



**GARY H. SANDERS**  
(b. 1946)

**INTERVIEWED BY**  
**SHIRLEY K. COHEN**

**October 16, 1998**

**ARCHIVES**  
**CALIFORNIA INSTITUTE OF TECHNOLOGY**  
**Pasadena, California**



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### **Preface to the LIGO Series Interviews**

The interview of Gary H. Sanders (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, astronomy, LIGO

## **Abstract**

Interview October 16, 1998, with Gary H. Sanders, then project manager for LIGO; currently (2010) project manager for the Thirty-Meter Telescope.

Recalls building cyclotron, Stuyvesant High School. Physics major, Columbia University (BA 1967): Mel Schwartz, Leon Lederman, Jack Steinberger, T. D. Lee; politically active. PhD, high-energy physics (MIT, 1971). Three years with Samuel C. C. Ting at DESY in Germany. Princeton postdoc with A. J. S. Smith. Brookhaven and Fermilab. Leaves for Los Alamos, 1978. To Brookhaven, 1984, kaon decay experiment.

Proposes neutrino experiment, Los Alamos. Meets Barry Barish, member DOE review committee. Discusses neutrino oscillation experiments. Involved with SSC [Superconducting Super Collider] in 1989 through Ting, who builds a detector for it. Troubles between Ting and Roy Schwitters, SSC director. Barish as co-leader of U.S. groups with Ting. Ting detector project falls through; Sanders and Barish pick it up. 1993, Congress cancels SSC. Barish returns to Caltech; Sanders to Los Alamos to GLAST [Gamma-ray Large Area Space Telescope]; investigates WWII human radiation experiments at Los Alamos.

Rochus (Robbie) Vogt removed as LIGO director, replaced by Barish (1994), who brings Sanders in as project manager. His first impressions of LIGO. Comments on Kip Thorne, Rainer Weiss, Vogt, and Ronald W. P. Drever. NSB review of LIGO, fall 1994. Many LIGO scientists left. Caltech as ideal LIGO venue.

Collaboration with foreign gravity-wave groups. Common data format. LIGO Scientific Collaboration. LIGO origins at Caltech in 1970s. Discusses need for openness in LIGO.

## **Administrative information**

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**Contact information**

Archives, California Institute of Technology  
Mail Code 015A-74  
Pasadena, CA 91125  
Phone: (626)395-2704 Fax: (626)395-4073  
Email: [archives@caltech.edu](mailto:archives@caltech.edu)

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**CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES**

**ORAL HISTORY PROJECT**

**INTERVIEW WITH GARY H. SANDERS**

**BY SHIRLEY K. COHEN**

**PASADENA, CALIFORNIA**

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**Interview with Gary H. Sanders**  
**Pasadena, California**

**by Shirley K. Cohen**  
**October 16, 1998**

**Begin Tape 1, Side 1**

COHEN: Good afternoon, Dr. Sanders. Let's start with your telling us a little bit about your family background—how you got into science.

SANDERS: I grew up in New York City. My parents were the children of Jewish immigrants; their fathers and mothers had come to this country at the turn of the century. My father worked in a publishing house, doing administrative work, and my mother was a homemaker. They were people who were very intent on and supportive of education, so it certainly was an environment that stimulated education. But you asked how I became interested in science. I never wasn't interested in science. I still don't understand what it is that makes me, or makes someone, a scientist. My mother once showed me, when I already had a doctor's degree, a paragraph that I wrote in third grade, which said that I wanted to grow up and be an experimental elementary-particle physicist.

COHEN: Wow!

SANDERS: I was eight years old then—that would have been in 1954.

COHEN: That was before Sputnik.

SANDERS: Sputnik was in October of '57. I don't recall [writing] that, but it was clearly a very well-formed idea. I do know that ever since then, I've always been interested in particle physics. Not in the mathematics of particle physics; I think it was a psychological thing—I think it

attracted me in a psychological way. Probably I would need a psychologist to understand the image.

COHEN: Where did you go to school in New York City?

SANDERS: I went to the public school system in Brooklyn. But I went to a science high school—Stuyvesant High School, in Manhattan. There were several of those high schools. You took a test in order to get in. Stuyvesant was a marvelous environment. There was nothing good about the building; it was a ratty old building. But it had an incredible faculty and a very bright bunch of students. It was just a wonderful environment to be in. That was also the first time I got involved with a scientific project. And here I am coming to talk to you about LIGO [Laser Interferometer Gravitational-Wave Observatory]—another interesting project that happened years later.

When I went to Stuyvesant, in 1961, a group of students was organized by a faculty member there named Alfred Bender, to build a cyclotron. It was a little group—six or seven or eight students—and we mostly, during the years I was there, built a cyclotron. There was a design when I got there. We would come to school at 6:00 or 6:30 in the morning, before classes, and work for a few hours in the lab, winding magnet coils, or building a piece of electronics, or stacking lead bricks for shielding around the thing—or machining in the machine shop, making “dees” or making the vacuum chamber for the cyclotron. By the time I left, the system was mostly together and parts of it were working. That was a multiyear, large science project for a high school student. It was called the Cyclotron Club, or Cyclotron Committee, and it was my central activity in high school. And I guess ever since then I’ve built experiments and done the science with them. I didn’t get to do the science—the alpha-particle production—with that cyclotron, because I graduated and went on to other things. But I was reminded three or four years ago—after my mother died a few years ago, there was a set of papers she kept for me—and I noticed, in a picture in my high school yearbook, that I was the chairman of that committee. I thought of this when I was coming in for this interview. I suddenly realized that very often a lot of you is what you are when you are young. So there I was in high school, and I was already kind of a project manager. Although what I remember doing was building a magnetic field and measuring instruments—

COHEN: So you don't remember being the manager?

SANDERS: —and building a particle detector. But there I was—I saw in the yearbook that I was chairman. So whatever made me end up in that role must have something to do with me as a person. It's just an intriguing thought that came to me as I was thinking about this interview.

COHEN: Well, one has opportunities when one is at a certain place—I mean, you were in New York and you went to this science high school.

SANDERS: Right. If I had grown up somewhere else, in Kansas or some small town, I would have—

COHEN: Well, you would have had a chemistry set.

SANDERS: That's right.

COHEN: Probably. So then you went to what college?

SANDERS: I went to Columbia University as an undergraduate. I stayed in New York City and lived on the campus up there. I was a physics major and also took a lot of mathematics courses. I enjoyed that. I worked very hard. I was also involved in political activities. That was the height of [student political] activity in the 1960s. So I worked in the Congress of Racial Equality and the Students for a Democratic Society and civil rights and anti-Vietnam War things. But I ended up getting a physics degree there. That was a very good Physics Department. Mel Schwartz and Leon Lederman and Jack Steinberger and T. D. Lee were really quite an outstanding group of people to take classes with. I remember taking the Feynman course from Mel Schwartz; it was quite an experience.

COHEN: Al Schwartz?

SANDERS: Mel Schwartz, who later won the Nobel Prize [1988] for a two-neutrino experiment, along with others from Columbia [Leon Lederman and Jack Steinberger]. He was an inspiring

teacher. They were all good teachers, and they worked us very hard. I graduated in '67 and went to MIT to take a doctor's degree. And then I knew I wanted to be a particle physicist—once again.

COHEN: Were these big machines working already at that time?

SANDERS: Brookhaven [National Laboratory] was out on Long Island—not far—and they had the Cosmotron, and by the time I graduated they were running the Alternating Gradient Synchrotron. Then there was another machine, the Princeton-Penn Accelerator—PPA. While I was at Columbia, we got news of important experiments—the omega-minus hyperon, in February of 1964. That had a big impact on us. I remember Mel Schwartz walking into class and giving us a report on the announcement that that thing had been seen.

COHEN: What is that?

SANDERS: The omega-minus. It completed one of the quark groups—the multiplet of particles—and it confirmed some of the thoughts of [Murray] Gell-Mann and others. So that was an event I remember as an undergraduate.

So when I went to MIT, I wanted to go into high-energy physics. I had actually been called up by Sam [Samuel C. C.] Ting, who had just gone there.

COHEN: He won a Nobel Prize, too.

SANDERS: He won the Nobel Prize, too [1976]. He had just moved to MIT from Columbia, but I didn't know him. I had never taken a class from him. He had been a young faculty member at Columbia, and he had just taken a tenured position at MIT, and he was looking for students. So he called me one day and asked me to come up to MIT. I remember flying up in a blizzard. I met him, and he gave me a homework assignment to write a paper on vector dominance theory.

COHEN: To see if he really wanted you?

SANDERS: Right.

COHEN: I see. That was a rather unorthodox way of getting students, wasn't it?

SANDERS: Well, he knew a little bit about me, I guess, from my records and from talking to people at Columbia, but he was testing me. And he was someone who would test people. I was supposed to give him this paper when I showed up at school a month or so later. So I gave it to him, and he accepted me as a student.

COHEN: So you had already been admitted to MIT, then?

SANDERS: I had already been admitted to MIT, that's right. I had been admitted, and he was looking through the folders of incoming graduate students and picked me out to give me this homework assignment to see whether I got past his scrutiny.

In your outline, you ask about people or events that had a significant influence on my career.

COHEN: That's right.

