10 AFTER THE END OF PHYSICS, 2001–2007

10.1 BENA, GRAÑA, FREY

Having told Carroll that I would give up physics if a cosmological constant were found, how could I go on? Well, I had just taken on three new grad students after finishing my book, and I had to take care of them. And we still needed to see if all those de Sitter vacua were there. And there were all these cool things about AdS/CFT to look at. So life went on, and Carroll did not get the office.

Iosif Bena, originally from Romania, was perhaps my most independent student. I think he is the only one I did not write a paper with while they were a student, although we did work together later. I gave him only one project, to work out the precursor (section 9.4) for spaces that were less symmetric than AdS. We might call this *non-AdS/non-CFT dualities,* or today simply *gauge-gravity duals*. I wanted to verify that conformal symmetry was not essential, so he did the general Dp case, without benefit of conformal symmetry. I think that my only advice, beyond the idea, was that he speak more slowly and check his work. He then took an interest in my model with Strassler, extending our D3 → D5 polarization to many other brane systems, each with their own peculiarities. He went on to postdocs at USC and Princeton, and then a faculty position at Saclay, where he is an expert in supergravity solutions and their many applications.

When Mariana Graña, from Argentina, was first reading string theory with me, she seemed to have a particular interest in how the Calabi-Yau solutions of Candelas, Horowitz, Strominger, and Witten were fixed by the $N=1$ SUSY conditions. I had always wanted to do such a calculation, ever since Strominger had told me how much money he had made from it. So rederiving my solution with Strassler in this way was a great exercise, which we then extended to many other examples.[1] She then, entirely on her own, went on to find the effective low energy supersymmetry breaking for branes in fluxes, an important and technically difficult exercise. So she became an expert in flux compactifications in string theory, writing one of the classic reviews.[2] After Santa Barbara, École Polytechnique, and École Normale Supérieure, she ended up at Saclay, just as Bena had, where their strengths are nicely complementary.

Andrew Frey, from Wake Forest, also started out with a project from the work with Strassler. The $N=1^*$ theory had an infinite number of supersymmetric vacua labeled by D5 and NS5 quantum numbers. Different vacua could be connected by domain walls. Strassler and I had looked at some examples, but Frey found the general case. He got the surprising result that not every pair of solutions was connected directly; in some cases, one had to go through multiple steps. Another, more senior, group at the same time missed this. Beyond this, he was active and broad at UCSB. He and I wrote a paper on $N=3$ warped compactifications,[3] which I was interested in only as an oddity, SUSYs usually coming in powers of 2. He also had work on $N=1$ SUSY, on BPS states, dilaton stabilization, a careful study of Lorentz breaking in warped space (my suggestion), instabilities of the KKLT model, and new warped solutions. Several of these involved other students, including Graña, Matthew Lippert, Brook Williams, Anupam Mazumdar, and a postdoc Alex Buchel. After postdocs at Caltech and McGill he is now on the faculty at Winnipeg, and has moved more toward particle astrophysics.

10.2 SILVERSTEIN AND KACHRU

One of the great things about the ITP is getting to work with remarkable young people at key stages in their careers. The period after the second superstring revolution was particularly fruitful. I have already written about Strassler and Bousso, and now Eva Silverstein and Shamit Kachru arrived in early 2001 to run an ITP program, along with local Steve Giddings. The program was nominally about M-theory. For many of us, the focus was to move on from the highly supersymmetric situations used to understand the theory to less symmetric and more physical ones.

In particle physics, Randall and Sundrum had shown that in five-dimensional theories, warped compactifications gave a new mechanism to produce a large gauge hierarchy in four dimensions. This had led to widespread interest in the phenomenology of such higher-dimensional theories. Thus, it was very natural to ask whether these models might be more closely connected to string theory. Herman Verlinde had already pointed out that

T-duals of $N=4$ string theories naturally led to warped compactifications. Giddings, Kachru and I showed that this could be extended to $N=1$. In particular, it automatically accounted for the stabilization of the hierarchy: it arose from the quantization of the fluxes. Many of the fields could be stabilized, but not all. To stabilize the Kähler moduli would require nonperturbative effects.

At this point, there was still no example of a de Sitter vacuum of string theory, much less the enormous number that would be needed to explain the cosmological constant via the discretuum. There was even a widely quoted no-go theorem by Maldacena and Nunez, and earlier by de Wit, Smit, and Hari Dass. If this were true generally, it would mean that either the cosmological constant would have to be wrong, or string theory would. This result was widely quoted. But the West Coast group working on the problem knew that it was nonsense. The no-go theorem held only for classical backgrounds. One might as well claim that atoms don't exist, because they are classically unstable. There were other exceptions as well, such as noncritical dimensions as in work of Myers (see chapter 7, note 4).

