5 HARVARD, 1982–1984

5.1 WISE

I start with Mark Wise, my effective mentor. Mark was a great physicist and a wonderful person. As a student, he had already done important work on weak interaction physics, and as a postdoc he would have important results on supersymmetry and on cosmology. As a person, he was generous and self-effacing, and had a great sense of fun. When he turned sixty a few years ago, a time when most prominent scientists are celebrated by a major conference, Mark insisted that his birthday be celebrated by renting a skating rink and going curling.

My first meeting with Wise went something like this: "So you're Polchinski. We've been hearing about this guy who doesn't write any papers. Let's write a paper." And we did. Unlike Mandelstam, and Susskind, and I suppose myself, Wise did not insist that every paper be *important*. Of course, many were, but he was happy to think about any physics puzzle. His puzzle here had to do with the masses of the bosons and fermions in theories of broken supersymmetry. They need not be equal, but in many models there was a sum rule on the masses-squared, roughly $\sum_i b_i^2 = \sum_j f_j^2$. This had been shown in tree-level models and a few others, and the question was how general it was. So we analyzed the problem—it was a good blend of our skills—and found that the sum rule applied to first order in the supersymmetry breaking but exactly in the other interactions.

It was a nice result, finished, refereed, and in print two months after my arrival. However, it did not have much impact, as it did not apply to the most interesting models. In fact, it has received only three citations in thirty-five years, something that Wise chuckles over whenever we meet. But apparently I passed, and he said we should look for a bigger problem.

5.2 COLEMAN

There were only three tenured professors of high energy physics at Harvard, with Steven Weinberg having left for Austin.[1] Glashow did not interact much with students, so Sidney Coleman and Howard Georgi each supervised half the students and postdocs, meeting weekly as the "Coleman family" and the "Georgi family." Both were delightful people, interested in the students and postdocs and generous with their time, though I believe that Georgi was much more burdened by administration. As a rather formal student, I was in the Coleman family. But we could, and did, cross over to the other family when it was interesting.

Coleman was as much a delight in person as in his writing. He came to the office promptly at 1:00 p.m. each day, and stayed late. He was always ready to talk, but the highlight of the week was the family meeting. This might be a student or postdoc presenting his work for critique, or Coleman himself holding forth on some point, or a general discussion.

Coleman often took students and postdocs walking in the area, or hiking up Mount Monadnock in New Hampshire. He also had us over frequently for dinner. He was fond of telling us that in the last year he had just gotten married, bought his first house, learned to ride a bike, and been diagnosed with diabetes. I went with him on one of his early bike outings, helping him negotiate some of the difficult Cambridge turns.

I thought that it was unfortunate that Coleman did not get involved in supersymmetry, because it meant that we could not look forward to his insights on the subject. But perhaps he felt that there were enough people working on it already, and there were still interesting questions for him in pure QFT. Indeed, in the time I was there he expanded his overview of magnetic monopoles, worked with a student on the ’t Hooft anomaly cancellation, and discovered a surprising new class of topological solitons.

This last discovery has been widely influential for possible particle and cosmological models. I had a tiny role in it. Coleman was happily describing the construction, which was based on a charge he labeled q, and he was constantly referring to q as he described this round object. And then he said, “I just need a good name for it." Having listened to him at length, the only possible name came to mind at once: Q-ball (as in *cue ball*), and so it became.

5.3 THE KIDS RUN THE CIRCUS

Sometimes, one gets a group of young people who are so outstanding that they run the show. Such it was here. Sidney was excellent, but not interested in the latest sensation. But this was more than made up for by the students and postdocs. I still marvel over the excellence of that group, many of whom are still leaders today.

So in this section I am simply going to list the members of this outstanding group (those whom I remember), where they are now, and what they do.

