



Joseph G. Polchinski
1954–2018

BIOGRAPHICAL

Memoirs

A Biographical Memoir by
Edward Witten

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JOSEPH G. POLCHINSKI

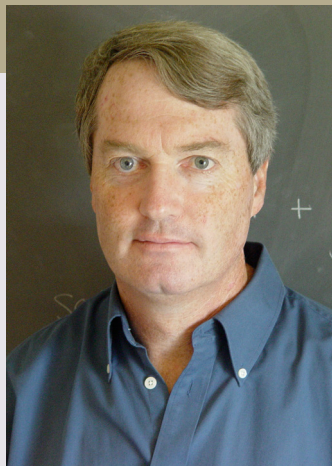
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Elected to the NAS, 2005

Joseph Gerard Polchinski was a highly creative and influential theoretical physicist and a leader in the quest for a deeper understanding of the laws of nature. He was a bold thinker who would blaze his own path, often opening new directions that others then followed.

In Joe's last year, when illness made it difficult to continue his work in theoretical physics, he wrote an insightful memoir of his life and scientific career [1] (which I will quote from at many points in what follows). His reflections on the process of learning to do theoretical physics, and on the specific scientific problems that he grappled with, make interesting reading, either for newcomers to the field looking for inspiration or for colleagues who lived through the period and knew Joe and his work.

In his memoir, Joe describes a long battle with shyness, through his youth and into early adulthood. But he developed into a highly interactive scientist with many collaborators on multiple projects and was extremely well-liked by his colleagues.



By Edward Witten

Growing Up

Joe Polchinski was born and spent his early years in the small town of Hawthorne, New York. He was very interested in science from a young age, with a passion at age six for the How and Why Wonder Books of Science. In his memoir, he describes himself as one of the few students to thrive on the “New Math” of the space race era of the late 1950s and early 1960s, in which children were taught set theory ahead of arithmetic. But he writes that he “missed the full benefit of the New Math” when his family moved to Tucson, Arizona, after he finished fifth grade.

In Tucson, Joe quickly got ahead in math, but soon ran out of math and science classes at his school. An attempt (as a freshman in high school) to study calculus at the University

of Arizona did not go well. He ended up studying no math or science for a while and then taking the required courses in other subjects in order to graduate early. He writes, however, that with a little more “common sense,” he might have found a better solution to the limitations of his high school and to other obstacles that he met later on in the path to developing as a scientist.

In the meantime, Joe channeled his scientific interests into hobbies—amateur astronomy and chess. He became a fairly proficient amateur astronomer, grinding a “creditable” mirror and building an eight-inch reflecting telescope. “Finding the Crab Nebula was one of my favorite challenges,” he writes. He also developed a passion for chess. “Chess dominated much of my school years. In my last two years, when I had run out of math and science to study in class, I spent many hours studying chess books...” He quickly reached the level of a fairly strong amateur, but—like many young people with scientific interests and a chess hobby—found it surprisingly difficult to get further. Like others, he learned the hard way that mathematical talent only carries over into chess up to a certain point.

A new world opened up for Joe in the fall of 1971 when he arrived at Caltech as a freshman. Finally, he had the chance to pursue his real interests in earnest. “Whenever I am asked where I am from, I always want to answer ‘Caltech.’ ...Caltech was so formative in my life...[compared] to anything that came before.”

Joe thrived in the Caltech environment and recounts many stories of physics and friendship at Caltech. Richard Feynman was an “idol,” and he had Kip Thorne, the pioneer of gravitational wave research and 2017 Nobel Laureate, as a freshman advisor. He experienced “heaven” in the form of summer research in the lab of Tom Tombrello, leading to his first publication [2]. “Happily, only one non-science course per quarter was required.”

Graduate School

Joe arrived at Berkeley as a graduate student in the fall of 1975, attracted by Berkeley’s storied history of leadership in particle physics. Berkeley, like Caltech, had long been a leading center of particle theory (and experiment). But Joe soon came to understand something that he had not fully appreciated when he was choosing a graduate school. In the years just before he started graduate school, particle physics had taken a dramatic turn, with the emergence of what is now known as the Standard Model of particle physics, in which all of the usual elementary particles forces are described by quantum

gauge theories, similar in concept, though not in detail, to the Quantum Electrodynamics that had been developed decades earlier to describe electromagnetism. All this had brought quantum field theory and new ideas and questions about quantum field theory to center stage.

When Joe came to look for an advisor, some of the distinguished professors at Berkeley were not interested in advising a student in these new areas. But luckily there was an option—Stanley Mandelstam. Mandelstam was well known for his work on S-matrix theory and on the Dual Resonance Model—the precursor of string theory. Unlike some others with that background, Mandelstam became actively involved in the new questions that were opened up by the emergence of the Standard Model.

To appreciate the thesis problem that Mandelstam posed to Joe, one should be familiar with the concept of electric-magnetic duality. I will describe this in some detail, as it will also play an important role in our story later on. In the absence of charges and currents, Maxwell's classical equations of electromagnetism are invariant under the exchange of electricity and magnetism. In the real world, the symmetry between electricity and magnetism is lost because we observe electric charges and we do not see magnetic charges—magnetic monopoles. But a classical physicist could well imagine a world with charges of both kinds and perfect symmetry between them.

Quantum mechanics at first sight seems to make a symmetry between electricity and magnetism impossible. To write a Schrödinger equation for an electron in a magnetic field, one needs to describe the electromagnetic field using a vector potential, and once one does that some of Maxwell's equations become trivial identities and it seems that it is not possible to introduce magnetic charges. Paul Dirac, however, had shown in 1931 that a magnetic monopole is possible in quantum mechanics, but only if its magnetic charge obeys a very special condition. The magnetic charge g of the monopole and the electric charge e of any possible electrically charged particle, such as the electron, have to be related by $eg = 2\pi n\hbar c$, where \hbar and c are Planck's constant and the speed of light, and n has to be an integer. Dirac's idea was that if a magnetic monopole exists, this would explain why electric charge is quantized in nature, since the electric charge of any magnetically neutral particle (that is, any particle that we have actually observed) would have to be an integer multiple of $2\pi\hbar c/g$, where g is the monopole charge. By the time Joe was a graduate student, Dirac's idea was somewhat in eclipse, partly because magnetic monopoles had not been discovered and partly because Dirac's proposal seemed to be only one possible explanation of quantization of electric charge. There were alternative

options such as Kaluza-Klein theory and “grand unified” gauge theories of all elementary particle interactions. But Dirac’s approach has actually made a comeback. It has been found that all (known) theories that explain quantization of electric charge do have magnetic monopoles, so it seems that Dirac’s idea was correct even if not the whole story. Actually, Joe’s later work on D-branes was an important part of this emerging understanding, but it was still far in the future during his graduate school days. As for why magnetic monopoles have not been observed, this question was part of the impetus for the theory of cosmic inflation [3], which dilutes the monopole abundance of the universe to an extremely low level and may well be the reason that we have not observed monopoles.

