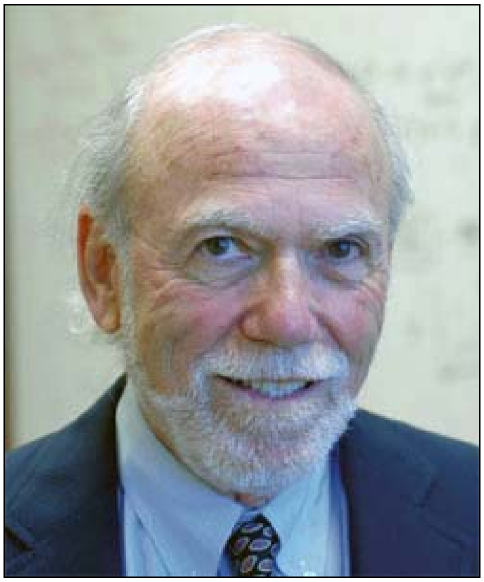

BARRY C. BARISH
(b. ca 1937)

INTERVIEWED BY
SHIRLEY K. COHEN

May – July 1998

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the LIGO Series Interviews

The interview of Barry C. Barish (1998) was originally done as part of a series of 15 oral histories conducted by the Caltech Archives between 1996 and 2000 on the beginnings of the Laser Interferometer Gravitational-Wave Observatory (LIGO). Many of those interviews have already been made available in print form with the designation "The LIGO Interviews: Series I." A second series of interviews was planned to begin after LIGO became operational (August 2002); however, current plans are to undertake Series II after the observatory's improved version, known as Advanced LIGO, begins operations, which is expected in 2014. Some of the LIGO Series I interviews (with the "Series I" designation dropped) have now been placed online within Caltech's digital repository, CODA. All Caltech interviews that cover LIGO, either exclusively or in part, will be indexed and keyworded for LIGO to enable online discovery.

The original LIGO partnership was formed between Caltech and MIT. It was from the start the largest and most costly scientific project ever undertaken by Caltech. Today it has expanded into an international endeavor with partners in Europe, Japan, India, and Australia. As of this writing, 760 scientists from 11 countries are participating in the LSC—the LIGO Scientific Collaboration.

Subject area

Physics, LIGO

Abstract

Interview in five sessions, May-July 1998, with Barry C. Barish, Linde Professor of Physics emeritus and director of LIGO [Laser Interferometer Gravitational-Wave Observatory] 1994-2005.

Recalls undergraduate education, Berkeley; graduate work on Lawrence Radiation Laboratory cyclotron; postdoc work on bevatron. Meets Alvin Tollestrup, comes to Caltech as postdoc, 1963. At Brookhaven National Laboratory. At Stanford Linear Accelerator Center with Henry Kendall, Richard Taylor, and Jerome Friedman. With Frank Sciulli, proposes neutrino experiment for Fermilab; work on tau leptons at SLAC. Move to Cornell.

Discusses history of magnetic monopoles and his work on monopoles at Caltech in 1980s. Discusses history of SSC [Superconducting Super Collider]; problems with Standard Model of Particle Physics; Aspen conferences to plan SSC; selection of Texas site. Involvement of Samuel C. C. Ting. Devises SSC experiment, with W. J. Willis. SSC's defeat in Congress (1993). Discusses his work in Italy on monopoles, in Gran Sasso tunnel. MACRO [Monopole Astrophysics Cosmic Ray Observatory] detector.

Discusses history of LIGO. Bar detector experiments of Joseph Weber. Initial meetings at Caltech. Hiring of Ronald W. P. Drever. Rochus E. (Robbie) Vogt as head, 1987. Disastrous technical review and project review, 1992-93. He takes project over from Vogt in February 1994. Discusses problems he encountered and lack of evolution between 1989 and 1994. Discusses LIGO's technical difficulties and evolution of its organizational structure. LIGO Laboratory and LIGO (construction) Project. Establishment of LIGO Scientific Collaboration.

Comments on Caltech; disinclination to serve on committees, enjoyment of teaching. Recollections of Richard Feynman. Influence of Tollestrup and Taylor.

Administrative information

Access

The interview is unrestricted.

Copyright

Preferred citation

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH BARRY C. BARISH

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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ORAL HISTORY PROJDCT

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Pasadena, California

Session 1	May 27, 1998
Session 2	June 10, 1998
Session 3	June 17, 1998
Session 4	July 16, 1998
Session 5	July 21, 1998

[N.B.: Because of a recording failure, the first half of the first interview had to be redone; thus the first two sessions are combined.]

Begin Tape 2, Side 1: June 10, 1998

COHEN: Good day. I'm glad to have you do this oral history for us. Maybe you could just tell us a little bit about your family background.

BARISH: I was born in the Midwest—Omaha, Nebraska. My parents were also born in the Midwest. Their parents both came from a part of Poland which is now Russia—the eastern part of Poland. They emigrated separately to the Midwest and met there; they came to Omaha, where they lived until I was about eight or nine years old. Just after World War II, we moved to Los Angeles.

COHEN: Now, had your parents had any college education?

BARISH: Neither parent went to college. After my mother died, I learned that she'd had a scholarship to the University of Nebraska, but—in kind of a tradition that females don't do things like that—her father prevented her from going. I think my parents got married very early because of that, actually—that is, she then ran away and they eloped. I don't know that exactly, but that's my thinking. She always said that she wasn't allowed to go to college, but until she died I never knew that she'd had this scholarship .

So neither of them went to college. My mother was very bright and very well read and so forth, but she never went to college. She always resented that. So my parents put a high value on education, even though neither of them went to college—my father because he got married so young. His father died, and he had to work while he was in high school to help support the family.

COHEN: Well, these were Depression years.

BARISH: Yes, they were Depression years. So he didn't go either, but I don't know that he resented it that much; it's just that that's the way life turned out for him. For her, it was a big blow.

Anyway, we moved to Los Angeles and lived in the Los Feliz area and I went to Los Angeles public schools.

COHEN: That would have been John Marshall High School?

BARISH: I went to John Marshall High School. I went to King Junior High School. And I went to Monroe Elementary School, which at that time went up through fifth or sixth grade—I can't remember exactly. Schools then didn't just move the kids along. They failed kids; kids didn't [automatically] get advanced. So I graduated from elementary school in Los Angeles with kids who entered the navy the next year. It was kind of peculiar: a mix of kids that was a little strange—very grown-up, tough kids.

Anyway, I was always interested in mathematics and science. As a kid, I didn't really know what that meant or what to do with it. I believe I was in the last class in the Los Angeles city schools to have a midyear graduating class. I had applied to Caltech—I thought I would go to Caltech, actually—but they didn't admit their students until March or April, so I went to Berkeley in the meantime and fell in love with it. I entered as an engineering student. By the time I did get admitted to Caltech, I was already—

COHEN: Ensconced at Berkeley?

BARISH: Yes, and so I had no interest in going. I went to Berkeley, and I lasted only one semester as an engineering student. I had thought that unless you were Einstein or something, you didn't do things like physics. It was engineering that you did. I didn't quite understand that you could make a career of physics. But as a freshman I took surveying and drafting and all those things that freshman engineering students took. I hated it, and I switched to physics anyway, which was what I liked, not knowing what that meant. Then it all just kind of fell into place.

COHEN: That's where you belonged.

BARISH: Yes, that's where I belonged. When I got my degree [1957], there was a policy at Berkeley that they didn't take their own students into graduate school. So again I applied, among other places, to Caltech. I was admitted to Caltech graduate school. [Laughter] But then Berkeley changed its mind and admitted a few students that year from the undergraduate school. I had, as a senior, worked on some research up at Lawrence Radiation Laboratory—

COHEN: So they already knew what your potential was.

BARISH: Yes, and I knew what I was interested in; there were all these accelerators there, and so forth. So when Berkeley changed its mind, I very much wanted to stay. I knew what I wanted to do. So I entered graduate school there. In graduate school, I had a kind of naïve notion. First, as an undergraduate I had taken a lot of graduate courses, so I quickly disposed of the legalities—candidacy exams and things like that. I was anxious to get into research. I also knew I wanted to do elementary-particle physics, which they did at the Lawrence Radiation Laboratory.

COHEN: Now, didn't you have to have a professor supervising you?

BARISH: Yes, and I didn't quite understand that whole thing very well. At that time, the most powerful and prestigious and influential professor at Berkeley was Luis Alvarez. He later [1968] won the Nobel Prize. He had a technique of taking the top students on the written candidacy exams and twisting their arms to join his research group, which most did. But I had an aversion to having my arm twisted, so I refused to do that. I decided I would just go off and do something that I thought I knew hadn't been done, and wanted to do myself, on the 184-inch cyclotron at Berkeley. So I went and did that, and quickly learned that graduate students can't survive in the power structure without a faculty advisor to mentor them, or to supervise, or to take care of the politics. So I went to the chairman of the department, who ended up signing my papers and so forth. His name was [A. C.] Helmholz. It was just for signing my cards, because, as I say, I didn't really want an advisor; I just knew that I needed somebody; I had learned that much. And then he didn't really run much of a research enterprise, so his group combined with Burton Moyer. Moyer was senior; he's since died. He was kind of a de-facto supervisor on my research—although all his work was on the bevatron and my work was on the cyclotron. But I was officially Helmholz's student. Moyer was comfortable with that.

COHEN: Now, this Lawrence Lab you're talking about is on the campus?

BARISH: It's up in the foothills right above the campus.

COHEN: OK.

BARISH: It has nothing to do with Livermore [Lawrence Livermore National Laboratory]. The Lawrence Laboratories were connected at that time, but they had some sort of a divorce; the basic science part in Berkeley was separated from the bomb research at Livermore. But at the time I was there, there was some connection between the two which I never understood. I still don't.

Anyway, I did my thesis on the 184-inch cyclotron. I finished fairly quickly [1962] and decided to stay for a year to do an experiment that Moyer's group was doing on the bevatron.

COHEN: So they offered you some kind of research position?

BARISH: Some sort of postdoc. I was at that time just getting married. I didn't know where I was going to go. My wife was still in school, and so forth and so on. So we stayed.

COHEN: Did you meet your wife at Berkeley?

BARISH: Yes, I met her in Berkeley just a year before that. We had just gotten married and I had just gotten my degree. There was no way to leave at that time, so I didn't even look for a job. And I had some skills that were useful on the bevatron, so I was working on that. And Alvin Tollestrup [then Caltech associate professor of physics] was at that time starting to develop a high-energy physics group here that was moving away from using the Caltech facilities, which were getting outdated.

COHEN: Here at Caltech?

BARISH: Yes. The high-energy group here at Caltech worked in the synchrotron building. There was Bob [Robert F.] Bacher and Matt [Matthew] Sands and Bob [Robert L.] Walker and Alvin Tollestrup, and a lot of students. Alvin Tollestrup was the first to have the vision that to pursue the leading edge of the field you had to go to bigger, national facilities. So he started developing a group to do that and somehow got my name.

COHEN: He had not been at Berkeley or anything? You didn't know him?

BARISH: I didn't know him at all. He showed up one evening when I was working on the floor of the bevatron. We started chatting, and he asked me if I would be interested in working on some experiment that did interest me. And that developed into my taking a trip down to Caltech and then coming as a postdoc in 1963.

So he was the one who hired me, and he was much more of a mentor than anyone had been at Berkeley. We worked initially on an experiment that they had in mind when I came—at Brookhaven National Laboratory. I eventually broke off into my own research effort, which was at the beginning of the Fermi National Laboratory, outside Chicago.

COHEN: So at Brookhaven you merely worked with the group project. Is that correct?

BARISH: Yes, on the project that had been proposed by Alvin Tollestrup and others; I was essentially the postdoc on that. Then I went from there to working on the very first experiment done at SLAC [Stanford Linear Accelerator Center]. I went there not with Alvin Tollestrup but with Jerry [Jerome] Pine [professor of physics], from here. We worked on very different parts of that [experiment], but I worked on one of the big spectrometers with Dick Taylor, Henry Kendall, and Jerry Friedman, and others. We did a whole variety of textbook measurements of high-energy electron scattering. At some point, I quit that, thinking there was no future in it—or that I didn't have a future in it. It looked to me like a bunch of detailed measurements. And instead I returned to do my own experiment at Brookhaven, which was a follow-up of the experiment that I had come here to do initially—except that Alvin Tollestrup and others from here didn't participate in this one; it was a different collaboration. So I left that experiment at SLAC about six months before [Kendall, Friedman, and Taylor] discovered the substructure, the quark structure, of protons. An unfortunate thing.

COHEN: They got the Nobel Prize [1990].

BARISH: They got the Nobel Prize for that, right. And Dick Taylor still likes to kid me about that, to this day.

Anyway, I had been interested in what is called the weak interaction. I saw an opportunity to pursue it in a way that would open different avenues—by using high-energy neutrinos in the new accelerator that was being built at Fermilab. I worked here with a younger guy named Frank Sciulli, and we developed a proposal, which was approved, and which turned out to be the very first experiment done at Fermilab. So I worked on neutrinos starting in 1970 or so, and I have been in and out of that business ever since, in a variety of ways.

COHEN: Now, it was decided at Caltech that anybody who wanted to do any kind of high-energy physics had to go a facility somewhere else? They couldn't do it here at Caltech?