Untenured faculty: John Preskill (Caltech), Lawrence Hall (Berkeley, phenomenology)

Junior fellows: Mark Wise (Caltech), Paul Ginsparg (Cornell, QFT and creator of arXiv), Lawrence Kraus (Arizona State, cosmology and public speaker/writer), Luis Álvarez-Gaumé (CERN, fields and strings), Peter Galison (Harvard, physics and history)

Postdocs:[2] Tadeusz Balaban (Rutgers, mathematical physics), Steven King (Southampton, phenomenology), Steven Sharpe (Washington, lattice), and me

Grad students:[3] Robert Brandenberger (McGill, cosmology), Andrew Cohen (Boston, strings and phenomenology), Stephen Della Pietra (Renaissance, mathematical physics), Jacques Distler (Austin, mathematical physics and strings), Ben Grinstein (San Diego, phenomenology), David Kaplan (Washington, QFT and nuclear), Greg Kilcup (Ohio State, lattice), David Kosower (Saclay, amplitudes), Anish Manohar (San Diego, phenomenology), Robert Mawhinney (South Florida, lattice), Ian McArthur (Western Australia, string theory), Greg Moore (Rutgers, mathematical physics and strings), Ann Nelson (Washington, phenomenology), Phil Nelson (Penn, mathematical physics and biology), Lisa Randall (Harvard, phenomenology), Richard Woodard (Florida, relativity).

It seems to me that this is a group of young people that has rarely been equaled. With all of them I can recall discussions of physics and other fun times. We will see many of them later in this and other chapters.

5.4 LOW ENERGY SUPERGRAVITY

Early attempts at constructing supersymmetric models were based on classical string actions. A Fayet-Iliopoulos *D*-term allowed for a simple means of SUSY breaking. Unfortunately, it did not seem to give the right symmetry breaking and spectrum.

The idea that supersymmetry could be broken at a much higher scale, as put forward by Susskind and me and several other groups around that time, allowed for other possibilities. The focus was on supergravity models, assuming the highest possible SUSY breaking scale. It was exciting to be thinking about a coupling between particle physics and (super)gravity, but it was very indirect. The gravitational field was very weak, and so played no direct role other than setting a scale.

So Wise, his previous collaborator Álvarez-Gaumé, and I set out to build a realistic model along these lines. We began with the simplest possible assumptions: (1) Begin with the standard model. (2) Extend to SUSY: this adds a field of opposite spin for each one present, and an extra Higgs multiplet. (3) Add soft SUSY breaking. Soft breaking means that one includes all gauge invariant terms with positive mass-squared. (4) This leaves dozens of parameters, so we adopted a simplifying assumption that had been introduced by Weinberg. The SUSY breaking was taken to be the same for all scalar fields. Weinberg came to this from a guess about unification. From the later context of string theory, one can say that it was not well motivated, but it did make things simple, and we followed it.

So, take this starting point, set the renormalization group running, and over a wide range of parameter space with no further choices, out came the standard model. The trickiest part was that one needed exactly one scalar, a Higgs, to get a negative potential and break its symmetry. But this happened automatically: a loop of a heavy fermions generates a negative mass-squared for a scalar coupled to it. The heaviest fermion was the top, so the scalar most strongly coupled to it broke its symmetry. It was very elegant: a large class of the simplest SUSY models flows exactly to the standard model plus broken SUSY.[4]

We followed this up with more detailed studies of the electric dipole moment and (with student Ben Grinstein) the decays of W and Z bosons.

5.5 RENORMALIZATION AND EFFECTIVE LAGRANGIANS

From Susskind, Wise, Coleman, and others, I had learned how to work on a variety of physical questions, and put aside the more formal ones that I had been drawn to as a student. But the questions were still there and one day came roaring back. John Preskill was teaching QFT, and I was sitting in the back, auditing the lectures on renormalization. At the end of the discussion of the cancellation of the infinities, he said, "I think there should be a better way to do this." And instantly I knew there was.

Again, what bothered me was that the proofs that renormalization works seemed extremely combinatoric and technical, but the results in the end came down to statements of dimensional analysis. What I realized was that things would become nearly trivial if, instead of describing the path integral order-by-order in perturbation theory, as nearly always done, we described it scale-by-scale in energy. As soon as I thought those words, I knew I could prove them. Of course, I mentioned earlier having put aside proofs, but this was a special case.

It took just three weeks for me to work out the proof and write it up. Probably it would have been better to take a little longer and make it a bit clearer, but I was afraid of being scooped—probably silly, but you never know. I think I presented it fairly well—first the idea, then a two-component model, then a precise statement of what was needed to be proved, and finally the proof. What made this proof clearly different is that it did not need graph combinatorics or Weinberg's theorem.[5]

Organizing the path integral scale-by-scale is messy: one has a differential equation that has to include not just the small number of normalizable couplings but the full infinite nonrenormalizable set. But this point of view was much more flexible than the traditional quantum field theory. In many situations one only knows what the theory is up to some energy—that is, it is an effective field theory—and this makes that notion precise.

