3 BERKELEY, 1975–1980

3.1 MOVING ON

After four years, it was time to choose a graduate school. I knew that I wanted to do theoretical physics, and my choices came down to Berkeley, Stanford, Harvard, and Princeton. I tried to be scientific about my choice, but had a strong leaning for Berkeley: several friends, including Zajc, were going there and I liked California. Also, I had a nice Hertz fellowship, which at the time was restricted to a small number of schools, including Berkeley.

At the time, Hertz had a strong defense orientation. One might think that liberal Berkeley would be ruled out, but it also had a close connection with nearby Lawrence Livermore Lab, designer of nuclear weapons. I took the fellowship and went to Berkeley. Participation in defense was not at all mandatory, aside from a pledge to aid the US in time of need. My own connection was limited to a one-day tour of Lawrence Livermore (but only outside the security fence, because I did not have a security clearance).

There was one off-note in my grad school search. Paul Martin, the Harvard physics chair, was passing through and stopped to do some recruiting. His main argument that I should go to Harvard was that East Coast physics was better than West Coast physics. I had never heard of such a thing; Berkeley after all had a great history. So I did not take this seriously, and off to Berkeley it was. If he had been more direct, things might have gone differently.

3.2 FINDING A MAJOR, AND THE EAST-WEST DIVIDE

I was one of those students who thought I had to learn everything before starting research. Retaking QFT and relativity were essential, of course. Field theory was taught again from Bjorken and Drell. So I learned the equations this time, but I still thought that the basic principles were not clear. At least relativity was taught by Weinberg, so I got that subject down, though from the point of view of a particle theorist, not a relativist.

I had a vague notion that I should learn more about the other possible majors, but I really knew that I was going to end up in particle physics. And so somewhat belatedly I began to look for an advisor. And then I learned what the Harvard recruiter was saying. With Caltech, the dominance of two charismatic professors, Feynman and Gell-Mann, had slowed the reaction to the discovery of asymptotic freedom and all that it implies. At Berkeley, another charismatic professor had the same effect.

In 1948, Feynman, Schwinger, Tomonaga, and Dyson had found the correct theory that incorporated quantum mechanics, special relativity, and electromagnetism. The theory, known as quantum electrodynamics (QED), was based on the principle of QFT. It made predictions of enormous accuracy. Thus, it was natural to look for the same kind of theory for the nuclear force. It would have to be much stronger: the coupling constant in QED is a convenient 1/137; for the strong force it would need to be around 1.

But this did not seem to work, to describe the strong force in this way. Thus it was that throughout the 1960s there was a search for new approaches.[1] One idea that attracted wide attention was the bootstrap: the idea that one did not need fields, but just a few principles like Lorentz invariance and crossing. This idea was suggested by some of the data, in particular the presence of a large spectrum of massive excited states. The leader of the bootstrap program was Geoffrey Chew, of Berkeley. He was said to be religious in his fervor for the idea. Even notoriously strong-willed field theorists such as David Gross and Steven Weinberg, who went through Berkeley as grad student or postdoc, were afraid to mention QFT.

The bootstrap did lead to some interesting work. It was elaborated into the dual resonance model, which in turn led to string theory. But this was the solution to the wrong problem, and too soon. Thus, when asymptotic freedom was discovered and the strong interaction was understood, the work was done not at Berkeley, but on the East Coast. Even four years later, when I was looking for an advisor, there was almost no one working on QFT. One prospective advisor told me, “You should have gone to

S-MATRIX

The scattering-matrix (S-matrix) is a tabulation of all possible experiments against all potential outcomes. Each entry of this matrix is a *probability amplitude*—a quantum version of probability—for a collection of particles to transition between a pair of initial and final quantum states. QFT provides a *local* description of this transition in spacetime by breaking it down into a step-by-step process in terms of a sum of Feynman diagrams. This is an infinite sum that is well-defined and finite provided that the coupling constant of the theory is small. Otherwise, this gives an infinite nonsensical answer.

