6 AUSTIN PART 1, 1984–1988

I have been dividing each section according to where I worked at the time. My stay in Austin ran for eight years, which would make for a much longer section. After some thought, I have come up with an event that singles out the midpoint of my time in Austin: it is when I started my book on string theory. So this separates parts 1 and 2.

6.1 THE GROUP

Austin had a strong history in theoretical physics. Alfred Schild, an early relativist who had recently passed away, had been a leader of the group. There was Bryce DeWitt, one of the first to develop quantum gravity, and a proponent of the many-worlds interpretation of quantum mechanics. John Wheeler had gone to Austin when he reached Princeton's mandatory retirement age (and he returned a few years later, when they changed their rule). George Sudarshan, codiscoverer of the V-A theory of the weak interaction was there, as was Yuval Ne'eman, co-discoverer of the $SU(3)$ color symmetry of the strong interaction. Duane Dicus, phenomenology and cosmology, and Richard Matzner, relativity, were also good colleagues.

A few years earlier, Weinberg and his wife Louise were looking to solve a two-body problem, and Austin was looking for ways to spend more of its oil money. So Weinberg and Louise were soon in Austin. Steve's salary was a subject of much speculation, but he never spoke of it. He also had an agreement that he could hire four faculty colleagues, with ample postdoc and student support.

His first three hires were Phillip Candelas (a relativist who would very soon be one of the leaders of the first superstring revolution),[1] Willy Fischler (my collaborator at Stanford, and one of the inventors of the invisible axion), and me. Vadim Kaplunovsky would join a few years later.

Weinberg had always been rather solitary. For example, most of his papers were single-author. But he was proud of his group. He instituted a weekly family meeting, as at Harvard, and he took his whole group to the faculty club every week. He tended to hold court at lunch, and I used to joke that he had three subjects of conversation: English history, Israeli politics, and DOS versus Windows. On the last, Steve was a notably text-oriented thinker; for example, he used very few figures in his books and papers, so he was one of the last DOS holdouts.

There was also a remarkably good group of grad students there, for a place rather out of the way. Perhaps the very low tuition—the same for in-state, out-of-state, and international—played a role.

Indeed, there were many international students. The students added greatly to the energy in the group. Finally, there was Adele Traverso, the group secretary. She was delightful but tough-minded, making sure to dress down each new group member for any infraction, so they would know who was in charge.

All in all, it was an exciting place to be.

6.2 WEINBERG AND PHYSICS

I had studied Weinberg's relativity book and papers at length, and heard some talks, but did not interact with him until Austin. I am embarrassed to say that my first impression of him was that he was a little slow: in talking with him, he seemed to get stuck on things that seemed obvious. But before long I realized that this was part of his genius. By not assuming things that everyone else took for granted, he would time and again discover possibilities that had been overlooked.

A minor example, which he was working on when I arrived, was *quasi-Riemannian geometry*. When gravity is written in terms of a connection, the curvature appears both in the metric and in the vierbein. Normally these are essentially the same, but he asked what happens if they are independent fields. As far as I know, this did not lead to much, though it did foresee some aspects of string compactification. But more interesting examples will come up later.

Weinberg's focus on his physics was famous. When he needed to learn something that I might know, he would question me in detail. But when my knowledge was exhausted, and I changed the subject, his eyes would visibly glaze over, and I knew that the meeting had ended. Many others had the same experience.

But I held nothing against him for this; this is what made him great. Even with his public interactions and other distractions that came with the 1979 Nobel, he continued to be creative. In the years after getting the prize, Weinberg published five papers with more than 1,000 citations, including one with more than 3,400. And over time I had ample opportunity to interact with him, as did all the group members and most notably the students.

6.3 STRING THEORY

Just as I was getting settled in Austin, the first superstring revolution struck. I had known very little about string theory before. When I went back to Caltech for a conference, John Schwarz told me I should read his latest papers. I tried, but it was all written in a noncovariant way, which I could not get past. Lenny told me that there was a new formulation of strings by Polyakov, which was more covariant, but it was a lot to absorb. And Witten was starting to write papers that hinted at string theory, such as his work with Álvarez-Gaumé on anomalies in higher dimensions, and his work on equivalence of different string actions under bosonization, which he presented at the same GUTs meeting where Rubbia and I spoke.

