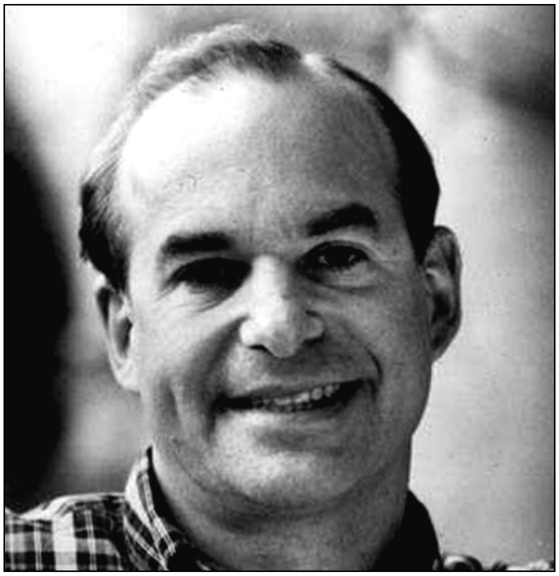

JOHN H. SCHWARZ
(1941 –)

INTERVIEWED BY
SARA LIPPINCOTT

July 21 and 26, 2000

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Physics, string theory

Abstract
An interview in two sessions, July 2000, with John H. Schwarz, Harold Brown Professor of Theoretical Physics in the Division of Physics, Mathematics, and Astronomy. Dr. Schwarz majored in mathematics at Harvard (BA, 1962) and then went to UC Berkeley for graduate work in theoretical physics. He offers recollections of his advisor, Geoffrey Chew; working on S-matrix theory; sharing an office with another future string theorist, David J. Gross. After receiving his PhD in 1966, he became an instructor at Princeton, where in 1969 he began work on string theory, prompted by 1968 paper by Gabriele Veneziano.

He comments on early years of string theory, his collaboration with André Neveu and Joël Scherk, Murray Gell-Mann's interest in the work, being denied tenure at Princeton and invited to come to Caltech as a research associate. General lack of interest in string theory in 1970s. Scherk and Schwarz continue working on it and note that the graviton shows up in the theory, suggesting a way to reconcile quantum theory and general relativity; they publish in 1974 and 1975, but papers are largely ignored. In August 1979, he begins collaboration with Michael Green at CERN and later at Caltech and the Aspen Center for Physics. By now there are several string theories, but all are plagued with anomalies; he

describes their breakthrough elimination of anomalies in 1984 at Aspen and his announcement of it at the Aspen physics cabaret. Comments on sudden burst of interest in string theory, especially at Princeton, and the involvement of Edward Witten. Shortly thereafter, Schwarz is made a full professor at Caltech.

Comments on the antipathy of Sheldon Glashow toward string theory and on his own dislike of the phrase "theory of everything;" on the latter-day history of string theory; problem of existence of five consistent superstring theories; talk by Witten at strings conference, USC, 1995, when it was recognized that the five are part of one underlying theory; discussion of "M" theory and membranes. Comments on annual string conferences, on Witten's visit to Caltech, on joint Caltech-USC physics institute, on prospects for the Large Hadron Collider at CERN and the development of a Supersymmetric Standard Model, on his receipt of the Dirac Medal from the International Center for Theoretical Physics in Trieste in 1989 and his election to the National Academy of Sciences in 1997.

Administrative information

Access

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Schwarz, right, together with his collaborator Michael Green, summer of 1984, Aspen, Colorado.
Courtesy John H. Schwarz.

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ORAL HISTORY PROJECT

INTERVIEW WITH JOHN H. SCHWARZ

BY SARA LIPPINCOTT

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with John H. Schwarz **by Sara Lippincott**

Pasadena, California

Session 1	July 21, 2000
Session 2	July 26, 2000

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LIPPINCOTT: We usually start these interviews by your telling us something about your family background, where you were born, and a little bit about your parents. Do you want to do that?

SCHWARZ: OK. Both of my parents were born in Hungary. My mother was born in Budapest and my father was born in a region of Hungary called Transylvania, which was passed over to Romania after the First World War. But he was born in 1903, well before the First World War, when it was still Hungary. He always felt very strongly that he was Hungarian and not Romanian.

LIPPINCOTT: When did they come over here?

SCHWARZ: Maybe I'll tell you a little bit about them first.

LIPPINCOTT: Oh, please do.

SCHWARZ: Before we bring them to the US. [Laughter] So my parents both went from Hungary to the university in Vienna to study. My father was there studying chemistry. In my mother's case, it was much more unusual in those days for a woman to travel to a foreign land to do university work. I think her parents were somewhat concerned about it. But anyway, she was very insistent and determined and did that.

LIPPINCOTT: Was she a scientist, too?

SCHWARZ: Yes. She was interested in mathematics, philosophy, and physics, and she ended up majoring in physics in Vienna. My father got his PhD there in chemistry in 1932, and my mother got her physics PhD in 1933. When she got her PhD, she was offered a postdoc by Madame Curie, in Paris. [Laughter]

LIPPINCOTT: Oh, fantastic!

SCHWARZ: But that didn't work out, because before she could start the position, Madame Curie died. The lab in Paris was taken over by her daughter, [Irene] Joliot-Curie, and my mother didn't want to work with her.

LIPPINCOTT: Why not?

SCHWARZ: Well, I don't know. I guess she just didn't feel as comfortable with her. So anyway, she got married. And my parents moved to Antwerp, where my father was appointed as director of research at the Gevaert Corporation, a photographic—

LIPPINCOTT: Can you spell that?

SCHWARZ: G-E-V-A-E-R-T. It's now Agfa-Gevaert, but in those days it was just Gevaert. So he went directly from his studies to be director of research there, because he was very outstanding in photographic chemistry. So my parents lived in Antwerp through the latter part of the thirties. My sister was born there in 1935. And they were living there in 1940, when the bombs started falling on them. And so at that point they grabbed my sister, got in the car, and drove to France, leaving all their possessions behind. They decided it was more important to get out quickly.

LIPPINCOTT: Yes!

SCHWARZ: My father's idea was that he could help the Free French government. But Free France wasn't free very long. And so they continued on some complicated trek, via Spain and Portugal, to Brazil, where they were for a few months. The reason they went to Brazil and then to Massachusetts was that these were places where Gevaert had branch offices. One in Rio and another one in North Adams, Massachusetts.

LIPPINCOTT: So the company did everything it could to help them escape?

SCHWARZ: Well, anyway, they gave my father positions at these branch offices. In fact, he was made director of the one in North Adams when he arrived there. They arrived in North Adams in I guess late 1940 or so. And I was born there, in November '41.

LIPPINCOTT: And how was their English when they arrived? Did you all speak English at home when you were small?

SCHWARZ: Well, I was always brought up in English. They felt very strongly about bringing us up in English. They were fluent, of course, in Hungarian and German and French. They certainly didn't want to speak German. [Laughter] Hungarian, they felt, wasn't useful.

LIPPINCOTT: Where did they learn their English?

SCHWARZ: I'm not sure exactly how fluent they were in English, but they were reasonably fluent. They always had some accent.

LIPPINCOTT: But they spoke it at home around you?

SCHWARZ: Yes, yes. And I was raised in English. My sister was originally raised in French; even though Antwerp is the Flemish-speaking part of Belgium, she was raised there in French.

LIPPINCOTT: What does your sister do?

SCHWARZ: Well, she's retired now, but she was working at the Educational Testing Service in Princeton, New Jersey, where she was involved in a variety of activities in test development, foreign languages, and so on. She still does some part-time work for the Peace Corps, testing volunteers on their language proficiency. She has a trip to Africa coming up a couple of weeks from now.

LIPPINCOTT: Does she still live in Princeton?

SCHWARZ: She lives very near it, just a couple of miles away.

LIPPINCOTT: Tell me about North Adams. You went to the elementary schools there?

SCHWARZ: Well, we actually lived in a town called Williamstown, which is where Williams College is, which is just a few miles away from North Adams. We left there when I was about five, so I didn't really go to school there.

LIPPINCOTT: Where did you go then?

SCHWARZ: We moved to Rochester, New York. This had to do with my father's career move. He got disenchanted with Gevaert pretty early on, when he decided they were collaborating with the Nazis at some point.

LIPPINCOTT: Oh. Were they?

SCHWARZ: [Laughter] At some point. Well, after Belgium was occupied, it was hard not to.

LIPPINCOTT: Oh, yes. OK, we're in Rochester. One thing I did want to ask you was whether your mother kept up her physics or not.

SCHWARZ: Well, I think it's a shame, but she really didn't. Later, after my father died, she did some teaching: that was more in philosophy than in physics, which she was always interested in.

LIPPINCOTT: So in Rochester you started your schooling?

SCHWARZ: Yes. I went to elementary school in Rochester. I was started in a private school called the Harley School. My sister was six years ahead of me. She was in the same school.

LIPPINCOTT: Why the private school?

SCHWARZ: Well, I guess there was some concern about the quality of the public schools. They did push me ahead, so I skipped third grade. As a result, I was from then on a year younger than my classmates, which wasn't such a big difference as to be a problem. We were in Rochester for about four or five years, and then—in 1950, I guess it was—we moved to Long Island, another career move of my father. We moved to a town called Glen Head, which is on the north shore of Long Island in Nassau County.

LIPPINCOTT: What was your father doing then?

SCHWARZ: He took a position in a company called Chemco, which was in Glen Cove, near Glen Head. He was the director of research there. Again, that was a company that did photographic work. And he was their director of research, making all sorts of clever inventions which made the owners of the company very wealthy. [Laughter] Not him, but the owners. He was better at his science than negotiating how to get all the financial benefits for himself. When we moved there, I was in fifth grade, in the middle of the school year. So then I went into public school. Actually, I had done one year in public school already in Rochester. My sister stayed in Rochester to finish her last year of high school. They didn't want to switch her out that last year, so she stayed with some friends.

LIPPINCOTT: Did you like being near Long Island Sound? Was it a good place to grow up?

SCHWARZ: It was a reasonable, middle-class type of place. So it was OK.

LIPPINCOTT: How about the school?

SCHWARZ: The school, again, was OK—it was nothing special. It was probably an average, middle-class type of place.

LIPPINCOTT: Were you a pretty good student?

SCHWARZ: Yes. I studied hard. I was the valedictorian and all that kind of stuff. The high school we went to was called Seacliff High School. The last year, they built a new school, which was called the North Shore High School.

LIPPINCOTT: Did you take any physics and math in high school?

SCHWARZ: Yes, I took all the usual courses. They weren't very challenging—it was the standard stuff. My parents, of course, were very encouraging to me in science. But they didn't try to push me ahead, which they might have done.

LIPPINCOTT: When I'm talking to scientists, I like to ask them if they have any early scientific memories. Was there anything that made you aware that this was something you'd end up in, or that interested you?

SCHWARZ: Well, it was clearly where my talents and interests lay. I didn't know exactly what specific area, but I was attracted to math and it came easily to me.

LIPPINCOTT: You majored in math at Harvard.

SCHWARZ: That's right.

LIPPINCOTT: So math was something that interested you even then, in high school?

SCHWARZ: Yes. Well, I didn't know much advanced math, but certainly what I was exposed to came to me very easily. It wasn't challenging.

LIPPINCOTT: Where did you apply besides Harvard? Anywhere else?

SCHWARZ: Yes. In fact, I deliberated long and hard whether to go to Caltech or to Harvard. And I think in the end what made the difference was that going from Long Island in 1958 all the way to California seemed a little too far from home.

LIPPINCOTT: How had you heard of Caltech back East?

SCHWARZ: Well, I don't remember clearly, but certainly we were aware that it was one of the good places to study science. But in any case, I did decide to go to Harvard.

LIPPINCOTT: Were those the only two places you applied?

SCHWARZ: No, I applied to a couple of others. But those were the two I remember. My final decision was between them.

LIPPINCOTT: How did you like Cambridge? Tell me about that.

SCHWARZ: Well, Harvard was good, so I never had any regrets about my choice. I think that worked out well for me.

LIPPINCOTT: Did you go there with the idea of doing math?

SCHWARZ: You know, I never remember having a difficult decision. It was always clear to me what was the next thing to do, even though I maybe didn't see two steps ahead. [Laughter] So when I went there, I just did what seemed obvious to me, which was to major in math.

LIPPINCOTT: Did you have any idea of making it a profession or becoming an academic of any kind?

SCHWARZ: I don't think I really worried about the question very much. I just studied math and enjoyed it. And I also took some physics courses.

LIPPINCOTT: Tell me about those.

SCHWARZ: I just took standard undergraduate physics courses, nothing very special.

LIPPINCOTT: Yes. Who was the guy that was teaching what would be the beginning core curriculum physics course? I. B. Cohen? Does that sound right?

SCHWARZ: Some of my physics teachers were named [Karl] Strauch and [R. V.] Pound. Well, the famous senior person—whom I never had anything to do with—was [Julian] Schwinger.

LIPPINCOTT: Oh, yes, Schwinger was there then. Did you know him or see him, ever?

SCHWARZ: No. He was teaching very advanced courses and I was taking elementary stuff. I got to know him in much later years, after he came to UCLA and I was at Caltech.

LIPPINCOTT: So there wasn't any real physics epiphany while you were there?

SCHWARZ: No, in fact I was struck by the fact that during my undergraduate days almost all the math courses I took I felt were taught very well, and almost all the physics courses I took I felt were taught very poorly.

LIPPINCOTT: Really!

SCHWARZ: [Laughter] Nonetheless, when I went on to graduate school, I decided to switch to physics.

LIPPINCOTT: Yes. That's kind of mysterious. Why is that?

SCHWARZ: Clearly it wasn't based on the quality of teaching I had received. But rather, at some point I became bothered because I didn't see what the point of mathematics was. It seemed to me like just some sort of game, where you make up some clever question and try to answer it. It seemed to me like a game. With physics, it was clear that you were trying to describe the real world, and that seemed to make more sense. But I think I now understand better what mathematics is than I did then—that you're looking for interesting structures, which I think is more than a game. So the characterization I would have made then is not one I would choose today.

LIPPINCOTT: Who taught you math at Harvard? Do you remember?

SCHWARZ: Well, my freshman math was taught by Professor [Lynn H.] Loomis, and Loomis gave a very good course. Other teachers I had included Andrew Gleason, a renowned person. And there were a number of others. My advisor was Garrett Birkhoff.

LIPPINCOTT: So then you applied to Berkeley for graduate work?

SCHWARZ: Well, I applied to three places for graduate school, actually. I applied to Princeton, Caltech, and Berkeley. And Princeton and Caltech turned me down, so I went to Berkeley. [Laughter]

LIPPINCOTT: Oh, that's funny!

SCHWARZ: Well, those other two places are where I spent the rest of my career.

LIPPINCOTT: Yes. And you intended to do physics in graduate school?

SCHWARZ: Yes, theoretical physics. I didn't know exactly what physics, but it was theoretical. So I went to Berkeley, which was the first time I crossed the Mississippi. [Laughter] This was 1962, and even then a trip across country was more of an adventure, maybe, than you'd think of today.

LIPPINCOTT: Did you drive?

SCHWARZ: No, I flew. The first year in Berkeley, I lived in the International House, which I thought was a nice way to meet a diverse group of people.

LIPPINCOTT: Were those all graduate students?

SCHWARZ: I think so. There may have been some undergraduates, but it was mostly graduate school students.

LIPPINCOTT: And Geoffrey Chew was your teacher there?

SCHWARZ: Well, he was the fellow who ended up as my advisor. I think that that relationship got finalized in my second year. Usually in graduate school—at least in theoretical physics—the first year you're just taking courses and learning basics. It's only in the second year that you're really ready to think about research and seeking an advisor.

LIPPINCOTT: What part of physics attracted you then? Do you remember?

SCHWARZ: Well, at the time, it seemed to me—and I guess it still does—that the most interesting and fundamental questions were in particle physics. I don't know that when I entered graduate school I had decided on particle physics. But at some point after arriving in Berkeley, it became clear to me that particle physics was the direction I wanted to go.

LIPPINCOTT: Were you aware of the quark theory, which I think Murray Gell-Mann came out with in '63 and George Zweig at CERN [European Organization for Nuclear Research] at the same time? Did you learn that immediately, more or less, as soon as Gell-Mann published?