This work was very exciting for me. For the first time, I felt that I had changed the way that people think about the world. Of course, aside from the details of the proof, most of the ideas were already known, especially from the work of Wilson and of Weinberg.[6] And in the next few years the idea of an effective field theory would become universal.

In my paper I thanked three names for inspiration. The first was Dan Friedan, for teaching me about the idea of effective field theory. The second was John Preskill, for his comment in class. If not for that, I might never have put together what I knew. The third was C. Arabica. I am disappointed that no reader of the paper has ever asked who that is. In fact, C. Arabica is the caffeine plant. I was distinctly buzzed that day in John's class, and that too played an essential role.

This work led me also to discussions with Tadeusz Balaban, a postdoc in Arthur Jaffe's group. Balaban was in the middle of proving asymptotic freedom, which he eventually did over the span of several *Communications in Mathematical Physics* articles adding up to three hundred pages. One could say that it is a landmark result, but it is almost unknown. It is another example where a convincing one-page physics argument can require orders of magnitude more to prove.

WILSONIAN RENORMALIZATION

Only once a theory is able to describe all processes involving any energy and at all levels of precision can it be considered a complete theory. Such theories are called *ultraviolet* (UV) *complete*, and renormalizable QFTs are examples of them. In contrast, nonrenormalizable QFTs can only describe processes with low enough energy, and hence have to be defined with a upper limit on the energy, known as a *UV cutoff*. These theories stop making sense at energies comparable to this UV cutoff, at which the theory breaks down and a new, more fundamental description needs to kick in.

UV cutoff

Parameters labeled by the UV cutoff

$$\Phi(x) = \int_{-K_0}^{K_0} d^4k\, \tilde{\Phi}(k)\, e^{ikx} \qquad S_{K_0} = \int d^4x \left[\Phi\left(\partial^2 + m^2(K_0)\right)\Phi + \lambda_4(K_0)\Phi^4 + \lambda_6(K_0)\Phi^6\right]$$

Figure 5.0a

This understanding stems from how a renormalizable theory behaves when focusing on low energy processes. When doing so, one can forget about the high energy fluctuations of the fields while allowing for their influence on the physics of the low energy particles. This "forgetting" can be implemented systematically in the path integral of a QFT by *integrating out* the field fluctuations of energy above some UV cutoff. This procedure takes the original UV cutoff, which could have been finite or infinite, and lowers it to a new, smaller value. Not only that, but the process also generates a new *effective action* for the low energy physics containing an infinite number of new, nonrenormalizable interactions.

Lowering the UV cutoff

$$\int \prod_{|k|=0}^{K_1} d\tilde{\Phi}(k) \underbrace{\prod_{|k|=K_1}^{K_0} d\tilde{\Phi}(k)}_{\text{Modes between the two cutoffs are integrated out}} \; e^{-S_{K_0}} = \int \prod_{|k|=0}^{K_1} d\tilde{\Phi}(k) \; e^{-S^{\text{eff}}_{K_1}}$$

Effective action

$$S^{\text{eff}}_{K_1} = \int d^4x \left[\Phi\left(\partial^2 + m^2(K_1)\right)\Phi + \lambda_4(K_1)\Phi^4 + \lambda_6(K_1)\Phi^6 + \underbrace{\lambda_8(K_1)\Phi^8 + \ldots}_{\text{Generates all possible interactions consistent with the symmetries}}\right]$$

New modified values

Figure 5.0b

The coupling constants of the effective theory at low energies differ from those in the original theory, and flow to new values as the UV cutoff is lowered. This flow is governed by the so-called β-function equations, which collectively define the *renormalization group* (RG). Renormalizable couplings tend to grow and become more important at low energies, and hence are sometimes called *relevant* couplings. On the other hand, nonrenormalizable couplings tend to forget their initial values at high energies and approach nonadjustable values determined by the renormalizable couplings. For this reason, these couplings are often called *irrelevant* couplings.

