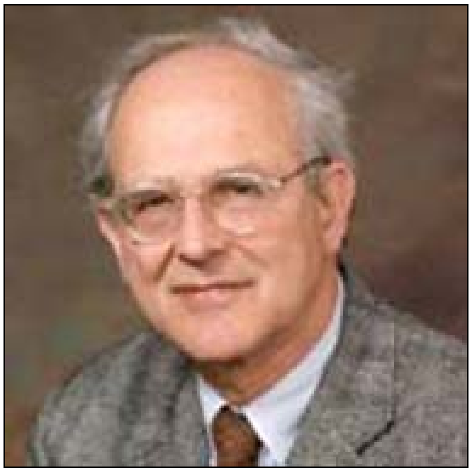

RAINER WEISS
(b. 1932)

INTERVIEWED BY
SHIRLEY K. COHEN

May 10, 2000

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the LIGO Series Interviews

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

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

Subject area

Physics, LIGO

Abstract

Interview, May 10, 2000, with Rainer Weiss, professor of physics at MIT.

Family background, Germany, Czechoslovakia; arrives U.S. 1939. Attends Columbia Grammar School. Interest in engineering. Flunks out of MIT; returns as technician with Jerrold Zacharias; cesium clock. Resumes undergraduate career at MIT; graduate student there. Princeton postdoc with Robert Dicke. Builds experiment to find scalar gravitational waves based on work by Frank Press and Hugo Benioff, Caltech. 1964 Alaskan earthquake; effects on his machine. 1965, returns to MIT's Research Laboratory of Electronics at Zacharias' behest. Military joint services support. Looks for scalar changes in Newtonian constant; work on photon redshift. Teaches relativity course. Works on spectrum of cosmic microwave background; interest in gravity waves.

LIGO's origins. RLE support for interferometric detector. 1971 funding for 1.5-meter prototype. Changes at RLE. Interest of Max Planck Inst. for Grav. Physics. Gravity-wave work in early 1970s: Kip Thorne and William Press at Caltech. Chairs NASA committee on space applications of gravitational work; meets Thorne in Washington; discusses MIT/Caltech collaboration; recommends Ronald Drever. Meets Thorne and Drever, Perugia; misgivings about Drever. 1983, joint LIGO presentation to NSF. Drever wants his own LIGO project, threatens to undercut MIT-Caltech project. Difficulties with MIT administration. Troika (Weiss, Drever, Thorne) unworkable. Comments on Caltech project mgr. Frank Schutz. Site-selection process. Drever's troubles building 40-meter prototype. Richard Garwin, IBM, pressures NSF to conduct LIGO study; Weiss organizes it in Cambridge, 1986, chaired by Andrew Sessler. Thorne and Weiss want director to replace troika. Selection of Rochus (Robbie) Vogt, 1987. Vogt's management style. Attempt to site LIGO at NRAO in Green Bank, W. Va.; selection of Livingston, La., and Hanford, Wash. Livingston's drawbacks. Misgivings about Vogt's directorship. Vogt's conflicts with Drever; Drever removed from project. Caltech faculty critics of Drever removal. NSF dismisses Vogt.

Administrative information

Access

The interview is unrestricted.

Preferred citation

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH RAINER WEISS

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Rainer Weiss **by Shirley K. Cohen**

Pasadena, California **May 10, 2000**

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COHEN: Why don't you tell us a little bit about your background, your family, what your parents did?

WEISS: Well, I was born in Berlin, Germany, in 1932. My father was a doctor, and later on he became a psychoanalyst, in New York.

COHEN: When did he leave Germany?

WEISS: Well, I'll explain that. He was Jewish; my mother was not Jewish. And they met each other in 1931 or '32. I was the product of that meeting; they were not married yet.

COHEN: An encounter.

WEISS: An encounter. My father came from quite a wealthy family, but he became a rebel and a Communist. That figured very strongly in his life. My mother was a rebel also, but she became an actress, and they hitched up somehow. And when he got in trouble with the Nazis, before they came into power in Berlin, there were already enclaves in Berlin that were Communist. Some parts belonged to the Weimar [Republic] and other parts belonged to the Nazis. I mean, it was total anarchy in the city.

My father was a doctor, and he got involved, in a workers' hospital, with a bad operation, which was done, as it turned out, by a Nazi doctor who had been planted in the hospital. My father reported on him to a medical board, and the Nazis wanted retribution. So they captured him and put him in a cellar in Berlin. And my mother, who was pregnant with me—

COHEN: This was even before the Nazis came to power?

WEISS: Yes, this was 1932 and they were not yet in power. And what happened was that my mother's father was a judge—still a part of the establishment. Because of that, he was able to get my father out of this jail. But then they took him and pushed him over the border into Czechoslovakia. So he left in 1932, and she joined him later that year. So they were living in Czechoslovakia when my sister was born, in '36. And at that juncture my father decided to go to Russia, because, you know, he was a good Communist. Well, he was slowly getting smarter about it. My mother wouldn't join him there, that was the thing. They had established themselves in Prague. I was a kid—six years old. I remember going on vacation with them in the Tatra Mountains, and I was listening to one of those gothic-looking radios—you know, those old wooden radios?

COHEN: Sure.

WEISS: And out from that radio came [British Prime Minister Neville] Chamberlain, explaining how we were going to have peace in our time. And lo and behold, here are a whole bunch of expatriate Germans, many of them Jewish, who just took off. I mean, they really took off from that place in the Tatra Mountains and tried to get to Prague, to get the hell out of Czechoslovakia before this thing was consummated. In fact, we all were involved in that.

COHEN: You left at that time?

WEISS: We left at that time. So we came to the United States. We were very lucky that way. My father being a doctor is what got him out, because a lot of people didn't get out. We were sponsored by the Stix family in St. Louis, a big company—to get us out. Many years later, in fact, I went and talked to the lady Stix and thanked her for this. Of course, she was very nice—very sweet—but she didn't know who the hell I was.

Anyway, so we got to New York in January of '39. Then my father became part of the Karen Horney group in New York. He had a terrible time getting an American [license]. He couldn't become a doctor. He couldn't understand a [multiple-choice exam]. You know, in Germany you don't have [multiple-choice exams]. Here, there was an exam given and you had to pick an answer—you had four choices. Well, he didn't get the idea that you had to pick one of those four choices, because none of them was quite right for him, so he would write all over the exam paper. He was totally undisciplined in this regard. Well, of course they wouldn't read that, so he didn't get his license to practice medicine for about eight years after he got here. And my mother supported the family.

COHEN: By acting?

WEISS: No. She did every odd job you could imagine.

COHEN: Just whatever she could.

WEISS: Whatever she could, right. And my sister became a playwright—first an actress and then a playwright. She's actually made a life here; her work has been performed here at the Taper [Mark Taper Forum].

Anyway, so my father then became a psychoanalyst and joined the Horney group. And I went to public school in New York and then went to a private school in New York called Columbia Grammar School, which Murray Gell-Mann had gone to. He was several years ahead of me. I was always being compared with him. You know: "That guy really knew something. You're just a bum." That kind of thing. [Laughter] I had a scholarship to that school; we didn't have the money to do anything. I don't know how it happened. So I graduated from Columbia Grammar School and then went to MIT. I wanted to become an electrical engineer, because I had run a business, as a high school kid, in hi-fi. I started building—it was at the beginning of high-fidelity radio, high-fidelity FM radios. In 1948 or 1949, there was a very famous circuit called the Williamson circuit, which I was building for people. There was a big fire in a Brooklyn movie theater, and I collected some of the loudspeakers from it.

COHEN: [Laughter] So you were really an entrepreneur.

WEISS: I had become an entrepreneur. I had my following—all these immigrants who wanted hi-fi; once they'd heard it, the demand grew by word of mouth.

COHEN: So you built these systems for them?

WEISS: I built these systems for them. And of course, if I had stayed and not gone on to college—

COHEN: You'd be rich. [Laughter]

WEISS: I'd be quite wealthy, yes.

COHEN: But you went on to MIT.

WEISS: I went to MIT—I wanted to learn how to do audio engineering well, because that was all I knew. But I very rapidly realized that I didn't want to become an engineer.

COHEN: So you switched to physics?

WEISS: I switched to physics, and I don't know why…. No, I'll tell you; it was really stupid. The [Physics Department] had fewer requirements than the others, and I was totally undisciplined—I didn't want any requirements. Well, the next thing that happened was that I fell in love with somebody. This was at the height of the Korean War. Like an idiot, I decided that I was just going to take off, and I flunked out. I chased this woman to Chicago. She was a pianist. But she changed my life, by the way. I had never thought of a lot of that stuff, and that's why I started the piano at twenty—or later, I guess. It was because of her. I was totally ga-ga, crazy in love. I didn't think of what the consequences of that would be. Of course, the girl went off with somebody else. You can never fall in love—I mean, you're not allowed to do that.

COHEN: Yes, yes.

WEISS: You know how it is. So I came back. And this was the beginning of my physics. I had such a bad record, having flunked out.

COHEN: MIT took you back?

WEISS: Well, I walked into Building 20 and looked in at the various little labs. There was a bunch of people doing something that looked to me to be sort of interesting, and since I knew all this electronics, I asked them, "Look, can you use a guy?" And I sold myself off as a technician for about two years.

COHEN: I see. So you weren't a student then; you worked as a technician.

WEISS: I came back as a technician. Then Jerrold Zacharias, who's my alternate father in all of this—I happened to land in his laboratory, and that was a very lucky thing, because Jerrold was working on high-precision atomic beam experiments. He had worked with I. I. Rabi at Columbia, and at the time was beginning to work on atomic clocks. And I was given the job of working with him on stuff that he wanted. He and I got along really well. He was very busy doing stuff for the military; he was a very important guy on all these committees. So he would come in with an idea once in a while, but after a while I was running his experiment, even though I wasn't anything. Well, as an experiment it didn't work, but that's another matter. He had developed the atomic clock, the cesium clock. And he had commercialized it. It's funny—Zach had gone into business. But he felt he needed it for his physics. He wanted a stable supply of people who could make atomic clocks. So he gave it to a company and got it going—the National Company, in Malden, Massachusetts. Eventually it got bought out; I won't go into the whole train of events, but it got bought out by Hewlett Packard, and now you can buy Zacharias's clock in a little box.

COHEN: The clock that you developed.

WEISS: Well, I was part of the development, but I didn't know what I was doing. The thing I worked on with him was something that didn't work, but [it was] a very important piece of my life. And that was that he wanted to make a clock that was even better than the one he was, by that time, commercializing. He wanted to use the clock to [demonstrate] the Einstein redshift. This is the fact that clocks of different heights go at different rates. Well, that was my introduction to gravity, you see.

COHEN: So it started way back then.

WEISS: And Zacharias's idea was to make a clock that was so stable…. The idea was—a very cute idea, but it just didn't work—to launch cesium atoms. They would go up and do a parabola, so they would fall down like a baseball. Then you'd have a very long time to watch them—that would be the secret of making a very good clock, the long time you had to look at them. By that time, I knew enough physics [to know] that it was going to be hairy, because the average cesium atoms, at room temperature and in a vacuum, will rise to about one and a half kilometers before turning around and dropping. Zacharias and I were hoping that there would also be slower cesium atoms, as predicted by theory, which would need only a few meters to rise and turn around; they would be moving at about one-twentieth of the average velocity, in other words. We were dealing with the very lowest end—the low-velocity end—of the Maxwell distribution of atoms. And this is the joy of Building 20. What we did was, we started with the apparatus being the height of one floor. We had the best vacuum you could make, so that the slow atoms wouldn't get hit. We had an attenuation of atoms that were supposed to do this—that is, not one out of 10^{12} atoms would get over to the detector. It was an incredible attenuation. So then we decided, "Well, hell, we've got everything working. We can extend the apparatus. We can drill a hole in the ceiling." And so we went to the next floor, with a longer [vacuum] tube. And that should [have given us] I've forgotten how many atoms—it's the square of a number. The number of atoms we should have gotten should have gone to the square of that. But we still didn't see anything. So we drilled a hole in the next ceiling. Eventually we had a three-story vacuum apparatus and we never saw anything. By that time, Jerrold Zacharias was fed up with the whole business. *Sputnik* had happened [1957], and he got very interested in the business of teaching and science education, and that's how he became famous. He ran the PSSC [Physical Sciences Study Committee], which is—

COHEN: Oh, OK. There was a whole religious movement on that.

WEISS: Yes, yes, yes. Right. But I decided, "Hell, I have to find out why this goddamn apparatus didn't work." So I made some changes, and I immediately discovered for myself that there were no slow atoms in the beam—there just weren't any slow ones—and that this whole idea was crazy. It was sort of a discovery—not a very important discovery. It turns out that here were the slow atoms, perambulating along, and they would get hit many times by fast atoms from behind. So there would be no slow atoms. Well, we lied to people for years about that, in all the physical chemistry books. It turns out that there are no slow atoms in a beam. I explained that to Jerrold and he accepted it. He saw the data. We never published it.

COHEN: You didn't publish it?

