Ronald Drever, December 1976

RONALD W. P. DREVER
(1932 – 2017)

INTERVIEWED BY
SHIRLEY K. COHEN

January 21, February 10 & 25,
March 13, and June 3, 1997

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Subject area
Physics, LIGO

Abstract
An interview in five sessions, January through June, 1997, with Ronald W. P. Drever, professor of physics (now emeritus) in the Division of Physics, Mathematics, and Astronomy. Dr. Drever graduated with first honors from the University of Glasgow in 1953 and received his PhD there in 1958. He moved from Glasgow to Caltech in 1977 to help establish the gravitational-wave project later known as LIGO (Laser Interferometry Gravitational-Wave Observatory)—first as a visiting associate, then a half-time professor (1979-1984), becoming full-time in 1984.

He discusses his postdoctoral work at Glasgow on the anisotropy of inertia; a fellowship at Harvard with R. V. Pound measuring gravitational redshift; and collaboration with John Jelley of Harwell looking for radio and light pulses from supernovae and the Crab pulsar.

Recalls his interest in Joseph Weber's experiments to detect gravitational waves and his own bar-detector work at Glasgow; his switch to interferometers; his "friendly rivalry" with the gravitational-wave group at the Max Planck Institute in Munich; his adaptation of Fabry-Perot cavities vs. the delay-line technique of MIT's Rainer Weiss. Recalls his collaboration with John Hall, of JILA, in Boulder, CO. Discusses his recruitment to Caltech by Kip S. Thorne; designing Caltech's 40-meter prototype interferometer; his various innovations; his disagreements with Weiss, Thorne, and particularly Robbie [Rochus E.] Vogt, LIGO director 1987-1994; his July 1992 dismissal from LIGO; his grievance hearing before Caltech's Academic Freedom & Tenure Committee, and its eventual outcome.

The interview concludes with comments on his current research and on the prospects for LIGO and allied gravity-wave projects.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH RONALD W. P. DREVER

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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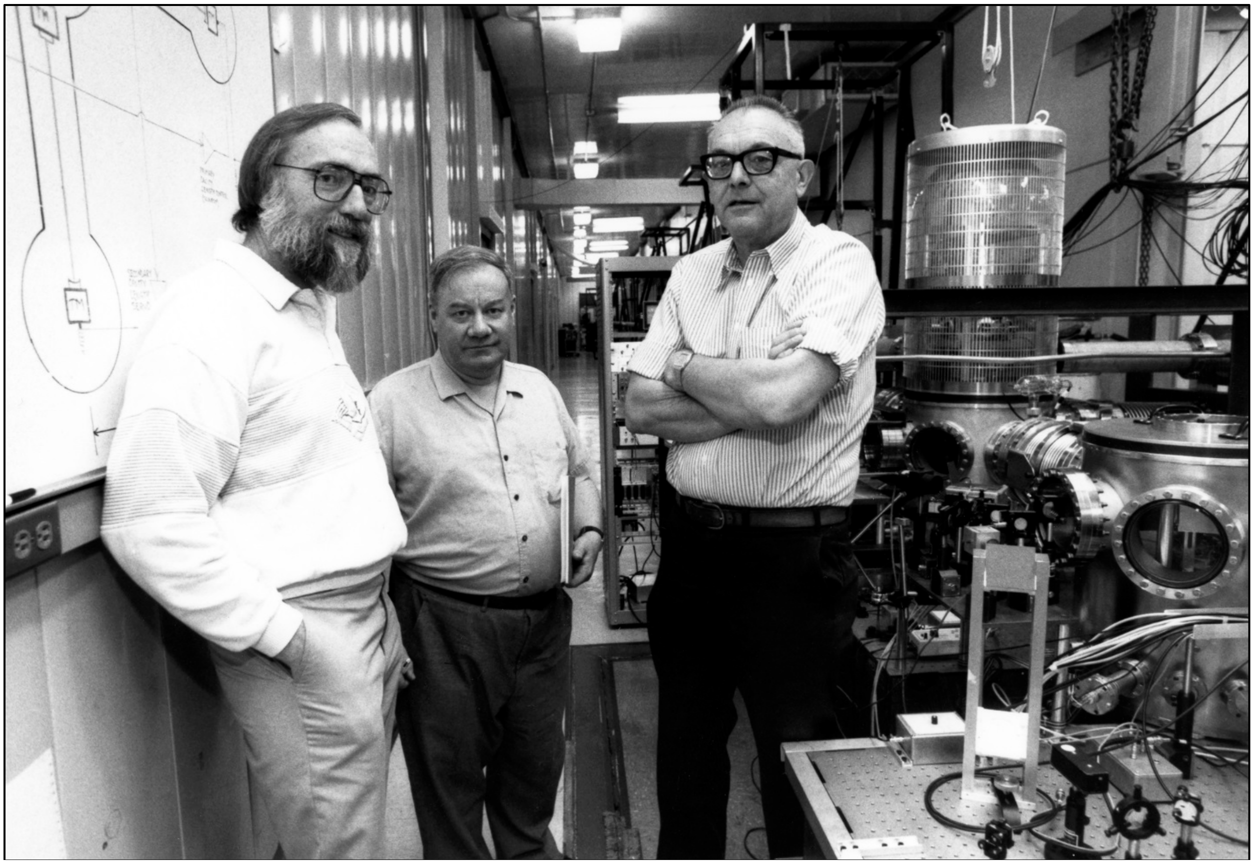
Ronald Drever, December 1976



Kip Thorne, Ronald Drever, Rochus Vogt, together with the 40-meter prototype, 1990

PREFATORY NOTE

TO THE INTERVIEW WITH RONALD W. P. DREVER

The interview with Ronald W. P. Drever, Caltech Professor of Physics, was recorded in the first half of 1997. Technical and logistical difficulties, in particular in the first interview session, resulted in inaudible portions in the recording. Some of this text was recoverable, but much was not. It was decided to note the inaudibility of words or of a phrase in square brackets rather than to tacitly suppress it. Although portions of text are missing from the interview, the present transcript gives the truest account of Drever's spoken word.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Ronald W. P. Drever **by Shirley K. Cohen**
Pasadena, California

Session 1	January 21, 1997
Session 2	February 10, 1997
Session 3	February 25, 1997
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Session 5	June 3, 1997

Begin Tape 1, Side 1

COHEN: Good afternoon, Professor Drever. I'm so glad that you've agreed to come and be interviewed. We'd like to know about your early years. Do you want to tell us something about your family?

DREVER: I was born in Scotland in a little village. The house is still there. I know the exact room where I was born. The family was an unusual one. My mother is English. She was from a family of three girls and had a rather unusual life, living in a very remote part of England in Northumberland, in a family that was not well off. But nobody had to actually work; they had money that they inherited, but not very much. They had this big old rambling farmhouse. My mother, for example, and the sisters never went to school. They were never trained—in this kind of Victorian style—never trained to do a job. So that's unusual.

COHEN: They had some schooling at home, I would assume.

DREVER: Yes, they had a governess, but they were basically not taught much. My mother is still alive and a very strong woman.

My father is Scottish. He came from a much poorer background, in an industrial area of the Clydeside. I understand that his father was a grocer, but we know little of his background.

He joined the army in the First World War and he was wounded. He must have been tough. He then took a degree course in medicine and became a doctor.

COHEN: Where would this have been, that he went to school?

DREVER: [Before the war, he] must have gone to some local school. I think it was called Alexandria, in Clydeside, a rather rundown area. After the war, he took his degree at Glasgow University. He was acting, I think, as a local doctor or something like that, in Northumberland—he used to drive a motorbike to see his patients.

COHEN: So that's where he met your mother?

DREVER: That's where he met my mother and got married. He moved back to Scotland, to a little village called Bishopton. When I was born, there were two doctors in this village of about five hundred people. When war [World War II] came, I was in school in Glasgow. Then the war got worse and it was difficult to travel. I had a complicated schooling because of the war: I went to a local school and the programs didn't match. After the war I continued at the Glasgow Academy.

You were asking about the things that were relevant for this [interview]. Probably the most relevant thing is that there was a very good science teacher. I was always interested in kind of practical, mechanical things—although my father knew nothing about that.

COHEN: So this was an interest that came from within you?

DREVER: Yes. He didn't know about that kind of thing; he was a doctor. But I had an uncle. He was an unmarried man, an artist.

COHEN: This was your father's brother?

DREVER: Yes. He failed at that; he couldn't make a living with his art. He did some commercial art, but he also had a backup thing, because he came from the same shipbuilding area. So he went into shipbuilding—a kind of semi-skilled labor, [useful] in wartime, of course. Anyway, he was a huge influence, because he would come and visit. He taught me how to make things; he was very skilled at that—at making things, as well as art, like carving things from wood and so on. I learned all my practical skills from him; [he was] a clear influence on me. Without him—I mean, he's the one who taught me how to make things. We did all kinds of things together. I built all kinds of gadgets with his help—models, steam engines, gasoline motors, all kinds of things. I was having a bit of this in school, but the big influence there was that I happened to have an exceptionally good science, physics, schoolteacher. That was truly important, I think.

COHEN: I hear that very often—a good science teacher.

DREVER: At that point—which was just about the end of the war, or just after the war—when I was back in school in Glasgow, I traveled every day by bus and underground. It was, I think, a good school—a tough school.

COHEN: Do you mean that you traveled into Glasgow to go to high school?

DREVER: Every day, that's right. My father thought it was the right thing. The family, by the way, was very poor when I was born. My mother and brother have been tracking down the history. There are some very interesting tales, beginning with how I was born; it was a problem. But anyway, that's not relevant to all of this. My father thought that this was the best school in the area. It possibly was; it was a tough school.

COHEN: He thought it was important for you to go? But you must have shown great promise in your work.

DREVER: I don't know. But anyway, this science teacher was important, because that was at the time when radar was all the thing. There was a time then, just after the war, when you could get all kinds of electronics. So I was very interested in that. My brother wasn't; he went into medicine; he's a doctor now. So it was kind of natural, then, that I would go on to Glasgow University.

COHEN: By this time, you must have moved into Glasgow.

DREVER: No.

COHEN: You still traveled back and forth?

DREVER: I still traveled. It was an hour each way—unless I missed the bus or train or underground. But that didn't matter; I could study on the bus very well. It wasn't wasted time. The key thing was to get first-class honors, which I did. So I went on and got a PhD there [1958].

COHEN: At Glasgow?

DREVER: At Glasgow University. I had an offer for a position at Cambridge [University]. I think it was a mistake that I turned it down—but, anyway, that's hard to know. But I had the possibility of going to work with [Martin] Ryle at Cambridge.

Anyway, I stayed at Glasgow, and it worked out pretty well. At that time, there was a big buildup of British universities—just after the Second World War—and Glasgow had been built up very strongly, with a synchrotron. A professor there, Philip Ivor Dee, had a big influence on me—he was a very strong character; he died years ago [1983]. He had been a student of Ernest Rutherford's at Cambridge. He worked with Rutherford; he was even involved in some of the early work, on resolving the proton. This obviously influenced him strongly. He built up the physics department [the Department of Natural Philosophy] at Glasgow. He was very strong in experimental techniques, and it certainly had an effect on me. He taught all his students how he thought they should do experimental physics, which was different from what is taught in the U.S.A. So in some sense, the techniques I'm using are Rutherford's, which are very different from what most other people use. It's a very empirical approach. You had to do practical examinations, in which you were set problems that you had to solve practically. It was a very good experience, I think, actually.

COHEN: You enjoyed it?

DREVER: I enjoyed it. Oh, I just loved it! That was as an undergraduate student. When I got to the PhD, the obvious thing at the time was to go into particle physics. That was what was happening there, with the accelerator [a 300 meV electron synchrotron—ed.]. I wanted to keep it on a small scale, so I went to work on radioactivity, basically, with a man who was well-known at the time, Samuel Curran, who was quite important locally. A year after I started with him, he left [1955]. He was known—at least locally, at that time—for developing counting techniques and low-level radioactivity measurements. Just after my first year, he left to become a major leader in the British atom bomb project at Aldermaston, so I was left with no supervisor. All the rest of my PhD career, I had no real supervisor.

COHEN: But you had a project, or a problem?

DREVER: I had to make up things myself.

COHEN: Nobody said, "That's OK"?

DREVER: Well, kind of. It was random. There were other people working who helped me, but there was nobody as good as Curran. In fact, I was influenced a lot by a Yugoslav who was a repeated visitor to Glasgow from the University of Ljubljana—Anton Moljk. He's still alive. [Moljk died September 5, 1998—ed.] It was hard for him to get out—he had had terrible experiences in the war. But anyway, he taught me more about how to do experiments; he was very skillful at building things out of nothing—out of rubber, glass tubing, sealing wax, and so on. That was an important influence on me, too.

I was always interested in other things, too—in gravitational things. One time—it was kind of an accident—I came up with one experiment.

COHEN: Now, this was while you were still a graduate student?

DREVER: Probably I had just got my PhD. I was still there [at Glasgow]. I was staying on there as a kind of postdoc or research fellow or something. I came up with the thought of this experiment. It was a very strange, weird one—very unusual.

COHEN: Was it along some lines of what you were doing?

DREVER: No. I don't quite know how I got on to it. I had always been interested in some of these old ideas—Mach's principle, and so on. A lot of theorists were interested in these ideas, and a book I read had a big influence on me; I think it was by Dennis Sciama. I don't know if it was by him or someone else. Anyway, one of these books had a discussion of this business of the anisotropy of inertia and the question of whether you could do an experimental check—that was the issue. Someone actually did attempt it with an atomic experiment. And then I thought of the idea of doing it with a nuclear experiment and it would be much more sensitive. That was almost all I thought about, in those days. There was a nuclear magnetic resonance experiment that I came up with. I didn't know a thing about that, but I happened to find some good books. There was no one around, that was the trouble; I was rather isolated. First, the department was mainly experimental physics. There were some good theorists in particle physics, but I was pretty well alone in these things. I had to just find out what I could, and I happened to hit on a very good book about NMR, by Hans Kopfermann, I think—*Nuclear Moments* [New York: Academic Press, 1958]—and then I saw how to make this experiment. It could be done with no apparatus and could still have very high sensitivity. NMR was very expensive and complicated, with these fancy magnets. By that stage, there had been some work done in this country, in which they measured the Earth's field using NMR. So I realized that I could do this experiment, using this technique originally developed to measure the Earth's magnetic field, to look for a fine splitting of a resonance line inside the nucleus, which an anisotropic inertia would have. And in some of the various theories, I would have ample sensitivity—if the theories were correct—to show it up, and by a big factor. And the beautiful thing was that I found you could do it with almost nothing. I was using lithium-7. But you had to find out, first of all, how to make the NMR system work and then expand it, to use it to focus this effect. And that was something I had worked on in the students' lab but had to make it much more sensitive for ^{7}Li.

Well, I borrowed equipment from the students' lab and so on; in fact, it went pretty fast. In about six months or so of messing around, I managed to find out how to do this and to actually make the equipment. The comical thing was that it ended up being done at home, in our back garden. Because we were in the country, and you had to find a place where it was quiet, with a uniform field, with no steel or iron around. My mother had a lovely garden; she was very keen on gardening. Over a twenty-four-hour period, the Earth rotates relative to the center of the galaxy. The hypothesis was that you would see an anisotropy caused by the center of the galaxy, and that you would see this effect in twenty-four hours—it was quite a popular theory. So it was a fun experiment. Right before I had done it, when I had part of the idea, I thought I should write something about it. I was very bad at publishing; I hate writing. But I did this, with Professor Dee encouraging me all the time.

COHEN: So you were telling him about this experiment?

DREVER: A little. So I sent a note off to *Physical Review Letters*. It was unusual to send something to an American journal; it was frowned upon; you weren't supposed to do that. Anyway, it was rejected. A note came back saying, "It's a good idea, but it's already been done by somebody else." It was being done by a chap called Vernon Hughes. It was essentially the same idea, but not using the Earth's field but using an NMR magnet. He was doing it in a very sophisticated way, but I realized that I could compete with him. My technique cost nothing and was probably better—it turned out it *was* better. But then, when I got this rejection from *Physical Review Letters*, I still thought I could compete and do better—"I can do it; the stuff I've got costs nothing."

COHEN: So you sent the idea in?

DREVER: Yes, it was a paper outlining the idea and already using—no, I did set an upper limit for the effect. It was rejected by *Phys. Rev. Letters*, and their argument was that it was already being done by somebody else. So anyway, I wrote another version of this, with some more ideas, and I went ahead and did the experiment. It was a fun experiment.

COHEN: So you were running around your backyard, stringing up—

DREVER: Stringing up things and so on, that's right. Our garage had a pile of car batteries, and, you know—

COHEN: It sounds like your family was very tolerant.

DREVER: Oh, yes, because it meant sitting up for twenty-four hours with it. I had to take measurements every half hour or so, and you had to keep the temperature of this big bottle of stuff [lithium-7 in solution—ed.] constant. You had to watch the temperature—it was all done crudely. And the photographic part was an ancient scope and camera. Everything was crude, but you could do the experiment.

COHEN: And you were doing this by yourself?

DREVER: Yes. There was nobody else at all, totally myself. It was a little bit dangerous, but that was the only way to do it. I made mistakes, you know, but there was nothing else for it. And it didn't take all that long. And I did write it up and published it.[1] And it was a bit more sensitive than this other chap's with the more fancy equipment, and mine cost essentially nothing—just a few car batteries and some wire. It was fun. Anyway, there was no effect, but that didn't worry me too much. The fact was, I found it so exciting that there might be something there of cosmology. The fact that there *might* be such an effect—

COHEN: Has anybody found it?

DREVER: No. It doesn't exist. I never contacted this other bloke. But later on, he published his work.[2] It was not as good as mine for a long time. It's now called the Hughes-Drever experiment in all the textbooks, but most people don't know that it was done totally independently. He didn't know about me, and I didn't know about him, when we actually did the experiments.

COHEN: So it was actually done independently—one with an NMR and one with—

[1] R. W. P. Drever, "Upper Limit to Anisotropy of Inertial Mass from Nuclear Resonance," *Phil. Mag*. 5:52, 409-11 (1960); see also Drever, "A Search for Anisotropy of Inertial Mass Using a Free Precession Technique," *Phil. Mag*. 6:65, 683-7 (1961).
[2] V. W. Hughes, H. G. Robinson, & V. Beltran-Lopez, "Upper Limit for the Anisotropy of Inertial Mass from Nuclear Resonance Experiments," *Phys. Rev. Lett.*, 4, 342-44 (1960).

DREVER: No, both were NMR, but my method was using the Earth's magnetic field, which is a natural field, and he was using a very fancy—the best you could get—powerful magnet, which was very difficult to get. I relied on a magnet you got naturally, for free—

COHEN: The Earth.

DREVER: If you're in a place in the country where there's nothing to disturb it. At that time, Dee was encouraging me. He said that I ought to go and work somewhere else and that I should go and take a year of research somewhere in the States.

COHEN: What year would this have been?

DREVER: Well, it ended up being 1959/1960 when I took this fellowship, at his encouragement. And at that time, the thing that suddenly got exciting was the Mössbauer effect and using it to measure the gravitational redshift. A race was going on between R. V. Pound at Harvard and the other main contender was a group in England, at Harwell, the atomic energy place. They were racing each other; they both had, more or less independently, the same idea. It was a critical and key experiment for physics, and they were racing one another to do it. So I wanted to go to Harvard and join this chap Pound, who was doing these exciting things.

COHEN: So you applied for a fellowship, and they asked you to come?

DREVER: I got it, I think, because of this weird experiment I had done. It was so unusual and kind of original. So there was no problem; I was invited to Harvard to join Pound, and I spent a year there—and that was a great year.

COHEN: Now, was that the first time you had been abroad?

DREVER: I had a kind of provincial existence: I didn't travel; I had no spare cash. I didn't go abroad for holidays, because of work. It was pretty near the first time I was out of the country. So that was a great year. I found the whole place astonishing and so different from what I had expected.

COHEN: Did you find a place to live in Cambridge?

DREVER: No, they put me up inside Harvard, in one of the student houses. By that stage, Pound had just finished the first part of this experiment. He had won the race against the group in England. He had done it with one other person, Glen Rebka. They had discovered an effect which the British team hadn't discovered at first—a temperature effect which was critical to the measurement. He was on to a second generation, and it happened that my skills were exactly right for that, so I went there to help on that. The second generation involved much stronger sources and better detectors, and I was an expert on making radiation detectors. I knew exactly how to build exceptionally good and unusual low-energy detectors, and that's what I did for him for the experiment. I built a very weird and unusual detector. It cost almost nothing. It covered a big area, had high sensitivity, and was very unusual, but it worked wonderfully—much better than anything before. The point was that it had a much higher counting rate, used much stronger sources. It was a great success.

COHEN: Now, this was really the first time you were working in a group. Is that correct?

DREVER: Well, it was hardly a group. Pound hardly had anyone.

COHEN: The name is vaguely familiar.

DREVER: Yes, he's a very distinguished scientist. He played a key part in NMR—but he was a bit of a loner, too. The person who worked with him, Glen Rebka, had just left, just as I joined, so I was pretty well working alone there, too—well, sometimes with a graduate student. So that was a very exciting year.

COHEN: Did you do much traveling that year? Did you see other things in the States?

DREVER: Not much. I was mostly working. I did take a vacation; I traveled across country by train to visit California and so on. It was a great vacation. I visited places like Berkeley and Stanford and so on, roundtrip by train. Of course, I was close to MIT, but I didn't visit very much. Then I went back [to Glasgow] and just worked away still on the same kind of stuff I had been doing before, in radioactivity. I was switching out of it. I wanted to see how you could make what are now multiple detectors and imaging detectors. I had a string of students, one after the other, at that point.

There was another funny accidental thing. I was doing this rather boring—not very exciting—stuff. But, of course, I had a group at that point, I had equipment, I didn't really have to find grants. You could more or less play around. I gave talks at various places, and kind of by accident I got to know a man at Harwell. You know Harwell? That's the atomic energy place. This was John Jelley. Unfortunately, he's now got cancer; it's very sad. But anyway, he was an enormous influence and a big help to me. I think it came about because one of my students had gone and got a job at Harwell and invited me down to give a seminar there. That's happened a lot; my students have got jobs all over the place, especially in Canada, and they invite me to give talks. And he introduced me to this chap John Jelley, who is very unusual—and somewhat similar to me. Because although he was at Harwell, which was officially an atomic energy authority, he refused to do any work that he was told to do; he just wanted to do his own thing, and he did. He was working on cosmic rays, which had some vague excuse to be done at Harwell. He actually made quite an important discovery about Cerenkov radiation from cosmic-ray showers in the atmosphere. He was known to be kind of unusual and did interesting and unusual things, not the routine stuff that most of Harwell was doing—though they had very good physicists there.

COHEN: So they did tolerate him?

DREVER: Yes, that's right. Although it was a running battle to keep going, not doing *any* of the official work that Harwell was supposed to be doing. His stuff mostly involved starting with this Cerenkov radiation, which meant looking into the night sky, with arrays of forty multipliers, for fast coincidences—to find cosmic-ray showers at very high energy. It's still going on, this kind of work. He was doing it with war-surplus equipment he got—war-surplus guns off of ships, and searchlights. He would mount these together and put the multipliers in the gunmounts, and so on, and set up these things outside Harwell, which is situated in the country. Anyway, the point of this story is that he had been doing these odd experiments, and so we got together. We were doing similar—

COHEN: You met him when you went there to give a talk?

DREVER: Yes, and he invited me to come visit there, and I did. We got to know one another and decided to collaborate on various experiments. I was still doing my own thing at Glasgow, but he invited me to join him on some of these experiments. They were interesting ones. For example, it was just around that time that pulsars were discovered [1967], and I was interested in that anyway. But even more interesting was that the girl who discovered them had been a student of mine at Glasgow.

COHEN: Oh, is that right? Jocelyn Bell. We're getting into radio astronomy, you see, so I'm on familiar ground.

DREVER: That's right. Well, the strange thing was—again, another kind of accident. She's Irish, but she had come to Glasgow to do her degree in physics—an undergraduate degree. And I happened to be allocated as a supervisor for undergraduate students.

COHEN: And she just happened to be in your group?

DREVER: That's right. She was also obviously better than most of them. And so I got to know her fairly well. She had an interest in astronomy, and at that time in England the key place was thought to be Jodrell Bank. I tried to get her a job at Jodrell Bank—I remember I wrote a reference for her—and they wouldn't take her on, and the story was that it was because she was a woman. But that's not official, you see. So she was very disappointed. Her second best was to go to Cambridge. So she went to Cambridge and discovered pulsars.

Now, John Jelley had the idea that maybe there were X rays coming off them. In the early days, all that was discovered were radio pulses. The idea was to look for light pulses, X-ray pulses, and that was just the kind of thing he liked to do—look at things in the night sky in a dark place. So we got together on that and did a whole range of experiments, none of which gave any answers. But that didn't really matter. They were all negative. But we looked for light pulses from the Crab pulsar, and it turned out that we were on the right track—

COHEN: Was it that some of your equipment wasn't good enough?

DREVER: No. I'm slightly wrong: This was done at Cambridge, a small telescope at Cambridge—John Jelley had several friends down there. There was a chap there who was keen on this and set up all this stuff to analyze the pulsars and so on; and it turned out that he somehow was slow in analyzing it, and it was eventually discovered here. Then we went back to his tapes, and it was there on the tapes, all the time—he had recorded it. Anyway, it could have been got, but it wasn't. Anyway, it was fun to do it. And I learned a little more about these astronomical things doing that. I got in the habit of going down there every month when the moon went down, because we required dark skies for all of these things. And so I did a whole string of things with John Jelley. We looked for X rays, and pulses also; the idea was that they would cause fluorescence in the upper atmosphere and you could see that. We were close, but it just wasn't quite sensitive enough. They were found afterwards, but by conventional means. They had not been seen by this method, and it was just marginal.

Anyway, I got involved with lots of things of that kind. He was very keen—and so was I—on looking for coincident optical pulses or radio pulses that might come from supernovae. This had been predicted by [Stirling] Colgate. John Jelley organized all this. He had friends in Ireland, and we set up a coincidence experiment, very much like gravity waves—simultaneous signals. So, in this case, we started with a simple setup outside Glasgow; there was one at Harwell, one in Dublin—

COHEN: So you had your own array?

DREVER: Well, it was John Jelley's. We were part of this collaboration. It was a joint effort, and we set up these things. It all came to nothing, although we all had some strange unexplained events, and so on.

COHEN: But you were developing good equipment all the time.

DREVER: Yes, well, some of it—a lot of it. Very, very instructive. So then I had this quite interesting period [in the 1970s] when I was enjoying the visits down to Harwell as well as the stuff I was doing in Glasgow University.

COHEN: And you must have had your university duties to teach and all these things?

DREVER: Yes. Actually, I probably did less teaching than most doing research.

Anyway, so that was quite interesting and fun. It didn't get anywhere; the results were all negative. But that didn't worry me too much; it was quite interesting. Now, it was around this time that Joe Weber appeared on the scene, investigating gravity waves.

COHEN: Had you been interested in gravity waves at all beforehand?

DREVER: Yes, I had been interested in gravity, as I said. I think I knew about Weber's work from quite early on, after he started. And then he started to find gravity waves [1969], and I had to give a lecture on this.

COHEN: Had he already built this piece of equipment [a bar detector]?

DREVER: Oh, yes. And then I found that there was already a group in England doing some work in that field. A key thing was that there was a physicist called Peter Aplin, who was a very original experimental figure, and he thought quite deeply about the problems with these [bar-detector] experiments, and we both realized that we could do a lot better than previously thought possible. How do you set up a system to get more than the noise level? At the time, people thought you couldn't do much better, but this chap realized you could [with a split-bar detector]. It was kind of controversial; if it was right, it was going to be a very big breakthrough.

Begin Tape 1, Side 2

DREVER: So, this chap Peter Aplin—he started on a different line of thinking. We now know that some of it was right.

COHEN: To get more sensitivity?