SANDERS: I had already chosen a career, but he certainly influenced the development of that career. So, then I showed up, and almost the first day I was there he said, "You know, at MIT, to get a PhD you don't actually have to take any courses. You don't really have to stay here at MIT. You can come with me and my group to Germany, where we're building an experiment." The department at the time was a little wary of someone going off that far away right away without taking courses. And in fact they had a requirement that you had to take a qualifying exam—or a general exam, as they called it. So I stayed at MIT for a year and did some work in the lab and took some courses, but I passed my general exam at the end of my *last* year. At the end of the academic year, I got on an airplane and went to Germany, where I spent the next two and a half years. So my graduate-student career was three and a half years, and I was almost never at MIT again.

COHEN: So you got off the East Coast, but not to go west.

SANDERS: Not to go west, that's right. So I lived in Europe for about three years, a good part of which I was a graduate student and worked with Ting on an experiment.

COHEN: Was that a self-enclosed world, or did you really get to see some of Europe?

SANDERS: That's an excellent question. I felt it was a self-enclosed world. We worked at DESY [Deutsches Elektronen-Synchrotron], the German national lab in Hamburg, on the outskirts of the city. It was very far to the north. The days were very long in the summer and very short in the winter, but the winter always seemed longer. And it rained, sometimes for thirty days—not heavily but persistently. And we worked day and night. We had a group meeting at 9:30 every morning—there were fifteen of us in the group—and another at 3:00 in the afternoon.

COHEN: Were there any Germans in this group?

SANDERS: Oh, yes, about half the group. And we had another meeting at 9:00 at night. Three meetings a day, seven days a week. And it was a very rigorous time; we just worked like crazy. We ran our experiment on the accelerator for probably six months out of the year. So we worked shifts around the clock. I think the first six months I was in Germany I worked the midnight-to-eight-in-the-morning shift.

COHEN: Now, how many people were there like you—graduate students? I gather Ting was there also.

SANDERS: Ting was there most of the time—to the dismay of his chairman, who wanted him to teach. In the group of fifteen or so, we had two or three graduate students—two from MIT and a couple from German universities, whose thesis advisor wasn't Ting but they were under his supervision. So it was a small group by today's standards in high-energy physics. But I learned a lot about how to work very hard and how to check very carefully everything we did and how to plan very carefully. Ting said to me early on, "You'll get a degree in about three and a half years. You will work. MIT students take five and a half years, on average, before they get a degree. You'll do five and a half years' worth of work. The reason you should get out in three

and a half years is that if you're ambitious you don't want to take too long, because your ambition burns out."

COHEN: Burns out not because of the work but because of the time?

SANDERS: Because it would take so long that you'd lose touch with whatever your goals were, or you'd get distracted. Another way to say it is that if you take too long, it means you *are* distracted—you're not goal-oriented, you're not teleological.

COHEN: So did you get any time off to go visit your family?

SANDERS: Oh, now and then it happened, but very seldom, if you look at the interval of time. It was really a very hardworking environment. That's not so different in fact, in spite of Ting's reputation, from how hard a lot of graduate students work.

COHEN: But you were in an alien atmosphere.

SANDERS: I did feel as if I were on another planet. I went to Germany not speaking any German. But I was pretty good in those days at picking up languages, so six months later I was freely conversing. Terrible in terms of reading German, and I couldn't write in German, because I couldn't construct the grammar formally. I was learning it the way a baby learns it: I learned it by the ear. And I was there long enough that I began to think and daydream in German. At some point, you suddenly have the feeling "My God, I'm not thinking in English anymore!" That's an interesting feeling. Over the years, I've learned that if you go to a lab like CERN [the European Organization for Nuclear Research], in Geneva, or DESY, what you do every day is the same as what you do at Fermilab, say, or at SLAC [Stanford Linear Accelerator], up in Palo Alto. The language is different, but you end up just getting totally immersed inside the gates of the lab or out on the experimental floor. It really doesn't matter where you are. So I had this period of several years of immersion.

COHEN: Did you see anything else of Europe?

SANDERS: Oh, yes—there were some weekends that I got around. But we were pretty focused.  
[Laughter]

COHEN: It sounds like it.

SANDERS: Right. So Ting was certainly an influential person in my life.

COHEN: How did you fulfill all the requirements of MIT?

SANDERS: Well, I had to pass the general exam at the end of my last year.

COHEN: Did you have an individual project to write a thesis on?

SANDERS: Oh, yes. I certainly had a thesis. We did an experiment on testing the validity of quantum electrodynamics, and that was part of my thesis—and also measuring photoproduction of a particular meson, a vector meson, which was interesting in those days. So then I came back to MIT and finished up.

COHEN: And Ting stayed in Germany?

SANDERS: No, actually—that's interesting. Ting came back to the United States and proposed an experiment at Brookhaven which was an extension, or an elaboration, of what we had done jointly. He invited me to work on it with him, and I said, "I'm not going to work on that. It's just the same old thing." And I went off to my next job, at Princeton. The experiment that I didn't think was very interesting won the Nobel Prize. A little bit of serendipity was in there, so I'm not convinced that my judgment was wrong.

COHEN: Barry Barish [Linde Professor of Physics; director of LIGO] has a similar story.  
[Laughter] You probably know the story.

SANDERS: It had to do with SLAC.

COHEN: He got tired of working at SLAC. He said, “This is boring.” And that work won the Nobel Prize, too [Kendall, Friedman, and Taylor, 1990]. [Laughter]

SANDERS: I tell that story on myself, but I have no pretensions of having possibly won the Nobel Prize or something. I think what I said on that day was the right thing to say. It was the same old kind of experiment; it was derivative.

COHEN: Well, that’s a healthy attitude.

SANDERS: Right. But it was ironic. [Laughter]

COHEN: So you then got a job as a postdoc at Princeton.

SANDERS: I was a postdoc for a year, and then I became an assistant professor.

COHEN: Who were you a postdoc with?

SANDERS: [A. J.] Stewart Smith. He was an experimental high-energy physicist. I think he’s now the chair in physics. He had also worked as a postdoc before me with Sam Ting, but he and I had not overlapped working with Sam. We did an experiment at Brookhaven and then a later one at Fermilab. The one at Fermilab studied the particles that my thesis advisor had discovered which won him the Nobel Prize. So we were doing an experiment derivative of that one—on dimuon physics and the J-psi particle.

COHEN: But then, of course, you were teaching at Princeton.

SANDERS: And I was teaching, right.

COHEN: Probably a little different at Princeton than at MIT.

SANDERS: Well, MIT is a vast zoo of disconnected and inconsistent buildings and parallel fiefdoms. Princeton was much closer to the Caltech model—certainly no parallel fiefdoms. It was a smaller place—more coherent and cohesive.

COHEN: It's the big thing in the town.

SANDERS: Oh, yes. It's certainly not lost in the metropolis. And I got to know all of the members of the department. There was actually a coherence that I don't see at Caltech in the physics department. I find people here are more spread out. Some of it's geographical: at Princeton, we had moved into a new physics building, and everyone was in the same building. That made it marvelously coherent.

COHEN: It is different. My husband [Marshall Cohen] is an astronomer. It makes a huge difference. They're spread out.

SANDERS: And in fact, people are pushing for an astrophysics or astronomy building at Caltech. I think one of the goals is to bring people together, so that there is intellectual unity and some convergence. It's easier to be "other" if you're apart.

COHEN: Did you enjoy Princeton?

SANDERS: I loved being there. Although I found—and it was why I ultimately stopped being an academic, in the pure sense of being a professorial person—I would teach two or three days a week, and then I had to go to my particles in another city. So every week, there was a trip to the airport, and I'd go away for three or four days, and then I'd fly back, preparing my lectures on the plane. I found that very hard. For four years, I traveled to Fermilab. So I decided that in my next position I wanted to live in the town where my particles were, which was hard. I had to give up teaching.

COHEN: Did you have a family at this time?

SANDERS: I was married. Actually, I was married while I was a graduate student, but my second marriage occurred while I was at Princeton. By the time I left Princeton in 1978, I had two children. So I had a family. They were young children, and there I was going to Newark Airport to go to Fermilab almost every week. This was a time when the kids were little, so I said, “Let’s go live somewhere where there are particles.” And that’s why I ended up going to Los Alamos to the accelerator there to do electroweak-interaction experiments.

COHEN: So you finally went west.

SANDERS: I never thought I would go to a place like New Mexico, but one of my colleagues at Princeton preceded me and kept inviting me, kept inviting me, and I kept going out there to give talks. And I finally said, “Wow, this is quite a place!”

COHEN: It’s quite beautiful.

SANDERS: It *is* beautiful. So we enjoyed spending seventeen years there. I never intended to leave.

COHEN: You were seventeen years in Los Alamos?

SANDERS: Yes. I got there in 1978, and I left in 1994.

COHEN: A long time. I’ve heard tell from Don Cohen [professor of applied mathematics] about how good the schools are for young children in Los Alamos.

SANDERS: The schools are good in the town. They’re not as good as they think they are, but they’re good. I’ve seen better schools. My children liked the schools. The town was a wonderful place to bring up children: doors were all unlocked; there wasn’t much crime; the air was clean. Children should live with the illusion of safety, and that place made that illusion a reality—instead of living someplace where it’s not so safe and having to create an illusion of safety. My sister lives in New York City, and an awful lot of effort goes into cocooning the children and protecting them from all the things that are going on around her. So Los Alamos

was a good place to bring up children. For much of those years, I worked on experiments at home.