So the group of us discussed this problem intensely. Already at Strings 2001 in Mumbai, which had been held in January, Silverstein had delineated some of the key features needed. In particular, there was a universal instability to be dealt with, from the radius of the compact space. Taking account of scaling from ten dimensions to four, all interactions went to zero as the radius went to infinity. In order to obtain a de Sitter solution, one needed at least three terms to give a potential with a stable positive minimum. At the ITP Silverstein completed a model with a stabilized radius, taking as her three ingredients orientifolds, fluxes, and a noncritical dimensionality.

MODULI STABILIZATION

The geometry of the compact space of a compactified string theory is dynamical due to the presence of gravity. It is characterized by its *moduli*, a set of dynamical fields whose values measure the size and shape of the compact space. A simple example of a *modulus* is the radius of a sphere. Different ingredients in string theory, such as D-branes, orientifolds, and fluxes, exert different forces on these moduli, and can be used to stabilize them to particular configurations.

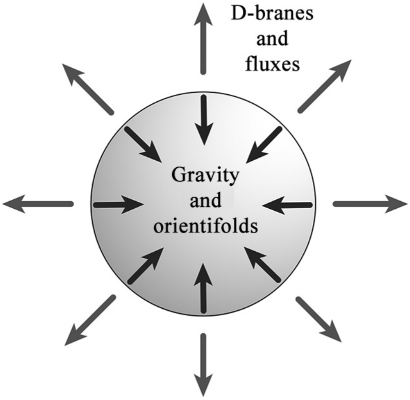

Figure 10.0a

The name of the game in *string theory phenomenology*—the task of making contact between string theory solutions and the observable universe—is to combine these ingredients in the right quantities to generate the desired features in the low energy theory. The effective cosmological constant, for instance, is equal to the minimum of the potential energy of these moduli. If the potential energy contains many local minima, then the low energy theory is endowed with a landscape of possible cosmological constants.

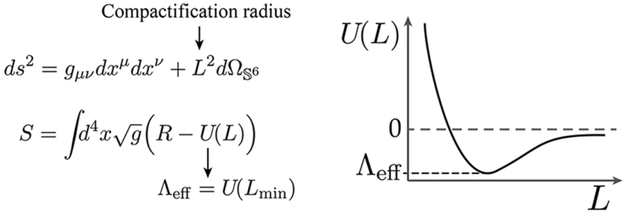

Figure 10.0b

It took a couple of years more to stabilize all the moduli, but Kachru, with a series of collaborators, found completely stabilized de Sitter solutions, using branes, fluxes, and D-brane instantons. This started with his work with Giddings and me, then with Pearson and H. Verlinde, and ended up with Kallosh, Linde, and Trivedi (KKLT). And so with this, people began to take seriously the possibility of a large set of de Sitter solutions, aptly named the string landscape by Susskind.[4]

At the ITP program, I also worked with Silverstein and her student Allan Adams on a different project. Ashoke Sen had shown that open string tachyons represented the decays of unstable D-branes, and he was able to describe the decay using open string field theory. Closed string tachyons would be much more complicated: rather than decays of branes in a fixed spacetime, they would represent decays of spacetime itself. Also, the closed string field theory was much less tractable. But we were able to sort many things out.

Tachyons like that of the bosonic string, which fill space, presumably have no stable final state. But we realized that there were closed string tachyons that were localized just like the open string ones, in spacetimes that are flat except for conical singularities. And so for these we could make sense through a combination of linear sigma models at short distance and the spacetime field equations at large distance. The result was rather simple: the singularities just spread out in an expanding shell.

10.3 PAULI, HEISENBERG, DIRAC

Around this time, the centennials for four of the founders of quantum mechanics were celebrated—Fermi, Pauli, Heisenberg, and

Dirac. I missed Fermi's, but spoke at the other three. It was fun to review some of the history of each of them, and to notice how modern some of their ideas were. I think my talk for Pauli was not so interesting. My theme in Zurich was just to look at all the parts of the standard model that Fermi was responsible for, with a rather silly graphic of miniature Paulis labeling each one. But it was interesting hearing some of the other speakers, notably the historian Norbert Straumann. It was from him that I learned of Pauli's unpublished interest in the cosmological constant—had I known earlier, the story leading from Pauli to the multiverse would have made a more interesting talk.