DREVER: To get more sensitivity. There was lots of discussion about this. There were various people who were involved with it. Stephen Hawking got so impressed by these ideas, and what's more, he could explain it in a way that other people could understand, and this chap [Aplin] wasn't very good at explaining it. This all happened before I was in it, so I don't know all the history. What happened was, however, Stephen Hawking published a paper on this [split-bar detectors], pointing out that this was a way of getting much more sensitivity and [inaudible].[3] It really was the ideas of this chap, and they acknowledged that, but Peter Aplin hadn't published, hadn't written anything about it. But fundamentally these were his ideas.

COHEN: But he was given some credit?

DREVER: Oh, yes, he was mentioned in the paper. And there was a project in England, basically to duplicate Weber's system, which was done by a chap called Douglas [W. D.] Allen, at the University of Reading, and he was way ahead in duplicating that. This chap Douglas Allen was working, again, at Harwell, or it may have already been part of the Rutherford lab—I'm not quite sure where he was. He designed and built quite a big apparatus, using the facilities of Harwell, and started to set up experiments on this kind of thing. It was around about that point that I got interested. I also saw how you could come up with a slightly different [inaudible].

What I did was—because I'm not so strong in mathematics—I played around with an early computer model of this thing in the students' lab, with the students' analog computer. I played with it, and suddenly made a kind of discovery, which was that if you did this right, you could actually pick out the shape of the waves. And I found it kind of accidentally, in the students' lab, with a model of the apparatus that the students used. **[**Inaudible**]** So I thought I would have a shot at it, too. [Long inaudible portion] I thought I would have a shot at it, too. It was really the same idea as Aplin's; [inaudible], but the fundamental idea was really the same as his. But I realized how we could do a lot more than that—that we could see the shape of the waves. Dee encouraged all this very much. He said, "Go ahead. Build the apparatus and then ask for the money afterwards."

COHEN: So you had some money from the department, then?

DREVER: Yes, very little. But we found ways to do this very cheaply—much more cheaply than anybody else, including the people at Rutherford lab, who managed to be allotted funds. [Inaudible] Anyway, we threw together an apparatus. [Inaudible] So I pushed on, got

[3] G. W. Gibbons & S. W. Hawking, "Theory of the Detection of Short Bursts of Gravitational Radiation," *Phys. Rev. D*, 4:8, 2191-98 (1971).

encouragement, and some money, and so on, and not just trying to duplicate what Weber had done but going one better. The plan was, assuming that Weber was right, we predicted that with a relatively small apparatus we could detect these waves, and we could detect them with a very short resolving time. And, if we had two sets of these things, we could actually measure the *speed* of the gravity waves. That would be a very important thing to do. At that stage, one of my former students—he was a very good student, and the best I had—wanted to work in Sweden. He got a job in Sweden, and we decided that I would try and build two of these detectors and he would set one up in Sweden and—

COHEN: What was the name of the student?

DREVER: This was Peter Dougan. It never actually happened, but that was the plan—that we would build two detectors; we would look for the coincidences. We'd put one in Sweden and with the resolution that we could get, we would measure the gravity waves, and that would be a fantastic confirmation. It all depended on Weber being right [about having detected gravity waves], which of course he wasn't.

From the very beginning, there were a lot of technical problems [inaudible]. People at Rutherford had been working on them—at Rutherford and Reading. [Inaudible] A chap was working with me—Jim Hough—and we just managed to do the experiments so much more effectively, and so much faster, that within about a year we had built better instruments than they had managed to make in years of work, with more resources, and so on. The key thing, which I had been taught, was to do things very fast: Don't try to do accurate experiments; do rough experiments very, very quickly. Just do them as fast as you can—don't care if they don't work. Learn as much as you can, very, very quickly, until you find out how to do it right. And the last thing you do very carefully, but the key thing is to move very fast at early stages—this was quite against the grain. Anyway, in this area, it was extremely effective, and I managed to explore dozens of [methods]; I'd also do it on a shoestring.

Some of it was comical, but it worked; that was the great thing. You have to cement transducers onto this bar, and you want to try it with different kinds of glue, but once you've glued them, you can't get them off again. But the thing is, they all cost money, and I didn't have much money. So I ended up using—again, a rather strange accident—an epoxy glue from two things that you mixed together, a hardener and a resin. I found that if I didn't set it but just melted it, I could get [the transducers] off again. I did that, and it turned out that it was much better than if done the proper way.

These kinds of things were breakthroughs, and they were kind of accidental, but it was partly from trying many things very fast. We ended up building detectors that were much better, as well as much smaller and cheaper, than the ones being built by other people. And we did experiments. We set up two in Glasgow, and they worked very well for that period. [Inaudible] We were actually, I think, probably the first group to show that [Weber was wrong]. But as it wasn't happening in the States, it was rather ignored there. In fact, we were not quite the first; the very first experiment was done in Moscow, by [Vladimir B.] Braginsky, and that was also rather discounted, because it was happening in Russia.

COHEN: So nobody was looking at these other things?

DREVER: Well, not much—or a little bit. But anyway, the first one was a quick one done by Braginsky, and the next one was the one I did, which was much more extensive. But the thing was that it was a good experiment. We did a whole string of them. We developed various kinds of searches. [Inaudible] It was really exploring all the techniques for the first time. [Inaudible] So I spent more and more time on this and I was very excited by it.

COHEN: When was the first conference that you went to, where you say you met Kip Thorne?

DREVER: Oh, there were a whole lot of conferences.

COHEN: Now, were these conferences just on gravity waves?

DREVER: No, mostly they were on astrophysics. There was a particularly dramatic one, which you may have heard of, in New York [6th Texas Symposium on Relativistic Astrophysics, New York, December 18-22, 1972—ed.] In this country, people were [investigating Weber's claims], particularly Tony [J. Anthony] Tyson at Bell Laboratories; a chap at the University of Rochester, David Douglass; and a chap at IBM. The IBM one was [James L.] Levine [with R. L. Garwin—ed.]. [Inaudible]

COHEN: They didn't get a result either?

DREVER: No. At this conference, Tyson had brought tapes from, and exchanged tapes of data with, Joe Weber, and Joe Weber got Tyson's tapes and announced he had discovered gravity waves in collaboration with Tyson, or his tapes. And then Tyson said, "No, that can't be." Because he had given him the dates during Daylight Saving Time, and there was an hour's difference, and [inaudible]. It was very unpleasant.

Already—before all that—I had began to wonder about interferometers, because fundamentally that's the most direct thing to do**.** But at that stage, it was clear that [long portion inaudible]. And also there was some pressure in England. In fact, I was given an offer: If I wanted to set up a similar thing to the [cryogenic detector] at Stanford, I could do that. I could get money if I wanted to do that.

COHEN: And you would have done that in Glasgow?

DREVER: No, probably at Cambridge. Probably. It wasn't a firm offer, but that was what was generally being discussed.

COHEN: But you didn't feel this was the way to go?

DREVER: No. Stanford was doing it. I had no expertise, it wasn't clear that it's the right way, so I decided not to. I thought that maybe the interferometers might be more promising, and I talked with John Jelley about setting up vacuum pipes. [Inaudible]

COHEN: So you were really looking at this new technique—laser interferometry—at this time?

DREVER: That's right, I was thinking hard about this. Also I thought it could probably be done cheaply. [Inaudible] I didn't really start on it for a long time; I was thinking about all kinds of things, all kinds of weird and wonderful things. I spent two years thinking about it. Then the key thing was that I heard about a chap called Robert Forward, in this country, who was a student of Joseph Weber. I got to know him pretty well. He decided to go ahead and make a laser interferometer. But he decided—and I don't know if it was *his* idea; I've heard conflicting things; I've heard Rai [Rainer] Weiss say that *he* invented the idea of making an interferometer. But this chap Robert Forward decided to make one.

COHEN: Now, where was he?

DREVER: He was at Hughes [Research Laboratories].

COHEN: Oh, out here.

DREVER: Yes, and while working at Hughes he managed to persuade Hughes to do this. He actually set to work and actually built a gravity-wave detector, a small interferometer—and I knew about this somehow. But anyway, he came visiting in Scotland and visited us. We had a lot of discussions, and that made me think it was much more feasible, perhaps, and that we should be able to do it somehow. And then I heard about—

Now, Rai Weiss did have a key idea. I'm not sure who really thought first of using interferometers; everybody, including Joe Weber himself—probably it *was* Joe Weber. But anyway, Rai Weiss realized that at the time there was [inaudible]. And Rai Weiss realized that you could use that as a way to get [inaudible]. So he proposed that. Now, he didn't publish, but somehow, I think it was in a review somewhere; it was some unpublished internal report or something. And I got a copy of this from this friend [long inaudible portion]. But then I didn't. First of all, it was very expensive, and I needed money. So I came up with a different method, and this was possible partly because of the group at Reading—although there'd been friendly rivalry with the people at Reading. There was a very good electronics expert who built [inaudible]—the very best in the world**.** And he actually built us [long portion inaudible]. It was a friendly rivalry. So we were somehow doing the same thing as the people at Reading, but they were very nice and very friendly. And the chap retired and offered us all his equipment for nothing, and it was much better than ours. It didn't work as well [laughter], but it was much bigger. They had nice vacuum pipes and so on. So we got all this for the cost of the truck to Glasgow.

COHEN: You took it from Reading to Glasgow?

DREVER: Yes. And then I decided what I could do was, I could modify this [inaudible]. So I invented a special kind that would work—it had one key idea for me [inaudible] White cell. And I invented a different kind of system, which was quite compact and so on. It was still going to be enormously better than any of the bar detectors, although [inaudible]. But still it was going to be [inaudible] at the time. And I decided to have [inaudible]. We had to get money to get a laser, but that was the only thing we had to buy now. But we did get money. Professor Dee was very helpful; he managed to get money to buy a laser.

COHEN: He was wonderful. He just encouraged you every step of the way.

DREVER: I owe him an awful lot; he's an outstanding man. So we started to build this thing. We found out that there was competition. Not from Rai, who was moving very slowly at MIT, but in Germany, where there was a group at the Max Planck Institute in Munich. And they decided to [inaudible]. They were really computer experts, but they were very good engineers and they had lots of money. And they had been working for two years or so when we first heard about [inaudible] who started all this. They really copied Rai's ideas exactly. For a time there was a friendly rivalry. I like friendly rivalry. It's nothing like fierce competition.

COHEN: Well, it makes the blood go.

DREVER: It makes it more interesting. We shared ideas and so on. We got more money, so we got going pretty early, but then we found a problem—problems of scattering. They happened to be worse in the particular objects that I was using for [inaudible]. It happened to be the very worst [inaudible]. And that was critical. Then we tried to see a way around that [inaudible]. I came up with an old idea I had had, and this was an important one. It was the idea that we could use Fabry-Perot cavities. It was kind of accidental. It's now the technique everybody uses. [Inaudible] but everybody back then thought I might be mad. It comes from an ancient idea [long portion inaudible].

The strange thing about this was that it was through the students' lab at Glasgow that I learned about this. Actually, I learned about this from [inaudible]. And [inaudible] was a very good student lab. They had experiments [inaudible] in which students—you made it all yourself. The key part of the apparatus was this very delicate Fabry-Perot [inaudible], they're called, which are typically very expensive and very [inaudible]. But in this lab you were given bits of ordinary window glass and a glasscutter. So you'd make it yourself, and you learned how to do it. You could take any old piece of window glass, cut it, coat it, and make your own mirrors of it, find where it was flat, and then set this up in such a way that [inaudible]. I found this so exciting—that you could make this from bits of window glass and bits of paper and rubber bands and screws and you'd end up with [inaudible] resolve the sodium lines, which at the time I thought was just incredible. From this crude, almost nothing, you could build this thing that had this fantastic resolution. I still remember being impressed as a student. I knew how hard it was to make it—to get the coatings right, and so on. That was incredible, that you could make this from nothing. Then I thought, Could we expand it? Could we make it for hundreds of yards?

We have done that, and that was the key idea. And the great thing about it [the Fabry-Perot system] was that compared with the delay line, which Rai Weiss had proposed, I thought it was going to be cheap, because the mirrors can be very small. I had thought about hundreds of yards, whereas in Rai Weiss's thing, you have to [inaudible]. We thought we could afford to do that; all we needed was cheap small pipes, and so on. But nobody had ever made a Fabry-Perot cavity like that, and there was not [inaudible]. It was also a way of dodging this scattering problem. So I thought, "Well, I've got a key new idea here."

I wasn't sure if it would work. I didn't do much about [inaudible]. It would have to have more stable lasers than we had ever had before. We would have to somehow lock the laser to this cavity. Now, I already, in working with Robert Pound, [long portion inaudible]. And so I didn't know anything special about it, but I thought [inaudible]. So I phoned around to find out who the best experts were [inaudible]. I talked to Rai Weiss and John Hall. I went to Rai Weiss and outlined this idea. [Long portion inaudible] This thing depended on having lasers.

OK, so I went on to Boulder, Colorado, where John Hall was. We got on wonderfully. [Long portion inaudible] So he said, "Come and try it here." Because he had all the equipment; he was the authority on lasers. [Inaudible] And I said, "Yes, sure." But he was very [inaudible]. In fact, he assured me [inaudible]. And we went back to Glasgow and tried to start something there and that also [long portion inaudible]. Jim Hough made a similar thing in Glasgow—not quite the same [as the Boulder device], cruder. It also worked after a bit, so that was the key thing that showed us that it was possible. It turns out that that seems to be what I am getting credit for now: this laser-stabilizing thing, which was kind of incidental but is now, in some sense, a success. It's what everybody is using now. I wish now that I had patented it and gotten money out of it.

[Tape Ends]

RONALD W. P. DREVER

SESSION 2

February 10, 1997

Begin Tape 2, Side 1

COHEN: I think it would be a good idea to start today with your coming to Caltech, or even perhaps to step back a bit, to your being asked to come to Caltech. How did all that happen?

DREVER: Actually, I've forgotten just exactly where we got to last time. To recap a little bit, I had started at Glasgow making these two split-bar gravity-wave detectors and doing experiments with them. When they were finished, I spent a couple of years thinking about what one should do next. I decided that making interferometers looked promising—in fact, [I thought about it] for several years before I ever got into it. I kept feeling that this was really, fundamentally, the right way to do it, but it just looked very hard. After a couple of years of thinking about all kinds of crazy schemes to detect gravity waves—I have notebooks full of them—I came to the conclusion that maybe the interferometers were the most promising thing to do.

At the time, it was generally thought that cryogenic versions of [bar] detectors were the best, and they were being developed in a big effort—it looked like a huge effort to me—at Stanford and also at Louisiana State University. These were big efforts, and I really couldn't compete with them, because they were going to be so very expensive. Interferometers were maybe something that could be cheap. Particularly, I thought they might be appropriate to do in Scotland, because what was needed was space, and space was essentially pretty free, and they could perhaps be cheap things to make. So that was another important advantage—that this might not cost very much and might be competitive with the high-tech cryogenic efforts.

COHEN: Now, Ron, we're dealing with the early seventies now. Is that correct?

DREVER: It was about '74, maybe.

COHEN: OK. Now, were all these people coming to meetings, so that the ideas were being discussed?

DREVER: Yes, it was a time when there were many meetings. I was influenced by a review paper by Kip Thorne at one of these meetings that referred to the unpublished work of Rai Weiss about interferometers. I got an unpublished copy on microfilm; that also reinforced my thoughts. In particular, Rai pointed out a new aspect—that you could pass a light many times through a system and get more performance out of the interferometer. [4]

But I was going to go with the interferometer anyway. The other person who influenced me a lot at that stage was Robert Forward, at Hughes. He visited Glasgow at one point, and we had lots of discussions. I was very impressed by what he was doing. He was actually making an interferometer at the time, and it looked to me very promising. I wasn't aware of anyone else doing anything about it at the time. It was clear that Rai Weiss was thinking about it, but Forward was actually building one, and that struck me as extremely interesting.

So I thought maybe we'd have a go at it. Even then, we had very little money. The situation was kind of odd at Glasgow. There was a lab, and the way it worked there—there was money to do things as long as you didn't have to buy any equipment. People weren't exactly free, though. And what was lacking was vacuum pipes and so on. And a laser. So we decided to apply for a grant. We did, from what was called the SRC [Science Research Council] at the time—later called the SERC [Science and Engineering Research Council]—to purchase a laser, as a key thing. We managed to get, at the end of the bar period— I think I told you: The people at Reading closed down. The man in charge retired and offered me his equipment. We trucked this up to Glasgow.

COHEN: Right. You told me about that last time.

DREVER: Right. And we modified that for a laser operation. At that time, the other people who were doing this were the German group, in Munich. I visited them and knew about their work and was very, very impressed by it. I was quite envious. They obviously had lots of money, as far as I could tell, and they had wonderful engineers, and everything was built just beautifully—so much better than we could do. They were working very hard on the delay-line system, an exact copy of the kind of thing that Rai Weiss had been suggesting. I thought we couldn't

[4] R. Weiss, "Electromagnetically Coupled Broadband Gravitational Antenna," Progress Report 105, MIT Res. Electron. Lab. 54 (1972).

compete with that. So we set this up with a different kind of optical system—a variation of what's called the White cell [delay line]. It was a kind of delay line, but very different from anything anyone had before. The idea was that we were going to magnify the motion of the bars with a lever system and use tiny mirrors and the laser system, which would give us, on a small scale, the performance of a much larger apparatus. In principle, it was fine. When we started to get it going, what we found was that it had a much higher noise than we expected, and it took us a long time to find out what it was. A key thing in finding out was a comment from one of the German group, Walter Winkler. They were also finding problems with their delay-line system, which wasn't working as well as expected. It turned out that it was the same problem for both of us. He had realized, and afterwards I realized too, that scattered light could be much more serious than I had at first thought, because it was coherent. And they found that that was their problem, and we found that that was also the problem we were having.

Once we saw that, it was a kind of breakthrough. We could see now that when we tried to adjust the optical paths, equal by ten equal things, they would always keep changing. Then we realized that the scattering light was messing it up. And the particular optical scheme I was using, this White cell, had very small mirrors at one end and light beams overlapping on top of one another, and that was the very worst condition with scattered light. So in our case it [the scattering] was a huge effect. This showed us that this scheme was really no use and made me go back to the idea of a Fabry-Perot cavity. This was a key breakthrough, I felt.

COHEN: Was this about the same time?

DREVER: Well, I'd thought about it in earlier years—I think I may have mentioned this—because I remember doing experiments as an undergraduate student on this. But now I suddenly realized that there was a solution there to this scattering problem. Also, that this would be a way to make an interferometer system that was much cheaper [and] used much smaller vacuum pipes than what Rai Weiss was thinking about.

At the time, this was quite a new idea, and I still think it's a real breakthrough. It later caused all kinds of problems with Rai. This was a very important issue. The delay line which Rai had proposed—it had been invented by another chap, called [Donald R.] Herriott; the Herriott delay line—used great big mirrors and lots of spots: a hundred spots and several of these great big mirrors, and so on. The thing required huge vacuum pipes and had the scattering problem that the Germans discovered. It struck me that the thing to do was to have just two spots on small mirrors and have the light trapped between these two mirrors, so that the light would always travel between the same two pairs of spots. The thing that triggered me at that point was that any scattered light— There were only two spots of light on the mirrors anyway—there weren't hundreds of spots of light—and so any scatter would just cause the light to go back on its own path anyway; it wouldn't make any difference. So the first point about this [idea] is that it was triggered by the scattering problem: Here was a way of somehow making [the scattering problem] completely innocuous. And then the other major advantage was that the mirrors only had to be big enough to have one spot of light and not a hundred spots of light, which meant that the vacuum pipe could be smaller by a big factor. And, again, I was going after cheapness. It was something we could afford to build cheaply in Scotland.

The big difficulty was that Fabry-Perot cavities, up until then, had usually been tiny things with spacings of centimeters between them, not lots of meters. And it wasn't clear that you could do that; nobody seemed to have ever done it before. As soon as you thought in detail about this, you realized that this required a purity of the frequency of the light from the laser—extremely high wavelength purity, much higher than lasers normally gave. Could that be achieved? At the same time, more or less, that I was thinking about how to do this, it struck me that you couldn't just take a laser and point it to this cavity when the cavity was meters or maybe hundreds of meters long. The light wouldn't resonate. It had to resonate in this cavity. So I wondered how we could do this. The experience I had had working with Robert Pound at Harvard during the year I spent there was useful, because it showed me the value of modulating the effect that you're trying to measure. He was modulating gravitational redshift effects, with the Mössbauer effect. Although the experiment was totally different, I could understand now in a general way how this modulation idea was a great help. So I could do the same thing here. I would modulate the frequency—or, in this case, the phase—of the light. And I made up a way of how we could take the light from a laser, beat it with the light in this cavity, and modulate, and I drew up two schemes for doing this. Here was a way that we could actually lock the laser to the cavity. It didn't strike me at that point as particularly special; it struck me that it was an obvious way of extending the same kind of techniques that Pound had been using for his redshift experiment. We found afterwards that he had had a similar idea to mine but in a different context, but I was unaware of that at the time.

So, as I had really had no real experience with lasers or anything, I decided to make this trip to the States and visit people. Basically, I explained this idea to Rai Weiss, and he didn't understand it and said it couldn't work. I talked to John Hall, and he eventually understood it and got very enthusiastic about it. It was then that I realized that people hadn't thought about [this] before; I hadn't realized that it was really new. So then John Hall and I decided we would try it out first in his lab—he had all the stuff. Simultaneously we built the setup to try at Glasgow and at Boulder.

COHEN: John Hall was in Boulder, Colorado?

DREVER: Yes, at JILA [Joint Institute for Laboratory Astrophysics], in Boulder. His name is John, but everybody calls him Jan. I spent a couple of weeks there, and we made this thing work wonderfully there. I think I told you about Pound being there and seeing it. At that time, the person working with me at Glasgow was Jim Hough, a former student of mine who had just gotten his PhD in a different field. Prior to all of this, the work I had been doing was nuclear radiation detection, X-ray detection. I had developed some multiple-wire proportional counters with the idea of using them for medical X-ray detection as well as for particle detection. Hough had done his PhD on a device of that kind, which we'd worked out together. Anyway, so he switched to this, and we both set to work, with gradually more people joining and helping. When we got a laser, we tried to set it up this way and lock it to a cavity. All this was new to us. We had to get someone who would make us special mirrors. We found there was a small company in London that could do this for us. We got this to work at Glasgow, too, in a relatively crude way. It was done much more nicely at Boulder.

COHEN: Were you talking about this at meetings?

DREVER: Yes, we did describe—

COHEN: So you had lots of meetings during these years.

DREVER: That's right. During this time, I was visiting Caltech frequently, although I think not officially.

COHEN: Who asked you to come to Caltech? Was that Kip?

DREVER: Yes, that's right. I had unofficial visits for a short period.

COHEN: But nobody was doing anything here [at Caltech]?

DREVER: No, nothing was happening here.

COHEN: You just came to talk to Kip and to see what could be done?

DREVER: That's right. No, nothing was happening here.

Braginsky also had an influence. I had a very high opinion of him and still do. He'd been working on a totally different kind of technique. He was doing things that he could do in Russia with rather limited resources, mostly in connection with making miniature, very high-performance bars out of sapphire. However, he's a deep thinker, and thought hard about these things, and pointed out quantum limits to the measurements, and so on, which I was very interested in, too, at the time. I went to hear him at conferences and so on.

COHEN: So you talked to Braginsky at conferences. You never did go to Russia, did you, to talk with him?

DREVER: Only once. That wasn't a chance to talk, that was a quick visit to see his laboratory. It was very interesting—but the science was all discussed at meetings, at conferences. There was a very influential conference for me at Erice, where Braginsky gave a series of lectures and we had many discussions.

COHEN: Do you know what year that would have been?

DREVER: No, I really don't. [The Erice conference was in 1975—ed.] For me, it was a very important conference. It was when Braginsky brought up and discussed at length the ideas of what is now called quantum nondemolition. It was very controversial.

COHEN: Now, that was his idea—this quantum nondemolition?

DREVER: That was his word. "Quantum nondemolition" was his expression. He invented it. It's probably from the Russian—anyway, it was a funny way to describe it.

So that was very instructive from that point of view. It turned out afterwards that he and other Russians—in particular, the Russian literature of many years back, had a lot of ideas, too, which I wasn't aware of. I think he had talked about making a Fabry-Perot cavity, but not the same way. I found out afterwards that some of the ideas were there. However, the other key aspect that I suggested—not just the use of the Fabry-Perot cavity, which at the time I didn't know anyone else had thought about, though afterwards I found out that Braginsky had thought about it—was to make a symmetrical system in which you had two identical Fabry-Perot cavities 90 degrees to one another: two arms of an interferometer. You put the light into them and bring the light out again—interfere the light together again. The key thing was that the system was symmetrical, so that any residual fluctuations of the laser frequency would cancel out. That was another key physics point in the thing I was proposing. Now, Braginsky never seemed to think of or want to do it that way. In all the things he talked about, he talked about a clock; a generator, by which he meant the laser; and you had a thing that was monitoring this. You were using that clock to measure the length of your arm—so it was inherently an unsymmetrical system. That was a key distinction, though perhaps that's not noticeable in the literature. I was sold on the idea of making symmetry, so that all kinds of problems that you don't even know are problems don't even appear. That was a key part, OK?

At that time, at Glasgow, we felt that there was some kind of friendly rivalry with the German group.

COHEN: They were independently doing their thing?

DREVER: Oh yes, totally independently. And we felt a bit envious, because they seemed to have more people, more money, more of everything. And everything they built was so beautifully built, and ours was kind of thrown together.

COHEN: But these people did come to the meetings? You all talked together?

DREVER: Oh, yes. We couldn't complain; they were very friendly. We got on very well with them.

COHEN: So you were just jealous because they had so much stuff?

DREVER: Not really jealous, no. I wouldn't say we were jealous, but we envied them. Envy, rather than jealousy.

Then I came up with this cavity idea and thought "Ah, this will put us well ahead." So we described it at the next conference or something. The first publication of this idea, which I think is still a critical one, is in a very unusual place. Because the next significant conference happened to be in East Germany. Remember that this was still the cold war days. It was behind the Iron Curtain. And this was, for me, quite an important conference. It was in Jena, in East Germany. It was quite an excitement going through the Curtain on the bus, traveling inside East Germany and seeing how miserable the place looked compared with West Germany. Anyway, I gave a talk describing this idea at this conference, and it was written up in the proceedings. That's where it was first published—not in a real publication at all. And I'm sorry now I didn't publish it, because at the time I thought of writing it quite a different kind of way. I'm bad at writing. I hate writing. It was, at least, in print, in this conference proceedings. And then I thought, "Well, we'll go ahead and try and do that." I'm probably repeating myself. It's a bit out of sequence there, but that's the way I was thinking about it.

Where are we now? I've got to stop and think for a minute.

COHEN: I'm trying to bring you to Caltech.

DREVER: Right. I'd applied for money to make this next-stage interferometer. I'd decided to give up on this one with the bars and to build something that was more [inaudible] with longish arms. We had a lot of help with all of this from Glasgow University. That was—I've already mentioned him—Philip Dee. And also John Gunn; I think I forgot to mention him last time. He played a very important role. He was a younger professor than Dee; the pair of them worked together. He was a theorist, and when Dee retired Gunn took over as head of the department. He's still alive [Dr. Gunn died in July 2002—ed.] and has been helpful and very friendly ever since. But he was a theorist, not an experimentalist, so he didn't affect the science I did, whereas Dee had a great effect on the way I did my science. Gunn had no effect on it, but he had a great effect on stimulating and encouraging and helping in all kinds of background ways—to get funds and so on.