BARISH: The facility here [the synchrotron] lasted about five years or so after I came, and there were people who stayed working on it. I think eventually the funding agencies encouraged them—let's put it that way—to turn off the facility here and use the national facilities. I don't think it took a lot of encouragement. In some universities they held on too long; here it didn't take a lot to let go. But the first wave of going off elsewhere was Alvin Tollestrup and people who were hired, like Jerry Pine and myself—we had no tie to the facility here.

So then I worked at Fermilab for some time, doing neutrino physics. We did a set of initial measurements which were very important in studying the quark structure of matter. Then I, like a good vagabond high-energy physicist, worked in a variety of places over the years: back at SLAC again on another experiment, at Brookhaven again, at Fermilab.

COHEN: How does that work? Can you decide on an experiment and then look to see what the best machine is to do it on, or where you can most easily get time? How does it work?

BARISH: It isn't like astronomy, where people just get telescope time. It's much more competitive, because it takes much more resources. There's usually a director of a national facility and a scientific program committee who look at experiments that would use the facilities to do something. These are not just proposals for time, like what's done at astronomical facilities, but more that somebody has a physics proposal and it takes a certain amount of resources, and there may be competition, and the science is judged, and all these things. It's a long process to get something approved.

COHEN: So is it just altruistic people who serve on these committees, to decide?

BARISH: Yes. I've served on them. Typically, most high-energy physicists are a little less mobile than I am. They tend to grow up within one laboratory and they're tied to that, both because they know how to do things there and they have a particular physics interest that at least is what's pursued in those facilities, while somehow I always think the grass is greener somewhere else, once I get into something and see that it's not as glamorous or as wonderful as—

COHEN: It's got to be better somewhere else. [Laughter]

BARISH: Right. So I've kind of bounced around a little more.

COHEN: And how does that work? Do you have to build some equipment that goes with you? Or do the people there set it up?

BARISH: It's often a combination. In particle accelerators, there's a beam of particles that has to be created. You need help from the engineers and technical people at the laboratory, so it's usually a collaboration that includes people from the laboratory—not always, but usually from the laboratory—along with people from the [visiting] university. Through the years, these collaborations have grown to the enormous size we know today. But during most of my career it was roughly five or ten people, eventually growing to twenty or thirty. And now it's as much as a thousand people on a huge experiment. It's an enormous enterprise.

COHEN: So you went to SLAC, and then—

BARISH: I went to SLAC. And at SLAC I became interested in a particular problem, again having to do with the weak interaction, which I've always been interested in. There was some sort of workshop that was sponsored by Marty [Martin L.] Perl—who got the Nobel Prize [1995] for the discovery of the tau lepton—on tau lepton physics. And he gave me an assignment, and I went and did it. It was kind of fun. I realized in doing it that the largest number of taus that were made were made at the facilities at Cornell, whereas the fewest results had come from Cornell, which puzzled me. So I gave a talk at SLAC, and in the talk I mentioned that there was a lot of potential at Cornell. And someone from Cornell who was at the talk invited me to give the same talk at Cornell. The talk at SLAC was in a workshop, so it was a half-hour talk, and at Cornell I would have to give a seminar that was an hour long. So when I came home I took the presumptuous attitude that I would sit down and actually figure out what could be done at Cornell, so I could see how they reacted.

COHEN: So you went there to tell them?

BARISH: I went there and gave a seminar on the science, kind of. And then I told them what I thought could be done there. It turned out that it was clear why they hadn't done this. I didn't understand why they hadn't, but it wasn't like I had some fantastic idea. They hadn't done it for two reasons: one was that the senior people there had grown up in the facilities there, which were oriented toward a different kind of physics problem, and they were focused on that physics problem, which structured the program there. The second reason was the way they had developed to pursue that physics program. You often have to get rid of backgrounds in experiments. There's a signal or something that you want to detect, and there are backgrounds. The way they went about getting rid of the backgrounds basically threw away all these tau leptons. So it wasn't that it was all in their data; they had been thrown away right from the beginning. So you had to go in and change how they got rid of the backgrounds—or throw away the other backgrounds but not the taus. We then made a little trial of how to do that, and it worked; eventually we developed quite a vigorous effort.

COHEN: So you were working at Cornell?

BARISH: So I worked at Cornell, in parallel with the magnetic monopole work [I was doing]. I had the two things going: I was also working at Cornell on developing this monopole thing, which had been a long-term interest. That was moving along very vigorously. And a junior faculty member here joined me—Alan Weinstein.

Begin Tape 1, Side 2: May 27, 1998

BARISH: One thing you learn early as a student is that there's this wonderful set of four equations that describes the whole subject—Maxwell's equations. And if you're a little bit aesthetic about it, you look at the four equations and they're completely symmetrical—something that particle physicists love—except for one difference. One of the equations has a little symbol called a *rho,* which is the single electric charge. And another equation, instead of having the *rho* or some other symbol, has a zero. And that zero is there because there's no single magnetic charge; it could have a little *m* or something for the single magnetic charge. If it had a little *m*, then all the equations would be totally symmetrical. And so—maybe on a little bit more scientific grounds but still a little bit because of aesthetics, the aesthetics of symmetry—there was some motivation for finding magnetic monopoles. That motivation served as, I would say, the scientific reason—other than curiosity; a bit more than curiosity—for why people even treat [magnetic monopoles] as something to look for.

The 1930s was actually the first time that the subject of magnetic monopoles became a scientific one. At that time, [British physicist P. A. M.] Dirac showed something that's still the only explanation—the real simple explanation, anyway. And that is that he could explain a property that isn't explainable otherwise. And that is that the electric charge of a particle is always an exact multiple of the electron charge, which is 1.6×10^{-19} coulombs. There's never 1.65, or 1.2, say. So [electric charge] is what we call quantized. And the only way he could explain the quantization of the electric charge—or the way he did explain it—was that there's also a magnetic charge. If there's a magnetic charge, then the whole thing fits together and we can understand why there's a quantization of electric charge.

That's a very strong argument and a very dramatic result. Most things in nature are continuous, and yet here we see that charge is quantized. It doesn't explain why the charge is what it is, but [it explains] the fact that there *is* a single quantized value for the charge. So from that time on—from the 1930s on—the search for magnetic monopoles was quite a popular subject for experimentalists, as new techniques developed for accelerators to look for magnetic monopoles. People would look for magnetic monopoles in any sort of collision in accelerators; they were looked for in cosmic rays and high-flying balloons; they were looked for in seashells and in the ocean. That's one of the very first experiments—maybe the only physics experiment—done with moon rocks when they were brought back.

COHEN: Really? Looking for monopoles?

BARISH: Looking for monopoles, yes. A few of these experiments actually produced headlines about the discovery of the monopole, but eventually they all proved to be incorrect. So it's a subject that I've followed from the time I started becoming a scientist.

Then I went to a summer school in Scotland. I'm trying to remember the date. You had mentioned the *Engineering and Science* article [May 1984]. I don't know what's in it; it would be interesting for me to see what I said.

COHEN: I think it's 1984.

BARISH: Well, this summer school was in 1980, maybe 1979, and it was in Scotland on the edge of a golf course—a famous golf course, Saint Andrews. And I was teaching at this summer school, which had a funny format. They had four lecturers—two theorists and two experimentalists, so each day there was a theory talk and an experimentalist talk. And then the next day you were off and the other lecturers talked. I happened to be paired with a theorist named [Gerard] ’t Hooft. And he was giving lectures on grand unification, the dream of all particle physicists, in which all the [fundamental] forces—electricity, gravity, the nuclear forces—are unified. Which was a very popular theoretical idea, and still is. Anyway, his lectures came first; I would come and sit through them, because I gave the lecture after him. I understood about five percent, or something, of what he said. I was half listening, and it was too hard anyway, and I would kind of polish my lecture, or something. This went on, and at some point in those lectures he talked about magnetic monopoles. It turns out that somewhere in the same time frame, 1978 maybe, he and, independently, a Russian named [A. M.] Polyakov showed mathematically that if there *is* grand unification, and if the mathematics of grand unification is the mathematics we generally use for non-Abelian gauge theories—which is a long and fancy term for the kind of way we form the fields—then there are fundamental topological defects in the theory. And when you look at these [defects] in detail, they have the properties of magnetic monopoles. So what these two physicists, Polyakov and ’t Hooft, showed independently is that magnetic monopoles are fundamental to grand unification—independent of whether we’ve found the right way to formulate grand unification—because they’re buried in the mathematics itself.

There were two other possible effects of grand unification that have caused a lot of experimentation: One is that baryons, or protons, may not be stable—that is, they’d have a lifetime of 10^{32} or 10^{54} years or something—and these huge water tank detectors were built [to investigate this]. The other is that neutrinos may have mass. So there were, and still are today, three different experimental effects of grand unification that one can kind of probe. One is that neutrinos may have mass, and people still look for that, including myself; proton decay; and magnetic monopoles. Anyway, that’s OK—it just provides more stimulation, like Dirac’s [insight]. But that isn’t what hooked me. What happened next is that [’t Hooft] mentioned that the mass of these monopoles would be related and near the unification mass—the place where all these forces, or interactions, are the same. Well, that turns out to be—we don’t know exactly—

but something like 10^{16} GeV. And that's a lot of zeros; it's a very, very high energy, way beyond anything accelerators can do now. And he just kind of [mentioned] that—or he did it mathematically, I don't remember. But it perked my interest. So at that point I started thinking—because I had followed monopoles since I was a kid—that this thing, if it does exist, is 10^{16} GeV, and all the normal particles we have are like 1 GeV. A proton is 1 GeV and an electron is much lighter than that. It occurred to me that all these searches that had been done for monopoles may have been insensitive to very, very heavy monopoles; maybe the reason that monopoles hadn't been seen was because they were so heavy the searches couldn't see them. And so rather than prepare my lectures for the following days, I began going through, one by one, all the different ways that people looked for monopoles.

For example, why would one look for monopoles on the surface of the moon—in moon rocks? Well, if a monopole is the mass of ordinary matter, like protons or electrons, then it can't penetrate very far; so if the moon has been bombarded by particles for a long time, the lunar surface would be a rich area of monopoles. But if the monopoles weigh 10^{16} GeV, the moon would be essentially transparent to them. They would have so much energy that they wouldn't just land on the surface, they'd go right through it! So moon rocks are useless. And you can eliminate all these [searches] one by one: accelerators couldn't possibly produce monopoles, because monopoles are much heavier than anything an accelerator could produce.

By the way, I'm not the only one who thinks this. But it became clear to me that summer that none of the searches addressed the question of grand unification monopoles. We had made a basic assumption in searches for monopoles, that they had masses something like the other particles—not 10^{16} greater. So then I took this up as a kind of a hobby. I had two young graduate students at Caltech, and I had them work on developmental projects that had to do with ways in which we might try to detect these very heavy monopoles. There were actually two Caltech theses [Charles E. Lane, "GUT Monopole Detection with Scintillator" (1988), and Gang Liu, "In Search of Slow-Moving Ionizing Massive Particles" (1988)] in the late eighties that came out of that early attempt—which wasn't the final way we approached the problem.

In 1982, there was yet another announcement of the discovery of a magnetic monopole, this time a grand unified monopole, by a guy at Stanford named Blas Cabrera—a very good physicist, actually. He, like me, had recognized that we needed a new technique to search for magnetic monopoles. He had a technique. He used a technique kind of like the one used on the moon rocks. It was actually Luis Alvarez who looked for monopoles from the moon rocks, and this was the same technique but with a twist of its being at low temperatures with some different kinds of detectors that could actually detect these very hard-to-detect monopoles. He had a little ring, like this, and looked for [monopoles] as rare particles in cosmic rays. I had come to a rather different conclusion, being more influenced by theory than he was, I guess. [I thought that] if they existed, they couldn't violate a couple of principles which it seemed to me were hard to handle: that is, if there were too many and they were so heavy, then there would be too much mass in the universe. A second problem is that we know that there are galactic magnetic fields, and if you run these monopoles, which are electromagnetic, through the fields, they tend to short out the fields. So if you have too many monopoles, there wouldn't be any fields. In both those cases, you can theoretically determine how many monopoles there can be before you run into trouble. So from the beginning, when I came back from Scotland, I had one premise—that you had to build a detector that was the size of a football field or else you violated these principles. So if they're in cosmic rays, they had to be very rare there. Cabrera didn't, I guess, think about that; he just wanted to do [the search in] a different way. So he had this little detector, and he thought he had detected one. It's a very nice technique. But he did the experiment better—ten times better—and he didn't see anything. So that was that.

Anyway, after he did his work, it got easier to get funding and so forth. There was attention to the field. During that same period, we had developed the technique we wanted to use. We decided that it should be deep underground—which made it a cheaper prospect—because these [heavy monopoles would] penetrate the earth so easily. We built an experiment, which was proposed in 1984—and it's in the *Engineering and Science* article in 1984? That's interesting.

COHEN: I'll run it off for you. There's a picture of the thing.

BARISH: Really? I'll show you a picture of the real thing. We proposed the experiment in 1984; it finally was approved and funded and so forth. We started building it about 1988, maybe, and we completed it seven years later. It's in Italy.

COHEN: Did you build it in Italy?

BARISH: We built a lot of it here, in the synchrotron building, and we shipped a lot of it across the sea. Now it's in the process of doing its big, long run. It will finish taking its data by the year 2000.

Then in about '91—or something like that; I may not have the dates right—the Super Collider was starting to develop its science program. And this thing was so highly political that I thought—even though the science was wonderful—I'd just kind of stay away from it. Taste-wise, I was very involved in the work I was doing on magnetic monopoles.

