8 D-BRANES AND ORIENTIFOLDS, 1992–1995

8.1 UCSB AND ITP

The University of California at Santa Barbara had one of the leading string theory groups in the world, with Andy Strominger and Gary Horowitz, two of the discoverers of Calabi-Yau compactification; Mark Srednicki, one of the inventors of the invisible axion and a creative thinker in particle physics and quantum theory; Steve Giddings, one of the young leaders in string theory; and now me.

It was probably the largest and strongest string group in the world, outside of New Jersey. Of course, New Jersey had Princeton/IAS (Witten, Polyakov, Gross, Klebanov, Migdal, Wilczek, Nappi, Callan, Verlinde) and also Rutgers (Banks, Shenker, Seiberg, Friedan, Zamolodchikov).

I have always been astonished to think about the growth of UCSB physics, from becoming a university in 1944 to being a department that in some measures is as high as fifth in the country. All the other top departments have been around since at least the previous century. The coup by the gang of four—Jim Hartle (relativity), Ray Sawyer (particle physics), Doug Scalapino (numerical condensed matter), and Bob Sugar (lattice gauge theory)—did not start this, but greatly accelerated it.

In 1978, the High Energy program director at the National Science Foundation, Boris Kayser, saw a need to enhance collaboration between physicists at different institutions and in different fields, and also to support postdocs who were leaving physics for lack of support.[1] He persuaded his superiors to fund this, to the tune of around a million dollars per year. There was a call for proposals, and the story, as told by the winners, is that all the established departments said, "We know what to do with the money, give it to us." But UCSB's gang of four had a unique idea, to use the funds to bring scientists from around the world to interact for as long as six months, rather than the typical weeklong conference. There would be time to conceive new projects and carry out the collaboration there. And the outstanding Walter Kohn agreed to come to UCSB to be the first director.

The NSF liked the UCSB proposal the best, but it wanted to see a greater contribution from UCSB. So the gang proposed that UCSB would contribute four faculty positions, a huge bargaining chip that no other group could match. These Permanent Members (PM) would mentor the postdocs and help design and run the scientific programs. But they had to convince their new chancellor, Robert Huttenback, to back them. Huttenback, just arrived from Caltech, knew about the competition because Murray Gell-Mann had boasted to him that Caltech's proposal would dominate UCSB's. So Huttenback gave the gang what they asked for: UCSB got the Institute for Theoretical Physics (ITP), and the gang of four became the Founders. And so my position exists because of Murray's boast.

The first four PMs were Frank Wilczek and Tony Zee, both broad particle theorists; Jim Langer, condensed matter; and Doug Eardley, gravity. By the time I arrived, Wilczek had moved to the IAS and Langer had become ITP Director.

Kohn had become a regular member of the physics department after finishing his five-year term as Director; Robert Schrieffer had moved to Florida State after finishing the next five-year term as Director; and Jim Langer had become the third Director. I was the replacement for Wilczek,[2] and Matthew Fisher replaced Langer as PM the next year.

For its first fifteen years, the ITP operated on the sixth floor of Ellison, the rest of which housed the history, geography, and political science departments. The whole institute, every office, could be seen from the point in the middle, with one corridor on the left, one on the right, and one perpendicular. I liked the coziness of that, but it had long outgrown its space. A year after I arrived, the university completed a dedicated ITP building, soon named for Walter Kohn. With a beautiful design by Michael Graves, right across from the cliffs above the Pacific, it nearly doubled the ITP's capacity.

8.2 INFORMATION LOSS

The format at ITP was the same for its first twenty years: two programs in the first half of the year and two in the second, each running for five months. Typically, there would be one program from each of the three main subfields—high energy, condensed matter, and astrophysics—while the fourth might be a new direction, an interdisciplinary area, or a second subject from one of the main areas. It was also possible to have a one-month mini-program, scheduled on shorter notice, if something new came up.

At the start of 1993, shortly after I arrived, the two areas were high energy physics and relativity. The respective subjects were nonperturbative string theory and small-scale structure of space-time. Effectively, this was to be one double-size program, bringing together string theorists and general relativists. What it developed into was a giant program on the black hole information paradox.

When Hawking's paper first appeared, my reaction was the same as that of most quantum field theorists. When we burn a lump of coal, the disorder increases monotonically, so the entropy is maximized at the end. But this is the coarse-grained thermodynamic entropy. If we look at the microscopic quantum state, for coal that starts out in a pure state, the final state must also be pure and the microscopic von Neumann entropy must end up zero. Hawking was saying that for black holes, even the microscopic disorder increased monotonically, so that the final state was no longer pure but mixed, meaning that information had been lost and the Schrodinger evolution of quantum mechanics had to be modified.

