a lot of thought as a postdoc, understanding why it was so hard to solve. Each particle generated a large quantum contribution to the vacuum energy, and somehow all these would have to cancel perfectly. Normally, one would need a symmetry to enforce this. Supersymmetry could do it, but it is a broken symmetry, so the cancellation should be inexact. One might also look for a dynamical mechanism, whereby the CC backreacts on the matter fields so that they cancel. But gravity is an irrelevant interaction (in the renormalization group sense). This means that a quantum effect at length l acts on spacetime at the much longer and weaker scale l^2/l_p (with l_p the Planck scale), much too small to cancel the CC. In effect, what was needed was some way for long-distance physics to feed back into the short-distance action.

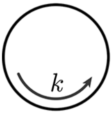
THE COSMOLOGICAL CONSTANT PROBLEM

The accelerated expansion of our universe points to the existence of a mysterious positive energy density permeating all of spacetime, known as the cosmological constant (CC). If the CC was zero then the expansion would not accelerate, and if it was negative then the universe would have a boundary. A CC can be incorporated into GR as a constant in the action integrated over all of spacetime.

$$S = \int d^4x \sqrt{g}\,\left[R - \underset{\substack{\uparrow \\ \text{Cosmological constant}}}{\Lambda}\right]$$

Figure 7.2g

The origin of the observed CC is a complete mystery. A natural source could be from the vacuum fluctuations of quantum fields. This is given by the total energy density from all particles running inside the loops of bubble Feynman diagrams. Summing these internal energies up to the highest energies where we can trust QFT, say up to a cutoff at the Planck scale where gravity becomes important, produces a CC that's proportional to that cutoff. This predicts a humongous value for the CC.



Energy density

$$\int_{-K_0}^{K_0} d^3k \left(\frac{1}{2}\,\underset{\uparrow}{\hbar\, w_k}\right) \sim K_0^4 = \Lambda$$

Loop particle energy

Figure 7.2h

Unfortunately, this predicted value clashes with the absolutely minuscule observed CC, differing by more than 120 orders of magnitude. This failure along with the general absence of a natural mechanism to generate a small CC is known as the cosmological constant problem.

$$\Lambda_{\text{predicted}} \sim M_{\text{Planck}}^4$$
$$\Lambda_{\text{observed}} \sim 10^{-124} M_{\text{Planck}}^4$$

Figure 7.2i

Most string theorists, having seen such remarkable properties as *T*-duality, expected that string theory had some trick that we had not yet figured out. So, like the renormalization problem before, the CC was one of the big questions that I always kept in mind. When I wrote my first string paper, on the Polyakov path integral, my first calculation was the cosmological constant.[3] It showed no particular suppression, although this was just for the toy bosonic theory. But even with (broken) supersymmetry, studied by others, no suppression emerged.[4]

At this time some new ideas emerged. These had nothing to do with string theory, but with pure quantum gravity. They led to great excitement for two or three years, and then it dissipated. Today it is not even mentioned to students; it is one of those subjects they can save some energy not learning.

I had thought this would be a good story to tell, but I have found it difficult. It is not so much fun to remember ideas that rather thoroughly did not work, even after great promise. So I will try to be brief. Actually, there are two stories. One, due to Coleman, was based on axion wormholes. The second, due to Hawking (and also Duff and P. van Nieuwenhuizen, and Aurilia, Nicolai, and Townsend) was based on four-form potentials.

The essentials of the Coleman story: Quantum gravity plus axions give wormhole solutions connecting different points of spacetime. At first sight, passing through these wormholes would destroy information, but Coleman, Giddings, and Strominger argued that summing over all configurations in the path integral made the wormhole (baby universe) coherent but random. All the constants of nature would get random contributions, but if one measured the constant repeatedly, the same value would be found everywhere. But by considering the full path integral, with arbitrarily many de Sitter regions coupled through arbitrarily many wormholes, Coleman found that the path integral was infinitely peaked at zero cosmological constant, Λ, $e^{e^{1/G\Lambda}} \to \infty$ as $G \to 0$.[5]

This was remarkable. And it fit the idea that long-distance physics needed to feed back to the short-distance action, the large Euclidean de Sitter space acting back on the baby universe action. But there was also doubt. The double positive double exponential was not like anything in field theory. One would like to derive the path integral from a Hamiltonian, and this would normally lead to minus signs, or even phases from the determinant. Several groups looked at this, including Willy Fischler, Igor Klebanov, Lenny Susskind, and me. Besides finding no evidence for a peak at zero CC, we fulfilled Lenny's ambition of getting the word *googolplexus* into the title of a paper (the number of states needed to get a small CC). And those who studied the predictions for other constants of nature found that they were unphysical or ambiguous.[6]

After a couple of years, the subject was dropped as uninteresting, and even now it is painful for me to try to reconstruct the arguments. I imagined that some features of this idea might return in the future, but they do not seem to have.[7] I will return to this in around sixteen years, book-time.