We wanted to try to build an interferometer as big as we reasonably could. Glasgow had had an electron synchrotron, which was scrapped. We managed to persuade the university to remodel the space a bit so we could make something that had 10-meter arms. Right from the very beginning, I planned not to make the minimum thing. Like the previous time, with the bars, my feeling was that we should not just make one bar detector, but two of them. We had actually planned to make four of them and try and do something with them—same thought here. We would not make just one interferometer, we would make two interferometers, so that we'd have a crosscheck, and we could actually use these. But I realized that that would be expensive. At the time, at Glasgow, the expense was in the vacuum system—the vacuum pipes and the tanks. So I came up with the thought of putting more than one interferometer into the same vacuum pipes. I spent a lot of time thinking of different ways of doing that—we'll come back to that; this is later significant for LIGO [Laser Interferometer Gravitational-Wave Observatory]. But the first way I thought that was practical to do it at Glasgow was to make a square rather than an L-shaped thing, and then we could put two Ls in opposite corners of the square, and they would be sharing at least two of the vacuum tanks. So the thing as a whole would be a good deal cheaper than having two independent things, but we could still make it operate like two independent detectors. So from the very beginning, we asked for, ordered, and got enough vacuum pipes and so on to make a square system with 10-meter sides on the square, which would just fit into this space we managed to get from this synchrotron being removed. It turns out that there never were two interferometers built into this, and also only half of it was used, but it was a square from the very beginning.

So we got money, and we started to build that. I was thinking hard about how to make the interferometer; I did a great deal of thinking about how to do this. The optics was partly—I gained a lot from the discussions with Jan Hall—but it was fairly obvious.

COHEN: So you were back and forth to Colorado in this period?

DREVER: That's right. Only for short times, but they were significant. Basically, up to that time it looked to me like it was going to be very simple—that we'd just have a laser, lock it to the cavity, and there wouldn't be very much to it. The laser got much more complicated. But then, in thinking about how to do the details—the idea that we'd have to suspend the test masses and mirrors by wires, and we'd have to line them up carefully, and how would we do that?—it seemed to me that the obvious thing to do was to monitor their positions and angles by optical levers and cause them to move by having magnets and coils. At the time, as far as we knew, no one else was doing anything like this. So these were fresh thoughts.

COHEN: Would people come to visit you, by the way? Did Kip come? Did Rai Weiss come? Did any of these people come to Glasgow to see what you were doing?

DREVER: Rai Weiss never did—at least, nowhere near that time. Rai Weiss never traveled much. He didn't even go to most of the conferences. Kip did come. And other people did, notably Robert Forward. But it was more that I met people at conferences, not so much people coming to Glasgow.

Okay, so a lot of the key ideas of how to build this interferometer were worked out at that time. It depended a lot on the experience we had had with making those bar detectors. We decided to do everything—as much as we could—the same as we had done before, because they [the bar detectors] had worked wonderfully successfully; we couldn't have expected them to work as well as they did. So we just duplicated lots of things without thinking about it very much—like the seismic isolation, which is now a big deal in the LIGO. We just covered what we had done before, because it worked. Without trying to analyze it, we'd done what was very simple. There was lots of lead lying around the lab because of lead shielding from the synchrotron. We had also used tons of lead in shielding, several tons. So we had a huge supply of lead, nice lead bricks. And we used rubber. I remember we made isolation by cutting rubber matting off the floor and [using] layers of this rubber from the floor. And these lead bricks. It was all very crude and simple. But the thing was, it worked well. So we essentially, more or less, replicated the same thing as far as possible, to support the test masses in the system. But it had to have these controls to orient them. That turned out to be a practical problem, because they were kind of complicated and required several systems. So we had to learn a bit about doing all of that.

We eventually developed all that. From time to time, there would be meetings with the Germans. It was interesting to see that they were using different concepts—some were the same, some were different, but they were mostly kind of different, because we were working independently. They didn't use optical levers, they used shadow-sensing things. They didn't use lead and rubber stacks; in fact, they had little isolation at first. So it was interesting to see the differences. Both methods were basically working about the same.

While this 10-meter system was being built—I think it was in the middle or just the beginning of that—Kip came along to invite me to come to Caltech.

COHEN: To visit or to actually move there? Did he ask you to come visit for a while, or did he really give you the proposal—that you're going to start all this research here and we need you?

DREVER: I have forgotten the details of that. I may have had some small visits prior to that, I'm not quite sure. Anyway, the key thing was that he came, along with Carlton Caves, who at the time was one of his students, and, I think, an exceptionally brilliant student and very interested in the experimental side of things, although he was a theoretical student—in the theory behind the experiments and the quantum nondemolition. He played a key role in the development of the quantum nondemolition ideas. I had met him at conferences and so on. We got on very well; I still have a very high regard for him. So Kip brought him, and they spent a few days at Glasgow, and basically they invited me to go over to Caltech. That was a very difficult question for me.

That was a very difficult question because at the time, as far as I could tell, things were going wonderfully well at Glasgow. We were building this new thing that looked like it had new ideas in it. It looked competitive—perhaps better than anything anybody else was doing—and we had very good support from the university. Although money was small, still we could live with it, we could do it. And it all looked great, and it was all going very well. I had a very happy group. The Germans were also pursuing their own ideas, and they were not the same as ours, and I liked that. I like it when there is someone else attacking something in a different way, not as enemies but as friends. We were helping one another.

Kip wanted me to come to Caltech, and of course that was very difficult, because obviously Caltech was known everywhere as a top, world-class place and Glasgow wasn't—although Glasgow was still a better place than most people thought in Britain. But it was not a world-class place. So this was a kind of opportunity and quite exciting. On the other hand, things were going very well where I was. Was this going to be better? Kip kept insisting that I could get much better financial support so I could build much better equipment and so on. That was the main thing he could offer. So this was very tough for me to do, and I got advice from everybody I could on this one. I had visits back and forth and many discussions with Kip. The division chairman at the time [Caltech's Physics, Mathematics, and Astronomy Division, 1978-1983] was Robbie [Rochus E.] Vogt. Well, not quite: When [the talks] first started, it was Maarten Schmidt [PMA chairman 1975-1978]. The first discussions were with Maarten Schmidt, and later on there were discussions with Robbie Vogt, when he became the division chairman. We had a lot of discussions, arguing this out.

Of course, I didn't realize at that time that things were very different in the U.S.A. than they were in Britain and in ways that were non-obvious at the time, to me. People thought differently and acted differently, and that was one of the problems; I didn't realize that they were so different. But anyway—actually, they were more or less the same. An important thing in all these discussions, before we came to an agreement to do anything about it, was that I wanted to be my own boss, because I knew that worked for me. I wanted, you know, to try my own ideas, whatever they were, and no matter how crazed they looked to other people I wanted to try them, because that's what always works. Some ideas may have been crazy, but they've worked. So that was very important, and I brought that up in these discussions. And I think Kip or others said, "Well, it's more usual that you'd have a couple of faculty members working on a project." And I said that I'd rather be my own boss and work by myself. And I thought that was understood.

We'll come back to that later, because it's really an important point. So it was my understanding that we discussed this point—that I did not want to be under anybody, I wanted to do it my own way. Because that's what I had at Glasgow—I had total freedom, except limited by the available money. Essentially, I could do *anything* I liked. It didn't even have to be physics. I could do, literally, anything I liked, as long as it could be done with a relatively small amount of money, because I had a few people; I had some technicians, and that was different from the American system. The technicians were provided by the university; you didn't have to particularly ask for them. There were technicians there [at Glasgow] who could work with me, and they would do anything I wanted to do. If I wanted to do some work in another field, I could do it, and they would do it. You could do a lot without asking anybody officially for any money. And I thought that freedom was extremely important. I afterwards found out that that freedom wasn't available in this country, but at the time I didn't know that. I wanted to be completely free to be in charge of the project myself and run it any way I liked and be free to do what I wanted to do. OK. And I felt that was understood.

It was still very tricky to know what to do. By that time, Dee had retired, and Gunn was in charge of the department, so obviously I discussed all this with him, and he was extremely helpful. He suggested that the thing to do was to have a special arrangement with the University of Glasgow and with Caltech to try it out first. It shouldn't be binding either way. And have a five-year period in which I worked half-time—because I didn't want to go. Obviously, my project was, at the time, the most important one in Britain in this area. I didn't want to abandon it. That wouldn't be right—to go away and leave it. And there was this kind of feeling, which was afterwards reinforced by discussions with the SRC. They regarded me as the key person, and if I went away to America it would be a terrible loss. They had been sinking some money into this project. OK, so I thought I could go for this half-time period. Also, Gunn proposed that the university would be extremely helpful; they would hold a position for me to come back to, if I wanted to, and they would continue with my pension payments and all these kinds of things. So in every way, the University of Glasgow was extremely good.

COHEN: And actually Caltech did something that they were not used to doing, either, which was to give a half-time professorship. That was unusual.

DREVER: Yes, but at the time I wasn't sure what it was. I wasn't aware of what was common or not then. What I was aware of, at that point, was that Glasgow was being extremely helpful and very good. I had nothing but help and encouragement from the people at Glasgow. But I was advised that Caltech was a better place, and there would likely to be more chances of doing more exciting things.

COHEN: They thought about your career.

DREVER: They did. I have absolutely nothing but the highest feelings about them. They were very, very good. And that was probably universal at Glasgow. They all advised me to go, and I thought that I'd try this five-year period, being mostly encouraged by the thought that there was going to be more money available to do things and also that the general stimulus was much greater, obviously, at Caltech than at Glasgow. So we started that way; there was this five-year period. Now, a lot of things happened in that period.

COHEN: I have this period starting in '79. Is that right?

DREVER: I can't swear to that, but it probably is.

COHEN: You had already made the commitment.

DREVER: Correct.

COHEN: So [before that,] it was under discussion, say, from '77 to '79.

DREVER: Right. What was happening at Caltech? Basically, I joined Kip, who was pushing all this, of course. I had an office with these theorists and theoretical people who were working with him. The first thing was that I had to make a proposal, which was something I was not used to doing, because in Glasgow you really didn't need to make much of a proposal. You could do a lot of things without applying to anybody for money. There was just available money—in small amounts, but it was there. And I had in fact written a proposal for the SRC, but there the requirements were so much easier. It was altogether a better system; you just wrote two or three pages and that was all, and it was reviewed in a much more kind of friendly way. I was on the committees also then that were doing some of the reviews, and it was altogether a much more economical and streamlined system. We didn't spend a lot of time writing proposals. Although money was essentially limited, they did everything possible so that it was done with the least effort on the part of the scientists.

And I was horrified to realize what you had to do to get equivalent money here. You had to write these enormous documents, and so on, with all these details. The great thing about the system I was used to is that it didn't depend on that. The judgment was made on the basis of "Is the person a good person? If he is, we put our trust in him and we don't need the details." And that seemed to be exactly the right principle. As evidence of that, later on, when money got even tighter, the government cut money for everybody, and they said, "Well, if we cut the money, we have to make it used more efficiently. We'll let people use the money any way they like." We could change how we did things without any problem. That was a beautiful idea. With no red tape, you could do almost anything.

So, anyway, first of all, I was to write this proposal, and Kip played a major part in helping me, because I had no idea how to do it. I find writing very difficult anyway. He played a major part in writing a proposal. I was horrified at the details you had to go into, but eventually after a lot of struggle—it was a painful thing—we did write a proposal, based mostly on the Glasgow work, with lots of photographs and so on. We described how we could build the same thing, more or less, at Caltech. We did write this proposal and submitted it and eventually it was granted.

The timing of this switch to Caltech was kind of unfortunate, because the first Fabry-Perot interferometer was kind of half-built at Glasgow, but we hadn't gotten to the stage of testing it. It would have been better if this had happened after it had been run a bit, so that you could build the next-generation one. The only thing we could do was assume that the design I was working on was OK and essentially build the Caltech one the same. So it was essentially exactly the same as the Glasgow one, except here we could get more space. I remember hunting around all over the place, including looking at the Synchrotron [Hall], for space where we could do this. Eventually a key idea came from, I think, Robert Christy. He came up with the idea that we could build a kind of L-shaped building around the Central Engineering Services building, and that way we could get about 40 meters.

COHEN: Two arms.

DREVER: Two [40-meter] arms. That was kind of a breakthrough idea, and it was kind of out of the way. And so that's what was started. Basically, I designed a 40-meter system, but it was in every respect the same as the Glasgow one. Now, the Glasgow one had been built at minimum cost, because we had very little money. Things like the vacuum tanks were really too small. However, the easiest thing was to duplicate it [the Glasgow system], and that's what we did, even though we knew that, for example, when we visited the German group, they had much larger vacuum tanks and we were kind of envious. But they were going to cost a lot more. So we thought "We'll just do the same thing at Caltech [as at Glasgow]." I'm sorry now that we didn't make them bigger, but we thought that would be more economical, so that's what we did. We duplicated the design—it was the same size and everything.

Begin Tape 2, Side 2

DREVER: So, we were planning this Caltech interferometer.

COHEN: Who was working with you at the time?

DREVER: That was a problem. To get people was the hardest thing of all. We've always found it very difficult to get people. I started off just by myself, of course. Early on, we got a couple of grad students.

COHEN: Were they from Kip's group?

DREVER: No, because they had to be experimentalists. In fact, over the period—I mix up the different ones, perhaps. There were some good grad students and some that weren't so good. A very good, an early one, was Mark Hereld [PhD 1983]. He was extremely good. Some of the bad ones were the ones who were really tied into theory, and they were attracted by the glamour of it but they weren't good at making things work. So, good and bad ones. But Mark Hereld—and I've forgotten the other chap's name—they worked together. One of the things I soon learned—which, again, I found very surprising; the difference between things in America and things in Britain—was that those two just didn't seem to want to work together. At the time, I didn't understand it.

COHEN: Do you mean that the students wanted to work alone?

DREVER: Students didn't want to work with one another. I afterwards realized that this was a general thing; that people didn't want to work together—they all wanted to be independent. That was a major difference, which took me a long time to understand, and that was one of the problems later on, I think. You see, at Glasgow, it was an extremely friendly setup. It was a small group, but everyone in the group was working very hard. We had a common aim—to try and discover gravity waves and make this apparatus and so on. And it wasn't so much that people were trying to do better than their neighbors. When I got here, it was a totally different atmosphere. The students seemed to have been taught that they had to compete with their fellow students, and it took me a long time to understand this. There was a kind of semblance of trying to work together, but after a bit I realized that they really weren't, and they all wanted to be independent. I didn't know this for a long time. Maybe I got this wrong, but certainly the impression I got was that this was a fundamental difference: People did not want to work together. That was one of the problems, because I would work with people and discuss all the ideas I had with them and expect them to discuss all their ideas with me and together we'd try to come up with joint ideas which had input from everybody. That didn't seem to be the way people wanted to work. That was a major problem later on.

COHEN: That's probably Caltech, and the fact that it's going to be very hard to get a job, and you had to show what your work was.

DREVER: I don't think it's just Caltech.

COHEN: Maybe it's the United States.

DREVER: I think it is. And maybe it's an effective thing. But I didn't realize that difference. People were trying to get ahead of one another, rather than trying overall, as a group effort, to make something work the very best way they could possibly make it work. I think this is a deep difference, and it's not obvious. Japan, for example, is much more what I was used to.

COHEN: Group effort?

DREVER: The Japanese work together in the way I was working. We worked together very happily, and it was kind of a joint effort.

COHEN: But look, Ron. On another level, you came, and you didn't want to share being in charge with anybody—.

DREVER: No, I didn't.

COHEN: —so in some sense you could understand people wanting to be independent.

DREVER: Oh, yes. Perhaps that's true. Although, mind you, we were working very happily in some joint efforts with other people, like with John Jelley. I think I told you earlier that I had a very happy collaboration with John Jelley. Also a very happy collaboration with Jan Hall. People who were kind of equivalent.

COHEN: Well, you each had your own show.

DREVER: That's right.

COHEN: When did Stan [Stanley E.] Whitcomb come along? He was in on this from early on, wasn't he?

DREVER: Yes, that was fairly soon [1980]. I think Kip proposed that there had to be another assistant professor. At the time, I really didn't understand what an assistant professor was. There had to be someone else like that on the project, and somehow Stan was brought up to me. I didn't pick him. We tried to advertise, and so on, for people. An early important person, of course, was Bob [Robert E.] Spero, who is still working on the LIGO project.

COHEN: Is he still here?

DREVER: He's still here. And he was extremely good and was really a key person.

COHEN: Was he a graduate student or a postdoc?

DREVER: He was a postdoc, and he worked with Riley Newman at Irvine. I'd known about Riley Newman's work from conferences on the measurement of Big G and things like that, and I had a high opinion of him and his lab, and so on. Bob Spero and Mark Hereld were the most important early people. There was a period, then, which I think was a very productive period, when I was traveling back and forth [between Caltech and Glasgow] every three months. That was more or less the arrangement. I was designing the interferometers for both places; they were essentially the same. A lot of the design was done on an airplane flying over the Arctic. I would go on these long, eleven-hour flights with no interruptions. It was comfortable. Basically, I would take notebooks and cover them with drawings. I did most of the outline design of these interferometers on these flights.

COHEN: Disconnected from Earth.

DREVER: Yes. Well, it was a good, long, quiet period. It's not often you'll get a quiet period like that. It's the great thing about eleven-hour flights.

COHEN: Economy? Or it didn't bother you?

DREVER: It didn't matter. They were always the cheapest flights, but they were good. They were great. I loved these flights, OK? It gave me time to think, and so on. There were no distractions.

COHEN: So you did this every three months? You went back and forth?

DREVER: Yes. And a lot of the key drawings—things that were rather difficult; layouts and so on—were done on the plane. One of the key aspects was how to hang the test masses so you could control them. I came up with the idea of what I called a control block, which was suspended itself. The test mass was suspended from that, and you oriented the control block, and the test mass hung below it with two loops of wire. This meant you didn't have to actually apply forces to the test mass. Nobody had done anything like this before. I think it was a good idea. I spent a lot of time on the mechanical design, trying to make a design that had high resonance frequencies. It was a difficult design problem, but I did it all on these planes. The detailed machine shop drawings were done at Caltech by Mark Hereld. He actually got these things machined up more or less to the sketches I had made on the plane. Similar ones were being made at Glasgow—almost the same.

COHEN: So a lot of progress was being made in this period.

DREVER: Yes, a lot of progress in the detailed design and the construction of all the practical tricks to make these things actually work. By the way, that idea has now got a different name. People are calling it a double pendulum.

Basically, we were building up the vacuum systems, and so on, at both places and testing out the optics in both places—although a little bit later at Caltech. We eventually locked the laser to a cavity at Glasgow and at Boulder. Then we started to have the problem of trying to make two arms work. Most of these things were done first at Glasgow, because the Caltech one was started a little bit later; we got to most of the testing phases at Glasgow while the Caltech one was being made. I realize now that it was a little bit hard on the people left behind who were working while I was gone. Jim Hough [at Glasgow] was pushing on while I was away. At this end, Stan Whitcomb was essentially brought in and was doing the same job—putting together vacuum tanks and so on during the times I was away.

COHEN: Yes. There had to be someone in charge when you weren't here.

DREVER: That's right. But it was going on, I thought, fairly well. I didn't really pick Stan Whitcomb; I think he was a student of Robbie Vogt's. Robbie Vogt thought he was a good person and persuaded him to come. Anyway, he seemed to be doing OK. That was the way it was going for quite a bit.

Once we started, we had a great deal of difficulty—we're talking about the science of it, now. We had a great deal of difficulty, once we got the laser locked to one arm, in making a second arm lock. That proved much harder than I had expected. It took weeks and weeks of trying to see why we couldn't do this. The thing was, the test masses were shaking about too much. Eventually we managed to solve all that, but at the time it was almost looking as if the whole thing was impossible. Although we could lock the laser to one cavity, we couldn't lock another cavity at the same time. That was a technically difficult problem. Once we did manage to make it go, it looked great. Then we used the same technique to look at the second arm. We didn't actually recombine the light beams. What we found was that all these optical things turned out to be much harder than what we had ever expected. The simple design that at first we thought was going to do the job—well, once we got it going and we started to measure noise and so on, the noise was much worse than what we were expecting theoretically. We had to try and find out why it wasn't working as you would expect theoretically, and so on.

The same thing was built, more or less, here at Caltech, and the same kind of situation [arose]. And, well, that's what I was expecting. But this is something that Kip Thorne, I think, *hadn't* expected. Anyway, the noise was orders higher than it should have been and required a whole string of long experiments to try and find out what it was. The same kind of thing was being found by the German group, too. The first problem had been the scattered-light problem; we had fixed that one by the cavity idea, and the Germans found a different way of avoiding it, a clever way—of phase-modulated light in a certain way in their delay-line system. So they had essentially solved that, too. They had better performance than we had, and we couldn't quite see why. Then we gradually began to learn the dozens of things that we had done wrong. There was a long learning period, in which we learned about mechanical—

The first test masses were complicated structures. They looked to be simple but they actually had separate mirrors and so on, and they were quite complicated, and we, after a bit, found that there were mechanical resonances that gave off all kinds of noise. Then there were various optical problems. One of the problems that for a long time practically defeated us was that we couldn't understand what it was that— We used devices called Pockels cell modulators, named for the person who invented it. It's a special kind of crystal that you can put a voltage to, and it changes the phase of the light going through the crystal; it changes the velocity of the light in the crystal. We bought these from what appeared to be the best American companies. They had losses; they seemed to give us noise.

COHEN: Now, tell me, Ron, did the meetings continue through this period? All of you were getting together all the time?

DREVER: Not all the time. There were annual conferences, or sometimes more than once a year, at various places.

COHEN: But still everybody was talking?

DREVER: Oh, yes.

COHEN: The Germans, the people in Colorado—

DREVER: Yes, that's right.

COHEN: So you knew that nobody was solving these problems very simply?

DREVER: Oh, yes. Also the French. Alain Brillet, in Paris, was interested in this. He wasn't actually building a gravity-wave detector, but he was an expert in these things. I've forgotten now how I got to know him. He was also a friend of Jan Hall's and had worked at JILA. Anyway, he was thinking hard about gravity-wave detectors of this kind, too, and he was an expert on stabilized lasers. I mention him because he was a very helpful person, and he made some useful suggestions, like to try to use triangular cavities at first, which we did, because we didn't have any fancy optical devices called isolators, which you need if you use linear cavities.

COHEN: How about all the laser people here at Caltech? Did you ever talk with them very much?

DREVER: Hardly ever.

COHEN: Because there's a lot of laser work going on here.

DREVER: But totally different. Very, very different. I did talk with them, but none of it ever really led to anything. I mean, basically they were dealing more with beams and fibers, and ours was stuff with beams and vacuums. I talked with [Amnon] Yariv, and, yes, he was very helpful, but he really didn't influence things. Most of the influence came from Jan Hall and some from Alain Brillet, who was extremely helpful, too. I remember that when we began to realize some of the difficulties of locking, we decided we had to have the mirror separate from the test mass, and we had a piezo transducer between the two. He [Brillet] had worked on that much earlier, on stabilizing lasers, and he had developed a very fast transducer. One thing I remember—he was very friendly—he actually gave us, at Glasgow, one of his special piezo mirrors he built, which was one of the fastest in the world, and it was much better than one we could make. It was very cleverly done, and it was a gift! That kind of thing was so nice, which I've not experienced really here so much. He gave us this, which he'd invented and developed and so on, and we copied it and made more of them and so on. It was very good. Also, using triangular cavities and so on, which we later abandoned, but it was how we started, both at Caltech [and at Glasgow]. Essentially, we had to do the same things; it was difficult to see how you could do different things. The setups at Glasgow and Caltech were at the same stage, so it was hard to avoid that, and it was a bit of a pity, in a way, but you couldn't help it. It would have been better if we had been at different stages, but all these things were happening simultaneously at Glasgow and Caltech.

So, there was serious noise, much worse than expected. Then we gradually learned, from hard experience, what we were doing wrong. One key thing came from the Germans. That was that they had discovered that motions of the laser beam were a serious problem. They came up with a way of fixing that by having an auxiliary cavity that the beam went through. This is now called a mode-cleaning cavity—they had a different name for it at first. Anyway, that was what it did. It was a clever idea. We copied this idea, and it really made a huge difference. Then we realized that that should be a key part of all these things—this mode-cleaning cavity. We put them in both, and that helped a lot.

COHEN: Now, Ron, did all this go on during your five years back and forth?

DREVER: I'm not sure, exactly. It was during that time and perhaps extending on after that.

COHEN: So you had plenty of problems to work on, I gather.

DREVER: Yes, but we were gradually beating them. The performance was getting better and better every year, the key thing being this mode-cleaning idea that came from the Germans. And a little bit later there was another key idea, which I came up with, about the stabilizing of the lasers, but we'll come back to that later on.

COHEN: Now, let's get to the point where you made the decision to stay here.

DREVER: Things seemed to me to be going very well, although it was obviously difficult being half-time in both places. Glasgow was being very helpful, and Caltech was being very helpful, too. Then Caltech said that I had to decide whether I was going to be there full time or not. That was a major problem, because it seemed to me that this was a satisfactory arrangement, and giving up the Glasgow one was hard.

COHEN: Well, that was your whole life. I mean, you had been there always.

DREVER: Well, it wasn't so much that; it was basically that we'd got a very good group there. It was at least as good as the Caltech group and maybe slightly better. Maybe not; I mean, it was hard to tell; the two were different. But there was more technical support [at Glasgow]. We had technicians who were not very expert but who were very helpful, very keen. And there was a very good atmosphere. Well, there was a good atmosphere here, too, but a little different. Anyway, that was a very difficult question—whether to give up on this Caltech thing or what? Where was it going to end up being best? We could see at this point that there was still a lot of development, but the thing would eventually have to grow bigger. It was hard see how there was going to be money in Britain to make something on a big enough scale. It looked much more likely that it was going to be at Caltech, and Kip kept emphasizing that. I'm not sure at what point MIT came into the picture. Maybe it was in the middle of this.

COHEN: Well, I think it was when you started to have discussions with NSF [National Science Foundation]. I have a date here of 1980, with NSF saying that you'd have to work with the MIT group too.

DREVER: Yes. Right. That influenced things, too. I'm not sure exactly of the timing of all this. Rai Weiss had had an early idea, as I said, on this. He was doing experiments—in my view, very slowly—on a very small scale. While I was working at Caltech, the stuff we were doing at Caltech was way ahead, in my view, of what Rai Weiss was doing. It was also going much faster and was much better in practically every way. Rai had a very small effort. Basically, he made a small interferometer and it really didn't work very well. It wasn't the fact that it was *small* but that it wasn't designed very well, in my opinion. There were lots of things that seemed to me to be just kind of crazy about the design: the way the mirrors were supported and so on; there was no seismic isolation. It was just a bad design, and it didn't work well. He was applying for grants, too. On the whole, I was getting much bigger grants; what I was doing looked much more promising. Now, maybe Rai had problems getting money—that was probably part of it. Anyway, it just didn't seem to be designed right. So we met often. At the time, it was my impression that Kip was saying that there had to be a collaboration for any major thing.

COHEN: I think NSF was saying that, too. I mean, that's how I read the record.

DREVER: Yes, that's right. I've been told since, by Rai, that that wasn't the case, but at the time it was my understanding that NSF was saying there had to be collaboration. I saw it coming from Rai, in particular. Kip and I had many discussions with Rai. I felt that he was trying to knuckle in to the project that we were trying to do, and he was also always very competitive, I felt, about the delay-line idea that he had come up with. He wasn't at all interested in the Fabry-Perot system. There were all kinds of arguments, although we got on in a friendly enough way. But in almost every way you could see there were two ways of doing things, he would choose a different way to do it. That may not have been specially planned; in my case, it definitely wasn't. It was just that we saw a way to do it that required a very pure laser frequency, and Rai's method—well, he could have done it like that, the Germans did. But Rai decided to go the reverse and have the laser purposely made very near a white light—at greater frequencies—to avoid the scattering problem. It was just totally the reverse. So in almost every way you could think of— He wanted to have much higher power lasers. In the middle of this period, I came up with the idea of re-siting of the light. Now, I'm not sure if that was after I decided to stay at Caltech or not—maybe that was a little bit later. That turned out to be a key idea. Maybe that was later, around '83.

COHEN: But you made the decision to stay?