The other two talks had a bit more heft. With Heisenberg, the theme was unification, from his world formula to our M-theory. Heisenberg actually had presented his ideas at Caltech when I was a student there. It seemed very crude at the time, just fermions with a nonlinear interaction. He also wanted some modification of the uncertainty principle so as to produce a minimum length. But in retrospect, he was only a few steps away from matrix theory: just introduce a matrix structure to get nonlinear commutators, and supersymmetrize. Another foresighted idea was the S-matrix. This leads to another connection, Heisenberg → Chew → Veneziano → strings.[5]

The lecture for Dirac was in Tallahassee, Florida, where Dirac retired, and where I got to meet his daughter, Monica. I started with a review of Dirac's remarkable career even after quantum mechanics, and quoted his very modern point of view: "One must be prepared to follow up the consequences of theory, and feel that one just has to accept the consequences no matter where they lead."[6] For him it led to antimatter, and for me it led to the string landscape and the multiverse. It also led Dirac to magnetic monopoles. The main part of my talk was about this, proposing two principles: (1) In any theoretical framework that requires charge to be quantized, there will exist magnetic monopoles, and (2) In any fully unified theory, for every gauge field there will exist electric and magnetic sources with the minimum relative Dirac quantum. I illustrated this with five examples: grand unification, Kaluza-Klein theory, lattice gauge theory, the Kalb-Ramond theory, and D-branes. So I argued that magnetic monopoles are our most certain prediction about physics beyond the standard model, though unfortunately the scale is not predicted. Notably, the nonobservation of magnetic monopoles led Dirac to recant on his quote, but he should have been more patient: we have only explored a tiny range of scales.[7]

10.4 MORE ODDS AND ENDS

Here are a few papers from this period that I do not want to forget, but are not worth a full section.

10.4.1 Crunching with Horowitz

The ekpyrotic idea, a universe bouncing at a brane, arose at around this time. One of the arguments for a consistent bounce was the example of the resolution of singularities in string theory. Liu, Moore, and Seiberg had studied the toy model of a null orbifold, and found that the singularity in amplitudes was resolved. But this ignored the backreaction. Of course, the well-understood string singularities were timelike, and the ekpyrotic singularity was spacelike, so the issues with backreaction would be more severe.

Horowitz and I studied the backreaction using both general relativity and string theory, and concluded that a single particle caused the spacetime to collapse to a strong curvature singularity, even in regions arbitrarily far from the particle. This was not surprising: intuitively, a bounce would require infinite fine-tuning. This did not rule out all possibilities for a bounce, but emphasized the contrived nature of the idea.

10.4.2 Emergent Gravity?

I tend to be skeptical, of both my own work and that of others. I guess, having thought hard about various problems, I am always wary about new claims about them. I am not always right—I have mentioned Rubakov as one who surprised me twice—but I should have an item on my CV for *papers prevented*. Certainly, the Weinberg-Witten theorem, restricting emergent gravity, provided one of those potential alarm bells. Of course, AdS/CFT had shown that the theorem had an exception, but that required a whole extra dimension.

A model by Zhang and Hu, which appeared to get a $3+1$-dimensional graviton from the boundary of a $4+1$-dimensional quantum Hall system, had to be understood. So with a grad student, Henriette Elvang, we studied this model to see how it might have evaded the theorem. Their idea was to take parallel copies of the quantum Hall system, for example, in the 12 and 34 directions, and sum to give an $SU(2)$ symmetry. Then turning on a magnetic field, there would be a potential that would lead to massless degrees of freedom on the $3+1$-dimensional boundary. Moreover, one could get any massless spin, in particular, two. Note that this is not holographic but antiholographic: the field theory lived on the higher-dimensional space, and the would-be graviton would live on the boundary.

To analyze this, we first took a large-N limit so that the massless degrees of freedom lived in flat space. We then found that the spectrum was not that of a 3 + 1-dimensional space, but a cone of 1 + 1-dimensional theories. For example, the low energy density of states was larger. So the system might be interesting, but it was not Lorentz invariant, and not gravity.

Henriette went on to work with Gary Horowitz and has been very successful in gravity and field theory. She is now a professor at Michigan. At the time I had too many students to take on, and I felt that Gary was a better fit for Henriette. I tend to give my students rather ill-defined problems, trusting that there is some gem underneath; this often works out, but a student who likes to calculate would be better off with Gary.[8]

10.4.3 Integrability

Our postdoc Radu Roiban, my former student Bena, and I were discussing whether we might be able to get an analytic understanding of confinement, at least at large N and perhaps with SUSY. Our idea was to use AdS/CFT to rewrite the boundary four-dimensional field theory in terms of the two-dimensional string world-sheet (the string action would be complicated, because the conformal invariance was broken). We would then hope to use two-dimensional methods to solve the theory.