By the time that Joe was a graduate student, it was understood that magnetic monopoles can appear as “solitons” or classical “lumps” of energy in gauge theories of elementary particles [4, 5]. For weak coupling or in other words near the classical limit, where gauge theory is well understood, these magnetic monopoles are completely different from electrically charged particles, which arise as quanta of the field, rather than as classical lumps. However, it is not obvious what happens for strong coupling and by the mid-1970s, there were concrete proposals that, in the strong coupling world, there can be complete symmetry between electric and magnetic charges [6]. This is somewhat analogous to Kramers-Wannier duality between high and low temperatures in the two-dimensional Ising model. The conjectured symmetry became known as electric-magnetic duality. At the time, electric-magnetic duality in four spacetime dimensions was a wild speculation, thoroughly unverifiable because of the difficulty of understanding strong coupling.

Mandelstam’s interests involved a slightly different aspect of the relation between electric and magnetic charge. The central mystery of the strong interactions was confinement of quarks. In the Standard Model, protons and neutrons are made of more microscopic objects known as “quarks.” The quarks carry “color” or “color electric charge,” roughly a conserved quantum number somewhat analogous to ordinary electric charge. But although the quarks are supposedly more fundamental than the protons or neutrons, we never see an isolated quark. Quarks are always bound together into color-neutral particles such as protons or neutrons. This permanent binding of quarks is called “confinement.” From a modern standpoint, quark confinement is the central and most surprising observation about the strong interactions. To understand it more fully remains a fascinating challenge, even today. Asymptotic freedom of nonabelian gauge theory [7, 8] gives a partial answer, and more insight came from lattice models and from computer simulations. But even today, we do not understand quark confinement as well as we would like.

Mandelstam's insight was that quark confinement can be understood as a sort of dual version of superconductivity. (Related ideas were also developed by Gerard 't Hooft and Yoichiro Nambu.) A superconductor exhibits a Meissner effect: magnetic fields are expelled from a superconductor. If one could insert a magnetic monopole into a superconductor, the Meissner effect would force its magnetic flux to collapse into a thin tube, called an Abrikosov-Gorkov flux tube. (Monopoles are not available, but Abrikosov-Gorkov flux tubes are created and observed in the lab in other ways.) As a result, magnetically charged objects would be "confined" in a superconductor: for energetic reasons, they would always appear in magnetically neutral combinations. Mandelstam's idea was that confinement of quarks in the strong interactions is a dual version of this, with electricity and magnetism exchanged (and with ordinary electric charge replaced by the color electric charge of the strong interactions). It is a beautiful proposal, which is now actually believed to be qualitatively correct, but it was very difficult to develop the idea much further.

The thesis problem that Mandelstam proposed to Joe was to give a precise definition of the 't Hooft loop operator. This operator is supposed to describe the response of a system when probed by an external magnetic charge, somewhat as the Wilson loop operator - which was better understood and was already known to be useful in strong interaction theory - describes the response to an external electric charge. Understanding the 't Hooft operator was supposed to be a step towards understanding the relationship between electricity and magnetism, and thereby, understanding quark confinement. But it was difficult to get a good understanding of the 't Hooft loop operator with the methods available at the time. Joe struggled and obtained an answer of sorts, which was the main content of his dissertation. But he was never entirely satisfied with what he had. (What is now regarded as the most natural definition was given almost thirty years later [9].)

"Mandelstam was always generous with his time," Joe writes. "But he was a difficult advisor, because his thinking was deep, but his explanations were often oracular. So I was never sure if I was making progress. Sometimes, in response to a question, he would turn to the blackboard and just think for several minutes before responding. I never knew whether this meant that this was a good question or a dumb one."

Postdoctoral Years

Joe had barely published as a student, largely because of the difficult nature of his thesis problem, and he was relatively slow to start publishing extensively in his postdoctoral years. Fortunately, very strong recommendations from Mandelstam and a few other

senior professors helped him get excellent postdoctoral positions at Stanford and then Harvard. He began his postdoctoral years attempting to rigorously prove Mandelstam's picture, only reluctantly accepting that this was out of reach. This part of the story is easy for me to understand, since I personally experienced a similar obsession with the problem of quark confinement during much the same period, and like Joe, I only reluctantly accepted that this problem was too hard. I am sure we were not the only ones. Over time, Joe started working on more accessible problems and with other physicists. "It would take me a while to realize that it is important not just to write papers but to give talks about them—not only to get attention, but to be forced to clarify your work, think it through, and get valuable feedback."

At Stanford, Joe gradually started working with colleagues on supersymmetric models of particle physics. This work introduced him to a much wider range of physics questions and methods—and physicists—and it was the work by which he first became known, though he was to make much more significant contributions later. His first paper that attracted relatively wide attention [10], written with co-authors at Stanford, explained why a certain supersymmetric interaction is not renormalized beyond one-loop order. One of his papers on supersymmetry from the postdoctoral years [11], written with Luis Alvarez-Gaumé and Mark Wise during his second postdoc at Harvard, was really quite influential (over 1200 citations). The most important result of this paper was a fairly natural mechanism to spontaneously break the weak interaction gauge symmetry, as observed in nature. At the time, the top quark had not yet been discovered, but was expected to exist because it was needed to avoid an inconsistency in the Standard Model. Their approach to supersymmetry and the weak interactions required a very large mass for the top quark. Such a large mass was not a popular idea at the time, but it did turn out to be the right answer when the top quark was finally discovered a dozen years later. Supersymmetry has not yet been discovered experimentally, but their approach to gauge symmetry breaking may still be relevant in the future if supersymmetry eventually turns up at energies somewhat higher than we have so far been able to probe.