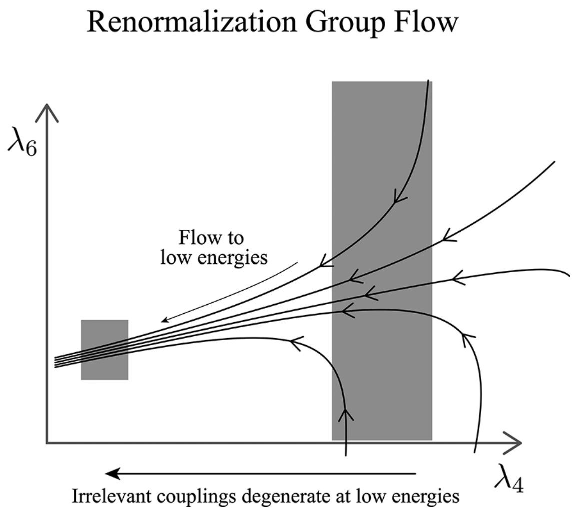

Figure 5.0c

5.6 MONOPOLE CATALYSIS

I am not going to discuss every paper I've written, but only those that have some story. The paper that I wrote right after the one on renormalization did not have much impact, but there are a couple of stories.

Over the years, a number of physicists have commented to me that my papers are distinctive, in that many of them are written not to discover new things but to explain what we already know in a new way. The renormalization paper is a prime example. It is not that I always set out to do this, but that I have to understand things, and can't proceed if I don't. My next paper was of the same type.

Grand unified theories like $SU(5)$ will have magnetic monopoles, as shown by 't Hooft and Polyakov. These are soliton states, not pointlike but with a size set by the GUT scale. We know that baryon number is not conserved in GUT theories, so there should be baryon number–violating scattering processes involving monopoles. The surprise, as shown by Valery Rubakov and Curt Callan, was that the rate for this process was determined not by the GUT size but by the much larger baryonic size.

This conflicted with my understanding of soliton amplitudes. The symmetry (anomaly) argument of Rubakov and Callan was too indirect for me, but eventually I found a toy model that allowed me to understand the details of the process, and see that they were right. Normally, I have a pretty good intuition for QFT, but Rubakov has twice done things that I thought were impossible.

The other came a few years later, also with baryon number violation. 't Hooft had shown that instantons could violate baryon number, but it was very slow, taking more than the lifetime of the universe. This seemed like a simple calculation with the Euclidean path integral. But Rubakov and collaborators showed that *if you heat the system up*, then rather than the very slow Euclidean tunneling process, you could move thermally with little suppression. Again, this seemed to conflict with my intuition, and again I wrote a paper, with collaborators Michael Dine, Willy Fischler, Olaf Lechtenfeld, and Bunji Sakita, to explain it to ourselves.

So don't bet with Valery Rubakov on quantum field theory! (Another surprise from Rubakov and collaborators was the *braneworld*, well before it became popular).

5.7 PHENOMENOLOGY

I had chosen a fortunate time to be a postdoc at Harvard, and do my one stint of phenomenology at a place where the lines between theory and phenomenology where particularly thin. During the time I was there, I had the excitement of seeing the discovery of the 40 GeV top quark, supersymmetry, and the $\zeta_{8.3}$. The less credulous readers will point out that the top was discovered more than ten years later at 170 GeV, supersymmetry has not been found, and *what* is the $\zeta_{8.3}$ anyway? Such was life in the Wild West (or should I say Alternate Truth?) days of particle physics.

The culture at Harvard seemed to be that one expected most new experimental observations to be wrong, so you should write your papers about them before they are withdrawn. There were even a couple of maxims about this from Howard Georgi: "Not more than one half of an idea per paper," and "Don't hide your light under a bushel basket." The latter was apparently a biblical injunction against slow publication.

In the case of the 40 GeV top quark, our result actually appeared at the same time as the experiment, and did not agree with it. At the fourth annual Supersymmetric Unification workshop in Philadelphia, Carlo Rubbia presented his evidence for the discovery of a 40 GeV top quark at CERN. I reported on our minimal SUGRA model. It favored a heavy top, around 150 GeV, though this was rough because other parameters could be tuned. I said in my talk that our result argued against the 40 GeV top, but the ideas of a random theorist had no weight compared with an experimental result. This result was soon withdrawn, I believe due to a better understanding of the statistics. But our model also did not last long. The very minimum assumptions that we had made put an upper bound on the superpartner masses, which was soon ruled out.