So the naive reaction was that Hawking had mixed up the coarse-grained and microscopic descriptions, and a more careful treatment of the quantum state would find Hawking's mistake. I first learned why Hawking's paradox is so difficult from a talk in Austin by Susskind, who had started thinking about the problem around the time I left SLAC. As he explained, the difference between the coal and the black hole is that the black hole had an event horizon. So with the coal, a quantum degree of freedom could rattle around for a while inside and then escape, but for a black hole, once it fell past the horizon there was no escape.

THE INFORMATION PARADOX

Black holes are a minefield for paradoxes and confusion. According to GR, they form whenever matter is packed into a sufficiently small ball of a special radius that is set by the total mass, known as the *Schwarzschild radius*. At this radius, the spacetime develops an *event horizon*, the point of no return. Everything within it is doomed to crash into the *black hole singularity* where the force of gravity blows up, and which is—quite literally—in everything's inescapable future.

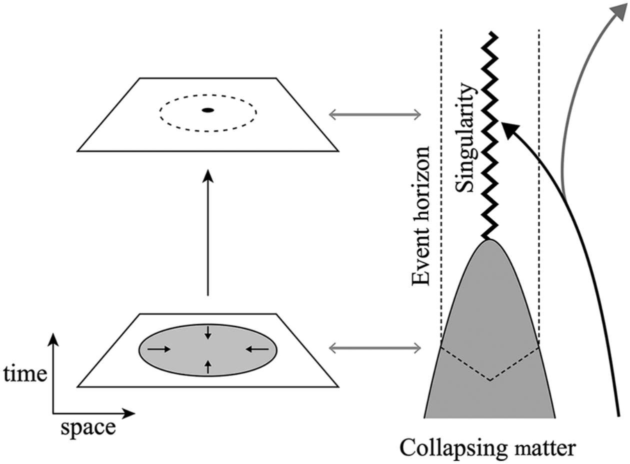


Figure 8.0a

Quantum mechanics further obfuscates the story of black holes. Quantum effects have been shown to lead to black hole evaporation because of how the event horizon interacts with the vacuum of QFT. Empty spacetime in QFT is actually filled with entangled particles across any partitioning of space. The event horizon is a special partitioning that splits these entangled particles apart into a member trapped inside the black hole and a member on the outside that escapes. The particles on the inside carry negative energy and reduce the mass of the black hole, while the outside particles carry away positive energy. The collection of outgoing particles is known as the *Hawking radiation*.

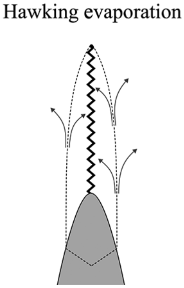


Figure 8.0b

This evaporation process conflicts with various aspects of the *unitarity* of quantum mechanical evolution. The first issue is that the state of the emerging Hawking radiation is independent of the state of the collapsing matter that initially formed the black hole. This contradicts the condition from unitarity that different initial states must evolve to different final states. The second problem is that the Hawking radiation carries *entropy*, a measure of its total amount of entanglement with the black hole, that only increases as the black hole evaporates, resulting in a final *mixed quantum state* of the radiation. However, unitary evolution requires that the total amount of entanglement must be conserved, and hence an initial *pure quantum state*—one with zero total entanglement—can only evolve to a final pure quantum state. This requires the entropy of the Hawking radiation to fall back down to zero by the end of the evaporation. This conflict constitutes the *information paradox*.

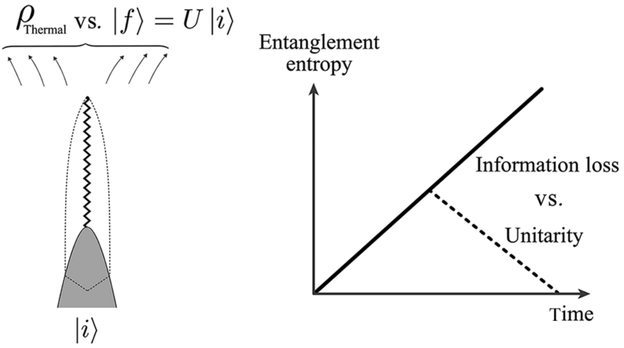


Figure 8.0c

I am not sure why this problem rose to prominence just when it did. I think Lenny's talks had introduced many string theorists to the difficulty and importance of the problem. Also, Callan, Giddings, Harvey, and Strominger (CGHS) had recently presented a seemingly solvable two-dimensional model of black holes, which allowed explicit studies of the problem. This was probably the most active subject of the program. But it seemed that there were ambiguities in the definition of the model, so Jeff Harvey said that it was like a Rorschach test: you could get whatever you expected.