[Tape ends]

BARRY C. BARISH

SESSION 3

June 17, 1998

Begin Tape 3, Side 1

COHEN: I listened to your Watson lecture on the Superconducting Super Collider (SSC). Evidently, Gerry Neugebauer [Robert Andrews Millikan Professor of Physics] introduced you, and he said, "Let me just read one of his letters of recommendation: 'Here is a man that knows everything. I envy him.'" Now, I don't know who wrote that letter for you. [Laughter] But anyway, that was your introduction at your Watson lecture. So why don't you tell us a little about the history of the Super Collider and how you did get involved, finally.

BARISH: OK. Well, the Super Collider was to be the culmination of the main problems, I think, that are outstanding in elementary-particle physics. The general picture in elementary-particle physics is this: First, in the early 1960s and then the later 1960s and early 1970s, a theory developed, starting with Shelly [Sheldon L.] Glashow in 1960 or '61 and then [Steven] Weinberg and [Abdus] Salam in a little bit more of a phenomenological way in the late sixties. It's now called the Standard Model of Particle Physics, and it has the feature that it unified the electromagnetic and weak forces of nature. I would say that in the early seventies—in the early work at Fermilab and so forth—the model was very well confirmed. The fact that it predicted something called weak neutral current—something that I worked hard on—was one of the major steps. And then the second was the discovery of some of the particles that went along with this theory—charm particles, and so forth. That theory fell into place in the 1970s. It was magnificent, in the sense that it fulfilled the dream of physicists of bringing together the different forces and also because it worked so well and was guidance for a whole set of things.

So that was a very exciting time. In elementary-particle physics it's not an overstatement to say that we've been recovering ever since. Basically, what's happened since that time is the attempt to find out what the physics is beyond this model. And maybe the first break in that is the evidence that the neutrino has mass, which has come out only this last week or so—although it's been hinted at for a while. The two thrusts of the field since the late seventies have been to find out what there is beyond the Standard Model—because otherwise you're just free to think about anything—that is, what the constraints are. If you know the neutrino has mass, that gives you *n;* you know what it is; that gives you some constraints. You need something that constrains nature. We know that this model doesn't explain everything, so we know there's something else there and we need to figure out what that is—we need more theories. So it's been a terrible struggle for experimental physics.

The second problem is that in the Standard Model itself there is one fundamental flaw, or whatever you want to call it. That is that we describe the interactions of particles; we describe something about what particles we have, something about the forces. But one of the main features of the theory of particles is an ad-hoc idea we call symmetry-breaking. And that explains the fact that particles have mass. The basic theory has zero mass for particles, but real particles have mass. And some of them have a lot of mass—they are very, very heavy. So the question of where this mass comes from has been the outstanding problem in the field. The lack of experimental clues does not tell us "What is there beyond this theory?" But the origin of mass is the outstanding problem in the field. That problem was the principal motivation for the Super Collider. The parameters of the Super Collider, and the motivation for it, were to cover the bases—to be able to make sure, whatever the mechanism for the mass was, that the machine's range of ability was enough so that you wouldn't miss it.

COHEN: Let me ask you another question here. Where does this effort come from? Is it from people at the different places? Is it a groundswell? The idea is that you are going to a bigger machine, and where does the demand come from?

BARISH: It's a community, and the community interacts a lot. There are workshops and forums where ideas on how to move forward in the field are wrestled with. The Super Collider itself came out of a series of workshops that were held in Aspen and which I did not directly participate in—or in any of them that were in the late eighties. Actually there's a combination of ways these things start: First, inside the laboratories—they look at their own future. And so there are programs that develop new accelerator techniques or other new techniques. The theoretical community, of course, proposes what kinds of physics might be there, and that helps give the scientific motivation for which of these things might pay off. Some things, like the Super Collider, fall a little bit outside of that, because rather than being a new and improved accelerator at Cornell or SLAC or Fermilab, it was a new facility entirely. What happened in this case was that the community gelled around a need for a new accelerator that was a big enough step in energy to be able to completely cover the range of possibilities that we could foresee for explaining the problem of mass. I've oversimplified it some, but basically [that's it]. And this came about through some sort of studies done by panels that were put together by the Department of Energy, which funds much of the field.

COHEN: So this was a government effort also?

BARISH: Some of it is government-sponsored, by the high-energy physics part of the Department of Energy. And then there was a series of summer workshops in Aspen, where people would go off and talk about the future. In this case, it was not the future of physics but the future of facilities. And it gelled around a very large proton-collider facility, with parameters not completely known. As a result, the DOE funded a rather extensive engineering study program, which was conducted in Berkeley, to pursue the realities of building a high-energy collider. How much would it cost? Would it really work? What are the main problems? What's the engineering? What kind of site do you need? And so on. That was done in the mid- to late eighties, in Berkeley.

I participated only a little bit in that study; there was a fundamental debate over whether or not you could accomplish this with antiprotons hitting protons, instead of protons hitting protons. There's an economy in the former method, because antiprotons have the opposite sign of protons, so rather than having two different accelerators—one with proton A and one with proton B, which hit each other—you can use the same accelerator with particles going in opposite directions. So there's an economy in doing that, but, as you can imagine, antiprotons aren't as prevalent as protons. And so there was some debate on that issue, and it was felt that the two sides were both polarized and biased. So I was asked—because I wasn't involved—to pull together a group of people to look at that question. We basically decided that the saving wasn't worth it—that the problems of antiprotons were too limiting. That was the extent of my involvement.

Anyway, the formal design of the machine came out of this study in Berkeley. Unfortunately it wasn't carried quite far enough, because it really wasn't a design that was site-specific, and it wasn't a design that somebody then just went and did. So in the end when people talk about the escalating costs of the Super Collider, there's a little bit of apples and oranges. There's the problem that a group of people, who were not the ones who wanted to build it but who worked in Berkeley, were somewhat unrealistic and put forward a proposal that wasn't what the people who were actually responsible for building it and making it work tried to do.

COHEN: So the two groups really weren't talking to each other?

BARISH: The leadership of the group in Berkeley wasn't the leadership that was picked in Texas. One can argue that maybe if that [original leadership] had been kept on, they would have done something more consistent with what was done at Berkeley, rather than making the SSC a little larger and more expensive and so forth. But the main cost escalation was there almost from day one; that is, you brought a new group of people in and they rejected a lot of what had been proposed.

COHEN: So they essentially did their own plan.

BARISH: Yeah, they did their own plan, and it was somewhat more conservative and more costly. It wasn't one of these situations where something just keeps growing in cost; it was a hand-off problem. The new group said, "Well, if we're going to do this, it's too shaky. We have to do it better here and there," and so forth. So the cost almost immediately doubled. But doubling the cost when it's billions of dollars is not so easy. It doubled, and then it grew somewhat after that, and it became a political thing—well, it was political even before it became controversial. I stayed away at first, because I was enjoying what I was doing at Cornell and on monopoles. I preferred to do science rather than politics.

COHEN: You were already working with the Italians at the time?

BARISH: I was working with the Italians and I was working at Cornell—both. That was already more than I could do. Anyway, they developed the Super Collider and picked Waxahachie, Texas, as the site.

COHEN: That [site selection] was strictly political, wasn't it?

BARISH: It was political. The natural thing would have been to do it at Fermilab. The reason it wasn't done at Fermilab, in my view, is that in order to get national support, there had to be national competition for a site. If it started as a new facility at Fermilab, they would have had the support of the state of Illinois. Once you made it a national competition, it was very hard to select Illinois—although Illinois was a natural choice, because there was so much infrastructure and expertise and things like that already there; you could do it more economically than in a green field. Texas isn't exactly a green field, but it's whatever it is—a brand-new place where you have to build up your infrastructure and so forth.

Anyway, that was a political decision—partly political, not completely. There was a National Academy of Sciences group that evaluated all the site proposals from different states. But the final decision was made by politicians. And the Texas proposal was a very strong one. It wasn't purely a political decision; there were a lot of components of the selection that we wouldn't argue with, I think. My main argument would be that it should have been at Fermilab, but that really wasn't in the cards; I don't think they could have gotten a national consensus.

Then there were about four or five rather large groups that formed to propose the initial experiments. One group emerged as quite a strong group and more or less took the lead, in the sense of deciding what the design of an experiment should look like.

COHEN: Which group was that?

BARISH: It was called SDC [Solenoidal Detector Collaboration]. The leader was somebody from Berkeley named George Trilling, who graduated from Caltech [1955 PhD], by the way. That was developing quite well. It's important in science to have different approaches, especially on a hard problem—to have something that checks something else and validates it, and so forth. So in the plan for the SSC experimental program from the beginning, although it was debated, there was a plan for two experiments. The proposals from the other three or four groups for the second detector were all less than adequate. One of them was put forward by Sam [Samuel C. C.] Ting, who is a Nobel Prizewinner [1976] from MIT, and who's somewhat controversial. It became a bitter competition, kind of a war between all these experiments, and basically everybody annihilated everybody else, in some sense.

It's a long, sordid history. When it all kind of fell apart, eventually everything was off the table except Sam Ting's experiment. He managed to defeat all the others, but he was unacceptable to the laboratory management. So it all collapsed, and there was no second experiment. At that point, as a precursor to what happened in LIGO [Laser Interferometer Gravitational-Wave Observatory], I got approached by the SSC laboratory management and senior advisors to pull together a new, fresh effort for the second experiment. And I eventually took on this challenge with a physicist named Bill Willis, from Columbia University—with whom I'd never worked before, by the way. The two of us created a group from the remnants of the groups that hadn't been approved, from some new groups, and from our own idea of how to actually do an experiment, which was different from any of the others. He and I worked extremely well together. He's a very, very bright guy. He had worked at CERN [the European Organization for Nuclear Research] in Geneva most of his life, so I knew of him, always respected him, but I had never worked with him. And this [project], as you can imagine, was highly visible. It was a little like being in a fishbowl. We were picking something up from the ashes, and there was a lot of controversy and a lot of power struggles going on.

COHEN: At this time, there was no clue that the SSC was going to get done in? I mean, it was perceived well?

BARISH: While the Super Collider itself was shaky, the approval in Congress at that time still had a reasonable majority. It was after the election in '92, when we changed to a Republican majority, that things switched. We started this back in '91. We continued to do a design. It worked quite well; I won't go into details about it. We had a huge experiment—it was 1,000 people from something like eighty-nine or a hundred institutions in twenty or thirty countries. We went completely through what's called the conceptual design. We designed an experiment, we simulated it, we saw how we could do the science. Meanwhile, the first year that the new Congress came in, they voted against both the space station and the Super Collider. That was the first time it looked like there might be real problems. There was a lot of controversy. The space station was in worse trouble than the Super Collider that first year. The Super Collider was reinstated by the Senate. So the House voted against it and the Senate reinstated it and then it kind of came together and the funding continued. During the year in between that—we felt we couldn't judge the politics, so we just put our heads down and moved on with the science part. And we finished the design of this detector and produced a 1,000-page book containing a technical design, cost, and a detailed analysis of the physics capability.

COHEN: Let me interrupt you there. So there was no going to Washington to talk to congressmen and things like that?

BARISH: Oh, there was. Oh yes, of course there was. There was a lot of community support for it, and people went to Congress. I'll mention the difference between LIGO and this in Congress in a minute.

COHEN: OK.

BARISH: So there was a fair amount of going to Congress. There was every expectation that it would be difficult but it would be OK. I think in the end—well, first, a lot of mistakes were made. I won't say who I think is to blame, but a lot of mistakes were made. The second year, the space station did much better than the Super Collider; they had a larger political base. But they also did more to satisfy the congressional objections than the Super Collider proponents did. I think that was a management difficulty—

COHEN: The space station was probably more sexy, too, in some way.

BARISH: It was more sexy, and so forth. But I think we could have done better—the scientists and the people who managed it, and the DOE, and so forth. But the final point is this: One of the things the SSC was predicated on was a fairly large foreign contribution, and the largest of the foreign contributions—or participation—was supposed to be from Japan. The Japanese, as far as any of us could tell, were ready to sign up. But basically Clinton never stepped forward and did what he should have done. It's interesting, because he just gave a commencement speech at MIT—just this last week or so. He mentioned the recent discovery of the neutrino mass, and in that speech he also said that maybe we should reconsider the decision not to go ahead with the Super Collider. And in fact, it really was that the executive branch was passive. Even though I didn't support him, [President George H. W.] Bush was quite strongly favorable and helpful in Congress to the Super Collider. So the change of administration helped to do it in. Anyway, I wasn't involved in the [politics] of the Super Collider.

COHEN: You were just involved with this one experiment, which was going to be the second experiment?

BARISH: Yes. Every week I would go to Texas. I'd leave on Tuesday morning, after I taught my class on Monday. I'd go to Texas until Thursday or Friday. [This lasted] for about two or three years—whatever it was. My family moved to Santa Monica, close to the airport, at that time.

COHEN: Oh, was that the reason? [Laughter]

BARISH: That made it kind of nice; we moved while I was commuting so much. And then at the end of 1993, in October or so, the Super Collider went under.

COHEN: Congress said they were not going to fund it?

BARISH: Yes. And then they had to go through all the business of turning it off. They had to spend millions of dollars to decommission the work they had done. That wasn't necessary for our stuff; we just disbanded, because ours was still a paper project, on the detectors that were going to be built.