Hawking's idea was simpler, in that it used the quantum mechanics of de Sitter space but without the wormholes. He did add one more degree of freedom, a four-form field strength. In four dimensions, such a field is nondynamical: it would be constant over all of spacetime, but with an arbitrary value. If its value is integrated over the path integral, this will pick out a zero for the CC by a calculation similar to that for Coleman, but now with a single exponential $e^{1/G\Lambda}$.

But there was another problem. Even if all else worked, the mechanism would lead to an empty universe. A universe with excitations, especially a highly excited universe like ours, would be exponentially suppressed. So Fischler, my student Daniel Morgan, and I wanted to see if there was a process by which energy could appear from virtually nothing by tunneling.[8] This had already been argued by Farhi and Guth, based on a path integral saddle point, but there were subtleties. We confirmed it in a Hamiltonian treatment, but the effect was probably too small for our purpose.[9]

There was one other new CC idea out there, the anthropic principle. As soon as you go beyond the standard model to some form of unification, it often happens that there are mechanisms that allow the constants of nature to vary. We've seen two above, the four-form field and the Euclidean wormhole. Other possibilities would be a slowly rolling scalar (Banks), a downward rippling scalar (Abbott) or membrane (Brown and Teitelboim), or any kind of complicated potential with many minima. In these conditions, it may be that the constants of nature differ over time or space, or in branches of the wave function.

Weinberg, building on ideas of Linde and Banks, essentially said that this is all you need. Under these conditions, essentially all observers will see a very small cosmological constant. The argument is that for observers, or any kind of organized structure, to form, there had to be a lot of space (bits), and a lot of time (events), and this requires the CC to be very small. So if this *constant* is actually some sort of variable, the observers will only exist in the few regions of small CC.

One of Weinberg's striking abilities was to take some new idea, even a very radical one, and turn it into a calculation that could be tested. By replacing *observer* with *galaxy*, he could show that the CC could be no larger than around one hundred times the matter density, a large improvement over the prediction that it was set by the Planck scale 10^{120} or the weak scale 10^{60}.

This was remarkable to me, and upsetting. This problem that I was spending much of my time on, which was supposed to be the clue as to the nature of quantum gravity, did not need a solution; it was nearly automatic. But it required giving up the idea that finding the constants of nature, the lifetime goal for me and for my colleagues, was possible: it depended on details of astrophysics and partly even biology.

But Weinberg had a prediction: the CC had no reason to be exactly zero. Rather, it should be given by some random number less than one hundred times the mass density. Of course, the observed upper bound was already pushing down by one or two orders of magnitude, but one or two was much better than 60 or 120, and one could imagine a refined calculation.

And there were already some signs of a nonzero CC, such as the age problem (stars apparently older than the universe), which would be solved if there were a nonzero CC. So I spent the next ten years hoping that the evidence for this would go away. I do not know how many others were in the same state. To me, Weinberg's argument was so clear, and should have been known to everyone. But I had the benefit of talking to Weinberg in person, as well as my long history of unsuccessful attempts. Most others would find it easier to continue their denial.

My fretting would have been much better spent asking, does string theory produce the dynamics needed for Weinberg's argument? Fortunately, the question was still there for Raphael Bousso and me ten years later. It was a measure of the general *anthropic denial* that no one else asked this question first. And it is a tribute to Weinberg for his unique way of doing science, even asking questions that others might fear.

7.4 SCIENCE FICTION AND SCIENCE

Around this time, I saw a very interesting preprint by Kip Thorne and collaborators. General relativity has solutions with closed timelike curves, where an observer could return to a past event. They had a scheme for constructing these. Start with a wormhole with both ends near a point.[10] Then, boost one for a long time before bringing it back to rest. Due to time dilation, the times are shifted: an observer who enters the unboosted end leaves the boosted end in the past. Thorne, Morris, and Yurtsever wanted to see whether physics could make sense in such a space, perhaps even having observable consequences.

