4 SLAC/STANFORD, 1980–1982

4.1 STARTING OUT AT SLAC

My postdoctoral appointment was actually at the Stanford Linear Accelerator Center (SLAC), about two miles from the physics building. Although this was an experimental lab, at the time most of the theorists were housed there as well. The theory director was Sid Drell, coauthor of the QFT text that I had spent so much time studying. My first meeting with Drell was a near-repeat of my first meeting with Thorne nine years earlier: "Hello, Professor Drell." "WHAT DID YOU CALL ME?" And so he was Sid from then on. Sid was very involved in arms control at that time, but did some physics in collaboration with his colleagues Helen Quinn and Marvin Weinstein on a variant form of lattice gauge theory. Their interests were not close to mine, but they were a friendly group and good to talk to. Stan Brodsky was another faculty member who was fun and energetic, but whose interests then were different from mine. But twenty years later I would remember it, and it would lead to a nice body of papers. There was also Fred Gilman, who had mentored Mark Wise to the Harvard Junior Fellowship, but whose interest in weak interaction phenomenology was not near mine.

I should also mention Stephen Parke, another postdoc who went on to do important work. We talked physics and socialized a lot, but did not work together—it would have been natural, but my collaborative sense was not yet developed. Some other members of the postdoc group, though more phenomenological, were Geoff Bodwin, Eve Kovacs, and Tom Weiler.

The postdoc years are a chance to learn new things. One should generally not just continue working on their dissertation problem. So of course, this is just what I did, for a while. I believed that I could prove what Mandelstam wanted to show, that quarks were confined with infinite strength. I had studied several important results in QFT where things could be proven exactly. Coleman's theorem, forbidding spontaneous symmetry breaking in 1 + 1 dimensions, was a prime example. I had the idea that in 3 + 1 dimensions, a similar mechanism would forbid free quarks. Streater and Wightman, proving the PCT theorem (parity × charge conjugation × time symmetry) and the spin-statistics theorem, gave further examples of the power of rigor.

I did get one little paper out of this. I was very interested in 't Hooft's classification of possible phases of the electric and magnetic fluxes. A lattice gauge theory model had appeared that seemed to contradict this classification. This should not be, and indeed, a closer look revealed that the modified theory had twice as many conserved fluxes as normal, two electric and two magnetic. When these were taken into account, 't Hooft's conditions were properly satisfied. The moral was that one had to be sure to include all conserved quantities to understand the phases.

This was a nice little paper, though not especially significant. Still, it should have gotten more than three citations in thirty-five years. But it would take me a while to realize that it is important not just to write papers but to give talks about them—not only to get attention, but to be forced to clarify your work, think it through, and get valuable feedback. Even eight years later, I gave no talks about the first D-brane paper. If I had, history might have moved faster.

But after a few months, I had not made real progress. Moreover, I came to realize that trying to prove things was not generally a profitable approach in QFT. It had seemed like a good idea. With the basic nature of QFT still apparently mysterious, making things rigorous would seem to provide a desirable base. But as I looked at other proofs, I realized that in many cases, what one could understand was often far greater than what one could prove. One example was Polyakov's argument that in 2 + 1 dimensions, instantons lead to quark confinement. The argument takes a few lines, and is convincing. But the proof, due to Gopfert and Mack in 1981, ran to one hundred pages. For confinement in 3 + 1 dimensions, it seemed likely that the proof would have to be much longer. My interest was the physics, the simple physics argument, not the one-hundred-page details.

Also, an argument by 't Hooft showed that proving confinement might be difficult. He was studying the phase structure of QCD.

He pointed out that if one could have a phase transition between states of free and confined quarks, as seemed to be the case, then there could not be a simple principle that said that quarks are always confined. So I was not completely without common sense, and started looking around for other directions.

4.2 SUSSKIND

About once a week, a whirlwind would settle on SLAC. Lenny Susskind and his group of visitors and senior postdocs (Willy Fischler, Peter Nilles, and Stuart Raby) would meet in Susskind's small office and talk physics for most of the day. Unlike most of the theorists, who were at SLAC, Susskind was officially at Stanford, but he had a SLAC office as well.

Where I was the extreme introvert, Susskind was the extreme extrovert. Even when I learned how to collaborate, my style was still to talk, perhaps for an hour, and then go away for a few days to think about things. Susskind, on the other hand, seemed to be able to work by talking, without a break, and to make progress in this way. In the many years that I have known him, he has almost always been surrounded by young people, talking through his current puzzle.

Although our personalities were very different, our interest in physics was much the same: we wanted to understand the basic principles. Neither of us was drawn to mathematics for its own sake: we used only enough to solve the problem at hand. Of course, my own approach was still developing, and was surely influenced by Susskind.[1]

4.3 SUPERSYMMETRY

What Susskind and his friends were excited about when I got there was *supersymmetry*. So I will start with a short review of grand unification and supersymmetry.