SCHWARZ: Yes, I was aware of these developments while they were taking place.

LIPPINCOTT: And that was a turn-on, too, I guess.

SCHWARZ: Yes. But the quark ideas were not that hot in Berkeley. Geoffrey Chew had very much his own slant on how to do things, and I was very strongly influenced by him. In retrospect, I feel very fortunate that I was his student. I think it launched me in the directions I went in the future. So I think it all worked out very well for me.

There were a number of facts that were quite unusual. One was that when I entered Berkeley, the physics department had over 400 graduate students, which is about twice the number they have today in Berkeley.

LIPPINCOTT: Oh, my!

SCHWARZ: So it was far and away the largest physics graduate program in the country—much larger than any at the present time. It was a very big operation, and Geoffrey Chew himself had about ten graduate students at the time when I was one. Many of them I'm still in touch with.

LIPPINCOTT: Who are some of them?

SCHWARZ: Well, far and away the most famous would be David [J.] Gross. David and I entered and graduated at the same time. We shared an office when we were graduate students. And there's a funny story in connection with that. In 1963-1964, Berkeley was building a new physics building, called Birge Hall. And at that time they were trying to hire Murray Gell-Mann at Berkeley. And when the building was completed, Murray hadn't yet made up his mind whether or not to accept. Or at least if he had, he hadn't told them. [Laughter] So they were taking occupancy in this new building, and they had reserved the best office, with a view over the bay and the campus and so on, for Murray. And they couldn't leave it empty. On the other hand, if they put another professor in there, they couldn't kick him out if Murray decided to come. So in their wisdom they decided that the solution was to put two graduate students into this office [laughter], with the idea that they would be much easier to remove if it became necessary. So that was the office that David and I had for the last two and a half years of our graduate career, which was very nice. So that was the first time I was indebted to Murray. I've been indebted to him on numerous subsequent occasions.

LIPPINCOTT: Who else was in your class with Geoffrey Chew, if I can put it that way?

SCHWARZ: Sure. One person is a lady named Ling-lie Chau, who's now a professor at UC Davis. Another one is a guy named Jerry Finkelstein, who's at San Jose State.

LIPPINCOTT: You were already thinking about strings then, weren't you?

SCHWARZ: No, that idea hadn't arisen yet.

LIPPINCOTT: At all?

SCHWARZ: It comes later.

LIPPINCOTT: OK. Tell me about your thesis, then. What was it on?

SCHWARZ: Geoffrey Chew's big program was what was called S-matrix theory, which was an approach to describing the strong nuclear force. And there's a whole, long story in connection with that. He had a very well-defined program, and he was very highly regarded around the world. There were two main approaches to particle theory in those days. One was Chew's S-matrix program, and the other was quantum field theory, which was not very well regarded at that time. It was recognized that it worked well for quantum electrodynamics, but it was believed that it didn't work for the strong interactions. So the S-matrix theory approach of Chew's was viewed as the leading way to think about the strong interactions in those days. And Chew was an extremely influential person. At major international conferences he would give the summary lectures, and so on, and he was in the National Academy. So that's the program I was working on. And the thesis problem he gave me followed a couple of other projects I had done in my graduate studies on the analytic S-matrix. Actually, I think one of the papers I wrote when I was a student, which I co-authored with David Gross, is more interesting than the paper I wrote for my thesis. [Laughter]

LIPPINCOTT: Really?

SCHWARZ: In any case, my thesis was some very uninteresting problem in pi nucleon scattering, which in hindsight is almost a joke. [Laughter] I remember when I left Berkeley for Princeton, where I was hired by Murph [Marvin L.] Goldberger, I remarked to Murph that I found my thesis quite embarrassing. [Laughter] He said, "Don't worry about it. Fermi's was terrible, too."

LIPPINCOTT: Did you select your thesis problem or did Chew?

SCHWARZ: Chew suggested it. It was just kind of a make-work thing—so that wasn't too inspiring. But in any case, I learned a lot in my graduate studies, which served me well subsequently, so I have no regrets about that.

LIPPINCOTT: So you went to Princeton. Was Goldberger influential in getting you there?

SCHWARZ: Yes. You know, Chew and Goldberger were old buddies from when they were both students of Fermi in Chicago. They had written important papers together. It's pretty clear how these things work: Chew calls up Goldberger and says, "Hey, I have two hot students for you, David Gross and John Schwarz." So he offered jobs to both of us. But David actually went to Harvard as a junior fellow at that point, and I went to Princeton.

I was hired in the position of an instructor. That's not a position that's used nowadays at that level for a brand-new PhD, at least not in physics. I think mathematics does it here. What it is is a postdoc who has to teach. [Laughter]

LIPPINCOTT: As opposed to just doing research?

SCHWARZ: Right. So anyway, I was an instructor. I didn't mind.

LIPPINCOTT: Did you teach undergraduates?

SCHWARZ: No. With no training, preparation, or whatever, they just told me, "You're teaching such-and-such." So right after I arrived, I was teaching a graduate course in classical mechanics. I put a lot of work into it. I think I did a reasonable job of it. That was my first year there.

LIPPINCOTT: Was Goldberger the head of the physics department at Princeton then?

SCHWARZ: No, he wasn't the chair, but he was sort of the leader of the theory group. The other senior member would have been Sam Treiman, who just died a half year ago.

LIPPINCOTT: What kind of research did you do when you got there?

SCHWARZ: I continued working on the S-matrix and Regge-pole theory. The first couple of years I wrote a number of papers, none of which are terribly important.

LIPPINCOTT: But the strings began to creep in, did they not?

SCHWARZ: OK, let me tell you where they come in. The first paper that led to strings was a paper by a fellow named [Gabriele] Veneziano, who is an Italian at CERN. And that would have been in 1968. I arrived at Princeton in 1966, so it was two years later. What Veneziano did was to try to demonstrate how these S-matrix Regge-pole ideas of Chew et al. work by giving a specific mathematical example—a nice simple formula that captured some of the ideas. And that result came as quite a surprise to people, so there was a lot of playing around with it, trying to understand what it meant. It was a formula that was supposed to describe the scattering in which two particles collide and give two other particles. So very soon thereafter, various people found generalizations of Veneziano's formula to describe reactions in which any number of particles would come out of the collision. But these were just made-up formulas. No one at that point had the idea that this was anything particularly fundamental or important. It was just hoped that these were formulas that would capture some general features of the interactions of the strongly interacting particles called hadrons. It certainly had some qualitatively nice features, so it intrigued people. But then it was discovered, by various people, that in fact what these formulas were were the quantum-mechanical scattering amplitudes for strings. So the idea of the string was introduced to explain these formulas, which had just been made up out of the blue. The history is bizarre, because it would have made much more sense to start with the idea of strings and say, "What are the formulas for scattering strings?" It would have come out to the same formulas. But that's not the way the history went. [Laughter]

So I got involved with this stuff. Not in '68, but in '69 I started working on it. You see, at that point, when they had the idea of these strings, then it became clear that this could be part of a fundamental theory and not just some made-up formulas. But still the idea was that it was a theory to describe the strong nuclear force.

So I got involved in that project. By '69, David Gross had moved to Princeton, so we were back together again, and we were collaborating on this string theory work, computing some of the quantum corrections to these formulas which had been found earlier and discovering certain surprising features of these quantum corrections. We were discovering that there were new kinds of particles that were arising. But in following work of others, it became clear that the formulas were only consistent if spacetime had twenty-six dimensions rather than four dimensions.

LIPPINCOTT: Was twenty-six the number that you and Gross arrived at?

SCHWARZ: No. Actually it was a guy named Claud Lovelace who first came up with that. We had found these strange behaviors in this quantum amplitude, and it was only when this number was chosen to be twenty-six that the strange behavior had a nice interpretation. The nice interpretation is that it was strings with ends, where the ends joined to give closed strings. But that interpretation only worked with this funny dimension, and this funny dimension didn't make any sense at all, because we knew that space has three dimensions and not twenty-five. When I talk of twenty-six, by the way, I'm counting time.

LIPPINCOTT: Yes. One dimension is time and twenty-five are space.

SCHWARZ: Right. There's always one dimension of time when I'm talking about weird dimensions, and the rest are space.

So that was crazy. But this string theory had many other wrong features. The elementary particles form two classes, called bosons and fermions, and this was a theory that only had bosons and didn't have fermions. So that was a serious deficiency. It also had certain particles called tachyons; these are particles that have imaginary mass, which in special relativity would mean that they could travel faster than the speed of light, and that's unphysical, too. So, for all these reasons, it was clear that we needed a better theory to describe strong interactions. So I got involved with a Frenchman who was at Princeton at the time, named André Neveu.

LIPPINCOTT: Where was he visiting from?

SCHWARZ: He was from Paris. At the time, he was at a lab in Orsay, outside of Paris. The entire group at Orsay later moved into Paris, to the École Normale Supérieure, but they didn't make that move until 1975. Later on in the discussion I'll mention the École Normale Supérieure. So anyway, Neveu and another Frenchman, Joël Scherk, were both at Princeton.

LIPPINCOTT: Scherk being from the same place?

SCHWARZ: Same place. These were the two outstanding young talents in France. They had both been dispatched to Princeton to be on the cutting edge. That story's kind of funny: In 1969, I got promoted to assistant professor at Princeton, and these two guys arrived. They had training which was equivalent to an American PhD. But in the French system, what they call a *doctorat* comes much later in your career, so they didn't have a title that Princeton could recognize as the equivalent of a PhD. So they were classified as graduate students and assigned to me as advisees. [Laughter] So one day in the fall of '69, these two guys show up in my office. I had no prior warning who they were or anything. As far as I knew, they were just new graduate students. So I asked them all the questions that I would ask a new graduate student, like "Shouldn't you take a course in quantum mechanics?" [laughter] or "Shouldn't you take a course on electromagnetism?" And they said, "No, no. We don't need that. We've already had it." So I said, "OK. Go away." [Laughter]

LIPPINCOTT: Were they about your age?

SCHWARZ: Well, let's see—about my age. They may have been a couple years younger, because I was three years postdoctoral, and they were probably brand-new PhDs. I had gotten my PhD when I was twenty-four. They were probably a similar age at a similar stage. So anyway, they made this appearance in my office and then they disappeared. And then a couple of months later, they came back to me and said, "We have some results we would like to show you." And this is a time when I had been working with David Gross on these quantum corrections and the string scattering processes. And they had independently been working on the same thing. One of the problems in these formulas was that there were some very bad infinities, and what they had done was to understand the right mathematical structure to describe these infinities and to use some obscure mathematical transformation—dating back to a mathematician named [Carl] Jacobi—to re-express them in a different form, where it was clear how to deal with them. So they came to show me this result and I was just blown away by it. It was clear that they had made a major discovery. So at that point I was convinced that they didn't need a course on quantum mechanics or electromagnetism. [Laughter] And then, after that, the two of them started collaborating with Gross and me. The four of us wrote a few papers together on string theory. But by 1970 Scherk had gone back to France. Neveu stayed on. David Gross got interested in other problems—which made him famous, but that's another story—and I was working with Neveu. So we said, "We need a new and better string theory that incorporates fermions." So we worked on that, and we made some interesting progress. And the paper we wrote actually was a new theory with bosons only—in the version we first had. But independently there was a fellow at Yale named Pierre Ramond who wrote a paper about fermionic strings. His strings weren't interacting, but still it was an interesting generalization of the Dirac equation to strings. So we got ahold of his paper just about the time we were finishing ours, and we were struck by the mathematical similarity of what we had done and what he had been doing. Then we realized that Ramond's fermionic strings could be made to interact with our bosonic strings and it was all part of a consistent theory. Over the years, we've used different names for this theory, and it has evolved into what we today call superstrings. But in those days, we used other names. In our first paper, I think we called it the dual pion model, and later we called it spinning strings. So there's been a whole succession of names. But in any case, this is the predecessor of modern superstrings.

LIPPINCOTT: But supersymmetry wasn't involved at this point, was it?

SCHWARZ: It's funny how that came about. Because the idea of supersymmetry—which is a conjectured symmetry of space and time; it extends our usual ideas of spacetime symmetry in an amazing new way—arose in 1971 independently in a couple of different places. It arose from the work of Ramond, Neveu, and myself. It first appeared in a two-dimensional setting. So I have to explain what two dimensions have to do with anything, because we've already mentioned four dimensions and twenty-six dimensions.

LIPPINCOTT: Yes.

SCHWARZ: The way the number two arises is that these strings themselves are one-dimensional objects. But as they propagate in space—if you think of the history of a string, it sweeps out a two-dimensional surface in space and time. And much of the mathematics of describing strings makes reference to that two-dimensional surface. So supersymmetry was discovered in the string setting.

LIPPINCOTT: Really? I didn't know that.

SCHWARZ: In this two-dimensional context, in 1971. It was in the formulas that Ramond and Neveu and I had, and then it was explained more clearly in a subsequent paper by [J. L.] Gervais and [Bunji] Sakita. Gervais was from Paris and Sakita was, I believe, at City University [of New York] at the time.

LIPPINCOTT: And you reduced the number of dimensions, didn't you, to ten?

SCHWARZ: OK. So around this time, in 1971, was just when we were discovering that twenty-six was needed for the bosonic string. And I think it was in 1972 that we understood better how the twenty-six worked for the bosonic string. And then once it was understood how to explain that clearly, I could immediately work out what the corresponding number was for this theory, and I found that it was ten. So I think I was the first one to find the number ten, although other people published that result also. For example, [Peter] Goddard and [Charles B.] Thorn also found it.

LIPPINCOTT: Where are Goddard and Thorn from?

SCHWARZ: Goddard is in Cambridge, England, and I believe he was even then in Cambridge. Charles Thorn was in Berkeley. Currently Thorn is at the University of Florida [at Gainesville], and so is Pierre Ramond. And Goddard is still in Cambridge, where he is master of Saint John's College.

LIPPINCOTT: So you and Neveu and Ramond published a joint paper about this?

SCHWARZ: No, we never published with Pierre at that time. But we did have a couple of papers with Charles Thorn. Thorn was still a student at Berkeley. He was a student of Stanley Mandelstam. I didn't mention Mandelstam earlier, and I probably should have. He was another very influential theorist who had a big effect on me. I should probably also mention—jumping back to Berkeley—that two other junior professors when I was a graduate student were Steven Weinberg and Shelly [Sheldon L.] Glashow.

LIPPINCOTT: Oh, really!

SCHWARZ: My first year at Berkeley, I took an electromagnetism course from Glashow and a field theory course from Weinberg. These are people who went on to become quite famous; I thought it might be worth mentioning.

LIPPINCOTT: And Glashow was not a big fan of string theory for a long time, was he?

SCHWARZ: No. But in this era he didn't even know that he didn't like it yet. [Laughter]

LIPPINCOTT: How does he feel about it now, by the way? Has he come around?

SCHWARZ: This is kind of jumping to the end of the story, but we can do that.

LIPPINCOTT: Just where he's concerned.

SCHWARZ: Sure. I haven't talked to him about it recently, but I'm told he's much more sympathetic to it.

LIPPINCOTT: Well, he should be. [Laughter] OK. So your work with Neveu made you pretty well known, didn't it? Isn't that what attracted Murray Gell-Mann's attention?

SCHWARZ: Right. We did this work in '71, and in the academic year 1971-1972 Gell-Mann was on a sabbatical at CERN, and there were some people doing string theory at CERN at that time. A couple of the names that come to mind are Lars Brink and David Olive. And certainly these people became very interested in our results. Information didn't propagate instantaneously in those days, like it does now. You know, the preprints had to arrive. You dug them out of some pile and eventually you'd find it. But in any case these people became enthusiastic about this and worked on this theory, and clearly Gell-Mann got wind of it. He never worked on this himself, but he sensed from what they were saying that this was possibly important.

LIPPINCOTT: It didn't bump into his quark stuff, did it? I mean, it wasn't incompatible with Murray's quarks?

SCHWARZ: It wasn't clear how it would relate. In fact, one of the problems in relating string theory to experiment or observation was that we couldn't see how to build the quark idea into it very well. There was roughly the idea that the quark should be stuck at the ends of the strings. That's easy to say, but it was hard to implement the formulas. So that didn't work very well, but that was sort of the vague idea that was floating around.