As a veteran reader of science fiction, I was well aware of the grandfather paradox, and I was sure that Thorne was as well. The observer could go far into the past and then kill his grandfather before he himself had been conceived. So he would not have existed to start the process and kill his grandfather. But there is a lot of free will involved here, so it did not make for a sharp argument. But I realized that one could easily do away with that. You can just replace the observer with a billiard ball, aimed so that it travels from the past through the future wormhole, and then leaves the past wormhole in just such a way as to intersect its previous path and knock it off course. Then it will never be there to pass through the wormhole. So there seems to be no consistent answer to whether the ball passed through the wormhole.

So I sent Thorne a message with this, and he got excited. His next paper was about motion in wormhole spaces. He had a possible solution to the conflict. If the ball was deflected just a little bit, then after passing through the wormhole it could meet its former past at just the right place to make it all consistent. He acknowledged me prominently for my note, which felt very good. But I wondered whether Thorne would be taken seriously after writing such papers, especially while he was trying to get the billion-dollar LIGO project approved.

THE POLCHINSKI PARADOX

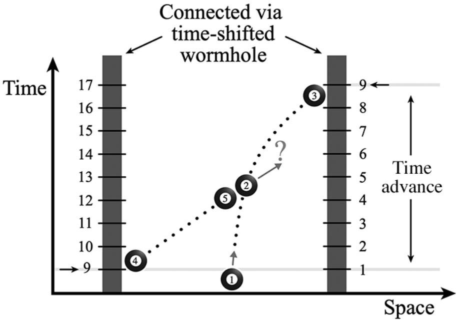


Figure 7.2j

I have named a number of things, but I do not have many things named for me. The Polchinski equation is just a refinement of Wilson's. So the *Polchinski paradox*,[11] a science fiction–motivated idea for which I wrote no papers, seems to be my claim to fame.

My other bit of science fiction was due to Weinberg. In his characteristic way, he had asked the question, how do we know that quantum mechanics is linear? So he designed a generalized theory with nonlinearity, satisfying certain consistency conditions. This could be compared with experiment, and he found that any nonlinearity had to be extremely small. I was studying the structure of his model and realized that the EPR problem was no longer avoided: information could be sent faster than light (Gisin discovered this as well). One could find a variant theory that avoided this, but the consequence was that different branches of the wave function could communicate. I summarized this by saying that Weinberg's original theory allowed us to build EPR-phones, which send messages faster than light, while the modified theory allowed us to construct Everett-phones,[12] which can communicate between different branches of the wave function. A few months later, this made it into a column in *Fantasy and Science Fiction*.

7.5 SHORT STORIES

7.5.1 Nonperturbative Strings

String theory, impressive as it was, was still just a perturbative theory. Finding the complete description was one of the big questions. Even for the standard model, we knew that nonperturbative effects led to rich physics and were essential to confinement, chiral symmetry breaking, and more. With quantum gravity added in, we could expect much more excitement in a nonperturbative theory.

The simplest guess would be to copy the structure of particle physics, replacing quantum field theory with some sort of string field theory. Effectively, this amounted to breaking the string worldsheet up into pieces corresponding to string propagators and interactions, which would follow from a string field action. But what worked for particles may not have been the right thing for strings. The classical action for closed strings required a sum over an infinite number of terms. Even worse, the quantum theory required an infinite number of additional terms at each order in $\hbar$. Effectively, the amplitudes were being put into the action by hand—it seemed to me more like an effective theory, or an action for a composite object like a hadron. There were some nice constructions in this approach, like Witten's open string action, Sen's soliton solutions, and Zwiebach's BV symmetry, but it did not seem like enough.

A different direction that came up at this time was a solvable matrix quantum mechanics, found by Gross and Migdal, Douglas and Shenker, and Brezin and Kazakov, which was equivalent to string theory in $1+1$ dimensions. In this low dimension, the only degree of freedom was a scalar, but the model was still rich enough to be interesting. In effect this was an early version of holography, a connection that Polyakov in particular emphasized later, with AdS/CFT. This was fascinating for me, and I wrote six or seven papers on it. These were mostly about the connection between the $1+1$-dimensional space and the matrix model, including the emergence of space and gravity, the finding of all classical solutions and some of their interesting properties, the attempt to go beyond $1+1$ dimensions, and some issues about the nonperturbative definition.