DREVER: Yes. It was a very difficult one. I was getting advice from everybody. I made the decision, yes, to stay, but Glasgow was still very helpful: They said that if I wanted to come back to Glasgow I could, essentially any time. And if I wanted a position at Glasgow I could get it, and so on. So they were very helpful, but I decided I would have to hand over the thing to Jim Hough. I expect Jim Hough, who was second in command at the time, may have been a bit resentful, because in a sense I was kind of leaving them. The feeling was that, having done all this stuff in Britain, I was a traitor in some sense, because I was going away—a defector, or something. The kind of feeling of being abandoned, and so on.

I think the British funding authorities were unhappy—I heard afterwards that that upset them a bit. I was still trying to help everything in Britain to keep going as well as possible. I continued to—even though I was full time at Caltech—to try and help in every way I could. I continued to go back and forth very frequently. I kept all the ideas in common. Everybody knew what both [places] were doing. We shared things, and so on.

But in any event, I decided to come to Caltech. I think it was Ed [Edward C.] Stone who was the division chairman [1983-1988] at that time. He made a kind of ultimatum that I had to make a decision by a certain time. It was very difficult to know if [deciding to stay full time] made sense, but I eventually decided to go ahead with it and do it. OK, I'm not sure now about the timing of all these things, whether that was the time when the things with MIT became critical—when there was pressure to form this collaborative effort with MIT.

COHEN: I have a date of '84 here. It says, "shoot-out with MIT." [Laughter]

DREVER: So that's a little bit afterwards.

COHEN: Yes, you were here permanently at that time.

DREVER: Yes, OK. The project was going quite well, although still way less than the theoretical expectations. We were fighting all kinds of experimental problems to try and make it work better. We were gradually building up the system and trying to beat all the problems with it.

COHEN: Well, I think maybe we should stop now. I have 1987 as the year that Robbie Vogt became director of LIGO.

DREVER: Well, there was a lot that happened in between. Somewhere in this period—I'm hazy about the timing of things—there was this gradual buildup of the issues with MIT. There was this kind of pressure to have a collaboration with MIT, which I really wasn't wanting. As I said at the very beginning, I wanted to do my own thing. I didn't want to have to collaborate with anybody else.

COHEN: But that evidently got—at least from my notes, NSF was insisting that you work together. Now, you question that.

DREVER: At the time, I was told that that was what it was. Rai has since told me that that wasn't the case. Now, I don't know if I should repeat here what Rai told me about it.

COHEN: Why not?

DREVER: I mean, I don't know if it's true, or if I've got it right. Rai told me recently that the pressure for collaboration *wasn't* coming from NSF. Rai felt that there was going to be some pressure to have a bigger effort involving several universities, and Rai didn't want that. Rai told me that he thought the best thing was to have a collaboration with Caltech so that it would be only the two places and not several. And so *he* pressed for collaboration with Caltech, because he thought that it was better than a collaboration with several other places.

COHEN: Well, that makes sense.

DREVER: At the time, I knew nothing of that. I thought it was being pressed by NSF. I was also a little annoyed at Rai, because I felt his experiments weren't as good as ours. The only idea he had produced was this one, which wasn't working very well. He was against, basically, all of my ideas. He was kind of knuckling in on the thing and wanting to try and do it differently. What annoyed me also was that in the meetings we had together, he would come up with all kinds of fancy plans, and so on, with dates for doing things, and I felt that he was trying to run the show. And yet the techniques that worked were the ones that we'd developed. And I didn't like this. I felt he was trying to come in and take over, because he was much more of an organizer than I was. And yet the ideas were my ideas, and his weren't working well. He wanted to throw them out and do it all his way.

COHEN: It doesn't sound like a happy collaboration.

DREVER: No. And I wasn't really happy with it. The other thing—I don't know if this happened then or maybe a little bit later—

COHEN: Let's finish this bit about Rai, and then we'll go on to something else next time.

DREVER: OK. The important thing was that somewhere—and I don't know exactly when—in this period when Rai's experiments were really going pretty badly, I was shocked to hear that Rai was talking with a company about how to make a huge gravity-wave detector this way, with arms 10 kilometers long. He got a company to do the engineering on this. This seemed to me very strange, because we hadn't solved the problems on a small scale. But he got a company involved and produced eventually [October 1983]—I got this—what's been called his "Blue Book." He got a local company, in Massachusetts to do the engineering design for building a huge-scale thing like LIGO. The original plan was even bigger than LIGO has ever got. The first design was going to be 10 kilometers square, with diagonals filled in also—the square being, I assumed, the same as the Glasgow square I had designed years ago and had kind of half built at Glasgow. He was going to make one 10 kilometers square, with diagonals as well, all on a huge scale. And he'd had this company work out the details, and it was going to cost a huge amount of money, and it was a very, very big thing. And then he was proposing that this should be a joint effort between Caltech and MIT. That really worried me, because it looked like—

First of all, I thought it was premature. We hadn't solved anything like the problems on the small scale, and here was this absolutely huge thing. He was proposing this, with *his* interferometer being much, much poorer than the ones we had made. Yet he was planning to make this great big thing, and he was obviously acting as if he was going to be in charge of it. I was very worried about all this. It seemed very strange to be doing all this. A lot of this was being done in secret. We didn't know it was happening. We thought he was working on the small one, but in fact he was putting a major effort, clearly, into designing this great big project.

COHEN: This was after you were supposed to be working together jointly, with your NSF grant together?

DREVER: I'm not quite sure exactly when it was. It may have been, or it may have been before. I'm hazy about the sequence of these things. I was getting bigger grants than Rai. Later on, when we were forced to collaborate, I was giving a large chunk of my money to Rai, which didn't strike me as right.

COHEN: Because you didn't like what he was doing.

DREVER: Yes. Because I was getting better grants than he was. But I think that was probably a good deal later. Anyway, Rai's ambitious plans for a huge system worried me. Now, quite soon, it was realized that this 10-kilometer square was just going to be—

Well, first of all, I also knew that you had to build a bigger one, but I wanted to go up in stages. I wanted to get this 40-meter one working as well as we could and then go up a bit—perhaps 300 meters, or maybe one kilometer at the most, in the desert. I thought we could find a place in the desert where we could do this at a relatively small expense, an intermediate-scale one. Now, there was a problem. Kip kept saying that you could never get money in the States to build an intermediate-scale system, because it wouldn't be big enough to detect the real gravity waves expected. So you wouldn't get money for it. And yet my feeling was that you *had* to go that way, because you had to tackle the experimental problems step by step. We had found so many problems, and there were going to be more every time you scaled up. Here Rai was going to jump to this enormous thing. I felt that it was almost certainly not going to work.

So there was a problem. It was my view that we should go up gradually. However, Kip kept saying that we couldn't get money to build an intermediate-scale thing, which was basically what I wanted to do. Perhaps one kilometer, which I felt could still be made fairly cheaply. In Scotland, too, there was a proposal we were kind of involved in—I was partly out of it then. We did find a site in Scotland where you could build a one-kilometer one, and the cost, by present standards, wasn't high. We thought that was a sensible scale-up of size.

COHEN: But you were told that that would not be possible?

DREVER: I was told that in the States, it couldn't be done. So that was a bit of a problem.

[Tape Ends]

RONALD W. P. DREVER

SESSION 3

February 25, 1997

Begin Tape 3, Side 1

DREVER: I've been looking back at some of my old files, and they have reminded me of things. I haven't seen these computer files in years. I still haven't got all the files that I have, but these are some of them. I'd like to go back.

To summarize, I had been asked to come to Caltech and agreed—mostly because I was advised by people at Glasgow, particularly Professor Gunn, that I would have a half-time position for a period of five years. During that half-time period, a lot of productive work was done. That was '79 to '84. During that time, I was half-time in both places. During that time, simultaneously was being built a first Fabry-Perot interferometer at Glasgow and also at Caltech; and as I mentioned before, they were common designs in most ways. At Caltech, a lab was built for me. I got some students who played key parts in a lot of it. One or two people played a key part in it—particularly Bob Spero, who was a very important early person. A key student was Mark Hereld, who did a lot of the early work on the detailed design. I made the rough designs, mostly on airplane flights; he made the detailed design. That, on the whole, was going quite well. We got this 40-meter interferometer working. It was a long distance from its theoretical performance, but we soon learned about all the problems. There had to be a simplified design. There were thermal noise problems and mechanical resonances and all that. Gradually it was being rebuilt, again and again and again, improving by large factors continuously—almost a factor of 10 a year, for a lot of years of its performance.

Then there was the smaller effort at MIT run by Rai Weiss, who built a small interferometer which never really worked quite as well. Then we were being pressed to have a collaboration with MIT. Kip Thorne said this was going to be vital to get bigger money, and we all agreed that we had to get larger money in the end. How much was an issue. It had always been my view that it would be best to step up rather gradually, going to something about ten times bigger, perhaps 400 meters or something like that. Rai Weiss wanted to jump immediately to a huge interferometer. In fact, initially he was talking about a square 10 kilometers, like the Glasgow 10-meter square scaled up. He soon found that was an unreasonable cost, and it was scaled back to 5 kilometers for several years. He wanted to jump to that. I wanted to go to something intermediate, perhaps a single few-hundred-meter system somewhere convenient out in the desert. And that was a problem.

Now, Kip's feeling was that it was impossible to make an intermediate system. Kip pressed me that I didn't understand how funding worked in America. To build an intermediate-size system was still going to cost a fair amount—what we thought at the time was a lot of money, perhaps $30 million or $20 million. Kip kept assuring us that that amount of money couldn't be got unless [the system] was pretty well guaranteed to detect gravity waves. We all felt that this intermediate system couldn't detect gravity waves; it was to develop the technology. So Kip said that couldn't be done. It had to be a jump to a much larger system, or we couldn't do anything. That was a controversial issue, but I had to believe what I was being advised by Kip and Rai both—that because of the way finances went, it was much easier to get larger money for a larger system than an intermediate amount of money for an intermediate system. This was quite the reverse from the situation in Europe or in Japan, where an intermediate-scale thing was probably much easier to do and much more practical. That was a difficulty, but all I could do was believe Kip's and Rai's views on this—that it was not practical to ask for intermediate money and that we should take a gamble and go for something much larger. I thought, at the time, that this might be partly because Rai's own experimental work really hadn't been terribly successful. He had this kind of rival idea—which he had first, of course—of the delay-line system, but when he made one it really didn't work very well. But the German group at Munich essentially used his ideas and they made it work quite well. Over all this period of time, my feeling was that the key work was being done in Germany, at Munich—it later moved to Garching, close to Munich—and at Caltech.

COHEN: That was an interferometer in Germany also?

DREVER: An interferometer, but in almost all the technical details that it *could* be different, it was different. It was really entirely based on Rai's original concept. I had, and still have, a very high opinion of the people doing it. They were actually computer engineers, but they were very smart. They had no lack of money. They had good backup engineering support to make things—machine-shop support and so on. It was done very elegantly. They solved a lot of the problems. They had many advantages. They were all working full time on this—they had no teaching duties or anything else. They were good people and they did a very good job. There was always a bit of a rivalry between them and my effort at Glasgow, which was still going on—and also here.

COHEN: I think you mentioned that, but you thought it was a constructive rivalry.

DREVER: Oh, yes. I enjoyed talking with them. I think they enjoyed talking with me. We got on well together, and I thought that was a good situation. They came up with some different solutions. Some of the key ideas that everybody has used came from them. The device called a mode cleaner was their idea; that turned out to be a key thing later on, for everybody. But more of [the key ideas] came from my Fabry-Perot system. It eventually won this competition, in a sense; that's kind of what is being used in most places now. But anyway, it was a good thing.

There was this slight controversy with Rai Weiss at MIT—he was pressing, and so was Kip, that we had to go with a very large system. I was nervous about it, because it was my experience— You see, I had had more experience than the other people here in how difficult it was to actually make interferometers work according to the theoretical expected performance. Kip, as a theorist, assured me that it would work to the theoretical limits, in which case you'd say, "Go ahead and make one. You can just theoretically predict it's going to be good enough." But the experience I had had over those several years, already at this stage, in making the first interferometers work was that they were much poorer than you would have expected theoretically, and there were literally dozens of practical problems that you had to solve. You could never be sure that there wouldn't be some other problem around the corner that would stop it from working. That made me much more careful about promising any performance. It made me very nervous, particularly about going up a factor of 100 in scale. However, I was pressed by Kip and Rai that that was what we really had to do: We had to push for a big thing. It had to be a joint effort, to get enough money for a big thing.

Basically, we agreed to start on that. There was a kind of collaboration formed, led by a committee consisting of Rai Weiss, myself, and Kip Thorne. And then Ed Stone, the division chairman at that time, played quite an important part—a lot of discussions with him, a helpful part in how we should do this kind of thing. It was pressed that we had to get a project manager, who would look after the engineering side of this joint project. In fact, he [Stone] recommended, and we eventually got, a man from JPL [Jet Propulsion Laboratory] , Frank Schutz. The idea was—and it was probably a good idea—to as far as possible get people from JPL, because they still essentially had a post at JPL they could go back to, when the project was finished. When it was finished, they weren't out of a job; they were still attached to JPL. I didn't know this man, but he seemed OK. He was brought in as the project manager for the larger-scale project. He started to do a lot of the organizing, and so on, for preparations for a big project. He brought in a team of engineers from JPL. I think he felt—and maybe the rest of us did, too—that a very important thing was to find the best sites for the project. Rai Weiss had had a company in Massachusetts do all this, and generated a book that we all called the Blue Book, in which this company had made estimated costs and so on for a large-scale system. It was like a proposal, almost, for a full-scale system, and it estimated the costs.

The costs were much bigger than any of us had really envisaged they were going to be. At the time, we thought they were out of the question. I've forgotten the numbers now, but it was probably $50 million or $60 million or something. At the time, we thought that was an incredibly huge cost. Of course, now we know that the real LIGO is costing much more than that, but at the time we all thought this was— And so did NSF. NSF thought that this was an impossibly big cost, too. But the cost was going to depend a lot on the site—whether it was flat enough, whether you should bury the pipe or place it on the surface. That was going to make a big difference in the cost. How big it was going to have to be—I mean, the diameter of the pipe. There, there was controversy again, because Rai Weiss's technique, the delay line, required intrinsically large mirrors and a large pipe. My system, the Fabry-Perot system, could be built with much smaller mirrors and a much smaller pipe. There was a lot of argument back and forth over which was the better system, and eventually the conclusion was that you couldn't tell which one would actually work better in the end. Basically, the Fabry-Perot systems that I made at Glasgow and Caltech were really working pretty well. The delay-line system made by the Germans was also working pretty well; they were more or less equivalent to each other. So it wasn't clear which was the better. The small ones at MIT were not really significant in this; they were working very poorly. The German one grew away from Rai's idea in one important way: Rai had always felt that a major feature of his design was that you didn't have to have a very stable laser. Whereas the Fabry-Perot system that I proposed required an extremely stabilized laser—but I had found a way of doing that. The Germans had eventually decided to make their system highly stabilized, too, to resolve scattering problems. But Rai resisted that. The thing he was doing at MIT had a totally unstabilized laser. That may be one of the reasons it never really worked very well. That's still a kind of controversial feature, perhaps.

So it was decided that the best thing was to make the vacuum pipe big enough to accommodate either system, which meant it had to be large. It was concluded, after lots of discussions and arguments, that it should be about 4 feet in diameter, which is the size of the present LIGO pipe. Then I realized that, in that case, if it was going to be that size, we could either accommodate one interferometer of the delay-line design, or, if you were going to use the Fabry-Perot design, which was much smaller, you could actually accommodate several in the same vacuum system. Somewhere around the middle of this, I actually proposed this idea. The argument I was making here was that this was going to be a very expensive installation. The high-cost part of it is the huge vacuum system and the pipes, not the delicate optics, and so on, which are relatively cheap in comparison. I always felt the things should be separated. I felt—and I tried to impress this on Ed Stone, but it was eventually rejected—that the best way to do this was to have the big vacuum system and so on built as a common facility, in a highly organized way, but the development of the interferometers could be small-scale group-science efforts, done by small-scale groups in a rather old-fashioned way, and people could come and put their relatively inexpensive interferometers into this expensive facility. I felt it would be sensible, and I still feel it might have been sensible to have divided it that way.

This was opposed. I was told this couldn't be done. I thought that was a good solution [to] this ongoing controversy about which was the best system to use. People would come up with other ideas. I thought it was a good idea to try and make it so that it could accommodate several different interferometers at the same time, even of different types. At the time, that was a revolutionary proposal. I remember raising it first at one of the many site visits. We had a lot in this period, which I suppose is around '88. No, earlier than that. Anyway, we spent a great deal of the time traveling around the country, looking at different sites, with this project manager organizing that. Actually, Rai had already had this company he had employed also look for sites. They made a quick survey of the country. It was all done again with this team from JPL. This team was a team experienced in building the Deep Space Network for tracking satellites, so they were experienced in looking at sites. This team went around the country looking at places and making seismic measurements and so on. We looked at many, many sites, all over the place. One of these visits was to look at sites in Utah, near the Great Salt Lake. One of the aspects of the site issue was that we were trying to find places that were federal land, so that we would not have to pay for them, so there was an emphasis on military sites. The project manager was very keen to use Edwards Air Force Base, because that already had a JPL facility attached to it, so it had links to JPL and it was out in the desert, and so on. He was very keen on that one. In Utah, we went to look at the place where they develop poison gas and chemical warfare and so on; they were also friendly toward scientific things. There was already an astronomical—I think it was called "Fly's Eye"—fly's-eye [cosmic-ray detector] experiment going on in this closed site [within the U.S. Army Dugway Proving Ground—ed.]. So that was a promising place, too.

While there, I, first of all, outlined to Rai and the other people there some of the ideas I had come up with for how we could accommodate several interferometers in the one system. In a smaller way, that had already been planned years ago, in the Glasgow system. But this was different. What I was proposing now had an important new idea in it, which we probably should mention, because for a long time it was, and it probably *is*, going to go into the real LIGO. Right now it's been cut off for [inaudible] cost. The basic idea was to fit in a number of different interferometers into the one vacuum system; it's a bit like an accelerator—you put many experiments in one accelerator. But the difference is that you can deflect the beam in an accelerator with a magnet. You can't deflect the light beam at all. You can't put mirrors in, because it would cause losses. You have to keep the light beam going in a dead-straight line, and that makes it harder to see how to fit in many experiments. I came up with a solution to this. There's one critical area, and that's where the two arms intersect one another; you soon find that that area is a bottleneck if you're trying to do several things at once. I came up with a way of avoiding that by picking off little bits of the beam before you reached there. I came up with a kind of design in which you could accommodate, say, six different experiments at once. Each of them would have test masses that could be lowered through vacuum locks into the system and could be removed again without actually interrupting the operation of the other ones. They would deflect their light into a common set of beam-splitter chambers—it's hard to explain in words. But anyway, I had a diagonal set of chambers, one for each of the many experiments. It looked to be a practical way, at a modest increase in cost, to let you have as many as six different experiments running at once in the same system. I outlined how all this could be done. Rai was horrified at the idea, because it went against the grain. He always felt things had to be kind of crude and rough, and this was making it even more elaborate and sophisticated. He protested this idea strongly.

So we had discussions. It didn't involve much change, it just involved putting in more vacuum tanks at the center. Relatively small changes could make it operate in this way. So that was on the record, and we'll see, later, that this played an important part in getting the money for the real LIGO.

By the end of that period, with regard to most of the technical aspects of the design, I had come up with more of them than anybody else. Some had come from the German group; a few had come from people in the groups here in the States. And then the practical layout—well, Rai's company had done a lot of the practical details. But this idea of having what we called multiple-use at the time, of having several projects—I had come up with that. I had lots of sketches of how this could be done. When you look now at those old sketches that were done in the period prior to '87, and you look at the present LIGO, they're almost the same. All of those things have been carried through into the present project.

COHEN: The multi-use idea is part of it?

DREVER: Yes. That had to be modified later, because of lack of cost, but it's built into it. It's in all the main drawings. But to save money, it's not starting with the other chambers in place. But the buildings and all that are drawn to accommodate it; it's all been designed for that.

Then, at that point, we had a fairly detailed design. We had a project manager. We had gone and looked at many sites around the country. We had a lot of noise measurements on all these sites. Rai favored a site in Maine, which he had found very early on. It was a beautiful place, and he was very keen to go there. The project manager here favored Edwards Air Force Base as a site. I was slightly worried about that, because we were always being told it's a military installation for testing all kinds of military aircraft and we mustn't take cameras near it or anything like that, because we'll see secret airplanes flying around, and so on. This worried me, because I like to have a camera. However, they had offered an actual location, though there were some conditions to it. But there was a particular place where we had planned out a system just off one of the lakes. It happened to be where the Space Shuttle, when it was landing, would be flying right over our installation. One of the things we had to watch was keeping all the buildings low, so that they wouldn't be in the way of the shuttle coming in and landing.

COHEN: Sounds like problems.

DREVER: Yes. A lot of work went into that particular site. It went as far as getting surveys made, worries about a rare kind of tortoise. That was a terrible problem—you couldn't disturb these tortoises; they were an endangered species or something. Anyway, a lot of effort went into that location. I favored some other places in the desert, particularly one we looked at called Hawes, where there was another military installation being dismantled, so it was going to be an available site. It was quieter. I was a bit worried about the noise from aircraft at Edwards and this was quieter. It was a big radio station for relaying signals to aircraft all over the world, and they demolished the tower, so that looked like a possible place. I was keen on that one. I remember going out in the early hours of the morning there, with seismometers, to measure the noise there when it was very quiet, and it was a very quiet place. Anyway, we looked at all these sites. A lot of effort—probably too much effort—went into that.

COHEN: Now, did you know at the beginning that you were going to need two sites?

DREVER: That was a hard question. That was one of the other difficult issues. You see, we knew that you had to have two detectors somehow. The issue was, do you make just one here and depend on one in Europe or in some other country? Obviously, there could be one in Europe. The group in Glasgow that I started was going quite strongly. It was hard to get money there, but they had also been proposing—and I had helped with the proposal—a one-kilometer system. They found a possible site in Scotland for it, where they could get the land. It was forestry land. The money there wasn't so much—it was more like about $10 million. But still, for British funding, that was a lot. The German effort was going strong, too. But they hadn't got to the stage of trying to propose a large system, I don't think.

So that was a bit of a very difficult question, but I think both Rai and I felt that probably the best thing was to try. Well, there was a question: My feeling was to make, first of all, an intermediate-size one to start with. Kip's feeling was that you had to do the whole thing. If you were going to be self-contained, you really had to make two. There was also, regarding the argument for two, that you wouldn't be dependent on the thing getting funded in some other country, and that was a strong argument. Another argument for two was that Rai could have the one he would operate, more or less, in Maine, and Caltech would have one in the Mojave Desert that we would operate. I thought that was probably a very good solution; I think we both felt that—Rai and I. They didn't need to be the same. But later on, people would argue that they had to be identical. I still think that's wrong; I still think that was a mistake. But it was kind of a critical argument.

Anyway, I felt that would have been a good way to have gone, but my views didn't carry the day; they weren't strong enough or something—I don't know. I had wanted to have two systems. Each could essentially make their own system, and then the two could work together for the experiments and also exchange technology, and so on. But I was opposed, I'm not quite sure by whom. Anyway, it was eventually concluded that that would be impossible. All the time we were pressing to get more money, so I gave in to this argument that we had to push for a large system. This project manager was going ahead to organize the design of a large system. The funding was kind of odd at first. Because my project was bigger and more successful than the MIT one, I was getting more money from NSF.

COHEN: At this point, you were each getting your own money?

DREVER: We were each getting our own money. Then it was decided that we would also jointly ask for some joint money. We had three proposals going at once: One for my effort here, with me as PI [principal investigator]. We had one for Rai as PI at MIT. Then there would be this so-called joint one that we would both contribute to, which would pay for the project manager and for all the work of these engineering teams and so on. It ended up that my grant was significantly bigger than Rai's, so I was pressed to hand over some of the money to build up Rai's project, which I did. We were trying to work together. I handed over a significant amount of money to Rai to help his project. So we had these three proposals running for a bit, with the project manager organizing the design and a lot of the detailed stuff on the cost estimates for a large project, and there were various arguments back and forth, and meetings. I've forgotten them all. There were some outside critics of the project, before it got scaled up. There was an important meeting. I'd have to check my notes; whether it was in Washington, I'm not sure. Anyway, there was a critical meeting in the East—

COHEN: This is the late '80s now?

DREVER: I'd have to check the dates and where it was.

COHEN: OK. But is this about the same time as the site visits went on?

DREVER: It was after all the site visits had been done. Let me explain about this meeting. Why this meeting was critical was that this was one where the project was being criticized by some other scientists. So there was going to be a meeting with a kind of committee looking at it. I think it was the Academy of Sciences who wanted to look at it and all presentations. Basically, Rai and I and Kip all gave presentations on it. The project manager gave a presentation on the details. The general impression that we all had was that his side of the presentation didn't come off very well, and this committee weren't impressed by that; they were much more impressed by the scientific presentations from the rest of us—at least, that's the feeling I had.

COHEN: Do you mean that they liked the science and not the technology?

DREVER: No. The feeling we had was that although the project manager was doing his best, the presentation he gave— It wasn't so much the technology, it was the civil engineering and so on. It was that kind of thing he was talking about, and somehow it didn't come off so well. So after that— I'm not sure of all the politics; I wasn't even aware of all the politics that were going on. But eventually what I heard was that—and I may have this wrong—the NSF wanted the way the thing was being organized to be changed. It wasn't to be run by the steering committee, which was the three of us—Kip, Rai, and me—with the project manager. There had to be a single PI, who would be responsible for the whole thing. There should be somebody new: a project director, as they called it—someone experienced in building large projects. I think it was also partly that people weren't too impressed by the project manager.

COHEN: So they wanted one boss?

DREVER: They wanted one boss, that's right. So an effort was made to find somebody. At Caltech, the person who was eventually selected was Robbie Vogt [1987]. There were a number of reasons for that.

COHEN: Well, he was suddenly available.

DREVER: He was suddenly available; that was a very important one. He had been the provost, but there had been some internal battle between him and the president of Caltech at the time [Marvin L. (Murph) Goldberger]. The story was that he had been fired. In fact, that's what he said—that he had been sacked from his position.

COHEN: That's probably right.

DREVER: That's probably right, but I wasn't aware of exactly what had happened. So he was available. He was also experienced with large projects. I knew very little about him, but Kip had told me that he had also managed some other difficult projects, particularly the Owens Valley Radio Observatory. He had somehow pulled that together and made it successful—that's what I was told. He had experience in these things, and Kip was trying very hard to persuade him to become the director of LIGO; I gathered that wasn't so easy to do. Anyway, he was eventually persuaded to do that. He put all kinds of conditions on it. I had already seen that he would sometimes lose his temper. When he was division chairman [1978-1983], I experienced that once or twice, but not as bad as I found later. It seemed to me to be OK, and I was being assured by Kip and other people that this would basically make the project go much better. He [Vogt] would look after the financial side of it, and so on. So eventually it was agreed that that would happen. NSF was pressing for that. He immediately changed the thing a lot when he took over. He said we weren't working in the right kind of way and it should be much more organized. Particularly, there were weekly meetings at which everybody, including people at MIT, would report on the last week. There were also monthly meetings.

COHEN: Progress reports.

DREVER: Kind of progress reports. I had had something like that, but never as formal. We never had such formal things. He also insisted that he was going to be the only PI. Although I had running grants at the time, I had to hand them over. Basically, I took it to be understood that I had to hand these over to him, even though they weren't finished. He would be the sole PI for the whole project, both for MIT and Caltech.

COHEN: Was that one of his conditions?

DREVER: I think that was one of his conditions. Well, I didn't see anything particularly wrong with this, although later I saw that it was a mistake. But I did in fact hand over my unspent money, for my project as well as the joint one, to him. He also brought in what he called the chief engineer.

COHEN: Somebody new to the project.

DREVER: Yes. This was Bill [William E.] Althouse, who'd worked with Robbie Vogt for years on his previous projects, and so they got on very well together. So he was brought in as chief engineer. He played a key part in the early stages of Robbie's time on the project, because he always did exactly what Robbie wanted. He reinforced all Robbie's views, basically, to the rest of us, who found some of these views very different from what any of the scientists had been used to. Robbie had very strong views about everything, and it was just very strange to all of us. But anyway, that was the way it was.