COHEN: At Los Alamos?

SANDERS: At Los Alamos, using the machine to look for lepton-number-violating decays of muons. And I probably worked on that from 1978 to 1984 or 1985. And at that point, I didn't think there was much physics left in that machine. Perhaps I should have seen that when I went there. There was a neutrino physics experiment, which is still going on, which has actually reported the observation of neutrino oscillations—though I don't think Barry Barish and Felix Boehm [Valentine Professor of Physics, emeritus] believe that the result is correct, and they're probably right. That was an experiment. I actually worked on one of the early proposals for the experiment that we're now observing—on neutrino oscillations. But I got interested in doing experiments away, and I once again started traveling, in 1985.

COHEN: Where did you go?

SANDERS: To Brookhaven Lab, in New York—back to the East Coast—to look for lepton-number violations in decays of the kaon—the K meson. Related physics, but it was a really nice physics opportunity, with a really good group of people who were pulled together from Stanford, Princeton, UCLA, and several other places.

COHEN: Do all these places have distinctly different machines—so that you decide to go to Fermi or not to Fermi, or go to SLAC or not to SLAC?

SANDERS: Yes, the machines are different and the opportunities are different. An electron machine, a proton machine, different energy ranges. Some are colliding-beam machines, some are fixed-target machines. Some do both but in different ways. So there's a reason to go to a particular lab.

I worked on this rare kaon decay experiment from 1984 to 1989—

COHEN: You're using some kind of shorthand here. What kind of decay?

SANDERS: Rare kaon. It means a decay—a search for or a measurement of a decay—of the kaon, or K particle. A decay which is quite rare. And when people say “rare,” it *is* a kind of shorthand. It means a decay that happens less often than one kaon in a million or ten million—kind of at the level of  $10^{-7}$  or less frequently. We were looking for decays of that particle that would violate the conservation of lepton number—of muon number and electron number—which appear to be forbidden. There have never been any measurements of these kinds of transitions between lepton number with the sensitivity of one of these decays—every  $10^{11}$  or  $10^{12}$  kaon decays. So that was a *very* sensitive experiment. We had to measure many kaons; we had to measure their decays. We had to be convinced that if we saw an indication that a kaon was decaying and violating lepton-number conservation—which would indicate the existence of a new force or a new intermediary particle—that our experiment wasn’t being fooled by some ordinary process that was mimicking this very rare process. You can imagine something a million times more copious which now and then by accident mimics this very rare thing. Much of the experimentalist art is to assure yourself and the community that you haven’t been fooled in that way. That’s really the challenge.

We didn’t see that, but we pushed down the limits from the  $10^{-8}$  range down to the  $10^{-12}$  range. So experimentally it was a very important experiment. It would have been more important if we had seen lepton-number violation. The Standard Model of Particle Physics doesn’t predict it, but also doesn’t forbid it. There are ways you can introduce it into the Standard Model. I enjoyed that experiment, but I had gotten back into traveling—so somehow my intellectual nose just dragged me along, and that’s what I wanted to do.

COHEN: So you traveled for how many years?

SANDERS: I traveled from 1984 to 1989 or so. As we finished that experiment, I got involved in a neutrino experiment at Los Alamos, proposing it. That’s actually how I met Barry Barish, with whom I work now. We had a proposal for an underground neutrino physics experiment in a water tank, where we would stop the beam from the Los Alamos accelerator to a beam stop down at the bottom of a hole surrounded by 7 meters of iron. The only thing that would get out from the decay products through the beam stop were neutrinos. And we planned to surround it with a big 100-foot-diameter water tank lined with photomultiplier tubes looking in at the water.

And we would see the Čerenkov radiation from the recoil of electrons, produced in neutrino interactions. I worked with a couple of other physicists, and we designed this detector. We wrote a proposal to the Department of Energy in 1987-88.

COHEN: So even at Los Alamos, a government lab, you still had to write proposals?

SANDERS: Oh, yes. You don't get this money for free. They still use peer review and so on. And we were peer-reviewed and got very good marks. The experiment cost about \$40 million. By that time, I had built beam lines, I had built counting houses, I had built detectors. So I was playing the kind of role I play in LIGO—of being not just a scientist but the project manager. We had to make sure we understood how much it cost, what it would take, how many people it would take to build, how to schedule the building, and so on. And how to deal with industry to get pieces built that you couldn't build yourself. And the Department of Energy wanted us to convince them that we really understood this. So we went through a process called a Temple review, because it was led by a man named Ed Temple, who did all of these reviews.

COHEN: All for the Department of Energy?

SANDERS: For the part of the Department of Energy that wasn't the defense part—basically, the part that supports science. So Ed Temple shows up with five or six or ten people who are experts in various things, and you present your project: you present the science, the technology, how you'll build it, your costs, your schedule, your organization, and so on. And they stay with you for three or four days and they write a report. And they come up with their own cost estimates, if they disagree with you. On that panel was Barry Barish. In fact, he was the chairman of a review panel that had to consider the physics: Was this really the right experiment to build? It was a very expensive experiment. The DOE budget in nuclear physics at that time was about \$300 million a year, and we were talking about \$40 million for a single effort, so it was a big deal. And they gave us rather high marks in the physics. The Temple review concluded that we did understand how to build this thing, and they didn't change our cost estimate—which was not something they commonly did. In effect they said, "I think these guys

can do it.” But the experiment was ultimately killed, later on, in the congressional budgeting process.

So that’s how I met Barish—interacting with those reviews. He still does a lot of reviews of neutrino physics. He’s one of the people you would have take a look at a neutrino experiment—to get his advice and opinion in the peer review.

COHEN: Did that experiment ever go?

SANDERS: Well, similar experiments were done. That particular experiment didn’t go. But the one that’s now reporting neutrino oscillations is the one that my old gang came up with after that. A much smaller, less ambitious experiment, with a smaller tank full of mineral oil and a scintillator, but also looking at the beam from an accelerator being stopped. They worked at it, much less money—and they’re convinced that they’ve seen neutrino oscillations. If it turns out that they’re right, great! Right now, I think the community is not at all convinced. Although they’re convinced of the Super-Kamiokande result from Japan—it’s another report of neutrino oscillations but in a slightly different way. So right now, I think, the community says we do have neutrino oscillations. They’re believing the Japanese experiment; they’re not believing the Los Alamos experiment.

COHEN: Although both of them have the same results.

SANDERS: Well, no, the oscillations have different parameters and they’re not consistent with each other. So they’re both nonzero, but they’re not consistent with each other.

In 1989, actually—it was ironic—our neutrino project had been certified and blessed and sent off to Washington to get funded, and I went to a conference. The next chapter begins with me in a conference in Moriond, in France, which is one of the popular winter conferences because you can ski in the middle of the day. And I gave some talks on rare-kaon physics.

COHEN: Where was this?

SANDERS: Moriond. It’s held in a town called Les Arcs, which is a ski resort. I was at the conference when I got a telephone call from the States telling me that our neutrino experiment

had been shot down in the budgeting process. And I had had a phone call, just before I left on that trip, from my old thesis advisor, Sam Ting.

COHEN: Ah, he surfaces.

SANDERS: I hadn't worked with him or had much contact with him since I stopped being his student, in '71. I'd seen him half a dozen times over the years—he'd come to Los Alamos a couple of times as my guest and given a talk, and so on. But he called me and asked me whether I was interested in working on the Super Collider; in 1989 it was just being pitched in Congress. And I said that I had actually been thinking about it but I had this neutrino experiment that I was proposing. But I would be going to France, and I'd come by and see him. So we made an appointment. I drove up from Moriond—from near Grenoble—to Geneva and visited Ting, now knowing that my neutrino experiment had been shot down.

COHEN: How does that make you feel? I mean, you worked so hard on something. Do you have to be really philosophical about it?

SANDERS: You have to be philosophical. I had seen other proposals shot down. I had one at Cornell fifteen years earlier that was shot down. Sometimes they go, sometimes they don't.

COHEN: So you have to remove yourself, is what you're saying?

SANDERS: Yes. Peer review isn't perfect, but there is a process. And now I sometimes have a chance to sit on the other side of the table. I just did a review of the neutrino physics program at Fermilab; I chaired the review panel. You can see what's going on. People are really trying to make the right decisions. I never felt bitter or anything. You're asking for a lot, in terms of resources and so on.

So I went and saw Ting, in Geneva, and he had an idea for a proposal to the Super Collider. And he had just brought on that year what was at that time the largest detector in high-energy physics. So there I am in Geneva, and I go into this underground hall and I see this three- or four-story-tall detector. There were four of these instruments at the CERN LEP [Large Electron Positron] collider.

COHEN: Now, Ting was still at MIT?

SANDERS: Ting was a professor at MIT, but he had his group at the time at CERN. He's always been a guy who went—

COHEN: Wherever he wanted?