But it all came to a head in the fall of 1984, when Green and Schwarz found a new anomaly cancellation mechanism; Gross, Harvey, Martinec, and Rohm found the heterotic string; and Candelas, Horowitz, Strominger, and Witten found the Calabi-Yau solutions. Together, these gave a close connection between string theory and the standard model. I had spent the last several years on unification. My work was focused on supersymmetry, but I also informed myself about GUTs and Kaluza-Klein theory. Together they implied unification between fermions and bosons, between different gauge groups, and between gauge fields and gravity, while constraining the spectrum of particles. Moreover, these three ideas were nicely compatible with one another, and it was plausible that they were all part of some larger structure.

But there was one thing missing, even when all were taken together. Each had a lot of arbitrariness, in choice of gauge field, matter spectrum, masses, and coupling constants. A unified theory should have a uniqueness, and it was hard to see how this could come out of these frameworks. But string theory apparently did this. For example, there is no free gravitational coupling constant; rather, its value is determined by the value of the dilaton field. All other constants would be determined in the same way, as the values of fields, which are determined in part by field equations. So this does not solve everything, but rather transmutes it, from freedom in the theory to freedom in solving the field equations of a fixed theory.[2] This is the kind of progress one normally sees in physics, with equations that are often written in a few lines like Maxwell's or Einstein's, but have many solutions. But we will return to this later.

Looking over the papers I wrote while in Austin, the earlier characterization of my work still fits. Most of them seem to be written not to discover new things but to explain what we already know, perhaps in a clearer way. This led to a lot of fairly forgettable papers, but also some nice ones, though none that changed the direction of the field.

As I was learning string theory I first zeroed in on the question of why strings are forced to live in the critical dimension, twenty-six for bosons, when we knew that there were stringy solitons in any dimension, such as four. Of course, I realized that the stringy solitons were an effective description, valid only at long distance, while the critical strings presumably had zero width. But I guess I thought there should be some unified description of critical and solitonic strings. But after a few months I got stuck and moved on to greener pastures. Several years later, Andy Strominger was visiting and we started thinking about the puzzle again. This time we were clearer minded, and we found a nice construction in terms of conformal symmetry, which has been somewhat useful.

My next bit of self-pedagogy was the Polyakov path integral. Previous string amplitudes were based on light-cone calculations, but the Polyakov theory promised a covariant starting point. So I carried out the path integral: a straightforward exercise, but useful. My favorite part of it was that it allowed me to apply it to the

STRING THEORY

There are various indications that the framework of QFT is simply not powerful enough to describe gravity alongside everything else in a single consistent quantum theory. The leading candidate for this task is *string theory*, which is built on the idea that elementary particles are not pointlike objects as in QFT but are instead tiny, vibrating one-dimensional strings. These strings can either be *open* like a strand or *closed* in a loop, and together serve as the common origin for all different kinds of particles: each secretly a string vibrating at a different frequency. The most promising aspect of string theory is the automatic emergence of gravity, where the graviton is a vibrating closed string.

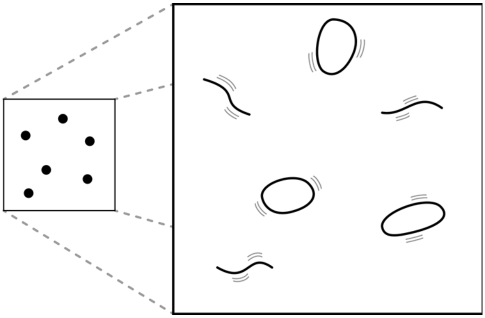


Figure 6.0a

While a pointlike particle traveling through spacetime threads out a *worldline*, a string sweeps out a *worldsheet*. When strings come together and interact, they do so smoothly as their worldsheets combine. These interaction vertices have a characteristic strength known as the *string coupling*. String theory avoids the infinities plaguing QFT precisely because the interactions are gradual, precluding the collision phenomenon of the instantaneous vertices of QFT.

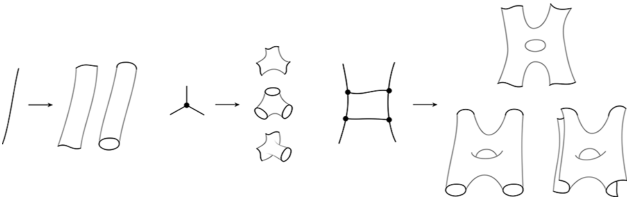
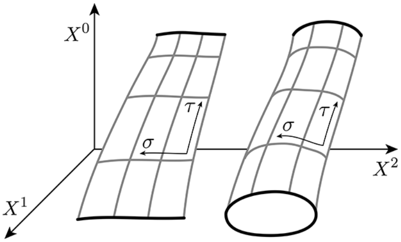


Figure 6.0b

The starting point in string theory is to define the shape and location of a string in spacetime. This means specifying the spacetime location of every point on the worldsheet, which is done through an *embedding coordinate field*—a multidimensional field with a component for each spacetime dimension. These coordinates include *bosonic coordinates* of the string worldsheet, which are the usual spacetime coordinates, and also *fermionic coordinates* that are not so easily illustrated.