WEISS: No. Jerrold's attitude toward the world was, You don't publish ideas and you don't publish bad experiments. OK? He published very little. Well, it later cost me plenty. [Laughter]

COHEN: [Laughter] So, by then had you gone back to school?

WEISS: I then became a grad student.

COHEN: At MIT?

WEISS: At MIT. It was the only place that would take me. I mean, with a record like that, you couldn't go anywhere else. I graduated, finally, in '55 or '56 and got into grad school. It was during the time of that experiment—somewhere in there—that I made the transition from being a technician to being an [undergraduate] and then a grad student. And with that, I became very close to [Zacharias] and his family. I became an alternate son, effectively, of the family. I loved the man. He was a wonderful person. By the way, this experiment, which is called the Zacharias fountain, is now working on a scale this big. It's a little, tiny thing only a few centimeters tall. Because you can cool atoms by lasers. So people have used this, and now you find that the literature is full of Zacharias fountains. But, you see, we weren't right. The thing was a little too early.

Anyway, so what happened to me then was that I continued doing clock research, mostly because that's what I knew. But I got very interested in gravity and relativity. I had so much fun as a graduate student. I got married in the middle of all of that, and my wife got pregnant, and that's finally what ended it. I had to get out, OK? But I would have stayed a graduate student forever, because it was fun. I could go from one experiment to the other, and I never thought about money or any of that stuff, so I did one experiment after the other. Some of them were pretty zany. But all of it was virtually just following my nose—it's what I wanted to do. [Zacharias] was no longer in the lab, directing it. I finally did my thesis on something very boring. I was thinking of how to make better and better clocks using molecules and stuff like that. But in the middle of it all, MIT finally decided that I had been there too long. And Zach got me a job as an instructor at Tufts University.

COHEN: Not too far away.

WEISS: Up there in Medford. I became an instructor there, as a graduate student, and I set up a lab for them and taught electricity and magnetism. And the following year—just about at the end of my thesis—they made me an assistant professor, because I was apparently doing a fairly decent job for them, so they were hoping I would stay. And I decided I wanted to go work for Bob [Robert H.] Dicke, at Princeton.

COHEN: Was he working on gravity things?

WEISS: Bob Dicke, at that time, had just become—oh, he had done something quite remarkable. There have been two geniuses in my life, and I don't say that lightly. One of them was Ed [Edward M.] Purcell, who was at Harvard, whom I got to know fairly well later on. He's my hero: He's an experimenter, but he knows physics. He's just a marvelous person. And the other was Bob Dicke. He was more peculiar and more idiosyncratic than Ed Purcell, and not as good a teacher as Ed Purcell. But what stunned me about Bob Dicke was that he was both a theorist and an experimenter. I feel that you should never get into a situation where you do only one thing. By that time, Dicke had become quite a serious physicist. And I felt that if you wanted to do physics, you had to know the theory *and* the experiment. Doing theory alone was dishonest. You were farting around—in effect—with ideas, and not doing anything. So you had to do an experiment—but you shouldn't do an experiment without understanding the theory. I don't know where that [notion of mine] came from. It probably came from Jerrold; he had a very rigorous way of looking at the world. So Bob Dicke was my first experience with somebody who did his own theory.

COHEN: What position did you have when you worked with Bob Dicke?

WEISS: I was a postdoc. Oddly enough, it's never occurred to me that it was the beginning of my doing gravitational radiation work—but I wouldn't have thought of that until [later].

So I got there. Bob was at that time interested in a theory of gravity, and he had decided that Einstein relativity was something that was very poorly founded experimentally. And that probably was true. I mean, he was looking at all the things that had happened in relativity after the invention of relativity and [he] realized that the experimenters had never really gotten into this. Sure, there were a few things that validated relativity the way Einstein thought about it. But Bob felt that there were experiments you could do in a modern way that could validate or invalidate the theory. I got to Princeton at a time when that was his main idea, and that's, of course, what attracted me to him. He was thinking of a scalar theory, a theory that is a little different from Einstein's. It incorporated certain ideas better. And he combined the scalar theory with a gravitational tensor field. One of Bob's big problems was that he didn't do a hell of a lot of going to the library.

COHEN: Oh. So he didn't know what other work was going on at the same time?

WEISS: Well, not only that. Actually, he probably knew what was going on at the same time, but he didn't know what had gone on in the thirty or forty years [before that]. And that got him into trouble a few times. Because scalar theories had been invented immediately after special relativity, and a lot of the things that Bob had invented were actually features of this theory, which was wrong. But he knew it was wrong, so he made a theory that was a combination of Einstein's and scalar, which was plausible; in fact, it became one of the alternate theories. And this was a job he gave me when I came to Princeton. I didn't know what he was going to do with me. I was a fairly decent experimenter by that time, so I knew that I was going to be doing an experiment, but I didn't know what. You know, you didn't bargain in those days. You went. So I was given the job of building an experiment to look for the earth receiving gravitational waves of a scalar variety. Bob's idea was that the earth would be driven by scalar waves to be excited in one of its normal modes. A scalar wave is completely spherical. We talked earlier today about Einstein and his mistake; well, scalar theories are exactly that. What happens is that the earth rings at 20 minutes—we all move in and out, everywhere on the earth. The earth oscillates at 20 minutes. With a very low loss, it rings for something like years after it gets hit at 20 minutes.

So I built an apparatus with a guy named Barry Block, who just died. Barry was a theorist, but he enjoyed tinkering, and he was at Princeton and MIT—we had met each other at MIT. Barry was blowing quartz fibers. He enjoyed making jewelry. He was a nuclear theorist, but he enjoyed doing things with his hands, but only very special things. He enjoyed making quartz fibers and springs and little jewels out of quartz—you know, fused quartz. He had made a little gravimeter, and he didn't know what to do with it.

COHEN: A what?

WEISS: A gravimeter. It's a device that measures gravity. And Bob Dicke said to me, "Look. I'm interested. Does the earth ring after excitation by scalar waves?" So we set up an experiment, Barry Block and I. In the end, it was just me, because Barry got bored with the experiment. You know, he enjoyed making the jewels. So to look for the scalar waves—and the idea was to measure this 20-minute oscillation in the earth, which we knew about because of Frank Press, who was here at Caltech and who had measured some of these normal modes. I read his papers. He came to MIT later on. And then you had another guy here—Hugo Benioff. Benioff was also at Caltech. He had actually been the first to measure these normal modes of the earth. And we were going to exploit that normal mode of the earth to detect these scalar waves. Anyway, I set up this whole experiment in a pit that had been used for another gravity experiment before that.

COHEN: Is this—?

WEISS: At Princeton.

COHEN: Yes. Now, was Joseph Weber doing his stuff yet?

WEISS: Well, let's see. Joe Weber was beginning to think about it; in fact, Joe was at Princeton at that time, and he was close to Johnny [John Archibald] Wheeler, and they were hatching this idea of looking for gravitational waves of the Einstein variety. I think I came at about the same time. I came down there, and Bob suggested that I look for scalar waves, using a gravimeter.

This was about 1962. I set the experiment up. My wife had family in Philadelphia and Washington; every once in a while we had to go visit. And this was Easter, 1964. Well, I wanted to make sure the experiment was OK, so I went to the pit—this little hole in the ground where we had this apparatus—and they were going to go on to Washington. So they went off, and then I went off to Washington and came back a little after Easter. And lo and behold, that goddamn machine was just jiggling all over the place. What had happened was that they had had a huge earthquake in Alaska. [It was] one of these deep-focus earthquakes—the famous Easter earthquake. It registered 8.7 or 8.8—a huge earthquake. Well, it had set the earth [oscillating] like a bat out of hell, and it ruined the experiment. We probably got a couple of months of data without a big earthquake, and we could set a limit on the amount of scalar waves exciting the earth. But after that, it was very hard to see anything but the residual of that earthquake.

COHEN: How long did it make the earth vibrate?

WEISS: Forever. I had to leave Princeton before it quit. I came back to MIT in about ’65. Zacharias had called me. Well, I wasn’t super happy at Princeton. I don’t like the weather there. [Laughter] And I loved New England: the weather was crisp, and you didn’t have these horrible, humid.... It was little stupid things like that.

COHEN: Well, that’s how one decides these things.

WEISS: Yes. So I came back. Jerrold asked me to come back, and he told me I could do anything I wanted. There was this very nice thing that MIT had, called joint services support. And what it meant was that MIT’s Research Laboratory of Electronics [RLE], which was where Zacharias had his lab, was supported by a military grant from the army, the navy, and the air force—the joint services. The support came with no strings or directives from the military except that the money had to be used to train scientists and engineers and do interesting research. This was a legacy of the Second World War, when the military research was hampered because the country didn’t have enough trained scientists and engineers. It was one of the major attractions of MIT—that you could come and get some support without having to write a proposal.

COHEN: Was it classified work you were doing?

WEISS: No, no! The military was absolutely the most wonderful way to get money. Their mission at that time—and that's something that's grossly misunderstood by all the people that got into trouble with Vietnam and everything else—the military was in the business of training scientists. They wanted not to get caught again the next time there was a [need for a] Manhattan Project or a Rad Lab [MIT Radiation Laboratory].

COHEN: They were far-sighted enough to know they had to train scientists.

WEISS: They had to train scientists, and all they wanted to do was train good scientists, they didn't give a goddamn what they were going to work on. And this was completely corrupted by the Vietnam War, OK? But this is now '65, '66, and it had been going on before that, too. The military, through this joint services support, was supporting all the science in the country, or much of it.

COHEN: Well, I know the ONR [Office of Naval Research] supported science.

WEISS: Yes, the ONR was the central feature of that. Immanuel Piori—later an IBM vice-president—was responsible for setting the policy for the ONR.

So I came in. And I had this cockamamie idea that I wanted to do something to look for changes in *G*—big *G*, the Newtonian constant. And the idea I had was crazy; it was the wrong idea. It was to make very good atomic beam gravimeters, things that would absolutely measure little *g*—you know, the thing that pulls you to the earth—and also start measuring the shape of the earth very carefully. The idea was to measure little *g* and measure the shape of the earth by using laser interferometry and thereby find out if big *G* was changing. And there was this idea that big *G* was changing because of a scalar field—this is a residue from Bob Dicke. And also the idea that [British physicist Paul] Dirac had had—that maybe the Newtonian constant *G* was a variable. And that had been coupled to the expansion of the universe. It was sort of a grand idea, that it was one of these numbers that was dependent on cosmology. The scale was a parts-in-10^{10}-per-year kind of thing. So I embarked on a program with military money to look for the scalar changes of *g*. It was a pretty crazy program. But in the process, there was a lot of fallout from that. The very first night, we realized we had to stabilize the laser. Lasers were new to me. They were brand-new at the time. And I had worked on atomic clocks. I said, "Hell, we ought to apply the techniques of the atomic clocks to frequency-stabilize the laser." And we did. We built our own lasers; in those days you couldn't buy anything like that. I had a very good student named Shaoul Ezekiel. Shaoul Ezekiel was my first graduate student. By the way, he made a life out of laser stabilization later on.

COHEN: Shaoul Ezekiel. Was he Arabic or something?

WEISS: He is a Jew, an Iraqi Jew. He and I tried to find an atomic line or a molecular line that you could use as an absolute reference to stabilize a laser, much as you did with a cesium line to stabilize a microwave oscillator. And we eventually hit on something, and in fact found iodine molecules that could be used to stabilize the argon laser—within rotation vibration lines. And we found several matches between the argon-ion laser, which was just then being developed. We then built our own argon-ion lasers, and these iodine lines, and that set off a whole research program that Shaoul—we called him Ziggy—took off with him. He did this with me, and then he became a professor in the Aero Department there, right away. He was probably only a few years younger than I was; he was a late graduate student. So he was ripe to start a whole research group. He started with us and he made a whole life out of this.

I had learned about the 3-degree [cosmic microwave] background radiation, because that happened about the same time as this gravity program. And I began to realize an interesting thing. The gravity program—this business of trying to measure *g*—of course, this laser stabilization was a nice offshoot, but it wasn't going to produce a hell of a lot of very good science very quickly. And I was being told by people in the department that they were beginning to worry about what the hell was going to happen to me. They figured that this program that I had started was so long-range that maybe I should—

COHEN: Do something with more immediate results?

WEISS: More immediate results. And I'm not the type of guy who takes advice like that, OK? Because I'm going to work on a problem that's important; I don't give a goddamn how long it takes. That's part of Zacharias's training, I must say. But on the other hand, I got turned on by two things. Cosmology was beginning to be more and more interesting. We had already gotten a little feel of that with the *g* change. And I had started a thing that was a little offshoot. In fact, it was one of the offshoots of what happened back before I got my degree—that period when I didn't want to ever graduate, you know, the forever student. I had gotten mixed up with a guy named Lee Grodzins, who was a professor at MIT working with the Mössbauer effect. You had [Rudolf] Mössbauer here at Caltech [1960-1964]. But his big stuff he did in Europe. I'll tell you why I know that: It's because I tried to understand his paper, and it was all in German, and he would have written it in English had it been done here. And why I got interested in Mössbauer techniques is because Bob [Robert V.] Pound at Harvard—this is now going back in time—had done the redshift that Zacharias and I wanted to do: the Einstein redshift, with this new and very jazzy technique, the Mössbauer technique, which completely beat out all the clocks. So I said, "Wow! I've got to learn about that."