Since his student days, Joe had been dissatisfied with the foundations of quantum field theory. Quantum field theory is the framework in which we understand elementary particle physics, but to make quantum field theory work—in the perturbative regime where we understand it best—requires renormalization, a process of canceling troublesome infinities so as to get meaningful, finite answers that can be compared to experiment. The proofs of renormalizability were highly technical, too technical for such a fundamental statement. At a happy moment while auditing lectures on renormalization

by John Preskill, Joe realized that a better and more transparent way was possible by following the logic of Ken Wilson's approach to field theory.

"What bothered me was that the proofs that renormalization works seemed extremely combinatoric and technical, but the results in the end came down to 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...It took just three weeks for me to work out the proof and write it up."

The resulting paper [12] was his first influential single-author paper, with nearly 1100 citations. He continues, "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." Joe acknowledged that most of the ideas were known in principle, largely in the work of Ken Wilson, and he was putting things together in a new way. In essence, Wilson with his ideas about the renormalization group had changed the way that one thinks about quantum field theory, but his ideas had not been incorporated in proofs of renormalizability; this was Joe's contribution.

At Harvard, Joe also spent quite a bit of time "ambulance chasing"—trying to explain exciting experimental results that turned out to be wrong. Up to a point, this was actually a good way to learn a lot of physics. He also gained exposure to some of the favorite maxims of Howard Georgi, including one that he quotes repeatedly in his memoir: "Don't hide your light under a bushel basket." Joe writes that this remark (which echoes a verse from the Book of Matthew) "was apparently a Biblical injunction against slow publication."

First Faculty Position

In the fall of 1984, Joe took up his first faculty position, in Steve Weinberg's group at the University of Texas at Austin, where he remained for eight years. By coincidence, that was also the year that string theory came to be taken seriously as an approach to unification of physical law. This resulted from a new discovery by Michael Green and John Schwarz concerning anomaly cancellation in string theory [13], after which the unified models of particle forces and gravity that could be deduced from string theory became far more compelling. From this point on, string theory was one of Joe's main interests.

At Austin, he worked on many different topics with many young people, especially students. “Early on,” he writes, “I collaborated more easily with students than with postdocs, and I conjectured that this was because they were better at doing what I told them to do.”

Two of his papers with students from the Austin years deserve particular attention because they turned out to pave the way for work he did later that really did change the direction of the field.

One of these papers, with Yunhai Cai [14], contained a new derivation and explanation of the Green-Schwarz anomaly cancellation that had triggered much of the excitement in string theory. Their derivation used methods [15, 16] that had not yet existed when Green and Schwarz did their work. Their explanation involved a nonpropagating field whose equation of motion accounted for the anomaly.

The second paper, with Rob Leigh and Jin Dai, requires more explanation. I have already mentioned that electric-magnetic duality was a wild conjecture from the 1970s. It was still almost entirely unsubstantiated a decade later during Joe’s years in Austin. However, by this time, an analogous but simpler duality, even more similar to the Kramers-Wannier duality of the Ising model, was well established in string theory. This is T-duality, a quantum symmetry that in string theory establishes an equivalence between spacetimes that would be quite inequivalent in field theory or simply in ordinary geometry. For example, a very small spacetime can be equivalent to a large one. This nonclassical symmetry of string theory is generally felt to be an important clue concerning the deeper geometry that may underlie the theory, though its full significance is probably not entirely understood even today.

Based on what was understood after the anomaly cancellation work of Green and Schwarz and the discovery of the heterotic string [17], there were five possible string theories: Type I superstring theory, two versions of Type II superstring theory, and two versions of the heterotic string. One of the five string theories—the $E_8 \times E_8$ heterotic string—was the leading candidate for describing Nature. But five string theories seemed like too much of a good thing; if one of them describes the real world, of what use are the other four? Prior to Joe’s work with Dai and Leigh, T-duality had been used to make some progress in answering this question or at least in reducing the number of independent string theories. Essentially based on ideas in [18], T-duality had been used to show that the two versions of the heterotic string are really different aspects of the same theory, in the sense that one can make a continuous transition between them.

Dai, Leigh, and Polchinski [19] applied similar methods to the other string theories. (Some of their work overlapped with results in [20, 21].) They discovered that the two versions of the Type II superstring were related to each other in much the same way as the two versions of the heterotic string. This was a nice result, but—in view of the close analogy with what was already known for the heterotic string—not revolutionary. But something potentially revolutionary did come up when they applied T-duality to the Type I superstring. By this time, there was much interest in supersymmetric membranes—higher dimensional analogs of superstrings [22, 23]. In fact, this idea had originated in part from earlier work of Joe with two other students, James Hughes and Jun Liu [24]. The term “brane” had been coined to refer to a membrane-like object of unspecified dimension (generalizing a string in one dimension and a membrane in two dimensions). Applying T-duality to the Type I superstring, Dai, Leigh, and Polchinski found that they could map it to a Type II superstring, but in a world with brane-like impurities that they called D-branes (the letter “D” referred to Dirichlet boundary conditions that were used in their construction). Their branes could have an arbitrary dimension, depending on the precise setup that they considered. This was potentially revolutionary, not so much because it appeared to again shorten the list of distinct string theories, but because it was a solid indication that branes must be part of the general understanding of string theory, and moreover, the construction of these branes showed that their properties were highly computable.

I can well remember my opinion at the time concerning the Cai-Polchinski and Dai-Leigh-Polchinski papers. The Dai-Leigh-Polchinski paper was very interesting. I did not have any idea what to do with it, and I would not have had the vision to call it potentially revolutionary. But I definitely understood that their relation between a Type I superstring theory and a Type II superstring theory with brane impurities was fundamentally unlike anything else that was known. On the other hand, I really did not appreciate the importance of the Cai-Polchinski paper. It seemed like a slightly improved derivation of a known result.

Of his work in Austin, Joe wrote, "Looking over the papers I wrote while in Austin... most of them seem to be written not to discover new things, but to explain what we already knew, perhaps in a clearer way. This led to a lot of fairly forgettable papers, but also some nice ones, though none that changed the direction of the field."

At the time, I would have placed the Cai-Polchinski paper in that category, though later events showed otherwise.

About his paper with Dai and Leigh, Joe wrote, "I did not appreciate what I had done....I gave zero talks about the paper, my lack of confidence and common sense stopping me. I had forgotten Georgi's maxim, 'Don't hide your light under a bushel basket.' Had I given a few talks, someone in the audience, or just the effort of writing the talk, might have led to the missing connections [between this paper and the Cai-Polchinski paper]. But it took me six years to make the connections."