That ITP workshop did not solve the problem. The first big step forward was seven years later, with AdS/CFT, and even today there are key issues to resolve. But it did succeed in communicating what the problem was and what the possible resolutions might be. Essentially, almost all ideas fell into one of three categories: (1) information is lost in the way that Hawking argued; (2) information escapes from the black hole interior, seemingly requiring traveling faster than light; or (3) black holes do not evaporate all the way, but end up as Planckian remnants. Each of these seemed to have unacceptable consequences.

At the conference at the end of the program, I ran a discussion in which I took a vote as to which alternative people expected. There were a few *remnants* and a few *none of the above*, but the bulk broke down 60:40 for information escape versus information loss. This just reflected the fact that the audience was 60 percent field theory/string theory and 40 percent general relativity. The former were more ready to give up relativity, and the latter to give up quantum mechanics. As for myself I was a natural agnostic, going back and forth among the possibilities, looking for a resolution.

Another highlight of the conference was Susskind, who gave two talks. At the start of the week he introduced the idea of black hole complementarity, but by the end of the week he had refined it so much that he insisted on speaking again, and the organizers (me, mostly) extended the session.

BLACK HOLE COMPLEMENTARITY

Even supposing the existence of a resolution to the information paradox opens a can of worms. A necessary feature of any resolution is that anything dropped into a black hole must eventually reappear in the Hawking radiation. This immediately raises a problem: the information would have to be present both inside and outside the black hole, and hence appears to have been cloned! This is problematic because it conflicts with the no cloning theorem of quantum mechanics.

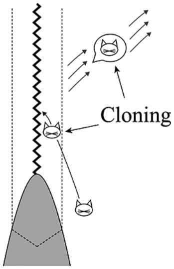


Figure 8.0d

A potential way out of this conundrum is the amazing coincidence that no experiment can actually witness this cloning. An observer that waits to capture the clone from the radiation prior to jumping in won't have sufficient time to catch up to the original copy to confirm the cloning; the observer and the information would sooner crash into the singularity. The failure of this thought experiment led to the idea of *black hole complementarity*, the principle that simultaneous precise measurements inside and outside the black hole are not physically possible, analogous to how position and momentum of a particle cannot be simultaneously measured in quantum mechanics.

Looking at my own work from this period (I do rely heavily on INSPIRE to make up for my memory), the program led me to several papers about the information problem: (1) constructions of string theory black holes, with Giddings, Harvey, Shenker, and Strominger; (2) an argument (with Strominger, who later expanded it) that models where degrees of freedom from the black hole interior escape into baby universes do not actually destroy information, but are examples of remnants—this was similar to the Coleman-Giddings-Strominger analysis of baby universes; and (3) with Lowe, Susskind, Thorlacius, and Uglum, a project initiated by David Lowe to determine whether string theory is local. If it were nonlocal, there might be no information problem. Lenny interpreted our result as saying that it was indeed nonlocal. I thought that it was inconclusive: it was not clear whether we were looking at the right observables. This is still an open question. We also analyzed the Nice Slice, a coordinate system first introduced by Robert Wald, where slices pass into the black hole but never get near the singularity.

8.3 WORKING WITH MATTHEW

Things were about to change in a big way, but first a little condensed matter interlude. Matthew Fisher, newly arrived at the ITP, and Charlie Kane, an assistant professor at Penn, were interested in the edge currents in the fractional quantum Hall system. Even for the simplest case of $2/3$, there was a discrepancy between the observations, which showed charges moving only in one direction, and the theory, which had charges moving in both directions. Fisher and Kane had the idea that disorder in the system would produce an interaction between the right- and left-movers, which would then flow to a new phase.

They did not see how to solve the resulting Hamiltonian, but thought that I might have some ideas. It resembled a conformal field theory such as one encounters with strings, but there were two complications: the disorder, and the absence of Lorentz invariance. But surprisingly, a bit of fiddling revealed an unexpected symmetry, which allowed the model to be solved. It had the desired feature that currents moved only in one direction, but also an unexpected feature: a neutral mode moving in the opposite direction.