Lee Grodzins was in an adjacent laboratory. He was a young professor and I was a senior graduate student. I kept going in and trying to figure out what they were doing in that lab. So I suggested, "Hey, let's do a photon-scattering experiment." And he had done some thinking about this. The idea was to see if photons—this is this crazy idea that light from a bright star was redshifted for some reason or another. It was an idea from [Erwin Finlay] Freundlich, an astronomer. I had read about it and Lee knew about it. Freundlich is not a guy you'd make much fuss about. And at the time what was raging was the Steady State cosmology—you know, the idea that the universe was static and there was none of this expansion, so [this photon idea] would solve a lot of problems in terms of making a Steady State universe. One of the hypotheses that came out of this was that maybe the redshift was due to the fact that photons were resident in the universe and that they were being scattered off other photons, and the longer they had to travel, the more they got scattered and the more they got shifted toward the red. That was the idea. So this was a perfect thing to try with the Mössbauer experiment.

So Lee and I set up an experiment where we took this Mössbauer source—much like what Bob Pound had done with the redshift—and we put a tube in there, a red-hot tube. We passed the gamma rays through the red-hot tube and looked for a shift in the frequency, and we didn't see anything. So that was an experiment I did as a sideshow. Then we did it over again—because I knew microwave techniques and Lee didn't—with a big, long microwave cavity. Same thing: we did it again and saw nothing. We published that, as a matter of fact. The whole experiment got published, because Lee was a different kind of individual than Jerrold Zacharias.

COHEN: I see. So that was interesting enough to publish, even though there was not a result.

WEISS: Yes. There was no result, but Lee felt that it was an interesting experiment and he wanted to publish it. I guess he was just starting, a young professor, so to him it was important to publish; it was published in *Physics Letters.*[1] Anyway, I tell that story because, when I came back to MIT—this [had all happened] before I left and went to Princeton—it dawned on me, after we stabilized the argon laser to do this zany gravity experiment, that we could do this again, but this time do it with light and microwaves. I resurrected the old cavity that I built with Lee, and we used the frequency-stabilized light, which for another reason Ziggy and I had worked on, and put that into this thing and—shot-noise limited—we split the living daylights out of the fringe and again established that the Freundlich hypothesis was wrong. But this time we did it with visible light against microwaves. The 3-degree background radiation had just been discovered, and I felt that this was the right comparison to make. The universe was awash in microwave photons, and the effect of the redshift was being seen in visible light. So this was the right experiment to do. And again, we saw nothing.

Now, why do I mention this? Because it's an important experiment in the history of LIGO [Laser Interferometry Gravitational-Wave Observatory]. It was the first experiment in which we actually managed to split the fringe. You know, we were doing interferometry at a very, very high precision, because we were looking at—oh, my lord!—parts in 10^{13}, parts in 10^{14}, trying to get precision of a strain, in effect. And that then moved me into two fields. Bernie [MIT radio astronomer Bernard F.] Burke played a big role in that. Bernie became my mentor. I didn't want Bernie as my mentor, OK? But he imposed himself on me as my mentor.

COHEN: That's Bernie.

WEISS: Yes, that's Bernie's style. And he was, at that time, the head of the division, and he was trying to give me advice. He said, "Look, you're not going to ever get tenure"—and I didn't know what tenure was—"if you continue this way, because none of the things you're doing are of any significance, really. And you haven't published anything—not enough," and all that sort of stuff. "You've got to do something and get published." He looked at what was going on in that lab, and he said, “Why don’t you try to do something with the 3-degree background?” That was his idea, not my idea. And I said, “Yeah, I’ve been thinking about that, but I wasn’t thinking of actively doing anything.” Bernie had already himself done one measurement with one of his students. And I said, “Well, if I can do anything with this, I want to prove that the spectrum is really that of a thermal black body.” And we had developed some techniques in my earlier life as an atomic physicist on measuring infrared stuff. So I got a new student named Dirk Muehlner. And Dirk and I began to look very hard at…. Oh, and there’s another piece of the story—where I had gotten Dirk. You’ll see, it will fit together. It’s just the way the world is.

COHEN: Go ahead. But we’re starting LIGO already.

WEISS: Yes. LIGO got buried in there. It’s buried in there in that crazy experiment to look at the tired-light hypothesis, but it really got started in a different place, and that’s where Dirk came out of. Dirk came to me, and we started thinking about the 3-degree background. But before that, I had been asked by the department chairman…. MIT at that time was very lax about its educational program; they figured that, hell, I had been to Princeton so I must know something about relativity, right? I had come back from a little trip, and it was September, and I was told by the department chairman, who was Bill Buechner—and/or George Harvey, who was associate department chairman and educational officer—“We need somebody to teach relativity. You’re going to teach relativity.” Well, what I knew about relativity you could stick in this finger. I mean general relativity, I’m not talking about special relativity. So I decided that I couldn’t tell him no. I had been teaching a lot of the undergraduate courses. **[Tape ends]**

Begin Tape 1, Side 2

WEISS: So I teach this relativity course. I’m one day ahead of the students, or a week ahead of the students. And in that course there was this student, Dirk Muehlner. Now, the reason why that figures in the LIGO story is because that’s where LIGO got invented, in that course. This was about 1968 or 1969, and I was, as I say, one day ahead of the students. I had a terrible time with the mathematics. And I tried to do everything by making a *Gedankenexperiment* out of it. You see, I was trying to learn it myself. And I had fallen in the trap that we talked about earlier today—about Einstein not understanding his own theory—many, many times over. I mean, in the process of learning it, the mathematics was beyond what I really understood. But I kept trying to understand. And the students in the course were very good—I mean, they knew I was bumbling. But at the same time, it was interesting to them, because I would always try to focus on what I knew about the experiments, and that was a rare thing. You see, people didn't teach a course on general relativity and focus on the experiments. So we did all the classical stuff, because, you know, you can get it out of the books. And we got to gravitational radiation. That was one of the things the students wanted in the course. [Joseph] Weber had begun to think about that: He had written a book [*General Relativity and Gravitational Radiation*, 1961]. And I tried to use that as the basis for teaching that section of the course. It was hopeless. I couldn't understand what Weber was up to. I didn't think it was right. So I decided I would go at it myself. The thing I had learned, which was simple and pristine, was that we could send light beams back and forth between things and measure what was going on with them; that was the only thing I really understood in the whole damn theory. I couldn't understand how forces were exerted on things—it was too complicated. And so I gave as a problem, as a *Gedanken* problem, the idea, "Well, let's measure gravitational waves by sending light beams between things," because that was something you could solve. The idea was that here was an object. You'd put another object here and make a right triangle of objects, floating freely in a vacuum. And we'd send light beams between them and then be able to figure out, "What does the gravitational wave do to the time it takes light to go between those things?" It was a very stylized problem, like a haiku, you know? You'd never think that it was of any value.

So there was the course, and we did a little cosmology in the course. I was very glad when it was over. What I got out of that course was Dirk Muehlner, who came to the lab because he thought that what I talked about was interesting. He was the most brilliant student I've ever had; I haven't had any like that since. But the course brought him, OK? And he wanted to work on something gravitational. Well, it turns out that he knew a little bit of infrared techniques, so he and I decided that we would go into this cosmic background program to measure the peak of the black body. That was really, in a way, a sop to Bernie Burke, OK? Because I would have much preferred to work on something else. But it looked like something that could be done.

So in the course of that, in conversations with Shaoul Ezekiel, who was still at MIT, and with Dirk Muehlner—you know, we would have evening sessions; it was a wonderful lab—I started realizing that this funny example, this *Gedankenexperiment* I had used in this course, actually fit together with that crazy experiment we had done on the tired-light hypothesis. And when I put that together, it began to look like, my god, the sensitivity for that wasn't crazy! You know, you'd just make [the instrument] bigger. And lo and behold, the idea, which was this *Gedankenexperiment*—when you coupled it with the idea of being able to beat down the fringe of the light, which had already been proved—it looked like it wasn't nuts to do that. So I told Dirk about it, and Dirk said, "Yeah, well, it's too hard to do for a thesis." He preferred to go on with the cosmic background. So he and I would work on that, and we eventually developed a whole bunch of techniques for measuring the cosmic background from [high-altitude] balloons, and in fact got ourselves into all sorts of interesting and hot.... Since you want to save time, let's not go into that.

COHEN: Right. Let's stay with the gravity.

WEISS: Stay with the gravity. But that was, in fact, the mainstay of the laboratory. It will keep coming back, because, you see, I had to have a program like that to be able to get students finished. That was the only way that I could actually get a degree for a student, because MIT was quite rigorous. I mean, any technology development was not a way to get a thesis for a student. So we were busy developing the balloon package and the first measurements of the cosmic background with liquid helium tanks—very much what Andy [Caltech physicist Andrew Lange] is doing now with balloons—we developed all those techniques for the first time.

I kept thinking about this crazy business with the [floating] objects and the light traveling between them. I guess I did some of it in Palestine, Texas [site of the National Scientific Balloon Facility]. I did some of it at MIT. But I started doing a calculation—imagining doing that as an experiment, much the way Bob Dicke would have approached the world. In other words, instead of building the thing, I sat there and thought the thing through. That was something Bob taught me. I'll go back to Bob for a moment, because it's important in the history of my own life how he operated. That's when I saw that I could never be the same as Bob Dicke. He, when he invented an experiment, disappeared for about a month, or three weeks. And he would come back with a sheaf of papers—I mean, this fat—and he gave them to all the people. He would have thought through every conceivable thing that—

COHEN: Before he ever started to do it?

WEISS: Before he did the experiment. He was going to measure how round the sun was, that was the thing. He came back, and he had thought through every contingency in the electronics, the optics, the mechanics of this telescope, and he threw this stuff at the three postdocs who were in his group, who came to me and wanted me to help them interpret what the hell Bob had given them. And I marveled at the depth of analysis that he had done to design this apparatus. To me, it was a total marvel, because most of us, when we design an experiment, build the experiment and then find out that we've forgotten this and we've forgotten that, and we make patches for the experiment and eventually it works. With Bob Dicke it was the other way around. He had thought of so many things that you had to effectively get rid of the—

COHEN: Peel it away?

WEISS: Peel it away so that you could get to the structure, and then maybe you would build some of the other stuff he had thought about. It was unbelievable. So that was the image I had in my head. I said to myself, "This is so crazy, this gravity thing. I'll do the same thing." I wasn't consciously thinking this, but later on I [realized] that this is what I was doing. I sat in a little room and thought of every conceivable noise source I could think of—what might interfere with [the detection of gravitational radiation]. I knew that the astrophysics of it demanded a certain level. You know, you had to get down to 10^{-16}, 10^{-17}. By this time Weber, although I didn't pay much attention to it, was already publishing that he had measured gravity waves—with his bar-detector technique, which I couldn't understand. And we could never get straight what his sensitivity was; I mean, that was a fundamental problem with Joe's experiment. In fact, that's what got him into trouble in the end. But if you did eventually figure out what came of it, he was at the level of what we now would call 10^{-13}, probably. In fact, anybody who looked at that experiment after he had made his initial statements, and who knew it, knew that there was so much energy that he was seeing in gravitational waves that he was going to wipe out the galaxy. I mean, all the energy in the galaxy would get stuck into gravity waves over many years. That was just not happening. Aside from the fact that it was a bad experiment, which I didn't know until much later.

So what I did was to conceive of what all the noise terms were that would get in the way.

And that's the thing I put in the RLE quarterly progress report. This was typical of the training I had had from Jerrold Zacharias: It wasn't a completed experiment, it was an idea, and you didn't publish a thing like that. But there was a piece of me that said that it ought to be put someplace, so I put it into the quarterly progress report—a big, long report. And then that was it; we didn't publish it anymore. And in there was the basis of, in fact, the whole damn thing.

COHEN: The whole idea of LIGO?