COHEN: But the work proceeded at this point?

DREVER: Yes. He started off without very major changes to the work, although he gradually changed things quite a bit. One of the things that happened was that the project manager [Frank Schutz] was still, of course, in the project. Robbie obviously didn't like his work too much and gave him less and less to do; eventually, he [Schutz] more or less resigned. I don't know whether he was pushed out, but the official thing was that he left. He said that he felt that he didn't have any work to do—that it was all being done by Robbie and Althouse and the other people. And also Robbie brought in an assistant—Ernie Franzgrote, I think his name is—who was actually a very friendly person. I got on extremely well with him. I think most people got on very well with him. He was very different from Robbie. We all liked him, basically, much more. Robbie was difficult to get along with; this chap was very nice to get along with. So things went on like that for a bit.

COHEN: The project proceeded.

DREVER: The project proceeded. Robbie immediately started to make another proposal. This was a major proposal to build a large-scale project. This was a much better written one than any of the previous proposals. We had earlier things written, with the direction of the former project manager, Frank Schutz, but Robbie wrote a much more serious one. During the first year, he got everybody working on the proposal; he cut back a lot of the scientific work. He said that the major thing was to get a key proposal—a very strong proposal—to get big money for a big project. He thought much bigger than any of the rest of us had thought. He had everybody pretty well working on making a proposal. So a major effort for that year was developing this proposal. There were a lot of arguments, of course, about details. It turned out that most of the technical things came out more or less the way I had planned them. There were some controversial aspects between Rai's technique and mine. We both wrote different parts of the proposal. Both techniques were described in the proposal. Robbie, who had very strong views on everything, felt that the two sites had to be identical. They should be run as one thing, not the way that Rai and I had both thought of, where we each had, in some sense, our own site. It had to be jointly run, and everything had to be more or less identical on the two sites.

A little later, a key decision would be made, about whether it was going to be a Fabry-Perot technique, which I had been developing, or the delay-line technique, which Rai and the Germans had been developing. The key point was that the Fabry-Perot was more economical on space, and so you could think of having several in one system. Both, in practice, had about the same performance—at Germany, Caltech, and Glasgow. The decision was postponed for a bit, but later made by Robbie.

COHEN: When was the decision made?

DREVER: I don't have the date when that was done. Maybe I should just point out another little piece I had forgot; it was actually around that period. Among the many ideas I'd proposed, in addition to this multiple-use one, was one which followed from that. This, again, caused terrible problems with Rai when I first proposed it. It was what we called mid-stations. I proposed that we should put, halfway along the interferometer, another test mass. With the Fabry-Perot system, you could do that and still let the beam get past it to the end. This way, you would operate another interferometer half the length of the main one. You would operate them both together. I showed that these two wouldn't cost much more than the main one. The fact that one was half the length of the other would give you a way of distinguishing gravity waves from other noise sources. We went through lots of arguments. [Inaudible] You could find something unique to gravity waves out of this.

It seemed to me that a key thing would be how to prove to people that what we were seeing, if we *were* seeing something, was gravity waves. This was going to be an important new idea. You could do it—and it wouldn't cost much more—by putting this half-length interferometer in. Rai was opposed to this idea, too. Partly it was because it wasn't really compatible with the delay-line system. He was violently opposed to this idea. There were lots of arguments back and forth; there were papers written. I have a whole pile of papers with arguments for one or the other. The other important argument I made for this so-called mid-station scheme was that it meant that you would actually be required to find coincidence with three interferometers at least, probably four. I thought you would do this at both sites, so you would have two interferometers at each site. You'd have fourfold coincidence. That, I was sure, was a huge advantage.

Partly I had experience of that in experiments I had done years earlier, before I ever came to Caltech, at Glasgow, looking for radio [and light] pulses from what might have been supernovae. This was work with John Jelley. We did experiments with three or four or five or six different sites in different countries, looking for simultaneous things, and it became obvious that the more [sites] you had, the *much* more convincing the signals were. So I pressed for that, too; that was another argument for it. There were a lot of arguments about those things, with Rai opposing them because they weren't really compatible [with his system], although we could see ways to make it partly compatible. That was another argument for the Fabry-Perot system. By the way, that's been agreed upon, and that's in the present LIGO.

COHEN: So that idea is incorporated?

DREVER: Yes. It's in the present LIGO.

COHEN: All the ideas are in there?

DREVER: Well, most of them, yes. So the decision had to be made—and Robbie Vogt demanded that *he* make the decision—about whether we would go with the delay lines or with Fabry-Perot. I think that came a little bit later in the sequence, but maybe I'll discuss it now, while I have this in mind. That was a very difficult controversy. There were very hard arguments. But eventually Robbie decided for the Fabry-Perot system, and the reason he gave for it was very largely a political reason rather than a scientific one. I think this weighed heavily in his view. First of all, the cost estimates had gone up greatly over what we originally thought we could ever get. Before Robbie came on board, we had had meetings with NSF. I remember one important meeting in Washington where they told us that the cost could not become more than $60 million. We hadn't a chance ever of getting more than $60 million. Once Robbie came onto the thing, he immediately estimated the cost to be far more than that. We had been told that it was just impossible. Anyway, Robbie implied that he could get the money somehow. He was making the argument that this was going to be a national facility—it wasn't just a thing belonging to Caltech and MIT jointly. Particularly, the feature that you could have several experiments in it at once made it much more appealing from that point of view. You could then think of it much more as something that various people could be doing experiments in. So he felt that was a very important political matter. Another one was the argument that I had made for this multiple use. Thinking ahead—assuming that the whole thing went ahead; I was still convinced that it was going to take years to—that interferometers were going to be a key problem, and the first ones wouldn't be the best ones. You'd have to go on making better and better ones. How would you ever do that? Because I'd had a similar experience during the old radio-pulse experiments that I'd done years earlier, on a much smaller scale.

Begin Tape 3, Side 2

DREVER: So it had partly come from my experience before, with the radio-pulse experiments with John Jelley. Once you're looking for rare events, once the system is working, you want to keep on looking—you want to keep on going at it. But then how do you develop your next one? That was going to be a problem here, too, because how would you test your next interferometer without putting it into the vacuum system? You'd have to close the whole thing down. So another point I had made, with this multiple use, was that you could use one of the possible locations inside for a test system, so that you could, without stopping your main experiment, be making a test one as well. Robbie latched onto that idea and thought it was a very important practical idea, which couldn't be achieved with the delay-line system. You could say that all the time you were running one experiment, you could be developing the next generation in the same setup. So he made that a key aspect of his argument.

COHEN: That was a very powerful argument.

DREVER: Yes. For those reasons mostly, Robbie finally decided, very much against Rai Weiss's advice, to go for the Fabry-Perot system but make the vacuum pipes, as we had all agreed, big enough to accommodate either, so that in case it turned out that there was some basic problem—that the Fabry-Perot system wouldn't work properly when it was scaled up—you could still go back to the other one. Of course, nobody was quite certain *what* would really work when it was scaled up to this size.

So that was the decision. This annoyed Rai Weiss very strongly; he was very angry about this for a long, long time afterwards and may have never gotten over it. Anyway, he was very annoyed, but that was Robbie Vogt's decision. He required that the people at MIT change their work and instead work on things related to this Fabry-Perot system. Although a lot of the work was common—the seismic isolation and vacuum stuff were pretty well the same for both. The German group continued with the delay-line system. In fact, what has happened now in Europe is that they're making something that's in between the two of them. At the time, that was an important issue; I think now that it's really not so important. I think now that more and more one can see much more sophisticated systems that are sometimes a mixture of the two. But anyway, that was the decision that was made.

COHEN: You were not unhappy, at this point?

DREVER: No, not unhappy at this point. It was a little bit difficult, at times, with Robbie, but basically I thought he was OK. He spent a lot of his time away, fighting for money, and so on. It turned out he was extremely effective at that. In fact, the proposal that was produced everybody agreed was a good one. Robbie put a lot of effort into it, making it much more professional than we had done. In fact, when you look at it right now, it contains all the key ideas of LIGO. It was smaller—half the size of the main proposal. So I thought, and still think, it was good, although I was worried that Robbie was cutting back a lot on the scientific development to get this proposal written. Though we had agreed that needed to be done. This proposal did get good funding. It wasn't asking for the complete funding for the whole thing, but it was for detailed design and so on. It got a lot more money than the previous proposals asked for by the previous project manager.

However, around about that time, Robbie started to make things harder for me. Particularly, he started to oppose some of my views on how to do some of the technical things. Some of these I really got quite worried about. One example I can think of was when we were gradually improving the performance of this 40-meter interferometer. It was getting better and better. We were finding problems, but we were also fixing them. It was still a long way from the fundamental performance—in fact, it still is, actually. It still hasn't reached the performance you really could get with the power, and so on. But it was getting better. There were some noise sources still there. An important technical development was this mode-cleaner idea, which came from the German group. It helped, both in Glasgow, with the smaller interferometer, and with the one here; it helped significantly. I wanted to push that same idea a lot further and make this mode-cleaner a big cavity, built almost the same way, with suspended mirrors, like the main interferometer. I felt this would make the light much purer in a simple way and would make the thing much more free of the problems we were having with modulators and other things about the laser. That was a case where Robbie opposed me. Partly, he didn't really know these technical issues. But some of the other people in the group felt that it was just more complicated, I think. They wanted to have a small mode-cleaner, and I wanted to push for a much larger one. Robbie decided that it should be a small one, not the large one I wanted to do. I had to give up on that one. It's curious—I still feel a little sore about that. Although afterwards, many years later, they did in fact build a large one—just like I wanted to do, but it was years and years later. Still, the mode-cleaner in the LIGO design, although I did design it, is on a scale that is still small compared with the arms—and it's curious that what happened in general in the European efforts followed my ideas in some sense more closely. Particularly, the biggest European effort—the Italian-French collaboration [VIRGO]—has a much larger mode-cleaner, very much like the one I would like to have had.

COHEN: Was the feeling, perhaps, that things were OK, and that you should just go ahead and develop it and not change anything anymore?

DREVER: That's right—it's reasonably OK. Well, I felt we still had a long way to go with the interferometer. We were coming up to where we were beginning to prepare for the real proposal, for the real thing. I should say that one of the places I found difficulty was in writing the proposals, but I'm not very good at writing; I find writing extremely difficult. Later on, I found the reason for that, but at the time it wasn't so clear to me. Kip is a very good writer. Kip really did a lot of the writing. Rai also likes writing. I don't. Sometimes Kip would write things that I was very unhappy about in the proposal. I remember we had lots of arguments, particularly where he would say that it had to have 4-kilometer-long arms, because that's what was estimated was going to be needed to detect the known sources. I felt that just wasn't true. In fact, it was originally going to be 5 kilometers, but we couldn't find sites for 5 kilometers, so it was made 4 kilometers. I felt this was a little bit dishonest. There were lots of arguments about the precise wording of statements like that, because my feeling was that you could not estimate the real performance. There were huge uncertainties in estimates of performance. Also [it depends on] the sources, so you couldn't really say that.

I think Kip got annoyed about me being very fussy—that we worded these things so that I could accept that the statements were really true. Kip wanted to make statements that I felt weren't true; they were overstatements—that it had to be 4 kilometers, and so on. That caused some friction, which I think grew eventually, perhaps. Some of those statements worried me, because I thought you couldn't make the design sufficiently precise to know how good it was going to be.

I'm not sure I'm getting all this in the right sequence. At one point, Robbie was getting on well at obtaining big money, but I'm not sure just where that came in the sequence.

COHEN: Were you still getting on with him at the point when he was getting the money? Or were you already having problems?

DREVER: That's what I can't quite remember. The dates can be checked. But let me say a few words about that. Robbie estimated that it was way above the money that NSF had told us we could get. However, he was much better at persuading people—persuading politicians, in particular. I didn't know about everything that happened. But in spite of all the work we had done studying all kinds of science—we spent a lot of money on this JPL group going out into the country and making measurements at all these sites and exploring them all and so on—Robbie had taken over all this and more or less dropped most of this JPL effort. Eventually, the conclusion was that he himself picked two sites that were not any of the ones that had been investigated in all those previous years of studying sites. I think now that they were picked for political reasons, because basically Robbie managed to persuade the politicians from those areas of the country to support the project if the sites were there.

COHEN: The [Hanford] Washington site was political?

DREVER: Both of them. The two sites picked by Robbie were at Hanford, in Washington state—that was a federal site. We had actually, maybe, looked at it before. I certainly recollect having a visit there; I'm not quite sure whether we looked at it seriously. Technically it was a good site, seismically it was a good site, and so on. Hanford was also regarded as a high-security place, because there were all kinds of nuclear reactors all over the place—test reactors for submarines and so on. It looked technically a good place, but I think everybody thought it was so remote and far away and awkward. Anyway, that was one of the sites that Robbie picked. The other one was in [Livingston] Louisiana—that was a curious choice. I don't know all the history there. But there had always been a bar gravity-wave group at Louisiana State University. It may well have been that they proposed this, near them, as a possible place. I think there was strong political support from local politicians there. Robbie got onto that and I think built that up. [It] was in a commercial forestry area. Technically it looked like it really wasn't such a good site: It wasn't on hard rock, and so on; the seismic noise was worse. But it was a site that could be got. The company that operated the tree growing was quite friendly, apparently, and could organize— They were quite helpful. It seems to me there was very strong support from the local politicians. Part of this I'm guessing; I'm not sure of the facts here.

COHEN: I would guess that that's exactly right.

DREVER: But part of this I'm guessing, because Robbie always kept all of these things very much to himself. That was one of the other things that was strange to all of us. Robbie demanded that none of us directly talk to NSF, or to anybody, about this project. He would do all the discussions and all negotiations having to do with the project. We were not to approach NSF about anything. Everything was to go through him, every single thing, because he argued that we would somehow not understand it right and mess it up. This is one of the things we all felt rather nasty about. But that was what he told us. He insisted on all these things. He would have to control all information for the project, so it was hard to know what was really going on.

COHEN: He didn't tell you what he was doing?

DREVER: No. Just some of it. There were times that it caused problems, too. So I began to have some problems with Robbie. I felt he was attacking me, and he was. An early one was this case about the practical decision of making a bigger or smaller mode-cleaner in the lab. He told me not to make a big one; he wanted the smaller one. Another thing was, for most of the initial period he had these weekly meetings, and I would report, basically, on the work of the group as a whole and then individuals on the research side. The project manager, while he was there, would report on that side of things. Later on, Althouse took over that kind of work and reported on that kind of thing. However, after a bit, Robbie said that he didn't like the way I was doing things. He didn't understand how I did my science. He started to attack me in many of these weekly meetings.

I should say a little bit about the attacks, because they really got worse and worse and worse, and they worried me a lot. Particularly, he kept accusing me of not using the scientific method. This was something that hurt me tremendously. I thought I *was* using the scientific method—the way I was taught to do science. In fact I was: I was using the techniques I had learned from my old professor at Glasgow, which really were [Ernest] Rutherford's techniques. These were basically the kinds of techniques that I had been using very effectively—this was to cut corners, do many experiments very quickly, decide which were the most important things, skip the fine details, and move very fast. I had used that kind of technique throughout, and it had put us ahead. Most of the time, my experience had been that if there was some other group doing something in more conventional ways, such as the Germans, I could do the work approximately twice as fast and get ahead probably with less money and effort. Robbie, I think, couldn't understand that. He would think in a more conventional way, and Rai would, too—that you had to divide up the project into little pieces, and have people work on different pieces, and work out all the details, and so on, and not make the kind of jumps that I would make. Because after a lot of thinking, it would be clear to me how we could avoid a problem. In general, my approach would be, when it came to a practical problem, not necessarily to beat it but to find a way to go around it rather than through it, in some sense. I kept doing that. The result was that we moved very fast. But that somehow wasn't understood by Robbie, and he kept thinking I was guessing. I would take a step that wasn't obvious, and it would work. Robbie would say, "He guessed!" Well, I didn't guess. My intuition was very powerful—it *is* very powerful. I could see what we ought to do and what the noise source was, and so on, but I found it difficult to explain. It was very different from the way that Robbie obviously thought.

So things got worse and worse, but I was trying to stick with it the best I could and just ignore the fact that Robbie would be shouting at me every week at these meetings. He would say to the rest of the group, "Ron doesn't understand how to do the scientific method. Don't do anything the way he says," more or less. Robbie was also attacking other people, too, at these meetings. He attacked almost everybody in turn. He attacked Rai very strongly at one point. It was a particularly unpleasant event. There were some very nasty, unpleasant meetings in which other people were really put into terrible situations.

So he kept accusing me of just guessing things, even though I could show that my guesses were correct and the thing worked much faster. He became more and more against me—I didn't know at the time quite why. Afterwards, I was told by other people that he was—well, this is where things get very difficult. All I can state are the kinds of feelings I have and my opinions; I can't say they're all correct or valid. Partly, I didn't know what was going on in the background, but colleagues have told me. Some of them knew more than I did about the background. What I've been told by others is that, in a deep way, Robbie was somehow jealous of me. That's what I was told. Because I was well known to scientists and other groups around the world and he wasn't. I don't know if that's true or not, but I was told that. And that he was unhappy about my way of doing things because it wasn't somehow as formal as his way of doing it, and that he was somehow turning the [Caltech] administration against me. I don't know if this is true, but I've been told this by other people.

So anyway, he began to attack me repeatedly, almost every week, in various ways. Then at one point he told me that I should no longer be in charge of the research effort in the lab. That hurt me tremendously, because that's where my expertise was. Instead Fred [Frederick J.] Raab, who was an assistant professor at the time, should run that. He was much more of a conventional kind of person and perhaps more in tune with Robbie's views. That was a terrible blow to me. I didn't understand it, because I thought I was doing everything that Robbie was asking me to do.

By the way, I missed something here: There was another important internal meeting at one point where Robbie said that he was going to run everything about the project here and I wasn't going to be in charge of anything. I remember it was a kind of complicated meeting, and I really didn't understand it at the time. [It was] in the division chairman's office—I'm not sure if it was Ed Stone or [Gerry] Neugebauer [division chairman 1988-1993] at that time. Robbie said that he was going to make all the decisions in the experimental work as well as the political ones—the large-scale ones—and asked if I would agree to that. I said, "Well, I've always done everything you've said anyway. I don't see that it makes any difference." So I accepted it. But what happened afterwards was that he did things that I didn't want, and eventually, as you'll see, things got worse and worse. At the time, I didn't realize it was a problem; later on, he used it against me, more or less, and took charge of everything.

So things became a bit hard, and I began to get more and more miserable, because basically he opposed me and started to attack me in front of the group almost every week at these meetings. Mostly I'm not good at fighting these things; I just more or less stayed quiet, because when I suggested something he would sometimes get very angry and shout at me in front of the group. He tended to lose his temper. I had known that in the past, but it got worse and worse.

So things became hard. I felt I was being attacked. Then being removed from the experimental side, from running the lab, was really a problem. He put some of the other people in the group. The more junior people were more important than I was in running it. I was meant to stay out of it. That seemed to me stupid. It slowed things down.

I don't know if I have mentioned one little incident. There were a lot of things going on, and I shouldn't discuss all of it here. Mike [Michael E.] Zucker, one of my former students who was a very technically competent person, was developing most of the system in the lab. We were trying a new idea that I had proposed, basically to make a two-stage laser-stabilizing system in which the laser is stabilized to a cavity. We then regarded that as if that were a laser, and we would try to stabilize the frequency of light even better by applying a phase difference by a Pockels cell between that and the main system. Anyway, we built this. It was much noisier than we all thought [it would be]. We spent a long time trying to understand why this thing didn't work as well as we had expected. Eventually, partly from an experience at Glasgow, we came up with the solution that somehow it was this Pockels cell device that was in between these two cavities. Then I suddenly came up with the thought that we could eliminate it. We could actually cause the main interferometer to control the laser directly. This was at the time a kind of revolutionary idea. I remember that Mike Zucker and everybody else were all against it. They said that it couldn't possibly work—that you had to readjust the light. So there was some argument about it. I kept feeling this was going to be a way to do it. What eventually happened was that it was agreed, after some argument, that Bob Spero and I could try it together one weekend when the main thing was more or less not operating. We did, and immediately the thing got much better—significantly better. That was the kind of thing I felt showed that I *could* do these things. The group wasn't going to do it without me, because they had opposed this idea totally. But anyway, this just made me unhappier. I had had to do this on a weekend, and so on, almost against the wishes of the other people, although eventually it worked. And then everybody switched to this method, forgetting about the fact that earlier it had been a controversial issue. That's the method now used.

Anyway, that was a little thing, but I found these things difficult. And Robbie turned more and more against me. I'm not quite sure I have all the arguments here in the correct sequence; I don't have all the notes on this. When he told me to stop working in the lab, that really hurt me. I thought, What can I do? I should go and discuss it with the division chairman, who was then Gerry Neugebauer. I went and talked to him in confidence and said, "Look, I'm having terrible problems about this. Robbie is wanting me not to do the experimental work and just to solve specific things I was asked to do." Now, I had been doing what he had asked me to do anyway. He had asked me, more or less, to design aspects for the large-scale system, which in fact I was doing. I had put a lot of time into office work. Around about that time, I had developed what I called a design for a first Fabry-Perot interferometer for a large-scale system—we still didn't have the name LIGO, I think—but for a large-scale system. I developed all the details of this design, over many months, and wrote it up with very complicated drawings and so on. It turns out that the present LIGO interferometer is very largely based on that. There are some changes, but it is very largely based on that. I produced this. That was a major effort.

COHEN: So what did Neugebauer say to you when you went to him with this complaint?

DREVER: The sequence is a bit confusing here. I went to kind of complain. And he said that maybe we should have a setup in which I was developing the next generation of detectors—advanced detectors—as a separate design effort. When he proposed that, I thought it would be OK. Then, I remember, after a few days he came back and said that we couldn't do that. He didn't say why not, but I assume it's because Robbie didn't want it. Then Robbie became more and more angry with me. He even complained that I had been complaining about him to Neugebauer, even though I thought that was confidential. At one point, there was a meeting in which Robbie and the division chairman took me to the provost at that time, Paul Jennings [provost 1989-1995]. Actually, when I was having some of these problems with Robbie, I thought that I should go and talk with him [Jennings]—and I did, I thought in confidence, too. He said that I should just carry on with it somehow and that Robbie knew how to do things. At that point, I was given a kind of ultimatum. I was told that I had to do exactly what Robbie said. I wasn't to be doing experimental work. I was to act like a guru; I was to advise them. When they asked questions, I was to answer them. That was basically what I was supposed to do. I felt that was terrible. That's not what my expertise was. It was to do experimental things and come up with new ideas and develop them. I hadn't come to Caltech for this. I was absolutely devastated. I had thought this discussion, or meeting, was going to be to tell Robbie to behave a little better, but it was quite the reverse. I was shocked—I remember, I almost broke down. It was one of the few cases where I almost broke down. And then I was led to believe—I don't know; that was a very tricky meeting—but my understanding was that I was told that I mustn't discuss this meeting or any aspects of it with another member of the faculty, or with anybody else. I said, "That's terrible. You're telling me I can't do the kind of science I came here to do and also that I can't even discuss it with anybody." I think it was Neugebauer—but remember, I was in a very anxious state in this meeting—who suggested that I could talk to only one person, the human-relations advisor at Caltech.

COHEN: The human-relations advisor?

DREVER: I think that's what her position is. She is there to advise people, in total confidence, about problems. I couldn't discuss it with anybody else. I was also told that the reason I could not speak with anybody else was that if any of this kind of difficulty leaked out to NSF, they would not fund the big project. They would regard it as a key sign that there was something wrong with the project. I think at that time the funding wasn't assured. So I thought that maybe that was rational; it had to be hushed up if there was any kind of problem, because that would be a strong argument against such a big project. So I thought I had to go with this. I was being told that I mustn't discuss it with anybody. So I did go and talk with this human-relations advisor. She was actually very helpful; she was more like a kind of therapist.

COHEN: Well, that's what she is.

DREVER: I went repeatedly to see her. She advised me, among other things, to get somebody's advice on these things, and that it was a bad situation, and so on. She was certainly very supportive of me, but I don't know if it really led to anything. But I went to see her many times, because I was told I shouldn't discuss it with anybody else. She told me to take careful notes about what was going on, because a lot of it didn't sound right.

Remember, I had come from another environment, another country. I didn't know what was normal in this country. More and more, I was learning that it was very different from what I was used to. None of this could have happened where I was. I didn't know what the norm was. She was advising me that the best thing was to try and go with it but to keep good notes and so on, and maybe get some legal advice.

Anyway, things went on getting worse and worse. I tried to do everything I could to keep friendly with Robbie and to discuss things. I did what he said. He said I should stop work at the lab but I should design bigger vacuum tanks for the 40-meter system. The ones I had made originally—the 40-meter ones I had started off with—were small. Remember, they were made more or less the same as the ones at Glasgow, which were made to minimize cost. At the start, too, I felt we had to minimize cost. So the pipes were small—18-inch diameter. We all agreed that they needed to be bigger than that. It also became clear technically that we had to put more and more of the optics into vacuum, not in the air, so we had to have bigger tanks. It was clear that we should have a bigger system. Robbie told me to go and spend my time designing a totally new vacuum system and we'd get more space.

Now, I should say also that before Robbie came on board, I had already decided that—and in fact had already acquired a very large vacuum tank, essentially free, from JPL. We were planning to have one big vacuum tank. This one was a space simulator from JPL. That would give us a lot of space inside. We had already gone ahead with trying to increase the height of the lab a little bit, to accommodate it. Robbie said that was a crazy idea, that we should start from scratch. In fact, I think he sold this vacuum tank. I was supposed to start from scratch, not worry about the cost and not try to save money, just build the very best thing for a 40-meter system. So I set to work and did that. A key part was that it be made of the biggest tanks that would fit into the lab, without changing the lab. I came up with an unusual design—I think it was, at the time. We had already been extending it by adding a number of these small 18-inch stacks to get more space. I came to the conclusion that the biggest size that we could practically fit in was 4-foot tanks. I worked out a way of joining a number of these together. They ended up being almost all huge ports. So it was kind of an unconventional system. It was certainly something very different from the usual design of a vacuum system. It was based on these 4-foot tanks, but all cut away, so they were joined in a very open way that I thought would be very convenient. It was going to be expansive. They had huge 30-inch ports between these tanks. Although it looked strange, it would be a very practical scheme that would be very good for the research. It also had large pipes. I designed the whole of this. There were a lot of new ideas in it, most of which have now been built into the 40-meter system, except for one that I was keen on but Robbie didn't like. The problem had always been, How would a person actually be able to reach into these large vacuum tanks? I proposed a kind of unusual design of ports, in which you would open these ports and you could almost stand in the thing. But people said that it would be hard to make. That particular suggestion was thrown out, although it was a nice idea.

Anyway, that was a small thing. So I more or less completed the design of these tanks and at the same time was working with Bill Althouse on the detailed design for the final big system, for the end stations that were going to house this multiple-use facility. The real details of how to make big vacuum tanks that we could pool test masses into and out of at first looked very difficult. But I came up with ways of doing that, and, in collaboration with Bill Althouse, he would bring in an outside engineer to help. After many schemes, I eventually came up with ways of solving all the engineering problems, and we came up with a design that has actually been adopted for the present LIGO. But anyway, we came up with the design of these tanks and an arrangement of them inside.