This was one of my few measurable predictions. Unfortunately, the neutral mode did not seem to be there. Apparently, it was observed recently, more than twenty years after the prediction, but the story was more complicated. I did learn that in condensed matter, authors are not determined alphabetically but often younger to older. And I did not meet Kane at the time (Fisher was the intermediary), but he has become quite distinguished for his work on topological insulators.

8.4 STRINGS '95

This was a slow time in string theory. The excitement from the first superstring revolution had passed, and many directions had been explored without clear result. But beneath the surface, something was brewing. Besides D-branes and supermembranes, there were the black branes of Horowitz and Strominger, the weak/strong duality conjecture of Font, Ibanez, Lüst, and Quevedo, the 5-brane conjectures of Duff and Strominger, the study of duality effective actions and spectra by Schwarz and Sen, and in gauge theory the tests by Vafa and Witten and by Seiberg, and more. All of these were weak/strong dualities, dubbed *S*-duality by Font et al., as opposed to the easy *T*-dualities. But in the "fog of war" the connections between these different strands were not obvious. As Andy Strominger has reminded me, he, Gary Horowitz, and I had lunch together nearly every day for three years, without realizing that their black *p*-branes and my D*p*-branes were the same.

BLACK BRANES

Due to the richness of string theory, black holes come in a variety of exotic shapes and charges. Black branes are a class of black holes that are infinitely extended in some directions and compact in others, and are often depicted as black membranes.

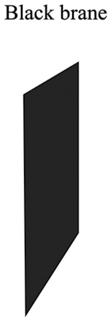
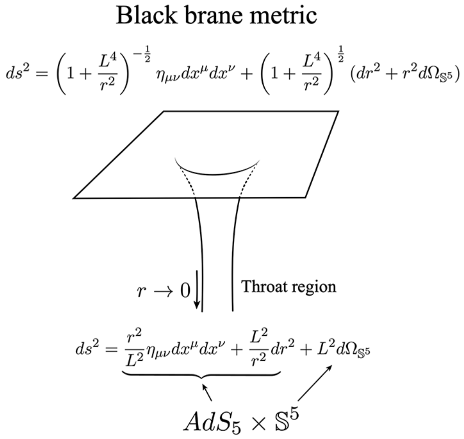


Figure 8.0e

The geometry of some black branes can have a surprisingly symmetric structure. The region near its event horizon develops a *throat*, where the geometry extends down an infinitely long higher-dimensional cylinder. The spacetime down the throat is a highly symmetric spacetime with a negative cosmological constant, and goes by the name of *anti-de Sitter* (AdS) space.



$$ds^2 = \left(1 + \frac{L^4}{r^2}\right)^{-\frac{1}{2}} \eta_{\mu\nu} dx^\mu dx^\nu + \left(1 + \frac{L^4}{r^2}\right)^{\frac{1}{2}} (dr^2 + r^2 d\Omega_{\mathbb{S}^5})$$

$$ds^2 = \underbrace{\frac{r^2}{L^2}\eta_{\mu\nu} dx^\mu dx^\nu + \frac{L^2}{r^2} dr^2}_{AdS_5} + \underbrace{L^2 d\Omega_{\mathbb{S}^5}}_{\mathbb{S}^5}$$

$$AdS_5 \times \mathbb{S}^5$$

Figure 8.0f

One thing that did strike me was Seiberg's paper on strongly coupled $N = 1$ gauge theories. His $N = 2$ papers with Witten a few months later have been much more extensively followed up, because the higher symmetry allows more calculations. But I was impressed that even for the presumably more physical $N = 1$ theories one could do exact calculations. I could not have done what Seiberg did. I would have needed to see something proven; probably this would require first figuring out what cutoff to use, and this would get nowhere. But what Seiberg said was that if a strong coupling duality passed several nontrivial tests, it had to be for a reason, and duality should be the default. He wanted to know what was true, not what could be proved. And as more dualities were found, they formed consistent webs.

For myself, I revisited D-branes a bit. With fellow aficionado Michael Green we looked at a possible new interpretation of D-branes, but it went nowhere. I also thought about Shenker's argument that nonperturbative string effects should be of order $e^{-C/g}$, and realized that D(-1) branes (D-instantons) were exactly of this order. This was a bit nontrivial: one had to realize that D-objects were independent, so one had to sum not only over string worldsheets but also over D-brane degrees of freedom. I also gave a set of Les Houches lectures entitled "What Is String Theory?" These consisted of several introductory chapters from my book (remember that?), and some of the recent attempts to go further. I included a short section on the various duality ideas noted above.