WEISS: Yes. And I was thinking that there had to be a better way. It was obvious to me that it had to be built on a very big scale, otherwise it wasn't ever going to detect anything. Now we get into the more modern history. I convinced the head of RLE that as a sideshow we ought to build a little prototype to try this idea out: hanging masses, radio frequency interrogation of fringes—a whole bunch of stuff that was already in there and had to be tried. We would do interferometry on suspended masses, which was outrageous; usually one would nail the mirrors down to the ground to keep the structure stable. And so I got support from the RLE; I would now call it generous support. The engineers were better administrators of science at MIT than the physicists were. A guy named Henry Zimmermann, an electrical engineer, was the head of RLE, and he thought this was a charming idea. This was the tail-end of—let's see, the idea came about 1968, 1969, when I was teaching the relativity course. Then, probably in 1970 was when I did these calculations. In about '71, I went and asked for some money from the lab. I never thought of writing a proposal. You didn't write a proposal; I mean, you went to the head of the lab and you asked. So they gave me $50,000, which was a huge amount of money. They scraped it together from someplace, and I bought a lot of stuff to build a 1.5-meter prototype of a LIGO. And we got quite far with it. But I could never put a student on it for long, because, you see, it wasn't going to produce a piece of physics. A whole progression of students came in and I would start them on this, and they would do a little and then they would go on to the balloon program and get their degree in the balloon program, either in infrared astronomy or in cosmic background measurements or something. The gravity wave [detector] was a sideshow and it was a major piece of the laboratory. But it was a sideshow, primarily because I felt it was just no way to give a student a degree. Then, by about 1973—it was only about two years later—[there was the] Mansfield amendment, which had to do with the Vietnam War. There were two of them, actually: One was to declare victory and get out; the other—and this was pushed by my liberal friends—was to make sure the damn scientists didn't help the military to make the Vietnam War go better, and it said that military support should go only to a military mission. That was the beginning of the end of military support. Somehow some people had gotten the idea that the military blocked their scientists and that they were then indentured. And that was a very bad thing, because, you see, they were so angry about the Vietnam War. It was part of the anti–Vietnam War movement. So the Mansfield amendment then was used by the management of the Research Laboratory of Electronics at that time…. Well, it had changed; it was in the hands of the physicists running it. It had become a lab that was now supporting mostly solid-state physics, no longer this laissez-faire kind of thing. And they were using the fact that the military, which was supporting the laboratory, had only a certain mission and could only support certain things. Cosmology and gravitation and radio astronomy [were out]. Well, the radio astronomy stuck. Bernie [Burke] was able to retain his lab, because he was developing detectors. But it was by the skin of his teeth that he was able to keep support from the military—eventually he lost it. But he was strong enough and well known enough. I wasn't, and the stuff I was working on became irrelevant to the military. So I immediately, for the first time in my life, wrote a proposal. I wrote a proposal to the National Science Foundation to support the gravity work—to continue the 1.5-meter thing, that interferometric detector. And I wrote a proposal to NASA to continue the ballooning work on the cosmic background radiation. I got the money from NASA, and that was pretty much what dictated what was going to happen next. But I didn't get the money from the National Science Foundation. What happened was, I got a call from the Max Planck group [Max Planck Institute for Gravitational Physics] in Europe who had been asked to review this proposal, and they were turned on by this interferometric method. They had been working on the bar detectors, OK?— the same method Weber used—and things had come to the end of the line with that. Virtually everybody in the world who had built the bar detector was seeing nothing. The next generation of those would be liquid-helium cooled and much, much better, but this was the room-temperature one. And they were fetching around looking for the next step, and they were really turned on by this idea. So they asked if there were people in my group who were working on it who would like to come to Germany. At the time we hadn't gotten all the way, to where it was functioning. What happened, however, is that they started working on it. I mean, you can't stop people; you can't do that. And the Max Planck group in fact did most of the early development, because they had the money. I was always very jealous of that. They had the money, and they had a large group of very experienced professionals who had been working on Weber's kind of detector. And they went immediately into interferometers—this was about 1974, probably—and I couldn't go forward. So I kept working more and more on the cosmic background radiation, because that's where I got money—having lost the military support.

So the Max Planck group actually did most of the very early interesting development. They came up with a lot of what I would call the practical ideas to make this thing better and better. I won't go into all the different concepts, but they kept up with delay lines, which was a particular kind of geometry. There was in the United States one other guy who was working on this—a student of Weber's whom I knew about. His name was Bob Forward, and he was at Hughes Research Laboratories. And he was chasing an idea. You see, this idea is not just mine. Other people were doing it. [In my case] it was an artifice for being able to teach gravitational radiation in a course; that's how it started in my life. There was a woman who did some research on this which actually surprised the crap out of me, to be honest with you. This was Marcia Bartusiak, who was writing a book about this. [*Einstein's Unfinished Symphony* (2000, Joseph Henry Press)]

COHEN: She's written some other books.

WEISS: Yes. And she did some research on this early stuff and found out, to my amazement, that Forward had gotten the idea from a guy I had talked to. And he claims that it came from me, through a guy named Phil [Philip K.] Chapman [MIT staff physicist 1961-1967], to him. I don't think that's quite accurate. I think it really came from Weber. Weber also had thought of using interferometry as a way of [detecting gravitational waves].

Well, it turned out that later on we learned this amazing thing. Kip [Thorne] did some research and found out that two Russians at Moscow State University had published this [idea] already in the Russian *Journal of Experimental Physics,* back before I had even thought about it. And I didn't know about that. I don't know their names, but there are the two names that appear in some of our writings now. They had a crude idea, which was an idea similar to what Bob Forward and Weber had—namely, using light as a way of measuring the distance carefully. So, you know, [interferometric detectors] grew up in a lot of places. The significant thing I did was to actually see how practical the idea was by [doing] this noise analysis, which I felt was crucial. And that's not being modest or anything.

There was one more piece to it. Kip had had Bill Press [as a graduate student]—and this is how the beginnings of LIGO came to Caltech, OK? It happened this way. The Germans had already started. And Ron [Ronald W. P.] Drever had also been working on these bar detectors, in Scotland, and he had gone to Germany and seen this [prototype interferometric detector] and thought it wasn't too bad. So he was beginning to get interested in interferometry. Just about the same time, the early seventies, Bill Press, at the behest of Kip—whom I hardly knew, and I'll get to that, how that happened, because it is important in the story—was working on a survey of gravitational wave experiments and astrophysical sources that was going to be published in the *Annual Reviews of Astronomy and Astrophysics.* [2] And he didn't have anything about interferometric detectors. I saw this, and the reason I saw it is because I had asked Kip to be part of a committee. I'll get to that in a second, but let me finish with Bill. So I sent the whole proposal—that thing I had written—to Bill and Kip. And I said, "Look, you ought to at least put that into your [article]. I'll never get any money if it isn't put into your compendium of all the ideas." By the way, Kip's book—Misner, Thorne, & Wheeler, you know, that big fat tome—has a problem in it which says that interferometric detection isn't a practical way of doing gravitational wave astronomy.[3] There's also an example that shows that the interferometry is not sensitive enough. So that's probably the reason that interferometric detectors never made it into the Press and Thorne review. Anyway, so Bill Press looked at [what I sent him]. I think he convinced Kip that, my god, there's something here! So as a footnote in [their review], they put in this other technique, this new [interferometric] technique.

In the meantime, what happened is…. Once you get money from NASA, you also have to do a lot of committee work for them. If you get any money from them for balloons or whatever, lo and behold, you get put on this committee and that committee. You have to do your dirty work to get [your money]. Well, I was asked to run a committee for NASA on applications of space for gravitation. I was chair of the committee, and I had on the committee Peter Bender, who later was identified with LISA [Laser Interferometer Space Antenna], which is LIGO in space. And Bob [Robert V.] Pound, the guy who did the redshift. And I had Charlie Misner on the committee and one other guy, Rudy Decher, a NASA engineer at Huntsville who had worked with Bob [Robert F. C.] Vessot on the suborbital hydrogen maser experiment to measure the gravitational redshift. And I asked Kip to be—he didn't want to be on the committee, although he was being supported by NASA—but I asked him to come to be a witness before the committee, and he came. I didn't know Kip then. That's when I first met him. We met somewhere in downtown Washington. Neither of us had a hotel room; we had to share a hotel room, the two of us.

COHEN: A quick friendship.

WEISS: Well, I was a little leery, to be honest with you. I didn't know the guy. He looked funny as hell to me. I probably looked funny as hell to him. I didn't know what the hell was going on with the guy. But anyway, we spent all night—I don't think we went to bed until about four in the morning. Kip was busy at that time thinking, "What should Caltech do as in an experimental way in gravitation?"—because he already had this very good theoretical group there. And he was actively thinking. We made a huge map on a piece of paper on this table in this hotel room, of all the different areas in gravity. Where was there a future? Or what was the future, or the thing to do? And I wasn't trying to sell him on it, but Kip came to it himself. He decided that the thing they ought to do at Caltech was, out of all that stuff, interferometric gravitational-wave detection. It looked like the most promising thing. Not more than three or four months before that, he had never even heard about it, because it came through Press, you know. So then there was a big discussion. And Kip was dancing around this problem of "Well, I can't do it by myself. Whom should I get?" And he had been toying with the idea of getting [Vladimir B.] Braginsky to come and work on bar [detectors]. But this, I think, turned him around. And I suggested Ron Drever to him.

I didn't know Ron Drever from a hole in the ground, but of all the people who had written stuff on this thing—and I was beginning to read the literature a lot more—he had come up with what I thought were some very clever ideas. And he had done the Hughes-Drever experiment, which was an experiment I knew about. And it looked to me like he was a real comer. So I suggested Ron, and then Kip asked me if I was interested. And I think I may have told him at the time that I was probably not qualified, as he found out for himself later on. Because I knew enough about my own history.

COHEN: This is your nonpublishing—?

WEISS: Exactly. And I'll tell you a very cute story about that that's related to this. Kip persisted, and he did ask me to apply. So I sent him what I had, and he sent me back a note after I sent him my CV, and he said, "There must be pages missing, aren't there?" [Laughter] Well, that was that. I figured, "Don't even pursue it." What then happened is very interesting, and this is really the beginning of LIGO. The NSF [National Science Foundation] had started to give me a little money. It was quite clear to everybody that Caltech was going to make an effort in this direction, and they were going after Ron. The NSF suddenly realized—this was in '76 or '77—that maybe this was going to go someplace. The trouble with Weber and that whole history was really very serious to them, OK? And Richard Isaacson [NSF program officer for gravity-wave physics] had just come into the NSF, and he had talked to his predecessor, and there was a bunch of proposals that they weren't able to fund. One of them was mine. His predecessor, Harold Zapolsky, who was at Rutgers, said to him—Rich has told me this story—"This is a proposal you ought to look at." Rich was, at that time, the head of theory—he's a theorist himself—but he was also giving money, a little money, to the people trying to do gravitational radiation work. So he then gave us some money. I couldn't spend it well enough; I don't remember the reasons. There were reasons. I still couldn't get a degree to a student, and I was slow getting started again after [my military funding] got shut down. I decided, because of the ambiance at MIT, that the only way—because I had students and I could never keep them on this thing—the only way I could ever get a student on this thing was if there was going to be a piece of physics coming out of it. That's the only way my faculty would accept it. I had tried with one student to get him a degree on this thing. The kid had been shit all over by the other faculties. In fact, some of the theorists said, "You've measured what?" And he said, "Well, I've measured the strain of 10^{-12} or 10^{-13}." I remember one guy on the faculty—I won't tell you who it was; I don't think it's important—saying to me and to the kid, and this was terribly embarrassing to both of us, "Well, I can look out the window and [see] that the sun hasn't exploded. What the hell do I need this for?" This kid had really worked very, very hard on some of the [interferometric] technology, and I thought [that reaction] was terrible. It convinced me that the only way I could make any progress with this at MIT was if it would go into a regime where it was sensitive enough to do science. So I asked the NSF for more money to do a study of how much [an interferometric detector] would cost, where we would [have a] site, what we would do, and how we would build [an instrument] that was on a scale that would make a scientific measurement. And Isaacson was in at that time, and they funded me.

This was in ’77 and ’78; this is the real beginning of LIGO as a project—at MIT, at least. And I made a consortium out of Stone & Webster, which is a big engineering house; Arthur D. Little, which is also an engineering house; and people at MIT. We had enough money now so that I could hire two people. Peter Saulson was on it—he became a professor at Syracuse University—and then a guy named Paul Linsay, who came from [Barry] Barish’s place here at Caltech. [We] started in late ’78, and by the beginning of ’82 we had much of the study done: it looked at how you make the vacuum system; it looked at the sensitivities; it looked at a whole bunch of problems of how you make a LIGO, and places to put it, and so on. A moderately well-done [study]. The NSF, by this time, because of Rich Isaacson’s urging, was working on Marcel Bardon. He was the head of physics at the time, and he and Isaacson were friends. And Marcel was very eagerly awaiting this study. And the way that it was proposed in the NSF at the time was that it would be open to all people in gravity-wave detection, and there would be a big meeting that followed the finish of [the study]. There would be a proposal, generated by the people who were interested, based on the information in it. So this wasn’t MIT’s project. I knew—because I was already, by that time, involved in COBE [Cosmic Background Explorer]—I knew what a big project was, and I knew that you couldn’t do this yourself. [You had to have] a large group pushing for it. That was obvious.

So my proposal was written with that intent. In fact, at the end of the proposal was a [recommendation for a] meeting of all the people in the bar [detector] business, anybody who was interested, and that group would develop a proposal. By that time, Drever had been hired by Caltech. I ran into Kip and Drever in Perugia, at a meeting of the General Relativity Society. My son went with me. It was the first time I had ever taken my son anywhere. He was thirteen or fourteen. And I remember the Germans were there, the Scots were there, the Drever group, I was by myself, and Kip was there. And we began to discuss the idea of how they would join in at the end in this study. Now, Drever had, I think, not quite yet agreed. He had come to Caltech, and he started setting things up, but he didn’t agree for a long time that he—

COHEN: He had a five-year period [in which he spent half his time at Caltech and half in

Glasgow].