Embedding coordinate: $X^{\overset{0,\ldots,9}{\mu}}(\tau, \sigma)$

Figure 6.0c

String theory is a theory of quantum strings. Quantum mechanics is infused into string theory by treating the embedding coordinate fields as quantum fields. The result is a two-dimensional worldsheet QFT with SUSY relating the bosonic and fermionic coordinates. In the simplest examples, there are five different ways of constructing this QFT, which lead to the five well known superstring theories. All of them contain precisely ten bosonic and fermionic coordinates, implying that spacetime is ten-dimensional.

$$S_{\text{WS}} = \int d\tau d\sigma \, [\underbrace{X^{\mu}(\partial_{\tau}^{2} - \partial_{\sigma}^{2})X_{\mu}}_{\substack{\text{Bosonic} \\ \text{coordinate fields}}} + \underbrace{\Psi^{\mu}(\partial_{\tau} + \partial_{\sigma})\Psi_{\mu} + \bar{\Psi}^{\mu}(\partial_{\tau} - \partial_{\sigma})\bar{\Psi}_{\mu}}_{\substack{\text{Fermionic} \\ \text{coordinate fields}}}]$$

Figure 6.0d

amplitude with no particles (vertex operators), thus determining the cosmological constant (which was nonzero in this bosonic theory) and the finite-temperature partition function. So I could connect some interesting physics to the Polyakov calculation. Several later papers also will make novel use of vacuum amplitudes.[3]

I had several follow-up papers with Andy Cohen, Greg Moore, and Phil Nelson, three of the outstanding students from my Harvard stay. We had talked a lot then, but did not write a paper together until meeting at a conference and finding a common interest in the Polyakov path integral. Our first project was to construct off-shell string amplitudes. We thought we had succeeded, but I don't think that we had gotten the gauge symmetry right, since we now know that only physical amplitudes are gauge invariant. Our most explicit example, where the ingoing and outgoing strings were contracted down to points, I think were in fact what we now interpret as D(-1)-branes, D-instantons. This began ten years of getting close to D-branes and not getting the point. Another paper with Moore and Nelson was an extension of the Polyakov path integral to the supersymmetric case, but here I was more of a follower.[4]

6.4 HUGHES, LIU, AND CAI

I had remained a postdoc for as long as possible, but now I had responsibilities. Supervising graduate students turned out to be a great thing. The common pattern with a student was that I would suggest an idea and we would meet weekly. Usually the idea turned out to be too hard for the student, so we would end up working together. I am pleased that with almost all of my students I ended up writing one or more significant papers. So the students got a great research experience, and many times I got to work out good ideas that I otherwise might have let slide.

My first three students were Jim Hughes, Jun Liu, and Yunhai Cai. The nine students that I supervised in Austin happened to come in groups of three, so I always think of them that way. Each had their own project, but they often ended up collaborating. Jun and Yunhai both came to Austin through the CUSPEA program run by T. D. Lee. This brought large numbers of excellent students from China to many US institutions for graduate work.

SUSY phenomenology was based on broken $N=1$ supersymmetry. There was an argument that one could not, staying within four dimensions, have a partial breaking such as $N=2$ to $N=1$. But I knew this had to be false, by construction. It was known that there were vortex solutions in which $N=2$ broke to $N=1$, found by Lancaster. These were not counterexamples yet, because they also involved Lorentz breaking from D to $D-2$. But by taking the low energy limit, this became $D-2 \rightarrow D-2$ while $N=2 \rightarrow N=1$, violating the argument. Of course, I am talking about BPS states, a universal idea now, but at the time it was rather new, and usually applied to monopoles rather than vortices.

So I gave Jim the problem of working out the four-dimensional action that breaks $N=2$ to $N=1$, with two dimensions as a warm-up. As would be the pattern with many of my students, this was too hard for him, but turned into a great joint project.[5] It was educational for both of us, learning the Volkov-Akulov treatment of nonlinear broken supersymmetry and the Green-Schwarz action. Although this was an explicitly QFT problem, it used many ideas from string theory.