The most interesting lesson from this system came from Steve Shenker, who showed that nonperturbative effects scaled as e^{-C/g_s}, where g_s is the closed string coupling and C a constant. In quantum field theory, nonperturbative effects scale as e^{-C/g_s^2}. So string theory had stronger nonperturbative effects than quantum field theory. What could they be? Perhaps I should have read my own papers.

7.5.2 Working with Bryce

Bryce DeWitt had strong opinions. He was fun to talk with, about topology change (he was against), about the many-world interpretation (he was for), and more. I had the fun of joining a project with him.

Bryce, with the help of a large team of postdocs and students (Jorge de Lyra, See Kit Foong, Timothy Gallivan, Rob Harrington, Arie Kapulkin, and Eric Myers), was trying to directly answer the question of whether quantum gravity might be nonperturbatively renormalizable by directly integrating the path integral on the lattice. It was not clear that what he was doing made renormalization group sense, but it was Bryce's characteristic way to choose his direction and plow through it. Anyway, it was a hard question, and perhaps one would learn something in this way.

At least Bryce had made things easier by replacing the metric with an $O(2,1)/O(2)$ sigma model, with a lattice action he had determined through some reasoning of his own. So the path integral involved two fields in four dimensions. I noticed that for his specific action, the theory could be factorized into a free field and a self-interacting one. So half the integral could be done by hand, with half still having to be done numerically. This allowed for a lot of checks, and I was able to debug some of the team's long computer calculations using very simple ones. Most notable was one case where there was a large discrepancy. I realized that there was a Schwinger term that needed to be included, and then the numbers fell right in line. Bryce commented that he had never believed in Schwinger terms until he saw them in the numerical data.

And there was a conclusion: there was no high energy fixed point. I wonder whether there might be a useful confrontation between this and asymptotic safety.

7.5.3 Fermi Liquids

When I first learned about the Fermi liquid theory, I was puzzled by how one could neglect the electromagnetic interaction. This was driven home even more when I taught graduate quantum mechanics using Davydov's book, which went through BCS superconductivity in detail. It was claimed that one could calculate things like the gap with great accuracy, while ignoring seemingly much larger effects. It was said that this worked because we were working with quasiparticles, not electrons.

I had never encountered the word "quasiparticle" in QFT, and I did not know of any such method that would allow one to just ignore an interaction. All I knew was effective field theory, so I tried that, and it worked. The finite fermion density was unfamiliar to a relativistic theorist, but putting in the proper scaling made it just right. All interactions were irrelevant except the one producing the superconducting condensate, which was marginally relevant. So superconductivity was due to asymptotic freedom, just like confinement. What I had done was well known in terms of the Fermi liquid theory, but expressing it in the language of effective field theory made it more transparent to field theorists.

In my typical way, I was not planning to give any talks about this, or write a paper. But I was co-organizing the 1992 TASI with Jeff Harvey, and it was suggested that I give a couple of impromptu lectures. So I gave one lecture on how effective field theory works, including a very efficient summary of my renormalization proof. The second explained how Fermi liquid theory fit in this framework, including the treatment of the BCS theory. This has been a fairly valuable review, and I almost did not write it. I should not hide my light under a bushel basket!

At the same time, the problem of high-temperature superconductivity was a great puzzle. Its low energy behavior, such as the conductivity, did not fit the Fermi theory. The low energy interactions were larger, but there was no other stable low energy field theory known. So I thought, maybe my new understanding of Fermi liquids would solve the problem. I worked at it for several months, trying several things, but eventually decided that I had little to contribute. It seems that it is still a puzzle.

7.6 STUDENTS

Finally, let me remember my third triad of grad students, Eric Smith, Djordje Minic, and Makoto Natsuume. All three of them began working with me in Austin but finished after I moved to UCSB. Eric and Makoto came with me, while Djordje stayed with his wife in Austin. All of them worked on varied subjects. For most of you, this will be just a laundry list that you can skip. But I remember many of these projects with pleasure, and am happy to see that all these students are still doing science.

Smith's first project was to show that *T*-duality held for a class of time-dependent solutions, something that had not been obvious in the literature. He then worked out the light-cone action and spectrum for the $1+1$-dimensional string theory. We talked about my work on condensed matter, and he followed some of his own ideas and moved more in that direction. He is now at the Santa Fe Institute.