WEISS: Yes, that's right. And so Benjamin, my son, went with me to a hotel room in which Kip, Drever, and I—and Kip, by this time, had been working on me, saying, "Look, why open this up to everybody? The bar people are not interested in this thing. And we're very interested." He wanted to make it not a multi-university thing but a two-university thing, Caltech and MIT. And here's where I'm a little shaky. I don't know why I agreed, but I did. In part I agreed because—well, I had this tremendous respect for Kip. I still have that—a love and a respect for him. He suggested this, and I thought it would be—I don't know. It had to do with my personal relationship with Kip, in a way, OK? I didn't know Ron at all; I didn't know how complicated he was until that night at that hotel. And then I realized, all of a sudden, that I was dealing with a person who was totally off the wall. I kept telling him about this plan and how we would have to do this together, and he absolutely resisted. He said, "I didn't come to Caltech to work with you. I want to do my own thing. Why do I have to work with you?" You know, that kind of thing. That kept up all night long—I mean, for most of the evening. And my son was sitting there, and he couldn't believe his ears. And Kip was trying to calm the thing down. And Ben told me later, "What are you trying to do? This guy doesn't want to work with you. What the hell are you trying to do?" So I said, "Well, he can't do it himself. I can't do it by myself. This is just too big a thing. We've got to figure out a way to do it together."

Well, that problem never really ended. I may have a year screwed up here, but anyway now what happened was that the report had to go in to the NSF. Kip convinced me that it ought to be a Caltech-MIT [joint report]. And we made a presentation—in October of 1983, I believe—finally. I was scared to death of this whole proposal, which was presented as an idea that would cost about $70 million—an outrageous amount of money; that's what came out of the industrial study—but was a very different kind of LIGO. It had two sites, but it was bare bones in every other way. And Ron [Drever] was drawn into this by Kip, kicking and screaming. He wanted no part of it; he wanted to do his own study. He wanted to do everything himself. Didn't want to be drawn into this thing. And Kip tried to convince him, although Kip didn't know that much about how big science gets done either, that it wasn't going to be done by him alone.

COHEN: Well, the NSF essentially told you that.

WEISS: Well, no. That's where the story's all screwed up. Kip and I don't agree on this; you're going to get a different version [from him]. I knew it couldn't be done by one institution alone, OK? I had to convince Kip that that was the case. And I convinced Isaacson, who had been willing to go along for a while with retaining separate efforts, that that was not salable. It was simply not salable. And so Rich finally—and Marcel was beating on him—said, "Look, this is ridiculous. You can't have a fight at the beginning of this thing already." Kip makes it sound like it was a shotgun wedding at that point—or Robbie [Rochus E. Vogt, LIGO director 1987-1994], or whoever. But [the wedding] was inevitable! I knew it. It was ridiculous to think otherwise, and the NSF said so in the end. It took them a long time.

But we got through that review. We got through the review even though we had no good management plan. It was a very good idea, and we got support from some very important people. I later learned—in fact, just recently—that a guy who gave us enormous support was Stan Deser, who's a theorist at Brandeis. It was all orchestrated by Marcel Bardon. Marcel had taken Deser aside and told him, "Look, I want you on the [NSF] physics advisory committee for a special purpose." [Stan] told me this just about a month ago. I ran into him for other reasons, and it turned out that he had been put on that committee to [get] this thing. The whole idea was something that [the NSF] wanted, and it was quite clearly being orchestrated by Marcel and Rich. Every move of the way was being orchestrated within the NSF, and they were just hoping like hell that we would behave ourselves and do the right thing. And if we didn't, they would try to coach us. But they were very sensitive about that. They didn't try to tell us how to propose. But they would grease the way in certain directions, and hopefully we would follow those directions. And this is when I began to realize that the NSF wanted this something terrible.

COHEN: Why do you think that? Why did they want it?

WEISS: I can tell you why; it's very straightforward. It was Rich. Rich had done some work of his own on gravitational radiation theory. You see, there was a big question at one time as to whether gravitational radiation existed at all in general relativity. This is way before the Taylor-Hulse object. The question was, "Would the radiation leave its source?" And Rich did a calculation that showed it would.

COHEN: I see. So he was really interested scientifically.

WEISS: Sure. He's a scientist. And on top of that, nobody else supported gravity. Here was a wonderful little niche for the NSF, which was something they could do alone. They didn't have to worry about NASA, they didn't have to worry about the DOE [Department of Energy], it was their thing.

COHEN: I see. OK.

WEISS: And Marcel was totally turned on by Rich. Rich would tell him about all these great things that would come from gravitational waves; I think he may have exaggerated a little bit. And he gave Marcel every book in the world that was ever written on the popularization of gravitation. And when Kip finally wrote his books, they were used by Rich to convince everybody at the NSF that this idea was great. It was quite obvious to me that Rich wanted it, he convinced Marcel, and Marcel saw this as an opportunity for something unique and interesting. And, by god, a lot of them were thinking of Nobel Prizes. OK? That's the sin in this field, if you want to know the truth.

COHEN: The sin?

WEISS: Yes. I think it's the key thing. That is, if I have to pick a thing that made Ron Drever so impossible, it would be that. That's my own take on this. I tried to accuse him of that in Washington once, but he'll never admit it. And the NSF would talk about this being one of the things that would happen. If this happened, it would be a new field, and consequently they would be responsible for a Nobel Prize in physics. OK? And that's very important to an agency. So I think that's the background.

All right. Where were we? OK, we have this rickety arrangement that's barely holding together between Caltech and [MIT]. It's really not between Caltech and MIT, it's between Ron and myself. And Ron is trying to do it himself. He privately goes to JPL [Jet Propulsion Laboratory] and tries to have a study done that's a rival to the one he had already been a part of, which we had presented [to the NSF] together with Kip—the three of us together. He clearly wanted to become independent. He just didn't buy it. In the meantime I go to my management at MIT. You'll laugh [at] who it is. It's John Deutch.

COHEN: Of computer fame.

WEISS: Yes, of computer fame. John Deutch, who was dean of science, and Francis Low, who was provost. And I tell them about this project that's been invented. I go to them and say, "Look, the NSF's very much behind this." They don't believe me, of course. "And I need something from you." I go to John Deutch and say, "I need a project manager. It won't be for long. I need the support of a project manager, because I don't have the money now. The NSF isn't ready; it hasn't gone to the National Science Board. But on the other hand, if we don't have a project manager, there's no way to hold this together. And what will happen is that by default it will go to Caltech." So they just screw around with this. By the way, I get thrown back and forth between [Deutch and Low], and nothing happens. It's a measly $50,000, or something like that; at that time, that's what a project manager might cost. But I didn't have the money for it, all right? And what happened very quickly at Caltech is that when they saw that MIT was faltering—I mean, Caltech can act very fast when they want to; MIT can't. What happened is that they found a project manager [at Caltech]— Frank Schutz—because of the connection with JPL. So what happens is that MIT does nothing. They're very glad when Caltech jumps in and effectively takes over the project. And I got mad as hell. OK? You might as well know it. And I've been mad as hell about those people forever.

COHEN: You're mad at the MIT people.

WEISS: Of course. Not the people at Caltech. They're the saviors of this thing. I'm really bullshit at John Deutch and Francis Low. They just absolutely let this thing go. And I asked Francis after the inauguration of [the Hanford site]. I mean, Claude Canizares [director of MIT's Center for Space Research] came to me, or [MIT physics professor Arthur K.] Kerman—one of the two—and said, "Why is MIT not represented here?" And I said, "Because we gave it away."
[Tape ends]

Begin Tape 2, Side 1

WEISS: We also presented our case in front of the physics advisory panel of the APS [American Physical Society]. Every ten years there's a big study of physics. And [Princeton physicist]

Dave Wilkinson, whom I know well and who was with me on COBE, was running the section on gravity. Kip, Drever, and I made a presentation there, and we got an endorsement from [the APS], which was very important. And, by the way, Wilkinson got turned on at that time. He came to me at one of the COBE meetings—and this is relevant to my staying at MIT; this was about 1984—and said, "Look, why don't you come to Princeton?" He had heard about MIT not wanting to do this, and they wanted it at Princeton. It would have been a good deal, but I didn't do it. Well, my wife was not well—family things, OK? So anyway, the project gets turned down by MIT, in effect. And I learned later on from Francis Low that it really—I thought it was just bumbling, but it turned out that they didn't think the science was going to work. I learned this later—in fact, this year—because I pointedly asked Francis, "Why the hell did MIT misbehave so?" And he said, "Well, I can't speak for John Deutch," but they didn't think the thing was organized well. They didn't think the NSF would really support it, and they didn't think it was going to work. So that was that.

So that was Francis. I also wrote a little note to John Deutch, but he is in such trouble that he doesn't want to talk to anybody. At any rate, what then happened is that there was an attempt made by the three of us—Kip, Ron, and myself—to organize the project. But the NSF was getting queasy about how we were structured. I began to get very queasy also. The NSF would much have preferred a director for the project. I certainly would have preferred a director. But it was quite obvious that there was no way to make a single director at that time, unless you hired more people. And the reason is that I couldn't be the director. In a way, I would have been a legitimate director, and I'll tell you why—because I had the experience. Of the three of us, I had the most experience with big science at that time. On the other hand, it was impossible for me to be the director, because the project was going to be centered at Caltech, the project manager would have been at Caltech, and Ron wouldn't have tolerated [my being the director]. I'm not so sure Kip would have tolerated it either, but he was more guarded about it. It's quite clear that Drever couldn't be the director; that was clear to every one of us. After I really got to know him, it was quite obvious that that would have been hopeless. Kip might have been able to be the director.

COHEN: But he probably didn't want to.

WEISS: But he didn't want to. And he didn't have the technical capability. So we did the best we could at the time. We made a strange organization.

COHEN: That was the troika?

WEISS: That was the troika. And the way it was set up was—and it was pretty childish. Kip tries to be an amateur psychologist; you must know that by now. He tried very hard to figure out my motivations; he thought he understood Ron's motivations. And he was desperately trying to figure out how to make this thing go. I have to admire Kip—he put a good bit of his time into this. He tried to take these two people, who were really not getting along, and figure out how to make it function. So he would invent what I would call kindergarten themes, OK? He would try to give Ron a title; he tried to give me a title. It was pretty ludicrous. I don't remember the titles, but they were pretty stupid. "Chief scientist in charge of …," and the other one was "chief scientist in charge of " this and that. It was all pretty nutty. The fundamental problem was that I was given the job—and I wanted the job—of dealing with the project manager and worrying about whatever we needed. We had to get the sites and the facilities and stuff together. And Ron was given the job of worrying about the detector—how it would be made. I resented that a little bit, mostly because I thought I had ideas about the detector. But I backed off, and I began to play a role which I have been playing ever since. And that is, I've tried to suppress my ego—which is there, there's no doubt—to try to keep the thing going. It was quite clear that that was the only role that I could play. In other words, I would do whatever was necessary to keep the project going. And I think Kip realizes that; I think we both share that as an understanding.

This happened over and over again, through the history of Robbie [Vogt], through the history of Barry [Barish, director of LIGO 1994-2005]—well, less so with Barry. Barry I feel I can talk with. But Robbie was the same way. I would do everything necessary so that the thing could keep going forward. Whatever it was. So I began to realize that I had to work with Frank Schutz, who was OK. He and I became very close. We would interact daily. And I would try to help him get the project moving. In the meantime, there was this terrible problem: Ron would not let a decision be made. If a decision was made, he had to be part of the decision, whether it be about the siting—of course, that's very important—or how to approach the vacuum system or something like that. There was nothing that Frank Schutz and I could do that Ron couldn't. It was written into our constitution, in effect, that Frank and I could deal with minor decisions—of course, that was not defined—but any major decision had to involve all three of us, the troika. And that was a hopeless, impractical thing, given Ron's paranoia. There *was* no minor decision. This was the trouble with the whole scheme.

So we would try to make progress. Eventually what happened is that Schutz and I found that the only place we could operate was on siting the LIGO. Schutz very rapidly began to close in on that. He wanted to focus on the site so that he could start doing cost estimates and honest-to-god something that was real. I found a site in Maine, which in fact we decided to go to. And I kept coming out here almost weekly, because Ron couldn't decide on where there would be a good site out here. We went all over the Mojave Desert, all over Arizona. I mean, I didn't realize that you can go with Ron to a restaurant and he can't even make up his mind what to order! Eventually Kip had to force the decision, and they decided to go to Edwards Air Force Base. So a lot of effort went into developing at least one thing that looked to Schutz like he could do without being interfered with all the time by Ron, who was right next to him.

So I worked with Schutz on that. We did a lot of the archeological studies, and god knows what. We did stuff that was very rudimentary but important to get the project off the dime. In the meantime, the [project] was having trouble getting through the National Science Board. They kept trying at the level of the NSF. There was money going to MIT and there was money going to Caltech, and the way it was done is that we both gave money to the project office, which was at Caltech. That's the way that we did it, so that there would be parity between MIT and Caltech. But we never got the big money. We always got enough to study and to keep going. I don't think we were ever really lacking for money, but we could never take the big step of starting the real contract. OK?