Jim and I worked out the $D=4 \rightarrow D=2$ case, and then with Jun we extended it to $D=6 \rightarrow D=4$.[6] As we noted there, there were several applications: (1) We completed the construction of $D=4$, $N=2 \rightarrow N=1$ SUSY;[7] (2) we had found a new and more general form of the Green-Schwarz action, based on a scalar field rather than a vector field; and (3) this allowed us to construct supersymmetric membrane actions, 3-branes in $D=6$ being the case we studied.

I had a bad trait, sometimes, of not following through on my ideas. Having solved the original puzzle, we moved to new directions, such as writing the string field equation in terms of the renormalization group. These were fun, but not so notable. But Bergshoeff, Sezgin, and Townsend classified all possible supermembranes, finding that the maximum, 2-branes in 11 dimensions, was the same as the maximum dimension of supergravity. This led to parallel activity for several years, string theory and membrane theory, with little communication between them. Membranes could not be quantized the same way as strings, and so most string theorists, myself included, assumed that they were an aberrant offshoot of the real theory.[8] But those whose expertise was supergravity knew that there was something important there. Only with the second superstring revolution, eight years later, did the whole dual picture become clear.

With Yunhai, again the goal was to explain more completely something that was already known. The open superstring theory was shown by Green and Schwarz to be consistent only if the gauge group was $SO(32)$. They showed this by a calculation in the effective field theory of $N=1$, $D=10$ supergravity, where there was an anomaly unless the gauge group was $SO(32)$. But it should be possible to understand the anomaly in terms of the fundamental string theory, not just the low energy approximation. This is what Yunhai and I set out to do. The key terms in the string path integral were again of my favorite type, the vacuum graphs. Here there were three: the cylinder, the disk with a crosscap, and the sphere with two crosscaps. These summed to $(N-32)^2 \times (\infty)$. The infinity was from the volume of spacetime, times a normalization.

The three factors in the expansion of the square are from the graphs, with N counting the Chan-Paton factors at the boundary. It was natural to interpret this as the vacuum-to-vacuum amplitude for the dilaton. This was correct for the NS-NS (boson-squared) sector of the integrals, but there was an equal contribution from the R-R (fermi-fermi) sector for which there was no corresponding particle. This had to be a nondynamical 10-form field. We now know this as the form carried by the D9 brane.

I have two regrets about this work. First, the authors of the paper are "Joseph Polchinski and Yunhai Cai." In high energy theory the convention is nearly universally alphabetical. In this case, this started as a joint project, and Yunhai made some good comments and some calculations, but it quickly went much farther than the original idea. I was still rather solitary in how I worked, and when things got really interesting I would race through to the end. This happened here, so I ended up with a long paper written almost entirely by myself, and I did not see any other way to sign the paper. But this would do Yunhai no good, either pedagogically or when he went to apply for jobs. Of course, the right thing was to slow down just a little and give Yunhai a piece of the project that was his own. But I can say that I did learn, and became a good advisor before long.

The second regret is that I never gave a talk about the result: my shyness speaking about my work still lingered (I think that I rarely felt that my work was important enough). I was at a string meeting in downtown Santa Barbara around that time, and did not ask to speak. When I told Michael Green about the result, he said I should have spoken.[9] Indeed, the paper has received over 400 citations. But, like quite a few of my papers from that period (including the two with Jim and Jun), it got rather few at first, but then exploded after the second superstring revolution. Perhaps if I had been less shy about speaking, physics would have moved faster.

Jim, Jun, and Yunhai each did a few postdocs and then moved on to other things. Jim is at Microsoft, Jun got a second PhD in finance and is now a professor in this field at UCSD, and Yunhai became a magnet designer at SLAC. Even after the first superstring revolution, there were no jobs for string theorists. There was widespread doubt about string theory as physics, so that only a handful of places were willing to hire in it. It would be best at least if one had accomplishments both in string theory and in "normal" physics. Only after the second superstring revolution, when the web of connections emerged, did most departments feel that it should have a string theorist or two. Personally, I think young people should work on a wide range of problems, but I suppose that this is harder now as things become more specialized.

6.5 MORE FUN WITH PHYSICS, COLLEAGUES, AND STUDENTS

Conformal symmetry came to the attention of many of us through the Polyakov path integral, where it is part of the symmetry algebra. A conformal transformation is like a position-dependent scale transformation, and there was lore that any scale invariant theory would be conformally invariant as well. The argument for this seemed weak: the conformal algebra had more elements and so should have fewer invariants. So I set out to find the truth. There was previous literature on this, for classical field theory. But scale and conformal dimensions typically received quantum corrections, so I wanted a quantum argument.