In any case, I wasn't particularly made aware of it, but I surmised that Gell-Mann had communicated to people who were in Pasadena that I should be invited there, because I was invited to visit for one week in the fall of '71.

LIPPINCOTT: To give a talk?

SCHWARZ: Yes. I guess it was George Zweig who invited me. [Tape ends]

Begin Tape 1, Side 2

SCHWARZ: Zweig was a professor of theoretical physics at Caltech. And I remember him and [assistant professor of theoretical physics] Jeffrey Mandula being especially hospitable to me during my visit. So I was here for one week, and I think I gave three or four lectures. This was the fall of '71. And I came because I was invited to visit—we never talked about jobs. I didn't raise the question, and they didn't raise the question. I remember that I also visited Santa Barbara on that same trip.

LIPPINCOTT: Who was there? Was [James] Hartle there then?

SCHWARZ: You know, I remember my visit to Caltech rather clearly, but the only thing that sticks in my memory about the Santa Barbara visit was the guest house, because it was decorated with these leftovers from the Hearst Castle. [Laughter] That's what I remember clearly about that part of the visit; I don't remember who I talked to there.

LIPPINCOTT: Did they have the Institute for Theoretical Physics then?

SCHWARZ: Oh, no. That came much, much later.

LIPPINCOTT: When you were at Caltech on that visit, did you give a symposium in 201 East Bridge?

SCHWARZ: No, I just gave some theory talks in 469 Lauritsen, to the theory group.

LIPPINCOTT: What did you think of Caltech then?

SCHWARZ: Well, it seemed like a nice place. I'm sure I had a good impression of it. Let me get the dates right. So later in that academic year—early '72—I was informed by Princeton that I would not be receiving tenure.

LIPPINCOTT: Gosh! That's kind of amazing.

SCHWARZ: They did promote two people to tenure at that time. One was David Gross and the other was Curtis Callan, and both of those people have done fantastically well—so even though I would like to say that they made a mistake, I can't say that they made a mistake. [Laughter]

LIPPINCOTT: I think they'd probably view it as a mistake now.

SCHWARZ: [Laughter] In any case, it was Goldberger who gave me the news, and he was later the person who did give me tenure at Caltech. [Laughter] This period, the early seventies, was a very bad time for the physics job market.

LIPPINCOTT: Tell me why.

SCHWARZ: There was a big malaise in the country in connection with the Vietnam War. Nixon didn't like academics [laughter], and the funding was really drying up for research. And the universities had been on this big spending binge, hiring binge, in the late sixties and got overextended. And so, all of a sudden, they kind of hit a wall around 1970, and the jobs just dried up. It was a really hard time to get a job.

LIPPINCOTT: When you were told that you weren't going to get tenure, were you aware that Caltech was interested in you?

SCHWARZ: Not yet. I don't remember the dates precisely, but this would have been early '72 that I was informed about not getting tenure, I guess.

LIPPINCOTT: That must have been a blow.

SCHWARZ: You know, I don't think I had even been thinking about getting tenure at Princeton. I don't remember worrying about it. But I was a little insulted that they bothered to tell me that I wasn't going to get it, because I think I was planning on moving on anyway. [Laughter] I just found it insulting. [Laughter] I would have been happy if they had just said nothing and let me go, but they made a big point of it. In any case, I never applied to Caltech, but a few months later they offered me the position of research associate.

LIPPINCOTT: Do you think that was through Murray Gell-Mann?

SCHWARZ: You know, I hadn't had any communications with Murray. I mean, I knew him from wherever—I'm sure I met him on several occasions—but I hadn't had any communications with him during that year. But there's no doubt that he was in communication with the people at Caltech. He was clearly influential behind the scenes, but nobody told me that.

LIPPINCOTT: How about Zweig? He probably put in a word for you.

SCHWARZ: Yes, I guess that when I visited here, that went well.

LIPPINCOTT: Was it [Robert] Bacher who was division chairman then?

SCHWARZ: When I arrived, the chair [of the Physics, Mathematics, and Astronomy Division] was [Robert] Leighton.

LIPPINCOTT: Leighton—oh, yes. And Harold Brown was the president?

SCHWARZ: That's correct.

LIPPINCOTT: Were you to have teaching duties?

SCHWARZ: No, this was a non-teaching research position. Now, in the Caltech scheme of things—at least in those days—research associate was the highest rank they had that was nonprofessorial research faculty. It was a position that could be continued or renewed indefinitely, subject to the support of some professor and some government funding.

LIPPINCOTT: Did you have to bring funds with you?

SCHWARZ: No. The high-energy physics group here had the same source of support then that we have today, which is a large DOE [Department of Energy] grant. The name of the agency might not have been DOE yet [In 1972 it was still known as the Atomic Energy Commission—ed.], but anyway it's the same thing. So there's a large grant that supports all the particle physics, both the experimental and the theoretical. The theory portion of the grant is actually a small fraction, because experimentalists are much more costly than theorists; they have more expensive habits.

LIPPINCOTT: Yes. Who did you expect to work with when you came? Did you know anything about that?

SCHWARZ: Well, I hadn't been told that I'd be working with or for anybody. I always assumed I would just do what I wanted to do, which is I think what they intended as well. So that's what it was, because there wasn't anyone here who was doing string theory.

LIPPINCOTT: You were it, then.

SCHWARZ: I was it. But I was well supported—not in the sense that I had a high salary but that there were funds available for me to bring visitors and people with whom I wanted to collaborate.

LIPPINCOTT: Did you bring [Joël] Scherk?

SCHWARZ: Certainly on one occasion I brought Scherk. Gell-Mann had gotten a gift—I think from the Fleischman Foundation—of some sum of money, which lasted for a number of years and was pretty much at my disposal for visitors, and I used that for many visitors for a number of years. Neveu visited for a year at one point, but his interests had kind of drifted apart from mine, and we didn't collaborate at that point. There were some postdocs hired at different times whom I worked with. I also had some graduate students to work with. But anyway, Scherk came as a visitor around January of '74. I think he came for about half a year.

LIPPINCOTT: And he came specifically to work with you on this string theory?

SCHWARZ: Well, he was invited by Caltech, but clearly we knew each other and we had common interests. It was very natural that we would work together.

LIPPINCOTT: Let's get off the work for just a minute and talk about more mundane things, like how you liked Pasadena and how you found Caltech when you came. Did you think it was a friendly place? Was there a lot of interaction with other members of the department?

SCHWARZ: In the 1970s, both Gell-Mann and [Richard] Feynman were very active and very enthusiastic about physics. Feynman was developing these parton ideas, which he was all excited about, and Gell-Mann was working with Harald Fritzsch and others on developing ideas that turned into QCD [quantum chromodynamics]—the *correct* theory of the strong interactions. [Laughter]

LIPPINCOTT: Did you talk a lot to Murray or to Richard Feynman?

SCHWARZ: I certainly interacted with these people on a day-to-day basis and went to many of their lectures and socialized, and so on and so forth. I had more interaction with Gell-Mann. I think professionally we had more interests in common.

LIPPINCOTT: Where did you live when you were here? You weren't married at that time?

SCHWARZ: No, I didn't get married until quite late, so I just had a rented apartment for the first few years. In '75, I bought a condominium on Catalina, right across from where the graduate dorms are now.

LIPPINCOTT: So you were sort of wedded to Caltech. You weren't part of the Pasadena scene.

SCHWARZ: Yes. I was pretty happy with that position, at least for the first few years. Because, as I say, it was a difficult time to get a good job. I was at a first-rate place, with a very intellectually exciting atmosphere. And I had a pretty high position.

LIPPINCOTT: But string theory itself was not a big deal in physics at that time. You say that you were "it" here. There weren't that many people anywhere doing it, were there?

SCHWARZ: Well, from the period of about 1968, when string theory started, it kind of increased in activity up until about '71, and then it started decreasing. At the peak, around '71 or '72, there were probably a couple of hundred people worldwide working on it, so it was a fairly active area of research. And by '73 or '74, almost everyone who had been working on it had dropped it.

LIPPINCOTT: Why?

SCHWARZ: There were several reasons, which made a lot of sense. There were these things that weren't quite right about string theory. It required too many dimensions. It had these unphysical tachyon particles. It didn't include the quarks in the right way. So even though it had qualitative features of strong interactions, in detail it just didn't work quite right. So that was on the one hand. On the other hand, another theory came along—quantum chromodynamics, QCD, around 1972 or 1973—which was immediately recognized to be the right theory. There were a lot of compelling arguments.

LIPPINCOTT: The right theory for the strong interactions.

SCHWARZ: For the strong nuclear forces. And they still feel that way today—that was the right answer. So those are powerful reasons to stop working on string theory.

LIPPINCOTT: But you didn't.

SCHWARZ: Now, when Scherk came to Caltech in 1974, he had just written a review article about string theory—and I had written one a couple of years earlier. And we were both still interested in it. I think we were kind of struck by the mathematical beauty; we found the thing a very compelling structure. I don't know that we said it explicitly, but we must have both felt that it had to be good for something, since it was just such a beautiful, tight structure. So, one of the problems that we had had with the string theory was that in the spectrum of particles that it gave, there was one that had no mass and two units of spin. And this was just one of the things that was wrong for describing strong nuclear forces, because there isn't a particle like that. However, these are exactly the properties one should expect for the quantum of gravity.

LIPPINCOTT: Yes. That's nice, isn't it?

SCHWARZ: [Laughter] So we had been aware of this for many years, but we hadn't pursued it, because we weren't thinking about gravity.

LIPPINCOTT: And you weren't thinking about the problem of reconciling quantum theory with general relativity.

SCHWARZ: No. We knew that that was an issue, but it wasn’t our problem; we were trying to understand the strong interactions. And in those days physics was much more compartmentalized than it is now. The first thing that people who were brought up in particle physics were taught was that you can forget about gravity, because if you just look at the force between two protons, or even between an electron and a proton, the gravitational force compared to, say, the electric force, is smaller by ten followed by thirty-eight zeros or something. It was just fantastically negligible. So we were taught to forget about gravity. It had nothing to do with our problem. Particle physicists wouldn’t talk about gravity. I mean, if anyone tried to, they’d be viewed as a crackpot. It wasn’t part of the problem. Of course, there were other communities that worked on general relativity, and that was perfectly respectable—trying to understand the properties of black holes and so on. But they were mostly doing classical physics, not quantum. Kip Thorne would have been the outstanding example of that at Caltech, John Wheeler at Princeton, and so forth.

LIPPINCOTT: They weren’t concerned with small things.

SCHWARZ: No. But these communities didn’t talk to one another. So the idea that particle

physicists could say anything useful about relativity was a very bizarre notion.

LIPPINCOTT: Well, who was it that was bothered by the incompatibility between quantum mechanics and general relativity then? What branch of physics would have seen that as a problem?

SCHWARZ: OK. There were some people—I think ’t Hooft and Veltman might be examples.

LIPPINCOTT: Who?

SCHWARZ: [Gerard] ’t Hooft and [Martinus J. G.] Veltman, who shared the Nobel Prize this past year. They had looked at what happens if you take general relativity and try to study it as a quantum theory. They demonstrated that there were infinities—or they gave strong evidence that there were infinities—so that it wouldn’t make a consistent quantum field theory. Even without their detailed calculations, it was pretty clear from just general considerations that that was likely to be the fact. Just from what people called power-counting arguments, one could make the case that it wasn't going to give a consistent quantum theory. And that is in fact the current viewpoint.

LIPPINCOTT: But it wasn't anything that you were worried about.

SCHWARZ: We were aware of this, but, again, it wasn't a problem that we were particularly concerned about. However, when Scherk was here in '74, at some point in our deliberations we said, "Just for the fun of it, let's see whether this massless spin-2 particle behaves in the right way to give the standard gravitational force of the Einstein theory of general relativity." And having posed the question, it wasn't actually very hard to answer by invoking some appropriate theorems and making the case that indeed that was right.

LIPPINCOTT: Wow!

SCHWARZ: And the reason we found this exciting was that we knew that string theory was going to give a consistent quantum theory.

LIPPINCOTT: Yes.

SCHWARZ: And it became clear to both of us, immediately, that this was the way to make a consistent quantum theory for gravity. So we figured that we'd just tell the world and they'd all get excited and start working on it. We even wrote up a little article which we submitted to the Gravity Research Foundation's [1975] essay competition. [J. Scherk & J. H. Schwarz, "Dual Model Approach to a Renormalizable Theory of Gravitation." Reprinted in *Superstrings*, vol. I, ed. J. Schwarz (World Scientific, 1985)] And I spoke about it at a conference in Tel Aviv.

LIPPINCOTT: You published some papers along those lines, right?

SCHWARZ: Yes. And we published our work, of course.

LIPPINCOTT: In ’74 and ’75.

SCHWARZ: Right. We published papers. And to my astonishment—I don’t know if Scherk was astonished or not—but to my astonishment, nobody was interested. Nobody took it seriously—not even the relativists or the people who had been working on string theory before. Nobody! So we were puzzled by that, but I was completely convinced that this was the way to go. I had to learn some relativity, because I didn’t know all that much. Now, I should point out that independently of us there was a fellow in Japan named [Tamiaki] Yoneya who wrote a paper—which appeared, I believe, before ours—pointing out that general relativity could be attained in this way. [T. Yoneya, “Connection of Dual Models to Electrodynamics and Gravidynamics,” *Prog. Theor. Phys.* 51, 6 (1974) 1907-1920]

LIPPINCOTT: You’d think that for such a fundamental problem as trying to reconcile these two very good theories, each of which worked in its own domain, people would have kissed your feet or something.

SCHWARZ: [Laughter]

LIPPINCOTT: You felt it, obviously, when you had this idea about the graviton. It must have been like a light going on.

SCHWARZ: Right.

LIPPINCOTT: So it is amazing that—

SCHWARZ: Yes. It took, basically, ten years to convince the rest of the world. [Laughter]

LIPPINCOTT: [Laughter] They weren’t waiting for the answer.

SCHWARZ: There were a few people who caught on earlier than ten years, but almost all took ten years. So, lets see, what did I want to say...?

LIPPINCOTT: Well, one thing you did.... I have a note here that you reduced the size of the typical string way, way, way down to the—

SCHWARZ: That's right. We said, "Let's see if we can take this idea seriously." So in our original paper on this subject [J. Scherk & J. H. Schwarz, "Dual Models for Non-Hadrons" *Nucl. Phys.* B 81, 1 (1974) 118-144], we didn't just make the point that you can get gravity by taking a suitable limit of string theory, but we said that string theory should be used to unify gravity with the other forces as a consistent quantum unified theory. That was our idea. And one advantage of this is that in a theory of gravity, having extra dimensions is not bad. You see, before it had been a problem. When we were just doing strong interactions, it didn't make sense. But in gravity, the geometry of space and time is determined by the equations of the theory. So it became a possibility that the equations of the theory would require that six of the dimensions, for some reason, would curl up into some invisible little ball or something, and then it could be perfectly consistent with observation. It wouldn't make sense to give that kind of a story if you were just doing strong interactions, but in a theory of gravity, that kind of story made sense. We certainly understood that.

The theory has one free parameter, which is the characteristic size of the string. So if you want to use this theory for gravity, then the first thing you have to ask is the strength of the gravity; if it's right you'd get Newton's constant. And so that determines the size of the string. When we were trying to describe the strong interactions, that size had to be more or less the size of a proton, which is 10^{-13} centimeters. But now, if we want to describe gravity, the size had to be more or less what's called the Planck length, which is 10^{-32} centimeters—some nineteen orders of magnitude shorter.

So this was a major change in the size of our strings [laughter], but it didn't change the mathematics. We still do more or less the same math as before—it's just a very different interpretation.

LIPPINCOTT: OK. So did this make a stir at Caltech? Never mind the rest of the world. I mean, how did your colleagues here react to this new idea?

SCHWARZ: I don’t remember much reaction here either. I want to be a little careful about what I say, because Murray [Gell-Mann] was always very supportive of me and certainly at some point he became very enthusiastic about this program. Now, whether it was in ’74 or ’78 that he became enthusiastic, my memory’s a little vague. But somewhere in that period he certainly recognized—well, I can be a little more specific. For example, if you look at that picture on the wall, that was a conference I attended in January of ’78, where I spoke about these ideas.