COHEN: Even though you were working on the science.

WEISS: We were working on the prototypes, we were doing a whole bunch of stuff. There were a lot of fights about the architecture of the detector, and Ron was very difficult to deal with in that regard. At the time, I had a lot of respect for Ron, of a different kind. I began to understand Ron better as a scientist. I also found out why he was impossible to deal with: He doesn't think the way you or I think; he thinks in pictures.

COHEN: I know that. He's told me that.

WEISS: Yes. And he doesn't remember what he thought the day before, so you could never make a decision. You could watch his process. He'd go through the same logic about a decision on how big the laser beam should be or how many mirrors there should be—I don't know, pick anything in the interferometer. And you would discuss this with him, and you would get to the same point, and he would agree that his viewpoint was not right—or maybe he wouldn't quite agree—and then the conversation would start all over again the next morning, from exactly the same place. And we'd come to the same conclusion. This would go on day after day; you'd never come to a resolution. That was one of the troubles. So I don't know if it was his inability to make a decision or his inability to agree to something. I don't know, but it was very, very difficult to come to a decision. In the meantime, his group was falling apart; Stan [Stanley E.] Whitcomb took off [1985]. I tried to keep Stan here; I spent hours talking to Stan. Stan was having terrible trouble with Ron when they were trying to build the 40-meter [prototype interferometer]. Each time Ron would go back to Scotland, they would get things done here and Stan would feel a little more like a human being. He would deal with the graduate students and the postdocs, and things would happen. Then Ron would come back and effectively decide that everything that had been done was worthless. And he would have a new idea. Eventually Stan just gave up. He couldn't get the support from Ed [Edward C.] Stone, who was at that time the head of the division [Physics, Mathematics, and Astronomy]. So that was a whole sideshow that was going on. I was trying to keep Stan here, because I had huge admiration for Stan. In fact, later on when Robbie came in [1987], I convinced, thank god, Robbie—and it was hard—to bring Stan back in again [in 1991, as deputy director]. I don't think I want to go through all the interactions. It was very unpleasant for both Ron and me.

COHEN: And Kip, by this time, had divorced himself from this a little bit?

WEISS: Oh, no, no. Unfortunately for Kip, he saw that the thing wasn't working properly. I mean, I could not give in on everything. It was just getting to the point where…. And he could see that. At the same time, however, he realized that he had gotten Ron, and Ron would never let him go on this. [Ron would] effectively tell him, "Look, you've gotten me here under false pretenses. I thought I was going to get this and this and this, and now look where I am. I'm in this terrible situation with Weiss and these MIT people who are going to eat me up," and all that sort of stuff. And Kip, I know, felt terrible underneath about that, because there was a little truth in it. I mean, I don't think that Ron ever thought he would have to deal with somebody else.

So Kip was still in there; I mean, he had to. The watershed in the whole thing came when Dick [Richard L.] Garwin wrote a letter to Marcel. That's leaving out a lot of history, but let's leave it go. That was in May of '86. This is now three years into the joint troika activities and trying to get the siting and trying to get the studies done for the costing and all that stuff. And what happened is that Garwin had gone after [Joseph] Weber. Garwin, who was by that time at IBM and had the free run of the place—he was sort of a guest scientist, being paid very well. He's a very bright man, and this looked interesting to him. He made a little Weber antenna—a little Weber bar, not a big one, and he discovered that he did a much better job than Weber. He knew exactly what he was doing. And he found nothing, [no gravitational waves]. OK? So he then went after Weber. I don't know what the conversation was like, but he probably said, "Look, I've built something like what you have. I don't see anything. You must have something wrong." Well, Weber told him, "No, *you* have something wrong." You know how it is. And then there was this huge debate about sensitivities and a whole bunch of stuff. What happened is that Garwin got mad and he went after Weber as though Weber was doing pathological science. He thought Weber was lying. I think Weber was just being sloppy as an experimenter. So Garwin had it in for this whole field. He thought the whole thing was fraudulent, just as bad as *n* waves or any of that stuff. You know, bad science—pathological science. And he thought he'd kill this goddamn field, OK? And here he finds that the NSF didn't pay any attention to him, and, my god, from this little thing that Weber [built, which was] not very expensive, all of a sudden they're thinking of this multimillion-dollar proposition. I don't know if you know Dick Garwin.

COHEN: I went to junior high with him.

WEISS: Oh, really?

COHEN: Yes. I just remember him being very quiet.

WEISS: Is that right? OK. He was a sphinx.

COHEN: We didn't know he was brilliant.

WEISS: OK. Well, Dick thought he had slain this dragon, and then all of a sudden out of the ashes here was a phoenix that had risen. So he sent a very complicated letter to Marcel, saying, "Look, if you're going to persist with this"—and he didn't say it was ill advised, but that was implied, OK?—"you would do well to have a summer study." Now, this was in May. And Marcel knew about the troika, of course. And instead of going to Kip or to Ron, he came to me. I know why he did it. I mean, I was effectively the responsible member here. Not that Kip is irresponsible, but he had no experience with things like this. So Marcel effectively demanded that I put a summer study together that summer and get Garwin involved in it and I'd be the chair of it. And I told Marcel that it was impossible and that I couldn't get anything together for the summer. I mean, people had made plans. And if it was to have any value at all, I couldn't be chairman of it. After all, you couldn't make the guy who was pushing for the project the chairman of [a study to look into it]. It was ridiculous. He agreed finally. And so what happened is that I went to see Viki [Victor F.] Weisskopf, who was my department chairman at the time, and I said, "Look, I've got a problem. Do you know anybody I could go after to be chair of a thing like this?" And he mentioned Andy Sessler [then director of the Lawrence Berkeley National Laboratory]. So I called Andy; I didn't know him from a hole in the ground. I called Andy and said, "Look, I'd like to come out and talk to you." I didn't really tell him completely what it was about. I didn't realize how good a man he was. But that was very, very good advice from Viki. So I went out there and told [Andy Sessler] about the whole problem. I spent the morning with him. I told him I needed to get somebody to chair a summer study on [LIGO]. And he was interested—not in gravity, but in new things in physics. And he said to me after the talk we had that, yes, he would think about this, providing I could get Boyce McDaniel, who was at Cornell, to jointly chair the study with him. So I called up Boyce and told him that I had been with Sessler. They may have talked on the phone ahead of time, I don't know. And I went to Cornell within a couple days of [meeting with Sessler]. I got Boyce and I went through the same thing with him, and he agreed. Now, I think he had a private thing with Garwin; he didn't like Garwin. That's my own feeling, OK? That's the only reason he did agree.

So I had gone to Boyce and Sessler. I haven't talked about this for years. What I then did is, over the summer—because I couldn't get a summer study; it became a November study—

I got a whole bunch of very good physicists. Val Fitch. I tried to get Nobel Prizewinners—people [the NSF] would listen to. Val Fitch [1980] was certainly one. I don't remember all the people on the committee, but Boyce and Andy were on it and Garwin was on it. And this committee then arranged for a whole week of presentations, first about the science, then about the technology and about what industry could do, and about the sitings and all the technical engineering details. And then, on the very last day, we had a session on the management. That's the way I orchestrated this whole thing.

COHEN: Where was this held? Here?

WEISS: No. It was held in Cambridge, at the American Academy [of Arts and Sciences]. At the time, I wasn't a member of the academy, but I had to find a place, a venue. To be honest with you, I did it because I felt—and thank god I did it that way—that this was a crisis. And the crisis was so bad that I didn't want to deal with Ron about it. I talked with Kip about it a little bit. He wasn't very happy about the whole business. I suspect it was all aspects of it: the fact that the NSF had come to me rather than the troika, that I had decided to have the study in Cambridge, and that I was pulling it together without extensive consultation with the troika. It was a crisis, and I felt that I did not have the time to argue about each step—as would have been inevitable if the planning involved Drever. But I said, "If I'm going to do this and run it, I want it so I don't have to be traveling across the country all the time. It's going to be done on the East Coast." And I took charge of the whole thing, because I had had enough of this troika nonsense. This was too important. If we blew it here, the thing was over. I wasn't trying to take anything away from Caltech, it was just a matter of convenience. I was given the job—the NSF told me to do this. They didn't go to the troika; they came to me. And I know why they did—because they saw the whole thing as ineffective and they figured I could probably do it.

The study was kind of seminal, because it brought a lot of people in—the Stanford people, and I got people to talk about lasers. I won't go into every detail, but the point was that the only thing that was unpleasant about it—it was a very good experience for all of us—was the day it started. And that's when I was very glad that Boyce McDaniel and Sessler were cochairs. Garwin announced on Monday morning that he had only six hours for this whole thing and we had to condense the whole thing into six hours. But he was the guy who [had asked for it]! I was ready to choke the son of a bitch! I just couldn't imagine a guy acting that way. So thank god, Rich Isaacson held me back. And Boyce saw how angry I was, and he decided that he would take on Garwin. He said, "Look, we'll take notes for you." Thank god for him, OK?

COHEN: Yes. He's a nice man.

WEISS: Of course he is. A delightful man. So is Andy Sessler. Anyway, they listened to this. They were turned on like crazy. This was fascinating to them. And Val Fitch was turned on, too, but I already knew [he would be], because he was interested in gravity. He himself had been thinking of going into this.

OK. We got through all the technology and so forth, and everything was quite well done, I thought. And then we got to the management situation. Unbeknownst to me, Kip had already met with the committee privately. I demanded that the committee meet with me about the management. I took them to Legal Seafood. We all met down there. And I told them what the problem was. I said, "Look, this thing is dead unless you make a recommendation that there be a single director. You've got to get rid of this troika. It doesn't work. And the recommendation I think you ought to make is to throw it out and say, 'Look, you go to MIT, or you put MIT and Caltech together in some way, but you have to find a director and agree on a director.'" And they did.

COHEN: So that was done by this committee.

WEISS: That was done. It had to be done, OK? Well, I went back to MIT and I went to John Deutch, who was by that time provost, and said, "Look, we just had this big thing about gravity-wave detection and interferometric detection." And he knew a little about it. I told him [who] the committee members [were], and he said, "Well, you couldn't have gotten through that." And I said, "Of course we did. But we have a job to do. We've got to find a director, because the management doesn't work." Well, Kip did the same. Kip came back here to Caltech and told—who was president at the time?

COHEN: Probably [Marvin L. (Murph)] Goldberger.

WEISS: Goldberger, right. And so there was this vying all of a sudden, opening the whole thing up again. The two institutions were separately looking for directors of LIGO. Well, MIT only had one idea about the director: They said, "Why aren't you the director?" I said, "I can't be the director. Caltech won't buy it, and I don't want to destroy the project. So I am not the director, forget about it. You go find somebody else. I think you ought to start looking at people you might want to hire as faculty to come in as director of this thing." Well, that was an outrage. MIT wasn't going to do that. They could not understand why I couldn't be director, and they wouldn't listen to the arguments. Second, they didn't care terribly much. And what happened out of this was Robbie [Vogt, then Caltech's provost]. OK?

COHEN: He had just had this big fight.

WEISS: He'd had this huge fight with Murph [Goldberger].

COHEN: Murph thinks that's the worst thing he's ever done—to sic Robbie on the LIGO project. [Laughter]

WEISS: It wasn't the worst thing he ever did, because Robbie did some good in the beginning. I hate to say it. I'm going to be very fair about this; I think I have to be. The first thing I did when I heard about [Robbie's appointment] is I started calling around. MIT hadn't come up with anybody. Caltech came up with Robbie. Kip, right from the beginning, sold Robbie as hard as he could to me. He said, "This is a fantastic opportunity," and he was telling me about [what Robbie] had done for the division and about how he was exactly the right person and so forth. And I knew Kip well enough [that I could say,] "Look, I'll come back to you," and I started calling around the country myself. And what I got was singularly good stuff about Robbie. The NSF thought Robbie was wonderful. Why? Because he had saved a very complex situation at Owens Valley [Owens Valley Radio Observatory], in a situation where Leighton dishes were being built and all the science was being done and they were about to have to cut that off and a whole bunch of stuff. So he was given very high praise by the NSF people. I talked with a whole bunch of people in the astronomy part, just because I had to get some calibration on this. To a man, they all said that Robbie was superb: "If you could get him, that would be the saving of this project." Rich Isaacson said the same, but he had no independent knowledge. I talked to several Caltech faculty, and the guy I happened to know was Tom [Thomas G.] Phillips. Tom was, of course, part of that whole business at Owens Valley—hired by Robbie, I believe, [from] Bell Labs. I knew Tom when he was at Bell Labs working on indium antimonide detectors. And he was the only guy who was really totally honest with me, and I didn't believe him. He said—I'll never forget the words—"Well, you and Ron won't be the same after he comes in." And I didn't know what he meant. I remember asking Tom directly, "Will he screw it up?" [And he said,] "Oh, no, no, no. He'll make it work. He'll make it happen. But you and Ron will not be the same."