As a warm-up, we studied the conformal $AdS_5 \times S^5$ theory. We knew that there was a method due to Luscher and Pohlmeyer to find infinite symmetry algebras for a wide class of nonlinear sigma models. Indeed, as we were working on this we learned that

Mandal, Suryanarayana, and Wadia had recently found this for the bosonic $AdS_5 \times S^5$ string. So we generalized this to the superstring, and this also worked.

Having an infinite-dimensional symmetry seemed like it would enable us to do many new calculations. But we quickly learned that this kind of algebra, the Yangian, was different from the usual algebras of physics, and we still had a lot of work ahead. We also learned that this same method, integrability, had been applied a few months earlier by Minahan and Zarembo on the CFT side of the theory, where we were on the AdS side. So integrability became an active area, and Roiban ended up as one of the twenty-six joint authors of a massive review.

My own further attempts were not so successful. It was not my way to learn the necessary technical machinery for this subject. I figured, "this is a symmetry, I know all about symmetries," and set out to figure this out, with the help of my latest student Nelia Mann. We both worked quite hard, and were able to do a few interesting calculations, but in the end it was not a good approach. One of my limitations is that I am best working on problems where the physics and the math are close together. When one has to start dealing with objects whose physical content is not evident, I lose my way. But Nelia went on to write several nice papers with other students and postdocs, worked with Jeff Harvey on pomeron phenomenology at Chicago, and is now a junior faculty member at Union College.

10.5 GROSS, KAVLI, NOBEL, AND THE FUTURE

This section is a bit outside the main flow here. But I had to explain how ITP programs suddenly became KITP programs, and one thing led to another. The original ITP had grown more than 50 percent under Gross, taking full advantage of the new building. Over time he brought in three new permanent members: Lars Bildsten, replacing Doug Eardley; Leon Balents, replacing Matthew Fisher; and Boris Shraiman, representing a new effort in biophysics.

This led to an increased presence in astrophysics and biophysics, and before long even the new building was too small. And so Gross went looking for a donor and came back with Fred Kavli. I do not know how they connected, or the details of their negotiations; Gross keeps his cards close to his chest. But in the end, we had the funds to build a nice extension of the building, ending up with perhaps 2.5 times the size of the original ITP before 1993. Moreover, it made the building more connected and added many new public and working areas. So in 2002, the ITP became the KITP. At first it seemed strange, but after a few years ITP seemed naked without the K.

We had hoped to raise enough to enlarge the building and begin an endowment, but construction prices were rising fast. We did expect though that this was a beginning of a relationship with Kavli, which would lead to an endowment in the future. So it was a bit of a shock when, a year later, Kavli gave an equal donation to establish a Kavli institute in astrophysics at Stanford: we were being franchised! Over time there were twenty Kavli institutes. We did get some additional Kavli support, but still needed an endowment to protect our activities from the indefiniteness of NSF support.

But we did have the new building, which was a beautiful and stimulating place to work. To celebrate, in October 2004 we had a three-day conference in Gross's grand style, bringing together the leaders of all areas of physics to discuss the future. And delightfully, two days before the conference Gross, Wilczek, and Politzer received the Nobel Prize. So the meeting became a celebration, for twenty-five years of the (K)ITP, for the new building, and for David's long-awaited prize. Wilczek also attended, expressing his satisfaction at winning his first Nobel Prize.[9]

10.6 COSMIC STRINGS

One of the discoverers of the Higgs mechanism, Tom Kibble, also pioneered the idea of topological defects in cosmology. For example, solitonic strings could form at a phase transition and then expand with the expansion of the universe, growing to a cosmic length. Early in the first superstring revolution, Witten noted that fundamental strings might do the same. If so, they would be a spectacular observational signature.

But Witten noted that there were several obstacles to this. Fundamental strings generally had tensions close to the Planck scale, which would lead to excessive fluctuations in the Cosmic Microwave Background (CMB).[10] Also, there were potential instabilities. Heterotic strings carry axion charge, so the strings will actually bound axion domain walls, producing a confining potential that prevents their growth. Type I strings were unstable against rapidly breaking up into small open strings. And type II strings were confined by NS5-brane instantons.