LIPPINCOTT: Where was it?

SCHWARZ: San Francisco. And I know that it was Gell-Mann who arranged for me to be invited to speak at that conference. So certainly before that, he would have felt that this was something that deserved to be communicated. Now, in that picture are most of the famous theoretical physicists of that era—’t Hooft, [Stephen] Hawking, [C. N.] Yang—and I don’t believe that any of them remember what I spoke about at that meeting. [Laughter]

LIPPINCOTT: Was Geoffrey Chew there?

SCHWARZ: No, he wasn’t. Somehow, during the period of the seventies, Chew’s influence waned, and he became more and more marginalized, and I think many people felt that he sort of went off the deep end with some of his ideas. If you look at the young physicists today, I suspect that many of them haven’t even heard of him, which is hard to imagine for someone who had been the intellectual leader of the community just in the 1960s.

LIPPINCOTT: Well, he’s in the histories. I had heard of him. You know, he’s always mentioned.

SCHWARZ: Yes. But if you were to poll our postdocs, I would suspect that many of them haven’t.

LIPPINCOTT: Really! But the S-matrix kind of tanked, didn’t it?

SCHWARZ: This work had a lasting imprint on the subject. Nonetheless, being in the limelight can be a very short-lived, ephemeral thing. [Laughter]

LIPPINCOTT: So, you were still a research associate here.

SCHWARZ: That's right.

LIPPINCOTT: Then you went to the École Normale Supérieure in September 1978 to August 1979, on a Guggenheim. Is that right?

SCHWARZ: Yes. I was invited by those people. This group, which included Neveu and Scherk, had moved to the École Normale in '75.

LIPPINCOTT: And they invited you to come?

SCHWARZ: They invited me to spend a sabbatical year, or whatever you call it—the year—there. And I got a Guggenheim to help support me during that year.

LIPPINCOTT: But you were still more or less in the wilderness. Is that right? I mean, string theory hadn't been embraced yet.

SCHWARZ: That's certainly true. I went there to work with Scherk. And Neveu was there, too, but he was working on somewhat different things at that time. Now, Scherk had had some health problems. I don't think it's right to go into detail, because he had some psychiatric problems and he wasn't able to function as well as he had previously, but he was able to work about half-time. So during that year we collaborated on some work which I think is reasonably interesting—it's still cited nowadays. We produced a few papers during that year on a particular mechanism for breaking supersymmetry. [e.g., J. Scherk & J. H. Schwarz, "Spontaneous Breaking of Supersymmetry Through Dimensional Reduction," *Phys. Lett.* B82 (1), 60-64 (1979)]

There was a development in the interim that probably should be mentioned and had to do with the understanding of supersymmetry. I mentioned earlier how supersymmetry had arisen in two dimensions in string theory. And then in 1973, a couple of years later, [Julius] Wess and [Bruno] Zumino had decided that it would be interesting to see whether you could have supersymmetry in the physical four dimensions. And their idea was to just kind of emulate what was happening in two dimensions in string theory and see if they could do the same thing in four dimensions. And they found that they could. So they invented various four-dimensional quantum field theories.

LIPPINCOTT: Again, that's one time dimension and three spatial?

SCHWARZ: Yes. So they found various quantum field theories with supersymmetry. And that became a very active subject. And a couple of years later, several people—some important names being [Daniel Z.] Freedman, [Sergio] Ferrara, [Peter] van Nieuwenhuizen, [Stanley] Deser, Zumino—showed how to include gravity in supersymmetric theories, and they developed what was called supergravity.

LIPPINCOTT: Supergravity?

SCHWARZ: Which is just taking general relativity and making it supersymmetric. So what that meant was that the spin-2 graviton had to have a supersymmetry partner, which was a spin-3/2 gravitino. And this gravitino was interpreted as a very special field in supersymmetry, which allowed you to interpret supersymmetry as a local gauge symmetry. That was developed in the mid-seventies, and Scherk and I were both very interested in it and contributed to it to some extent.

I collaborated with Scherk on a paper in 1976, when he was in Paris and I was here. I had another visitor here—Lars Brink, from Sweden, whom I've worked with often over the years—and we were developing some supersymmetric Yang-Mills theories in various dimensions. And we communicated with Scherk and included him on the paper about that, which I think is an important paper. [L. Brink, J. H. Schwarz & J. Scherk, "Supersymmetric Yang-Mills Theories," *Nucl. Phys.* B121 (1) 77-92 (1977)] Because an important subject is what's called ten-dimensional super-Yang-Mills, and it's closely related to something called N=4 super-Yang-Mills—meaning there are four supersymmetries. So we had developed those theories at that time, and they're both important nowadays. And at that time, in '76-'77, Scherk did some important work with people named [Ferdinando] Gliozzi and [David I.] Olive.

LIPPINCOTT: Are those Americans?

SCHWARZ: No. Let's see. Gliozzi would have been Italian and Olive is from England. This is work I was not involved in, but wish I had been. [Laughter] What they did was to show that our superstring theory not only had this two-dimensional supersymmetry that had been identified in 1971 but that if you interpreted it correctly, it actually had ten-dimensional supersymmetry. So that was—to me, at least—a very exciting discovery. It meant that this theory contained all the supergravity and all the supersymmetric theories that these other people had been talking about. And I always felt that the supergravity theories didn't really make much sense by themselves, because they weren't consistent theories. Only inside string theory, where the quantum mechanics was under control, did this really make sense.

LIPPINCOTT: By this time, had you pretty much eliminated in your own mind the idea of a singularity? In other words, was 10^{-32} centimeters the teeny-weeniest you could get? You know...eventually we'll talk about the implications for cosmology.

SCHWARZ: Well, I think you're referring to the fact that in general relativity you can get spacetime singularities.

LIPPINCOTT: Yes.

SCHWARZ: And we know from the work of Hawking and [Roger] Penrose that generically these things arise.

LIPPINCOTT: Yes.

SCHWARZ: And they give a real problem of making a consistent interpretation of general relativity. I think it's fair to say that in those days we certainly didn't have a good enough grasp of string theory to say how these singularities would be handled in string theory. We were studying string theory—really, until the mid-nineties—in an approximation scheme, where there was a small coupling constant, in studying the behavior of the theory of weak coupling. And to address problems such as singularities, you have to be able to go beyond the weak coupling description of the theory to make any progress. That's not something we were in a position to do until the mid-nineties.

LIPPINCOTT: OK. So we have you in Paris. And then you go to CERN in August and you meet Michael [B.] Green—or you knew Michael Green?

SCHWARZ: I knew Michael. That's right. So following my year in Paris, I took one month at CERN. Michael Green had been at the Institute for Advanced Study for a year during one of my Princeton years, and we knew each other casually. We had never worked together, but we certainly were acquainted with one another. And he had done some work in the early days on string theory, although he had then gone on into lattice gauge theory, which is a rather popular subject.

LIPPINCOTT: And he was at the University of London but also visiting CERN at the same time?

SCHWARZ: Yes, I guess he was just a summer visitor like I was. That's right—he was at Queen Mary College at the University of London. So we got to chatting in the CERN cafeteria, and I got to talking all excited about this earlier work of Gliozzi, Scherk, and Olive and how I thought this needed to be understood better and that they had given this argument for spacetime supersymmetry.

LIPPINCOTT: Was he familiar with Gliozzi et al. and with the work you were talking about? Or were you telling him about it?

SCHWARZ: My memory isn't clear, but I suspect he was familiar with it—I'm pretty sure he was. In any case, I made the point that the way they had described it, it was some kind of mathematical miracle, and that there ought to be a better way to understand it. So we started working on that together at CERN. We didn't make any major progress that month, but we agreed to continue discussing it. And so over the subsequent years, he visited Caltech for several extended stays. I don't remember exactly how many months in which years, but cumulatively probably more than twelve months over the next four years. Also, I spent one three-month period in London in the fall of '83 working with him. And we also crossed paths for several months in Aspen, Colorado, at the Aspen Center for Physics.

LIPPINCOTT: Yes. I wanted to ask you about that. When did you start going to Aspen?

SCHWARZ: My first summer in Aspen was in 1969.

LIPPINCOTT: Really? That early?

SCHWARZ: Yes. I went there from Princeton. This is a great place to go to do research in the summertime; it's a very supportive, unstructured environment. It was founded around '62 or '63 by three physicists, and it started as an offshoot of the Institute for Humanistic Studies, but then early on it became independent.

LIPPINCOTT: Yes [laughter], and very far afield from. So can we digress and talk about that? Do you have to be invited to go there? Or could you apply to go there? How did that work?

SCHWARZ: You had to apply. In '69 it was pretty easy to apply. Then you were accepted and you went.

LIPPINCOTT: Yes. You were an assistant professor at Princeton then.

SCHWARZ: Yes. Nowadays they get probably twice as many applications as they have slots for, so there's a rather elaborate admissions process to select the participants nowadays.

LIPPINCOTT: You're involved in the administration of Aspen, aren't you?

SCHWARZ: I'm currently a trustee and a general member. At some time in the past, I was the treasurer for three years, so I've had a long involvement with it. In the summer of '71, I went to France to work with Neveu; I spent that summer with Neveu in Orsay, and that was the only summer I didn't go to Aspen after 1969. [Laughter] So except for '71, I've been to Aspen every summer since '69.

LIPPINCOTT: And you got Green to come there in '80—that summer after you met him at CERN?

SCHWARZ: Probably. I don't think he was there every summer, but most summers in that '80-'84 period. And we worked together very hard there. In our collaborations, after a year or so, we solved the initial problem that we had set up, which was to find a better way of explaining how supersymmetry is realized in the string theory. That led to a number of developments. It led to a clearer description of exactly what theories there are, which we called Type I and Type IIA and Type IIB and so on. This had all been very murky before that. And we developed a new formalism for describing these theories. So nowadays, when people are studying these theories, sometimes they use what they call the RNS formalism, which refers to Ramond, Neveu, and me. And then sometimes they use what they call the GS formalism, which refers to the stuff I did with Mike Green, later. These are just different ways. Depending on what problem you're working on, one or the other is sometimes more convenient.

LIPPINCOTT: Well, I read in Brian Greene's book [*The Elegant Universe*]—which I think is an excellent book, by the way, and you said you thought it was the best book on string theory—

SCHWARZ: The best popular book.

LIPPINCOTT: Yes, the best popular book. He said that what you and Michael Green succeeded in doing that was important was to eliminate these infinities, or anomalies, from the theory.

SCHWARZ: Yes. I haven't quite got there yet.

LIPPINCOTT: Oh, OK. I don't mean to push you ahead.

SCHWARZ: That's OK. The point I wanted to make first was that in this period, 1980 through 1984, Michael and I published quite a few papers, and in each case I was quite excited about the results, and I think he probably was, too. And in each case, we felt that people would now get interested, because they could see how exciting the subject was. But there was still just no reaction, and the culture had already changed quite a bit from what I described in the early days: I described a culture in which relativity and particle physics did not communicate. That had already changed, independent of our work. I mentioned the development of supergravity; that was a case where people with a particle physics background had been building general relativity into the theory. So by now there was an emerging community which was sort of bridging relativity and particle physics. But they were very much committed to working in the framework of what they called supergravity and Kaluza-Klein theory. And so the bottom line is that this was quantum field theory and not string theory. So on that basis I felt that it was misconceived, because it wasn't going to be consistent with quantum mechanics. And I interacted with these people, because I had more in common with them than anyone else; we spoke at the same meetings and so on. They were all aware of what Michael and I were doing, but none of them got particularly interested in it. To be fair, I should say that there were a few people besides Gell-Mann who were aware of what we were doing and were supportive.

LIPPINCOTT: Who were they?

SCHWARZ: Let's talk about Edward Witten for a bit.

LIPPINCOTT: Oh, yes.

SCHWARZ: He got the point. In the early days he wasn't a factor, because he was younger. I don't know exactly what year he got his PhD, but I would guess it was around '77 or '78. [Witten got his doctorate from Princeton in 1976—ed.] Once he got his PhD he was a major figure, but prior to that he wasn't. So after the mid-seventies he's clearly one of the leaders of theoretical physics and extremely influential. And certainly by '82 or '83, he was very interested in what we were doing. I wrote a review article in 1983 on this stuff, which he studied very carefully [J. H. Schwarz, "Superstring Theory," *Phys. Reports—Review Section of Phys. Lett.*, 89 (3): 224-322 (1982)]. He even wrote a little paper of his own—not one of his most important papers, but anyway a nice paper on the subject.

LIPPINCOTT: Did you communicate with each other?

SCHWARZ: Well, Michael and I knew Edward, and we'd talk from time to time. And I visited Princeton from time to time, because my sister was there and her children and so on. So I visited Princeton pretty often, and he was there, and I talked a lot with him.

LIPPINCOTT: Was he working on the same thing that you were working on with Michael Green?

SCHWARZ: Well, he wrote important papers on supersymmetry. He wrote on a wide variety of things. I mean, Edward, as you know, is just fantastic. In any case, in our discussions with him it became very clear that one of the really important problems that we had to think about was something called anomalies. We had been aware of this ourselves to some extent, but I think in our discussions with Edward it became even clearer to us how important this was.

LIPPINCOTT: Can you give a definition for a layman of what an anomaly might be in this?

SCHWARZ: These theories have the same kinds of Yang-Mills gauge symmetries that you find in the Standard Model of elementary particles, which is one of the attractive features. However, there was a danger that the quantum corrections would destroy those symmetries. And if they did, it would lead to a mathematical inconsistency of the entire theory. This kind of inconsistency would only arise in a situation where the theory was not left-right symmetrical—not mirror-symmetrical—and there were certain string theories that had that feature and others that didn't. So for example, the Type IIA theory was mirror-symmetrical, so certainly it wasn't going to have these anomalies. But the Type IIB theories and the Type I theories were not mirror-symmetrical, and it appeared as if they would have these anomalies and therefore be inconsistent. So you might say, "Well, why not just live with the IIA?"

LIPPINCOTT: Yes.

SCHWARZ: But the thing is, we knew that *nature* is not mirror-symmetrical. So having an asymmetrical theory was a *good* thing, if we could make sense of the theory.

LIPPINCOTT: Oh, OK.

SCHWARZ: So the question was whether there would be any circumstances under which these anomalies would not occur and the theory would therefore be consistent even though it was not left-right symmetrical. That was a problem we worked on for a couple of years, off and on, until we found the right way of doing it.

LIPPINCOTT: "We" meaning you and Michael Green?

SCHWARZ: Yes. In 1984, Witten published a paper with a fellow named [Luis] Alvarez-Gaumé, a Spaniard, entitled "Gravitational Anomalies." [*Nucl. Phys.* B234 (2) 269-330 (1984)] This was a very important paper, because it showed that these anomalies were not only properties of ordinary Yang-Mills gauge theories but that they also occur in special ways in theories of gravity. So the anomaly problem was, in a sense, even worse, because besides the anomalies we knew about—the Yang-Mills anomalies—there could also be gravitational anomalies. And in their paper, they did succeed in showing that in the IIB theory, the anomaly canceled—there were different contributions to it and it actually canceled.

LIPPINCOTT: So this was one string theory, among several, that seemed to be all right?

SCHWARZ: Yes, right. And the IIA theory we had already known was OK. They showed that the IIB theory was OK. But we still had the problem with the Type I theories. And for the Type I theory, at the level of understanding we had in those days, there were many possible versions, with different symmetry groups. So, for example, you could have an orthogonal group describing rotations in any number of dimensions, and it didn't matter. These were not physical rotations, but just some abstract ones.

So anyway, we resumed studying that anomaly problem in the summer of '84 in Aspen. And we started discussing it with two physicists from Chicago who were also in Aspen, named Dan Friedan and Steve Shenker. Michael and I had been using the GS formalism to study this problem, and Friedan and Shenker suggested to us that maybe using the RNS formalism, the older formalism, might be better. [Laughter]

LIPPINCOTT: *They* suggested that to *you*. [Laughter]

SCHWARZ: Yes, for this particular problem, so we started talking with them about it. But very soon after we started talking, those two left Aspen and went somewhere else. And Michael and I were there on our own and continued pursuing it. [Tape ends]

Begin Tape 2, Side 1

LIPPINCOTT: Let's talk about your announcement with Green in 1984 that you had figured out a way to eliminate these anomalies. Was it at Aspen that you announced it?