SANDERS: Well, wherever the physics he wanted to do was. And he had just finished this detector, and it was working. It was just beautiful—it was the ninth wonder of the world. He was saying, "I'm going to propose something for the Super Collider. And I have Russian collaborators and Chinese collaborators and Germans and Italians and Americans. We're going to bring an international group with a lot of resources from outside the United States, and we're going to build an incredible detector with very fine resolution for muons and electrons." And that was something that appealed to me; he and I shared the same philosophy about how to do particle physics experiments, with a very fine resolution, using particles that you could filter, purifying the signal very well, so that the result you'd get was more likely to be reliable. So there was a philosophical meeting of minds.

So we talked. And what I had to do, if I wanted to work in this group, was bring Los Alamos into this. Now, Los Alamos is a Department of Energy lab; the biggest part of its program is defense. They don't usually make commitments to a thing like that. The experiments we were doing there were comparatively small. Even the \$40-million project was something to do at Los Alamos. But I also took the view that the Department of Energy was going to propose a \$5-billion project to the Congress, and Los Alamos was one of the two or three largest Department of Energy labs. We'd be fools not to be in this kind of physics, and I was very attracted to this kind of physics. So I went home and I went to see the director of the lab, and he was turned on.

COHEN: Who was the director?

SANDERS: A guy named Sig Hecker—Siegfried Hecker. His associate director, with whom I actually was working part-time helping in the administration, was Fred Morse. They were

turned on. If you're taking down names, here's another one: Pete Miller. They were very supportive, and these were high administrators at this big—

COHEN: Were they physicists?

SANDERS: Sig Hecker is a metallurgist, Fred Morse is an aerospace engineer, and Pete Miller is a theoretical fluid-dynamics guy.

COHEN: I see. But they were working in the administration?

SANDERS: They were the director and associate directors, but they all had science backgrounds and they understood the science. They understood that this was a really challenging project and that if Los Alamos couldn't play a role in providing key technology, who could? We ought to be in it. So they were supportive of my doing this kind of physics and trying to form some kind of a collaboration with Ting. And in fact the director of the lab made a trip with me to meet with Ting in Geneva in December of '89—some months later, at the time of the nuclear testing talks. He and all of the directors of the nuclear weapons labs were going to be at this meeting. I remember driving all the directors of *all* the U.S. nuclear weapons labs around in my car, taking them from CERN to the Palais des Nations, where the nuclear testing talks were held, and thinking, "What if I had a crash—that would be horrible!" [Laughter]

So we had a meeting, and Los Alamos joined. I became a player in that experiment. We started to write a proposal—it took a year to write—for a \$750-million experiment. The problem was that the U.S. government wanted at most a \$500-million experiment. Ting's attitude was, "Look, I have Chinese, Russian, French, Italian, and German collaborators. I'll bring extra money in. Leave me alone." This is a relevant comment, because it has something to do with what later happened in LIGO. There are some analogies. Anyway, the government says, "\$500 million," and Ting says, "Leave me alone. I will get money elsewhere." He probably could have done it. [His attitude was] "I'll do it my way."

So the proposal was ultimately reviewed. I became the leader of one of the systems: the innermost system, actually—the silicon tracking system, which was probably a \$50-million

piece of this big detector. Very high technology, and Los Alamos enjoyed very much stretching themselves to build this very complicated instrument.

COHEN: Well, it kept them in the forefront.

SANDERS: That's where they wanted to be. And I enjoyed this very new kind of detector. So we wrote a proposal and did R&D and got some money for it and it worked. And in late 1990, the peer review process at the Super Collider was over. Six proposals had come in, and two were selected: one other detector and our detector. Then the [Super Collider] Laboratory and Sam Ting had to negotiate a management arrangement, and that's where it all fell apart, because Ting didn't want to play by their rules. This evokes a little bit what happened with LIGO later. Also, Ting had a lot of enemies in the U.S. who were afraid that he would consume resources, do things in his swashbuckling style, and they would have less. He would get more; they would have less. He was a very powerful person, and everyone was a little wary of him. He and the director of the Super Collider [Roy Schwitters] had no meeting of the minds, so forces were against him. And at some point he just decided to pull out and go back to Geneva and work on his project there and propose something for the European competition.

COHEN: Didn't he have any loyalty to the people like you that he had brought into the project?

SANDERS: He did something interesting. He didn't sit down with me and say, "I think I'm going to walk out." But he left things intact for me. Here's what I mean by that: This was January of 1991. I could see this thing was falling apart. I could see bickering in the community. I could see that the final conclusion of an agreement to go ahead with this extravaganza wasn't going to happen. I actually got on an airplane and went to visit the director of Brookhaven and a number of people at different physics departments and the director of the Super Collider—basically to say, "You've got 450 American physicists in this project, and this thing's going to come apart. And you guys are fighting. Don't we [have an obligation] to get on common ground and find a way?" All these arguments fell on deaf ears. It was like dealing with warring Irish factions or something—there was no meeting of the minds.

And then it became clear that what was going on was that people were afraid of Sam Ting—afraid of his being in sole control of it. So I decided that what was needed was a prominent American physicist who would come in and join the leadership team. And the American physicists would deal with him as their spokesman, and Ting would handle the international part. They would be coequals. I remember sitting one weekend in February of 1991 with Steve Ahlen, a colleague of mine from Boston University. I said, “Who can we bring in?” and I referred to it as a white knight: “Let’s go through a list of all the physicists in America who are prominent experimental high-energy physicists. Can we find someone who could come in and take co-leadership of this experiment and make it acceptable to all these factions.” And we got a little list of people together and we picked Barry Barish.

Early in February of 1991, we sent an e-mail message to Barry and laid out for him how we thought this experiment was falling apart and what was needed was someone to come in and be the spokesman for all the U.S. groups—for the 450 American scientists. And nothing came back from Barry for three or four days. We didn’t know whether he’d gotten the message. What he was doing was, he was thinking. He ultimately replied positively, as long as he could be equal with Ting. We set up a meeting in March in Geneva with Sam Ting and Barry. We didn’t want anyone outside to know this was going on, partly because it would tie Barry’s hands—put pressure on him—and partly because there were people who were really trying to undermine politically, in Washington, the acceptance of this experiment. If they knew that we were trying to bring in somebody who was viable as a leader, that could be undermined. The other problem was that it wasn’t clear that the Europeans and the foreigners in this vast international collaboration were willing to accept a co-leader. Many of them were loyal to Sam Ting. And it wasn’t at all clear to Sam Ting, either: he acted like he wasn’t sure he wanted to do it, or that it was the right thing to do; he didn’t want to commit himself to it. I think he had already decided he was going to leave—I say this in retrospect—and if a new leader came in, it would make it easier for him to step out. But he would try to sell it to the international parts of the collaboration.

So there was a day in the second week of March when Steve Ahlen and I flew into Geneva Airport. Barish flew into Geneva Airport. We met in a bar at the Hilton at the Geneva Airport and drove out to CERN and met with Sam Ting and went off and had dinner. And then Ting and Barish went into a room for a few hours and came out and said, “OK, we’ve agreed.”

And the following day there was a group meeting, held in a little town called Saint-Genis in France, in a conference hall that they rent to business groups. And no one in the collaboration knew about the new plan, except for this group of four or five people. And Ting gave a speech in which he said that it wasn't at all clear that this experiment was going to be financially signed off on at the Super Collider, in Texas, and that maybe what was needed to make it more acceptable was that we bring in someone to help be part of the leadership team. Then he went through the same process that Steve Ahlen and I had gone through, and made a list on the blackboard of all the physicists who weren't already in Super Collider detectors and who were respected. He discussed them and gave his so-called candid opinions of them. He actually brought the whole group through the process that we had gone through—it was marvelous!—and held an election.

COHEN: And Barry got elected?

SANDERS: And then Barry was brought in.

COHEN: How to manipulate—you set the agenda. [Laughter]

SANDERS: A week later at the Super Collider, Ting and Roy Schwitters finally came to no agreement, and the whole thing collapsed. But Barry had made a presence in front of the collaboration at a meeting there, and now he and I realized that we were in a position to pick this up. A bunch of European groups had left—walked out. Some stayed behind, despite the fact that it was disloyal to Sam, because they really wanted to do this. Their group, their institute, their budget, and their physics interests said, “I want to do this!” So some stayed and ultimately more leaked back.

So we organized a meeting in April of '91 here at Caltech in Beckman Auditorium, where we invited anyone who was interested in any of the defeated proposals, or in our group, to come and discuss the possibility of forming a new collaboration to propose a detector for the Super Collider. So Barry arranged this here on campus, and we had people flying in from all over the world. It was a rather large meeting. There were probably 100 people representing all these groups. And the sticking point that had caused Sam Ting's problem was that he had a

\$750-million project and the U.S. government wasn't willing to commit—despite the funding sources, no matter where the funding came from—anything more than \$500 million. They were always worried about defaults, because then ultimately it was their fiduciary responsibility to complete the project. So I worked with a couple of young physicists—Renyuan Zhu, from Caltech, and Bing Zhou, from Boston University—before that meeting, and we produced a modified design for the detector which cut the cost down to \$500 million. We presented it at the collaboration. We told them that the design wasn't fixed, which was an incentive for them to get involved. You don't want to give them something fixed—these were proud, smart people. The net result was an agreement to form a collaboration. We wrote a letter of intent to the Super Collider. We had about a year of forming a group, writing a technical proposal, going through the same kind of reviews—what it would cost—that I had learned and Barry had learned in the previous experiment. How to convince people that you really do know how to spend hundreds of millions of dollars.