COHEN: Why did you have to go to Texas to work on it all the time?

BARISH: Because so many institutions were involved. It was built at the [SSC] laboratory itself, at the facilities near Dallas. We had a central engineering group. Since I was in charge, or in co-charge, that was our central place. The other experiment was less centered at the Super Collider than we were, because it was run by a group from the Lawrence Berkeley Lab, which had a fair amount of infrastructure, which we didn't have. We basically had to develop our infrastructure in Texas. Anyway, it died.

COHEN: Were you in shock at this point? How did you feel about it?

BARISH: My wife says it's denial—that's the word she uses. First, I was very angry at certain people—people I thought didn't do things very well. Not just at Congress—that's a vague way to be angry. I was angry at the guy who was the director [Roy Schwitters], who I think did badly, and at the DOE, and so forth. I felt I had spent a lot of energy and effort on something that wasn't going to come to fruition. On the other hand, the exercise stopped at a natural place, because we had gone through the intellectual exercise of designing what you would do on one of these things. I knew what to build. We had solved all those problems.

COHEN: So you did the intellectual part?

BARISH: The first intellectual part. Then there's all the hard work of building it. Then there's the hard work of making it work. And then there's doing the real science, which is of course the best part. But it was a long way from that. So it wasn't as if we had started to build a lot of stuff. If it had to die, that was kind of a natural time.

COHEN: So there was some satisfaction, at least?

BARISH: A little bit. I had enjoyed working on it. And, as I say, I worked very well with Bill Willis.

COHEN: Had you let all your other stuff go at that point?

BARISH: Alan Weinstein basically took over the Cornell work. In Italy there was no one to do that; I had been carrying that out with whatever free time I could find during the whole period of the Super Collider. After it went down, I quickly went back into both the Cornell work and the work in Italy. I was quite happy to go back to the research I had been doing before.

COHEN: I would like to backtrack a little. I'd like to ask you something about the people you worked with in Italy. Is there anything else you want to say about the SSC?

BARISH: No, I don't think so.

COHEN: OK. I'd like to ask you a little bit about your Italian colleagues. How did you get connected with them? Because I don't think you've talked about that at all.

BARISH: OK. I don't remember exactly what I said, so it may be a little bit disjointed.

COHEN: Well, you were just suddenly working there.

BARISH: I think I mentioned that I became interested in this [monopole] problem a long, long time ago, and then I was at a summer school in Scotland. Then I came home and—kind of as a hobby, with some students—tried to develop some techniques for [detection], always taking seriously one piece of astrophysical information, which is that if there were magnetic monopoles, they would have had some effect on whether there were magnetic fields in the galaxies. If there were too many magnetic monopoles, they would have shorted out those magnetic fields. But we have seen magnetic fields in galaxies; other people had [shown] how [the motions of] stars form electric currents that form these fields. You can actually go through an exercise and come up with a limit on how many monopoles there can be. If you do that, you find that there can't be very many monopoles around or they would screw up these fields. What that meant was that they were exceedingly rare, and therefore you had to build [a detector] roughly the size of a football field in order to look for them. Other people didn't take that part of it as seriously, but that was the chief thing we looked at. In 1982, Blas Cabrera, an experimentalist from Stanford, who didn't really take that part of it as seriously, had used a very nice technique—but it couldn't be used on something the size of a football field—and he thought he saw a monopole. He did what a good physicist should do. It's not that he announced that he had actually discovered a monopole and then others showed that he was wrong. He announced that he had evidence that there *could* be a monopole—and then he did his experiment better, and he didn't see it. So he did a very nice job of following up on his own experiment. Maybe the first one should have been better, but he never knew what was wrong with it.

COHEN: He was honest.

BARISH: He was honest, and he's a very clever guy. But his technique wouldn't work over a big area. So I started working on other techniques, initially with the idea of doing it here. The little article you found in *Engineering and Science* was about that period, when we were developing our technique here. It was based on the magnetic properties of a monopole. That technique may have worked, but eventually I realized that if certain tests worked out, we could detect magnetic monopoles by more conventional techniques. And I was more driven by the problem of detecting magnetic monopoles with something big enough—or trying to—than a clever technique. So even though I was very vested in this technique—one student here did his thesis on developing it—we dropped it.

I then formed a group of Americans that had some interest in developing a magnetic monopole detector. I initiated several working meetings in which we developed these ideas and did some tests that looked very promising. We determined you could do this experiment with normal techniques, but you had to be deep underground. Once we saw that you had to be deep underground, we then looked for sites. There were possibilities in this country: the salt mine near Cleveland, which was being used for an experiment on proton decay, and other places. But it turned out that the Italians at that time were developing a very large underground facility. They were motivated by a different physics problem and also by some social situation—it was a make-work project. They were developing a large amount of space underground, kind of like a laboratory without an accelerator, in the Gran Sasso tunnel. It was being developed by Nino [Antonino] Zichichi; he managed to get highway construction funds to build this underground facility. So when we started surveying where we might do the project, we became aware of the fact that they were building a very large underground space to do science in Italy.... It's a funny thing about Italy, they have a lot of tunnels. I don't understand why.

COHEN: It's because the roads are built straight; if something's in the way, you dig a tunnel.

BARISH: I guess so. The economics must work differently. Because we build mountain roads and they build big tunnels. And this was a huge tunnel project.

COHEN: It's the Roman legacy.

BARISH: Right! Anyway, as this lab was being developed, they were entertaining different ideas to do science in it, and there was a group of Italians also interested in pursuing the magnetic monopole problem. I went to Rome, representing our group, and met them. I knew one of them vaguely, but I didn't really know the group very well. Its leader was Enzo Iarocci; he has recently been made the president of the INFN [Instituto Nazionale Fysica Nucleare], which is the funding agency for elementary-particle physics and nuclear physics and so forth in Italy. There was a lot of suspiciousness between the two groups, and some differences in how people thought one should approach the problem, and so forth. The fortunate thing was that although I hadn't known Iarocci previously, I got along quite well with him.

COHEN: So they welcomed the Americans coming in?

BARISH: He did. The rest of it we were able to smooth out, because there was compatible leadership on both sides and we were able to form a friendship. We're very friendly now. We were able to pull the two groups together, so that in the end the Italians came over here and we jointly wrote a proposal at Caltech, in '84 or '85, something like that. Basically, the MACRO [Monopole Astrophysics Cosmic Ray Observatory] detector is what we've ended up building together. They built part of it and we built part of it. But it's worked quite well as a collaboration. Iarocci is no longer a part of the group; he's moved on to become more of an administrator in Italian science. But as I say, we're very friendly. Another Italian is now in charge, and I work less well with him. We wouldn't have pulled this off together, so it's fortunate that it was originally somebody else. And we expect to run this project for another two or three years. At the end of this week, I'm going to Italy.

COHEN: So that's the Italian project.

BARISH: That's the Italian project.

COHEN: Maybe we should leave off there. We have yet to talk about LIGO, which we can do next time.

BARISH: OK. **[Tape ends]**

BARRY C. BARISH

SESSION 4

July 16, 1998

Begin Tape 4, Side 1

COHEN: Good morning, Professor Barish. Welcome back. I think this morning we're going to talk about LIGO. According to my notes here, you were involved in the original committee in 1976 when Kip Thorne [professor of theoretical physics] first proposed to the institute that there be such an endeavor here at Caltech. Do you remember that committee?

BARISH: Oh yes, I remember it. I was involved partly because I've always had an intellectual interest in this field, even though I was in a different field, and some people knew that. I assume that's why I was involved. From my point of view, the only interesting aspect of that process was that the Caltech physics department is more or less the same size as it was thirty years ago—within a few people. And when something is a constant size, it's an interesting process how you go about making a change. So with that as a background, you have to ask how a group of people in a department actually institutes change—brings in a new area—when all the forces are to continue in the areas that people are already working in. That process went quite smoothly in this case, I think. It's interesting to me not because we did such a great job but [because] it shows a kind of flexibility in the division [of Physics, Mathematics, and Astronomy], or in the professors, to have taken on something new with at least some thought that it might affect them somewhere else. There wasn't a conscious decision that there would be fewer appointments in nuclear physics or particle physics or whatever—but there would have to be some effect somewhere. I'm sure some people are optimistic and just think that if you take on something new, it's great and you're going to grow. But I wouldn't think that's generally the case. Since then, we've taken on one other new field, which is condensed-matter physics—which actually has led to more professorial appointments. To me, both these developments are interesting, because they represent changes in the direction of the physics department over twenty or thirty years. Otherwise all these faculty meetings are concerned just with promoting people and hiring somebody new and so forth—not quite as interesting as this process.

So the process itself, as I remember, really did focus on the fundamental value of this physics and on whether it was the kind of thing a place like Caltech should do, and how it would fit into the balance of the other activities that were going on. I remember it as quite interesting, and even slightly exciting.

COHEN: Was there any feeling about the ultimate cost of the project, or didn't that come up?

BARISH: No, no. I don't think we knew anything at that time. I think we were blind to that. Basically, there had been the previous bar-detector experiments of Joseph Weber at the University of Maryland, which were wrong, or presumably wrong. Moving into this area, it wasn't even completely clear at that time. It looked like we could use interferometers to do a more sensitive job [of detecting gravitational waves]. And it was really taking on the research activity of developing that technique.

COHEN: So you were really starting at ground zero, in some sense.

BARISH: Yes, and I don't think there was any concept of the magnitude of the project. It was more an issue of the commitment of members of the faculty to a new area, in a small faculty that picks and chooses and doesn't try to cover all areas.

COHEN: And it would have been Kip Thorne who presented this project to you?

BARISH: Kip was certainly the prime mover. The interesting questions were the ones I just mentioned. The process was actually not very controversial; it was fairly straightforward. I may be wrong—maybe Kip remembers it differently—but I don't remember it as being highly controversial in the committee or in the department afterward. So for me it was kind of an exciting thing to see how a group of people takes on something new, knowing that it will affect them.

COHEN: In those initial meetings, was there some decision as to how many professors would be appointed in this area?

BARISH: I think we knew that it would involve more than one appointment—that it was going to be a new direction, and that if we did it we wanted to be on top, whatever that means, or in the forefront. I think it was understood to be a major commitment—I don't mean financial, but a major commitment of the faculty to do this. And that's what I remember as the main issue. I also remember the discussions as being quite high-level—that is, about intellectual matters, not political matters. And I was delighted that we could decide to do something without deciding that you'd lose your slot—that this guy's going to retire and he won't be replaced, or something like that.

COHEN: Well, very good. Then after those initial meetings, you had very little to do with it. Is that correct?

BARISH: After those initial meetings, I had nothing whatsoever to do with it, except occasionally interactions with [professor of physics] Ron [Ronald W. P.] Drever, who had some small amount of technical overlapping interest with some of us in high-energy physics at times. So there was a little bit of interaction with Ron, but not really very much.

COHEN: Did you have anything to do with Ron's appointment?

BARISH: I wasn't on the [search] committee. If I did, I don't remember it at all.

COHEN: Do you remember anything about the chance of appointing two professors, and Ron absolutely refusing to countenance this? That is, I have some record of perhaps [MIT physics professor] Rai [Rainer] Weiss coming if he had been asked.

BARISH: Maybe that was all determined before it surfaced for me. I thought it was more of a choice between the two. Those were the two names that were brought up—I remember that much. There were two things that I remember—and maybe wrongly. One was that somehow Ron was regarded as very clever, maybe more clever, and he had done an experiment to disprove Weber, which Rai hadn't done. And secondly, the commitment to him was more retractable or something, because he came [1977] under some arrangement, which I don't know in detail, in which he was still shared with Glasgow. So it seemed to me that they could cut it off if he didn't work out, which wouldn't have been true if we had hired Rai from MIT.

COHEN: I see. So you don't remember it as Ron vetoing the hiring of another senior person?

BARISH: If he did that, it wasn't anything I knew about.

So anyway, Ron started coming [as a visiting associate], on some sort of shared basis with Glasgow. And the [LIGO] effort developed here, but I was fairly distant from it, except for the fact that he overlapped with us a little bit; there was a little technical interchange.

I don't remember how I was involved, but I was involved—or maybe I was just in the division—when Robbie [Rochus Vogt, R. Stanton Avery Distinguished Service Professor and professor of physics] was brought in [as director of LIGO, in 1987]. I knew of the problems that developed between MIT and Caltech; I guess you couldn't miss it. There still wasn't a project; it was at the level, at that point, of asking for significant R&D funding from the NSF [National Science Foundation]. And a review committee was formed which included a lot of prominent physicists. They demanded that the two groups get together and that there be some coordination. First, there was this business of having a troika, with Kip involved. Then eventually—I don't remember this; I'm sure you have better records from Kip or somebody—there was a second phase. I think they determined that that didn't work, and they had to bring in a project leader to go through this five-year R&D program. And that's when Robbie was brought in.

COHEN: Brought in as director.

BARISH: Yes. That must have been about 1987. And I still didn't actively have anything to do with the project. I was just aware of these things—and probably inaccurately.

COHEN: So you don't have any sense of the fact that all this money was going to be spent on this project?