There were two other major developments during Joe's years in Austin, though neither one involved his research at the time. First, "in the summer of 1988, having realized that I would never be a great scientist, I decided to write a book on string theory." Joe had just taught a course on string theory from a modern point of view emphasizing the role of conformal invariance, and thought that in a year he could turn his lecture notes into a book. It actually ended up—he estimates—taking 30 percent of his time for nine years. Though he was too polite to say so directly in his memoir, his decision to write a book must have partly reflected disappointment that a previous book [25] (of which I am a co-author) did not incorporate more of conformal field theory. Joe's book, once it finally did appear, has been widely used.

The last development that I will mention from Joe's years in Austin concerned the energy density of the vacuum or equivalently Einstein's cosmological constant (CC). Here the question is why the vacuum energy is so small, well over one hundred orders of magnitude smaller than one might expect on dimensional grounds. In fact, it is so small that at the time it was generally assumed to be precisely zero.

As a postdoc, Joe had been well acquainted with this problem, and had thought a lot about the attempts—none of them compelling—to solve it via microphysics. "Most string theorists, having seen such remarkable properties as T-duality, expected that string theory had some trick that we had not yet figured out."

But there was no progress in that direction. And a number of field theory ideas generated temporary excitement but seemed to flame out. "There was one other new CC idea out there," he writes, "the anthropic principle." As articulated by Steve Weinberg [26], the idea is that if the constants of nature including the cosmological constant can take different values in different regions of space and time—or in different branches of the quantum mechanical wavefunction—then galaxies or other complex structure will only form in those regions (or branches) in which the cosmological constant is sufficiently small. Assuming that observers who can discuss the question can only exist in regions where galaxies—or at least complex structures of some kind—have formed, it follows

that any observer would see a cosmological constant that is not too large. From this point of view, the cosmological constant has to be small—to be conservative, it cannot be more than perhaps 100 times the average matter density in the universe—but there is no reason for it to be exactly zero. So in fact, the cosmological constant was predicted to be not too much smaller than the then current observational upper bound.

“This was remarkable to me,” Joe writes, “and upsetting. This problem that I was spending much of my time on, which was supposed to be the clue as to the nature of quantum gravity, did not need a solution, it was nearly automatic. But it required giving up the idea that (understanding) the constants of nature, the lifetime goal for me and for my colleagues, was possible: it depended on details of astrophysics and partly even biology...And there were already signs of a nonzero CC, such as the age problem (stars apparently older than the universe), which would be solved if there were a nonzero CC. So I spent the next ten years hoping that the evidence for this would go away. I do not know how many others were in the same state. To me, Weinberg’s argument was so clear, and should have been known to everyone. But I had the benefit of talking to Weinberg in person, as well as my long history of unsuccessful attempts. Most others would find it easier to continue their denial. My fretting would have been much better spent asking, does string theory produce the dynamics needed for Weinberg’s argument? Fortunately, the question was still there for Raphael Bousso and me ten years later. It was a measure of the general ‘anthropic denial’ that no one else asked this question first.”

Moving To Santa Barbara

In 1992, Joe was recruited as a “permanent member” of the National Science Foundation-created Institute for Theoretical Physics (ITP) at the University of California, Santa Barbara. The job of the permanent members, apart from their own research, was primarily to run the programs, typically of five months each, for visiting members. Generally each year one (or more) of the three main programs would be particularly close to Joe’s interests. Joe spent the rest of his career at Santa Barbara. He thrived in the highly interdisciplinary and interactive environment of the ITP and did his best work there.

One of the first programs going on at the ITP after Joe arrived was devoted to the quantum physics of black holes. Here there were actually multiple puzzles. The most basic involved black hole entropy. According to Jacob Bekenstein and Stephen Hawking, a black hole has at the quantum level an entropy $A/4G\hbar$, where A is the area of the black hole horizon and G is Newton’s constant. This entropy is inversely proportional to \hbar and therefore extremely large in ordinary terms. For example, the Bekenstein-Hawking

entropy of a black hole with the mass of the Sun is roughly 10^{20} times the entropy of the Sun. In the rest of physics, entropy is the logarithm of the number of states. If black hole entropy is the logarithm of the number of quantum states, then the number of quantum states available to a black hole is incredibly large, something like $10^{10^{77}}$ states for a black hole of solar mass. No one had any idea how to count the quantum states of a black hole and come up with such an incredibly big number. Quantization of the classical solutions of General Relativity was certainly not going to work, at least not in any obvious way, since the “no hair” theorem says that at the classical level, a black hole is completely featureless.

There was also a more refined set of questions related to Stephen Hawking’s discovery that black holes in quantum theory do not live forever but “evaporate.” Hawking’s calculation seemed to show that the final state in black hole evaporation is nearly thermal, a highly “mixed” state in the quantum mechanical sense rather than a pure quantum state. It seemed that this would have to be so even for a black hole that starts out in a pure state. But evolution of a pure state to a mixed one contradicts the basic unitarity of quantum mechanical evolution. In fact, Hawking had claimed that conventional quantum mechanical evolution breaks down in black hole evaporation. Many other physicists suspected that, somehow, the laws of quantum mechanics would hold true. But no one knew how.

None of these questions were answered at the Santa Barbara workshop, and opinions were divided about the most likely way forward. “As for myself,” Joe writes, “I was a natural agnostic, going back and forth among the possibilities, looking for a resolution.” Joe wrote his first papers on quantum black holes in this period (for example, [27]), but his most important contributions came later.

D-Branes and RR Charge

Ideas about duality had always been important for Joe’s work. As a graduate student, when Joe worked on Mandelstam’s approach to quark confinement, the background to this work had included the wildly optimistic speculations of that period about electric-magnetic duality in four dimensions. And T-duality of string theory was the starting point for his paper with Dai and Leigh, which was still slumbering in obscurity when he moved to Santa Barbara. The paper was almost totally unknown, to judge from citation counts.

The years 1993-5 were a turning point. (It is not possible to provide here a full account of this period. See for example [28-35].) Ideas of duality were revived and extended beyond what most of us had thought possible. Imitating the old wild conjectures about electric-magnetic duality in gauge theory, new conjectures were put forward that were supposed to govern the behavior of string theory for “strong coupling,” when ordinary methods of calculation break down. The new conjectures were modeled on the old ones, and they were just as wild. But there was a new reason to believe them: the new duality conjectures for string theory had a striking mathematical analogy with T-duality, which was already known to be correct in a different corner of the string theory world (small volume rather than strong coupling), and had been extensively explored by Narain [18], Dai, Leigh, and Polchinski [19], and others.