So there I was, living in Santa Fe, New Mexico. Barry was living here in Pasadena. For three and a half years, starting in the spring of '91—actually, I had started earlier, but ultimately there was a period of three and a half years of going every week from Santa Fe to [the Super Collider Laboratory] in Dallas. Every Monday morning at 5:00 A.M. I left my house. At noon I was in an office at the lab in Dallas. And I came home Thursday or Friday night.

COHEN: I think that's when Barry moved to Santa Monica.

SANDERS: It was near the end of it. I was still employed by Los Alamos; the two labs had signed an agreement by which I was on loan. I wasn't sure that the Super Collider was going to be built. I was willing to bleed for it, but I wasn't sure that Congress wasn't going to pull the plug on it. So they agreed to fly me down [to Dallas] every week, and I'd do my work and then I'd go home. And I kept my house, and my wife kept her job in New Mexico.

And Barry and I would meet there. We had a group. We built up a department of 100 people and a collaboration that was more than 1,000 people from all over the world. I was the project manager and Barry and a faculty member from Columbia, Bill Willis, were the cospokesmen—the three of us formed the leadership team. I was actually the line supervisor of

everyone who worked on the experiment there; the project manager was also the department head at the Super Collider Lab who had the budget authority, and everyone worked for me.

And we started spending money on this \$500-million detector. Probably got \$40 million or \$50 million into it, at the point where they were about to dig the hole to build the underground hall and we were about to let a contract out to build a \$130-million superconducting magnet. And then in the summer of '93, Congress canceled the project. And in October of '93 the [legislative] reconciliation process took place and it was really killed.

So there we were in October of '93. **[Tape ends]**

### **Begin Tape 1, Side 2**

SANDERS: I guess I had seen very large scientific projects canceled. I remember conducting a congressman around who had just voted to kill the Clinch River breeder reactor—not a project that was my favorite, but we were billions of dollars into that, and for him it was easy.

COHEN: Well, at one point the only congressman we had who had any science knowledge was somebody who got C's in chemistry in high school.

SANDERS: Right. There were a few friends. Bennett Johnston [D-La.] was one of them. He was actually the leading supporter of the Super Collider.

So there I am in late 1993. This thing is falling apart. We're scrambling to get the groups that were funded onto other projects. Many of them took the technology they were developing for us to the European equivalent of the Super Collider. Many of them are doing rather well now. And when I meet them, they say, "Thank you, thank you for bringing us along and providing us with the R&D money, and helping to lead that, because we took what we were doing there and now we're—"

COHEN: Start-up money.

SANDERS: It was start-up. So the LHC [Large Hadron Collider, at CERN] that's going on in Geneva now has calorimeters and muon detectors and other systems being built with technology

that groups had started on in our project. It's always nice to see that not everything got killed—not all the intellectual work got killed.

COHEN: But it's too bad.

SANDERS: Yes. So it was an interesting experience. Ten percent of our group ended up with no job. In fact, ten percent of all experimental high-energy physicists in America ended up getting squeezed out of the field, because that's what the budget did. There was a two-year transition, and some people just got left by the wayside. That was very hard. I remember that Barry and I spent a lot of time writing letters and making calls, trying to help people. In many cases, it worked; in some cases, it didn't work, because the field just got smaller.

So Barry came back to Caltech. Actually, from December of '93 to probably March of '94, I don't think I had any communication with him other than a few e-mails about helping this person or that person get a job. And he was working on MACRO [Monopole Astrophysics Cosmic Ray Observatory], the experiment he was doing in Italy—which he was always doing.

COHEN: He never left that.

SANDERS: And at Cornell. He never left those. And I started to work on high-energy gamma-ray astrophysics—a GLAST [Gamma-ray Large Area Space Telescope] project, which is something that's still going on now but I'm not involved on it.

COHEN: Was that at Los Alamos?

SANDERS: It was at Los Alamos, but it was a collaboration with Stanford and other universities. And I got involved in investigating the human radiation experiments, because I came back to Los Alamos in December of '93, just when all the news about plutonium injections came out. The director of the lab said, "Gary, you're good at organizing a group of people. I'm going to give you some medical doctors and some records people and some physiologists and ethicists, and I want you to find out what Los Alamos did during World War II." And it turned out that most of what had been done came out of Los Alamos. And so for eight months during 1994 I was leading a group of twenty-five people; I don't know anything about biology, but we organized

this thing. At the time, I was starting work on another experiment, and Barish was back here. We were not communicating very much. And Robbie [Rochus] Vogt [Avery Distinguished Service Professor and professor of physics; then director of LIGO] was having his brief altercation with the National Science Foundation over some of the same issues that got Sam Ting into trouble. This is my view: If you want hundreds of millions of dollars, your supporters are going to ask for a certain level of insight and accountability and visibility into what's going on—and Sam Ting always gave that, but always did things his way anyway. But it got him into trouble at the Super Collider when he insisted on no limits. And I think ultimately both men couldn't stand the scrutiny—the short leash they were on.

So I gather that in February of 1994 Robbie Vogt was removed [as director of LIGO], after great pain.

COHEN: Well, you know something about that from just being here, of course.

SANDERS: Well, I know something about it from a few months later when I showed up here and saw the battered people—the people who had suffered through this kind of war that had gone on. And I gather that Caltech, at that point, turned to Barry, who had just arrived back from a big project and was clearly someone who was astute, smart, and understood physics and how to organize something that was big.

COHEN: And he was on neither side.

SANDERS: And he was on neither side. He had been on some of the oversight committees—

COHEN: He was very respected.

SANDERS: That's right, he was a very respected guy. That's also why he ended up on my short list when I was looking for a white knight to save the Super Collider experiment. So they asked him to take over [as director of] LIGO. I gather he spent about a month or so thinking about it. And he decided to do it and do it in certain ways, and not too long after that he called me and said, "Gary, I'm doing this thing. Would you like to consider coming out here and helping me build this thing?" I said OK. I'll tell you something else about me. I started by telling you that I

was a physicist—kind of primitive—all the way back. I've never done an experiment that I didn't know in an instant whether I wanted to do it or not. The decisions were always made aesthetically, on the physics goal. If that physics goal turns me on, the rest is derivative.

COHEN: You don't care how hard it is?

SANDERS: Yes. And LIGO, I knew what LIGO was. It was over from that moment. And I've learned that the few times that I went and worked on something that I didn't have that flash—*Yeah!!*—gosh, it was agony! Physics is just too hard to do unless you're turned on.

COHEN: I'm sure that's right.

SANDERS: So, with the physics, there was really no doubt. I had a wife and a family and a house that I liked on a mountaintop in Santa Fe. We were going to live there forever. But we came out here and looked it over, and a few months later we did end up coming out. And we found the transition to California very difficult—far more traumatic than we thought, just in terms of how you feel about yourself and your space and where you are. It's an interesting experience to be fifty years old and realize that you really have built a nest and you really like it. But we're actually very happy now. We live far from here. We live in Laguna Beach, with a view of the water.

COHEN: You don't even live in Pasadena?

SANDERS: I don't. We lived in Pasadena for a couple of years while we were selling our house, and we lived in a Caltech house. And then we looked around. We were convinced we did not want to live in the smog, with helicopters overhead, and so on. So we live out at the beach now. I come in four days a week, and I come in very early—really early. I avoid the traffic; I'm almost in the opposite direction from most of the traffic. I leave here around four in the afternoon. And it works. I'm home at 5:30. And on Wednesdays, I work at home. And I have a very good Internet connection and a conference phone. It's the only time I can get any time to actually read things carefully, and write things. You know, the rest of the time is all meetings.

COHEN: So tell me more about when you first came here [as LIGO project manager and deputy director]—what were your impressions.

SANDERS: I was turned on by the physics.

COHEN: I mean, I appreciate now that your working with Barry was just old hat—

SANDERS: That's right. But that's an interesting chapter if you're talking about an oral history at Caltech. First of all, it became very clear to me that Caltech—from Tom Everhart [Caltech president 1989-1999] to Charlie Peck [chairman of the Division of Physics, Mathematics, and Astronomy, 1991-1998] to Kip Thorne [professor of theoretical physics] to David Morrisroe [Caltech treasurer]—at the time really wanted LIGO rescued. NSF, on this project, had told the Congress, "Don't give Caltech \$35 million this year." That usually is the death of a project. It was effectively canceled.

COHEN: You hadn't dealt with someone like Kip, who doesn't know about "no."

SANDERS: So I met all these people, and it was very clear to me that institutionally, on this issue, Caltech was a coherent unity. Wanted this thing to work. I think they were very grateful to have Barry. And I suddenly found myself being treated like a visiting rock star, or something, which was kind of interesting. But I also was quite confident that I could do this—I looked at the experiment. I felt that this was a very supportive environment, and that had a lot to do with my decision to come here. I had just come from a project in which the Super Collider Lab, the Department of Energy, and the Congress didn't really support what they had decided to do, and had other agendas at all times. And what I saw was, at the National Science Foundation and at Caltech, a unity of goals—and that was very important. I had decided that I was never again going to work on an experiment in which the consensus that started it was unlikely to last through the execution of the project.

COHEN: So you weren't put off by the bitter fighting that had gone on?