Shortly after, Rubbia reported evidence for several monojet candidates, just a single jet with unbalanced momentum. The missing momentum could be carried by various kinds of new particles, particularly a gluino or squark pair. Assuming that we were seeing superpartners, the first question was whether they were squarks or gluinos. Lawrence Hall and I realized that for high-scale SUSY breaking, the RG flow would have a large effect on these. In particular, squarks would gain mass but not gluinos, so the latter would almost certainly be lighter. A nice prediction, but spoiled by the fact that the monojets were found to be misidentified standard model particles.

The third observation was at the Crystal Ball detector at DESY. This was designed to study bottom-antibottom states $\Upsilon, \ldots$, around 10 GeV. It reported several events at 8.3 GeV, produced in the process $Y \rightarrow \gamma + \zeta$. There was no obvious candidate; it might be a light Higgs, or even a colored state. A puzzle was that the ζ was produced at very different rates in different states Υ, Υ′. Stephen Sharpe, Ted Barnes, and I thought that this might be a wave-function effect, and set out to calculate the relativistic wave functions; Pantaleone, Peskin, and Tye did this independently. It was a fun calculation, one that I had never done before. It did not solve the problem, but this went away by itself when it was realized that the apparent events were actually due to a flaw in the detector.

So, two exciting years of ambulance chasing. But it was good: I learned a lot of physics that I would not have learned otherwise.

5.8 TIME TO GROW UP

It had been a great two years on the East Coast. I had learned a lot of new physics, and new ways to do physics, and had written a couple of significant papers. I had met a large number of excellent scientists, both at Harvard and on various visits around the East, many of whom I still interact with to this day. Now it was time for the next step, a faculty position.

Job openings in physics are often cyclic, driven largely by the economy. At this time, the job market was not good. But I was confident, based on my work in the past two years.

In fact, I had turned down a faculty job the year before. At Harvard, Princeton, and a few other places, it was understood that untenured faculty were glorified postdocs. There was no expectation of a later promotion to tenure. So from my perspective, all it meant was teaching and less time for research. I think this startled them; few if any had turned it down before. But I was aware that I had been slow to get into research, and now that I was making progress I did not want to cut it short. Also, I had a two-body problem. Dorothy's MIT position was over, and the next position she found was at the University of Illinois at Urbana-Champaign (UIUC). So I wanted time free to visit her.

We had our two-body problem again, so we each looked at the jobs that were advertised in our field, and there was very little overlap. Dorothy had an excellent opportunity to remain at UIUC, moving from her temporary job to a tenure track position. John Kogut, lattice gauge theorist at UIUC, believed that he could bring me in on a spousal hire. If I had been a condensed matter theorist that would have been great, but there was virtually no one in high energy theory, and I would have felt tremendously isolated. For me, there was a possibility of an untenured position at Princeton, and perhaps something at SLAC, but there was nothing for Dorothy at either one.

Fortunately, Texas came to the rescue, with a position for me in Weinberg's group, and a lecturer's position in German with the promise of a later tenure track job. This was neither of our first choices, and not one that we had expected, but it was an excellent compromise. Texas had a long history in German, going back to its early settlers, and had a large department.

Notes

1. On Weinberg's blocky 1980 office computer was a sign "Contrary to appearances, this is not Steven Weinberg." The suspect was Paul Ginsparg.

2. The difference between a junior fellow and a postdoc is that the fellows got free lunches at the law school, and once a week they had fancy

dinners, capped off with excellent wine, chocolate, and cigars. But I'm not bitter, really. I don't even like cigars.

3. I have not included some that I did not know at the time, such as Boris Shraiman, Catherine Kallin, and Subir Sachdev.

4. I have to make an apology here. Several groups were making supergravity models, but we asked a slightly different question. Rather than a specific high energy model, we integrated out the high energy theory, replacing it with soft SUSY breaking subject to Weinberg's assumption. So we did not pay too much attention to the specific models. But the nicest part of the result, the symmetry breaking from the top mass, had been discovered and published first by Luis Ibáñez and Graham Ross. We should have cited it, but we did not learn about it until after our paper was published.

5. Weinberg's theorem was an intricate statement about the high energy limits of multiloop integrals. I had nothing against Weinberg, or his theorem which I had spent much time studying. But I wanted an argument that depended only on dimensional analysis.

6. The notable field theorist Edouard Brezin was visiting when I presented my talk. I was happy that he followed and appreciated the subtleties of the argument, and also when he informed me he had heard Wilson say that he regarded his own work as a proof. I agreed, though not many others would have seen the point: once you start to work scale-by-scale, the rest is just bookkeeping.