SCHWARZ: Right. We were working in Aspen in the summer of '84. We had to compute a particular amplitude, which we did. So we found the anomaly associated with a certain Feynman diagram of string theory—the so-called hexagon diagram, because it had six lines coming out of it. We got a certain answer. And then we realized that there was a second diagram that would also contribute to the anomaly, which is one that's kind of twisted, like a Möbius strip. And it gave another formula for the anomaly of the same structure as the first one. I remember quite clearly that we were going to a seminar at that time, and before the seminar I mentioned to Michael that maybe these two different contributions to the anomaly might cancel for a particular choice of the symmetry group. And at the end of the seminar—I don't remember what the seminar topic was or who gave it—Michael said to me, "SO(32)." [Laughter]

LIPPINCOTT: That was the gauge group?

SCHWARZ: That was the gauge group. So, remember, we had this Type I theory that we had defined for an infinite class of symmetry groups. So now the conclusion was that out of this infinite class, only one of them would be consistent—all the others would be inconsistent, because it's only for this one that the anomaly cancels. And that is the correct result. But we just had one piece of evidence for it at that time, so we weren't yet totally convinced that it was not some accident, something which was not meaningful. So we had to do more tests and studies to really nail it down. What we did was to analyze the low-energy approximation to the theory, and in that low-energy approximation we could also study the gravitational anomalies, because these hexagons and Möbius strips hadn't analyzed the gravitational part, just the Yang-Mills part.

So with this low-energy approximation, we could also analyze gravitational anomalies using the formulas from this paper of Witten and Alvarez-Gaumé. We just copied the formulas out of their paper and plugged in our numbers, and we saw that it worked beautifully. So we were very excited and started talking to people about it. But before we had a chance to make any formal presentation of it, the Physics Center had what they called a physics cabaret.

LIPPINCOTT: [Laughter] Really?

SCHWARZ: [Laughter] This is something that happens very infrequently. There had been one in the mid-seventies, in which Gell-Mann was involved.

LIPPINCOTT: He was an actor in it?

SCHWARZ: Yes. It was physicists acting and having fun for the benefit of other physicists; there are only physicists involved—both in the performance and the watching—and their families. In that mid-seventies skit, at some point Murray jumped out of the audience, ran up on the middle of the stage, and said, "I figured out the theory of everything," and he starts going on and on and getting louder and louder.

LIPPINCOTT: Was that the first time that phrase had been used?

SCHWARZ: I don't know that he used exactly those words, but something to that effect.

LIPPINCOTT: Oh, I see.

SCHWARZ: And then two guys dressed in white coats came up, grabbed him, and carried him off the stage. [Laughter] Well, there hadn't been such a cabaret for ten years, but now in 1984 they were going to have a second one, and the idea arose to have the same skit again. But Gell-Mann wasn't there. So I was asked whether I would play this role. [Laughter] I said, "OK."

LIPPINCOTT: Did they ask you because you had just had this breakthrough?

SCHWARZ: No. Nobody knew about it.

LIPPINCOTT: Oh, they didn't know about it. Oh, wow!

SCHWARZ: In fact, I'm not even sure whether the breakthrough was before or after I had agreed to do it; I don't remember that detail. In any case, when my time came at this cabaret, I ran up on the stage and said, "I figured out how to do everything. Based on string theory with a gauge group SO(32), the anomalies cancel! It's all consistent! It's a finite quantum theory of gravity! It explains all the forces!" And then the guys in the white coats came and carried me off.

LIPPINCOTT: And the audience was just goggle-eyed?

SCHWARZ: Everyone just assumed it was a spoof [laughter], just like it was ten years earlier. But the funny thing is, that was actually our first announcement of our results. [Laughter]

LIPPINCOTT: That's great!

SCHWARZ: This anecdote was written up in the *Aspen Times* a few years later.

LIPPINCOTT: I've never seen it. That's terrific! So did you have a more sober presentation in Aspen after that?

SCHWARZ: Well, right after Aspen, at the end of August, we both came back to Caltech and our first item of business was to write up our result.

LIPPINCOTT: Michael Green was visiting Caltech? Oh, yes, he was a visiting associate. So you wrote it up at Caltech.

SCHWARZ: Right. And before we even finished writing it up, we got a phone call from Ed Witten saying that he had heard from people who had been in Aspen—he hadn't been in Aspen himself—that we had a result on canceling anomalies. And he asked if we could show him our work. So we had a draft of our manuscript at that point, and we sent it to him by Fed Ex. There wasn't E-mail then; it didn't exist, but Fed Ex did exist. So we sent it to him, and he had it the next day. And we were told that the following day everyone in Princeton University and at the Institute for Advanced Study, all the theoretical physicists, and there were a large number of them, were working on this. [Laughter]

LIPPINCOTT: My gosh! So there was no sense in your mind of any competition between yourself and Witten and between Caltech and the Institute for Advanced Study? You wanted it out to all these people?

SCHWARZ: Well, we gave Witten his advance version of our manuscript [laughter], and—

LIPPINCOTT: But this is before you published it.

SCHWARZ: Yes. It was before the preprint was circulating. I don't think we even had a final version yet, at that point. [Laughter]

LIPPINCOTT: Well, it was good of you, a lot of people would think.

SCHWARZ: So overnight it became a major industry [laughter], at least in Princeton—and very soon in the rest of the world. It was kind of strange, because for so many years we were publishing our results and nobody cared. Then all of a sudden everyone was extremely interested. It went from one extreme to the other: the extreme of nobody taking it seriously, to the other extreme—which I thought was just as misconceived—of many people thinking that we were very close to describing all the experimental data. And I knew it wasn't that easy [laughter], so I wasn't that optimistic, even though I thought we were on the right track. I think most people have a more sober appraisal today than they did then, after fifteen-plus years of additional struggle and still not being there. People recognize that it's not such an easy problem.

LIPPINCOTT: Yes. You hear that there are high points and there are dips. There was a dip in the nineties—but we might be going too fast. So you discovered a few more symmetry groups that didn't have these anomalies, besides the two that—

SCHWARZ: Right. The initial version of our preprint, which we had sent to Witten, mentioned only SO(32), but one of the crucial facts about SO(32) that made it work is that it's a group whose dimension is 496. That's crucial for getting the gravitational anomaly to cancel. And I remember one night in Aspen, Michael and I were walking to a movie and Michael remarked to me that another group whose dimension is also 496 is E8 x E8. E8 has dimension 248. So if you have two of those, that's 496. So we thought that was amusing, but we looked at it very quickly and decided it didn't work. We had been too quick.

So shortly after we wrote our paper, we decided to look more carefully at E8 x E8, and then we realized that it does work, also. It can't be realized in the Type I superstring; that was one reason we didn't focus on it too much at the beginning. So in the revised version of our manuscript—it was just a revision, because we had sent a preliminary one to Witten [laughter], so we made a revision off of that. In it, we included the E8 x E8 as a second possibility, but pointing out that it didn't work in the Type I string theory and so we didn't know exactly how it would be used. So our paper in the published form had the E8 x E8 as a second group for which the anomalies could cancel, although we didn't know any string theory that had that as a symmetry group. So a couple of months later, a group of four people in Princeton—

LIPPINCOTT: The string quartet, they call them?

SCHWARZ: Right. Actually, I introduced that term.

LIPPINCOTT: Oh, you did?

SCHWARZ: Yes. That was the same old David Gross, and Jeff Harvey and Emil Martinec and Ryan Rohm. The four of them found another kind of string theory, which they called heterotic string, in which the E8 x E8 group could be implemented.

There were two versions of the heterotic strings. One was the E8 x E8, and the other was SO(32), the same group we had had with the Type I. So there was a second SO(32) theory, which was kind of puzzling. Why should there be two of them? But in any case that's what arose. But the heterotic string was kind of funny, because it combined properties of our superstrings with the old theory of bosonic strings. So some features would look like they were twenty-six-dimensional and some features would look like they were ten-dimensional. But they managed to patch them together in a consistent way, the mismatch between those two dimensions somehow getting fed into the symmetry group.

LIPPINCOTT: In the meantime, you were made full professor. You sprang from senior research associate to full professor.

SCHWARZ: Right.

LIPPINCOTT: Probably on the strength of this breakthrough in the middle eighties, right?

SCHWARZ: I can say a little about that, if you'd like.

LIPPINCOTT: Yes.

SCHWARZ: OK. When I came here I was a research associate. And then—somewhere around 1980, I don't remember the exact date—I was promoted to senior research associate. But that was a phony promotion, because the titles were just redefined at that time, so it didn't really mean anything. And as I said, in the mid-seventies I was very happy with the position, because people weren't clamoring for me in a senior position, and I was at a good institution and well taken care of and free to do what I wanted. But gradually, as my work became more important and my ego got larger or something, I felt that I deserved a professorship. Because even though my position had some advantages in terms of freedom and so on, I did start to get the feeling that I was sort of a second-class citizen, and that annoyed me.

LIPPINCOTT: Yes. I don't blame you.

SCHWARZ: Because there was a period there in the early eighties when I was advising more graduate students than all the other professors in theoretical particle physics combined. And I think there was one year when I wrote more research papers than all of them combined, and one year where I gave more seminars than all of them. [Laughter] But anyway, so I was getting a little annoyed by that. There were five of them and one of me. Also, at the beginning of '84 I was offered a professorship at the University of Chicago, and at that point it had been decided that later that year Caltech would make a decision.

LIPPINCOTT: Who was the chairman of the division then?

SCHWARZ: [Edward C.] Stone.

LIPPINCOTT: So did you let him know that you had this offer from Chicago, or did they just find out about it?

SCHWARZ: Well, I communicated with Murray [Gell-Mann], basically. I don't remember talking with Stone about these things. But Murray told me that my case would be considered in the fall.

LIPPINCOTT: In the fall of '84?

SCHWARZ: Right. And it was in the spring of '84 that I got the offer from Chicago. So I put off Chicago; I decided to give Caltech a chance.

LIPPINCOTT: Then Chicago offered you this before your breakthrough at Aspen, even.

SCHWARZ: Right, and Caltech was planning to consider the case before that, too—although they didn't actually get around to it until afterward. So I guess that helped to make it an easier case for them. They were able to get some good letters from Witten and others. [Laughter]

LIPPINCOTT: So at last you are a professor, in 1985.

SCHWARZ: Yes.

LIPPINCOTT: Did that change your work life here?

SCHWARZ: Then I had to teach. [Laughter] I did teach several times—quite a few times, actually. When I was a research associate, I didn't have to teach, but occasionally I did on a voluntary basis. I taught maybe one-third of the time or something, and I did a lot in terms of advising students. I even did things like freshman admissions and all kinds of stuff, which I didn't have to do.

LIPPINCOTT: When you were a research associate?

SCHWARZ: Yes. I remember that once I did a two-week tour of the Midwest, going to the various high schools and interviewing students. [Laughter]

LIPPINCOTT: Well, they really got their money's worth out of you.

SCHWARZ: [Laughter] I did all kinds of stuff, which was kind of fun. It was a little arduous, but it was interesting.

LIPPINCOTT: So would you say your life changed markedly then, after Aspen in '84 and your full professorship?

SCHWARZ: Yes. The promotion had nothing to do with my life changing, really, but certainly the breakthrough and the recognition of string theory did, because now it was a major activity. I was not a major celebrity, but somewhat of a celebrity. There were magazine and newspaper articles all about it.

LIPPINCOTT: And you wrote a textbook with Ed Witten and Michael Green?

SCHWARZ: Yes, that's right.

LIPPINCOTT: *Superstring Theory*.

SCHWARZ: Yes. I guess it was in summer of 1985 that Michael and I decided we ought to write a book, and after discussing it with one another we said, "Let's see if we can get Witten to join us."

LIPPINCOTT: Who did most of the writing? Who did most of the work? Do you mind talking about that?

SCHWARZ: No.

LIPPINCOTT: This is a textbook, correct?

SCHWARZ: Yes. It's an advanced monograph, 1,000 pages, full of equations. It requires a background of quantum field theory, general relativity, and so forth, so it's quite advanced. But it certainly has been used in various places as a textbook, and it's sold very well.

LIPPINCOTT: Who published it?

SCHWARZ: Cambridge University Press. It's certainly the first book they did that was written in TeX.

LIPPINCOTT: In TeX?

SCHWARZ: Yes, using the modern computer technology. TeX is the word-processing system that's used for scientific papers. When we wrote it, Michael was in London and Ed was at Princeton and I was here in Pasadena. We had a separate computer file for each chapter. We each wrote a first draft of one-third of the chapters.

LIPPINCOTT: So the work was pretty evenly divided?

SCHWARZ: Yes. And then we would send chapters to the others, so we had them circulating. But E-mail didn't exist in those days.

LIPPINCOTT: Even after '85?

SCHWARZ: What did exist was that the experimental groups in London, at Princeton, and at Caltech had developed computer links for exchanging data.

LIPPINCOTT: Yes. There was something called ARPAnet that people used.

SCHWARZ: Yes. There were these predecessor things that the experimental groups were using for exchanging their data, so they arranged for us to piggyback off of that. So we used the services provided by our experimental friends to send our files to one another, because we were writing these things on the IBM PC1, or whatever it was. Every time I TeX'd a chapter, it would take about five minutes. [Laughter] And then, if I wanted to print it out, I couldn't figure out how to print one page—I'd have to print out the whole thing. It was just horrible. [Laughter]

LIPPINCOTT: So you're kind of a pioneer of the Internet as well.

SCHWARZ: Yes. So we gave this thing to Cambridge camera-ready. They had never done a book like that before.

LIPPINCOTT: It's still in print?

SCHWARZ: Yes, it's still around. A year ago or so, in '98, there was a new book, by Joe Polchinski at Santa Barbara, which is much more up-to-date than ours. So that's probably the main text that people would use now. But there is still some material that can be found in ours that he didn't reproduce, because his is mostly filled with the newer material.

LIPPINCOTT: You spent a term at Santa Barbara, at the Institute for Theoretical Physics there.

SCHWARZ: Yes. I've been there several times, actually.

LIPPINCOTT: 1986?

SCHWARZ: Yes.

LIPPINCOTT: Was that the first time?

SCHWARZ: 1986 may have been the first extended visit. I've visited several times since then.

LIPPINCOTT: Whom did you work with there?

SCHWARZ: Well, the Institute for Theoretical Physics has programs in different areas, and every now and then they have a string theory program, so each time I've gone there it's been when there's a string theory program. For example, next June I'm planning on going there for a month. Next year, they're having a string theory program again. It's just a great place. They always give me an office with an ocean view. What more can a person ask for? [Laughter] You get the best physicists and a perfect environment.

LIPPINCOTT: OK. You're going to talk to me about 1984—a school in Jerusalem.

SCHWARZ: Right. There is a winter school in Jerusalem which takes place over New Year's each year. It starts around Christmas and goes for about two weeks. This was organized by Steven Weinberg in those days, and I was invited to lecture for the 1984-1985 school. The title of the school that year was "Physics in Higher Dimensions."

LIPPINCOTT: This is really like a big conference—a two-week thing?

SCHWARZ: Yes. There are about eight or ten lecturers each year, who each give four or five lectures. I was one of the lecturers that year.

LIPPINCOTT: Where is it held in Jerusalem?

SCHWARZ: At the Hebrew University. So I had agreed to speak there, perhaps a year ahead. And then there were all these developments, on the anomalies and heterotic strings, that came up in the interim, so that's what I spoke about in my lectures there.

LIPPINCOTT: Who else was there?

SCHWARZ: Well, one of the student participants was a woman who was an undergraduate at San Francisco State University, who was very enthusiastic and agreed to Xerox all the transparencies and was very helpful in organizing the thing. Anyway, this lady is now my wife. [Laughter]

LIPPINCOTT: Oh, this is Patricia?