BARISH: No. When Robbie came in, that was the first time I became aware that it was going to be reasonably expensive. But I don't even think at that time, as a high-energy physicist used to big projects, that it really would have appalled me at all, actually. I didn't really know the size yet. They had spent maybe $4 million or $5 million over a few years for this R&D thing, which seemed large. But when Robbie came in and they began the engineering study, I don't think there were numbers on the table for what it would cost for a project—until Robbie worked on it. So I think the first I knew of—whatever it was—the $212 million or $192 million was in 1989 or about then, when they turned the proposal over to the NSF. And again, even at that point—from '89 to '92, when it was going through the approval process—I wasn't very close to it. I was busy with the SSC and so forth; LIGO really wasn't on my horizon. The next time I became at all connected was after problems developed and they formed—too late—this oversight committee, which was chaired by [former JPL director] Lew Allen.

COHEN: Was that after Robbie had the blow-up with NSF? Or was that blow-up a result of this?

BARISH: Well, there were a series of blow-ups. No. The emotional things are what people key on, but I don't think they are the real issues. LIGO went through two different reviews, which were disasters. I think the best understanding of the project itself is to read the results of the two reviews. There was a technical review and a project review. The project review was by Ed Temple, of DOE, and Ned [Edwin L.] Goldwasser, who used to be deputy director of Fermilab. And there was the technical review, which was done by a whole bunch of reviewers looking at the project. And both those reviews were far harsher and worse than any review I'd ever seen.

COHEN: It's interesting: Kip didn't say anything [laughter] about those reviews.

BARISH: I'm very aware of them, for two reasons. One is that the first I saw of the thing, when I was on the oversight committee, was the technical review. What some people remember about that technical review has nothing to do with what I think was the important thing. [H. Jeff] Kimble [William L. Valentine Professor and professor of physics] got up and made some sort of criticism about the project and the review committee, which some people felt was out of place. It probably was out of place.

COHEN: Now, was this actually a criticism of the science, or a criticism of the way people were doing it? What was the criticism? Was it of the basic ideas?

BARISH: I remember it as being more emotional than having specific content—that they didn't hire good enough people and they weren't attacking the right problems and it wasn't going to work. Kimble can be quite emotional. And I think his criticism was out of place; it belonged a little more within the family. But that really wasn't the important thing. The main issue, in my mind, was that on the whole broad front of technical reviews, they did poorly. I've seen a lot of reviews from the DOE and from high-energy physics projects; there are often problems, but they are not as universal as what I saw.

The technical review was in the summer of '92, and then in the spring of '93 was the project review. The project review looked at how they were organizing to do it, whether the cost made sense, whether they could actually build the thing, and the technical strengths—all the things you look at that are separate from whether they had designed it correctly to reach their goals. And that, too, was a terrible disaster, I would say. I knew about those [reviews], partly from Ed Temple and Ned Goldwasser, who were in my field and who ran the project review. And the technical review I saw—parts of it, anyway— when I was on the oversight committee.

So I think my view of this is a little different from the view of others. I think the project was in very big trouble technically, independent of the personality problems that people focus on. [Those reviews] had a big effect on me, because eventually, when they came to me to [take over LIGO], I was very reluctant. I took a very hard look at it, because I didn't want to get into something rotten that couldn't be made to work. I thought that romantically it was nice; LIGO was a great project, in that sense. But things were in place, and you had to live with most of what was planned.

I had no direct or indirect connection, really, with the Drever thing, except that I was on a small committee put together by Gerry Neugebauer [then PMA division chairman] to try to arbitrate that situation. But we didn't look at the project's [technical] difficulties, we just kind of listened to these guys and arbitrated. The committee consisted of [professor of physics] Tom Phillips, myself, and somebody else; we ended up with some sort of arbitrated thing, and it never amounted to much, except it did define Drever's role in the project—even today, actually. But we didn't go into the project itself.

COHEN: The technicalities of the project.

BARISH: Yes. It was really kind of colleagues trying to solve a problem within the physics department rather than it's being done from the outside, and this was Gerry's thing. I didn't really learn a lot about LIGO, I just learned about the personality problems. And then, during the problems with the NSF, the only connection I had at all [with LIGO] was when Robbie Vogt asked to see the project management plan—this was after they had had trouble with their own project review—that we had formed for the GEM [for Gammas, Electrons, and Muons: the special abilities of the detector] experiment for the Super Collider. Since we had been project-management reviewed, and so forth. I gave him that, and I had discussions with him. He was critical of its bureaucratic, formal [approach]—you can imagine.

COHEN: Yes.

BARISH: So I gave it to him, and then I wished I hadn't, because I got a lecture about how this was an awful way to do things [laughter], without him ever having read the damn thing. Anyway, that was my only connection until I got a phone call. And I didn't know that the project would blow up; I don't hang around in the Athenaeum. I don't know these things very well.

COHEN: Well, you're either in Japan or Italy. [Laughter]

BARISH: So the next I heard was—well, there were grumbles and problems, but I'm used to projects having people problems. That's often—

COHEN: That's how it goes.

BARISH: Yes. This was a little bit different, because it was almost a small family. There were a lot of those kinds of problems at SLAC and at Fermilab, but what usually happens is that they are solved by the director—say, by putting somebody off in left field to work on something different. But you can't quite do that on a campus, and certainly not with faculty. So I don't think these problems were really any worse. It's just that it's hard—

COHEN: They were confined.

BARISH: They're so confined, yeah; it's like you need a divorce but that's not possible.

COHEN: Well, I think that was the entertainment on the campus for two years.

BARISH: Yes. So anyway, then I got a phone call from [professor of physics] Charlie Peck—

COHEN: Who was by then the—

BARISH: Division chairman. At home, on the weekend. I would guess it was February 1994. I could be off by a month, but approximately February 1994. My situation at that point was that in the fall of '93 the Super Collider had been voted down in Congress. After licking my wounds and helping a lot—I mean, I was very lucky, because I had a real faculty job, but my colleagues who had jobs at the Super Collider were in trouble, and I spent a lot of effort on placing people and so forth. But by winter I was pretty much out of that, except for the tail-end things, and I was back to working—quite contentedly, I think—in Italy and at Cornell. I wasn't actually looking for anything. In fact, I was quite happy to be doing more research. I had been kind of soured by the SSC experience, as you can imagine—although, as I say, I felt lucky that I had done something intellectually; I didn't hurt myself, and so forth. Anyway, then Charlie called me—I didn't have any sense of this before—and said that it was about something very sensitive and he didn't want to talk to me on the phone, and could he come out and visit and walk on the beach with me. Well, this is not like Charlie Peck. [Laughter] Although I had known and worked with him for years, even in Pasadena I didn't get him over to my house. So this was strange. I knew it was strange, but I didn't quite make any sense of it. So this was on a Saturday, and he came over on Sunday and laid out the situation and said they had discussed having me take over [LIGO]. He didn't tell me a lot about Robbie's [dismissal], except that it was definitive and over. So it wasn't as though they would have to remove him to put me in; that part—at least as presented to me—was a done deal. And the reason I reflected on it was that I was, first, really ignorant and, second, had an impression that [LIGO] was not in good shape.

COHEN: From seeing these reports that you had seen.

BARISH: Yes. And yet I was torn, because I felt some considerable loyalty to Caltech, and the need, and my availability—and also an intellectual pull. I had always liked the subject. So I was kind of torn within myself about whether I was just afraid to take something on and was making excuses or the thing really was no good. Then there were time constraints. If I had had six months to decide, I'd go and learn the subject and look at it very carefully, and so forth—but I had to decide quickly. I told them I needed a month to decide. I worked really hard for a month and learned far too little. I learned very little because there was so little documented and the people involved were incredibly closed.

COHEN: They didn't want to talk?

BARISH: Well, it was not so much not wanting to talk with me. It was that the whole environment was not an open environment. They gave me a 1989 proposal to read. Well, this was 1994. And to show me what they understood about the current costs, they gave me the 1989 thing. Everything else was in people's private file cabinets. Robbie—for, I think, human and natural reasons—was quite defensive. He was willing to talk and spent a huge amount of time with me, but I didn't learn a lot [from him]. I'd get lectured, but there wasn't any content. So I had a really hard time assessing the whole thing—whether it was technically sound, whether the costs were anything like what they were, whether the people who were doing it were any good. And I knew, from talking to the NSF, that we couldn't stumble again—that this was it. I had to, if I moved in, somehow do it in a way that was going to succeed. And I didn't know whether I was overestimating or underestimating the problems, or whether the fact that I couldn't learn things meant nothing. I just had a lot of trouble.

COHEN: Was that with the MIT people also?

BARISH: I visited MIT. But when the problems got to be serious, one of the consequences was that the relationship between Caltech and MIT totally soured. MIT was pretty well cut out and bitter. And I didn't know those people very well. They were new to me, and it's easier to talk to a colleague you know and know how to read, and so forth. So that was not very useful, either. I may be overstating it, but it was a hard problem. I mean, I spent a month and I dug in and I tried to understand things. I kept flipping from thinking that these were problems that could be fixed to thinking they couldn't be. Eventually—kind of at the wire—after a month I decided to take LIGO on, but not because I had convinced myself that it could succeed or that it was in good shape underneath. It was more the opposite—that I couldn't convince myself that I couldn't make it work; I couldn't convince myself that it was rotten; I couldn't find the—

COHEN: You just didn't know.

BARISH: I didn't know. I thought, when I was starting the month, that I was going to decide on the basis that I would do it if I could see that it could be done. But I didn't decide on that basis. I decided to do it because I couldn't prove to myself that it could *not* be done. And that's a rather different thing. In the end, a lot of the reasons I couldn't understand and couldn't decide, I think, had a real basis that still exists. And I don't think very many people other than myself understand that. I could decide the superficial things quickly: how much money it would take to build the big buildings and the vacuum system, all the things that people look at now and say, "Oh, LIGO's wonderful!" Those things I never worried about. I could assess all that quite quickly; that's not the real issue.

COHEN: What is the real issue?

BARISH: The real issue is still coming. So that's why we—

COHEN: You mean, Will LIGO work?

BARISH: Well, the real problem is that, first, it's a very hard project under any circumstances. It has to be done in a way that it—I mean, in my mind it was absolutely doomed to failure in the mode it was in. I'm going to slightly overstate the case, but the reason I couldn't learn anything other than by reading a proposal from 1989 is that nothing had *happened* between 1989 and 1994. LIGO stood still. I'll give you some examples, and this is not what even Kip knows, because I don't tell people this. The project stood still. This project can't stand still, because the problem is that nobody really knows how to build the technology to detect gravitational waves. The technology has to evolve. The whole idea is to build something that can evolve afterwards.

So if you look at what LIGO looked like in 1994, and it was absolutely identical in every manner to 1989, something's wrong.

Something was terribly lost. And that is five years of evolution, of improving whatever was on the table in 1989. And [that time] was lost and it can't be regained entirely. I can build buildings, I can do a lot of things. But I can't fix that. Some of the things could be fixed, but with a lot of pain. Others can't. If there was a real flaw, it's that somehow they managed to pull things together enough to turn this proposal, which is reasonably sound, in to the NSF with the 1989 design, which a lot of people could have said isn't good enough to detect gravitational waves—and it's not. And then, for a combination of reasons which are not totally the personality reasons, the project [lacked the] intellectual environment to move on. And it didn't move on. It just worked on politics—on getting the big money.

COHEN: Did they think the design was so good that they didn't have to [improve on] it? Or were they just intellectually unable to proceed?

BARISH: I think it's a very complicated issue. But the heart of it is that in most experiments you design up to a certain point—you decide what you're going to do, you freeze the design, and you build it. LIGO can't be done that way. And it's even been stated—in all the statements made by Robbie or Kip or anybody else—that this project is going to evolve. But what happened is that they didn't institute what you needed to do to let it evolve. Somehow, also, that wouldn't have mattered as much, maybe, if they had started right away. But it took five years. It took three years to get the first approval from the NSF, and then congressional approval in 1992. And then two years later they still didn't have a penny to build it. Not one penny had been released to construct LIGO when I came in, and that's because they had failed these two reviews I told you about. Not because of Drever, but because they had failed the technical review and hadn't satisfied the reviewers. And they failed the management review and were supposed to turn in a management plan. It was when those two things still weren't converging that the NSF said that they couldn't go on. Basically the project hadn't moved forward. I'll give you just a couple examples of what was lost in the five years. It's slightly technical, even though this isn't a technical interview.

COHEN: That's OK.

BARISH: In 1989 the most obvious laser to be used in an interferometer was an argon-ion laser—a gas laser. And that's what was used. It's still being used on the 40-meter [prototype] interferometer. That's a gas laser. It's like radios when they had vacuum tubes. The big revolution, which has happened in the last decade or so, is the practical development of solid-state lasers—just like we went to transistors for radios. And these are everywhere; they're in CD players! We have solid-state lasers everywhere. Interestingly, if you look at every other gravitational-wave project in the world—the Japanese, the German and Scottish, the Italian and French—they're all using solid-state lasers. LIGO was still using argon-ion lasers, because they couldn't grapple with the problem. It was so obvious to me that it was crazy in the long run to stick with that. You had to change sometime, because, like using little vacuum tubes, they drift too much; they don't have the power, and there's no way to look to the long-term future when you go to a higher power. It's not a simple matter of later just putting solid-state lasers in, because lasers have a particular frequency. And the mirrors and all the guts of this thing are coated and made to reflect a specific frequency. So if you replace the lasers with a different kind of laser, you have to replace all the optics, too. That's extremely expensive.

COHEN: Do you think they just didn't want to look at the problem?