Minic wrote a couple of papers with me and a postdoc Zhu Yang on solutions to the $1+1$-dimensional string theory. He then worked with Duane Dicus on quark dynamics, on his own on the Luttinger liquid, and with Shyamoli Chaudhuri on 1 + 1-dimensional string black holes. It was a tough time to get a job, and Minic went through many postdocs before getting a faculty position at Virginia Tech, where he has been very successful. He and his wife Joy made many sacrifices, but his enthusiasm for physics was great, and it is wonderful that it worked out for them.

My first project for Natsuume was a follow-up on my work with Strominger on noncritical strings, showing that the effective field theory could be derived from a renormalizable one, and so confirming our construction. The second was a bit of speculation, seeing if he could make a generalization of the string S-matrix to higher-dimensional objects (not much success). On his own, he did nice work on the S-matrix of 1 + 1-dimensional string theory. He also worked out some challenging α' corrections to the Garfinkle, Horowitz, and Strominger (GHS) string theory black hole. And together, he and I understood gravity in the 1 + 1-dimensional string theory. Makoto ended up at KEK. Besides his research, Natsuume has written a number of popular and pedagogical books on string theory and AdS/CFT in Japanese.

7.7 FAREWELL TO AUSTIN

Dorothy and I were happy in Austin. Our two sons were born there, Steven in 1986 and Daniel in 1989, and were growing up with Texas accents (though they could drop these when they were just with us). We enjoyed life in Austin, apart from the weather. And both of us were in departments that we could thrive in. So we were in no hurry to look elsewhere, and a few times I got feelers but was not interested.

Figure 7.3
Polchinski family in Austin, 1992

But California was still home to us. Though we were each born about 2,500 miles away, in opposite directions, we met in California, and each of us had many formative experiences there. So if opportunities for both of us were available, we would be very tempted. But Dorothy's field in particular, German linguistics, was very small, and there were no prospects for openings in sight.

And then UC Santa Barbara came through. Universities back then were not as responsive to two-body problems as they are now. But UCSB had a drive to grow to the top. Dorothy's position was not as ideal as at Austin, going to a smaller department with interests less in tune with her own. But she could pursue her research, and over time she was able to build an impressive program. Of course, it put us closer to our families. And for me, it was a great opportunity, with excellent colleagues and a position at the Institute for Theoretical Physics, where I could focus on science.

So we did not hesitate for long. For me the most painful part was telling Weinberg. He is a great man, and he was proud of the group he had built.

Notes

1. Petr Hořava and Mike Green were both interested in these new *T*-dualities at around the same time, with Hořava's work in particular overlapping some of ours.

2. The word "brane," which first appeared in the title of that 1987 paper, has now appeared in 8,500 titles.

3. More precisely, what I was calculating was the dilaton potential, rather than a constant. There was a tendency to conflate these in the early papers, with the expectation that higher-order effects would fix the dilaton and produce a constant.

4. A very nice paper by Rob Myers showed that the special values of ten dimensions and zero vacuum energy were not actually required in string theory. With Shanta de Alwis, a postdoc who had come to work on Weinberg's generalized gravity, and Rolf Schimmrigk, a student of Candelas, we tried to generalize this to give small values of the cosmological constant and the SUSY breaking, with limited success.

5. [*G* stands for Newton's gravitational constant.—Ed.]

6. A fun, but ultimately uninstructive, toy model of quantum gravity was to treat the string worldsheet as a 1 + 1-dimensional spacetime.

7. [In fact, Euclidean wormholes have made a resounding comeback as the key ingredient for understanding quantum aspects of black holes.—Ed.]

8. Daniel also had some nice single-author papers, on forms of the renormalization group (showing Weinberg's and mine to be equivalent), and looking at the behavior of black holes with cutoffs. He went into public science policy after graduation.

9. There was another problem, which did not appear until much later. Note the resemblance between the $D=4$ four-form and the $D=10$ 10-form of my work with Cai. Hawking's form should be interpreted as a D-brane field, and therefore quantized, rather than the continuous value needed to cancel the CC.

10. We are now talking about wormholes that exist along the time direction, as opposed to the earlier wormholes that exist only for an instant.

11. [The Polchinski paradox is the billiard ball version of the grandfather paradox.—Ed.]

12. [Hugh Everett III was the one who proposed the many-world interpretation of quantum mechanics, the idea that different branches of the wave function correspond to separate parallel universes.—Ed.]