SCHWARZ: This is Patricia. So that's where we met—we met in Jerusalem. Of course, she's from Northern California and I'm from Southern California, but we met in Jerusalem. [Laughter]

LIPPINCOTT: And she was a physics graduate student?

SCHWARZ: Actually, she was an undergraduate at the time and subsequently became a graduate student at Caltech.

LIPPINCOTT: How would an undergraduate in physics get to go to this school?

SCHWARZ: Well, that was bizarre. She wasn't really very well qualified for it, but she had seen an advertisement of this school on her bulletin board at San Francisco State University. She made a casual remark about it to her department chairman that "Gee, that looks interesting." And without her knowledge, he submitted an application on her behalf, and she was awarded a scholarship to attend. When she found out about it, she was very surprised and said, "I'm not really qualified for that." So her chairman, Gerry Fisher, said, "Well, you'd better go, because I've arranged everything." [Laughter] So she went.

LIPPINCOTT: What a break for her!

SCHWARZ: So that was an unusual situation.

LIPPINCOTT: And all kinds of physics were being discussed? You were the only string person?

SCHWARZ: I was the only string theory person. But, as I said, the subject was "Physics in Higher Dimensions," and string theory was ten dimensions. Another topic that was very hot those days was called eleven-dimensional supergravity, and the person who was lecturing on that was Michael [J.] Duff. I forget where he was at that time; until recently he was a professor at Texas A & M and now he's at Michigan. He may have been somewhere else then. But anyway, Michael was lecturing on eleven-dimensional supergravity. I always felt that eleven dimensions was a ten-percent error. [Laughter] Because ten was the right answer. But, as we discovered many years later, eleven-dimensional supergravity does have a place in string theory. In fact, in our textbook—[the one] with Green and Witten—we remark that eleven-dimensional supergravity is an intriguing theory but we don't know how it fits into string theory, and it would be very interesting to know. It took another ten years to find the answer to that question. [Laughter]

LIPPINCOTT: Is that the membranes? When you go and get into membranes?

SCHWARZ: Well, it's related to what's called M theory.

LIPPINCOTT: And was Patricia interested in this kind of physics?

SCHWARZ: Yes, she was.

LIPPINCOTT: She's into string theory now, isn't she?

SCHWARZ: Right. She's developed a Web site about string theory, called superstringtheory.com, which she puts a lot of effort into. She's still working on it and still developing it.

LIPPINCOTT: So she came here to Caltech and was your graduate student?

SCHWARZ: No, that wouldn't have worked.

LIPPINCOTT: You weren't married at that time?

SCHWARZ: Well, we got married in July of '86.

LIPPINCOTT: Oh. And you met in the winter of 1984-1985?

SCHWARZ: Yes. And she entered the graduate school here, I believe, in the fall of '86. Yes, I'm pretty sure that's right.

LIPPINCOTT: Whom did she work with?

SCHWARZ: It's a complicated story. She was formally an advisee of [professor of theoretical physics] John Preskill, in the particle theory group. But I think there was sort of a misunderstanding, because I had assumed that Preskill would really advise her, and I think Preskill assumed that I would really advise her. Somehow she fell through the cracks and didn't get the support and advice that she deserved.

LIPPINCOTT: Did she get her doctorate?

SCHWARZ: Well, what happened was that after about four or five years she kind of dropped out without finishing her degree. She got into Web design and other things, and then after a few years we agreed that this was really a waste and that she should finish up. But then the question arose, if she's going to resume, who's going to advise her? So we were able to arrange for her, when she resumed her research, to be advised by Renata Kallosh, who's a professor at Stanford.

LIPPINCOTT: Oh. Andrei Linde's wife.

SCHWARZ: That's correct. Renata was her actual advisor for the last year. And [professor of theoretical physics] Steve Frautschi signed the papers to make it official from this end.

LIPPINCOTT: So it's a Caltech PhD.

SCHWARZ: So she got a Caltech PhD in the end. She received that just a couple years ago.

LIPPINCOTT: That's a pretty unusual setup, isn't it?

SCHWARZ: Yes. But anyway, because of all this confusion about advisors and so on, it seemed like a good arrangement. She felt more comfortable working with Renata.

LIPPINCOTT: Well, do you have any other memories of Jerusalem, besides meeting your future wife there? [Laughter] Was your talk well received? It must have been.

SCHWARZ: Yes. Well, certainly the people there knew that this was becoming a hot subject. But it was sufficiently new that none of the other participants there were really up to speed on it.

LIPPINCOTT: Did it go over their heads, mostly?

SCHWARZ: Perhaps it did. I mean, the people who were up to speed were really the people who were in the Princeton area, and they were not there. Because there was a very large number of people at the Institute for Advanced Study and in Princeton University who were hot on this stuff—on the order of thirty or forty people.

LIPPINCOTT: Did Weinberg understand it?

SCHWARZ: Well, in general terms. I don't think he took the trouble to get into the details. There's no question that he had the ability to. He just wrote a definitive book on supersymmetry. The guy is an awesome scholar. If he decides to learn something, he understands it better than anyone else when he's finished. But I don't think he's ever—I was going to say that he's never put that effort into string theory, but that might not be true, because I believe he has taught a course at the University of Texas on string theory. So he probably knows it pretty well, but he hasn't really contributed to it.

LIPPINCOTT: Did he have any reaction to your talk? I guess he was glad to have it.

SCHWARZ: I think he was pleased that he had invited me, in view of subsequent developments. I don't remember any specific comments about whether or not my lectures were successful. I don't know if they were. But the following year they had a school that was devoted to the subject.

LIPPINCOTT: Devoted to string theory?

SCHWARZ: Yes. I did not participate that following year, though.

LIPPINCOTT: OK. [Tape is turned off]

JOHN H. SCHWARZ

SESSION 2

July 26, 2000

Begin Tape 3, Side 1

LIPPINCOTT: We left off last time in 1985, when you became a full professor. I'd like to pick up there. String theory now was booming, in a way—as opposed to the neglect that it had suffered before. So your life changed. One thing I wanted to ask you about was the MacArthur Fellowship you got in '87. What did you do with it? May I ask how big it was?

SCHWARZ: Yes. Awards in those days were based on age, so it increased a little bit if you were older. I was somewhere in the middle. [Laughter] I was in my mid-forties at the time. Probably half the recipients are younger than that and half are older, more or less. I don't remember the precise amount I got, but spread over five years, it was something like $280,000, which is nice. The tax law changed the year before I got it, so prizes and awards of this kind were taxable already when I received it. Had I received it a year earlier, it would have been tax-free. [Laughter]

LIPPINCOTT: What did you do with it, or isn't that germane?

SCHWARZ: [Laughter] I don't know if it's germane, but we did get a nicer house. We were living in a little condominium on Catalina before that, and we got a nice house down on Arden Road, which is also walking distance from campus. So it was certainly helpful in that regard.

LIPPINCOTT: Yes. Now that you're a full professor, you have teaching duties.

SCHWARZ: That's right.

LIPPINCOTT: Although you say you taught before.

SCHWARZ: Yes. When I was a research associate, I did teach occasionally. But of course as a professor you're expected to teach one course each quarter, basically.

LIPPINCOTT: Did you have a particular course that you liked teaching?

SCHWARZ: Well, I've taught different things over the years.

LIPPINCOTT: Do you ever teach just on string theory?

SCHWARZ: Yes. I gave a course on string theory a few times, especially when we were writing the book on string theory. It was helpful to me to be teaching it at the same time, so I did that a couple of times in those years. More recently, I have tended to steer away from giving a course specifically on string theory; rather, I give courses on advanced topics that are useful for people who want to do string theory but aren't specific to string theory. This would include some of the more advanced mathematical topics that you need to do string theory, or some of the more advanced physics topics, like supersymmetry, that are important in string theory. So I'm giving courses on things of that sort.

LIPPINCOTT: Are there any courses on string theory here at Caltech?

SCHWARZ: Well, I did give some, and this fall Edward Witten is going to be giving a course on string theory, in the fall and winter terms, and it will be continued in the spring term by one of our other new professors, Steve Gubser. We're certain that there's going to be a great deal of interest in this course.

LIPPINCOTT: Are these for graduate students?

SCHWARZ: They will be aimed at graduate students, but my guess is that half the people who attend are going to be faculty.

LIPPINCOTT: Do you think that undergraduates aren't ready for that yet?

SCHWARZ: It would be an exceptional undergraduate who would have the preparation. There might be one or two, perhaps, who would, but generally the answer would be no. Certainly anyone is welcome to come who feels they can derive some benefit from it.

LIPPINCOTT: Yes. Before we leave the eighties, in your CV here it mentions that you gave some lectures in China in ’87 in Beijing.

SCHWARZ: Yes.

LIPPINCOTT: Was that a long stint?

SCHWARZ: No, we were only in China for a couple of weeks. So there was one week in Beijing and another week in a city named Hangzhou.

LIPPINCOTT: Did you talk at Beijing University?

SCHWARZ: Well, in Beijing I spoke at a number of places. I was basically a guest of the Chinese Academy of Sciences. I remember when I showed up there they asked, “How much material do you have prepared?” So I said, “I could give you three one-hour lectures.” And they said, “Well, we’re not very up to speed on this physics, and our English isn’t very good, so we’d like it if you spoke very slowly. So we’ll schedule you for four—or maybe it was three—two-hour lectures.” [Laughter] So I had to take my three hours of material and stretch it out over several more hours, which is what I did. I said that I’d be willing to go on for two hours if they gave me a break in the middle, so they agreed to that. That same week, I also gave a seminar at Beijing University and at another physics laboratory, where they have an accelerator.

LIPPINCOTT: Were these to faculty or to students?

SCHWARZ: Well, I’m sure there were both faculty and students present.

LIPPINCOTT: Did they seem to get it?

SCHWARZ: Well, they were interested and polite and treated me very nicely.

LIPPINCOTT: Good. You know, at some point I wanted to ask you about the people who were resisting superstrings, the prominent one being Sheldon Glashow at Harvard.

SCHWARZ: Yes, right.

LIPPINCOTT: Did he begin fulminating in the eighties, or was that later?

SCHWARZ: When the subject wasn't popular, it wasn't something he needed to address or felt he needed to address. But after it took off in late '84, he did make some outrageous statements, some of which got picked up by the media. Because, being a Nobel laureate, he was rather influential.

LIPPINCOTT: Yes.

SCHWARZ: He said some things which were very unfortunate. In fact, I think he even had an autobiography he wrote where he repeated some of these things. He made remarks like, "People who work in this shouldn't be paid," and, "It borders on theology." So it was stupid things like that.

LIPPINCOTT: What were his physics objections to it? Or didn't he—

SCHWARZ: Well, I don't think he really knew anything about the subject. But what he did know—and to some extent it is a legitimate concern that we share—is that it was very difficult to relate the subject to experiment. The ideas are most obviously relevant at extremely high energies, where they can't be tested, so it's very difficult to figure out what the consequences are for ordinary energies and for experiments that can be done. We're convinced that there are such consequences, but it takes a lot of understanding of the theory to really extract what they might be. Most of the success in physics in the prior years, such as the development of the Standard Model, was an interplay between theory and experiment—where one made a sequence of small steps where there'd be some theoretical advance and then an experiment would perhaps confirm it, and then you'd find something else that was surprising. It was kind of a back-and-forth process. I think Glashow was one of the people who felt that this was the right way for the subject to progress. And the idea that someone would come along with a grandiose theory at some much higher energy that would somehow have all this as a consequence I think he found very repulsive.

LIPPINCOTT: Did he speak to you personally about this? Did you ever have any talks with him?

SCHWARZ: Well, I didn't seek him out. But we did cross paths a few times. I remember one conference banquet in France, I think, where we sat across from one another. I guess this was in the early nineties. And I think on that occasion I reminded him that it was over five years ago that he had been quoted in a national magazine as saying that string theory would be dead within one year. And I asked him if he wished to revise his estimate. [Laughter]

LIPPINCOTT: Was he the one who used the phrase "theory of nothing"?

SCHWARZ: I don't know whether he used that or not. The phrase "theory of everything," which has been used in connection with string theory, is a phrase I don't like myself and have tried to avoid. It was introduced by somebody else. There are several reasons I don't like it. One reason I don't like the phrase "theory of everything" is that it gives other physicists the impression that people who work in this field feel that their work is more important than what other people are doing, and this creates a certain hostility or bad feelings. My personal feeling is that what we're doing is interesting and important but what other people are doing is also interesting and important, and any phraseology that's going to create a wrong impression I think is unfortunate. So that's one reason I don't like that expression. Another is that I think it's misleading, because even if we did solve all the problems we're trying to solve, there would be many things that were not explained—it's not a theory of everything. [Laughter] It's a theory of something—something that's very fundamental and very interesting. But there's a lot more to the world than what you can learn from the basic underlying microscopic physical laws.

LIPPINCOTT: Yes. Well, it's an interesting phrase, because it really existed before string theory, didn't it?

SCHWARZ: I think that this phrase was introduced by a fellow named John Ellis at CERN around 1985, in connection with string theory.

LIPPINCOTT: Oh, is that right?

SCHWARZ: Yes.

LIPPINCOTT: But the idea is that it's the theory that takes quantum mechanics and relativity into account. And this is what's so—

SCHWARZ: Yes. Einstein had used the phrase "unified field theory" in his failed attempts to combine general relativity and electrodynamics. He couldn't have succeeded, for several reasons. He didn't know about the nuclear forces, which are very important, and also he was uncomfortable with quantum mechanics, even though he had played an important role in its development. And certainly quantum mechanics and knowledge of other forces—all of these things are very crucial to getting the whole picture straight.

LIPPINCOTT: So if string theory does pull these two other theories together—that is, relativity and quantum mechanics—are you saying that there's still stuff we won't know? What else is there?

SCHWARZ: Well, if you understand the fundamental microscopic rules of the universe, you can't explain why there's life on Earth, who's going to win the next election, whether the stock market's going to go up or down. [Laughter] There are many things you cannot explain with this information.

LIPPINCOTT: [Laughter] Oh, I see what you mean. You might hurt the biologists' feelings. OK.

SCHWARZ: I guess one can make a distinction between what we might call environmental sciences and physical sciences. What I call "environmental" in this sense is one where you could ask whether the rules are the same in another galaxy or something. So if there's life somewhere else in the universe, it's likely that the details of how it works are different on Earth, because we might not use exactly the same chemicals or whatever. But the physics should be exactly the same.

LIPPINCOTT: Yes.

SCHWARZ: So things that are exactly the same are somehow more universal. And things that vary I'm referring to as environmental in this context. So all these environmental things are not going to be explained by string theory.

LIPPINCOTT: Well, I think when physicists talk about the theory of everything, they're implying that some of these other matters aren't of equal importance, and that's what you object to.

SCHWARZ: Yes. Obviously other matters are very important. Biology's certainly important. And even political science and government—they're important, whether we like them or not [laughter], because they affect our lives. String theory doesn't affect most people's lives very much, except as an intellectual curiosity.

LIPPINCOTT: Yes. So when we get into the nineties, there's another kind of dip in the fortunes of string theory, isn't that right? The fact that you've had maybe up to five gauge groups that this works for?

SCHWARZ: Yes. Starting in '84-'85 there was enormous enthusiasm for the subject, and hundreds of people jumped into it, and there was quite a bit of progress throughout the late eighties. I guess things did slow down somewhat in the early nineties. Not dramatically—there were still interesting developments and there were international conferences where a couple of hundred people would always show up. So it remained an active field, but there weren't major breakthroughs occurring at short intervals, and some people did drop out of the subject, and perhaps the total number was even decreasing there for a period. The job market wasn't that great for young people looking for faculty positions in string theory in the early nineties. I guess it's fair to say that it was pretty bad, actually. I didn't feel at the time that it was a dark period, but in retrospect I guess you could say, compared with what followed and also what preceded it, that there was perhaps a dip, as you say, since certainly in the late eighties there was this enormous enthusiasm, which died down a little bit as people made a more realistic assessment as to what it would take to achieve our goals.

LIPPINCOTT: I was wondering whether one of the problems was that there were too many versions of the theory. That's what I've read in some popular accounts.

SCHWARZ: Yes.