The problem with the old duality conjectures in gauge theory, going back to the original one [6], was that they had always seemed impossible to use or test in any way. They had long lingered in a hazy world, tantalizing and unknowable. Suddenly new approaches made it possible to test the old duality conjectures and it became clear that they—and an amazing web of generalizations—were all true.

Moreover, by mid-1995, similar arguments were applied in string theory and it became clear that an analogous picture holds in string theory and governs its strong coupling behavior. Extending what had been discovered using T-duality, it emerged that, in a sense, there is really only one string theory. The five originally known string theories seem like different theories in the weak coupling, large volume realm in which they were discovered, but once one understands what happens when the volume is small or the coupling large, one can smoothly interpolate from one string theory to another.

Beyond this, string theory turned out to be important even if one is only interested in understanding ordinary quantum field theory. Many field theory dualities that previously were unknown or untestable turned out to be deducible from statements in string theory, and in many cases that is how they could be best understood. Strong coupling became much more understandable than before, both in string theory and in field theory. It even became possible to make new models of quark confinement, qualitatively vindicating the ideas of Mandelstam, Nambu, and 't Hooft about the dual superconductor.

Of course, there were gaps. On a cosmic plane, whatever ideas one had about the mysterious deeper geometry underlying string theory were turned upside down and needed a radical rethink. Any thoughts on those questions, which remain unresolved even today,

were turned upside down again a couple of years later by the discovery of holographic duality between gauge theory and gravity [36]. This will be important later in our story.

At a more down-to-earth level, one of the main ingredients in the string duality picture was very little understood. To make string duality work, one had to assume the existence of “branes” that carried a type of conserved charge known as Ramond-Ramond or RR charge. Not much was understood about these branes. To the extent that they were known at all, they were known from classical solutions, which actually were of unclear validity (since it was not clear how to interpret the singularities that they contained) and in any event seemed to give only very limited information.

Joe Polchinski suddenly solved this problem in the fall of 1995 [37]. He did so simply by combining the Cai-Polchinski and Dai-Leigh-Polchinski papers from his Austin years. The Cai-Polchinski calculation could be reinterpreted to mean that a certain object, a D-brane filling all of space, carries RR charge. And then a T-duality argument, or a relatively simple repetition of the Cai-Polchinski calculation, showed that all of the D-branes of Dai, Leigh, and Polchinski carry their appropriate RR charges.

Moreover, the D-branes of various dimensions carried a full set of electric and magnetic RR charges. Joe’s computation of these charges exhibited in a most elegant way Dirac’s quantization of magnetic charge, and its generalization to branes by Nepomechie and Teitelboim.

So the mysterious objects with RR charge were the D-branes that had been sitting in an obscure and little-read journal article since Dai, Leigh, and Polchinski had published about them in 1989. I can well remember how electrifying it was when Joe told me this in the fall of 1995. It was an incredibly simple and unexpected answer. And because D-branes are so simple, it was clear that it was going to be possible to understand and calculate a lot of things that had been out of reach before.

Indeed, Joe’s paper on the RR charges was followed by a sort of D-brane gold rush, in which many of us participated. For a year or so, it was possible to do very interesting things with D-branes that were not that difficult if one had the right idea. One result in particular stands out. This concerned the entropy of a black hole, one of the topics of that first workshop when Joe had just arrived at the ITP. By imagining a black hole made of D-branes and exploiting the simplicity of D-brane theory, Strominger and Vafa [38] did the first solid counting of the quantum states of a black hole, beautifully recovering the Bekenstein-Hawking formula. Black hole entropy really is the logarithm of the

number of quantum states. In the 20+ years since then, the Strominger-Vafa calculation has been repeated, generalized, and sharpened in an incredible variety of ways, with D-brane theory as one of the main tools.

For D-brane theory, and the computation of black hole entropy that it enabled, Polchinski, Strominger, and Vafa received the 2017 Breakthrough Prize in Fundamental Physics.

I had the good fortune to be one of the first—possibly the first—to whom Joe explained his discovery about RR charge. The reason that this happened is that at the time he and I were discussing an apparent contradiction in the conjectured duality between Type I superstring theory and one of the heterotic string theories. The puzzle involved T-duality, so it was obvious that the Dai-Leigh-Polchinski paper was relevant. We had a partial explanation, but the pieces did not completely fit together until Joe's discovery about RR charge. Once that was available, we were able to get a very satisfying answer with very strong support for this conjectured duality. Our paper [39] was submitted to the arXiv just 18 days after Polchinski's paper on the RR charges of D-branes.

Writing about this period, Joe says, "I began to realize that I had finally, at the ripe old age of 41, 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 seriously. But for almost everyone else, it was a new thing: string theory was no longer just string theory, it had D-branes as well. These made many new calculations possible."

The Cosmological Constant

In 1998, there came a big surprise, arguably the most dramatic experimental find in fundamental physics since Joe's undergraduate days, when the discovery of a new elementary particle (the J/Ψ) with surprising properties had unexpectedly confirmed the existence of quarks.

Gravity was expected to cause the expansion of the universe to slow down. Astronomers had tried to measure this effect for decades, but a convincing measurement had not been possible. Finally, by 1998, a new method involving observations of distant supernovae made it possible to measure how the expansion rate of the universe was changing. But the result was the opposite of what was expected. Instead of slowing down, the expansion of the universe is accelerating [40, 41].

The gravitation of ordinary matter cannot explain this, because gravity is attractive for all ordinary matter, whether galaxies or dark matter. The simplest interpretation is in terms of an extremely small but non-zero value of the energy density of the vacuum, Einstein’s cosmological constant (CC). Other interpretations of the acceleration of the cosmic expansion require more elaborate theoretical models and assumptions, for which as of yet there is no observational evidence.

The accelerating universe was most definitely a fundamental observation about gravity, but—like their colleagues in other areas of theoretical physics—most string theorists did not know what to say about it. “My own reaction was different,” Joe writes, “from my interactions with Weinberg. I had half-expected the CC, and had feared it...” because of its implications for the quest to understand fundamental physics.

But he was interested in investigating whether the anthropic explanation of the cosmological constant could be realized in string theory. He tackled this problem with Raphael Bousso, then a postdoc at Stanford.