BARISH: I think the project environment was so closed that at first they felt attacked and defensive. It was so closed that basically they were just intellectually not moving ahead. The environment was closed completely from any outside input, because Robbie put this shield around it and there was never any information that went out, beyond the 1989 proposal. And there was no internal debate, either. It was just a terrible intellectual atmosphere. I don't think it was any one person deviously saying, "This is my design, and nobody can do better," or anything. It was just not scientific—

COHEN: Or conducive to growth.

BARISH: Conducive to what you needed for this particular project. The example I've given you is of the laser, which to me was so obvious right away, even though I had never had anything to do with this subject. Eventually, when I came in, I worried about all the people who had gone on with such long-term dedication and interest in this, and such suspicions of somebody coming in, so that I took a while before I forced them to change the lasers and the process and so forth. But we changed the lasers. That we fixed. The next problem I'll tell you about is something that we haven't completely fixed; it's a legacy of the five years of standing still. It is, I think, actually the hardest technical problem in LIGO, other than all the precision work you have to do. It's the control system. The problem is that we are trying to measure two lengths and compare them. There are these two arms, and we send light down them and compare the lengths. That's what an interferometer is. In the laboratory, the interferometer is made to work by making the mirrors on the two [arms] as steady as you can, so that they don't shake on a table—you bolt them down, and so forth. We can't do that in LIGO, because what LIGO is trying to do is measure the fact that some distance changes when a gravitational wave comes through. So when a distance changes, the mirrors are supposed to be free, so that they can move. The way they are made free is to hang them from wires. So you have these things that normally—in the laboratory—are bolted down, and here they are hanging from wires. You want them to be like pendulums so they can move around. To mitigate the movements and lack of stability of the mirrors due to seismic effects, tides, and so forth, what you have is a whole bunch of very sensitive sensors—this is the hardest problem in LIGO—which measure the extent to which a mirror is starting to tilt, and you move the mirror with little magnets to keep it aligned—so that stabilizes tilt. There are about fifteen or twenty of these movements that have to be monitored and corrected, so that any remaining motion is due to gravitational waves.

The problem with this technique, the reason it's very difficult—I mean, you can do it all in principle; you correct these effects at different frequency than what LIGO works at for detection of gravitational waves. All that's understood. The subtleties of it were understood by the people who worked in LIGO. But the difficult problem arises because of LIGO's geometry: the [laser] beams go down these two vacuum tubes at right angles to each other and then come back—the mirror or test masses that must be controlled are what we call highly coupled. That is, if I take a mirror and I fix the tilt, it affects the distance between it and another mirror.

This is the same kind of problem I encountered back when I worked on the cyclotron. There was always the guy who was the “magic operator.” If you really wanted to make the machine work well, there was Joe Blow who knew how to do it. What he did is the following: There were a bunch of knobs on the control panel, and this guy knew that when he twisted knob 8, it affected knob 1. In his mind, he knew that they were coupled somehow. So if he twisted a knob to tune the beam, he knew he had to also do something to another knob. And that was something he couldn’t quite write down—and therefore another operator wouldn’t do it. But somebody who was experienced would know to do it.

That’s exactly the problem that exists in LIGO, except that in LIGO’s case it’s multidimensional. There are fifteen or twenty knobs. So you can’t [just depend on] somebody who has this creativity to know how to do it. You somehow have to design, from the beginning, a way to ensure that when you twist knob 3, you do the right things for knobs 4, 5, 6, 7, 8, 9, and 10—because they all affect one another. And how they affect one another isn’t completely calculable, though some of it is. The fact that [everything is] coupled makes it very hard.

The second thing is that you can’t introduce any noise into the system when you do this, because you are trying to do an extremely sensitive experiment. But you have all these magnets and electronics that are controlling the mirrors, and you can’t introduce any noise. So the electronics has to be very quiet, which is very demanding. And the electronics is also what we call nonlinear—the circuits are nonlinear. So the way the electronics was designed for LIGO—and still is, to a large extent, on the 40-meter—was that they had hired one electronics engineer, who grew up in the old days at Hughes Aircraft and who did these kinds of servo-control groups for radar. He was a crotchety old guy who had everything in his little file cabinet, and he would tell you how he was the only one who could build this and that, and so forth. He designed a lot of the electronics in the 40-meter. And that’s all fine, except it’s the wrong electronics. They got a very good, old-time guy who had done controls, and he built electronics like a good, old-time guy would, using what you would use in the seventies, and maybe in the eighties, but not in the nineties. And the problem is that, in order to do this right, you need to have something more intelligent than this guy who did the cyclotron with two knobs. You needed to involve computers, so that they can know if you—

COHEN: I was going to ask you, Where are the computers?

BARISH: In the electronics built by this guy, it's all what we call analog electronics. That means it's all just built into the circuits. What had developed enough by the nineties was enough advancement in ability to do digital circuits, which means you can control it with computers. You can design the same kinds of circuitry in digital circuits—at least in large parts of the system; in some parts of it you still can't—and you can replace the analog stuff with digital stuff. Then you can control the computers and learn how to adjust all these servos right. Otherwise, you've essentially decided up front what all these couplings are, and how it all behaves—and that just doesn't work. You can't do that. So in my mind, the main reason they have so much trouble making the 40-meter work reliably is exactly this problem: the temperature changes a little bit, and it changes what you have to do on too many knobs for anybody to handle.

COHEN: There's not enough control.

BARISH: You have to do it in the digital way. So what we've done, and what I've done, is to reimpose, as much as we can, digital circuitry. However, you can't do that everywhere after the fact—that's the trouble. It should have been designed [that way] from the ground up.

COHEN: During those five years you spoke of, this should have been going on.

BARISH: And then it would have been an integrated design—a design that made sense. What we've done is a patchwork, which doesn't make sense totally. And we're going to suffer. I don't know that it won't work, but there's going to be a lot of pain. And a lot of that pain is, in my mind, a legacy of five years of standing still and then trying to make the changes now.

So those are just two examples. There are more. These are matters I don't generally talk to anybody about, because they're just too sensitive. But if you ask me where the problems were for me in LIGO, as I say, it wasn't in building the buildings or building anything else. The design itself is fundamentally very difficult; it has to be flexible. You have to design something in which you can make changes. There are a lot of legacies from the five years of standing still—technical legacies that are in front of us. I've always felt this was the most serious problem. I thought I could build this thing, and let people take pictures of it, and say it was on cost, on schedule, and all that—which is what they're saying.

COHEN: That's not why they wanted you, I don't think.

BARISH: People look at LIGO now and say it's a great success, but that doesn't mean anything. What I did, out of nothing but a sense of what I could get away with, was to set a promised date for when LIGO would work, at the design we're doing now. That would be two and a half years after we finished the construction, not the day after, when we turned on the light switch. If we make it work sooner, great! I doubt if two and a half years is enough, because there is so much—

COHEN: Catch-up?

BARISH: Catch-up on the patchwork things, which are going to take time. In principle, you could turn it on. It should do some work, if it's designed right. People have made accelerators which are just as complicated, and turned them on very, very quickly. But those machines were designed and integrated well. This one isn't. It's not a design that I'm really proud of, at this point.

So I think the story's just beginning. And I think we're going to suffer a lot, but we haven't entered into that phase yet. That phase starts next year, in the middle of '99. But I think we're going to suffer a lot over the coming three years or so. **[Tape ends]**

Begin Tape 4, Side 2

BARISH: So my feeling is that the challenging and the interesting time, and the complicated time, is yet to come. It starts about a year from now. And then we say that it's going to work in 2002. Anywhere within shouting distance, I'll be happy with. Most of my energy for the last three years has been spent on trying to do the things that will make this coming two years or so happen and work. And I'm still scared of it—very much scared.

COHEN: Have you hired a lot of new people? Or are you continuing with the same group?

BARISH: There are very few people left who were here when I came in. There was a terrible problem when I came in. Because of all the difficulties that had happened, the group was very battered psychologically; there was a fortress [mentality]. They were very loyal to Robbie. New people who were brought in were not treated well. It was a very sick situation. I didn't have the luxury, even if it were allowed at Caltech, to make a clean sweep and start over again, which would have been the best thing you could do, actually—just get rid of a lot of people who made nothing but trouble. The problem is that so little was documented [after] this 1989 report that there was no corporate memory, or whatever you want to call it, existing.

COHEN: No, I know how those things work. I know very well how those things work.

BARISH: It was an issue of job security. The important documentation was in individuals' file cabinets and people's heads, and until that technical information got out, you couldn't move on. So I had no choice; I had to try to keep people. I assumed that people would leave, and my problem was to hold them long enough to extract all the [information] that we needed in order to move on. [Meanwhile,] I hired a whole bunch of new people.

COHEN: You must have told Charlie Peck that you would need certain things if you were to take over this job. I mean, didn't you come in with conditions? One would assume that, with a job like this, you would have asked for certain things.

BARISH: Well, not so much from Caltech, more from the NSF. What I did was to re-validate the project. I asked for this timescale that gave us two and a half years to turn on [after the construction was completed]. And I asked for a four-year time period to build it, starting from that day, rather than what Robbie had already done—nothing. I also re-costed the project. I asked for about $15 million extra, to bring in some strong people to strengthen the staff during construction, and maybe a few other things. But [I dealt] mostly with the NSF; I didn't really ask Caltech for anything.

COHEN: So what ultimately happened, Barry? I mean, what did you do with all these people who didn't want to leave?

BARISH: Well, at the different levels, we did get out most of the information that was useful. But if I ask myself what my biggest mistake has been in LIGO, it's letting some basically sour, rotten-apple people who no longer could work constructively continue and be destructive forces for too long in LIGO. I now have a sense of why corporate managers make a clean sweep and get rid of people. There was a lot that I saw happen—I just can't believe that human nature is that way. But most of that is over, at this stage.

The people who were there felt they owned LIGO. And I brought in people who were higher level than they were, or who were paid better, or this or that. Partly because the people [who were already there] really weren't the right people. But nevertheless, you can imagine that from their point of view they were being shunted aside. And yet I didn't fire them. And yet they didn't leave, and so they became destructive forces. And there had been this myth that only certain people who had been around LIGO forever could make it work, because there was so much magic involved. You have to be careful when people start talking about magic being involved: People can't work on it if they can't really learn how to do this, [and they can't learn] unless they've been around for seven years, and nobody new can come in and learn it. Like the twenty knobs that you can't make work if you don't know everything about it. That means, basically, that the thing is fundamentally unsound.

So that was the environment—and now that environment is pretty much gone. Some very good people who were in the original group are still part of LIGO now, like [detector leader] Stan [Stanley E.] Whitcomb.

COHEN: Bob Spero, also, whom I should talk to, has been in the project from the beginning, is that correct?

BARISH: He's gone.

COHEN: Oh, he's gone?

BARISH: Bob Spero was not a constructive force at all. Bob Spero doesn't know how to be—

COHEN: I don't know him. For some reason he's on my list.

BARISH: He's at JPL, and probably quite bitter. He couldn't adapt to the new order, if you will.

COHEN: And I understand Robbie Vogt has no place in this now. He did for a while, but that's over.

BARISH: Robbie Vogt—and it's a failure of mine, I guess—Robbie, probably from the beginning, should have been cleanly removed, in retrospect. I made an enormous effort to try to find a new role [in LIGO] for him. It didn't work, for a lot of reasons. I think mostly that he was—I would describe it, kind of oversimplistically, as like a wounded animal. He had been through so much that he had these sores, so if there was anything that seemed like a slight criticism, he'd overreact. It was just untenable to have him in a position where you relied on him. He would go crazy, kind of. So that didn't work. Then I tried to put him in a position that was more free-floating, where he could do whatever was of value but we didn't have to count on him to run people, where he [could] mistreat them, and so forth. And that didn't work either, but it took two or three years for him to finally decide on his own that he should go. I consider it a failure on my part, but maybe it was inevitable. I think it's hard for anybody who has been in a position of power, directing something, to stay on in a different position. That's hard for anybody, but if you take somebody with Robbie's personality and ego it really was impossible. I get along very well with the guy personally. We were always able to talk. I mean, he drives me crazy; he talks too much and so forth. But I get along with him well. We have long, long talks. I met with him every week, for a time. I tried to change things, but in the end I had to put the project first and him second. So he was a casualty, and I didn't really find a role for him.

COHEN: Now, you never had a problem with Ron Drever, because he was already gone when you came in?

BARISH: Well, he saw this as an opportunity to somehow reemerge. But I have a reasonable, if somewhat distant, relationship with him, and I think it's constructive. I think he is, to some extent, a broken man. He has gotten a fair amount of money from the institute. He's doing some work. But it's pretty peripheral.

COHEN: He complained bitterly about not being allowed to go to meetings when I interviewed him.

BARISH: Well, he can't go to certain meetings—working meetings—but LIGO itself is completely open now. All our papers are open. All our seminars are open. There's an outside community that's involved. Formerly, there was none of this. But if I'm sitting and designing a circuit and there are five people, you can't have just anybody randomly come in and worry about your process—

COHEN: So this is what he was complaining about?

BARISH: I don't know—I don't know when you talked to him. But he basically wants to be able to participate in any meeting. From his point of view, he knows how to do everything. But that's disruptive. This isn't picking on him—no one else sits in on every meeting. The people who are in small meetings are the ones who are working, who have the job. So from his point of view, I suppose we are closed.

COHEN: But he goes to some meetings?