SANDERS: No. Actually, what I saw was that Caltech wanted to do this and they really thought that I could make a difference. I felt very welcome. They needed work financially, and they introduced me to the people at Caltech who matter. I spent an hour with Morrisroe talking about [LIGO]. It was clear that this place wanted it to go. They were willing to do what they had to do to make it go. I knew the program managers at the National Science Foundation. David Berley, who was the program officer, had been the program officer at the NSF and earlier had worked in high-energy physics. He had done experiments at Los Alamos; he and I were actually collaborating on an experiment that was going on at the time at Los Alamos. And I had known him when I was at Princeton, back in 1971, when he was running Brookhaven's experimental program. So he could look me in the eye and say, "We really want to see if we can make this go," and I could believe it. So it did matter that there were people here I was calibrated on and also people here who convinced me. And I saw Caltech as a place that was pretty good at picking frontier scientific directions and then aligning its resources and its intention to carry them out. That impressed me. I hadn't seen that at MIT, for example, which is a place that's usually in a fair amount of disunity. It's even worse than the fact that it's culturally not unified. There's a cultural memory of some origin, I think, that has generated a place which is in conflict and tension—at least, and I'll be as positive as I can, in a state of mutual nonsupport.

COHEN: Well, even in the way they've treated [MIT physics professor] Rai [Rainer]Weiss.

SANDERS: That's right—you still see it today. And that's not an isolated incident. Sam Ting could thrive at MIT, because he could just devour anything that was in his path and make his own world. And he would use MIT as a shingle. Despite the fact that there are brilliant people here, and some very strong egotists—and a certain amount of parallelism and city states among the groups—when a direction is picked within the institution, Caltech has a pretty impressive ability to unify. Again, it must have to do with the cultural origins of Caltech which are perpetuated somehow. And even the fact that it's not a very large place—that contributes to it. So I sensed this.

I also decided very early on that LIGO, despite all that you hear about how hard it is to do—and to do it is very hard, in the sense that they're trying to push things to the quantum limit—technically it's a really amazing thing. But as a project, to execute the project you have

planned—I found this, and Barry, I think, concluded the same thing: We said, “This is easier to do than what we’ve been doing at the Super Collider.” It may seem like hubris, but I want to confess that when I look at carrying out the LIGO project—just building what we’ve set out to build—it’s an easier project than what we were going to do with the Super Collider, in many ways: management-wise, technically, money-wise, schedule, almost every way. There were more complicated technical systems that were beyond the state of the art in the Super Collider experiments than there are in LIGO. On the other hand, there are some physics limits that we’re trying to do in LIGO that go beyond what we were trying to do with the Super Collider. In terms of building it, turning it on, and making it work, this is an easier challenge. I’m convinced of that even today. People who marvel at the challenge of LIGO might find that hard to understand. I remember after being here on two or three trips—early on, in that exploration of whether or not to come here—Barry and I looked at each other and one of us said, “It’s easier to do than what we’ve [been doing with the Super Collider].”

COHEN: Of course, Barry had the loyalty to Caltech, which you did not have.

SANDERS: Absolutely, right.

COHEN: I mean, that was part of his decision.

SANDERS: My loyalty has always been to what I was doing. I really enjoyed being at Princeton. I venerated it. I’m pleased to be at Caltech; it’s a great place. But in fact what gets me up in the morning, and what I’m focused on, is wanting to build LIGO and see gravitational waves. And 100 years from now, if we do see gravitational waves, it won’t be that important that it was done at Caltech. It will be important to Caltech, and it will be noted, but that’s the result.

COHEN: I know. Kip thinks that way, too, frankly.

SANDERS: The loyalty is whatever compelled me to become a physicist at age eight, [and whatever] compelled him to drive now for twenty-five years to measure gravitational waves. That’s what it is, make no bones about it. Don’t lose focus, OK? That’s why Rai Weiss and Kip Thorne, more than anyone else in this business, are the two who have plowed through whatever

has gone on. I mean, I know them both well. And I know the Dreviers [professor of physics Ronald W. P. Drever], and I know the Vogts, and I know all the major figures now in the field. I am a minor figure in the field. I'm a newcomer and so on, even if I'm the builder of it. The guys who had the intellectual coherence and dedication to it—on the day we see gravitational waves, they will be there standing and looking at the plots, no matter what happens, no matter what explosions, disruptions: it's Weiss and Thorne. There is a focus in those two that I've seen that no one can match.

COHEN: I haven't talked to Rai.

SANDERS: You should.

COHEN: I may have to go to MIT to do it.

SANDERS: I think the piece is incomplete [otherwise].

COHEN: I know that.

SANDERS: And he comes out here, so he could arrange [something].

COHEN: I know, but he's always busy.

SANDERS: He will come out and talk to you. We can work on that with you. He's a missing piece. I convinced Jane Dietrich, who wrote the recent *Engineering & Science* article,<sup>1</sup> to talk to Rai. I said, "He's an MIT faculty member, but he—"

COHEN: She finally had a few phone conversations.

SANDERS: She did, and she told me it was very valuable. It would have been a puzzle with a missing piece.

COHEN: Oh, I'm sure.

SANDERS: Kip's dedication was absolutely clear. I said to people, after I had spent my first couple of weeks with the LIGO group, that it was like visiting a battered women's shelter. These people were battered—I used that word earlier this afternoon. They were battered. It was kind of like they were pulled into a shell—almost in the fetal position. When you talked to people, their body language was very defensive, a little suspicious: “Am I allowed to talk to him?” and this kind of thing. They were under stress. I'm not faulting them, I'm telling you the result of what they had experienced.

COHEN: It was terrible. I mean, you know, I was part of this community, too. I know what went on. Things at universities can get very bitter. You don't kill your enemy here; he stays right on here with you.

SANDERS: Well, as I told you earlier in this discussion, I've seen some pretty tumultuous events in physics: the demise of the detector and the demise of the Super Collider. Those were tumultuous events. I didn't just see people affected in those—I was one of them!

COHEN: You see, this is a family here.

SANDERS: I guess you're right. If brothers and sisters beat each other to a pulp, they would be not only abused but fixed and trapped in the abuse.

COHEN: And none of them are going anywhere. They're all still here.

SANDERS: But in the end, actually, an interesting aspect of the story is that many of them aren't. Many of them left. Almost every one of them is gone.

COHEN: Do you mean the people who were originally—

SANDERS: The people I visited who were part of that original LIGO group, and whom I characterized as victimized, as abused—appearing to be like someone from a battered women's shelter—are not in LIGO anymore, with one or two exceptions. That's an interesting lesson I've learned in life in the past few years. Robbie Vogt—I had heard all these stories about him before

I arrived. But he was very candid with me the first time I met him. I went with Robbie and Barry to have lunch at the Athenaeum. And Vogt looked at me and said, “You know, I’m a dinosaur. The world has moved on, and I guess I have to be put out to pasture. I’m never going to change, but I do understand what’s happened.” He didn’t get into one of the kind of defensive discussions that he got into later. I thought, “This guy does have a pretty clear view.” Because he could have done LIGO in an earlier time, perhaps, the way he had done it, and he probably would have succeeded—if it had been the fifties or the early sixties.

COHEN: Or even—and many people felt this way—if JPL [Jet Propulsion Laboratory] had managed this project and it wasn’t an institute project.

SANDERS: That’s right. I had some concerns about him, because he had a lack of confidence in his ability. His decision-making process was very tortured. He tortured himself when he had to make a decision. It had a retarding effect on LIGO. The engineers came to me and said, “Please, if you come here, here’s the one thing I want you to do. When we bring you something that’s been worked on and is ready for a decision, please work to make a decision.” They felt that Vogt couldn’t make decisions. They felt that that was independent of the stress that he was under. Robbie will tell you, “How can I deal with complex decisions when I’m being attacked?” I’d have to grant him that. A manager, a leader, a scientist, an imaginative person has only a certain capacity to pay quality attention, and it was clear he was probably saturated.

COHEN: Well, he was very good in an organization like JPL, where people did what they were told.

SANDERS: Right. By the way, back at my old haunt at the time, Los Alamos, Robbie Vogt was considered a star. He was on their oversight committee from the University of California, and he was considered one of the people you wanted to show up at those oversight committee meetings. He understood things and he gave great advice. So when I went back to Los Alamos and told them that I thought I’d be leaving to go do this, and that Robbie Vogt had been removed as the director, they couldn’t understand. And I even talked about that with Robbie. He said, “I have to go sometimes to Los Alamos—it’s the only place where they still respect me.” That’s what

he said. What he was also saying, I think—to pick up on your image—is that he was at least stepping out of the family by going to Los Alamos. He was leaving the block, and leaving Caltech.

COHEN: And how about your meeting with Ron Drever? How did you find that?

SANDERS: Again, I had been told things about him, so it wasn't a blank slate. I guess what I concluded very early on about Ron, and what I still feel, is that he's a very impressive guy intellectually. And now that I know a little bit about the field and the history of the development of ideas—and those that have emerged as the key fundamental techniques—he's probably contributed more of the techniques that are being used than any single person. But he's totally unsuited for working in a large project. As I said in my *Engineering & Science* interview—and I think I was quoted accurately—at some point you have to decide, “This is what I'm going to build,” and you have to stop perfecting it and you have to build it. And Ron can't do that. He constitutionally cannot do that. And in fact the advice from the sages is, do what comes easily for you. What he should do is be in the business of bringing new ideas into fruition, into reality. That's what he's doing now. I may be being impudent, but I think he's in a better place now.