So I invited Robbie out to the East Coast, and I realized right then and there that he didn't travel well. At least, that's what he told me. He got sick on planes. And I began to realize right then and there that if he became the director, he wasn't going to show up at MIT much. And I was upset and worried about that, because I had a whole bunch of people on this. If you take a director now, you've got to look like you're directing—not just directing from afar but also trying to direct locally. So I worried about that, and I confronted Robbie with it. And he promised me—we even got an office for him—Robbie promised me that he would show up once every couple of months.

COHEN: It didn't happen?

WEISS: It never happened, OK? Very early in Robbie's [directorship], he came East once. This was right after the NSF was trying to get MIT to play a bigger role in the project. And Marcel Bardon, Rich Isaacson, and one other person came to MIT to John Deutch's office. And Jerry [Jerome I.] Friedman [then head of MIT's Physics Dept.] and I, we all met with the NSF people in John Deutch's office, and [Robbie] came along. And the NSF starts this thing about, "Here's Caltech doing this and this. What will MIT do? This should be a joint project between the two universities." They were counting on that; it was very important to them that it not be just one university. And John Deutch asked me for a piece of paper. I don't know if you know John. He's very tall. He grabs the piece of paper, pulls out his pen, and he writes a great big zero and shoves it under Marcel's face. He said, "That's what we're going to do." And he walks out. Robbie looks at this, and I think he learned something from that. See, before that he kept telling me that he was very worried about these MIT gorillas. He'd say, "Caltech is a small place.

MIT's five times bigger. They'll eat me up." He didn't say that anymore, after that. [Laughter] But it was quite obvious to Robbie. Robbie's not stupid; he's very smart. The NSF did their job: they tried to keep it a two-university thing, but it didn't work. And so they effectively built this cooperative agreement, which we're still living under, which makes Caltech the lead. All the money runs through Caltech, and MIT became a contractor, OK? That happened right then and there—very shortly, within about a week or a week and a half after that. That's when the whole pattern was established. Effectively, that's the way Robbie maintained his direction, and he started doing what he had to do, but he did it in an awkward way. OK? He systematically had to make a single research group out of the two groups. He said right off that that's what he had to do, [and] he made a whole set of decisions very quickly. Some of them were good decisions. In retrospect, one of them was a good decision. And then three or four of them were absolutely bad decisions, technically bad decisions. But I didn't want to fight with him. And what happened is about one third of my group, or maybe even half of my group, decided this was hopeless and they were not going to play a game with us. And they went and found other jobs. They thought that the thing was over, in terms of their own ability to get anything through.

A whole set of decisions that were not able to be made by the troika were rapidly made. And I told Robbie that he had to make them, but he didn't make them the way I wanted him to make them. Well, I think he's enough of a psychologist to realize that he had that grace period. He probably knew that I would be desperate to do anything to keep this thing going. And he knew he had a much harder problem with Ron. That's the way I sort of justify this. Because he reads people reasonably well. So he made certain decisions, like, for example, to go with the Fabry-Perot and not the delay line. That was a good decision, although it was very intense. And he also made a very bad decision: he decided to keep the argon laser instead of the Nd:YAG laser, which we were pushing and which is in fact now being used. We had a big program on that, but Ron didn't like it. He then decided against a certain suspension technique that we were working on. Well, I wish to hell he hadn't, OK? Because [now] we're living with something terrible. So every decision that was made was made based on advice he was getting. I felt, and my guys felt, that he was listening only to Ron, and Ron was not necessarily the best technical advisor. I felt in the end that he did it mostly because a decision had to be made.

So several people in the group went away. We were, at that time, building a new 5-meter prototype, and Robbie felt that that was also divisive and that we instead should all work on the Caltech [40-meter] prototype and make that better. It was another decision, and that was a very hard one for most of my people, because they fundamentally knew enough about the effort [at Caltech] and they didn't think much of it. And they didn't want to work at Caltech. But that was Robbie's attitude—that you had to condense the groups, and he chose to condense them [at Caltech]. And I went along with it. I went along with it because that's the only way I could see that…. You know, if I had fought with him, it would have been [a further] example of how impossible this project was. And in the process what happened is that my whole group changed. New people came in, a lot of people left. And we then started working on things that we were assigned, which was new. Then what happened here is that they started building a new version of the 40-meter. The old version was so bad—the one that Ron had built—that I kept [telling] Robbie that he was dealing with an immature bunch of people. I mean, I agreed with him—he had to change what was going on there. And it wasn't Ron so much; it was the fact that the instrument was not an instrument you could develop LIGO with, nor could you do it on the basis of the way Ron was running that laboratory. And [Robbie] knew that. So we spent a lot of time helping Robbie to organize the effort on the 40-meter, changing it and making it into an instrument that ultimately could be used. But Ron was still in charge of the 40-meter at that time. There was no problem yet. And this is one of the places where I resent Ron. He said, "Well, I won." He kept telling me. "You see, I was right all along," he kept telling me. And I didn't want to argue with him. I told him that I went along with this because I was trying to keep LIGO going. I mean, it didn't make that much difference to me anymore, as long as we were able to go forward. Anyway, the next big event was.... Bill [William E.] Althouse became Robbie's close—

COHEN: Oh, he loves Robbie.

WEISS: Of course he does, because they worked together for years and years. And I was amazed. I didn't realize what Robbie was doing—that he wasn't going to try to build [the group] up much more. He had a very small group of people, and he got some very good engineers. Larry [Lawrence W.] Jones I worked with intimately for a long, long time—a first-class engineer from JPL. And Bill. And a vacuum engineer whom I spent—oh, god!—tens of hours a day with; you know, that was my job in the beginning. And he wasn't so good. And I started drifting more and more into what I would call systems engineering and worrying about LIGO as a facility, as a vacuum system—the things that make it into a facility that ultimately we'll do designs [for]—and less with the detector technology. I felt I couldn't give that to a young person in my group—they couldn't publish anything. [But it was] something I could do. They badly needed somebody at that level—what I would call a scientist level of engineering. And I started working with Bill Althouse a lot, and with Larry Jones. In fact, later on I worked on a lot of stuff. This was all a legacy from Robbie, where he had a few of the scientists working with the engineers very intimately. I was one of those.

I kept defending Robbie, even though he wouldn't come East. People began to get worried about that at MIT, and my people kept complaining about it. I think the thing really became serious when we started writing the 1989 proposal [to the NSF], which was a key part of the entire LIGO program. I had done a lot of work with the Congress, but Robbie decided to do it all himself. And [it became] very clear that Robbie [wanted] to be seen as the only representative of this project; it was very important to him that nobody else do that. In other words, for example, I had a lot of associations with Congress, because I had been selling this program. I had to stop all of that. But I understood, I didn't resent that. He had to present the project, and he might have said something different from what I said. So he got this big lobbying effort started, and he began to work on Congress. And I think he did a good job. Too much of his own vanity was tied up in it, and people began to laugh at him behind his back. I know that. "We have this crazy professor, this German professor, from Caltech coming here." But he knew enough to back off when he saw that the things were going a little bit bad. And he managed to do, with this lobbyist, a very good job of making sure that everybody who, after the site selections had been made.... And the site selection is another story, which I think is important, and I'll maybe come back to that. He kept a whole bunch of balls in the air in different places, so he got support even from places that were not selected. He played that well.

Maybe I should say something about the site selection, because that's important. The site thing fell apart. When Robbie came in, one of the first things he did was to get rid of Frank Schutz. He didn't trust Frank, and he didn't have respect for Frank's intellect. And maybe he was right. So he got his own people in. He replaced Frank Schutz with Bill Althouse. Bill was a good choice, I think. Bill's skills with people aren't very good, but he's a very good and skilled engineer—maybe not formally trained that way, but he's plenty smart. Now, what happened is that very early in Robbie's directorship, the NSF was still trying to figure out how to get LIGO going. And a guy named Eric Bloch, who was head of the NSF at the time, calls Robbie. Now it's Robbie; they don't call me anymore, which is good. I wouldn't have let them call me. I would have said, "We have a director now," OK? In fact, I had to do that a lot. And that was one of these funny things about Robbie. He was new in the field, but he would have been insulted if somebody called me—because I had been in this thing for years, you know—and I didn't immediately say, "Look, we have a director. The director is Robbie. I can't speak for this. Robbie speaks for it." That was very important to him, so I did that religiously, after I realized that that's what he needed. OK? And it extended to everything—to the invitations for talks, to the invitations for anything. You had to send the people to Robbie; that's what he wanted.

So the NSF called Robbie, thank god! This is right after the big NRAO [National Radio Astronomy Observatory] antenna collapsed in Green Bank, West Virginia [November 1988]. The NSF tried to sell Senator [Robert] Byrd [D.–W. Va.] on LIGO. I don't know if you know that.

COHEN: No.

WEISS: It's a very important part of the story. Eric Bloch figured, "Well, Byrd doesn't know the difference between a gravity-wave antenna and a radio antenna, so what the hell's the difference?" And indeed, Byrd didn't give a damn.

COHEN: He just wanted the project.

WEISS: He wanted [a LIGO site] in West Virginia. So that got pretty far. Eric Bloch called Robbie and said, "You get your ass down here and see if you can shoehorn LIGO into the NRAO facility." And, boy, they jumped! They went down there right away. Bill Althouse and Robbie went, I think—I didn't go; I was not part of that—and a couple of guys from JPL. **[Tape ends]**

Begin Tape 2, Side 2

WEISS: So Bernie Burke comes into my office, ranting. He says, "What in the hell are you trying to do? Are you trying to steal our radio antenna? What's wrong with you? Don't you have any sense?" And I said, "Look, it wasn't my doing." And he said, "Well, you're not going to get in there [in Green Bank]." And the radio astronomers got up in arms about it. They went to the NSF and said, "Look, we're not going to have the LIGO thing [at Green Bank]." So Robbie was confronted with his first crisis, and we helped him get through that crisis. It was a matter of how he should deal with this. And he would agonize. When he gets upset, he agonizes to the point where—I remember coming out to Caltech especially, just for that. And so we finally decided that the right way to handle it was to make a public solicitation [for the site selection]—send letters to every bloody governor [in the country]. And that's what was done [in the fall of 1990].

And then, because I was so strongly identified with Maine—I had worked with the governor and other people there to get [a LIGO site] there—I think that's the reason I was kept out of it. I was never made aware of any of the studies or anything. This was all done separately. I was asked occasionally about ground noise and things like that, but I never saw the report. And what happened is that Robbie, with Bill, and then, I think, by that time Stan [Whitcomb] had come back—they did a report for the NSF and made a whole bunch of pairings for [the two] sites. That's eventually how [Livingston] Louisiana and [Hanford in] Washington State got chosen [February 1992]. Maine was a very strong contender in there. By the way, Robbie will say, if you get to interview him, that the decision wasn't made by him. If you talk to the people at the NSF, they'll say that Robbie gave them a very biased and crappy report. The guy who made the decision was Walter Massey, Eric Bloch's successor as head of the NSF, and Walter made the decision based on whatever went on in his head. I mean, he looked at the criteria. It turned out that of the pairings, Louisiana and Washington looked like a good pairing. There were no choices made, according to the NSF report: the LIGO project just took all the pairings, and certain obvious ones they left out. If the LIGO project didn't want Louisiana, [the report] sure as hell didn't make it look [that way]! But they *didn't* want Louisiana! There were problems in Louisiana.

COHEN: Why is that? Was it hard to get to? Or was it the weather?

WEISS: No, no. There are hurricanes. There's a pipeline that runs right underneath [the site]. It's noisier.

COHEN: So it's just not very good.

WEISS: It's not as good a site as others would have been. Now, I can't say that this was all Robbie's fault. I was shown the ground-noise spectra from Livingston, and it looked OK. Bill Althouse would send them to me. But I was not sufficiently made aware of some of the other problems—like, for example, that the second biggest pipeline in the country runs under LIGO. It's an outrage, absolutely terrible. And the fact is that we're barely thirty feet above sea level. I mean, it's just scary as hell. It's not the place to build LIGO. And I'm worried about it. Every time I go down there, I worry about it. The building is done. But goddamn, it makes me mad that we went there, OK? Now, the NSF blames Robbie, and Robbie blames the NSF; it's one of those things. But the Washington site is wonderful. There's nothing wrong with [Hanford]. I mean, sure, the radioactivity, but….

So along comes the 1989 proposal, with these sites in it. Robbie really tries to pull out every stop. He stops all progress on the project, which is fine; he had to do that—to write a first-class proposal. He felt that all the prior proposals that had failed were in some way flawed. Everybody in the whole project came to a standstill for about six months to write this quite elegant proposal. It should be in the [Caltech] Archives; it's a very interesting proposal. We put all our hearts into it. And in the course of writing that, I lost Peter Saulson, who was on leave. Robbie intimidated him by saying, "Why aren't you working with us on this proposal?" And Peter effectively didn't like Robbie's style. If we had a team meeting or something like that, Robbie would say something snide about a person but not look at them. I don't know if you know that—how he operates. That's what he does. If he's trying to get a person who is over in that seat to do something that he wants, he will talk to the person opposite. He won't look directly at the guy and say, "Look, I need this from you." He says, "Wouldn't it be nice if Mr. So-and-so was behaving more politically correctly?" or something like that. And you know, the poor guy's sitting there—he knows goddamn well it's about him. Peter couldn't stand this after a while—and a couple of other people couldn't, but Peter in particular. He decided, "To hell with it!" And he had been offered a professorship at MIT. He decided right there in one of those meetings that he wasn't going to take it anymore. It was a terrible blow for me. I stopped the meeting. I took him all over Pasadena, all over the place. I said, "How can you now walk out on this?" He said, "I can't deal with that man. And I'm not going to forever deal with this problem." By the way, he's back in LIGO, but [he came back] only after Robbie walked out. That was the biggest casualty in our lab, aside from a lot of people [leaving], because Peter was such a very good person. But he just couldn't deal with Robbie. So what happened is that—and I think I'll probably end with Robbie. I won't get to the Barish era. I don't think there's much to say; I think Barish is superb. I think he's doing a first-class job, by the way, so that you know.