But at a KITP program on string cosmology in 2003, Silverstein was giving a talk about superstring vacua, and made reference to F strings and D strings (fundamental and Dirichlet). For cosmologist

Ed Copeland, the idea of having two kinds of cosmic string, and their bound states, was a novel one, and he became excited about the possibilities. I was aware of the problems with this idea, but it was a good time to revisit it, with the first fully compactified theory in KKLT, and its cosmology in Kachru, Kallosh, Linde, Maldacena, McAllister, and Trivedi (KKLMMT). And indeed, the problems potentially went away.

First, KKLT was a warped compactification, reducing the scale of the string tension: it could easily fit with the constraints. Further, any of the instabilities might be suppressed due to separation in the compact dimensions, or warping. And Tye and collaborators had shown that cosmic strings arose naturally in KKLMMT-like brane inflation models. So Copeland and I, together with brane maven Rob Myers, worked out the phenomenology of these potential strings.

Like Copeland, I became quite excited by this: a potential direct signature of physics near the Planck scale. And so this became a large part of my research for the next few years. The first major question was, if cosmic strings were found, could we distinguish fundamental strings from field theory solitons? Indeed we could. As we noted in our discussion of Matzner in section 6.5, when two strings cross each other, if they are solitons then their ends always reconnect, a classical process. But for two fundamental strings, the crossing is a quantum process: the strings may reconnect or pass through. I had worked this out as an exercise many years before for the bosonic string, in part with Jin Dai. Now, with visiting KITP grad fellows Nick Jones and Mark Jackson, I extended this to supersymmetric F- and D-strings, and their bound states. So if cosmic strings were ever found, we could indeed make direct measurements of properties at the string scale.

But, following this up, the job would not be quite so simple. What we would measure would likely be some correlators on string networks. These had been studied numerically, and different groups had gotten radically different results. One key quantity was the typical radius of the string loops that broke off from the main network; these were the main source of the gravitational waves emitted from the network. Remarkably, estimates ranged from the horizon scale (the only obvious scale in the problem) down to the Planck scale, for what was a well-posed classical problem. So with my latest student Jorge Rocha, and in part with a visiting postdoc Florian Dubath, we made a scaling model which explained why there were actually two scales: there was an infrared divergence. We tried to get more detail out of this, but improved numerical methods from the Tufts group eventually gave the sharpest picture, and if cosmic strings are found these simulations will be essential.

I think that Jorge was more interested in working on black holes than on cosmic strings. Fortunately, I found him a good black hole problem before he left. This was to study the dual of an $N=4$ theory coupled to another field that could carry away energy. In this way, one could study decaying black holes even in AdS. I think this was the first study of such a hybrid system. (Jorge is back in Spain, and still happily working on black holes).[11]

At this point, it seemed that we had to wait for the experimentalists. Unfortunately, so far improved measurements from WMAP and then from Planck had only lowered the upper bound on cosmic strings. But it was good to have an opportunity to think about observation.[12]

Thinking about cosmic strings led to a surprising observation: open heterotic strings could actually exist, in the $SO(32)$ theory but not in the $E_8 \times E_8$. This came from thinking about a general classification of strings. Originally, cosmic strings were referred to as *global* or *local*. Global strings had fluxes running in their cores, and local strings did not. But from thinking about all the examples that arose in string theory, I realized that there were two more: *Aharonov-Bohm* (AB) and *quasi-AB*. Further, the right way to distinguish these was not by what was in their cores (which could, after all, depend on duality) but by their properties at long distance, which controlled their stability. Thus, global strings had a long-range axion field, and so were confined. Local strings could break into short open strings. AB strings were stable due to discrete symmetries, unless the same charges were carried by massless fields (quasi-AB), in which case they could tunnel and then expand in the perpendicular direction.

Applying this classification to the heterotic string, one finds that for the $SO(32)$ string, it can be any of local, AB, or quasi-AB, depending on the compactification. The $E_8 \times E_8$ string had to be global. The puzzle of an open heterotic string is that the degrees of freedom moving on the two sides are very different, and had no consistent boundary condition. Indeed, what happens is that when a worldsheet field on the $SO(32)$ string reaches the boundary, it does not reflect but rather leaves the string and becomes a spacetime degree of freedom. This odd picture could be described explicitly in open string field theory. Unfortunately, I have never found anything useful to do with it. David Morrison did notice that I had posted it, coincidentally, on the 10th anniversary of the D-brane paper.[13]

10.7 DOWNTIME

The last few years in this period were a bit of a downtime for me. Certainly, I had plenty of fun and joy, and some good physics. But also I had extended periods of anxiety. Sometimes these just plagued the early hours of sleep, but other times they took over the day, and my work. One that I remember clearly was my induction into the National Academy of Sciences in 2005. This should have been a time of great celebration, and there was some, but throughout I was filled with an ill-defined anxiety.