BARISH: Yes. Basically he's not a problem. Again, in Robbie's case it's a tragedy; the guy worked for five years, or whatever, and he's out on the street. He's bitter. He feels he's gotten no credit. Everything's bad about that, as far as I can see, despite all the effort. In Ron's case, I think the tragedy is a little bit different. That is, if you look at LIGO, there are a lot of innovations and so forth that came originally from ideas of Ron's. And they're in there, so he's made his mark. And he should and will be part of any positive science that comes out. That was the agreement this little committee made. So he's supposed to be part of the collaboration, and he is. But he's not contributing—and not only to LIGO. If you look at the stuff in his lab, it's political. It's suspending masses with magnets and stuff, but it's to walk people through and show them things—it's not really serious. I don't know, maybe he's a little older now, but I just don't think he has recovered from that.

COHEN: Well, he may have come to the end of what he can do. I mean, that happens too.

BARISH: Yes. I don't know.

COHEN: Well, actually, I know you have to go. But I would like to talk to you again, because there are some— **[Tape ends]**

BARRY C. BARISH

SESSION 5

July 21, 1998

Begin Tape 5, Side 1

COHEN: Good afternoon, Barry. I'm glad to see you again. We certainly spoke about the workings of LIGO and how the construction is going, but I understand you've done a good bit of organizing of how the science will be run. Could you talk about that a little?

BARISH: Yes, I'll make a few comments. They are more visionary than in place, but I'll tell you what we're doing from two aspects; one is LIGO itself and the other is the broader international community.

LIGO grew out of a part of physics that didn't have very much experimentation at all. So there was no community to speak of that existed before the fact. It mainly grew out of the general-relativity community, which Kip Thorne's in. For the most part, that's been a theoretical subject, so we're at the beginning of an experimental field. Even if a project is large, usually there are a lot of people to draw on; in this case, that wasn't true. In the meantime, though, efforts in this general area have grown for twenty years or so. And they've become highly visible, of course, with LIGO.

So the first issue is what kind of people are in LIGO and how you create a scientific community in LIGO itself—first in LIGO itself and then I'm going to go broader yet. In LIGO itself, we have a group of people—right now, I think there are 135 people—who are paid directly by LIGO, not by some company that we have contracted to do something. They're basically paid for by LIGO.

COHEN: These people would be Caltech employees then, or MIT?

BARISH: Caltech, or possibly MIT, but they may be living at the sites [Livingston, Louisiana, and Hanford, Washington]. Right now there are 135 people, of whom, I would say, more than 100 are technical, meaning they're either engineers, scientists, or technicians. Some are contractors—people with special talents that we need. So what we've done is build a large team with all the expertise we need. They're doing a job—a very interesting job—of building something. They have a partial but not complete overlap with the group of people who will make this thing work, or exploit the science of it. So there is going to be a transition: first, from the group of people who build it to the group of people who are involved in the experiments. Then it becomes a research facility and not a [construction] project.

That's a funny kind of transition, because you don't really do just one thing or the other. I had mentioned that when I first took over LIGO I didn't feel there was much latitude, and I organized the construction project in a kind of nonimaginative way, like you would build a bridge. That is, a hierarchical structure: Each person reports to somebody above and has a certain very well-defined responsibility and maybe a team of people who work for him or her, and a budget to go with it, and the task of delivering something on a certain schedule, and so forth. Well, that's not very much like how a research laboratory operates. So at the same time we grappled with how that set-up should evolve into a research laboratory. I decided that these two stages were so opposite to each other that we would create the research laboratory environment in parallel with the construction. But some people who will continue with the project will have a role in the LIGO Laboratory, which is what we call [the research side]. So we have one entity called the LIGO Project and we have another entity we call the LIGO Laboratory. The LIGO Laboratory functions like any research facility.

COHEN: Now, some of the same people are in both groups?

BARISH: Some of the people are in both groups. Some of the people may end up in the second group, but they're slowly evolving into this second group if they have some time that isn't devoted to building the facility. One example is that we have a fair number of computer-oriented people who are building up the facilities to do science with—data analysis tools, and so forth. So that's a group of people in this Laboratory who aren't involved in the construction project. And then there are others who are migrating to the Laboratory.

COHEN: Now, these people are physically here and not at the building sites?

BARISH: No, some are at the sites. And then there are the people who will run the laboratories there—not just build them but run them. And so we have, at each laboratory, a physicist who's in charge and some scientists who will not only work on the facility but do research. So we're building this up in parallel. Right now the LIGO Laboratory is small compared to the construction project, mostly because the construction project is so large. We're spending, if you want to measure it by dollars, an average of more than a million a week on the construction project—somewhere between $1 million and $2 million a week.

COHEN: That's impressive when you think that it's a university project. [Laughter]

BARISH: And of course it isn't that every week. One week we may spend $20 million, when it's a big thing. But we're spending $80 million this year. So the Laboratory part seems small compared to that. The budget for what I call the Laboratory is almost $8 million this year, so it's only one-tenth of the whole thing—but it's $8 million! It's a big enterprise, and it's a reasonable piece of what we'll eventually have. Because the eventual Laboratory, when the construction ends, will be large by Caltech standards—somewhere like $20 million to $25 million a year. That runs both sites and also the Caltech and the MIT research.

COHEN: And these laboratories will be at these sites?

BARISH: Well, we call it all one Laboratory, but it includes the work done at Caltech and MIT—which is a big piece of it. All the data analysis is here [at Caltech], the administration is here, and a lot of the science is here. We have a strong effort on R&D and science at MIT. And then at the two sites. So that whole thing will run at a fair budget, but it's already a third as big as that. So we already have this Laboratory set-up, and it basically runs by the same rules, or guidelines—neither of those words are quite right—but the same style that we're used to on the campus in any sort of research set-up.

COHEN: So these people sort of are proposing the experiment that they will eventually do? Or already creating software?

BARISH: LIGO isn't lots and lots of different experiments. Some are—various experiments in data analysis. But [the Laboratory people] are doing work that will lead to research papers. As you say, they propose things. The environment is more to do research and learn things rather than to produce a product.

COHEN: Who are these people, Barry? I mean, are they staff people? Are they tenure-track people?

BARISH: Eventually, if we have a $20- to $25-million Laboratory—which will be fully staffed by the year 2000, more or less, as some people roll off of the construction project—we'll have about 120 people, of which there are more or less 20 at each of the two sites, 20 at MIT, and 60 here.

COHEN: Is that why you're taking over Millikan Library?

BARISH: Yes. [Laughter] The sixth floor of the library is where we're going to concentrate totally on data analysis. It will be the heart of the data analysis of LIGO. So there are roughly 60 people on campus—half the total of 120. But of the 120, close to 100 will be technical in one form or another. Half of those will be physicists, at all levels—postdocs, faculty, students, and so forth. So about 50 physicists, 50 technical people, and 20 to make the thing function.

COHEN: Are you going to have more professorial appointments there?

BARISH: Yes. Now, we're still talking about inside Caltech and MIT. MIT has been actively searching for either one or two new faculty members. They actually made an offer this year, which was turned down. At Caltech, making a junior faculty appointment is part of the long-range plan in physics. The Caltech administration agrees with the plan that we bring in somebody young who is fully on board and has his research program. When LIGO becomes a research facility—which is [scheduled for] 2002—it may be that that faculty candidate will emerge from our younger people or postdoctoral people who have excelled in LIGO. Or maybe it will be somebody we haven't found yet.

COHEN: So that's still evolving.

BARISH: That's evolving. It's actually in the long-range plan in physics. And I think if we identified somebody tomorrow, we could make a serious proposal. I think it's the number-one priority in the physics long-range plan. So both faculties [Caltech and MIT] will grow somewhat. And [LIGO] will involve more faculty, students, and postdocs, and fewer engineers, purchasing people, and so forth, than we have now. I think, except for size, it will be a lot like other research programs here.

So there are [at present] about fifty scientists and graduate students, inside Caltech-MIT or at the two sites, who are part of this LIGO Laboratory. And they are also part of something called the LIGO Scientific Collaboration, which we organized a year ago; it now includes a whole bunch of other universities, including Stanford, Oregon, Louisiana State University, Louisiana Tech, Michigan, Penn State, Florida, and so forth. Some are people who had been wanting to be [involved with LIGO] for a long time; some are complete newcomers; some are people that I recruited. It's a fairly large group. The LSC has a total of something approaching twenty institutions and a couple of hundred scientists, counting graduate students and so forth, committed to have a scientific program involving LIGO in one form or another. Some are very technical. Some do more data analysis. Some do development of new techniques. We have collaborators from Australia, Scotland, Germany, Russia, and maybe a few others I can't remember right now. So it's an international community that's not just interested but has made a commitment to do something; all these people actually have a defined program.

We'll make an arrangement whereby each group that joins says what they're going to do. We revisit the group every six months and see what they think they've done and what they really did. And this collaboration has a kind of democratic organization—a way to elect officers and so forth and so on. The real idea or spirit of it is that inside LIGO itself there should be what I would call equal scientific opportunity, whether you're at Caltech, MIT, or a collaborating university. Naturally, there's a big advantage in being a Caltech or MIT person; the other people are tagged on. But you can't get good people to tag on unless they really have the same opportunity. We want good people. There's always at least a perceived advantage for the insider, so we've tried to create a separate organization, with its own set of rules and so forth, that isn't run by Caltech and MIT. And that's this collaboration, which has a way to elect officers and so on.

COHEN: How often do you meet?

BARISH: Twice a year. Every six months, we reassess what each group is doing. The next meeting is in Colorado, at a collaborating institution, next month. So that's the collaboration—it's starting to work OK. It helps coordinate the proposals from the various groups to the NSF and writes a separate white paper. It's a little hard to do, when you have a group of people with different priorities. But the idea is to give the NSF some guidance—what's important and what isn't important and so forth.

The LSC is growing, and I think it's on the right track. I wouldn't say it's a wonderful collaboration yet, but I think it's going to be very, very important in the longer term. Right now most of the work is being carried by Caltech and MIT; the others are trying to find their way. When it will become important is when we start thinking of real improvements to the device, which are going to be crucial, even understanding some of the subtleties—when somebody who then doesn't have a well-defined task can go and look at hard problems. And also in trying to do the science, where having more strength and new ideas is going to be very important.

So [the collaboration] is really a very good thing. We started with LIGO being a very closed shop—I talked about that earlier—so this is a very, very big departure. The first departure was to open it up intellectually to other people who wanted to see what we do; now our work is available, and the writing, and the papers, and so forth. But the LSC is [a further step]. You mentioned Drever before; he's involved as one of the members of the scientific collaboration, so he'll do his science. And even Kip Thorne is a member of the data analysis group—he's part of the group in this collaboration.

We're trying to make LIGO intellectually viable and a place where people can do science that's possible and broadly accessible. But there are other people in the world who have nothing to do with LIGO but who have, in parallel, developed either techniques or instruments or are interested in them. The main players are the French and Italians, who are developing their own interferometer, which is quite comparable to LIGO; it's called VIRGO.

COHEN: Where is it?

BARISH: Near Pisa. It's sixty-percent funded by the Italians and forty-percent by the French.

COHEN: Is it as big a project as LIGO?

BARISH: It's the size of one of the LIGO laboratories, essentially. And then there's a somewhat smaller project [GEO] near Hannover, Germany—a joint Scottish-German project. And then there's another one in Japan. There are very serious aspirations for a facility in Australia. Now, in most physics, when you have these facilities they compete with each other. And the nature of the history here is also basically competition.

COHEN: They all want to be first.

BARISH: But the problem in this case is a lot different, in that the gravitational-wave signal is generated for these devices by some sort of event somewhere in the universe a long ways away. And the earth is transparent to [these signals], so wherever you have a detector they will all simultaneously see the same signal.

COHEN: No matter which hemisphere they're in?

BARISH: No matter which hemisphere they're in. The orientations and so forth can change the signal, but we know how to look at that. So the most powerful thing scientifically you can do, when you see something, is to simultaneously ask every other device that's operational in the world, "What do you see?" It's the best way to try to understand what's going on—to not make mistakes, and so forth. So it's always been my vision to break down this whole business of groups competing with one another and to have, as much as possible, a joint effort—first to discover and then to exploit the gravitational waves, using all the tools in all the places at one time.

LIGO has a big advantage in taking the lead to try to pull this [enterprise] together, because we have two laboratories. To the others it looks as if we can do the whole job ourselves, while they're dependent for confirmation of their observations on each other, or on us. So by making the gesture, which we are doing, to open it all up, [we've created a climate that's] quite receptive; there's not a lot of arm twisting. In principle, it's receptive. So what I've done is a couple things: one is to, on a technical level, try to make the efforts as coordinated as possible. We need to do the same kind of developmental work on things like optics and mirrors, so why should we spend a lot of money doing it ourselves and the French do it themselves? And then maybe they'll do it better, or we'll do it better—if you share the [developmental work], it's better off. So we're trying to see ways where there's mutual interest, where we can develop things together. I would say there's modest success there. For example, we're working on the development of a new kind of mirror that would use sapphire as the material. The key step of creating the sapphire is being done by us, but the work on some of the optics is being done by the Australians and some of the work on testing some of the properties is being done by the French. So it's joint. But we're not quite to the point where the whole thing is coordinated yet. I hope it moves in that direction.

The second step—in which we've had more than modest success, and a step beyond anything that was ever done in particle physics—is that we have managed to agree to have the same data format for every device in the world. Everybody's agreed to it. What that means is that you can look at data from LIGO or data from the French-Italian detector or the German detector. They are in the same format, so anyone who writes data analysis programs can use one or the other. You can easily use somebody else's data.