Both the strong and the weak nuclear forces had been understood around the time I got to Caltech. Together with QED, these three forces (or four, if you count the Higgs field as a force) seemed to account for all of particle physics, a theory known as the *standard model*. Of course, there was still gravity, but it is extremely weak on particle scales, and could be neglected at first. All three particle forces were based on the framework of QFT, and more specifically on gauge theories. These were like electromagnetism, but with the fields extended to matrices: 3×3 for the strong force, and $2 \times 2 + 1 \times 1$ for the combined weak and electromagnetic interactions.

The similarity of these forces suggested some more unified origin. Georgi and Glashow, in 1974, noted that the three forces fit nicely into a 5×5 matrix, *grand unification theories* (GUTs). Moreover, it made at least two predictions. One was the ratio of electromagnetic and weak forces, the weak mixing angle, which came out to pretty good accuracy. The other prediction was proton decay. The baryon number is a symmetry of the standard model, but not of the additional fields needed for 5×5. These fields mediate such processes as $qqq \to l$, turning three quarks into a lepton. This had not been seen, but seemed within reach.

Supersymmetry (SUSY) was another idea, which nicely complemented GUTs. With GUTs, there could be symmetries within spin 0 (scalar), spin ½ (fermions), and spin 1 (gauge fields), but

SUPERSYMMETRY

Everything in the universe is made up of particles that are either bosons or fermions, depending on whether their *spin*—an intrinsic type of angular momentum—is an integer or half-integer, respectively. Supersymmetry (SUSY) is the surprising feature of some putative theories of the universe where the two kinds of particles mirror one another in number and dynamics. A QFT is supersymmetric when its action remains the same under transforming the bosonic and fermionic fields into one another.

SUSY QFT	SUSY Transformation
$S(\Phi, \Psi) = \int \left[\partial\Phi\partial\Phi + \Psi\partial\Psi\right]$	$\Phi \to \Phi - \epsilon\,\Psi$ $\Psi \to \Psi + \epsilon\,\partial\Phi$

Figure 4.0a

not between different spins. A complete theory might be expected to relate different spins. A theorem showed that this was consistent with QFT—not a trivial result—and so this was beginning to be explored right around the time I was finishing at Berkeley.

SUSY actually solved at least three problems with GUTs. First, the weak mixing angle was a little bit off with GUTs. With SUSY added, it came out better. Second, searches for proton decay were starting to exclude the GUT prediction, but with SUSY the energy scale was higher and so the decay rate much slower. The third problem was more theoretical. In the standard model, all the interactions are dimensionless except the one that gives mass to the Higgs field. Quantum corrections (loop effects) will naturally generate a large scale for the Higgs, such as the GUT scale, much higher than the scale of the standard model where it is expected. But supersymmetry can lead to cancellations that do not follow from symmetry alone, and might cancel the mass correction to the Higgs. Of course, SUSY implied twice as many particles as had been seen, but this was consistent with supersymmetry breaking.

A narration from the future: For the most part I am writing this chronologically, but this requires a comment. As we all know, the last forty years have so far not discovered supersymmetry. In fact, the only *fundamental* discovery, in my narrow sense of the word, is the cosmological constant, which I will get to. But for now we will relive the glorious time in the past, when all things seemed possible.

4.4 *D*-TERMS

It was this cancellation of quantum corrections that drew the attention of many theorists, including Susskind and his friends. The latter's particular interest was the *D*-term, a sort of special mass term in supersymmetric theories (the *F*-term is the more common one).[2] Witten had shown that the corrections to the *D*-term canceled for any supersymmetric theory that was embedded in a nonabelian theory at high energy. If this was the whole story, it would imply that physics at low energy depended on the spectrum at much higher energies. This was not normally seen in QFT, and might have important consequences.

So Susskind and friends hung out in his office thinking about how to calculate the quantum corrections to the *D*-term. They were happy to have a newcomer listening in. I had taught myself how to do some of the main calculations in SUSY; it was clearly an exciting direction. And after a bit I was able to go from skulking to making suggestions. Before long we had solved the problem, and I had made substantial contributions.

The result was that the condition for the correction to D to vanish was simply that the sum of the charges of all the particles in the spectrum vanish, $Tr(Q) = 0$. This was a simple result, just five pages. It meant that some new high energy/low energy connection was not needed. It was my first real contribution to theoretical physics, and my first exposure to doing science collaboratively.

4.5 SUSY BREAKING

A few months later, Raby, Nilles, and Fischler left for faculty positions. Susskind and I continued to talk regularly, but neither of us had a particular project in mind. This changed when Mark Wise gave a talk at SLAC. Mark, along with Mark Claudson and Luis Álvarez-Gaumé, and in parallel with several other groups, was constructing realistic supersymmetric models of physics.