When Strings '95 began at the University of Southern California that March, there was some feeling of gloom. This was both professional and scientific. The first problem was that there were

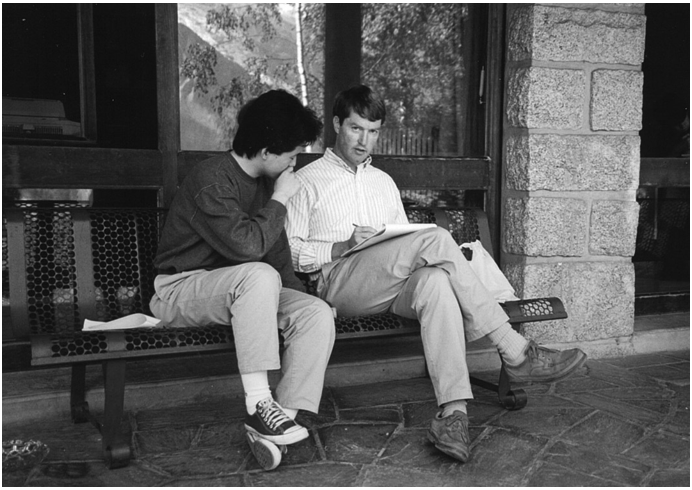


Figure 8.1
Joe with his characteristic ballpoint pen and pad of paper and student Makoto Natsuume in Les Houches, 1994. Courtesy of Donald Marolf and Makoto Natsuume.

large numbers of excellent postdocs who were not getting jobs. The organizers ran a session at which the postdocs could express their unhappiness. Somehow I was asked to moderate, but I had no ideas to offer. The scientific problem was the post-revolution slowdown noted above, as well as the lack of prospects for experiment. Susskind addressed these in his after-dinner speech. His theme was that we did not need experiment, that we could figure string theory out without it. He supported this by looking at various past theoretical discoveries, and showing how they could have been reached by thought alone. But I was not convinced that we could have figured out quantum mechanics without experiment.

However, one of Susskind's points has stayed with me. He said that he wanted to know the mathematics of the equations, not the mathematics of the solutions. Techniques for solving a problem, as in geometry, can be much more elaborate than those originally needed to define the problem. Like Susskind, I always wanted to find the simplest example that would make a point.

The irony was that both problems had been transformed just two days earlier, when the second superstring revolution began. Everyone had heard Witten's talk, but the full magnitude of it took some time to absorb. My recollection is that the talk began with Witten saying that the organizers had asked the speakers to think about big questions, so he was going to give the strong coupling behavior for every string theory in every dimension. The strategy was remarkably simple (he noted that he was building on recent work of Hull and Townsend). Essentially, assume that weak/strong duality (*S*-duality) is true and see if this is consistent, as Seiberg and others had recently done to great effect in SUSY gauge theories.

Taking any of the five known string theories in ten dimensions, there was a dilaton field ϕ and an effective action $S(g_{\mu\nu}, \phi, \ldots)$. Duality would mean that under $\phi \to -\phi$,[3] perhaps with some other changes of variable, the action would again be that of some string theory. Indeed, this worked for the type IIB string—it was dual to itself. For the heterotic $SO(32)$ string, the dual theory was the type I theory (and vice versa), which had the same gauge groups and supersymmetries. Type IIA theory threw a bit of a curve: the strongly coupled limit is eleven-dimensional supergravity. Finally, the dual of heterotic $E_8 \times E_8$ was a puzzle in the initial talk, but Witten and Petr Hořava soon identified it as eleven-dimensional supergravity on an orbifold. For each of the five ten-dimensional string theories, and eleven-dimensional supergravity, there was a unique candidate strong coupling dual. Moreover, these dualities extended to BPS excited states, and to lower-dimensional states, in a highly connected and consistent way. And combining these *S*-dualities with the *T*-dualities, it seemed that all string theories were connected.[4] It was rather overwhelming, for me and the rest of the audience. Even Witten described his conjectures as shaky, as compared to *T*-duality. But just in the previous year the work of Seiberg and Witten had finally made this kind of reasoning convincing in supersymmetric gauge theories, and so it did not take long to believe this for the more mysterious regime of strings.

At the end of Witten's talk, Mike Green and I looked at each other and said, "That looks like D-branes." For me what was most striking was that the $e^{-C/g}$ effects that I discussed above occurred extensively in Witten's dualities. But there were a lot of things to think about. Witten's talk ran to roughly sixty slides, and there was so much in it that was new. I ended up with a list of fourteen homework problems just to understand the talk. There was a disproportionate number of questions about K3s, as there had been in the talk. And the open string questions were near the end, in the talk and in the list.