There were a variety of difficulties. The observed value of the cosmological constant is roughly about 10^{-120} in natural units. In other words, it is about 10^{120} times smaller than one would expect just on the basis of chance. To make it plausible that just by chance there is a vacuum state with an energy density as small as we observe (and thus small enough to let us be here to discuss it), one needs a theory with at least 10^{120} vacuum states. It is not immediately obvious what sort of natural theory, not contrived specifically for this purpose, would have such a huge number of vacuum states.

There had been a few previous attempts to find a model with the right properties. One ingredient often used was a neutral scalar field with a suitably chosen potential. Another ingredient was a three-form field whose four-form “flux” or field strength would control the value of the cosmological constant. There were a variety of problems. It is difficult to get 10^{120} or more vacuum states from a simple model without artificial assumptions. When one manages to do so in a relatively natural way, there are cosmological perils; some models that aim to get a small enough cosmological constant this way end up predicting an empty universe. See for example [42, 43] for a discussion of these approaches and their difficulties.

One common feature of all the attempts prior to that of Polchinski and Bousso is that the authors were economical in the ingredients that they assumed. They tried to make a model with a single neutral scalar field or a single three-form field. However, attempts

to use string theory to make models of elementary particle physics had told a different tale. Starting in the mid-1980s, when Green-Schwarz anomaly cancellation [13] and the discovery of the heterotic string [17] made it possible to use string theory to make interesting and semi-realistic models of elementary particles coupled to gravity, all such attempts had generated a plethora of extra structures. Depending on the precise assumptions made, a model of elementary particles and gravity derived from string theory might well generate not a single neutral scalar field or a single three-fold field, but hundreds of fields at each type. No one constructing these models wanted all those fields. I speak here from experience, as I was one of the protagonists. One or two neutral scalar fields, for example, might be useful to solve some problem of particle physics or cosmology (for example, to generate cosmological dark matter, or to solve what is known as the strong CP problem of particle physics). But no one had wanted hundreds of them. They were unwanted guests at the dinner table.

Polchinski and Bousso were actually the first to find a good use for the extra structures generated in string theory [44]. They reasoned as follows: Suppose that some model has a single three-form field. Its flux is quantized by a version of Dirac-Nepomechie-Teitelboim quantization. Up to a certain point, each value of the flux will correspond to a different vacuum state of the model. If the flux is too large, it generates such a large energy density that the model breaks down. The precise number of vacuum states that one can make with a single three-form field is model-dependent. A number like 10 or 100 is reasonable, but not 10^{120} . As an example, let us suppose that with a single three-form field one can make 50 vacuum states. Except by an incredible coincidence, none of those 50 vacuum states will have a cosmological constant (vacuum energy density) nearly small enough to represent the real world.

Now let us suppose that one has instead 100 three-form fields, each with a flux that can take 50 values. Since the fluxes can be chosen independently, one can now construct $100^{50} = 10^{500}$ vacuum states. So with these numbers, we can easily get so many vacuum states that there will be numerous vacua with energy of order the observed value, or even smaller. On an anthropic interpretation of the universe, one would expect that we would be living in a typical vacuum state, under the constraint that the cosmological constant should be sufficiently small to allow the formation of complex structure, and possibly under other (presumably less stringent) anthropic constraints.

Polchinski and Bousso called this structure a “discretuum” of vacua, where the term is meant to suggest a discrete set of vacua that are so numerous as to resemble in some

respects a continuum. In this construction, not only is the energy density different in each vacuum, but the effective laws of physics—the masses and interactions of the elementary particles—are likewise different.

Polchinski and Bousso went on to analyze the cosmology of their model. Inflationary cosmology gives a natural mechanism to populate the states of their discretuum. One simply starts in some state with a positive (and presumably large) value of the cosmological constant. This causes cosmic inflation [3], a very rapid exponential growth of the universe. Starting from an exponentially expanding state of high energy, occasionally a quantum tunneling process will occur that will typically reduce the vacuum energy. The universe then jumps in some local region to a different point in the discretuum. In an exponentially expanding universe, such events will occur independently in many different places in spacetime. Whenever a jump leads to a point in the discretuum at which the vacuum energy density is still large, inflation goes on. New tunneling events will occur, usually further reducing the vacuum energy. Soon the whole discretuum, or at least a vast part of it, is populated—including the rare vacuum in which the cosmological constant is small enough that it is possible for complex structures to form.

In such a model, what we usually think of as the Big Bang was really the last tunneling event that occurred on our past worldline. This is the tunneling event by which the part of the universe that we are living in transitioned to the vacuum state that we see around us. If this last Big Bang triggered the sort of processes that are usually assumed in inflationary cosmology, then it will be a hot Big Bang like the one that astronomers have observed.

If something like this is correct, then physics as we know it prevails in our—vast—corner of the universe, but matters are quite different elsewhere. Some very general principles like relativity theory and quantum mechanics will presumably hold everywhere, but details like the masses and coupling of the elementary particles are different in each region. It has been suggested (by Martin Rees) that a “universe” of this sort might be better called a “multiverse” and that those “laws” of nature that are different in each vacuum might better be called “bylaws.”

Joe writes that although he understood perfectly well that their model called for an anthropic interpretation, he was very reluctant to say so in the paper for sociological reasons.

“I thought it [the anthropic interpretation of the cosmological constant] was so compelling that even experimentalists would realize that they were measuring random numbers, and be discouraged. I did not want to be the cause of that. But of course I overestimated ...the credence that experimentalists gave to theorists...As Bousso and [Leonard] Susskind [another relatively early advocate of the anthropic interpretation] both knew, it is wrong to suppress what you know. Georgi again: ‘Do not hide your light under a bushel basket.’”

The Bousso-Polchinski paper had a large impact when it appeared, and has remained influential. However, views have always been mixed. Many physicists are open to the possibility that the anthropic explanation of the smallness of the cosmological constant may be correct, and some find it aesthetically pleasing. Those with that outlook generally have considered it significant that the rather special dynamics needed to make the anthropic approach possible arises spontaneously—as an uninvited guest—in a class of models that were developed with completely different motivations. On the other hand, many physicists strongly reject anthropic explanations, for precisely the reasons that Joe described in writing about his years in Austin. Finally, there is a large group of agnostics—physicists who do not necessarily reject the anthropic explanation and in some cases consider it the best that we have now, but who still hope that a more conventional scientific explanation may appear, leading to a unique answer.