Susskind saw that their model was a good place to study the relations between three important scales. The first was the observed weak interaction scale. The second was the boson-fermion energy difference. This could not be too much larger than the weak scale, if the cancellations were to hold. The third scale was that set by the spontaneous breaking of supersymmetry. What Susskind noted was that in Wise's model, the boson-fermion difference and the supersymmetry breaking scale are not the same but differed by a parameter $\alpha = \mathrm{SUSY/boson\text{-}fermion}$. Susskind asked Wise if the parameter could be large, and Wise said it could.

Susskind wanted to know whether this situation was really stable, or whether quantum corrections would destroy the separation of scales. Happily, I was able to make substantial contributions. This kind of problem, with physics at several scales, is today routine, but at the time was rather new. But all my time thinking about QFT had prepared me for it.[3]

So we carried out the analysis, and indeed found that the multiscale structure was stable. This meant that whatever was responsible for the boson-fermion splitting at visible energies could actually arise at a much higher scale, such as $(\text{weak} \times \text{GUT})^{1/2}$, or even $(\text{weak} \times \text{Planck})^{1/2}$. Indeed, the idea of connecting SUSY with gravity led to much work in the following years.

One of the pleasures of the project was writing up the paper. Susskind is my only collaborator who has done this in real time, the two of us sitting in his living room, each with a small glass of wine, writing the paper line by line.

After this, I wanted to work out some more explicit models. But Susskind, having understood the key point, was not interested in details. So I wrote two papers on my own. They dealt with the spectra of gauge fermions, and of the scalar potential, in the models we had studied. Looking back on these early papers, I remember some nice bits that I am still pleased by today.

We did spend some time trying to make a lattice theory of SUSY. We tried a number of approaches, and I had thirty or forty pages of a paper written, but the two structures, SUSY and the lattice, did not want to come together and we did not finish it. Some of these ideas have been rediscovered and taken further, but they do not seem to be the best way to capture the rich strong-coupling dynamics of SUSY.

So the next papers Susskind wrote, the year after I left, were about gravity. This included his famous paper with Tom Banks and Michael Peskin, showing that black hole information loss, as suggested by Stephen Hawking, would imply large energy nonconservation.

4.6 TIME'S UP!

Two years, the standard term at the time for a postdoc, was really only 1.25 in postdoc years, because one had to apply for one's next job in the fall. At that point I had three papers: a condensation of my PhD dissertation (written just as I was leaving Berkeley), my little-known lattice phase paper, and my *D*-term paper with five authors. Unimpressive even for a first-time applicant, much less a second-timer. But again, I seem to have impressed my letter writers enough to get a position at Harvard. I wanted to spend a few years on the East Coast, to be exposed to new ideas and new people.

Dorothy had just finished her PhD. In her field it was typical to next do a few years of short-term teaching. She applied, and got a short-term position teaching German at MIT.

So two years after our wedding, we left the Bay Area and hopped into our Datsun B210 for the drive east. Along the way we spent several weeks at the Aspen Center for Physics. As anyone who has been there knows, it is a remarkable combination of science and recreation. I met many excellent scientists there for the first time. I actually wrote a paper there, in collaboration with Mary K. Gaillard, Larry Hall, Bruno Zumino, Francisco del Aguila, and Graham Ross, a distinguished and varied group. We spent the time at Aspen discussing our common interest, the mass scales of supersymmetry breaking models, and after two weeks we felt that we had enough that we could write it up.

Sidney Coleman, my soon-to-be supervisor, was also at Aspen then, so we got an early start on discussions of QFT. Sidney was an avid walker and hiker, a frequent visitor to Aspen, and so gave some of us first-timers advice on reaching the nearest 14,000-foot peak. This nearly led to Dorothy, me, Tom Weiler, and Lawrence Hall tumbling off Castle Peak with a large boulder, when we misinterpreted his instructions.

After Aspen, we had a memorable trip east: a hot drive through the Midwest (which Dorothy still jokes nearly led to a divorce), spectacular thunderheads not familiar to Californians, staying with my collaborator Raby and his wife in Ann Arbor, a drive across Niagara Falls, and a stay with Peter Galison (Junior Fellow in particle physics and history) while getting settled in Cambridge.

Notes

1. Dimitri Skliros recently noted that my work on perturbative string theory reminded him of Chern. Indeed, a short book by Chern was one of the few math books that I enjoyed, and even then I was not able to finish it.

2. The nonspecialist should not confuse this with the D of D-brane! Note that the font is different. D is short for Dirichlet, while as far as I know the *D* was an arbitrarily chosen label.

3. A few years later, Steven Weinberg commented that he could tell which parts of the paper were by me and which by Susskind. I was pleased because he had greatly influenced my understanding of the subject.