LIPPINCOTT: So that's not as enticing as one single string theory that works. If there are too many, it's almost as bad as—

SCHWARZ: Well, it was puzzling to us why there seemed to be five consistent superstring theories. We only needed one theory to describe the world, we felt, at least if it was the right one. There were much fewer than in quantum field theory. In quantum field theory, there are an infinite number of quantum field theories, with all sorts of freedom to do things differently, although none of them contains gravity in a consistent way. Here we had five theories, each of which contained gravity, but we only wanted one.

The story really changed at the end of '94 or beginning of '95. I think the climax was a lecture that Ed Witten gave at a conference at USC [University of Southern California] in the spring of '95, which was a conference called "Strings '95," the annual strings conference. There had been some related work earlier; I won't say that every new idea was in that one lecture, but a remarkable number were. People were kind of blown away by it. So what became clear, with that lecture and subsequent developments, was that in fact all of these string theories are related to one another in different ways by what we call dualities. So we were able to recognize them as just five different special cases of a single underlying theory; it became clear that there really is just one theory. And this non-uniqueness has to do with the fact that this single theory has many different solutions, or quantum vacua. And that's what we had actually been counting; we hadn't been counting different theories. So there seems to be a unique, underlying theory.

LIPPINCOTT: Is this what's called M theory?

SCHWARZ: Well, the term "M theory" has been used in this connection. It's used in two different contexts, so it's a little bit confusing. One of the new discoveries that arose at this time was that in certain situations there can be an eleventh dimension, and the five string theories all have been formulated as having ten dimensions—nine of space and one of time. But we learned from the work of Witten and others that when the interactions become strong, there's actually an eleventh dimension that appears. So the term "M theory" was actually introduced first to describe this eleven-dimensional situation. But then the term has also been used to represent the underlying theory.

LIPPINCOTT: The mother theory.

SCHWARZ: Yes. I think Ed Witten had the idea that "M" should stand for "mystery" or "magic" or something like that. But not long after the war with Iraq, it was tempting to think of it as the mother of all theories. [Laughter]

LIPPINCOTT: [Laughter] Yes, and there have been a number of...well, "matrix" was another one, wasn't it?

SCHWARZ: Yes, there were lots of things that started with the letter "M." And one of the proposals for giving a more precise mathematical description of M theory was due to a number of people who developed what they called matrix theory. So if you accept the matrix-theory proposal, then you can think of the "M" as standing for matrix theory.

LIPPINCOTT: What about membranes? That's what came up in Witten's talk in 1994?

SCHWARZ: Well, that's another word that starts with the letter "M." The eleven-dimensional theory contains extended objects of various dimensions, just as the various string theories do.

This is another remarkable fact that was discovered in the mid-nineties, because up until that point we just had the fundamental strings, which were one-dimensional objects. But we realized that when you go beyond the perturbation-theory analysis that we had been using until then, you could identify objects of various dimensions in these theories. And in the eleven-dimensional theory there are objects that have two dimensions—sometimes called 2-branes, or membranes. And then there are also objects that have five dimensions, called 5-branes.

LIPPINCOTT: And then there's something called a *p*-brane.

SCHWARZ: Well, when we use *p*, that's a generic—*p* represents a number. So 2-brane is the case where *p* is equal to 2, and so forth. [Laughter] It's a silly terminology.

LIPPINCOTT: Maybe. But you no longer think of these strings as one-dimensional—you yourself?

SCHWARZ: Well, the strings are one-dimensional.

LIPPINCOTT: They're not all now membranes?

SCHWARZ: OK. In the Type IIA theory, it turns out that you can identify the fundamental string as a membrane of the M theory with one of its dimensions wrapped on a circle. So in that special case, there is a dual interpretation of the string as a membrane. But in many of the other cases, it's more natural to think of the strings as being fundamental. In fact, in any of the string theories, viewed in perturbation theory, the strings are fundamental and one-dimensional objects. So it's only when you go beyond perturbation theory that all these new sets of issues arise—which are very interesting and show that when the interactions are strong, the strings are *not* so fundamental and are more or less on an equal footing with a whole range of other kinds of excitations.

LIPPINCOTT: OK. Well, let's get away from the physics for a minute. You got a chair here in 1989. That was the Harold Brown Professorship of Theoretical Physics.

SCHWARZ: Yes.

LIPPINCOTT: Was that the first of those?

SCHWARZ: Yes, this was a new chair at the time.

LIPPINCOTT: How did that affect your life as a Caltech faculty member?

SCHWARZ: It didn't have much effect.

LIPPINCOTT: You got more money for visitors, I'd guess.

SCHWARZ: Well, the financial impact when you have a chair is that you get some extra research funds to spend on your professional expenses, so that was a benefit I derived from it.

LIPPINCOTT: And now you're in Feynman's old office.

SCHWARZ: Yes.

LIPPINCOTT: So at some point—[laughter] not when you were a humble research associate, but at some point—you got this shrine up here.

SCHWARZ: That's right.

LIPPINCOTT: Do you feel like you're in a shrine, or do you feel like it's yours? How long have you had this office?

SCHWARZ: I moved in here approximately ten years ago. He died in '88, and it was vacant for over a year, and then at some point I think I asked the chair if it was OK if I moved in, and he said that it would be. I completely redecorated it, so it looks very different from the way it did when he was in it. So I don't give that too much thought.

LIPPINCOTT: OK. Now, you mentioned the string conferences. These are annual?

SCHWARZ: Yes, there's an annual strings conference. Strings 2000 was just held in Ann Arbor, Michigan. Strings '99 was in Potsdam, Germany. And Strings 2001 will be in January in Bombay, India.

LIPPINCOTT: When did these begin?

SCHWARZ: Well, in the beginning, they didn't carry the numbers, so it's hard to say exactly which was the first one. But certainly since the mid-eighties there have been roughly annual conferences on string theory. They didn't always, as I say, carry these numbers; starting in the early nineties, they started to carry these numbers.

LIPPINCOTT: There was one here at Caltech in January.

SCHWARZ: That wasn't part of the Strings XX series, because, as I said, the 1999, 2000, 2001 conferences were elsewhere. The one we had here in January was sort of halfway between two of these other conferences. We entitled it "String Theory at the Millennium." It was run in a rather different style from these annual string meetings. The usual string meetings go for a full week, and there are roughly fifty thirty-minute talks, and they're very intense. And we wanted something rather different for our millennium celebration, so what we did was we had four days of lectures. The first three days, we had just four lectures a day, which were ninety minutes each. So this way, twelve of the leading experts could discuss aspects of the subject in great depth. And all twelve of those talks were really excellent. Each one had interesting new material—so that was extremely successful. And then on the final day, which was a Saturday, we had eight of the more senior people, including myself, give thirty-minute talks which were meant to be mostly speculations about the future of the field. At least, that's what the speakers were instructed to do. Not all of them actually tried to do that; they just talked about their most recent ideas. [Laughter] But that was the idea. And all of those lectures are available on the Web. We have a Web site for that conference, so anyone who wants can listen to any of those lectures anytime they feel like it.

LIPPINCOTT: I notice that Murray Gell-Mann came to that conference.

SCHWARZ: Yes, Murray came. He was our banquet speaker after dinner. We had a banquet at the Athenaeum.

LIPPINCOTT: How was his talk at the banquet?

SCHWARZ: Well, it was nice. He reminisced about string theory and his warm feelings toward the subject and so on. It was a nice occasion. I'm glad we were able to get him to do that. I've organized three dinners at the Athenaeum where Gell-Mann was the featured speaker. [Laughter] The first one was in 1989 for Gell-Mann's sixtieth birthday. It was a big job. We had two days of lectures and the banquet at the Athenaeum, where I emceed and introduced him. That was the first. The second dinner at the Athenaeum was on the occasion of his retirement from Caltech. I somehow got stuck with organizing that dinner, too. [Laughter]

LIPPINCOTT: He's emeritus here, in the catalog.

SCHWARZ: That's right.

LIPPINCOTT: But he's at Santa Fe [the Santa Fe Institute] all the time.

SCHWARZ: Right. So that was his retirement. It was roughly '94, somewhere around there. So this was the third Athenaeum dinner where I got to introduce him.

LIPPINCOTT: Well, you know, he's not that much of a fan of a lot of physics and physicists, so it's kind of unusual that he was so warm about string theory. Well, I mean, I think he thinks it's important.

SCHWARZ: Murray is someone who I've always felt has very good taste in physics and an uncanny ability to spot good ideas early. And I'm not referring only to string theory when I say that. He's, of course, always been very outspoken about his opinions, and he can be negative as well as positive. But fortunately I've always been on his good side, so I've always had a good relationship with him.

LIPPINCOTT: Yes. Was James Hartle at this conference?

SCHWARZ: He certainly would have been at the sixtieth birthday conference.

LIPPINCOTT: Oh, yes. But he doesn't do much work in strings.

SCHWARZ: No, he doesn't do string theory. He's a relativist. I don't think he came to our "String Theory at the Millennium" conference, but there were 300 people there. I don't recall him being there.

LIPPINCOTT: Yes. I just know he's another physicist that Murray does a lot of work with.

SCHWARZ: That's right.

LIPPINCOTT: In '93 you went to UCSB again for the session March through June at the Institute for Theoretical Physics.

SCHWARZ: Right. So they would have been having a workshop on string theory.

LIPPINCOTT: Yes. And then in '95 you went to Rutgers.

SCHWARZ: Yes. I spent a couple of months there in the fall. They have a very strong string theory group at Rutgers. Somewhere around 1990 or so, they hired four of the leading people all at once and instantly put themselves on the map as one of the top groups.

LIPPINCOTT: Who were those people?

SCHWARZ: The initial four were Steve Shenker, Dan Friedan, Tom Banks, and Nati [Nathan] Seiberg. And they've hired some other outstanding people since them, Sasha [Alexander] Zamolodchikov and Michael Douglas, for example. And more recently they hired Greg Moore. But there is also movement. Steve Shenker left a couple years ago to go to Stanford, and Nati Seiberg left a couple years ago to go to the Institute for Advanced Study. So there's movement.

But in any case, it was a very, very strong group, and also it's not that far from Princeton. I had a nice visit there for a couple of months that fall.

LIPPINCOTT: Did you accomplish anything huge there?

SCHWARZ: Yes. That's when the second superstring revolution was really at its peak. And there was a period that fall where there were important breakthrough papers occurring almost every week. So one week would have been the paper by [Petr] Horava and Witten on the eleven-dimensional interpretation of the E8 x E8 theory, which was dramatic. And another week would have been Joe Polchinski's paper on D-branes, which was absolutely vitally important. And another—well, I guess it was January of '96, so a little bit later—was the paper of [Andrew E.] Strominger and [Cumrun] Vafa on explaining black hole entropy in string theory. So it was just a period of very intense activity.

LIPPINCOTT: Yes. Could you talk a little bit about the implications for cosmology and for the Big Bang work? Because I'm thinking about your idea that a little 10^{-32} thing is the smallest thing there is. Whereas the Big Bang seems to have proceeded from nothing—I think we talked about this last time—from a singularity, which is a dimensionless point. But you have a dimension.

SCHWARZ: Well, general relativity certainly breaks down at very short distances and is plagued with singularities which generically arise. And certainly in cosmological solutions, when you look backward in time you come to some—what seems to be—point singularity, which is attributed to the Big Bang. But certainly, as you go backward in time, before you reach that singularity you get into a regime where all the effects of string theory should become very important. And there's quite a bit of work going on, trying to figure out exactly what the implications of string theory are for what the universe was doing in that era. It's not a subject I've gotten involved in myself. I was just noticing there's a conference, I think in Vancouver, next month on this topic—on string cosmology. I point that out just to say that it is an active area.

My personal feeling is that while there are undoubtedly interesting things that will someday be said, we probably don't understand string theory well enough to figure out exactly what they are just yet. So I'm skeptical that there's going to be major progress on understanding cosmology coming from string theory in the near future. Although eventually I hope there will be.

LIPPINCOTT: So in a way, cosmology's coming to the dead end that people thought physics was coming to.

SCHWARZ: Oh, no. Cosmology is a very active and exciting field these days, but that activity is not being driven by string theory. It's being driven by observations, where there is recent evidence for a cosmological constant coming from studies of Type IA supernovas and from the anisotropies in the microwave background radiation, which was just reported by Andrew Lange and his collaborators. It's all very fascinating work, and hopefully string theory will have something to say about these matters someday, but I don't think we're there yet.

LIPPINCOTT: And it should have something to say about black holes, too. In other words, do they proceed from a dimensionless point or not? I mean, that's kind of a basic question, isn't it?

SCHWARZ: Well, string theory has made some interesting insights into black holes already, not so much for the kinds of black holes you see in astrophysics but for more theoretical, ideal ones. One of the interesting discoveries of [Jacob] Bekenstein and Hawking in the mid-seventies was that black holes have thermodynamic properties, so that you can attribute a temperature and an entropy to them. And the meaning of entropy in other branches of physics has a very nice quantum mechanical significance. It has to do with how many different quantum states the system can be in. So given that black holes have entropy, this suggested that there should be a definite number of quantum states associated with a black hole. But from anything that the relativists were able to figure out, the properties of black holes were almost completely unique. There wasn't any indication of all these different quantum states. But what this work of Strominger and Vafa did was to show that, at least in a special case, you could use string theory methods—D-brane techniques, in particular—to compute the entropy of certain black holes by counting quantum states. They were able to make this connection between the number of quantum states and the entropy of the black hole by using details of string theory. That was a very exciting development, because up until then it wasn't entirely clear how seriously this entropy notion should be taken in this connection.

LIPPINCOTT: But you're not terribly interested in these cosmological questions?

SCHWARZ: Well, I'm very interested in them. I'm not actively researching them myself, but I certainly have been following the subject closely.

LIPPINCOTT: May I ask you what you are actively researching?

SCHWARZ: Sure. There are a number of interesting topics nowadays that I'm trying to think about.

LIPPINCOTT: Maybe this will be getting too technical?

SCHWARZ: It probably will [laughter], which is why I'm hesitating, thinking about how to express myself. You kind of caught me off guard on this question.

LIPPINCOTT: Oh, I'm sorry.

SCHWARZ: There are various issues in connection with D-branes. D-branes are very important in string theory, because gauge theories of the type that occur in the Standard Model can be associated to these D-branes.

LIPPINCOTT: Is a D-brane a membrane with multiple dimensions?

SCHWARZ: Yes. They can have various dimensions. It's a surface embedded in the higher-dimensional spacetime. And then the idea is that certain types of particles and forces exist only on the surface and not in the bulk away from it. So when you go in the bulk of the spacetime, away from the D-brane, you can find gravitational-type forces, but not the other forces, such as those that appear in the Standard Model. So there are a host of fascinating questions in connection with D-branes. I don't think I should go into detail about those.

LIPPINCOTT: OK.

SCHWARZ: But anyway, I'm thinking about some of them.

LIPPINCOTT: How about Glashow? Has he come around?

SCHWARZ: Well, he's moved from Harvard to Boston University, the last I heard. Harvard, in the meantime, has hired several of the very top string theorists. They don't deserve it, but they've got one of the best groups in the country now. Well, first they had Vafa. He's been there for quite a while. He's absolutely great! And then, a few years ago, they got Strominger, who's also great. And very recently they got Juan Maldacena added as a third string-theory professor. Juan, who just got his PhD a couple of years ago, has caused quite a sensation with a major discovery. And he's already got tenure now at Harvard. He's from Argentina, and just very, very good.

LIPPINCOTT: And is Glashow languishing at Boston University now?

SCHWARZ: Oh, I think he's enjoying life.

LIPPINCOTT: [Laughter] Well, that's good. So let's get back to maybe just Caltech. Now that you're a professor and doing some teaching, you must have graduate students. Do you have anyone in particular who you think is good or that you're working with a lot that you'd like to mention?

SCHWARZ: Well, I've had quite a few students over the years. I don't have the precise number, but I think about thirty of my students have gotten PhDs over the years.

LIPPINCOTT: Any that stand out in your mind?

SCHWARZ: Well, probably the one who's most well known in academia would be Michael Douglas, who, as I mentioned earlier, is at Rutgers now. I've had other students who have done well, too.

LIPPINCOTT: Any at the moment?

SCHWARZ: There's one fellow who's just graduating who's not going to continue in theoretical physics.