Two further developments are worth mentioning. First, some assumptions made by Bousso and Polchinski were justified much more fully three years later when Shamit Kachru, Renata Kallosh, Andrei Linde, and Sandip Trivedi (KKLT) constructed string theory vacua with positive cosmological constant in a more elaborate analysis [45]. This built in part on work of Kachru and Polchinski with Steve Giddings [46]. One of Joe’s last papers [47] was a re-evaluation of the KKLT construction using one of his favorite tools, effective field theory. The conclusion (generally accepted in the community) was that it holds up, despite numerous criticisms over the years.



Joe was a passionate cyclist, one of his favorite hobbies.

Finally, there is a classic problem in particle physics that can be interpreted as a cousin of the cosmological constant problem. Why is the mass of the Higgs particle so small in natural units—less than the mass scale of gravity by roughly a factor of 10^{16} ? In the Standard Model of particle physics, the Higgs particle sets the mass scale of the known elementary particles. Its relative lightness is called the hierarchy problem. Since the 1970s, physicists generally believed that if one could reach experimentally the energy needed to discover the Higgs particle, one would also discover a new mechanism that governs its mass. In the jargon, this would be a natural explanation of the hierarchy problem. However, the Higgs particle was discovered in the year 2012 at the LHC accelerator at CERN, and no natural explanation of the hierarchy problem has appeared, even though the LHC has actually reached energies significantly above what was needed to discover the Higgs particle.

The hierarchy problem could have an anthropic explanation, because, like the smallness of the cosmological constant, the relative lightness of the Higgs particle is needed to make possible the formation of complex structure in the universe. So the outcome of the LHC experiments can be taken as another indication in favor of an anthropic interpretation of the universe. However, it is prudent to note that new discoveries in either particle physics or cosmology might change the picture.

Other Projects

Before moving on to the last major chapter in Joe’s career, I will briefly mention a few of his other contributions.

Joe had a recurrent interest in relations between ideas and methods of particle physics and those of condensed matter physics. His lecture notes applying effective field theory to analyze metals and superconductors were widely read [48]. In his early days at Santa Barbara, with Charles Kane and Matthew Fisher, he helped apply his favorite conformal field theory methods to a problem involving the quantum Hall effect [49]. One of the key insights coming from string theory was holographic duality between ordinary quantum field theories and gravity. Joe became very interested in applications of this type of duality to condensed matter physics. This led to a number of papers such as a model of what condensed matter physicists know as “strange metals” [50].

With D-branes and other new string theory methods, Joe was able, starting in the late 1990s, to make some progress on some of the problems of quantum field theory that had been so vexing in his student days. With KITP postdoc Matt Strassler [51], he developed a beautiful model applying D-branes and other techniques to solve a model in which

they could demonstrate quark confinement and Mandelstam-Nambu-'t Hooft duality. It must have been satisfying to finally be able to say something about quark confinement. Joe went on with Strassler and other coauthors to apply string theory ideas to various aspects of high energy scattering at strong coupling [52, 53]. The last of those papers was one of the starting points of “conformal Regge theory,” now an important topic.

Some intractable-looking problems in two-dimensional mathematical physics are “integrable”—they have hidden symmetries that makes it possible to solve them. The first example was the Ising model, solved by Lars Onsager in 1944. Integrability developed into a major topic in two-dimensional mathematical physics. But are there integrable models in four spacetime dimensions? For decades there had been speculations that four-dimensional gauge theory might be integrable in the limit of a large gauge group. A breakthrough showing that a version of this statement is true was made by Joe with his former student Iosif Bena and ITP postdoc Radu Roiban [56]. This is now a major area of research.

Theoretical physicists have imagined all sorts of exotic objects, from magnetic monopoles to cosmic strings, that might have been produced in the early universe. However, the successes of inflationary cosmology suggest that many of these objects will not be observable in practice, even if they do exist. Cosmic inflation is very likely to reduce the abundance of monopoles and other exotic particles to an absurdly low level. But cosmic strings are expected to survive, so they may be our best chance to observe a really exotic relic of the early universe. Moreover, models such as the KKLT model do predict the existence of cosmic strings. For both of these reasons, Joe became very interested in cosmic strings. With Edmund Copeland and Robert Myers, he showed that KKLT strings have distinctive properties that might conceivably be observable [54]. He extended this analysis with graduate student fellows Nick Jones and Mark Jackson [55]. Cosmic strings have not yet been discovered, but there is still hope.

Gravity and the Firewall

In the mid-2000s, Joe experienced what he calls “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.”

At some point, his response was to resolve to concentrate on the fundamental question, “What is quantum gravity?” As he explains, “Even with the anthropic principle looming, the problem of finding the theory of quantum gravity remained...Moreover, it was the kind of problem that might be solved by theoretical reasoning alone.”

The most important aspect of his work on quantum gravity began at a KITP program in the spring of 2012 on “Bits, Branes, and Black Holes.” (The ITP had received an endowment from the Kavli Foundation and had been rebranded the KITP.) The name was different but the issues under discussion were familiar from the first ITP program that Joe had participated in upon arriving in Santa Barbara 20 years earlier. Is black hole evaporation consistent with the laws of quantum mechanics? If so, what is wrong with Hawking’s calculation, which appears to show that the final state in the evaporation of a black hole is almost purely thermal? Do our concepts of spacetime need to be modified to reconcile quantum mechanics with black hole physics?

To explain the issues in somewhat more detail, recall that in the classical world, there is no problem to copy a bit of information. If one has a message written on a sheet of paper, one can make a photocopy of the page and generate a copy of the original message. Quantum mechanically, matters are different. The basic unit of quantum information is a qubit, the quantum state of a spin $1/2$ particle. There is no way to copy a qubit, because any attempt to study or manipulate a quantum system will disturb it. Now consider a qubit that is thrown into a black hole. In classical General Relativity, nothing happens to the qubit (or anything else) when it crosses the horizon of the black hole, the surface of no return. An observer who jumps into the black hole could observe the qubit and measure its information content, to the fullest extent that quantum mechanics allows. On the other hand, according to Hawking, the black hole is evaporating quantum mechanically. If it is true that black hole evaporation satisfies the laws of quantum mechanics, this means that an outside observer could, in principle, extract the quantum information in the initial qubit by a careful measurement of the outgoing Hawking radiation. An extremely complex joint measurement of many quanta of outgoing Hawking radiation would be required, but quantum mechanics would allow such a measurement. It seems, then, that there are two copies of the original qubit: one copy is inside the black hole and is visible to the observer who jumps in behind the horizon, and a second copy can be detected in the Hawking radiation. A more detailed analysis using classical General Relativity seems to show that both copies exist simultaneously on the same spacelike hypersurface, an apparent violation of the concepts of quantum mechanics.