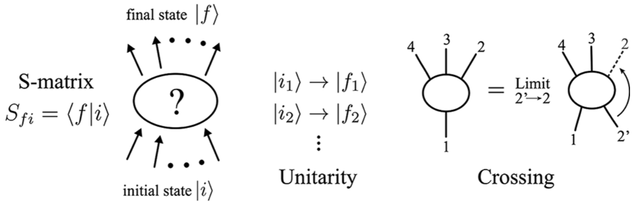


Figure 3.0a

Hence, if the theory had a large coupling constant—a strongly coupled theory—then QFT appears *not* to be an appropriate description. The S-matrix of a strongly coupled theory would have to be constructed by other means. The S-matrix bootstrap program was one such attempt, with the hope that general quantum mechanical principles would be sufficient to completely determine all physically possible S-matrices. These principles included the requirement that the S-matrix provide a one-to-one mapping between initial and final states (unitarity), symmetry, and that different processes are related by exchanging incoming and outgoing particles (crossing).

Harvard." Another told me he had stopped supervising students. But fortunately there was one choice.

3.3 MANDELSTAM

If Geoff Chew was the spiritual leader of the bootstrap, Stanley Mandelstam was its engine, solving the difficult problems that were required. When the world tilted from the bootstrap to QFT, it took Mandelstam a little while to catch up, but when he did, he did it in his own powerful fashion. It was never Mandelstam's style to start with some easy problem to learn from. He chose the most important, and most difficult, problems.[2]

The problem that he focused on in QCD was the nature of quark confinement. The fundamental fields of QCD, the quarks and gluons, are not seen directly in experiment. Rather, we see mesons and hadrons, which are bound states of the quarks. Somehow the quarks are prevented from escaping to be seen as individual particles. Asymptotic freedom points to the idea: the growing coupling at long distance binds the quarks. But a more explicit demonstration was needed.

The idea that Mandelstam hit upon was suggested by superconductivity. In superconductors, the Meissner effect repels magnetic fields from the superconductor. If one inserted a magnetic source into a superconductor, it would be confined into a tube and grow linearly. So what was needed to confine quarks, which are electric sources, was a *dual superconductor*, with electric and magnetic fields interchanged. Indeed, this suggested a duality symmetry, the equivalence of electric and magnetic theories under change of variables.

So Mandelstam seemed like the only choice, and I asked him if I could do research with him. He hesitated, probably because he had just taken on two other students, but then agreed.

Students are generally started off with a warm-up problem. This is for the student to get oriented to the advisor's research, and for the advisor to gauge the student. But as Mandelstam only worked on the hardest problems, he naturally gave the same to his students. My warm-up was to find a QFT that had both electrically and magnetically charged particles. This is in contrast to the known theory of QED, which has only electric charges.

I was unable to solve this problem, and I gave Mandelstam an argument why it was impossible. In a theory with both electric and magnetic charges, Dirac showed that quantum mechanics requires the electric charge e and the magnetic charge g had to be quantized, $eg = 2\pi$. For QED, $e = 1/\sqrt{137}$ is very small, and so we can use perturbation theory, expanding around $e = 0$. But when e goes to zero, g goes to infinity. In this limit, we had no way to calculate, or even to know if the theory made sense.

In fact, there is a solution, though I don't know if this is what Mandelstam had in mind. It requires the electric and magnetic objects to be different: the electric charges would be ordinary field quanta, but the magnetic charges would be solitons—sort of like bound states of many particles. I am not sure if this was within the intended bounds of the problem, but it involved too many new ideas for me.

In fact, it now seems that *every* quantum theory with electric charges also has magnetic charges. I may not have been the first to enunciate this principle, but I have made frequent use of it, especially with D-branes, and I will of course return to that. What

DIRAC CHARGE QUANTIZATION

The combination of electromagnetism and quantum mechanics results in interesting constraints on the physics of magnetic monopoles, supposing they exist. By virtue of being a monopole, the inverse square law decay of its magnetic field implies that its charge is determined by Gauss's law: it is equal to the integral of the magnetic field over an enclosing spherical shell.

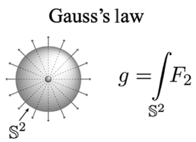


Figure 3.0b

In the presence of a magnetic field, the probability amplitude for an electrically charged particle to travel along a loop is proportional to a *quantum phase*. This is a factor given by the exponential of the product of the particle's electric charge and the integral of the magnetic field on any surface anchored to the loop.