In 1 + 1 dimensions it turned out to be quite easy to give a proof, using an important result by Zamolodchikov. He had shown that the scale transformation was monotonic, and a small twist of that

SCALE VS. CONFORMAL TRANSFORMATIONS

Scale and conformal transformations are generally different distortions of spacetime. A uniform inflating of all the coordinates is a scale transformation, while any warping that preserves angles is a conformal one. It should be noted that scale transformations are also conformal since they too preserve angles. It is a very surprising fact that some QFTs that are symmetric under scale transformations are automatically symmetric under the larger set of conformal transformations.

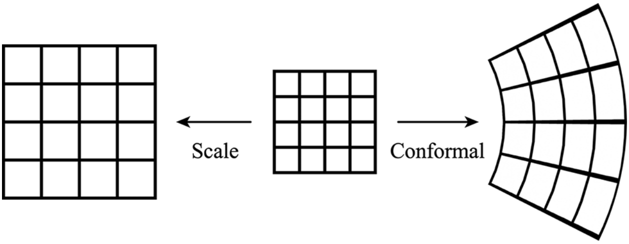


Figure 6.oe

gave the result that scaling implies conformal invariance. There were technical conditions, the most important being unitarity; without this there would be exceptions. I tried to find an argument in other dimensions, especially 3 + 1, but failed. I could find neither a proof nor a counterexample. This work attracted little attention at the time because it was not particularly relevant to string theory. But in later years there was renewed interest in such questions, and I will return to it later.

Most discussions of strings at the time dealt with low-lying states, small loops. But one could imagine highly excited states that were very long, perhaps spanning the universe. Cosmology could even lead to such strings being produced. Witten had recently considered this for the superstring and found several obstacles to their being produced. I will return to this later, but for now it was still interesting just to see how strings behave under various conditions.

An interesting question was, what happens if two strings cross? Do they pass through one another, or do they reconnect? My colleague Matzner was studying this question for cosmic strings from GUTs. He found that in this case, where strings were classical, they always reconnected. But for fundamental strings the answer would be more quantum mechanical, a probability for each outcome, and it was an interesting exercise to work it out. So the simplest way to address the question was to introduce some large periodic dimensions to wrap the long strings, and turn the problem into an S-matrix that was readily obtained. For my newest student, Jin Dai, I gave the same problem with open strings. Here there were two processes, an open string breaking in two (or the reverse), or a closed string breaking off from an open string.

The calculations were done, a good warm-up for Jin. They had modest relevance when cosmic strings came back for a while, but it mainly was "fun with strings."

Before moving on, I should mention what my colleagues were doing. Each of us was working on strings in their own way, and I benefited from all of them. Phillip Candelas was by far the most successful then, as one of the discoverers of Calabi-Yau spaces, a simple connection between string theory and the standard model. I should have understood his work better, but our approaches

COMPACTIFICATION

The best understood examples of string theory suffer from a dimensional surplus of six extra dimensions over the usual observed four. This can be dealt with by considering solutions of string theory where the extra dimensions are *compactified*—curled up into small compact shapes. Special shapes, such as Calabi-Yau manifolds, admit solutions with a symmetric four-dimensional spacetime, much like our own. Those solutions also feature some SUSY, which might be relevant for phenomenological applications.

Compactified spacetime

$$\underset{\substack{\uparrow}}{\mathbb{R}^{3,1}} \times \underset{\substack{\nwarrow}}{\mathbb{CY}^{6}}$$

$$ds^2 = \overbrace{g^{(3,1)}_{\mu\nu}\, dx^\mu dx^\nu}^{\substack{\text{4d flat}\\ \text{space}}} + \overbrace{G^{(6)}_{mn}\, dy^m dy^n}^{\substack{\text{6d compact}\\ \text{space}}}$$

Figure 6.of

This works for the same reason why we need bigger and bigger particle accelerators the deeper we try to probe into the structure of subatomic particles—higher resolution comes at the cost of higher energy. The same relationship also holds between the size of the compact dimensions and the minimum energy required to observe them. Just as you need to shine light on an object to see it, a compact dimension is probed by creating a particle that moves along it. According to Kaluza-Klein theory, the momentum of a particle moving along a periodic dimension is quantized in units of its inverse-radius. This momentum plays the role of an effective mass of the particle, and thus, by Einstein's famous relation E = mc^2, is a minimum energy cost to create it.