There was one piece that caused a great problem. I had started off by thinking that we could squeeze a lot of the auxiliary optics into 6-foot-diameter tanks. This seemed, at the time, very big. But when we worked out the details, it was clear that they wouldn't. I proposed making some much larger tanks to house the auxiliary optics. I felt that was going to be kind of impractical. Then I proposed that we have a whole string of smaller tanks—a bit like what we were planning for the 40-meter lab. I remember the terrible day that I proposed this to Robbie and he absolutely exploded and said, "That's a terrible idea. A string of sausages along these vacuum pipes. This is hopeless." He absolutely exploded and told me the next day that I had to stop the work on designing these tanks for the system, that it was a crazy idea. He also thought that I should go up to MIT and work with Rai Weiss on some scattering problems. That was really awful for me. The design of the main layout was practically finished. I had many new ideas of how we could have many vacuum tanks in this main central corner station for this big system. OK. So I had to go away. I went up there. The stuff on scattering wasn't important. It was calculations—the kind of thing Rai likes to do and I don't like to do. Anyway, I went away for a week. When I came back, Robbie had gotten, I think, Mike Zucker to take over what I was doing. In fact, they had produced a design that was very much like mine, with a lot of small tanks, although they turned them on the side and gave them the name HAM [Horizontal Access Modules] tanks. But it was a string of sausages, too. That really annoyed me, because it was like how I said we had to do them. It was a minor detail—a good idea—to turn them on their side; that was OK; that wasn't a major difference. Anyway, he [Robbie Vogt] said that I was forbidden to take any more part in the main design, because in fact the design of the large-scale system was effectively completed.

COHEN: But you were still working on the project at this point?

DREVER: Oh, yes, but I was not in the lab. I was totally working on these design things that Robbie asked me to do—finishing the design of the bigger vacuum system for the 40-meter interferometer. I did complete the general design of that.

Up until then, I had always got many invitations to give talks at international conferences. Robbie said that I shouldn't accept these invitations and that he would give all the talks about LIGO—or people that he told. And he told me not to accept invitations to give talks on this stuff. I had also taken many people in the group to various meetings; Robbie wanted to cut back on that. Things got more and more difficult. There was a conference in Japan. I wanted to give a talk on a particular idea that a colleague of mine, Brian Meers, from the University of Glasgow, who had actually worked under me there—he was one of the few people at Glasgow who came up with new ideas; he did a lot of analysis of the recycling idea. I haven't mentioned that, I think, in this session. But some other key ideas, which Rai Weiss was totally against, were power recycling and signal recycling, as those are now called. I came up with both of these ideas around '84 or so, and Rai didn't like them, because they weren't compatible with broadband interferometers. They would fit with my system. But they turned out to be key ideas, too. This chap, Brian Meers, developed the theory of them and showed a new way of doing it called dual recycling, which was a very elegant idea. It was a practical way of making the signal recycling, as it's now called. He had done a lot of interesting things on this. In fact we were working on a joint paper on another idea that we had both developed. He at Glasgow and I at Caltech had independently come up with the idea of making a system that would look for pulsar signals using both site bands at once—have two cavities and the output tube to the signal. We had both independently come up with this idea. So we agreed to write a joint paper on this and we did. He had done the analysis, and I had the same basic idea, and we'd worked a bit on this together.

While that was happening, he was killed [January 1992]. I'm not sure of the precise timing, but it was a very sad thing. He was a young chap, and he was keen on climbing. He had gone out to climb the highest mountain in Scotland [Ben Nevis] during unsuitable weather. Apparently he was practicing for a holiday he was planning in the Alps. He and a colleague [Patrick Grey], roped together, went out alone in bad weather. They fell from a precipice and were killed instantly. That was a terrible blow to everybody, of course, and to me, too. We all liked him, and he was just killed. Anyway, there was going to be a conference in Japan, I think. Robbie was already saying that I mustn't give talks, but we had written a kind of paper on this and Robbie had approved it. He had said that it was separate work and done jointly with this chap at Glasgow and it wasn't work on LIGO. I had said that I would like to give a small talk at this conference, partly because this chap had been killed—in memory of him. Robbie agreed that it wasn't LIGO work, but Robbie wasn't keen on me going to this meeting. In fact, eventually I bought the tickets with my own money, although later on I made a mistake in this, in the grievance report that I wrote—part of it was refunded. Anyway, I paid most of the expenses myself to go. When I was there, I did actually give a short talk about this work—I think that's right. There were two conferences involved. I'm not sure I've got this totally right.

COHEN: Shall we stop now, and you can maybe review this and we can start with it next time?

DREVER: We're getting to another conference, you see. I'm really confused right now about which was which.

COHEN: OK. Why don't we stop now?

[Tape Ends]

RONALD W. P. DREVER

SESSION 4

March 13, 1997

Begin Tape 4, Side 1

COHEN: Good afternoon, Dr. Drever. I think you wanted to say something about some conferences that you were involved in, that you spoke of last time.

DREVER: Yes, I was a little confused at the time about two conferences that were involved in the kind of problems between me and Robbie. The first of these two international conferences was the Sixth Marcel Grossmann Meeting [on Recent Developments in Theoretical and Experimental General Relativity, Gravitation and Relativistic Field Theories], which was held in Kyoto, Japan, in the summer of 1991. The other relevant conference was held in Argentina, in a place called Huerta Grande; that was in June and July of 1992. So they were a year apart. These were both important international conferences. Each of them is part of a series, and they're held every two or three years or so, and they're the major conferences in the field. For this Kyoto one, of course I was invited to give talks and so on. At that time, Robbie was already telling me I mustn't accept any invitations to give talks. He would give all the talks or decide who would give the talks. I was kind of sore about that, because it was quite a thing to be invited to give a talk, but Robbie said that he would give the official talk on the subject and might appoint people to give smaller talks, but I mustn't say anything. So although I think I had already submitted a talk, or accepted the invitation to give a talk, I had to withdraw it. I decided that it was really important to go, even if Robbie would not pay me. So I did go, but I sat there saying nothing and just heard Robbie give his talk.

COHEN: Did the [LIGO] project pay for you to go, or did you pay for yourself?

DREVER: I paid for myself. There was a point where there was a mistake [about whether or not I was compensated]; I'm not sure if it's that [conference]. I certainly bought the tickets myself, but I did get some money back for one set of these tickets. I had forgotten to mention that in this grievance document we had later on. Kip jumped on me for that, but it was an honest mistake. I did pay for it myself and later got some money back from NSF or something—not all of it, but some of it. Anyway, the project didn't pay for it. So I sat there through that one and heard Robbie give his talk.

COHEN: Did Robbie give you credit for the work you had done when he gave the talk?

DREVER: I can't remember in detail, but I think probably very little or none. I had designed the whole layout, but he would never give me credit for that. I'm not sure which one it was—probably that one [the one in Kyoto]—but I remember that it wasn't a very good talk. He was asked questions and it was obvious that everybody knew that he didn't know what the questions were about. Anyway, he gave the talk; that's really all there is about that one. But it was a case that was later regarded as a violation of my academic freedom. I don't know if we should talk about the politics of that here.

COHEN: No, just that you have mentioned it; it will be on the record.

DREVER: The next important conference was this one in Argentina.

COHEN: This was part of this series of international conferences?

DREVER: Yes, a different series. There are two independent series. One is the Marcel Grossmann Meeting. The Conference on General Relativity and Gravitation [is the other]. They both are international conferences, but they're quite separate. Each travels around the world to different countries each time. In this [second] one, again, I had stuff I wanted to talk about. I had submitted a talk and in fact had sent an abstract of it to Robbie first, to ask for permission to give this talk. This was the case where it was the work that had been done with this chap Brian Meers, who had, around that time, been killed while mountain climbing. But we had done this work, and we had already prepared a paper to publish on it. And Robbie had already agreed that this wasn't LIGO work, so I thought he surely couldn't refuse that I give a talk on it, since he had already agreed sometime earlier that it wasn't LIGO work. It was about what you might call advanced interferometers which LIGO was not developing. These were some very advanced ideas. So there was a succession of correspondence back and forth between Robbie and me on this.

COHEN: Where are you? Were you in Scotland?

DREVER: No, no, here. I mean e-mail. Because basically at this point it was very difficult for me to talk to Robbie; he would never let me see him. I would ask his secretary a hundred times to see him before I saw him even once. Once I did count it—maybe "a hundred" is exaggerating, but a large number of times. He insisted that I mustn't give a talk. I remember saying something about his agreeing that it wasn't on the LIGO stuff and surely I could give a talk that's not on the LIGO. He said no, I couldn't give a talk on a subject that wasn't the LIGO either.

COHEN: Were you still connected with the project at this point?

DREVER: Oh, yes. So I asked why not? Why couldn't I give a talk on something that was not part of LIGO? He said that it would give people the wrong impression of what was happening in LIGO. That seemed to be just totally wrong.

COHEN: So you went to the conference and gave a talk?

DREVER: Yes. Well, there was more [to it] than that, because I had discussions with the division chairman, Gerry Neugebauer, at the time on this, and his recommendation to me was to go to the conference and give the talk. So I did. It was only a short talk, just a short contribution. But I went to the meeting and gave it. Then, the day after I came back, I was asked to go and see Neugebauer and Robbie Vogt [July 6, 1992]. Basically they said, "You're out of the project."

COHEN: Now, you're saying that Neugebauer told you to go give this talk, but then again he was in the office, with Robbie, saying that you were finished with the project?

DREVER: I have to be careful. The trouble is that it's hard to remember exactly what was said. I don't think Neugebauer said that I should give the talk. I think he said, "If I were you, I would give the talk." He said something like that. He probably didn't know this was going to happen. The fact that I gave a talk I don't think was even mentioned, in fact, when they said I was out of the project.

Now, I was confused also at the time. It was difficult taking in some of these things fast enough. We'll come back to that later. Because there were lots of problems with Robbie. It turns out, I find, that I have difficulty taking in verbal information quickly. I was tested at one point, and it was found that this really was true; I'm slightly dyslexic, or something like that. It was a very interesting test, by the way. It was recommended to have a tape recorder and tape things, and Robbie wouldn't let me do that. At some of the critical meetings, I took my tape recorder and said, "Robbie, I'd like to record this meeting." And he said, "No, I won't allow you to do it." I asked why not. He replied, "Because I don't trust you. You'll take away the tape, and you'll doctor it, and you'll make me say something which I didn't say."

COHEN: Is that what he said?

DREVER: That's what he said.

COHEN: You told him that you had this little bit of a disability?

DREVER: Yes, he knew about that—that's right. But anyway, that was a slightly earlier meeting, but it was a very critical meeting, a very strange meeting indeed.

Anyway, so I got back from this meeting [in Argentina, July 1992], and I was told I was out of [LIGO]. I wasn't given any rational reason. The only reasons I remember being told by Robbie were two things: One was that I hadn't done any useful work in the last two years. That was just totally untrue, because, for example, I had designed the upgraded vacuum system for the 40-meter, which has now been built. That was obviously useful and I had done it. So he couldn't say that I hadn't done it. I did it because he told me to do it. That was useful work. I had done lots of things. The other reason was a vaguer one—that somehow I had been badmouthing him, saying bad things about him to other people. Now, that wasn't true either. It turned out later on that it was confirmed that it wasn't true. I don't know if we want to discuss that here, but it was actually confirmed later. Peter Goldreich] tracked this down. The very person Robbie named as saying this then put it in writing that I *hadn't* said anything about Robbie.

COHEN: So you went out of this meeting [in Neugebauer's office] a little bit upset, of course?

DREVER: Of course.

COHEN: What was the next thing you did?

DREVER: I didn't know what to do.

COHEN: Now, he had no control over you; you're still a tenured professor here?

DREVER: Yes. Some of the earlier things were pretty significant. Before I was actually thrown out, there were very nasty things that happened—but maybe you don't want to spend the time on them. There were some very weird things that happened and some very nasty things.

COHEN: Just give me an example of one.

DREVER: The strangest meeting of all was one that was shortly before this conference [in Argentina]. This was the one where I'd taken the tape recorder and Robbie wouldn't let me use it. This was going to be an important meeting. He said that he was going to have witnesses and set me some rules. Did I tell you about this meeting?

COHEN: No.

DREVER: I should mention it.

COHEN: Go ahead. Talk about this meeting, if it will give some indication of what was going on.

DREVER: It was very strange, and I wish I *had* been able to tape record it.

COHEN: You went in there with your tape recorder and he said that you couldn't tape it?

DREVER: That's right. But he had two other people sitting there as witnesses, Fred Raab and Stan Whitcomb. They were there to hear what was said. He said somehow that he was annoyed with me and so on and that he was going to make two rules. Now, I didn't write these rules down, but he said the rules verbally. Then these witnesses subsequently wrote down their versions of these rules, which weren't the same as he actually said, I'm sure. The first and strangest of the rules was that Robbie Vogt and I must never be in the same room at the same time. That's what he said.

COHEN: No matter who else was there?

DREVER: No.

COHEN: Not alone, but with anybody?

DREVER: With anybody. We must never be in the same room at the same time.

COHEN: OK. As you say, it's a strange story.

DREVER: The way it was written, however, wasn't exactly those words. When these witnesses made a written version of it, it didn't sound quite as crazy as that, but it was still pretty crazy. I have a note somewhere of what they did. I don't know if I have it written down here. But it was equivalent to that. That was rule one, that we mustn't ever be in the same room at the same time. I said, "What does that mean? It doesn't make any sense." The interpretation of that was the following: Robbie had weekly meetings about the project, for which he was the chairman. He then said that if I came into these meetings, he would immediately walk out, because of this rule, and so the meeting would be canceled. I would then be accused of destroying the work of the project. That was the rationale that he explained to me for this rule. [Laughter] The other rule was something like the following: That I shouldn't make any use of the project facilities—Xeroxes, telephones, or something like this—

COHEN: You weren't supposed to use facilities for the project?

DREVER: Yes. It was kind of crazy anyway, because I wouldn't be using them anyway. This was like saying, "[When did you] stop beating your wife?" you know, when I hadn't been beating my wife anyway. [Laughter] It was that kind of rule; it didn't really mean anything. The first one was the operative one—that was so strange. Then of course he went on and on, saying that I hadn't done any work and this kind of thing. That was a kind of significant meeting, I think, because it really was so weird. That was just before, I think, this conference in Argentina.

COHEN: Now, was Stan Whitcomb on the project then? Had he come back to the project at that point? Because he was gone for a long time.

DREVER: Yes, that's right. He was back, because he was one of these two witnesses for that particular thing.

COHEN: OK. So he was back in his position of deputy, or whatever his position was as the assistant to Robbie [Stan Whitcomb left Caltech in 1985 and returned as deputy director of LIGO in March 1991—ed.].

DREVER: That's right. I haven't said much about the various problems that were happening before that. There were all kinds of problems. I had talked with Neugebauer about some. You know, he [Vogt] said that I should not do any experimental work and so on, I should only answer questions I was asked. I shouldn't do any experimental work or propose anything. The thing would be done, instead, by junior people. This turned out to be one of the basically wrong things.

The Yekta Gürsel incident was another strange one. Maybe we didn't discuss that. Yekta Gürsel, this Turkish chap [from JPL]. In one of the meetings, he proposed to give a paper and Robbie held it back. It doesn't particularly involve me, but I was there. It's more an indication of the way things were happening and the funny things that were going on. Well, there were weekly and monthly meetings that Robbie organized. At the monthly ones, he had people from MIT come here. They were almost always held here. At one of them, this chap Yekta Gürsel, who had been in the project from the very beginning—he really started as a theorist in Kip's group, but he was keen to do experiments. He wanted to join in the work, and he did—he did a lot of very useful things building up the lab. He was a curious person: Unlike a theorist, he liked doing extremely practical things, and he was great at building up things. Anyway, at this meeting he had written a paper that had my name on it, too, but it was mostly his work and Massimo Tinto's, a friend of his. It was a theoretical paper. They had done a computer simulation in connection with the idea that I had suggested of having half-length interferometers. People had argued that that made the system less sensitive, and I had shown that it didn't, very much, and they confirmed this by a computer modeling of it. It didn't decrease the sensitivity very significantly, or as much as people thought. They wrote a paper on this. At this meeting, he [Gürsel] said that he wanted to publish it. He gave the manuscript to Robbie, and Robbie just exploded, and shouted at him—and eventually handed the paper back to him—in front of everybody at this meeting. The chap was, of course, just desolated by this. He sat looking utterly miserable and didn't say a word the rest of the day. Then the next day Robbie had me and him into his office, and he gave this chap a telling-off for looking so miserable—he [Gürsel] had spoiled the whole tone of the meeting for the whole day! So he got a double lecture. Shortly afterwards he left. It wasn't surprising. He was just feeling terrible. He couldn't stick it.

COHEN: So then you were off of the project and had to start organizing some work of your own?

DREVER: No, no, it was a long way from that. Because one of the problems was— I'll tell you another strange thing. At earlier stages, Neugebauer, in particular—already when things were getting a bit bad—tried to find ways to fix it. For example, he suggested to me at one point that I should set up a separate research project, perhaps to develop advanced interferometers. I said, "That's great! That's a way to do it." Then the next day he came back and said that that was not allowed. I'm sure that what happened—but I'm only guessing—was that Robbie said I couldn't do that. Because earlier, Robbie, in one of these very nasty meetings— There was a nasty meeting with the provost when they asked me to act as a guru, and I had asked whether I could do another project part of the time or something, have a separate thing, and Robbie had insisted that no, anybody working on LIGO had to work absolutely full time on it and not do anything else at all. It's funny to notice that the present PI [Barry C. Barish] works half his time on a totally different project, in spite of this rule by Robbie. [Laughter]

There were all kinds of other terrible things, like the door to my secretary's office being sealed up, and so on. Things like that were just terrible. The secretary was taken away from me and moved to the basement, and the door to the secretary's office was sealed up—not just locked but actually walled up. You see, when I first came here, Caltech had set me up well. I redesigned all the rooms on one floor there, including labs [on the third floor of West Bridge (the Norman Bridge Laboratory of Physics)—ed.], and I designed an office just the way I like it, with a secretary's office next door and an interconnecting door between the two of them. Robbie decided that was bad. I mean, I got on very well with the secretary; she was very good. At one point, Robbie had builders come in, take away the door and actually build a wall, so that there was no door at all. Now, he didn't even say not to go through the door. If he had said not to go through it, I wouldn't have gone through it. But he didn't do it like that. He had builders come in and walled up the door. It's still walled up, not even very well. You can see that there had been a door there. [Laughter]

COHEN: This is unbelievable. [Laughter]

DREVER: That's not very relevant, perhaps. But it shows you that things are strange.

COHEN: That things are strange, that's correct. Not the way we think of them at a university.

DREVER: Yes. OK. An interesting little thing—some of these are going back just a bit before that. I noticed it when I went through my files. I was getting a bit desperate and not knowing whom to get advice from here. I didn't quite know whom I could ask about any of these things. I had gone to the provost at the time and he hadn't been very helpful. In fact, he told Robbie that I did that, subsequently. I also went to Neugebauer, who tried to help, but Robbie got to know about it and came back at me even harder. At one point—and I have it recorded here—I had a phone call with Barclay Kamb [d. 2011]. Maybe he was acting provost.

COHEN: Maybe he was the provost, at that time [Kamb was provost, 1987-1989—ed.].

DREVER: OK. I hadn't many interactions with him; I don't think I even went to see him. But there's only one thing he said here. I phoned him and said that there were all kinds of problems and I couldn't understand what I should be doing. He gave me some general advice that I should act tougher and fight back. But also he made the comment, which is interesting—I have it noted down—that Robbie had been complaining about me to the administration for five years. That means it was almost from the time when I started. I was totally unaware of that. Because I thought that things were going quite well. But apparently, what I've learned subsequently, Robbie was somehow destroying my reputation among the administration, and I wasn't aware of that. Now, I think that turns out to be significant, because it gives an explanation of some of the subsequent things that happened, because the administration had a distorted picture of me. They didn't know me directly; all they knew was what they heard from Robbie. Robbie had apparently been saying bad things for five years. That's what I was told anyway.

Not knowing whom to get help from, in June [1992]—this was shortly before I got flung out, when it was really getting kind of crazy. There was a short spell when I was told not to come into work. Things like this! I got in touch with Peter Goldreich, whom I didn't really know at that point—although when I had first come here he had been very helpful, even though he's not in physics, really. So someone suggested that I should go and talk with him, and I did. He had a major influence on what subsequently happened. I had long discussions with him. He was extremely concerned about all this, although he wasn't at all involved. I think he was one of the people who, when the question of Robbie working on the project was first raised, I think he told me that Robbie had done some good things and tried to encourage me that he [Robbie] would be OK. Anyway, so I talked to him [Goldreich] at that point [June 1992], and it turns out that he had a very important influence, because he turned out to be the person who was most helpful to me and advised me what to do. Otherwise I wouldn't have had any idea of what to do with any of this. When I came back [from Argentina in July] and was thrown out of the project, of course I was feeling awful. I told Peter Goldreich— Is it too personal to talk about him, by the way?

COHEN: No, it's fine. You're not saying anything bad or good. You're just saying what happened.

DREVER: He was concerned about this. He thought it was kind of unfair, I think. I didn't know all this at the time, but, first of all, he made a lot of inquiries about me, mostly without my knowledge, of people who had known me in the past.

COHEN: He probably wanted to know where you were coming from.

DREVER: That's right. I think the picture he got was that these people were kind of, more or less uniformly, on my side, basically. Anyway, after these inquiries—most of the people were abroad—he decided to help me. I also got help from Maarten Schmidt, I think via Peter Goldreich, probably, because I didn't know him before that. And Wal [Wallace] Sargent. Various other people helped me, too: [H.] Jeff Kimble particularly. Because Jeff Kimble, a young chap, had apparently been the person Caltech tried hard to encourage to come here; he had done very important work on quantum optics. He said that he came partly because I was here, because he was in close contact with Jan Hall. As I said earlier, Jan Hall played a key part in the development of the stabilizing technique, so we were good friends, although we hadn't worked together very much aside from that one time. Anyway, Jeff Kimble helped a lot. A lot of other people I knew ended up helping in an important way later on. I don't know if I should list them at this point. Jan Hall. Carlton Caves, whom I've mentioned but who, at this point, had left Caltech. Dana Anderson, who had worked with me at Caltech and was now a professor in his own right at Colorado. In Germany, there was Albrecht Rüdiger, who had been kind of one of my rivals. But he turned out to be writing extremely supportive letters to me, in spite of the fact that in some sense we were kind of rivals. It was really very nice of him. He was the person about whom Robbie complained that I had been saying bad things about him. He [Rüdiger] wrote back to say that this was completely untrue. It was a very powerful letter, directly reversing the picture that Robbie had been giving.

COHEN: Did you go on, at this point, to the Academic Freedom and Tenure Committee?

DREVER: Well, this is what happened then. Peter Goldreich advised me that the only effective thing I could do, he thought, was to make a complaint to the Academic Freedom and Tenure Committee. I didn't know anything about any of these committees. The reason he told me to do that was because this somehow involved the administration in general—it wasn't just Robbie—and all the committees except this one were, in a sense, controlled by the administration, but this one was a freely elected committee. So it was the only one that you could expect, he felt, to be totally unbiased. I think that's what he told me. Something like that.

COHEN: Well, that's the job of that committee. That's what they're supposed to do.

DREVER: I had never thought of the academic freedom committee, but that was his advice. So I ended up, with his help— I tried to gather all my notes together, like I have for this thing [laughter], and write some kind of account of events for this committee, which I did. It took me a long time, and it was very difficult. I did it as well as I could, to make as honest a picture as I could of the things that had happened. This was a private document. It was called "Notes on a Grievance." It was just for this committee, and it was describing my account of what had happened and how it seemed to me that this was unreasonable—especially the key things, like not being allowed to give talks on my own work and so on and eventually being thrown out of the work I had been brought here to do. This committee interviewed me and interviewed Robbie and so on, and eventually came out with a report that I think they sent to the president [Thomas E. Everhart]. This was basically supporting me very strongly.

COHEN: This would be about what year? 1994? Where are we?

DREVER: The [committee] report was 1992. I've got the date of this; I did dig out the report. It's a good report. It says really tough things. October 1, 1992. I haven't time to go over everything it said, but it was highly critical of the way the LIGO project was organized. There were two things: They said I had not been dealt with properly and my academic freedom had been violated, at least more than one time, in different ways. Also, what was maybe more damaging in some sense was that Caltech had organized the LIGO project extremely badly. There was quite a lot of discussion in the report about how a large project should be organized.

COHEN: So they never talked about your relationship with the whole project?

DREVER: They talked about the project and said the whole project was extremely badly organized. A thing of this scale typically had oversight boards, and there weren't any. It was all being controlled by one person, Robbie Vogt. Caltech was not overseeing it to check that things were going all right. Normally a project should have a lot of people watching it. There should be organization charts. None of this was happening. So this was extremely critical of Caltech—the way it [LIGO] was being done. Then they recommended what they called "Remedies." I didn't get time to note them all down here, but they recommended remedies for the LIGO organization—that there should be oversight committees and so on, that what was happening should be much better defined. For me—this is where I've forgotten the details—I should be brought back into the project again, and as my work depended critically on having an interferometer and I had built one for the project, I should have access to that LIGO interferometer. I don't know if they said that maybe as an alternative I would have to be provided with the funds and everything else to build another, independent interferometer of the same magnitude.

COHEN: But they did say that you should have access to the interferometer that was there.

DREVER: Yes, they did, that's right. They said that. I may not have those details right. We can go back and check exactly. It's a written document; it's all available [Caltech Archives: Documents of the Drever-LIGO Controversy. These files are closed until May 1, 2015—ed.]. So they made this report, but I was still unable to do anything, because then nothing really happened.

COHEN: Do you mean that Robbie paid no attention to this report?

DREVER: Well, I don't know. It wasn't really Robbie—*nobody* paid any attention to it. This report wasn't aimed at Robbie. I think it went to the president—to Caltech, not to Robbie. You see, also, I wasn't particularly complaining about Robbie in my grievance, either. It wasn't a personal thing against Robbie. I was complaining that these things had happened to me; I wasn't attacking Robbie. I was rather saying that this shouldn't have been allowed to happen. The academic freedom committee also said that. They weren't so much blaming Robbie, they were more blaming Caltech for not watching that Robbie was acting reasonably.

COHEN: So that report would have gone to, say, the provost and the president?

DREVER: That's who it was aimed for, yes.

COHEN: Then nothing happened.

DREVER: Nothing happened. I was still outside the project and so on. Among the terrible things that happened was that the day I was thrown out of the [project], or the day after, Robbie sent around a terrible e-mail to everybody in the project, [saying] that I was no longer part of the project, I wasn't welcome on the premises, and I wasn't allowed, unless someone else came in with me, to take away my belongings, and things like that—as if I were a thief. I happened to get a copy of this e-mail, and it was a terrible thing, because it implied that I had been stealing, which was totally untrue, of course.

COHEN: So this was after the [Academic Freedom and Tenure Committee] report?

DREVER: No, this was before it. This was the day after I was thrown out. The important thing was that it told everybody on the project that I was not welcome on the premises, so I was therefore frightened even to go into the lab or go into the rooms. There was one exception: I could go into my own office. [Laughter] Oh, dear!

Then there was a long period of, I felt, kind of stalling. Because this [AFTC report] was really saying that Caltech should somehow get things right and that I should be back in the project or something. None of it happened. They did form an oversight committee [in December 1992] to oversee the project, led by Lew Allen from JPL [JPL director 1982-1990] as the chairman. I'm not sure if Robbie was on it; I have a feeling he was [Vogt was not a member of that committee—ed.]. I learned afterwards—and I knew at the time, too—that when such committees were formed, Robbie was sufficiently influential that they wouldn't put someone on that Robbie didn't want, basically. So it didn't seem to have much effect on anything. That's just my opinion; I don't know if it's right. I don't want to be sued for saying something like that. [Laughter] There may well have been some good people. I think there were some good people on it, actually.

COHEN: As far as you were concerned, nothing happened.

DREVER: Yes. There was a small internal PMA [Division of Physics, Mathematics, and Astronomy] committee formed under Barry Barish, somehow to advise Neugebauer, I suppose. They interviewed me, and so on. In fact, they seemed to suggest some idea that there should be some kind of arbitration about the situation. Anyway, nothing much happened. I have some of the dates there. I don't know, maybe there were things going on in the background, but I wasn't aware of them. Because this must have caused a lot of worries, I would imagine, for the Caltech administration.