COHEN: Is he happy?

SANDERS: I don't know. I think he feels cut off. I think he feels he was an intellectual father of this thing called LIGO, and he was pushed outside the fence and he's marginalized now. And that must be a very bitter experience for him. But the big enterprises—scientific projects that are large—develop their own compelling life. And they will consume individuals, in the course of being carried out. And this is a case that kind of shows that. If you are leading it, participating in it, and aligned with it, moving it forward, it will support you and you will support it. And if you're in the way.... I tell young people, when I talk about projects, that in the beginning of a project you're trying to assemble a team and a design, and it's agony to get to the point where you really are convinced you're ready to move forward. And I think of it as pulling on a rope to pull a locomotive up a slope: Once you get it moving, especially if it starts going on a level or downhill, you've got to learn to move with it, because it's moving. So there's a momentum

argument. And once a big project is in motion and you've got a couple of \$100 million dollars' worth of contracts out there, you can't take a week off. You've got to stay on top of it, because it will run over you. You can't stop it. You can't say to the funding agency, "No, I'm not going to talk to you." You can't get in the way of it. It will just keep on going, or it will get derailed. LIGO very nearly got derailed.

So in the end we hired more people, we organized things, we went through designs—Barry and I. He was very correct at the beginning to say, "I'm not going to take this on unless I get a chance to look at the cost estimate, the schedule, the technical design, and make changes in them." He was very clear with the National Science Foundation: "I can't lead this thing until I believe in it. Until I can tell you I can do it for this."

So we worked the first four, five, or six months—Barry started before I came. I came for several visits during this process and was present when we pulled it together. We did a cost estimate, a schedule, and made some changes in the design—ultimately, later, made some other changes in the design. In September of '94, we presented it. We were reviewed favorably and went in front of the National Science Board, with Kip present, in November of 1994. That was an amazing meeting, for reasons I didn't understand at the time, because the director of the National Science Foundation himself made a presentation to the National Science Board.

COHEN: Was that Lane at that time?

SANDERS: Neal Lane. So what looked perfectly ordinary to me—here's the director telling the board that this is great science and that he wants to go forward. This all seemed pretty natural to me. People said, "You know, this never happens. The director of the NSF never addresses the board on behalf of a single project. This doesn't happen." They told me that Neal Lane was being terribly courageous and aggressive and putting his own personal reputation on the line for what was a very troubled project—which was also the biggest and most risky thing that the NSF had ever tried. And that was quite a bold move. There is heroism in Washington.

COHEN: So tell me now: How do you think it's going?

SANDERS: We're about ninety percent through, if we count up all the tasks we had to do. That's just a way of telling you we're very far along in the construction.

I think an important part of the story—and I don't want to gloss over it—is that many of the original members of LIGO, whom I met in '94, didn't stay. They weren't asked to leave, but they didn't stay. They were given roles. They watched this thing grow up around them.

COHEN: Do you think they left because they'd been battered, or—

SANDERS: I don't know. I think it was a combination of having been battered and a resentment of the new guys who came in and of being unprepared—or perhaps temperamentally unsuited—for a new environment, one that demanded delivery at a certain pace, that demanded giving up a day of curiosity-driven work for a day of goal-oriented work. It was an alien culture to a science group on a campus.

COHEN: How many people are we talking about?

SANDERS: We're talking about twenty people who left out of probably an original thirty or forty. And many of the bright scientists left. The Bob Speros, the Alex Abramovicis. Andy [Andreas] Kuhnert, and ultimately Lisa Sievers. Many of them left; some of them were working at JPL in related kinds of science and technology; some left for other places. Barry and I—I'll speak for myself—were never quite able to understand how to run the project successfully and at the same time make those people feel like productive contributors. In some cases, people felt that they were owed a very senior role; although they were unsuited for leadership, some of them wanted to be leaders.

COHEN: So they couldn't quite make the transition from the planning of it to the building of it?

SANDERS: Or the transition from the R&D and the intellectual conception of it to the real building of it—which involved a whole different set of people. And it involved stopping being a scientist—though you couldn't *do* this without being a scientist—for a few years and doing what had to be done: writing down the requirements, writing down the design, writing down the statement of work that a company would use for building what you wanted.

COHEN: So in some sense you were asking them to leave the university-type atmosphere.

SANDERS: For a few years. And what I would tell people—I think it probably fell on deaf ears.... Jane Dietrich did this very well in *Engineering & Science*; she quoted me, she quoted Barry. I gave the example—I think it was me who was quoted on this, though it might have been Barry—you learn to be a builder for a few years, and then you learn to do science with what you built for a few years, and then your nose leads you in another direction and you learn to be a builder. And I've learned in the course of my career since the mid-sixties that I'm going to go through a rhythm of building, then doing science and publishing, then building. Sometimes these things overlap a little. There's a rhythm. I told the people in LIGO, who I could see were under stress, that this is like a sandstorm passing through a scientific research group. Grit your teeth and hunker down and do what has to be done, and the storm will pass. We'll be done with the construction, and you'll be able to be a scientist. And, boy, will you be able to do great science with this thing! It's a vision that I understand, and I've experienced it. And in fact it was a vision I understood—I think I told you—in high school, building a cyclotron for three years.

Oh, I understood that my coming from a national laboratory to a university—especially a truly world-class, excellent university like Caltech—was a cultural mismatch. I think the management, the leadership, of Caltech understood that they were trying to do something on the campus which was more characteristic of a JPL or a Los Alamos. There certainly are good scientists at both of those places, but they've learned to work in that kind of a setting. And I told people, even early on: I said, "This is intriguing for me. I have learned how to do big science experiments at a national lab. I've learned how to deal with a cross—a bunch of universities and a national lab, using a national lab to help unify them. And now I'm going to get a chance to build a big scientific project on a campus. That by itself is going to be an experiment." And it is. And we've learned some things—we've learned that a certain number of valuable people are going to end up getting rejected or alienated.

You measure scientists—and scientists measure themselves—by a number of different metrics. The undergraduate who is the best at doing the problems in electrodynamics may not be the person who builds the Super Collider. One person comes in as an assistant professor and instantly does absolutely great academic science, publishes a lot of papers, and gets tenure. The other one, who decides to build something for five or six years—a long-term goal—doesn't get

tenure and ends up at a JPL, where they do decade-long projects. These are different metrics. And when you're a student, they don't really tell you about that.

COHEN: No.

SANDERS: They have only that one [model]—the guy who can do all the problems. The role model is a professor; professors replicate themselves. Later on, you learn that there are other models.

LIGO could have been done at Los Alamos as a construction project. It could have been done by JPL. It could have been done out of another national lab, with a consortium of universities. There are a number of ways to do it. My role was to help make it work given the situation that I had. I recognized that it was at a university. I think for Caltech—if you look at Caltech over the century of Caltech—there's been a tradition of building great observatories. Some of those were large projects. They were somehow carried out. Sometimes with some agony—you can read the stories about Palomar. Caltech has had a tradition of fielding some very large instruments that were, in some sense, out of the scale of a traditional university.

COHEN: It took a Kip Thorne to do this, in every instance.

SANDERS: So I think, given the scientific potential, it's appropriate for Caltech to do. And I think it's actually Caltech's grandest observatory. I think it's more of a reach than the other observatories. More traditional astronomers—and you may know them better than I—might disagree with me. But that's my perspective. I find it intellectually a further reach than building another optical telescope or another radio telescope. I think it's right—it's what Caltech should do. That's where Caltech has positioned itself. So then the experiment of seeing how to do a project like this on a campus becomes a worthwhile thing to do, when it's set in that goal.

So we're going to do it. We're putting the detector in now. A year from now, one of our three interferometers will have light up and down the arms and mirror-control systems, with servos locked, and we'll be studying the performance of this instrument. Three years from now, all three will be working. We will be measuring the noise floor and getting ourselves in a position to do science. We promised the National Science Foundation that in 2001—the end of

2001—we would have all three systems working at the two sites [Hanford, Washington, and Livingston, Louisiana], with the design sensitivity of the initial LIGO. So at Christmastime in 2001, you'll see whether we fulfilled our promise to Congress, the NSF, and—most important—to ourselves.

COHEN: And you're optimistic?

SANDERS: I don't see anything in our way right now. We have really great people who are working very hard.

COHEN: Did you replace those twenty people?

SANDERS: Yeah. Some stayed—some very important people. Stan [Stanley E.] Whitcomb [detector group leader] is one of the unifying forces of the old LIGO and the new. He is someone who has a love of the goal, I think. Stan was a bridge during that terrible time, and kept things going. He's up there at Hanford today—two weeks out of three, on his hands and knees with wrenches, putting things together. You don't do that unless you love the experiment. He's come through what you call the controversy. He's a senior guy. And he's out there on his hands and knees.

COHEN: He did leave for a while.

SANDERS: He actually left Drever's group during the 1980s and then came back to work with Robbie. He wants to be there when we see the answer. So some of the key people stayed. Fred [Frederick J.] Raab, there are others. And a lot of new, young, bright folks brought in. And then there are a lot of people who aren't scientists—a lot of really outstanding engineers and administrative people.