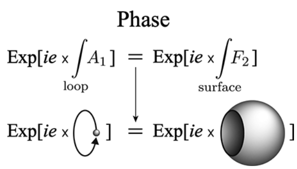


Figure 3.0c

If we imagine that the loop is shrunk to zero, then the integral over the magnetic field becomes the integral over an entire sphere. This is equal to the magnetic charge enclosed, assuming there's a monopole inside, and hence the exponent of the quantum phase would simply be the product of the electric charge of the original particle multiplied by the magnetic charge of the monopole. But since a vanishingly small loop is just like no loop at all, the quantum phase must be equal to one, and thus the product of the electric and magnetic charges must be a multiple of 2π. This is the Dirac quantization condition.

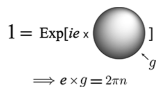


Figure 3.0d

about ordinary QED? I would bet, at high odds, that it does in fact have magnetic charges. But it is unlikely that either side of the bet could ever collect, because one needs to get to the Planck scale to be sure.

So Mandelstam gave me another project, which was intended to be my thesis. This was to construct the 't Hooft vortex operator. Like the earlier problem, it was a part of Mandelstam's broader program to understand confinement in terms of electric-magnetic duality. Ken Wilson had shown that one could distinguish different states in a gauge theory, in particular the confining state, by measuring a certain operator. This operator, defined as the integral of the vector potential along a one-dimensional path, had come to be known as the Wilson loop. Gerard 't Hooft, who was pursuing the idea of confinement from duality independently of Mandelstam, noted that there should be a dual to the Wilson operator, with the electric potential replaced by a magnetic potential. It was my goal to fill in the details of this.

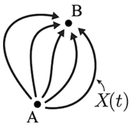
So I met with Mandelstam to discuss this about once a week for a year. Mandelstam was always generous with his time. But he was a difficult advisor: his thinking was deep, but his explanations were often oracular. So I was never sure if I was making progress. Sometimes, in response to a question, he 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.

I have always thought that my project was unsuccessful. But on reviewing it for the first time in a very long while, I realized that I had basically solved the problem. One needed an operator whose physics effect was only in one dimension, and which also had a singular gauge piece acting in three dimensions.

But I also had a lot of irrelevant stuff mixed in. I had not really mastered path integrals, which had not made it into the standard texts. So I was using canonical methods, which were very clumsy for this kind of problem.

PATH INTEGRAL

Quantum mechanics has the peculiar property that particles can be in more than one place at the same time. When a particle is observed at point A and then observed again at point B, it is said to have traveled along all possible paths connecting A and B during the intermediate times. The probability amplitude of these two observations is computed by the *path integral*—the sum of the exponential of the particle's action per path over all possible paths. Counterintuitive quantum phenomena, such as the diffraction pattern of shooting consecutive single particles at a double slit, arise due to the interference between different paths.



$$\langle B|A\rangle = \sum_{\text{paths}} e^{iS(\text{path})} = \int_A^B DX(t)\, e^{iS(X(t))}$$

Figure 3.0e

QFT is even more peculiar, allowing for new particles to be created out of *nothing*. For example, the probability amplitude to start and end without any particles allows for *vacuum fluctuations*—the spontaneous creation of particle and anti-particle pairs that pop out of the vacuum, interact, and then annihilate one another. These processes are represented by *bubble* Feynman diagrams, which collectively contribute to the *vacuum amplitude* of QFT. All of these intermediate configurations of particle and anti-particle pairs make up the different "paths" of the path integral of QFT.

$$\langle 0|0\rangle = \int D\Phi\, e^{iS(\Phi)} =$$

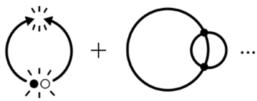


Figure 3.0f

In the case of scattering particles, the intermediate paths are nothing but the different Feynman diagrams connecting the initial and final scattering particles. They also include summing over all possible locations in spacetime where the interactions may occur.

$$\langle 0|\Phi(x_4)\Phi(x_3)\Phi(x_2)\Phi(x_1)|0\rangle = \int D\Phi\, e^{iS(\Phi)}\, \Phi(x_4)\Phi(x_3)\Phi(x_2)\Phi(x_1)$$

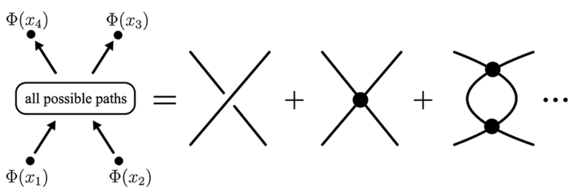


Figure 3.0g

Rather strikingly, my central problem was not clearly solved until twenty-five years later, by Anton Kapustin. This required several new ideas, such as conformal invariance, that had not yet been applied. He was also able to treat the nonabelian case. This was typical of Mandelstam, how far ahead he was in much of his thinking. Another example, the first paper that one studies in the Langlands program today, is the first paper that Mandelstam gave me to read forty years ago.