“Black hole complementarity” had been proposed as a way to resolve or at least postpone this problem. In its simplest version, black hole complementarity is simply the claim that there is no real contradiction, since the observer who falls into the black hole and the observer at infinity cannot communicate with each other. The idea of black hole



Receiving the Frontier Prize in Fundamental Physics in 2013, with wife Dorothy Chun and sons Steven (right) and Daniel (left).

complementarity is that both the outside observer and the observer who falls behind the horizon would describe their observations in a conventional quantum mechanical framework, but these descriptions would be observer-dependent and no one quantum description would apply for both observers.

At the beginning of the KITP program, Joe and Ted Jacobson presented perspectives on the black hole information problem. Joe writes that he was surprised at the responses of the participants. Almost all believed that black hole evaporation would ultimately turn out to be

consistent with quantum mechanics. On this particular issue, there was much greater consensus than there had been twenty years before, in large part because powerful evidence for the unitarity of black hole evaporation had come from holographic duality between ordinary quantum systems and gravity. But there was no consensus concerning the validity or implications of black hole complementarity, and on the contrary there was massive confusion about what black hole complementarity means. After reading the description of black hole complementarity in the last paragraph, the reader may sympathize with this.

So Joe took on the problem of constructing a toy model of black hole complementarity to make its meaning clear. He set this problem to two of his experienced graduate students, James Sully and Ahmed Almheiri, both of whom turned out to thrive on this problem and the new directions it opened up. After a time, the three of them also collaborated with Don Marolf, one of the senior physicists at UC-Santa Barbara. The more closely they looked at the problem of making a model of black hole complementarity, the more it did not work. The basic problem was that if it is true that an infalling observer sees nothing special when crossing the horizon, then modes of a quantum field just outside the horizon, which are going to escape as Hawking radiation, must be quantum mechanically entangled with modes just inside the horizon, which are going to fall into the black hole. This was known from Hawking's original work on black hole evaporation. On the other hand, suppose that black hole evaporation satisfies the laws of quantum

mechanics, implying that all the Hawking radiation emitted over the full lifetime of a black hole is in a quantum mechanical pure state. Then, once a black hole has lived most of its life, each new Hawking quantum must be highly entangled with the quanta that have already been emitted. But quantum mechanics does not allow the same quantum of Hawking radiation to be simultaneously highly entangled with two different systems—in this case, the modes just behind the horizon and the earlier Hawking radiation. This last statement is a close cousin of a fact that I explained earlier, that quantum information cannot be copied.

The conclusion [57] was that either (i) quantum mechanics is not valid in black hole evaporation, (ii) the effective field theory assumed in the analysis needs modification even at macroscopic distances, or (iii) the assumption that an infalling observer sees nothing special when crossing the horizon is not correct. None of the authors favored option (i), since holographic duality had given powerful evidence that black hole evaporation as observed from the outside can be described in the language of ordinary quantum mechanics. Each of the other options is radical in its own way. Effective field theory at long distances is the general framework in which physics is understood, and is almost impossible to modify in a consistent way. The fact that nothing special happens at the black hole horizon is a very clear prediction of classical General Relativity, and is again almost impossible to modify in a consistent way, because classical General Relativity says that we cannot even know where the black hole horizon is without a full knowledge of what is going to happen in the future.

Option (iii) was described as the existence of a “firewall” at the black hole horizon, and the problem became known as the rewall paradox, or the AMPS paradox, after the last initials of the authors. (Some of the ideas had been anticipated by Samir Mathur [58].) The firewall paradox had a tremendous impact among physicists interested in quantum gravity, and beyond. In subsequent work, it was studied, tested, probed, and reformulated in many ways, and many partial or possible resolutions were proposed.

The story was covered in *The New York Times* the following year [59]. Calling Polchinski “one of the theorists who set off this confusion,” the *Times* quoted him as saying, “It points to something missing in our understanding of gravity.” The *Times* went on to report that Polchinski was not satisfied with any of the proposed explanations up to that point, quoting him again: “My current thinking is that all the arguments that we are having are the kind of arguments that you make when you don’t have a theory. We need a more complete theory of gravity...Maybe ‘space-time from [quantum] entanglement’ is the right place to start...I am not sure.”

Summing Up

Joe Polchinski liked to quote a dictum of Paul Dirac, who used to say that one must have the courage to follow a theory where it leads. Throughout his career, but especially with his work on the cosmological constant and the multiverse, Joe showed himself a real follower of this dictum.

In the epilogue to his memoir, written when he was already gravely ill, Joe wrote that "[my life] has taken a rather linear path, from the How and Why Wonder Books to today, with few deviations. I have not achieved my early science fiction goals, nor explained why there is something rather than nothing, but I have had an impact on the most fundamental questions of science. But it was a close thing: at the age of 40, you could say that I had not lived up to my potential. And if someone else had stepped in during the six or more years between my finding D-branes and figuring out what they were good for, that might still be true."

"How far are we from finding the fundamental theory of physics, and what will we learn from it? Again, I am an agnostic, not good at predicting things. I only follow my nose...So we may be close, or we may still have big steps ahead. I hope to help figure this out."

By his own assessment, Joe "shook up" theoretical physics three times, with D-branes, the string multiverse, and firewalls. Of these, D-branes are an established part of mathematical physics, with far-reaching implications for string theory and quantum field theory. The paper of Bousso and Polchinski, putting forward a natural dynamics that could underlie the anthropic interpretation of the cosmological constant, has remained highly influential for two decades. It may be viewed in the long run as a real milestone in physics, but realistically it will take a while before we know if this is the case. The firewall paradox greatly sharpened the thinking of physicists about the issues involved in combining quantum mechanics and gravity. It has changed the direction of the field since it was put forward eight years ago. As of this writing, it appears that the firewall paradox, or at least some versions of it, may be en route to resolution [60, 61]. Sadly, Joe did not live to see this, though one of his former students (Almheiri) has been a major force in this work.

In addition to the Breakthrough Prize, Joe Polchinski's awards included the Dannie Heinemann Prize of the American Physical Society and the Dirac Medal of the International Center for Theoretical Physics in Trieste. He was elected to the National Academy of Sciences in 2005. He is survived by his wife Dorothy Maria Chun, a professor of applied linguistics, whom he met and married in graduate school, their sons Steven and Daniel, and his sister Cindy Reid.

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