Part of this was still a hangover from finding the anthropic principle in string theory. I feared that most of the routes to the discovery of the fundamental theory were blocked by it. Also, this was the time of the well-publicized anti-string books. I got caught up in this because Rosalind Reid, a KITP visiting journalist who was editor at *American Scientist*, asked me to review the book by Smolin. I felt that it was a good thing to do: many people, including some of our colleagues from other fields, were taking it uncritically. I tried to use it as an opportunity to present the positive argument for string theory, but I do not think that I was very effective. As Mark Twain said, “A lie can travel halfway around the world while the truth is putting on its shoes.” Smolin tried to avoid outright lies, but my reaction to reading the book, which I wish I had stated more directly, was “This is not the way a scientist writes.” Facts were twisted to create the impression desired, rather than being a way to reach truth.

Beyond this, I think that my anxiety was a long-standing aspect of my biology, going back even to my childhood shyness. I think that many of the decisions I have made over time have been driven more by anxiety than by positive emotions. Matthew Fisher, who had some experience here, was a strong believer in the value and effectiveness of psychiatric drugs. And indeed, after some experimentation, a small regular dose of Lexapro has kept me balanced. So one can say that the anthropic principle drove me to drugs. But one must follow science where it leads.

Over the years, I have noted one visiting speaker prepare with a dose of Adderall, and one with a dose of Valium (and offer one to me also). A doctor has told me that half her colleagues are on Lexapro; it is an occupational hazard for high-functioning people. And following the lead of Paul Erdos, who is said to have written all of his 1,500(!) papers under the influence of stimulants, I tried working a few times with Adderall. I had a couple of very effective days, but overall did not like its effect.

Notes

1. Just a few years ago, at my sixtieth birthday, Graña told me that when I first suggested this, she thought it sounded boring, but it developed into her life's work.

2. When I was writing a report for the École Polytechnique, I noted that her review was the most highly cited paper written there, over a period of years.

3. Warped compactifications, string realizations of the Randall-Sundrum idea, were developed by Strominger and by K. Becker and M. Becker.

4. For many years, the size of the landscape has been crudely estimated as 10^{500}, based on large Calabi-Yaus. But Taylor and Wang have recently shown that there is an F-theory geometry of dimension $10^{272,000}$.

5. At my talk in Munich, Helmut Rechenberg, curator of the Werner Heisenberg archive, informed me that this chain was even more direct than I had guessed. As early as 1954, Heisenberg wrote in a letter that in

Urbana he had met a particularly nice younger physicist with the name Chew, and they continued to correspond.

6. [The source of Dirac's quote: P. A. Dirac, 1977 Varenna lecture, quoted by M. Jacob in A. Pais, M. Jacob, D. I. Olive, and M. R. Atiyah, *Paul Dirac: The Man and His Work* (Cambridge, UK: Cambridge University Press, 1998).—Ed.]

7. One of the smaller detectors at the LHC is MoEDAL, searching for magnetic monopoles. When it was proposed, I was asked to write in support, based on this lecture, and I was happy to oblige.

8. Two other students whom I had to pass up earlier were Don Marolf, who worked with Bryce DeWitt and is now my own colleague and collaborator, and Scott Thomas, who worked with Willy Fischler and is now a leader in particle phenomenology. But I think each of these did better than they would have with me as their advisor.

9. [There is a color photo of the 2004 conference attendees in the photo insert of this book.—Ed.]

10. For heterotic strings there was a simple relation between the string tension, the gauge coupling, and the Planck scale, $\mu = g^2/16\pi^2 G_N$. This was too large by several orders of magnitude.

11. [There has been a surge of recent interest in this hybrid setup for studying the information paradox.—Ed.]

12. A couple of times, the observations of apparently paired galaxies, produced by the gravitational field of a cosmic string, have led to some excitement. But they have so far always turned out to be coincidences.

13. Morrison came to UCSB from Duke about ten years ago, with a joint position in math and physics. He plays a unique role in tying these subjects together. He and I have an ongoing friendly dispute about whether I know much math (I claim not). I think that the difference goes back to Susskind's distinction between the mathematics of the equations and the mathematics of the solutions, where I care only about the former.