Another striking example was eleven-dimensional supergravity. I have this distinct memory that on several occasions we would be discussing QFT and quark confinement, and Mandelstam would make some remark about eleven-dimensional supergravity or string theory. This was a bit mind-blowing for someone struggling with four-dimensional QFT, and for whom string theory was assumed to be an artifact from the past. I did not know what to make of this, so I just waited for Mandelstam to come back to earth. But a few years later string theory was back in the center of things, so Mandelstam was as usual well ahead of time.

3.4 COLLEAGUES AND VISITORS

Mandelstam's two other grad students at that time were Susan Elma Moore and Omer Kaymakcalan. Each of Mandelstam's students had a different project, but all were connected in various ways through QFT and QCD. Susan's project was to find variational states of heavy quarks, based on a Wilson loop model of the states. In his own work, Mandelstam was never following the herd, and he guided his students the same way. This could be challenging, as I have noted. In Moore's case, after writing her dissertation she changed fields—first spending some time trying acting, and then ending up as a doctor.

Kaymakcalan's graduate project was to understand the nonabelian properties of the Higgs phase. After his thesis, Omer went to Syracuse and worked on various projects with the members of the Syracuse group. Several of his papers, dealing with chiral Lagrangians, proton decay, and strongly coupled Higgs dynamics, were well cited. Sadly, Kaymakcalan was diagnosed with cancer at around this time, and passed away shortly after.

One notable classmate was Dan Friedan. Friedan stunned me, and I think everyone else, at his PhD seminar, when he showed that Einstein's equation, the basic equation of general relativity, could be reinterpreted in terms of one of the basic objects in QFT, the β function that governs the energy scale. I did not see what this could possibly mean, but a few years later it showed up as one of the key ideas in string theory. (I don't think that this connection was known to Friedan at the time—at least it is not mentioned in his thesis.) He also taught me a key idea in QFT, one that did not appear in textbooks at the time. This was the idea that one could separate operators into *relevant*, *irrelevant*, and *marginal* operators, which is central in organizing QFT.

Friedan had also had some difficulty finding an advisor; he had solved the problem by having a nominal physics advisor but working in fact with Isidor Singer, from the math department.[3] Singer was a famous geometer, best known for the Atiyah-Singer index theorem. He was beginning to take a strong interest in QFT, recognizing that it was going to lead to rich connections between math and physics (a development that would soon accelerate with string theory). So he was always happy to talk about QFT, and was a good sounding board for me.

Another helpful professor was Korkut Bardacki. Like Mandelstam, he was in the middle of making the transition from dual models to QFT. Not as deep as Mandelstam (few are), but sometimes clarity is more useful than depth. Two other professors I recall mostly for their advice: my strongest memory of David Jackson, whose famous text I will come back to much later, was that "It is not enough to be smart; you have to work hard." It was good advice, and much-needed given my lack of common sense. The other was Robert Cahn, a new professor at LBL, the lab affiliated with Berkeley. He also helped to fill the gaps in my common sense, especially when it came to finding my next job. Also, a growing group of postdocs at LBL focused on field theory and particle phenomenology, including Howie Haber, Eliezer Rabinovici, and Ian Hinchliffe. They added some liveliness.

Visiting speakers brought in new ideas that were missing at Berkeley, and these are still some of my strongest memories. Stephen Shenker, a student at Cornell and already a deep and broad thinker, taught me key things about quantum field theory and lattice gauge theories. Sidney Coleman, the famed quantum field theorist and pedagogue from Harvard, spoke about magnetic monopoles, and I was pleased to see that I had independently found some of his results. Lenny Susskind from Stanford, about whom we will hear much more, spoke about *Hot Quark Soup*. The connection between QFT and high temperature was largely new to me, but the Feynman-like presentation was the most memorable part. Another Stanford visitor, the postdoc Stuart Raby, brought the idea of technicolor, strong coupling instead of a Higgs field.