What happened then was that—I'm trying to piece it together—I would talk to Robbie on the average of once every two days. And we would have these psychiatric sessions, where I had to bolster him about whatever it was—his salary, that he wasn't getting along with [Caltech President Thomas] Everhart. Or that Everhart wasn't giving him enough due. Or that somebody had slighted him, or that I wasn't being sufficiently cooperative, or some damn thing. We would get to substance at about the end of the conversation. A good hour would be spent on calming the savage beast. And this went on for probably five or six years. In the meantime, what happened is that the people at MIT began to worry about Robbie. And the NSF began to worry about Robbie. First of all, there was the business of the committees coming in. Andy Sessler, who had saved our ass, was asked by the NSF to run a committee to look at what was going on. Well, Robbie made it known that he was being interfered with. He didn't want to be bothered with that goddamn NSF committee. So he effectively kept telling them, "Look, we're busy." He was telling a main committee, a major committee, "Get the hell out of my hair." And they had come here at the behest of the NSF to look at the project. I couldn't imagine that a guy would behave that way.

COHEN: You mean the committee came and he said, "We can't be bothered"?

WEISS: Yes. Well, he didn't [actually say that]–"be bothered"–but [that was the idea]. And Andy came to me and said, "Who is this guy?" I said, "Well, he's the result of your recommendation that we should have a director." He said, "I can't deal with people like that," and he kept grilling me about Robbie. And I said, "Look, he's done a lot of good things," and so forth, and I tried to make peace. Well, that was the beginning, that's when things began to fall apart, way before the Drever thing, OK? It was Robbie's interactions with committees like that. You said earlier, "Well, he couldn't have anybody above him." It was worse than that. It was that he couldn't deal with the criticism that came from the committee or the suggestions that came from the committee. After all, who was in charge? That committee wasn't going to make decisions and then get out of here. *He* was in charge, and he had to live with these decisions, not the committee. So that, to him, was one of the problems. And the NSF began to get very antsy about it, right then and there. I remember Marcel coming to me and saying, "You've got to get rid of him." This was way before the problems [with Drever]. Marcel called me when Robbie began to look at the budget and started escalating it from $70 million, or whatever it was, to $280 to $400 million. And Marcel finally blew the whistle on Robbie. Now, Robbie had something wrong about this. He figured that the NSF wanted this so badly that he could start raising the ante. And he started asking Drever for all the things that he wanted. Drever doesn't know anything. Drever would say, "Oh, the [project] was too expensive to begin with," but then he would invent things that would make it more and more expensive. And the major thing was—and this was the thing that Robbie was trying to accommodate, and it was a mistake. He was trying to accommodate the fact that Drever was trying to reinvent LIGO so that he could have an independent LIGO program and finally [not] have to collaborate with anybody. And he had gotten very far with Robbie on this. There would be a joint effort—a low-grade, nonimaginative effort, the joint effort between MIT and Caltech—but Ron would have his own set of tubes, his own set of everything, and he could do his first-class stuff on that and work independently.

COHEN: And Robbie was buying all this?

WEISS: Yes, he was. Not for long, but he was buying it in the beginning. And the thing got more and more expensive—a whole bunch of things, more buildings and bigger stuff. And the thing had grown completely wacky. And Robbie kept coming to me and complaining to me. He said, "You've sold those NSF guys something that was not possible to build. You just didn't budget enough money for it." And he kept me over the barrel on that. And I said, "Yes, but the thing has grown so." And he said, "Well, we had to grow," and so forth and so on. And that was a tension between us.

So Marcel calls me—this is Robbie's director. He calls me and he says, "Look, we've got to do something to contain that guy." This was way early—I think about a month or two into Robbie's directorship. Marcel said, "He's going to kill this project. He's making it too expensive. We don't have that kind of money. What do you recommend I do?" I said, "Look, he wants this, he wants that. You've got to hold the budget." By the way, I've never told this story to anybody. Well, I think probably now is the time I can say it. I mean, I did do something behind Robbie's back very early, because I didn't know what the hell else to do. The NSF called me and said, "Look, …." And Robbie has forever complained about the fact that the NSF held the budget and that what he wanted to build wasn't being built. Well, what he wanted to build was something which was—see, he wanted to accommodate the idea that there would be one detector that would be a research detector and one to play with. So simultaneously every site would have three detectors to begin with. And the thing got completely out of hand. That's because that's what Ron wanted. The field didn't exist; you couldn't spend that kind of money. And he kept escalating the cost. So finally the NSF said, "Look, you can only build so much." And Robbie then totally tore his hair out about how bad a compromise he had to make and the terrible legacy he was leaving for the field later on—that everybody would always have to make a decision, should they run or should they develop? If it had gone his way, he would never have to make that decision. But it would be another not quite doubling of the cost. But he didn't think about the cost. He thought there was an infinite number of dollars. I was amazed that, having been provost, he thought he could get away with a thing like that. I couldn't believe it.

COHEN: Well, he didn't get away with it. [Laughter]

WEISS: No, he didn't. No, no, he didn't get away with it.

COHEN: When did he start fighting with Ron?

WEISS: That came later.

COHEN: His battle then was just with the NSF.

WEISS: At first the battles were all with the NSF, and Ron was with him, because Ron didn't like the NSF, since he felt the NSF had forced him into this collaboration. [The NSF] had nothing to do with that! I was the one who forced him into this collaboration. And Kip won't recognize this; he thinks the NSF did it. It was not the NSF, it was the fact that it was impossible to do LIGO independently.

COHEN: So then what happened after that?

WEISS: Here I may not have it quite right, because I was not on top of everything. I was busy getting things done. But the next step was the falling out between Ron and Robbie. In part, what was going on was that Robbie was beginning to force decisions on Ron. Robbie had forced decisions on me. And the first one was that he was getting worried about the progress of the 40-meter [prototype]. And he began to take some of the responsibility for the day-to-day program on the 40-meter away from Ron. He tried to give it to somebody else. He picked Bob Spero, and Bob wasn't quite up to it. But it was becoming clear to Ron that somehow his advice wasn't being taken solidly anymore. And he began to complain. He started to complain to Kip. He started to complain to other Caltech faculty members that, "Well, this man doesn't know enough to make a technical decision." Up to then, all the technical decisions had gone Ron's way. Some of them now were going the other way—not in my direction, but in a direction that wasn't the way Ron wanted. And he didn't think through what he was doing. Ron has no sense of psychology. And it very rapidly got to Robbie that Ron was disagreeing with him. And then Robbie, at our monthly meetings, began to tease Ron. And Ron would then try to fight with him. But Ron gives up quickly in a fight. Robbie eventually began to see things in his paranoid way—that's one of the problems he has—that Ron was trying to undermine him. And it snowballed. Ron wasn't, I don't think, consciously trying to do any of that. What was happening was [that Robbie] would misinterpret every comment that was coming out of Ron. Once [the relationship] went sour, it went sour fast. Every statement that Ron made would be interpreted as.... You see, Robbie would parse the statement, and he would have discussions with me or with Bill Althouse. And Bill would energize this, and they would resonate on this. And then I would try to calm things down again. But then Robbie would consider, "Well, are you with me or against me?"—that kind of thing. What happened eventually was that the thing polarized badly. And with Stan Whitcomb helping a little bit—Stan had had his problems with Ron. [The situation] snowballed so quickly that it became polarized within a matter of about a month. Ron was questioning Robbie's technical decisions, and Robbie felt that he was more than just an administrator. I mean, I think if you picked a thing that he was sore about—if you ever said to him that he was a manager, he would jump all over you. [He'd say,] "I'm a physicist, and I can think about things like this, like anybody else can." I respected that, because he's not dumb. And Ron wouldn't give him that, OK? And that's, I think, fundamentally what finally happened to them. Ron pushed a button that made [Robbie feel like] a second-rate person, and Robbie couldn't deal with that.

So then the research program got taken away from Ron, the 40-meter. Then Robbie started a campaign of getting people on the project to write their recollections of earlier history, where Ron had done things that were—you know, that he had stolen ideas, that Ron wasn't as good as everybody claimed he was. And this was a problem. I can understand what Robbie was up against. Here was a faculty, much of which felt that Ron was a great genius—I mean, even Kip felt that way—and that Robbie was not behaving in a way that was protecting that genius. The genius was complaining. And so a lot of your faculty here, including the famous astronomer—

COHEN: Peter?

WEISS: Peter Goldreich, certainly. Goldreich did it because of the oppression of the minority. But I mean the guy—

COHEN: Oh, Maarten Schmidt?

WEISS: Maarten Schmidt, yes. All of a sudden, we found this terrible polarization here. In the meantime, the real sin was going on. The real sin was—and I was still trying like hell to protect Robbie—but the real sin was that the money wasn't being spent. Robbie brought the whole project to a standstill. And he had asked for a lot of money from the NSF. A lot of my friends and a lot of people who were against LIGO jumped all over this—the fact that they had been sacrificed to LIGO and NSF wasn't giving them grant money. That's what brought Robbie down, by the way—the fact that he couldn't organize the thing so he could spend the money. It looked like he was husbanding money, but he didn't see it that way. And I saw it very clearly. And people at the NSF got so angry and so worried about it that a real campaign started, from right at the top of the NSF, to try to get rid of Robbie. The MIT people got drawn into it. The president of MIT got drawn into it. The president of Caltech got drawn into it. There was a juncture—and I'll tell you what happened in my life. It happened after we had started the contracts with Chicago Bridge & Iron, who built the tubes. I was the scientific advisor for that. Robbie had a fit—a public fit—with the NSF guy who was there to see the beginning of it. And it was embarrassing for the project. He had a shouting match with the NSF representative in front of the president of CB&I and all the engineers, and they looked at each other and tried to figure out, "Who is this madman jumping all over the NSF guy? I mean, that's the guy who's got the money. What the hell are you doing?" And I remember that that's when I broke with Robbie, and that was very hard for me. I'll tell you, it's probably the hardest thing I've done. On the trip back from Chicago Bridge & Iron, which was in Plainfield, Illinois, Bill Althouse, who as you know was devoted to Robbie, and Gerry Stapfer, who had just been brought in to help Bill—another chief engineer—and I were in the car together. And I started the conversation by telling Robbie, "Robbie, you did something very stupid." He was in the front seat, and I was behind. And he looked around and said, "What are you talking about?" I said, "Well, this thing that just happened at CB&I is not the way a project director should behave. What type of impression have you left there?" [He said,] "Oh, the NSF was trying to horn in on that." I said, "That has nothing to do with it. We're dealing with a whole new bunch of people that know nothing about this problem, and you're acting like a person who's out of control." I said, "By the way, let me tell you something. You're in trouble all around, and I cannot protect you anymore." And I told him that the NSF was after him, that my management wanted him out, and that I suspected but was not sure that Caltech's management wanted him out. And I said, "It's time that you left. You've served your function. I hate to say it." And Robbie then got into one of these terrible depressed funks where he looks like he's about to die. We were in this car together. Nobody said a word. And he said to me as I walked out and we were about to split—I was going to my plane and he was going to his plane—"You always see things wrong." And within short order, the NSF just ganged up on him. When he came to Washington [January 1994], Bob Eisenstein [NSF assistant director for mathematics & physical sciences] personally fired him. And it was a terrible thing to watch, absolutely terrible. I mean, the man looked like he was totally defeated and that they were taking his life from him. And so I feel personally—and I've said this to Robbie, but I've not had a good conversation with him since—I said, "I couldn't support you at that critical moment," and he has never forgiven me for that. I think he thinks I'm partially responsible for his demise, that I had manipulated the NSF or something behind the scenes. It's not true. At the same time, I saw him become impossible. And the thing that did it was the thing I just told you—the CB&I affair. **[Tape ends]**

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[1] R. Weiss & L. Grodzins, "A search for a frequency shift of 14.4 keV photons on traversing radiation fields," *Phys. Lett.* 1:8, 342-4 (1962).
[2] W. H. Press & K. S. Thorne, "Gravitational Wave Astronomy," *Ann. Rev. Astron. Astrophys.*, 10:335–74 (1972).
[3] Charles W. Misner, Kip Thorne, and John Archibald Wheeler, *Gravitation* (New York: W. H. Freeman, 1973), see p. 1014.