But then the next thing I was aware of, after this [AFTC] report was issued on October 1, 1992, occurred on January 6, 1993. Peter Goldreich raised the whole issue again at a faculty meeting, to try and get something to happen—because, again, nothing had been fixed or anything. I was still stuck, not being able to do any science or anything. Peter Goldreich felt that he had to press Caltech somehow to do things. He went around to a number of faculty members discussing the thing and got enough of them to sign a petition that there should be a faculty meeting specifically to discuss the situation. To get them to understand the situation, he showed them some copies of the [grievance] document I had written for the academic freedom committee. Although it was in principle a confidential document, he said that if I allowed him to show it, it would be OK. It was shown to them kind of in confidence, because there was nothing else written anywhere that described any of this. Although it was written for a different purpose, it was the best I could do to give an honest description of what had happened. So he showed that to some of the people he knew on the faculty, and a number eventually saw it. They kind of passed it around, and then they gave it back again. I don't know all the details. I think there was a lot happening that I wasn't aware of.

There were some meetings; there was some correspondence back and forth with President Tom Everhart. This proposed special faculty meeting was somehow regarded as a kind of threatening thing for the president—that was my understanding. I really don't understand any of this—but that's what I was told, basically. A week or so before it was going to happen, Everhart announced that Lew Allen had accepted the chair of this oversight committee. That means he had moved at last to produce the oversight committee, although that wasn't directly for me. Also there was a letter to me in which he promised my re-association with the LIGO. Those were the words, I think. I have the letter—something like that. Now, that's, of course, what I was basically wanting. I presumed he had done this kind of under pressure—but it turns out it didn't happen.

Then, also, Robbie was complaining about the way this academic freedom committee had acted, although a lot of that was kept secret. I didn't quite know what was happening, but Robbie, it seems, was actually complaining about that. Partly as a result of that, the academic freedom committee had some more interviews, looked at the thing again, and produced a second report on the thing, having essentially met with, I think, everybody a second time. The second report was more or less the same as the first one. There hadn't been any significant changes to their conclusions.

Then I was advised—I've forgotten the details of this—that this Barish committee had suggested some kind of arbitration meeting, in which it was put to me that what would happen was that I should make a proposal for research. Now, although Everhart had given me a letter to say that I would be re-associated with LIGO, still they were kind of not wanting me to do that. They instead wanted me to somehow set up some independent research.

COHEN: Who's "they"?

DREVER: Well, I'm a little hazy on this. That's something I would have to check exactly how that was. I didn't have time to check that in my files. I'll tell you what I think happened. I think it was this February 11th [1993] thing. There was a meeting with President Everhart and about a dozen faculty members, including me. I was there, but I wasn't really saying anything. These faculty members were really pressing Everhart to do something. This was a meeting, I think, raised by them, with Everhart.

COHEN: Would this have included Peter [Goldreich] and Maarten [Schmidt] and these people also?

DREVER: I think so; I can't be certain whom it included. I think so; I'm not sure. At that meeting, I remember, he [Everhart] made a promise that in fact I would be part of the LIGO. The strange thing was that the next day he had me back for another meeting, in which he effectively said, "No, you can't go back into LIGO," or something like that, having promised it the night before. I remember I felt that was a terrible thing—that the president would promise these twelve professors to do something and then the next day withdraw his promise.

COHEN: Did he give you a reason?

DREVER: I'm not sure. I think Robbie Vogt was at that second meeting. I don't know if [Everhart] gave me a reason, but my own impression is that Robbie Vogt just wouldn't have it. Robbie Vogt seemed to have tremendous power. That's my guess. I don't think I was given a reason. But I felt that was a terrible thing for him [Everhart] to have done, just in principle—to have made a promise and then withdraw it the next day. Anyway, that was one of the reasons that I thought I was supposed to set up some independent research effort. That wasn't too bad for me. That kind of thing had been suggested a couple of years earlier, by Neugebauer, and I had said OK, until I was then told it couldn't happen. My guess is, again, that it was because Robbie didn't want it. I was told I should make a proposal and that Robbie would make a kind of proposal of what he thought should happen and a committee would arbitrate about which of the two proposals was the best technically and scientifically.

Most of it was a lot of stalling action. Maarten Schmidt, at that point, helped me an awful lot, too. Around about then, Peter Goldreich was maybe away, or he had spent so much time on it that he was exhausted, I don't know. I think he was actually away. Anyway, Maarten Schmidt helped a lot around this time. He pressed, particularly, for this arbitration meeting. So I put a lot of work into making a proposal, something like an NSF proposal.

Begin Tape 4, Side 2

COHEN: Where was Kip Thorne during all of this? He doesn't seem to have been part of these meetings.

DREVER: I'm not sure. Probably not. But I'm pretty certain that he was having a strong influence behind the scenes. I'm not quite sure. The thing was brought up at various times at faculty meetings and things. But in general Kip was not helping me—he was supporting Robbie. That was the general view.

So I had to write this proposal. In fact, I wish I could be doing it still. I thought hard about this. I didn't want it to overlap with what was already happening in the LIGO project, because there was no point in that—I should do something that wasn't happening there. I had already, some years earlier, invented two key ideas. One—which I've already mentioned, I think—was what is now called power recycling. Another one, which was partly with Brian Meers, amounts to what was then called signal recycling. These were two recycling ideas. Rai

[Weiss] didn't like them at all, but that was because they weren't so compatible with his way of making an interferometer, although they were possible. Robbie decided that they shouldn't be put into the first LIGO interferometer and that it should be kept as simple as possible. Later on, that view was somewhat changed. You just couldn't get the performance without using one of them, power recycling. But it was decided that signal recycling, which is the other half of this idea, would not be used. It's still the decision that it will not be used in the first LIGO interferometer. But it's a way of improving the performance in important ways.

So I thought that maybe it would be sensible for me to study particularly those things, because no one else was doing it. However, one of the technical aspects of it that seemed to me to be important was that these ideas were only possible because of the fact that early on when I was here we discovered that there were wonderful mirrors being built for laser gyros for international navigation in airplanes and missiles. I don't know if I mentioned that. That was an absolutely key discovery—the fact was that we could then get mirrors that were a hundred times better than people had thought possible. Now, with that, you could store the light in the system for a much longer time. These ideas really only worked if you could keep light in the system for much longer than the duration of the gravity wave. This previously had never been thought possible, but this made it possible, if the system was large enough. So I proposed to build an interferometer that was large enough to do that. On my estimate, the smallest size that would be reasonable would be 200 meters. I thought that was not very much more than 40 meters, and it was a size that could go on the campus. It's not like the LIGO, which has to be out in the desert somewhere. In fact, I actually found what looked to be a practical spot for doing it on the campus. It would be mostly buried, so it wouldn't affect anybody and it wouldn't affect the [campus] appearance or anything. So that was my conclusion—that we couldn't do this work effectively with something any less than 200 meters long. With 200 meters, you could just do it. I proposed to do that. My estimate was that I could build such an interferometer for about the same cost as the 40-meter one that had been built already, because I could save money in various ways. It wouldn't be terribly expensive. The main thing was that we could be investigating these totally new ideas, in which I could see lots of promise. It could let you look for new kinds of signals, from pulsars. I'm sorry this still has not been done. It's now being done in Germany, basically.

So I made this proposal. I thought that if I sent it to NSF in the ordinary way, I would surely get the money for it, because it wasn't that expensive and it looked extremely promising. It wasn't overlapping what was happening in LIGO. The techniques, once they were developed, could go into LIGO fairly easily. Then this arbitration meeting was held. That was on April 19th and 20th, 1993. Basically, the oversight committee under Lew Allen organized it. They brought in outside people as the so-called arbitrators. But there was some argument as to how they were picked, because Robbie would refuse some of the people I would want to have, and so on. But anyway, there were four outside people who were eventually picked.

There were lots of strange things about this meeting. In particular—I found out afterwards—these outside consultants, as they called them, were not told that it was a meeting for arbitration until after the meeting. I didn't know how to cope with any of these things. Maarten Schmidt offered to act for me, basically, in this meeting, because I didn't have any idea— Coming from another country, I just didn't know how these things worked, so he said he would act for me and speak for me at the meeting. There were various rules and so on. So when we went, he and I, to this meeting, it was totally different from what we had thought it was going to be. So much so that when we heard how different it was— First of all, we were outside the door of the meeting, and Maarten Schmidt, who was acting for me, was told that I mustn't be in the room when Robbie Vogt was presenting his case. He said, "Well, that's very strange." So there were lots of arguments about that. The other weird thing—you see, I thought it was going to be kind of an equal meeting, and that you could have one person helping you. That had been kind of agreed—I think it was Kip Thorne who was going to be helping Robbie Vogt. But then we found that there were about half a dozen people supporting Robbie Vogt. In fact, most of the people from the LIGO team were there. That seemed very strange, because we thought there were meant to be just two—you know, kind of equal. Then what happened when the meeting started was even stranger. I thought it was going to be a scientific discussion, with scientific reviews and so on, so I had prepared this discussion that I had used before, in judging—mostly in Britain, it's true, where you were judging one proposal against another. I had this technical proposal. But it turned out that there was essentially none of that. What happened was that Robbie Vogt had lots of people who had worked with me in the past come up and make personal attacks on me—not on the science but on *me*—one after the other, to this committee. They said things like I was a terrible person to work with and they didn't want to work with me at all. It was very strange. They were kind of personal attacks, with not any discussion about the science at all—with Robbie Vogt there watching what they were saying! That was terrible, of course—these people had been my friends. So I just couldn't understand any of this. It was so strange.

Anyway, I made a scientific presentation of my case, which I had prepared, with the science of it, and so on. But this was hardly discussed. What happened eventually was that these outside consultants there thought about it. I was out of the room when the discussion was going on. Basically [what] they eventually said—I don't have the details of it; I couldn't find if I have a copy of their actual report—was that it was quite clear, with all these people saying this, that I shouldn't be part of the LIGO project. They also said, because Robbie had said this, that I shouldn't be allowed to build *anything* large, because people had more or less said that I was no good at running any large-scale project and so I shouldn't be allowed to build a 200-meter interferometer. That seemed to me crazy, because in fact the thing I had built, the 40-meter, had in my view been the most successful one in the world. They were saying I couldn't do it. They said something like that; I don't have the real details. Maybe we could check exactly what it was, but it was something like that. So, basically, the proposal was turned down: I couldn't build a 200-meter interferometer. I could have some small amount of money for small research, and I shouldn't be a part of LIGO—something like that, I think.

Maarten Schmidt was very annoyed at this meeting. In fact, even before it started, when he saw that there was this crowd of people who were going to be kind of on the other side, he almost refused to go in, almost said, "This isn't fair. I shouldn't take part in this meeting." There was, for quite a long time, arguing back and forth, because he was refusing to take part because it was so unfair. On the other hand, these consultants had come from abroad. They had been paid to come here and so on; it didn't seem like you could just abandon it all. So eventually he was kind of pressed to go through with it. However, he felt the whole thing was totally wrong.

There were all kinds of very strange things. Part of this I remember because Maarten Schmidt wrote an official letter of complaint about this meeting, so some of these things come from that complaint, in which he pointed out the many ways in which this was nothing like arbitration and it was totally unfair. For example, one of the really strange things was that apparently there was some discussion with him—I was kept out of the room for a lot of it—in which he was told that the meeting *wasn't* an arbitration meeting. Then, according to his written account later on, at the end of the first day he was told that it *was*. Things like this.

COHEN: It sounds very confusing.

DREVER: Very confused. These outside people—they called them consultants, although we were told they were going to be arbitrators—were, I think, totally confused. We were told by at least one of them that they weren't even told it was a meeting about arbitration. So it was very strange and the net effect wasn't good for me. Anyway, even then, they did recommend that I would get some kind of lab. But again nothing happened—at least nothing that I was aware of happened.

COHEN: Were you supposed to prepare a proposal for this lab that they were going to give you?

DREVER: No. I *had* made this technical proposal. I found out afterwards, much later, that there were things going on which I wasn't aware of at the time. I think, during that time and long afterwards, Robbie Vogt was making a complaint against the academic freedom committee, but all that was totally secret from me. The case was being prepared by him and Kip Thorne against me and also attacking the academic freedom committee. But I didn't know anything of that until much later. I still don't know much about it. My notes may be lacking a bit here; I didn't get time to sort all my files. That arbitration meeting was on April 19th and 20th of 1993. The report was essentially more or less immediately afterwards, within a few days of that. However, nothing much happened.

I think, by the way, that the establishment said that I should be able to—maybe they said I should use the existing 40-meter interferometer. I would have to check on that. Anyway, I have a note here that the division chairman, Neugebauer, in October 1993, told me that the 40-meter might be available in two or three years but that it wasn't available for me now. I also have a note that the provost at the time, Paul Jennings, said that it would only be available in four years.

COHEN: So they were essentially telling you that you couldn't work on it.

DREVER: Yes. By the way, I realize that the four years is up just now.

COHEN: [Laughter] You could ask for it.

DREVER: Well, I have, but I've been told "No." [Laughter] Even then, they didn't say it would be available to me. So, again, nothing was really happening, and I was stuck, basically, with nothing. Maybe there were things happening in the meantime, but I wasn't really aware of them.

The next thing, which I really haven't been able to check in my notes, was that Maarten Schmidt decided that we had to do something, to get something to happen. I mean, basically the administration had been told now, twice, by the academic freedom committee, that they had to do something and they hadn't done anything! Time was going on. The next step was on January 28, 1994. The grievance thing had been way back in September, two years earlier. He [Schmidt] wrote a letter to [Steven] Koonin, who was presumably then the provost, pressing for some kind of remedy. [Paul Jennings was still provost in January 1994. Koonin did not become provost until February 1995. However, he had been chair of the Academic Freedom and Tenure Committee at the time of Drever's 1992 grievance hearing.—ed.]

It was a strong letter—I have a copy—complaining that none of the commitments made by President Everhart in February of 1993 had actually materialized, including the promise that I would be part of [LIGO] and the promise that I would have a lab. None of this had actually happened. He went on to kind of suggest some possible ways out of the deadlock, in some sense. Well, it wasn't a deadlock; Caltech just wouldn't do anything. I think he suggested—This may have come from other discussions, too. First of all, he had asked me, and I had tried to estimate, what the cost of building a new interferometer and lab would be, doing it economically. My estimate had been based on the cost I had figured out to build the original 40-meter. My guess was that it would be around $8 million. I still think that's probably not far wrong. The actual money I had had from NSF was, I think, a good deal more than that, and it took many years to build it, so the rough estimate I made was around $8 million. Also, my guess was that another 40-meter or a 200-meter would be essentially the same cost. I could make a 200-meter more cheaply than the 40-meter, because in my proposal I had done that. The 40-meter was expensive because it was in a building that was big enough to house it. I proposed that the 200-meter would not be in a building but would be pipes buried just below the surface of the ground with small huts at the ends. So the overall cost, I estimated, would probably even be less than building another 40-meter. So cost, I felt, was an issue. I mean, anything at all was going to cost a lot of money, and whether it was 200 or 40 meters wasn't going to make much difference in the cost.

So the case given in this letter by Maarten Schmidt was partly that I had been told I had to stay separate from the LIGO, in which case Caltech had to provide this new lab and the [funds] to build a new interferometer, and the estimated cost was about $8 million. And he had gone on to say that if Caltech thought they couldn't afford that, then we should go back to the other option, in which I could use the existing interferometer. So it didn't say that Caltech *had* to pay the $8 million; they could let me use the existing interferometer, somehow, that was part of LIGO. So there were two options.

I don't have all the details of the intervening discussions; maybe a lot of them I wasn't really involved in. But I know that Maarten Schmidt had many discussions with the provost, the administration, and so on, about it. The next thing I have notes on here was in May of 1994. The academic freedom committee produced a memo proposing that there should be two options and that I should pick which I wanted.

COHEN: Did you go back to that committee, or did they just pick it up as unfinished business?

DREVER: I didn't go back to them. I wasn't really involved in this. Maarten Schmidt was pressing to get something done.

COHEN: I see. So that's where this was coming from.

DREVER: Yes, that's right. Two options were proposed. Unfortunately, I haven't had time to dig out that memo, to know exactly what they were proposing—because I don't think they [the options] were quite the same as what had been suggested earlier.

Anyway, then I got a letter, which I did find, from the provost and the division chairman, who was now Charlie [Charles W.] Peck [PMA chairman 1993-1998]. Neugebauer had finished his chairmanship and resigned, and it was Charlie Peck. This was a memo from them saying that Caltech was going to offer two options and I was to make the choice. We could go back later to get the real written details, but they approximately amounted to the following: The first option was that I could go back into LIGO but with no promises about what would happen. I would have to obey all the rules of LIGO, and I wouldn't have any particular position, I would just be a member of LIGO. The second option was to stay outside LIGO and Caltech would build me a new lab and provide me with startup funds of $1 million, and they would help me to get money from NSF—the key thing being that it would be a new lab, totally independent of LIGO. They would basically set me up in a research situation—not just a lab, but a lab, shops, and a suite of offices, the whole setup for a reasonable group. Now, they didn't give any figure for the cost of that, although the letter that led to it was the one that said $8 million was the cost of a complete lab plus an interferometer. This turns out to be critical. They offered to give me $1 million in startup funds and to build a good lab, basically.

It wasn't clear to me which was the right option to choose, but everybody I sought advice from advised me strongly to stay out of the LIGO because, they said, it would just be more trouble again. It was suggested to me that if I went into LIGO, they would try hard to find some way of throwing me out, accusing me somehow. Everybody, including the division chairman, strongly encouraged me not to pick the LIGO option, although I was kind of tempted to do that. On the other hand, I could see that if it was a good lab, that [second option] might be possible. But they were *not* saying I could build a 200-meter interferometer, which would have been great, because I have no doubt that with that I could have done really new science. They weren't offering that.

Neugebauer, I think, particularly proposed a specific place for the lab. Before I made the decision, we went and looked at it. He had proposed that my lab could be set up in what is now called the Synchrotron Hall. That's the building that was built in the thirties for manufacturing the Palomar mirror.

COHEN: Where is this? Under Kellogg [W. K. Kellogg Radiation Laboratory]?

DREVER: It's a big building of its own. It's joined onto Lauritsen [Charles C. Lauritsen Laboratory of High-Energy Physics]. It's a big hall. It's a nice old building. It's a very large hall, with a big crane. There's a lot of space in it; however, it's not big enough to accommodate two 40-meter arms for a 40-meter interferometer; although it's more than 40 meters long, it's less than that wide. On the other hand, it struck me that this was good. This lab had been used over the years; it was called the Synchrotron Hall because a synchrotron was housed in it there at one point, but that had been removed some years ago. Now it's used for all kinds of things in the PMA division. At the time, the most recent use was to manufacture the radio interferometer disks designed by Bob [Robert B.] Leighton [d. 1997]. The Leighton dishes were all made there. So it's been a very useful facility. It's a large hall, with a crane, and a good place for large-scale things.

Now, they weren't saying I could have all of that, but they were saying—what Peck proposed—was that I could have an area in the back of it, which happened to have a roof over it. It used to house a generator for the synchrotron. As a main lab, I could have an extension down one side, which could be 40 meters long, and I could have a region across the back which wouldn't be 40 meters, it would be about half that. So I could make a small interferometer in it. But it could be turned into a high-quality clean environment, which is important for this work. The original 40-meter wasn't [in a clean environment] at the time. The original 40-meter was built when these super mirrors, these good mirrors, were not known to exist. They are very seriously affected by dirt. To use them, you really want to have a very good clean environment. So this would be better, in that sense. It could be built as a clean environment. He proposed—and that arbitration meeting had also said, partly because of all these comments from all the people who had worked with me, that I should be situated in a different area from where the LIGO was. So he proposed, in fact, to build new offices inside this same hall. This hall is very big. He proposed to build a suite, on top of the lab area, of offices, an electronics shop, a conference room—a whole setup. I said that I would need some other labs as well, for auxiliary things for students. He said, "Yes." He showed me quite a big lab in Lauritsen nearby. He said, "Maybe you could have this one." He didn't *promise* that at the time. He also had an area that had been used by somebody else on the side of this building. At the time, nobody else was really using this, and it looked pretty good.

It was also attractive because at that time the building was very quiet. It had been designed to be quiet, for the manufacture of this Palomar mirror. That struck me as very good. A lot of the work I had done in England had been done in hangars at Harwell, which had been an air force base. I found it was wonderful to do experiments inside aircraft hangars. Because they were huge, you could do all kinds of things easily, and they kept the weather out. This was a bit like a hangar. It also had a huge crane for all kinds of generalized experiments. What I didn't know at the time, and what turned out to be serious later, was that there was a plan for using a part of that same area, to one side of it, as an assembly area for another project that was going to be assembling small spacecraft for X-ray or gamma-ray astronomy. Now, I asked if it was going to be quiet: "Yes, it's just an assembly, there's no science being done there. It won't affect you." It turned out that there was a serious problem, which only was found afterwards. That assembly area was going to be a very high-quality clean room with a large number of powerful fans running all the time. That turned out to be a serious problem, but at the time I didn't know about this. It's probable that Peck didn't know about it either. I was told this would be a quiet place, and it looked very quiet at the time.

So, after lots of thinking, I decided to take that option. I wrote a letter officially accepting it, and so on, outlining again what had been promised—the various auxiliary rooms, and so on—as well. Also, the promise that it would be done reasonably quickly—as quickly as Caltech could do it. But the letter accepting it and agreeing with it from Peck said that it would take some time, because they'd have to get architectural design work done. OK. What happened then was that I put a major effort into designing this lab, with Physical Plant helping. I decided to put everything I had into this, because nothing much could start until we had the lab, so I didn't go on vacations and so on. But it went very slowly. My feeling was that Physical Plant had it as a low-priority item, because I would make drawings and point out errors on their drawings and nothing much would happen for a couple of weeks. Then it would look like what they'd done was done in one day's work. Every two weeks looked to me that way. I may be wrong about that.

Anyway, it went very slowly. But after quite a long time—I don't have the dates here; I think it might have been a year—the design was basically finished. It looked to me like it was going to be quite a good lab. But then, just when it was practically done, there was a meeting with Physical Plant in which I was told that there was a major problem. That was that it had been realized that this adjacent project to assemble spacecraft was going to cause a lot of vibration, because it was going to have thirty-four fan units running that could never be turned off. They would be running all the time, day and night. The way it was designed, they were absolutely as close as they could be to where the most delicate part of my apparatus was going to be—only about four feet away. I immediately said that this was crazy. Why did I only find this out when everything was more or less designed? The especially bad thing about this was that I was told that these fans, once turned on, must never ever be turned off, because if they were turned off, the dust would settle in the wrong places and the very special equipment that was being built would then be dirty.

COHEN: Whose project was this?

DREVER: Chris [Christopher R.] Martin's. So that sounded terrible, to have this vibration next door. Well, I worked hard on that. I suggested that they could rearrange the layout of this adjacent project by at least moving the fans to the farthest corner away from my apparatus, rather than where they were, which was the nearest corner to my apparatus. I showed how you could do this and it wouldn't make much difference; it would just essentially turn the thing end for end. But I was told that that no, that couldn't be done, because the design work had been done, and they didn't want to hold up this other professor's project. He wasn't there yet, but it was being prepared for him. So that was rejected, although that might have solved it. I still don't see why it couldn't be just turned around. Anyway, I was told it couldn't happen. So that was a major blow.

I didn't quite know how to get round that. I did have many discussions with Dr. Peck about it, and he did try to find ways round it. One idea that I did suggest, and that he in some sense kind of encouraged, was that maybe we could move— One end of one arm of the interferometer was going to be directly against this lab that had all the vibration in it. I'm not quite sure who proposed this first, but I think I did: We could extend the lab outside a little bit. It turns out that at that point it's below ground, because there's a hill there. We could extend the lab underneath a small road, which would move the delicate test mass at that end outside and not so close to this vibrating thing. They would also have separate foundations, which would isolate it, too. Whereas, going where it had planned to be was particularly bad, because that was the one part of the lab that didn't have a good foundation. It was a second floor in that particular region, which is very bad for a delicate apparatus. So I had suggested that we could extend this outside, and actually Dr. Peck had looked around with me to see if we might be able to extend it right into aeronautics, which was next door, and perhaps have a small room inside there.

COHEN: Is that what's going on there right now?

DREVER: Yes. However, then he decided that it was too difficult politically to try to involve another department but it might be possible to extend it just up to that. That seemed to me to be a good way round this, and it would also make it closer to 40 meters. But the main argument wasn't that; the main argument was to get away from the vibration. At that point, also, I think the Physical Plant people—they didn't ask me for advice on this—decided to have some vibration consultants advise them on this. They agreed with what I knew, too—that this was going to be terrible. Basically they suggested ways that you could cut it a bit.

So I wanted to do that, but I'm not sure if it was designed at that point. Physical Plant was not keen on doing it, or so I was told. First of all, they said it wasn't possible because there were underground water mains and things that it would conflict with; anyway, they weren't keen on doing it. Here I'm a little hazy; again, I haven't checked all the notes on this. But anyway, they didn't want to do it. Around this point, I decided that we should at least go ahead— Or maybe even before that. Yes. Sorry, I think I'm getting the sequence wrong; let me go back. I think I've made a mistake with the sequence here: The design of the lab was finished, I think, before the noise problem was discovered.

COHEN: So you had done all the work not knowing that those fans were going to interfere?

DREVER: That's right. I was happy with the design. It had taken an awfully long time, but I was happy with it. I thought it was going to be a good lab. It had offices and everything. I then approved the final design to go out for bids, and Charlie Peck decided to go ahead and do that. I went away for a conference or something. When I came back, I got the message from Charlie Peck that the whole thing stopped, because Provost Koonin said that the estimated cost was too high. We'd all known, throughout most of this year, that this was going to be expensive. Although I never had it in writing, the estimated cost that everybody talked about was $2.2 million, which actually was probably reasonable, because it wasn't just a lab, it was a good, clean lab, and it was also a whole suite of offices and a conference room and a whole lot of things. There had been no limit to the cost put. In the grievance option, there was no cost put. I think that Physical Plant, knowing that, had done their very best to make a good design.

Anyway, that was the estimated cost, and then Koonin said, "Caltech can't afford that." There was a meeting in which he told me this, and I felt, "Well, that's not right!" I had been told

I was going to get a good lab; there was no cost figure mentioned. The assumption naturally was that this lab was going to be whatever Physical Plant thought was going to be the right kind of lab. Everybody up until then had been happy. Charlie Peck had been happy with the design. I had been happy with the design. Physical Plant had produced it. It looked to be a good lab; it wasn't wasteful; it was a good lab. Surely that was what should be picked. Koonin just practically said, "No." He said that Caltech was short of money for "rehab," as he called it. He said, "It has to fit within a limit." First he said $1.3 million, then he made it go up a little bit to $1.5 million. That was quite unreasonable, when the estimated cost was $2.2 million. The number we had talked about for the whole project earlier was $8 million! And that was in writing to him—that our estimate for the cost of building an interferometer and lab, a working system, was going to be $8 million. We couldn't expect to get that from NSF. Basically, you see, this was going to be funded by Caltech. So this [new estimate] seemed to be completely unreasonable.

Anyway, that was what happened. The whole thing was just halted. I think it was then that the noise problem was found—though that sequence needs to be checked. This made it look even worse. And then there was the refusal to turn the design around. Next, I proposed other things to help reduce the noise problem, like the air-conditioning machinery. A big air-conditioning machine was required for the project that was causing the trouble. I proposed that it be mounted outside the building; it had been planned to put it inside the building. The consultants also said that it should be outside the building. There was even a possible place for it. I had a problem there, because the final decision was to be made by Provost Koonin, and he said, "No, it shouldn't be outside the building"—I never quite found out why. He said, "No, it will spoil the amenities of the place, having this outside the building. It would cause noise, and there happens to be a tree there which people sit under to have—" It wouldn't affect them; they could still sit under it, but he said it would cause noise. That was even worse, because if there was noise, it would surely be bad for me inside the building. Anyway, he refused that, so we were kind of stuck.