One final visitor was Edward Witten, a postdoc at Harvard. Witten asked me probing questions about Mandelstam's program. This was startling to me, first because he was the first person I'd met who understood Mandelstam's unconventional and technical approach, and second because he understood it better than I did after years of study. I would learn that this was a common reaction to Witten.

3.5 DOROTHY

This is intended as a scientific autobiography, not a personal one. I have included personal bits only to give general background. But of course I have to tell you about Dorothy, whom I met the year I arrived at Berkeley, and married four years later when I graduated. Dorothy had several Caltech connections, including a brother and a former boyfriend. She started as a student at Occidental College, near Caltech, but we did not meet until we were both grad students at Berkeley, she in the German department. We actually met through another Caltech connection, who was now also at Berkeley and active in organizing volleyball games and parties. I decided that I liked her, and spent several difficult months convincing her of the same. So a few days after turning in my dissertation, I was in Hawaii, where most of Dorothy's family lived, and we were married.

Tom Tombrello, whenever we met, would remind me what a good choice I had made in Dorothy. Although I had learned a few social skills at Caltech, I still had many rough edges. Having Dorothy straightened many of these out.

I was always afraid of her asking me why I loved her, because the first answer that came to mind was always that she was the sanest person I knew. It seemed not so romantic, though she had many other wonderful features. But having been around for a while now, I think that in making a list of qualities in a spouse,

Figure 3.1
Joe and Dorothy's wedding in Hawaii, 1980

Figure 3.2
Volleyball team in Berkeley, with Joe on the far right, ca. 1978

being the sanest person you know should be near the top.[4] So, while we each worked our way through grad school, we unwound with food, volleyball, skiing, and friends and family.

3.6 OTHER PHYSICS

In between working on Mandelstam's project, I spent most of my time trying to understand what this quantum field theory is. I read whatever references I could find. There was a short text that had been written by F. Mandl. This nicely complemented Bjorken and

Drell, leaving out many of the technical questions to focus on the physics. A book by Nishijima was also good for some points. Notable was *PCT, Spin and Statistics, and All That*, by Streater and Wightman, to which I will return.

For renormalization theory, the cancellation of infinities that is needed to get the physics out, the classic source was Bogoliubov and Shirkov, which was massive and very technical. Renormalization was presented much the same way in Bogoliubov and Shirkov and in Bjorken and Drell. It was basically a combinatoric argument, summing up the Feynman graphs at each order and showing that the infinities in each cancel. But as an illustration of the difficulty, a previous version of the proof worked for six or fewer loops, but failed for seven. It bothered me that such a key principle depended on such a complicated thread, especially after Friedan and Shenker had shown me that much of renormalization was just dimensional analysis.

But there were plenty of new wonders in QFT, while this one festered. 't Hooft and Polyakov discovered magnetic monopoles in QFT, and Polyakov and his collaborators discovered instantons

OLD RENORMALIZATION

Particles popping out of the vacuum can sometimes interact with scattering particles, resulting in Feynman diagrams with internal loops. The path integral sums over processes of all loop sizes. When the loop shrinks to zero size and the interaction vertices collide, the diagram gives a nonsensical infinite answer. Because small sizes are related to large energies or momentum by the Heisenberg Uncertainty principle, this effect can also be viewed as arising from the infinite amount of energy of the newly created particles.

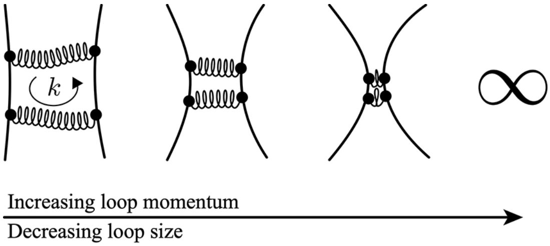


Figure 3.2a

Surely QFT would not be useful if the answer to every question we ask is infinity! Thankfully, finite sensible results can be extracted out of these infinities by adding new terms to the Lagrangian—known as *counter-terms*—that cancel against the unwanted infinities. This procedure is known as renormalization. The coefficients of these new terms are then determined experimentally.