8.5 D-BRANES

The start of the second superstring revolution did not go well for me. Counting Witten's talk as day zero, day two was my ineffective discussion with the postdocs. (My one conference talk, on day –1, was a review of the black hole information problem). On day fourteen, I arrived at the ICTP in Trieste to give a set of spring school lectures on string theory. My plan had been largely to repeat my lectures “What Is String Theory?” from Les Houches. These had gone well when I presented them eight months earlier, but now they felt years out of date. I did not have the time to absorb the new understanding. A set of lectures on D-branes would have been great, but their significance still had not emerged. So I did repeat the earlier lectures, feeling quite depressed as other speakers like Seiberg and Sen were zooming forward into the new era. Between this and the jet lag I slept very badly, so one time I fell asleep during my own lecture. (If you find this hard to believe, you can ask Seiberg.)

But I gradually caught up. With Shyamoli Chaudhuri I studied dualities in some models she had developed, heterotic string compactifications with maximal supersymmetries. With my first UCSB student, Eric Gimon, I studied type I string compactifications. Both projects had some K3s in them—I was doing my homework. And I had gotten past my natural skepticism, after seeing weak/strong predictions work.

By August I had learned enough that I could give reviews of string duality to nonexperts, and I was at the Yukawa Institute for Theoretical Physics to present one. My former student Natsuume had just returned to Japan, and gave me an extended tour of Kyoto, including a traditional bathhouse and many scenic points.[5] But the most interesting part of the trip was going to a tiny laundromat in Kyoto by myself. After loading the washer, I spent a little time looking at the Japanese magazines, and then sat down to do some physics. Next up was the homework about open strings. And immediately I ran into a problem.

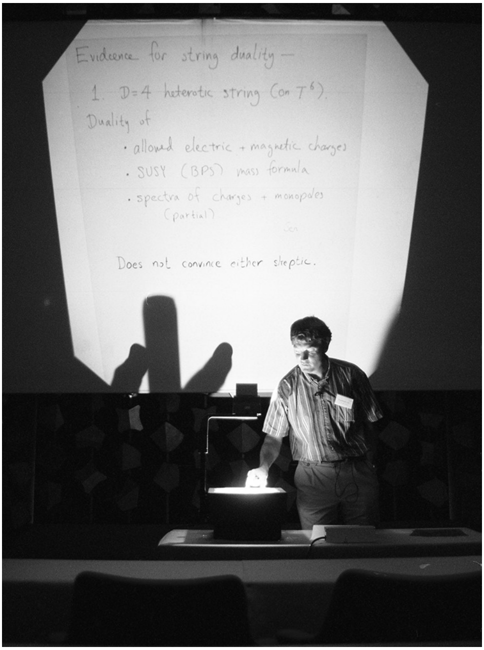


Figure 8.2
Joe giving his lecture at Yukawa Institute for Theoretical Physics in Kyoto, 1995, not far from the laundromat where the central role of D-branes in string theory was discovered. Courtesy of Yoshio Yamagishi.

The weak/strong duality between heterotic $SO(32)$ strings and type I strings from Witten's talk passed basic tests like the action, but overall it had been little studied. These strings were less clearly connected to phenomenology, and had the additional complications of orientation reversal and open strings. One of the first checks would be to put the system on a small circle, do a

T-duality on both sides, and see if it made sense. And it didn't: there was a range of parameter space where both sides were weakly coupled. But this would be a contradiction: two different results for the same theory.

When I got back to the ITP, I emailed Witten and said that I thought there was an inconsistency with his conjecture. So we first did some calculations of non-BPS states (the BPS states are automatic and do not provide a test), and there were indeed states in the heterotic theory which did not seem to have images in type I. We figured out what the problem had to be. Because of the type I orientifolding, the *T*-dual type I′ theory was not translation invariant. But that would mean that the dilaton is a function of position, and with a nonzero mean value it could still blow up in places, producing additional states. So we calculated this and it worked exactly.

It was an unconventional calculation, with discontinuities along eight-dimensional planes. I referred to these as D8-branes, and Witten asked, "How can these be D8-branes, they are supersymmetric?" And I explained that D-branes are BPS states, carrying Ramond-Ramond (R-R) charges. Witten seemed astonished, and said that I should write this up (I don't think we met in person, and don't recall whether his astonishment was conveyed by phone or email). So I dropped everything and wrote.