Actually, Charlie Peck helped there, to press Physical Plant to estimate how hard it would be to make this extension to get away from the noise, at least. They estimated that at being— First of all, when I suggested that we see if we could make the whole thing more cheaply—and it looked like there were lots of ways to do that; if one really had to save money, one could.

Because in some ways Physical Plant had done a very high-quality job, and you could save money by deciding to use less electrical power and so on. But I couldn't ever get them to really go through the whole thing and actually redesign it to economize money. Also, the cost of this extension was, and still is, estimated at $200,000, which is small compared with the overall cost of the thing. So that seemed to be quite reasonable. Then we were kind of stuck. I wanted to go ahead with what could be done with at least the money available. There was a problem with that. Are we getting into too much detail here?

COHEN: Well, why don't you just say what finally happened?

DREVER: OK. What happened was then Charlie Peck wrote a memo that was going to be signed by me as well as him, so I had to agree with what was in the memo, saying that I was asking them to go ahead and build the thing up to this cost, basically. But it had lots of other statements in it that I felt I couldn't honestly sign, because I felt they were untrue. I told him that. For example, there was a statement there that the reason the thing cost more than $1.5 million was the cost of this tunnel. That was absolutely untrue. The cost of the tunnel was estimated at $200,000. We were out by far more than that, even with no tunnel.

COHEN: So what finally happened?

DREVER: I felt I couldn't sign this document, and didn't. I didn't quite know what to do, so it was rather stuck. And he was pressing. Also at this time I had to sign away my rights. I had to say I wasn't going to ask for any more money. I felt that was wrong, too, because part of this promise had been that they would build me a good lab with no money limit. So it was kind of stuck. This was quite recent; I don't have the dates here, but this was very recent.

But anyway, what actually happened was that everything looked stalled and I was still stuck with nothing. This was now some years after the decisions had been made and they'd said they'd move it fast, but nothing was happening. So I was getting desperate, and then I said, "Well, look, I've got to get some science going somehow or other. Let's make even a temporary enclosure." I noticed that the LIGO had built a wooden mockup of a section of a LIGO beam tube very quickly in this big hall. They made that so quickly and cheaply. That would even do as a lab for me, something like that. Maybe I could get the same kind of thing done, in the same way. They'd built it in a few days and apparently very cheaply. So I asked if we could get an enclosure, at least, so I could put one arm down one end of the hall—made just this way, with wood, very quickly. And Charlie Peck said, "Fine." We made quick sketches and so on, and the carpenters more or less said they could do it very cheaply and very fast. They were about to start when I got a message that, no, it would violate fire precautions. The fire people wouldn't approve it. When, again, it was about to just go. It had been designed. The weirdness about that is that the LIGO one existed and presumably violated fire precautions, too. But anyway, it had been built without worrying about the fires, I suppose.

Anyway, they said it wouldn't be allowed. OK. So they went back again and designed kind of an intermediate thing, which was built with fire-resistant materials but still wasn't a proper thing. But it was stuff that could largely be improved into the final, good configuration.

[Tape Ends]

RONALD W. P. DREVER

SESSION 5

June 3, 1997

Begin Tape 5, Side 1

COHEN: Good afternoon, Professor Drever. Let's see if we can get, for the record, a little bit about what you are doing right now, in your independent state.

DREVER: Maybe we should go back, though. I played last night what we had said the previous time, and we hadn't quite got up to date.

COHEN: Where were we?

DREVER: I was trying to remember just exactly where that was. I left my notes behind. Basically, I had to make this choice of whether to go into LIGO or stay out of it. But if I stayed out of it—and everybody pressed me to stay out of it—I would be getting a startup fund of $1 million plus a lab built for me. That was more or less the agreement—plus a guaranteed part in the first gravity-wave search with the LIGO. I did in fact choose the option of staying out, and I started to try and get the lab designed—I don't know if we discussed that last time—and it seemed to go very slowly.

COHEN: Now, they did give you a space, which they made for you?

DREVER: Yes. Right. There were a lot of problems in between. The space that was offered at first, called the Synchrotron Hall, was a very fine lab. Did we discuss that last time?

COHEN: Yes, and you were saying that you were sharing it with someone and you were afraid that was too noisy.

DREVER: Yes, there were problems with noise, which came up at the last stage, when the design was essentially completed. Then there were discussions about my going to some off-campus site, which hadn't actually been purchased at that time. I decided to stay here, on this site. Then there was the problem—which I was told about at the last minute when we had agreed to go ahead and put out contracts and get the place built—at the last minute I was told that it would be too expensive and that Caltech wouldn't pay for it. That was a terrible blow, because more than a year had been spent designing it, with everybody's approval.

COHEN: Was this within the amount of money that you thought you had been promised?

DREVER: There was no specific sum. But it wasn't a surprise, it was the money that everybody I talked with had guessed it was going to cost. There was no limit *put* on the money. It was what everybody expected it was going to cost, so it wasn't a surprise. So it was a real surprise to me when it was all go and I was suddenly told, "No, we can't build it, it's too expensive." So the thing was stalled once more. The arguments went on for some time. Charlie Peck, the division chairman, wanted me to sign a document that was going to effectively sign away my rights to ask for any more money, to start something with very limited funding. I didn't want to do that. There were also very many statements which I couldn't sign, because I felt they were not true statements. So the thing kind of stalled a bit.

Finally, in desperation to try and get things going, I said, "Well, look, years are going past. We've got to get something going, even if Caltech isn't keen right now, somehow, for some reason, to pay the money they promised me. Let's at least try and get some experiments going." At that point, I had seen that the LIGO people had built, very quickly, a wooden mockup, a replica, of a section of LIGO beam tube, which had been made by carpenters in a matter of a few days and very cheaply. It looked like, if they could do that so quick, I could start off some experiments just in something like that. I suggested this. I said, "This would cost very little. Let's build an enclosure with wood, the cheapest grade possible, which could be done at very small expense and very quickly, and then we could start something while discussions go on about making the really good lab." Charlie Peck said, "Fine, go ahead." Physical Plant designed it. The carpenters said they could do it in a very short time—a few days. We said, "Fine, go ahead." Then the same thing again: "No, it can't be done, because it would break the fire regulations." After it had all been designed! And after the identical thing had been built by LIGO with no worry about the fire regulations. Again, it was the same picture. So then I pressed again: "Can you make it fireproof?" OK. So what finally was done was to put a very crude enclosure down one wall, which gave us a 40-meter length, but it was crude. There was no air conditioning, and it was not even sealed properly, but it was made at least of fireproof material.

COHEN: Now, where was this?

DREVER: This was inside the Synchrotron Hall. So our plan was going to be used, but not made properly, to save cost.

COHEN: To quickly do something.

DREVER: Quickly put together, to give us an enclosed area, at least.

COHEN: When you say “we,” Ron, who’s working with you?

DREVER: At this point, I had this startup money, and all I could afford was one person. So I did actually advertise, and we got Steve [Steven J.] Augst, who was officially an engineer. He was working perhaps in Canada at the time, and he saw the advertisement on the e-mail or something. We were looking for someone who was good at building things. That wasn’t really his forte, but he seemed to be very willing. He has a PhD in optics, but he agreed to come on as a kind of— Basically to build the thing and that’s what he’s been doing, and he’s been a great help.

COHEN: So he has come, and he’s working, and that’s working out fine?

DREVER: Yes, and he’s the only person I have working with me, essentially. He’s been making all the stuff. That’s fine, but I wish I had more, and more highly skilled— But nevertheless, he’s extremely willing to try all kinds of things. So the pair of us managed to get this enclosure and immediately got into it. The old vacuum system from the original 40-meter, which was now [inaudible], and I’ve started to set up some experiments there. The other thing that’s recently happened in relation to that—before we leave that issue—is that we still couldn’t get an agreement to get the real building started. But I pressed again, and with some help managed to persuade them to build at least an extension to get under the road—I think I mentioned this before—to get away from this vibration. A small tunnel extension. That was a difficult thing to press through, but the cost was low. That's been done also. I managed to get Caltech to do that, and it's just about finished now. We're going to move in next week.

COHEN: So one is seeing something now.

DREVER: It's something, but it's minimal. It has no air conditioning and no temperature control or anything like that. It's not a clean-room environment. The major thing is still being stalled. I'm going to have to press harder to try and get this thing. The promise was to build a lab, and it hasn't been done yet, but we have a little bit of space, anyway.

Also, I prepared a proposal to NSF. I have mixed feelings—you asked about the research that I'm doing now. I knew what I wanted to do; I think I said that last time. I had already made the proposal several times to build a 200-meter system to study dual recycling and try and make much higher-performance interferometers of a limited size. Even with 40 meters, I would be keen to do that. But it looked like [I couldn't] unless I could get more money, and NSF said that there wasn't much money, because LIGO basically was now consuming effectively practically all the money from NSF in the country for gravitational physics, and only a tiny fraction wasn't being given for LIGO. So I was unlikely to get anything close to the funds I'd received before. To do something on the same scale as before was going to be very difficult. I spent more than a year, while all the time thinking about scientific issues, thinking about what the most valuable thing to do would be. I did know what was the most important thing for LIGO to do, and that was to try to make optical systems of higher performance. I still think that's the key missing element in the LIGO project. It may not have been fully recognized by people that you really have to get much higher performance than what has been achieved with the present 40-meter on the optical side. I wanted to try and do that. I've been thinking of ways to do that. I did come up with some ideas, in the proposal I've made. The first idea I had was to get to much higher power, and the idea of using diffractive optics, which at the time was a new one. I'd had this [idea] some years earlier, as a way of avoiding having light going through the test masses. It turns out, then, that you could take higher power— I won't go into the details here. I'd discussed it with a few other people, including Professor [Robert L.] Byer from Stanford, some years back. And it turns out that later on he decided to do this, too—I don't know if it was independently or not. I discussed it with several people, including the Germans, to get advice. Anyway, I wanted to study that, and I made a proposal involving that. But I did feel all the time that it was going to be very hard to do anything really competitive there, because it needs a building before you even start to build an interferometer like the one I'd already built and now was excluded from. That one took some years to build. To make another one, you've got to have the infrastructure to start with, and then you've got to make it better. That was going to take many, many years. I thought that was still the most important thing to do for LIGO—basically to develop dual-recycling interferometers. LIGO still has not really decided to do this.

Dual recycling. Did I discuss that earlier? Let me go back a bit. Recycling, I think, is something we discussed before. Then I came up, shortly afterwards, with an extension of that, which is now called power recycling. Shortly afterwards, I had the idea of making a system that was resonant for the side bands of the gravity waves as well. At the time, I called it resonance recycling, because mostly I had in mind the application of looking for narrowband signals, like from pulsars. That was the first idea, I know of, of that kind. A year or more afterwards, Brian Meers—whom I've mentioned before as working at Glasgow with me, a very smart chap—after spending a lot of time analyzing schemes like this, came up with a slightly different version that had the same properties, which he called dual recycling, with different optical geometry. It was equivalent to my resonant recycling but in fact a better, more flexible geometry. You could show that they were largely similar, but it was a better one. He worked out the theory of this. I still think this is a very, very significant advance—that general concept. It's a good geometry. There need to be better ones, but that's a good system. In fact, we did some further work together on it before he was killed.

COHEN: This was in Glasgow?

DREVER: No. He was in Glasgow, and I was here, but we continued to collaborate. He visited here. We did do some later work, just before he was killed. I think we discussed that last time.

COHEN: Yes, we did.

DREVER: So I still wanted to do further work on that. But I came to the conclusion that it was going to be very hard to catch up for a number of years with that, so I was thinking about other directions that I could go, in the shorter term, with limited funds. I thought I'd go back to seismic isolation, because the other area that would clearly be a big advance would be to make the LIGO system good in lower frequencies. Everybody was aware that that would be a big advance scientifically, because the signals were stronger there and you could track signals from neutron star coalescences for longer times. But also everybody thought it would be difficult. I knew it would be difficult, but I've always felt that that problem was solvable. It was technically difficult, but it wasn't nearly as hard as the problems that had been solved in the optics. So I thought that would maybe be worth a shot.

I thought hard about how to do that. Other people were doing it—there were several schemes around the world for developing that. Anyway, I thought we should try some different ones. There are two key schemes: One, I think, nobody has tried yet. I began to try it. It was to couple the suspension points of the two ends of each arm of the interferometer by auxiliary beams. This would render any residual seismic motion [inaudible], and it could be removed. I already had started to get that built on the 40-meter when Robbie canceled that project and didn't renew the position of the man who was working on it; in fact it was a Chinese person. That was stopped halfway through, but I thought, "That's still a sensible idea," one I proposed to do.

Then, a further idea that struck me as worth investigating was the possibility of levitating the test masses magnetically. That's an old idea, and many people have thought about it but always thought it kind of impractical. I thought, "Well, maybe we should look at it again. Maybe there's a practical and simple way to make this. Maybe this would be a way of making a much longer period." I did spend some time, during this period when I had no lab and no nothing, messing about a bit myself, on a very small scale, trying various kinds of room-temperature levitation things, some stabilized by diamagnetic material, but they were rather small. Then there were the servo systems. I eventually came up with a configuration of permanent magnets, which looks very promising. So I proposed investigating that, knowing it was an easier thing to do—not requiring such high technology; it could be done without much of a lab. And so we could start there, and that's what I proposed to NSF. I proposed this in October of last year; there was a certain deadline by which NSF wanted proposals. I wrote a proposal with these things in it, and we still haven't had the official answer. [Laughter]

COHEN: Are they encouraging you at all?

DREVER: Yes. They are supposed to respond within six months; I think it has now been eight months. Anyway, what they said was that it had good reviews but they had no money, because all the money in gravity has gone to LIGO. However, they called me and told me verbally that they're going to give me some money and they're trying to get money from somewhere else in NSF.

COHEN: That's wonderful, because that's so difficult.

DREVER: The trouble is, it's not going to be enough. It's less than half what I estimated I would need. So, we'll get something.

COHEN: Well, many people are getting nothing, Ron.

DREVER: Yes. So that will be a help. But it's taken a long time, all these things, and I still don't have a lab. But we've been working. What I'm trying to do just now is the stuff with the magnetic levitation, which may well fail, but it's fun to try it anyway. We've now got a two-stage levitation system that looks very promising, but I can't guarantee that it's going to work out, because it's very dangerous and can be coupled to external Earth fields. We've got tricks to try and remove that, or reduce it. Whether it's going to work out, I don't know. The other one—coupling the ends—is perhaps more likely to be successful, but not likely to be such a major step forward—I don't know. But it's something that we are at least trying right now. My hope is that it will lead to being able to push gravity-wave work down to lower frequencies. I have some other plans that I haven't yet discussed, in general. One thing that attracted me to go in this direction was that I could see that we could perhaps do some science that wasn't gravity waves with the same apparatus. This design that we're working on now could function down to extremely low frequencies at poor sensitivity, but maybe then we might see gravity gradients of some interest from geophysical sources. Particularly a possibility may be, say, oscillations of the solid core of the center of the Earth. This may be something that could be detectable by this apparatus, I don't know. It's easier than gravity waves, and we could do it on the side. But I don't know if that will work out.

So we're trying these things, but there's very much a feeling that this is only second best, not as important as the detection of gravity waves. And I feel very much handicapped—almost being forced, for reasons I don't understand, not to make my full contribution to that. I do not understand it. But it does seem, somehow, that some people around are somehow— Or something is preventing me. I don't get the equipment to use that I built. I don't get the help that I would expect. I don't understand that. Nevertheless I have some money and I'm doing some experiments. But I can't help feeling that they're second best and that I could be doing more.

COHEN: Now, given that this goes ahead and the equipment is being built, what is your feeling about where the whole thing is going?

DREVER: About LIGO?

COHEN: About LIGO. Where's it going? I mean, looking at it from the outside, in some sense.

DREVER: I would like to maybe add more later. I haven't had time to really collect all my thoughts about this. [Pause] I don't know. There are very mixed feelings. Of course, it's very hard to judge, because we're in the stage now where the construction part of LIGO seems to be going ahead very successfully. There are over a hundred people now building these huge facilities.

COHEN: It's in Washington [state] that they're working now?

DREVER: And Louisiana. Both are now started. It's a huge thing and there are a lot of engineers working on it. As far as I can tell, the ones I know seem to be good. So it looks to me that that's actually being done quite well from a construction-project point of view. My nervousness about it is that it's not clear to me that the science of it is getting anything like the equivalent quality of study. I think the technical things are being attacked well, but the deeper ones—how you actually sense the effects; the apparatus you put in it—seem not to be receiving anything like as much attention. And I find this very strange, given that LIGO has more money now than any other project like it in the world. But most of the money is going into this big construction, which maybe is the right thing. Relatively little is going into being quite sure that the best scientific techniques and the cleverest people are being used. In fact, it's almost been frightening away clever people. That's the impression I have.

Certainly the fact that LIGO got this huge amount of money through the efforts of Robbie Vogt—that's what he was successful with. He got more money than anybody could have believed possible. Although it's not clear, when you look back— I was worried at the time that some of the statements made to get that money were marginally true, but nevertheless it succeeded, and maybe that's what you count. The money was obtained, and this huge thing is being built.

Will it be successful in seeing gravity waves? I think that's not at all clear. Now, it might be. If it is, everybody will say, "It's a fantastic success!" It might not see anything at all. Then people would say, "I told you so. It was a total waste of money from the beginning!" But at this present time, I don't know what's going to happen.

COHEN: So you think it could go either way?

DREVER: It could be either way. Now, my own hunch at the moment is that it's not going to work technically as well as expected at the beginning. Because I do feel that there's a certain lack of effort in that direction. Maybe that's intentional, or maybe it's for lack of understanding of the huge problems there are in stepping up from the 40-meter scale of the optics to the larger-scale system. I don't know. For example, there are two projects in Europe that are interesting to compare it with. They probably wouldn't have got going either, without the fact that LIGO put this huge money into it. That stimulated the rest of the world.

COHEN: Competition?

DREVER: Not so much competition. I think perhaps more the fact that the U.S.A. is putting all this money into it must mean that there's some sense in it. [Laughter] There is now an Italian-French project of nearly comparable size. It started in Italy, but it's a joint project with France, called the VIRGO project, to build one 3-kilometer system, which is rather similar to LIGO. It's got big funding from both these countries. A difference is that it's got very strong scientific backing by a large number of good scientists in France and Italy. There's another project that started with the one that I began in Scotland and Jim Hough took over when I left. He was the person I had trained up from a student. He took over and pressed hard to get money to build such a thing in Scotland—or a smaller version; we thought a one-kilometer one, but the British government didn't have the money. What eventually happened was that there was a corresponding group in Germany—as I mentioned earlier, a very good group—and they also proposed to build a similar-scale thing. In fact they had some very ingenious ideas about building a triangular-shaped system of 3 kilometers to a side in Germany, but, again, they couldn't get money, because shortly after that time there was the political situation of the wall being brought down between East and West Germany, which caused a drain of money in West Germany. Although these were both good scientific efforts, there wasn't the money. So what eventually happened was that they collaborated. The Germans managed to get the local government in Hannover to pay the main cost to build a system at much lower cost than the LIGO. It was smaller—600 meters. In fact, at a very small cost; they would get the land free, and so on. They're going ahead to build what they're now calling GEO 600. Most of the money is coming from Germany, but some contribution is from Britain. It's a joint British-German effort.

COHEN: You think they may be successful?

DREVER: Yes, I think it may be successful. That's the ironic thing. This is effectively the group that I started in Glasgow; it's partly their technology and partly German technology. These are both pioneering technical scientific groups. They don't have much money; the thing's very small. So they are making up for it by being as smart as possible. The system they're building is almost exactly what I wanted to do when I proposed to build a 200-meter system here. It's very much like that. They will use all the latest ideas—dual recycling, all the cleverest ideas you could think of—because they feel that they've got to detect something with less money than anybody else. So they're really trying harder, in a way. I admire them for it. They're also trying to do it very quickly. They may not be successful, because it's only a fraction of the size of LIGO and a very small fraction of the money. Everything is being done to save money. But it's going to be a more sophisticated system at the beginning than LIGO is. They will look for signals that I think are more promising at the beginning. Because there only is enough money to build one, and LIGO has built two with the aim of looking for mostly binary neutron star coalescences—impulsive events. This project in Germany will in fact start by looking for pulsar signals. I wouldn't be surprised if that's where gravity waves are detected first. But the trouble is, there's no real estimate. You can't make a good estimate of the size of the pulsar signals, because it depends on details of neutron-star structure that are not known. So there's no firm prediction. But it's possible that they might detect gravity waves before the LIGO does—different ones. That's a very interesting project.

So, compared with LIGO, it brings out the difference. LIGO is the one that has more money than anybody else, I think, and is done on a heavy engineering basis and certainly will have very good facilities. Well, maybe that's going to be a success because once it's there— One of the early feelings was that as long as [LIGO] is given money to keep going, you could develop better and better instruments to fit into the vacuum system. So, maybe not at the beginning but after some years, eventually this may allow really good things to surface.

COHEN: The substructure is there.

DREVER: That's right. A curious thing about it is, when I modeled all this early on in the project here at Caltech, this was what I was pushing for. In the early days, long before Robbie even joined the project, I remember having long discussions about this when Ed Stone was the division chairman. But my feeling was that although part of the LIGO—it wasn't called LIGO then—part of a big gravity experiment was large scale, it wasn't the key part. The large-scale part was the vacuum engineering of the big vacuum system. But the technical challenge was much more in the scientific part, which was a relatively small part of the whole thing. In fact, I suggested many times that the project should be cut into two parts. There should be a big engineering part to construct the big vacuum system. In fact, I coined the expression for this; we called it "to build a hole in the atmosphere." We just wanted a big hole in the atmosphere where we could put light beams—a vacuum through the atmosphere. Then there could be relatively small, separate experiments using this facility. Different kinds of interferometers and systems could be tried, and they could be developed by smaller groups, on a small scale. I was always told that was totally impossible.

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DREVER: I discussed this many times. I was told by Ed Stone that politically that can't happen in America, because the big money would be going for the part that wouldn't really do the detection. You couldn't guarantee that the other, smaller part would be done properly. So he said that it had to be one project. I was against that, because I would much rather have concentrated on the smaller-scale part of it. Now, the incredible thing I now notice is—although nobody [else] seems to be noticing it—that in some sense that's almost what's happening. This huge money is going into constructing this huge thing. However, unfortunately it's also eaten up all the money. There's almost none available. There's no independent development of interferometers—not much, really. So it's only half happening. But it looks like what's going to end up is that we'll have this huge vacuum system and maybe relatively poor interferometers to put into it. Years later, it might turn out to be what I was asking for at the beginning—but many decades later. So whether it's going to be a success is very hard to say. Right now I should say that the estimates of signals are, if anything, not getting stronger. The technical problems are looking harder. I don't think you could guarantee that it's going to be successful. Especially as this choice was made early on in the project—by Robbie initially, and it's been carried forward—to make the initial interferometers fairly simple and not use all the technology available. That's going to handicap the early stages of it a bit—but it could be changed, perhaps. So it's very hard to predict. I still have the feeling that it is, in some sense, a pity. There's a huge amount of money going into this, and maybe more could have been got for that money.

COHEN: Maybe if they had made the division of technology and science more evenly?

DREVER: Yes. I mean, the kind of things that I argued about at the early stages, particularly with Kip Thorne. He made statements, to get money, that it had to be 4 kilometers long. He said it as if that was the distance, and that was just untrue. At that point, nobody knew that, and to say it had to be 4 kilometers was just plain not true. I think he wanted it to be as big as he could get. But the way he put it: "It has to be four kilometers long." In fact, as I said, it's more likely that a 600-meter one might well beat it for a bit, although the extra length helps. But I'm still quite sure now that you could cut a kilometer off the length and use that money to fund scientific development of our technology, and you would win by an enormous factor overall. But I think the problem is the politics that probably make that impossible, rather than the science.

COHEN: Maybe one would have gotten nothing had they not gone this way. So you don't know.

DREVER: It could be. One doesn't know. Again, the bottom line will be what actually happens. Also, the chance of seeing the predicted signals is relatively small. Of course, everybody says, and it's true, that when you start a new area there's some chance of finding something totally unexpected. That's true. In this case, it's curious—people say, "Well, we could expect that the first signal will be the unexpected one." Of course I would agree with that, because the expected are too small. [Laughter] Although people haven't recognized that saying that is more or less saying, "Well, the expected ones are too small, but maybe there will be something bigger," which there certainly could be. Then if that happens, people will say it was fantastic planning and foresight and a great success. So at this stage, whatever one says, you cannot be sure. I hope it works well. I wish I could somehow help more. Maybe what I'm doing, although it's on a small scale now, may turn out to be important. You can certainly see many ways to build better detectors of gravity waves. This was pushed ahead, I feel, maybe a little bit too soon. There are many new concepts available that are better. There probably are better ways that might not have to be—

COHEN: Maybe being detached from the humdrum of getting daily problems solved lets you think of these things.

DREVER: Well, no, because I feel at the moment that I've got to solve much cruder problems by getting a building and lab made. This is not very useful for stimulating scientific thought. A lot of the experiments I'm doing right now are mostly things that were already solved before. Although I'm thinking of new things as much as I can, I don't think it's the most effective situation. But it's better than nothing. There are plenty of new ideas. This whole issue of quantum nondemolition methods is probably going to be important. But they all look too far away. But I keep seeing new possibilities. Right now, the space version of this thing [LISA—Laser Interferometer Space Antenna] looks almost more promising. But although the idea was developed in the U.S., it was concluded that there was no money for it. Basically it's now going to be built, or is more likely to be built, by the European Space Agency. That may change. But, again, it looks like that's going to take much longer than the LIGO. The curious thing is that we always used to think that the space thing would cost more. Now it looks like the LIGO is so expensive that it's just as much as we thought a space project was going to be.

COHEN: Now, the space project is all European? It's not here?

DREVER: Currently, yes, because there was essentially very little support from NASA. And so although the whole design was developed here, it's been handed over to Europe. Now, they're having money problems, too. Currently, I think, the plan is that it's not going to be launched until 2017 or something. But there's still some effort at JPL to try and press for new technology for making very small, miniature, cheap spacecraft. This might make it possible, if funding can be gotten for that, on a smaller scale.

I keep seeing other possibilities, too. The other area I keep thinking about—and this is vague at the moment—but I'm still not sure that there are not going to be ways of doing low-frequency measurements on the ground. That would be a very significant thing. I keep looking for possibilities of looking for the rotational aspect of the gravitational waves, because I can see— I don't know if I should say it. These are some unpublished ideas, not even discussed much. They aren't published because I haven't really seen a practical way to do it, but it seems to me that there's a totally other way of looking for gravity waves, in which you'd try to build an apparatus that's insensitive to Newtonian forces and would only respond to relativistic forces. The prototype for this would be something like a pair of rotation centers, like gyroscopes, and you'd monitor their relative angular position when they are, say, some distance apart which is large enough to be in a different phase of the gravity wave. That's roughly the idea. Such a thing could, I think, in principle be made. It has been proposed by other people, too. One proposal was to make a kind of figure-of-eight superfluid loop, or a figure-of-eight laser gyro. I've thought hard about these things. If one could make the sensitivity good enough, then it means that we might be able to do on the surface of the Earth things that previously we thought would have to be done in space. Then it looks like one could do low-frequency experiments on the Earth, and that would open a vast new area. So I think there's something promising there. I've spent a lot of time looking at the possibilities. Could I do something like this? In fact, I should say that one of the reasons that impelled me to pick this Synchrotron Hall as a place for a lab was that its height made it feasible to think of making relatively large loops, essentially vacuum-pipe loops, with laser gyros operating in a kind of differential mode—that maybe one could eventually do this. However, if you put in the numbers, it doesn't look like the sensitivity is currently good enough. But there's still possibilities of superfluid versions. So I think this is a totally different area that has not really been considered seriously up until now. It may be a better answer.

COHEN: Well, it's good that you can think of these things.

DREVER: But I don't know if it will.

COHEN: Well, one doesn't know. Well, Ron, I think we've got a good idea of where you want to go sometime.

DREVER: Yes. Maybe I should add some more later. This is not properly thought out. More can be said.

[Tape Ends]