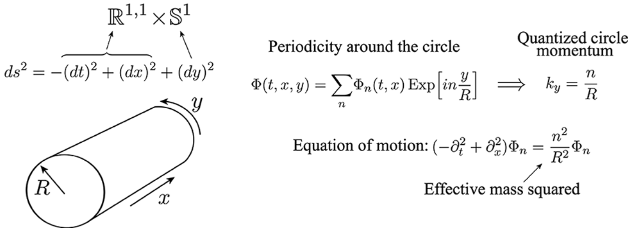


Figure 6.09

could hardly be more opposite, his geometric and mine field theoretic, with a minimum of geometry (wrapping a line in a periodic dimension was about my limit). While I was there he discovered mirror symmetry and also the first hint that all Calabi-Yau spaces might be connected through nonperturbative effects. These were fascinating, but I did not have the tools to follow.

Weinberg was trying to learn string theory much as I was, looking for simple calculations to do. I do not know why we did not work together; I guess neither of us played well with others (though I improved with time). But I did find his work interesting.

His first paper was to work out in detail the forms of the vertex operators, with attention to the normalizations and unitarity. His second was the bosonic open string theory, showing that it was finite for $N = 2^{13} = 8{,}192$ Chan-Paton states (though there would be higher corrections), the analog of the $N = 32$ that Yunhai and I studied. But string theory did not hold Weinberg's interest. I think it was because he wanted to derive the theory using the same principles as served him so well in QFT, but strings seemed to have new aspects that did not resonate so well with him. A conference talk he gave, "Strings without Strings," reminded me of his earlier philosophy that geometry is not central to general relativity.

Most early string work focused on supersymmetric states, where the dilaton would vanish order-by-order in string perturbation theory. But soon we would have to deal with states of broken supersymmetry, and the dilaton energy would have to involve cancellation between different orders. This would be easy to work out in field theory, but was clumsy without string field theory. So Fischler, with Susskind, showed how it worked, canceling string amplitudes against loop divergences.

So I learned a lot from my colleagues even when not working with them. I should also mention Clifford Burgess, Anamaria Font, and Fernando Quevedo, three of the international students, who independently wrote a very nice treatment of the low energy effective action of the superstring. All went on to successful careers in theory, with Fernando recently finishing a term as Director of the International Centre for Theoretical Physics in Trieste.[10]

Notes

1. Looking at the record, I see that Candelas was at Austin before Weinberg, so there must have been some negotiation that he would count as one of Weinberg's group.

2. Tom Banks emphasized this to me.

3. In passing I mention two other papers from this period on Polyakov path integral technology, one on the vertex operators and one on the factorization of the amplitudes.

4. In order to participate in the projects, I had to go to the computer center and get access to something called *bitnet*, which would allow us to communicate via our computers.

5. According to the acknowledgments, the problem was suggested by Luca Mezincescu, a postdoc recently arrived from Romania. I do not remember this, and do not know why he was not part of the collaboration. I think that early on I collaborated more easily with students than with postdocs, and I conjectured that this was because they were better at doing what I told them to do.

6. Jun was formally a student of Weinberg, but did all of his work with Jim and me, so I have always thought of him as my own student. But many students did get very good ideas from Weinberg.

7. A short explanation of how the no-go theorem could be violated is that the Haag-Lopuszansky-Sohnius argument on which it is based constrains possible symmetries of the S-matrix, but the action could have additional charges.

8. Michael Duff made the drive from Texas A&M to Austin to give us a review of membrane theory. In my wise-guy way I told him that I had only been joking when I invented supermembranes. To which he aptly replied "Many a true word is spoken in jest," an adage that apparently goes back to Chaucer. So be careful trading quips with Brits. And he was right about the physics, too.

9. This conference is famous for its banquet, which was so slow that one table phoned out for some pizzas to be delivered. It is also known for its after-dinner speech, in which Frank Wilczek explained why he should get the Nobel Prize, which he did fifteen years later.

10. I will not try to make a comprehensive list as I did for Harvard; it would be too hard. I will mention those I worked with in the text, but here are just a few others: Carlos Ordonez, Don Marolf and Scott Thomas (two who got away), and Brian Warr (who died too soon).

Memories of a Theoretical Physicist

A Journey across the Landscape of Strings, Black Holes, and the Multiverse

By: Joseph Polchinski

Edited by: Ahmed Almheiri

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