Interactions in the Lagrangian can be classified as *renormalizable* or *nonrenormalizable* depending on whether their infinities can be renormalized or not. While infinities from renormalizable interactions require a finite number of counter-terms to be tamed, nonrenormalizable interactions require an infinite number. This means that an infinite number of experiments are needed to completely specify a Lagrangian with a nonrenormalizable interaction, and hence such theories cannot be used to make any predictions.

Whether an interaction is renormalizable or not can be determined using simple dimensional analysis: renormalizable couplings have negative or zero length dimension, while nonrenormalizable couplings have positive length dimension. These can be deduced from the action using the property that the action itself is dimensionless. Note that this analysis immediately shows that GR contains nonrenormalizable interactions, and hence is not a well-defined quantum theory. This is the first indication of the trouble that lies ahead in trying to combine quantum mechanics and gravity.

$$S_\Phi = \int d^4x \left[\Phi\left(\partial^2 + \overset{l^{-2}}{m^2}\right)\Phi + \overset{l^{0}}{\lambda_4}\Phi^4 + \overset{l^{2}}{\lambda_6}\Phi^6\right]$$

renormalizable non-renormalizable

$$S_{QG} = \int d^4x \left[h\partial^2 h + \partial\Phi\partial\Phi + \overbrace{\underset{l^1}{\sqrt{G_N}}\, h^2\partial^2 h + \underset{l^1}{\sqrt{G_N}}\, h\,\partial\Phi\partial\Phi + \ldots}\right]$$

Figure 3.2b

and their nonperturbative effects. Coleman (rediscovering earlier work in condensed matter) showed that bosons and fermions, the two kinds of quantum statistics, could be turned into one another in 1 + 1 dimensions. He also provided wonderful reviews of these and other QFT topics (spontaneous symmetry breaking, large *N*, topological solitons, . . .). His lectures, first presented over several years at the Erice summer school, and then collected in his text *Aspects of Symmetry*, did much to bridge the pedagogical gap during the 1970s. The papers of Steven Weinberg, beautiful for their clear and systematic presentations, were also a great resource.

3.7 AND MOVING ON

After five years, it was time to write a dissertation. In theoretical physics, the custom was simply to combine one's published papers, often written with one's advisor or others, and insert some amount of overview. But I had a problem: I had written no papers at all (the undergrad papers under Tombrello didn't count). This is extremely rare. The only similar example is the great Ken Wilson, who went years without publishing. But he was busy recasting the nature of QFT. I was simply suffering from a lack of common sense and of any collaborative instinct, and an advisor who was much the same. Somehow, I cobbled together 130 pages about what I had understood about vortex operators, and related issues of field theory. But it is not something that anyone else would benefit from.

Dorothy had two more years to finish her own PhD, so my first choice was Stanford, though I applied to the several dozen departments that did this kind of physics. Not having written any papers was not a fatal flaw. The letters of recommendation from faculty really carry more information. I am sure too that phone calls from my "godfather," Bob Cahn, played a role in allaying concern about this applicant with no papers. So I ended up with my desired position, though not before some agonizing weeks while higher-ranked candidates made their choices.

All in all, this was a chastening time for me. I was used to being at the top of things. But quite a few of my cohort had already published significant work (some with their advisors, but many on their own), including Dan Friedan, Steve Shenker, Mark Wise, John Preskill (the selections for the prestigious Harvard Junior Fellowship), Larry Yaffe, Steve Parke, Subhash Gupta, with Edward Witten a few years older. Most of these have become good friends, and some collaborators, but at the time it was a difficult experience.

The postdoc period is valuable for giving young people exposure to different approaches to physics. For me, with my native lack of sense, my next two stops, at Stanford and Harvard, were particularly important.

Notes

1. It is interesting to look back at the 1967 Solvay meeting. The speculations were fairly radical, like breakdown of spacetime inside the nucleus. Few were near the mark.

2. Whether by inclination or by example, I tend to be the same way. Frank Wilczek once told me, "Joe, an important problem doesn't have to be hard!"

3. Another Berkeley student from that era, Andy Strominger (who we will hear much more about later), solved the problem by transferring to MIT.

4. Her answer for me was, "because you make me laugh." I guess laughter is a good complement to sanity.

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A Journey across the Landscape of Strings, Black Holes, and the Multiverse

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