LIPPINCOTT: Do you mean that he's getting his PhD and then dropping it?

SCHWARZ: Yes. He has various personal reasons for that decision. And then the other students that I have at the moment are all at rather early stages.

LIPPINCOTT: How do you work with them?

SCHWARZ: Well, it works differently in different cases. Some of the students I've had in the past I've collaborated with on projects. With others, I've suggested problems, which they have worked on on their own. And in some cases they've been completely self-driven. [Laughter] They invent their own problems and solve them and hardly ever come to see me. Michael Douglas was somebody of that kind. He really pretty much educated himself.

LIPPINCOTT: Yes. Let's talk about Witten for a bit, because he has come from the Institute for Advanced Study to Caltech. Was that last year?

SCHWARZ: Yes, he arrived last September.

LIPPINCOTT: And you say he'll be here for one more year?

SCHWARZ: He already has an offer to stay permanently at Caltech, and we're all very much hoping he'll accept that. But all that he's definitely decided so far is to continue for a second year. So we don't know when he'll be making his decision. We're not putting pressure on him, since we don't feel there's anything to be gained by doing that. He'll decide whenever he wants. [Laughter]

LIPPINCOTT: Well, when he arrived, did you work with him last year? What was your interaction with him?

SCHWARZ: We talked a lot last year. We didn't write any papers together. I hope that eventually we will. He's such a creative and clever guy—it's really exciting to interact with him; I would like to get involved in projects with him.

LIPPINCOTT: And what's this joint institute between USC and Caltech? Could you explain what that is?

SCHWARZ: Yes. We've developed a closer relationship with USC than we've had previously. They, for a number of years, had several professors in string theory: Itzhak Bars, Krzysztof Pilch, Dennis Nemeschansky, and Nick Warner. As part of this plan to bring Witten here, his wife, who's also a theoretical physicist, was offered a professorship at USC.

LIPPINCOTT: That's Chiara Nappi?

SCHWARZ: Chiara Nappi, yes. She's very good. I'm absolutely convinced that the USC people are discovering that they got someone much better than they ever expected. [Laughter]

LIPPINCOTT: They thought this was just going to be a convenient way to secure Witten.

SCHWARZ: I wouldn't put it that bluntly. I think that would be overstating it; they knew she was good, otherwise they wouldn't have done it. But I'm saying that she's even better than that, both in terms of her research and her teaching. She's very serious about teaching the students. I think she's going to be a real asset for them.

LIPPINCOTT: And the institute itself, though, was just started last year, with the advent of Witten.

SCHWARZ: A year before they came, we started planning a joint center. The benefactors who were making possible this move toward Witten and Nappi also provided funds for setting up a joint center.

LIPPINCOTT: Now, who are they?

SCHWARZ: Well, I think you'd better get that information from someone else. [Laughter] Anyway, Caltech and USC are both contributing. And the Caltech benefactor, last I heard, didn't want to be publicly identified at this time.

LIPPINCOTT: OK.

SCHWARZ: And I don't know where the USC money is coming from.

LIPPINCOTT: But how's it going to work? Is Witten going to be here on this campus or is he going to be on the USC campus?

SCHWARZ: His appointment is a full-time Caltech appointment. Our entire group—meaning the people who are working on string theory—goes to USC once a week. At least, that's what we did last year. We would go down there every Wednesday, where they have a whole new block of offices for the center. So the Caltech-USC center is at USC, at least for the first few years. It may eventually move to Caltech. If it survives for four or five years and if we get a new building and so on, it may eventually come to Caltech. But at least for the first few years, it will be at USC. The first director is Itzhak Bars, who is there. So what we do is we go down there Wednesday morning. We have a seminar at 10:30 or 11:00, and then they provide a luncheon. After lunch we have another seminar and then we come back. They've provided offices for us and so on, so it's quite nice.

LIPPINCOTT: You say "our group." What precisely do you mean?

SCHWARZ: Anyone who wants to go. But the topics of our seminars for the first year, and probably for the second year, have been quite exclusively on string theory and string-theory-related topics. So it's basically faculty and students in this general area of research.

LIPPINCOTT: And it's not confined to string theory?

SCHWARZ: Well, so far that's what the center's been doing. Now, we envision that if it survives and thrives, eventually it will become broader than that. But the details have yet to be worked out on exactly how and when that would take place. [Tape ends]

Begin Tape 3, Side 2

LIPPINCOTT: Why was it thought that this institute would attract Witten from the Institute for Advanced Study, particularly?

SCHWARZ: Well, I don't know how big an attraction this joint center with USC is for him. I think it is one plus, because it brings more activity in string theory to the area. The funds that the center has are being used both to invite visitors and also to hire some postdocs. So there are additional string theory postdocs there as a result, and the general level of activity in the area is enhanced by that. I'm sure that Chiara's professional situation was also a factor in making this attractive to them.

LIPPINCOTT: Yes. Do you and Patricia see them socially?

SCHWARZ: Well, the Wittens have had social interactions at various levels with various people. I don't remember very many cases where it was just the four of us, but we've had parties at our house and they've had parties at their house and we've gone to dinner at restaurants with speakers and so on together. So we do have various sorts of interactions like that.

LIPPINCOTT: Do you think he likes Pasadena?

SCHWARZ: I don't know. [Laughter] I'm pretty sure Chiara does.

LIPPINCOTT: That's good. OK, back to string theory. The Large Hadron Collider at CERN is about to start up. Isn't that something that's important to you? It may find some physical confirmation of supersymmetry, which would be helpful.

SCHWARZ: Yes. They're about to start construction on the LHC. First, they're going to shut down the LEP [Large Electron Positron collider] in another half year or so, and then they'll start construction on the LHC. They hope to start running in 2005. It will probably take a couple of more years before they have any results. I don't think we're going to see any results from the LHC for another seven or eight years, so it's not exactly right around the corner. But we are excited about the possibilities, because they will have almost ten times as much energy as in previous accelerators, such as the Tevatron collider at Fermilab [Fermi National Accelerator Laboratory].

LIPPINCOTT: Yes. Well, I read in the paper that they just found the tau neutrino at Fermilab. That's encouraging, isn't it?

SCHWARZ: Well, it's nice. We knew the thing was there. It's not revolutionary. [Laughter]

LIPPINCOTT: Was this the first time it was seen?

SCHWARZ: Perhaps. It's not a big deal.

LIPPINCOTT: But the LHC may find very, very massive particles. Isn't that right?

SCHWARZ: Well, that's the thing. When you have more energy, you can produce particles that have more mass, because you're converting energy into mass. Some of the particles predicted by supersymmetry haven't been seen yet—or all of them haven't been seen yet. And we suspect that the reason may be that their masses are too high to have been produced with existing machines—at least, in enough numbers. We're not precisely sure what these masses should be, but we have a rough idea, and if our rough idea is correct, then they ought to appear at the LHC. And discovering these things would be very exciting.

LIPPINCOTT: That would be another pillar of string theory, wouldn't it?

SCHWARZ: Well, it would certainly be very encouraging for string theory. But more important, perhaps, than being encouraging, it would be informative. Because there are a lot of these particles, and they have very definite properties which could be studied experimentally. We now have a Standard Model of elementary particles, and what I think is likely to be coming in the future is a Supersymmetric Standard Model, which would supplant the existing one. There's already something that people talk about abstractly. It's the minimal Supersymmetric Standard Model, which is the simplest supersymmetric extension of what we know. But nature doesn't always choose the simplest option, and we want to know the right option, and the experiments could help to pin that down. And once that story is really sorted out, so that we have a Supersymmetric Standard Model as well confirmed as the current Standard Model, that would give us a much better target for what it is we need to explain with string theory. Without that hint, it's really hard to figure out how to make contact between the theory and the observations. It really helps to cheat and peek at the answer. [Laughter]

LIPPINCOTT: Do you think that is the future of the field? You gave a talk at the January conference here on the future of the field. Is that what you—

SCHWARZ: Well, my talk was entitled "The Future of String Theory." That was more aimed at an audience of theorists. I guess I probably did mention this briefly. I was thinking more about the future of the theoretical developments in string theory. But thinking more broadly about the future of particle physics, it's clear that experimentation plays a very important role, and it's important that the experiments continue to be pushed.

LIPPINCOTT: We didn't talk about any relationships you might have to experimental groups here or at CERN.

SCHWARZ: Well, I don't interact on a day-to-day basis with these people, because I don't have anything that concrete or useful to say to them. But if they make a major discovery, I'm going to be very interested to know what it is. [Laughter]

LIPPINCOTT: Yes. You're not going to go to CERN to watch them turn this LHC on, are you?

SCHWARZ: I don't know. I mean, that makes as much sense as watching a spacecraft being launched.

LIPPINCOTT: [Laughter] OK.

SCHWARZ: Maybe less, because that's more dramatic.

LIPPINCOTT: Yes. I think we're coming, more or less, to the end of this interview. But tell me about your life here at Caltech now. Do you get involved in any of the things like the Faculty Board?

SCHWARZ: I was on the Faculty Board for a few years. I don't know exactly, but maybe it was ten years ago or so.

LIPPINCOTT: Doesn't every professor have to serve a little time on one committee or another?

SCHWARZ: OK. The Faculty Board is elected. So I did that. I'm on the graduate admissions committee in physics. In fact, for four years I chaired the physics graduate admissions committee. I'm pleased to say that now I'm just a member of the committee and not the chair, which is not so bad. [Laughter] I'm also on the board of directors of the Women's Center. I'm very proud of that.

LIPPINCOTT: Oh, you are.

SCHWARZ: Yes. I don't know how I got chosen for this signal honor, but anyway.

LIPPINCOTT: What do you do for them?

SCHWARZ: Well, we have meetings about once a quarter or so, just to discuss if everything's operating OK.

LIPPINCOTT: Do you think the physics department is doing all right, by and large?

SCHWARZ: Well, there was a long period when we were making outstanding appointments of experimental physicists and theory was really getting neglected. So I think a few years ago you could say that Caltech really was not where it should be in theoretical physics. And now I'm pleased to say that we're addressing this in a very serious way. We've already addressed the problem in theoretical particle physics. Besides Witten, we have appointed several other people. So even if he doesn't stay, we'll be an excellent group. And our next major need is in condensed matter. Again, we're weak there in theoretical physics. That will be addressed next year. So I think this balance is being dealt with. We're getting there. And in a year or two, we should be up to snuff in theoretical physics, too.

LIPPINCOTT: Could you tell me who, besides Witten, is promising who has come lately?

SCHWARZ: Well, we hired three other string theorists, one of whom, Hiroshi Ooguri, is already here, and then there's a young fellow named Steve Gubser, who is coming in January.

LIPPINCOTT: Where's he coming from?

SCHWARZ: Well, Steve graduated from Princeton University a couple of years ago, then he was a junior fellow at Harvard, and now he's back at Princeton University, I think as an assistant professor. Even though he just got his PhD very recently, we've hired him as a full professor. He's a very bright young fellow who shows enormous promise.

LIPPINCOTT: That's two.

SCHWARZ: And then there's a third guy, Anton Kapustin, who graduated from Caltech [1997] and is now at the Institute for Advanced Study. And he'll be coming as an assistant professor, but only in a year from now. He had to defer for one year, because of his wife's studies in New Jersey.

LIPPINCOTT: He was a graduate of Caltech; do you mean he has a Caltech PhD?

SCHWARZ: That's right.

LIPPINCOTT: I heard somewhere that [Caltech President] David Baltimore was instrumental in getting Witten to come. Do you know anything about that?

SCHWARZ: Well, certainly this kind of an initiative requires support at the highest levels.

LIPPINCOTT: Does Baltimore consult you about things like this?

SCHWARZ: Not extensively. We've talked once or twice on the phone about it. I'm pretty sure that Baltimore and Witten did meet at least once and that that went well. It's clear that Baltimore is very supportive of this whole enterprise, otherwise it wouldn't be happening.

LIPPINCOTT: Yes. Well, it's nice, because I know he's got this biology initiative going. It's nice that he doesn't confine himself to biology.

SCHWARZ: Yes. But the decision to have a major initiative in biology was made before Baltimore was selected as president.

LIPPINCOTT: Is that right?

SCHWARZ: So given that that was under way, it made sense to hire a biologist, I think, to see it through to completion. [Laughter] But he's not narrowly focused on biology alone. As president, he has the welfare of the entire institute very much in view, and I think he's doing an excellent job.

LIPPINCOTT: Now, is there anything I haven't asked you that you wish I had—that you'd like to add or like to talk about?

SCHWARZ: There are a couple of awards I got. They're not terribly important.

LIPPINCOTT: Please, tell me about them.

SCHWARZ: They are things like the Dirac Medal and the National Academy.

LIPPINCOTT: I have this on the list here, and I'm sorry I didn't bring it up. I wanted to ask you about the Dirac Medal.

SCHWARZ: There's this International Center for Theoretical Physics, which is located in Trieste, Italy, where Abdus Salam was the director for many years. It's a center that's sort of focused on bringing Third World physicists in contact with physicists from the developed countries. Salam died a few years ago, and now the director is Miguel Virasoro, who in the late sixties and early seventies worked on string theory, and the ICTP has been renamed the Abdus Salam International Center for Theoretical Physics. In any case, the ICTP has been giving awards each year—I don't know starting when; starting perhaps in the mid-eighties or so—what they call the Dirac Medal.

LIPPINCOTT: Why did they name it for Dirac, particularly?

SCHWARZ: I think because he was an outstanding theoretical physicist. I guess it's as good a name as any. [Laughter] So Michael Green and I received that in 1989.

LIPPINCOTT: You went over to Trieste to get it?

SCHWARZ: Yes. They let you come any time that's convenient within the following twelve months or so. So perhaps a half year or so after it was announced, I went there, and they had a little ceremony where they presented this thing and I gave a short lecture. I remember when I went to do that. It was right when Italy was hosting the World Cup, so everyone in Italy was really focused on that.

LIPPINCOTT: They paid no attention to the ICTP.

SCHWARZ: So back in my hotel room that night, I flicked on the TV to see news about the World Cup, and there I saw myself, much to my surprise. [Laughter]

LIPPINCOTT: Well, that's good! OK, anything else you can think of?

SCHWARZ: Let's see. In '97, I was elected to the National Academy of Sciences, which was nice.

LIPPINCOTT: What do you do as far as the National Academy is concerned? Do you go there annually?

SCHWARZ: I went to the first annual meeting, which would have been in '98, where they had a little ceremony for the new members. That was kind of fun. I got to see how the meetings work. Eventually I'll go to another one, but I didn't go to the last two.

LIPPINCOTT: Yes. So you're not on any of their committees or joining in their—

SCHWARZ: Well, the national research councils, of course, have all sorts of committees to write reports on various important issues. Those committees are not comprised of members only. So whether or not one serves on those committees is sort of independent of whether you're a member of the National Academy. I haven't actually been on a committee, although I did review one of the reports that I was asked to review and comment on.

LIPPINCOTT: So you're pretty much enjoying your life at Caltech now that, in a way, you're out of the shadows, wouldn't you say?

SCHWARZ: Yes. But it's nice not to have too much attention on yourself; that can interfere with doing your work. [Laughter] I'm not seeking more publicity.

LIPPINCOTT: Well, in '85 you got a lot of that.

SCHWARZ: There was a fair amount then.

LIPPINCOTT: I think people from the *LA Times* came out to talk to you.

SCHWARZ: Yes. There are periods when there's a flurry of interest, and certainly around '85 there were a bunch of magazine articles and so forth where my work would have been mentioned. And then more recently—again, with some of the developments in the field—there's been more media interest in it. I've talked to a number of reporters. The *LA Times* last fall had an article about me.

LIPPINCOTT: Yes, and then they had an editorial in '88 that you'll have to talk about. You mentioned that Glashow said that string theorists shouldn't be paid. Didn't they say the same thing in the editorial in the *LA Times*? [Laughter]

SCHWARZ: Yes, that editorial certainly was based on conversations with Glashow.

LIPPINCOTT: OK. Anything else you'd like to add? I think I've covered everything on my own outline.

SCHWARZ: I can go on and on, but perhaps we've got enough.

LIPPINCOTT: OK. Well, thank you very much. [Tape is turned off]