The paper took just a little over a week to write. Most of it was a careful presentation of what was in the papers with Cai, and Dai and Leigh. But there was one new calculation that I felt was needed. D-branes whose dimensions sum to six form an electric-magnetic pair, and so their charges had to satisfy the Dirac-Nepomechie-Teitelboim quantization, $qq' = 2\pi n$ for integer n.

Otherwise, the theory could not be consistently quantized. The calculation was of my favorite kind, a vacuum amplitude, but now with a cylinder bounded by two D-branes.[6]

On my first pass through, n came out to be $\pi\sqrt{2}$, and on my second it was $1/\sqrt{2}$. Either of these would be inconsistent, but these are the standard kind of error that one gets with a new calculation. On the third, it was exactly $n=1$. The D-branes exactly saturated the quantized charges, strongly suggesting that these were the sources of R-R fields, which previously had no weakly coupled limit and were just identified as charged singularities.

CHARGED D-BRANES

Joe taught us that D-branes behave as higher-dimensional analogues of charged particles. They are the source of a higher-dimensional version of the electromagnetic field known as the Ramond-Ramond gauge field. In a similar fashion to charged particles, the electric or magnetic charge of a p (space) dimensional D-brane is equal, by Gauss's law, to the integral of the corresponding field over a $p+2$-dimensional ball around it. The resulting charges are the smallest they can be while also satisfying Dirac's quantization condition, a nontrivial check of the internal consistency of string theory.

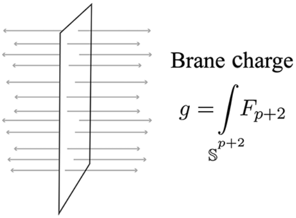


Figure 8.2a

Even as I was doing this, I started getting messages from people who had heard from Witten that I was writing an important new paper. And so I began to realize that I had finally, at the ripe old age of forty-one, done something that had changed the direction of science. More than that, it was a shock wave, for me and the rest of the field. I had been living with D-branes for eight years, but never taking them too seriously because of the lack of heterotic D-branes (still true at weak coupling, but now they were needed to understand strong coupling). But for almost everyone else, the role of D-branes was a new thing: string theory was no longer just string theory; it had D-branes as well. These made many new calculations possible, and rather suddenly string theory became D-brane theory. Of course, two years later it became AdS/CFT theory, which is still our most complete picture.[7]

For eight years, D-branes had belonged to me and a few other fans, but now it was out in the world for everyone. Within weeks, people found implications that I had never expected. Witten used them to calculate the bound states of strings and branes. Douglas, and Witten, connected D-branes to instantons. Witten and I finished our paper, which also demonstrated the duality between type I D1-branes and heterotic strings. Strominger determined the rules for branes to end on other branes. Townsend, and Schwarz, worked out the connection between D-branes and the eleven-dimensional M2-branes (the M now added by Witten to denote the eleven-dimensional theory). Vafa, with Bershadsky and Sadov and with Ooguri, did something topological with them that I did not understand and did not fully approve of.[8] Bachas determined the scattering amplitudes for D-branes. Strominger and I wrote a paper on D-branes and Calabi-Yau manifolds which was mostly his (he is always generous); my main contribution was an early study of brane creation at brane crossings. And a few days into the new year, Strominger and Vafa (SV) used D-branes to give a statistical interpretation to the black hole entropy for the first time.

You might wonder, how long had I known the answer to Witten's question? I had known that D9-branes were BPS states that coupled to R-R fields since my work with Cai, and that all D*p*-branes were equivalent under *T*-duality since my work with Dai

D-BRANES' DELAYED FAME

The figure below shows the yearly number of citations of Joe's first two D-brane papers. The community's appreciation of them lagged by a few years.

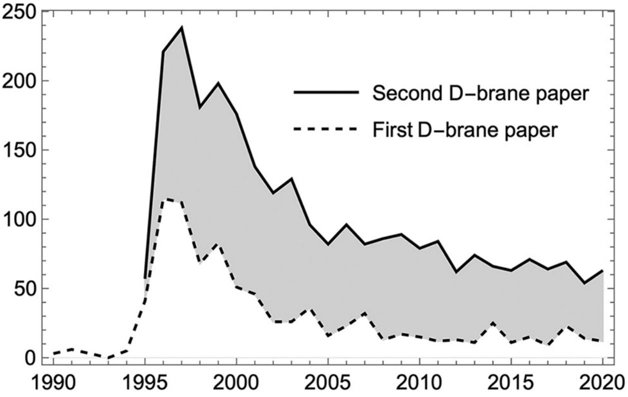


Figure 8.2b

and Leigh. But I think that I only fully connected them when Witten asked the question.