COHEN: That's fantastic!

BARISH: And everybody's agreed to do that. They haven't agreed necessarily on what happens next—like how you pull the data together—but that's different. The next step, of course, once there are data, is that in principle you can easily analyze the data together. And then there are the political and sociological questions of how you do that, which we'll grapple with.

The last thing we've done is more political, and it's very healthy, I think. We formed a new organization, called GWIC. It sounds a little funny, but you have to have an acronym for everything.

COHEN: Of course.

BARISH: It stands for Gravitational-Wave International Committee, and it's made up of all the major efforts in the world on gravitational waves. So there are representatives—the leaders of the groups—from LIGO, from VIRGO, from the German-Scottish and Japanese projects. The groups using the previous technique, bar detectors, are included, too. And even the people from LISA [Laser Interferometer Space Antenna], who are searching for gravitational waves in space, are included. So there are something like—I don't remember—fifteen or sixteen members. We had our first meeting last fall and the second one this past April. And GWIC has worked extremely well on several levels. Probably the most important is that it has brought the people who not only command the resources but have responsibility in this field forcibly together, in a captive way, for a couple days, and they talk to each other. There's a real exchange. And so dialogues are going on about how you cooperate on data formats and how you announce that you saw gravitational waves and compare with each other, and things like that. And on a more ordinary level—but I think it's important, this being a new field, which didn't even have its own conferences or places where people come and exchange papers—

COHEN: Or its own journal.

BARISH: Or its own journal. Basically it was tagged onto other things. The places where people would go and talk about gravitational waves were the Optical Society, because optics are involved, or general relativity [conferences]—but there it's mostly theorists—or various other meetings. But there was never any meeting of our own. There was a small meeting—well, not too small, and quite successful—that was organized in Italy and named after a famous Italian physicist named Edoardo Amaldi, who worked with Fermi. He was interested—in the early days of gravitational waves but the late days of his own life—in developing bar detectors. So they named a little workshop after him, and we have adopted that name for what will become the conference in the field, which will meet every other year. The next meeting will be in the summer of '99, next year, here at Caltech. The previous Amaldi meeting wasn't officially the big conference—it was at CERN, and the one before that was at Frascati. And now the conference will rotate around and be run by GWIC. It will run for a week; it's a full-fledged scientific conference, and we'll have a public lecture and talks on everything that's going on in the whole field. We're going to attempt to be international and have the talks distributed around—people from all around both as invitees and contributors. So it will be a real, first-rate conference, with standards and so forth.

COHEN: It's exciting.

BARISH: So that's one good thing that GWIC is doing. The funding agencies are listening to the group—they take it seriously. The group has made a couple statements about the importance of a Southern Hemisphere detector and what could be done in Australia. And I think it had some influence in Australia, where they've recently gotten some funding to take the first steps of what might become something. And now we're trying to see how to connect GWIC in at least one form to IUPAP [the International Union of Physics and Applied Physics]. IUPAP is the only really international organization of physicists which is run by scientists. There are groups in UNESCO and so forth, but they're run by science politicians. IUPAP is an international set of commissions, and it started as a place where standards are set. Now it consists of twenty commissions, including cosmic rays, condensed matter, magnetism, high-energy physics, and astrophysics. I happen to be the chair of one of the commissions—not the commission on gravitation but the one on high-energy physics. So I'm anxious to get the gravitational thing connected—not as a commission itself but as a connected part of a commission—so it will have a little more legitimacy, even though it was self-made by a bunch of people who had their own interests.

COHEN: GWIC is almost unique, isn't it? I mean, to have that kind of international cooperation?

BARISH: I didn't invent this; for years there has been a group called ICFA—the International Committee for Future Accelerators—that works extremely well in high-energy physics. It's been a forum where the major laboratories are represented and where people with ideas for new accelerators can meet and talk and people in the laboratories can coordinate, and so forth. GWIC is different, but in some sense, for me, ICFA was a model—and a very successful model. So that's where it is. Maybe we will find our holes in the road as we go down it.

COHEN: Will there be a dedication for LIGO? How will that work? Will you be breaking ground, with some ceremony?

BARISH: Caltech is probably going to want some sort of—

COHEN: Oh, I'm sure.

BARISH: I don't know quite what to do. Maybe when the construction project ends, we can have something. But I'll tell you honestly that what I want is to go low-profile. I've already talked with you a lot about how it's going to be a hard two or three years to make this work.

COHEN: Right.

BARISH: What I want is to take the focus off us for a while and let us go to work on what I think is really both the interesting and the hard part of LIGO, and get to 2002 without satisfying too many people. I want to have us concentrating on how to make this thing work, not worrying about somebody else worrying about whether we finish something on some date to show them, or this or that. So maybe we'll have a Champagne-breaking thing when the construction is done and then try to go into hiding.

COHEN: Well, that may be a good way to do it. I mean, they'll have their party and then they'll be quiet.

BARISH: Yes. And I'm sure people will ask us about [what we're doing]. But I'm trying to make as few promises and be as invisible as possible for two or three more years, while we make it all work.

COHEN: Now, tell me one more thing, Barry. Here you are, a member of the Caltech community. But you're not here very much. I know you like this place. How have you kind of made this—

BARISH: Caltech has been great to me. Maybe I haven't been great in return, in terms of service and so forth. In a sense, it's been my only job. I mentioned earlier how I got here: I was just hanging around at Berkeley—my wife still had to finish her master's degree in social work—and I got hired here as a postdoc and then I became a faculty member. So I've been here since 1963. That's a long enough time so that even if my average time on the campus is small, at least the integrated time is a lot. So I'm quite familiar with how Caltech works, and how it has evolved, and the people, and so forth. I don't like everything, obviously. There's a certain element of isolation and snobbery that I could do without. I went into physics to do physics. It was intellectually interesting and exciting to do it, and to be able to do something I was interested in and actually make a living at it. So what Caltech has provided, I think, is—despite all the things that I might criticize; the snobbery and so forth—it's a place that's nurtured doing science. Not just bringing in famous people—people you can point to as "great scientists"—but actually doing science here. For example, I think Harvard, which I know pretty well, is much more inclined to hire famous people to go there who did their best work somewhere else. Caltech, I think, is a little better at supporting work here. Probably the key thing for me over the years has been the fact that there is—at least, as I perceive it—a strong priority in resonance with my own priority, and that is to facilitate doing science. Whether it's providing you with the necessary space or students or time off or whatever it is, it's in the air.

So that's been the key thing that has kept me at Caltech, I think. At various times I've gotten offers to go somewhere else, and actually I was very close to going to the University of Chicago at one point, because I was working at Fermilab with neutrinos. I like the University of Chicago, actually. And there were other places, but that was the only time I was close to leaving. So I've stayed here, and I've liked it.

The second very important thing for me during many years here was that I was quite friendly with Dick Feynman. A lot of people were friendly with Dick Feynman. But he was unique, as we all know. I would have lunch with him half the days that I was here.

COHEN: At Chandler?

BARISH: At Chandler. Half the years that he was alive that I was here—which was a lot of years, a lot of time. He was interested in neutrinos, and I did neutrinos, and so forth. So he was a guy that I knew very, very well. And once our families took a trip together to Les Houches, in France. Of course, Murray [Gell-Mann] I knew, too. But the one person here who had a tremendous influence on me and whom I considered unique was Feynman. He had an enormous impact on me, not just intellectually but in kind of seeking the truth. All these things underneath that drove him—not just how smart he was—had a really big influence on me.

I half-kiddingly said that I haven't been a wonderful citizen. I don't serve. Actually, that's Feynman's fault. In my early days as a faculty member, I was put on a faculty committee—my first faculty committee. I've only served on two or three, and it may have been my second one. But it was the wrong committee to put me on. It was called Industrial Relations. Maybe they still have it, I don't know. [There is currently a faculty committee called Patents and Relations with Industry—ed.] At that time, people didn't travel quite as freely and so forth. As far as I could see, this was a bunch of senior faculty members who were using the committee as a boondoggle to go visit industries who maybe would give money to Caltech or be connected to Caltech, and that boondoggle aspect really turned me off. So I remember being at lunch once with Feynman, and I complained about this—kind of graphically, as you do with people you know well. And he looked at me in the typical Feynman way and said, "Well, why are you on this committee?" And I said, "Because they asked me." And he said, "Well, so, why don't you say no?"

COHEN: [Laughter] I can just hear him say that.

BARISH: And the next year, I said no. And then I said no again two or three more times—I don't know how many times. And now they never ask.

COHEN: They don't ask you anymore. Well, now you're busy.

BARISH: No, no—but for years. I haven't been on a faculty committee since the first two or three years I was on the faculty, because I said no for a few years and then I dropped off the list. There are a lot of people who want to be on these things. So I'm a bad citizen. I don't serve on faculty committees.

COHEN: How about your teaching?

BARISH: I've taught every single year I've been here. Every year. I've never missed a year.

COHEN: You probably like to teach.

BARISH: Yes. I've taught every year. The reason I've never had a sabbatical is because my wife's not really free to travel, because she has patients.

COHEN: But you do get to travel? [Laughter]

BARISH: Yes, but as long as I'm in town I might as well teach. I always like the students, and I like teaching. The problem I have with it is time. I don't like to teach if I can't spend the time to do it properly. And so at times—especially in recent years, when I've been so busy—I've been doing teaching that doesn't take a lot of time or has flexibility, like in a lab or seminar courses or this or that. Because I know that if I take on teaching mathematical physics or something—which I like to do—it will be just too demanding.

COHEN: How about graduate students?

BARISH: I was going to say that one of the things that's obviously great at Caltech is teaching, especially the undergraduates. It's great because they're so good. Now, I haven't taught anywhere else, so I don't know how it is in other places. I've always had a lot of graduate students. Even now, when I'm so busy running LIGO, I have two graduate students in high-energy physics and two in LIGO. I've typically had between five and ten for most of my career, but now I can't—I just don't have time for that. And I like graduate students; I can work well with them. They're great, too. You get a lot back from graduate students; you put a little bit in, and then they give you so much back. I haven't been very good at service duties; I suppose that's where I've slacked off.

COHEN: You've earned your keep. [Laughter]

BARISH: I blame it on having to travel and being busy and so forth. There are committees and there are service things. If you look at who does all the service things in physics, it makes me feel a little bit guilty. There's a lot of work that people have to do to make the thing run—

COHEN: So you don't worry about matters like the core curriculum, and stuff like that?

BARISH: I don't. I stay away from all that, as much as I can. I'm not on any faculty committees; I'm not on any physics department committees if I can help it, except something like an appointment or promotion committee—something that's ad hoc. The standing committees I manage to stay out of. Of course, somebody else has to do it, and I have to suffer with whatever curriculum they decide on.

Anyway, Caltech's been a very good place for me. I've thrived here. I don't have fantasies that I would have done better somewhere else in terms of being able to realize myself. I was lucky to be able to do physics. And the one job that I had worked out well for me. Maybe for somebody else, somewhere else would have been great.

COHEN: It seems as though most people sitting in that chair feel exactly like that. They think Caltech is a wonderful place, and I guess it is.

BARISH: Well, it's been wonderful for me. It's fit me well. In some ways, abstractly, I wish I were in a university that had a lot of culture around. I'd rather meet somebody who's an artist or philosopher than another engineer or physicist. So if you ask me what I feel I miss, it's the breadth that I might have gotten if I were in Berkeley or something. I might have gotten, but maybe I wouldn't have dealt with those people anyway—I don't know. But as far as my personal life goes, a lot of our friends are not in the institute. Mostly, because I work with technical people, I have a lot of friends, but not a real lot, who are not academics.

COHEN: And of course you've moved away from Caltech also. You moved to Santa Monica—to be closer to the airport. [Laughter]

BARISH: I don't have much more to say about Caltech. I don't know that I can generalize to say it's a great place, but it's a great place for me.

COHEN: Aside from Feynman, has there been anybody else who has been particularly influential?

BARISH: Oh, he stands way above anybody else. In my field, in experimental work, there are a lot of people I think a lot of. I've been influenced by them and by what they do and how they do it—no one, particularly, from here, from Caltech. I was allowed at an early stage to kind of break out and do my own thing. In some ways, Alvin Tollestrup, who hired me here and who since has left, was almost more of a mentor than I had as a graduate student; I didn't have one then. But he wasn't, really, because I rejected a lot that he put forth. Probably the closest to a real long-term mentor that I've had is Dick Taylor, at SLAC. He's a Nobel Prizewinner. I worked with him for a while. I walked away from that experiment [Kendall-Taylor-Friedman], which I think I mentioned. We're very good friends. But he gives me advice, always, and I sometimes listen. There is almost no one else I actually listen to; even though Dick can be a little off the wall, I listen to him. He's a guy with what I think are normal skills—not off-the-scale skills—who maybe had a little bit of luck. But he basically had an approach to doing science, to realizing himself, to doing as much as he could with himself, that I admire. I think I'm considered smarter than Dick Taylor intellectually, and yet I admire the man even though some might say there was some luck in what he did to get a Nobel Prize. There's maybe some [truth to] that, but I don't care. I think the guy has both a certain integrity and a way to—in life and in science and as a person—realize himself. I admire that. He's somebody I've always admired a lot.

COHEN: Well, I think we can wrap this up.

BARISH: Good.

COHEN: Thank you. **[Tape ends]**