COHEN: People who were hired for the project?

SANDERS: Hired to carry it out. Some are from JPL—kind of borrowed from JPL—in quality assurance and safety and reliability, and so on. We would have gotten into trouble so many

times without these folks, over how we were welding our long vacuum tubes. The expertise is just marvelous. Engineers like Larry [Lawrence W.] Jones and Fred Asiri and John Worden and Gerry Stapfer.

COHEN: Fred?

SANDERS: Fred Asiri, the guy who's built our civil engineering—our buildings. Larry Jones worked on the beam tube with Rai Weiss—just tireless, meticulous, careful, and totally dedicated. He came to us from JPL. These people are stars. They're heroes on this project. Larry's up there now, installing seismic isolation systems. So the team is very strong. They've coped with technical setbacks and technical adversity, the failure of our optical baffles—a redesign and a refit. And what I've found is that they're resilient and innovative. They can develop workarounds, their morale didn't fall apart. Many of them working at the elastic limit, you know? And pieces are coming in now like crazy—lasers and optics and controls electronics.

COHEN: I feel like I ought to go up there and see this thing.

SANDERS: It's going to go together. You should. We may have trouble making it controllable and understanding all the noise sources, and that's the part that's hard to quantify and hard to predict. But we've dealt with technical difficulties before. I think we will get the instrument close to our schedule, and I think it will work as we planned.

COHEN: Now, how do you find your competition in Europe—the people in Garching?

SANDERS: I've never viewed it as competition. That's been one of the interesting tensions in the development of LIGO in the gravitational-wave community in the four years I've been working on this. A year ago, many members of the French and Italian project [VIRGO] viewed themselves as in competition with us, in the sense that they would like—with their single instrument—to make an observation and report it. Others, and I think wiser ones, understand that with something as difficult to verify and measure as gravitational waves.... We see this in the neutrino oscillations business: someone sees it and most people say, "Now wait to see if it can be confirmed." If VIRGO sees [gravitational waves], or if LIGO does, the community will

look to the other. In fact, there are mathematical and analytical arguments to be made that the best information comes from a unified analysis of the data—a combination, not separate analyses of the data. So the intellectually right thing to do—maybe not the human nature way—is not to compete but to collaborate. A lot of that is actually happening. And within VIRGO—I'm just picking VIRGO as an example—they have groups of people who feel that they should compete. They should have their intellectual quiet in their academy, behind their walls, and look at their data and come out with their results, and then we'll see what happens with LIGO. They should have their privacy, and we should have our privacy. And then there are others who feel that we should work together. So we've developed, for example, a common data format. Early on, we said that we couldn't agree to analyze the data together, but what we could agree on—and it's not controversial—was the format. If later on we decide that we want to analyze the data together, we won't have to translate.

COHEN: That was brilliant. Whose idea was that?

SANDERS: It came from a number of people. Albert Lazzarini had a lot to do with it. Barry did. It just came out in discussions. I remember in January of 1995, the Aspen winter school on gravitational waves—the first little conference we ran for the so-called new LIGO. I got up there and gave a talk and proposed collaborations, but I didn't know how to do it. And I knew it would be controversial. And there was one hell of a conversation among the groups who were present. Out of that conversation came the idea that one of the things we could do was at least agree on a common format. So that idea was around then. We brought that idea back here, and Albert and Barry and others thought it was a good idea. Later, Barry and I talked with the leadership of VIRGO and got together with their people. And a year later, we actually were sitting down and having working meetings on agreeing how to write our data.

COHEN: So how about the Garching group? Was that a different group?

SANDERS: The Germans and the UK—Wales and Scotland—have a group to build something called GEO. They had a proposal for a multikilometer-long instrument also. They didn't get enough money for it. Between CERN and unification with the eastern part of Germany, they

were told they could build a 600-meter instrument. It's being done on the campus of the University of Hannover. They're building it. They have some sensitivity to pulsar sources of gravitational waves, but they don't labor under the illusion that [theirs is a] true discovery machine. But they're in the game, and they're doing some extremely clever and aggressive things technologically. And they are collaborating with LIGO and also with the space-borne instrument, LISA [Laser Interferometer Space Antenna]. They're in the middle of discussions about further collaboration with VIRGO. So what I see happening is that this infection is spreading, an infection that we itinerant high-energy physicists introduced; those groups are all collaborating. We've always worked that way, but those groups are now collaborating more and more.

So the LIGO Scientific Collaboration was formed. That's a collaboration outside the LIGO project that includes those of us who are scientists in LIGO. People from all of these groups, talking about the design during the R&D and now actually doing things toward the revised LIGO instruments—including some people working on the initial ones. So what's happening is something I'm very comfortable with. We've now got a good part of the world community of scientists who are interested in detection of gravitational waves and their exploitation for astronomy working together, creating the future of the field. That wasn't happening before.

COHEN: So you think all this is alive and well and healthy?

SANDERS: I think it's alive and well and emergent. I wouldn't quite yet say that it's healthy, because there are still strains, there are still people coping with the strain of new ideas and what will come out of it. I don't think it's a reversal—it won't fragment as a field—but it will undergo further synthesis.

COHEN: Now, Gary, often the last question I'll ask in an interview like this is a Caltech-based question.

SANDERS: OK.

COHEN: But you've only been here four years, so that's a little hard for you to be able to answer.

SANDERS: Right.

COHEN: Usually I get the answer, "Oh, I could not have done this anywhere else. This is such a wonderful place." Obviously, that's not going to apply to you.

SANDERS: Well, I told you a little earlier that LIGO could have been built out of a Los Alamos or a JPL. In fact, during the Robbie Vogt troubles, he explored the idea of having it at Los Alamos. So they're not crazy ideas, they're viable alternatives. I told you already about the tremendous amount of institutional support that got LIGO restarted. I'm even ignoring the intellectual and institutional support that got LIGO started in the first place. The fact is, there was a committee in the 1970s that was driven by Kip—I think [physics professor Thomas A.] Tombrello and Barish were part of it—and it got Caltech committed to the idea of bringing in a faculty member to lead this and go in this direction. That was sustaining intellectual support from the institute. I think it was a vision of where science was likely to go in the future. That paralleled, maybe, what Rai Weiss was doing [at MIT] with somewhat less institutional success. It's what I hope Caltech is doing today in biology and some other things.

COHEN: Well, I think so, yes.

SANDERS: But that's a healthy dynamic in a scientific institution, and it got LIGO started. In that sense, it makes Caltech different from any of these others. These others could have taken it over and carried it out, but the spark, the institutional fervor, was there already at Caltech. I saw it again when LIGO was restarted and I came here. I sensed the commitment there. And the National Science Foundation had it, too. So I think the way I'd answer your question is that Caltech is probably the optimal place to carry it out.

COHEN: Well, I hope so. Our new president [David Baltimore] has come from MIT.

SANDERS: He certainly knows MIT well, I'd suspect. He got a heavy dose of an introduction to gravitational waves, for a biologist—through Kip and through several visits with those of us in LIGO.

COHEN: I understand he's very smart.

SANDERS: He seems to be a very smart person.

COHEN: And he's excited by this, too.

SANDERS: And we're on our way to trying this thing on, and doing the science. So Caltech doesn't have to do much for a while. They have to provide a supportive institutional setting. We're launched. I think it's ours, actually, to mess up.

COHEN: Well, very good. Anything else? Any comments? Any last words?

SANDERS: You had an interesting point in your outline—my need for openness.

COHEN: Yes, well—

SANDERS: And I thought, Where did she get that? Because when I worked on the plutonium injection, one of my intellectual focuses was on the role of openness over the years.

COHEN: I got that from Barry.

SANDERS: From Barry, OK.

COHEN: That was his problem. He felt that things were not proceeding, with the Vogt way of doing things.

SANDERS: Right. Barry and I are in resonance on this. There's a poster in my office, a photograph of me given to me by all the folks I worked with on the plutonium injections, where

we declassified a lot of documents and got everything out, and I parodied Clinton's "It's the economy, stupid!"—"It's openness, stupid!" And I told the director of Los Alamos, "Every memorandum, every speech we give, every paper we write should be on the World Wide Web the next day."

COHEN: So people can see it.

SANDERS: Except for how to build a hydrogen bomb, it should be out there. I'm convinced of that. And in LIGO, all of our technical documents are on the World Wide Web. I didn't see that. I don't want to criticize Robbie Vogt, because I don't know whether it was a derivative of the war they were in or not. But people—even after he was gone and I was part of the leadership of LIGO—would come to me and say, "There's someone coming from this university, or from that company. Can I talk to them?" I really felt bad. At a university, the answer is yes.

COHEN: You shouldn't have to ask.

SANDERS: Exactly. Some of the people said, "We should put things on our internal web, and not let the public see it." And I said, "Why not? We are in a university." So this whole thing is very important to me. And LIGO wouldn't have gotten into trouble, had it—

COHEN: If it had been open.

SANDERS: If it had been open, right. OK, that's where that came from then. **[Tape ends]**

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<sup>1</sup> Jane Dietrich, "Realizing LIGO," *Engineering & Science* 61:2, 8-17 (1998).