8.6 FAMILY TIME

This may seem like an odd point for this change of subject. But in fact it is fitting, because just as my physics career was taking this spectacular jump, most of my mental energy was actually being spent on coaching Steven and Daniel's roller hockey team.

Both Dorothy and I were mostly unathletic before college, her from going to Catholic schools and me from general nerdiness. But in college we both enjoyed sports, and we met playing volleyball in our first graduate year. When our first son, Steven, came along this accelerated. From the age of one or so he wanted me to be throwing or kicking a ball to him all the time. Daniel seemed more easygoing, but he also joined in, and so life for us largely centered on sports.

Steven started playing roller hockey when he was six, and after a few years I was asked to coach. This did not come naturally to me. Even teaching physics had always made me anxious, and here I had no expertise. I took it on, and so spent the quarter mostly figuring that out. But this was the exact same time D-branes came along: somehow it all worked out.

Just to finish this subject, even Dorothy started playing roller hockey. For a while, all four of us were on the same team. Eventually, I switched to biking in the heights around Santa Barbara, Steven switched to ice hockey, and Daniel to wrestling and martial arts.[9]

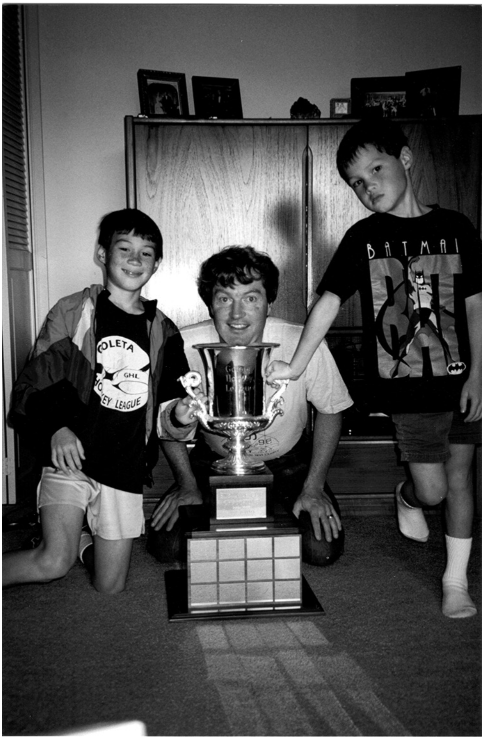


Figure 8.3
Joe the hockey coach for sons Steven and Daniel in Santa Barbara, ca. 1996

The whole family graduated from Berkeley, in spite of Paul Martin's advice. Steven graduated in economics and statistics. After several years working in the financial industry, he is pursuing work in psychology. Daniel graduated in molecular and cell biology, became a doctor of pharmacy at UC San Francisco, and is now a pharmacist at the Stanford Medical Center.

Notes

1. I am repeating this secondhand, and may get corrected. I hope that the best bits are true.

2. There is a history of the IAS with the title “Who Got Einstein’s Office?” Wilczek did not get the office, but he did negotiate to get Einstein’s house when he moved there. And I got Wilczek’s office at the ITP. So at one point, the tentative title of this memoir was “Who Got the Office of the Guy Who Got Einstein’s House?”

3. [The string coupling is e^{ϕ}, and so taking ϕ to $-\phi$ is the same as replacing the coupling with its inverse.—Ed.]

4. In his talk, Witten introduced the term *M-theory* to denote the eleven-dimensional theory dual to type IIA and heterotic $E_8 \times E_8$. The M might turn out to be *mystery*, *magic*, or *membrane*, when the theory was better known. BFSS (9.2) would add *matrix*. The term “M-theory” refers also to the full quantum theory, with all the dual limits.

5. I always had the feeling that I was embarrassing Makoto, as when I wandered off the usual paths, or lost my train ticket and had to try to explain to the conductor where I was supposed to be.

6. A diagram representing this calculation is featured on the cover of this memoir.

7. This book has not had much about my personal life, but here is an odd note. My son Steven was playing roller hockey, and a coach was needed. I had no aptitude for this, but neither did anyone else, so I volunteered. This was very stressful for me, and it came at the same time as the D-branes. What I remember is that the coaching consumed much more of my attention and energy than the D-branes.

8. I have told Vafa that one of my life goals is to understand one of his papers, but no success yet.

9. Though Dorothy and I have tunneled to a new fixed point, pickleball.