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STANLEY E. WHITCOMB
(b. 1951)

INTERVIEWED BY
SHIRLEY K. COHEN

March 7 and 14, 1997

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the LIGO Series Interviews

The interview of Stanley E. Whitcomb (1997) was originally done as part of a series of oral histories conducted by the Caltech Archives between 1996 and 2000 on the beginnings of the Laser Interferometer Gravitational-Wave Observatory (LIGO). The original LIGO partnership was formed between Caltech and MIT. Many of those interviews have already been made available. All Caltech interviews that cover LIGO, either exclusively or in part, have been indexed and keyworded for LIGO to enable online discovery.

Subject area

Physics, LIGO

Abstract

Interview in two sessions in March 1997 with Stanley Whitcomb, then deputy director of LIGO. Whitcomb talks about his upbringing and education in Denver, Colorado, his undergraduate studies in physics at Caltech, and his PhD work at the University of Chicago. He recalls being recruited onto the LIGO project as its first dedicated faculty member by his undergraduate advisor R. Vogt in 1980. He describes the politics and personnel, and technical and administrative challenges of LIGO's start-up phase in the early 1980s, including the involvement of

K. Thorne, the recruitment of R. Drever from Glasgow, and competing gravitational-wave initiatives headed by R. Weiss at MIT, and at Max Planck in Garching, Germany. He discusses the factors that prompted him to leave the project for private industry in 1985, his return as LIGO's deputy director in 1991, and the NSF's role in brokering an initially fraught LIGO partnership between Caltech and MIT under Vogt's leadership. There is extensive discussion of Caltech and MIT's divergent R&D approaches to gravitational-wave instrumentation and engineering in the 1980s and early '90s, their respective merits and drawbacks, the challenges faced in resolving these differences, the technical advances of the 1990s, and prospects for future success.

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 STANLEY E. WHITCOMB

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

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Pasadena, California

Session 1	**March 7, 1997**
Session 2	**March 14, 1997**

Begin Tape 1, Side 1

COHEN: Good morning, Dr. Whitcomb. Welcome to the Archives.

WHITCOMB: Thank you.

COHEN: Why don't you just talk about yourself—your family life, what your parents did, a little bit about where you went to school.

WHITCOMB: Very briefly, I was born in Denver and grew up in a suburb called Englewood. I was the oldest of three children; I have one younger sister and a still younger brother. My father was a postal worker for the U.S. Post Office. My mother had worked as an accountant during much of my growing-up years. We basically lived in two houses in the same suburb of Denver. So it was—

COHEN: A stable childhood?

WHITCOMB: A very stable childhood. Englewood is a small suburb. It has its own school system—a small one—ultimately ending up with a single high school, and several elementary schools feeding that. I attended the public schools there. My parents were very encouraging of education, particularly my mother. Part of that comes, I think, from her own growing up in the Depression.

COHEN: And not having the education she wanted?

WHITCOMB: Right. She attended high school and one year of college but was unable to afford to go farther and, I think, always felt that that was terrible. So she was very strongly in favor of education. I would say that my school system was not a particularly good one. It was in a kind of working-, middle-class suburb. Less than half the people in my graduating class, for example, went to college.

COHEN: You must have been quite a good student yourself.

WHITCOMB: Yes. In fact, I had decided at a rather early age that I wanted to be a scientist. In fact, I wanted to be a physicist.

COHEN: What made you decide that? Any particular incident, or just in general?

WHITCOMB: Just in general. I was a voracious reader and read all kinds of things. Somehow, well before I was in high school—while I was still in elementary school—I decided that I was going to grow up and be a physicist. I toyed briefly with mathematics and decided, ultimately, that I wanted to be a physicist. So I actually studied quite hard. I was a very good student. I was valedictorian in my high school class. When it came time to apply to colleges, I applied to Caltech.

COHEN: Was there any reason to apply to Caltech? Or did you just know that that's where physicists went?

WHITCOMB: I knew it had a very good reputation—but not much more than that, actually.

COHEN: So there was no high school teacher who said to you, "Go to Caltech."

WHITCOMB: No. In fact, at my high school it was actually rather rare for someone to go to a college outside Colorado. There would be maybe only a half dozen each year who would go to schools outside Colorado, and most of those were in Kansas, Oklahoma, and Nebraska.

COHEN: Had you seen Caltech when you applied to it?

WHITCOMB: I had decided in my junior year that I was interested in it. So my parents took us on a family vacation and came to Southern California. We drove out during the summer between my junior and senior year and visited the campus here. It seemed a nice sort of place. My parents approved of the look of the campus. And we had a very brief meeting with an admissions officer who was not particularly encouraging and said, "It's a very tough school to get into."

COHEN: You didn't meet any luminaries at that time?

WHITCOMB: No, no. Not at all. And I didn't know what that really meant. I actually applied only to two colleges: Caltech and one in Colorado. Fortunately, I was admitted to Caltech.

I arrived here in the fall of 1969, which was in fact the last all-male freshman class on campus. They admitted women into the freshman class the following year. So I was in that transition period that Caltech made—not an easy transition, actually, all the way around.

So, again, I continued to want to be in physics. That was a time when everybody coming to Caltech wanted to do physics. If you polled the incoming freshman class, 40 percent of them were physics majors. That didn't last until they graduated, but it was by far the most popular major.

COHEN: Whom did you have as teachers for freshman physics?

WHITCOMB: At that time the lectures were done by Gerry Neugebauer [Millikan Professor of Physics, emeritus, d. 2014] and Gordon Garmire. Gerry Neugebauer was my recitation instructor.

COHEN: Oh, so you got to know Gerry early on?

WHITCOMB: Right. And then, when I became a real physics major in the second year, Robbie [Rochus E.] Vogt [R. Stanton Avery Distinguished Service Professor and professor of physics, emeritus; Caltech provost, 1983–1987] was, it turns out, my faculty advisor.

COHEN: And that went fine?

WHITCOMB: Yes.

COHEN: You enjoyed that?

WHITCOMB: I had a very good relationship with Robbie. He was lecturing the Physics II class that year. I subsequently I had him for additional classes as a junior and senior, and I did a senior thesis working in his research group. So he and I had a very good relationship, which has continued over the years. Robbie's name will come up frequently in talking about LIGO [Laser Interferometer Gravitational-Wave Observatory] and my involvement here. He strongly encouraged me to consider the University of Chicago for graduate school. He also encouraged me to apply for a Churchill Fellowship to spend a year at Cambridge. And I did both of those things. I applied for the Churchill Fellowship and also applied to the University of Chicago. I took the Churchill Fellowship and spent a year in Cambridge, where I was one of the "visiting Americans who don't work too hard, because they're not there for a degree—they're not there for anything other than the largely cultural experience." I had involvement in a lab, and I had a research project that was probably the equivalent of a senior thesis.

COHEN: And that was a one-year thing?

WHITCOMB: Yes. I determined beforehand that it would be one year. It was going to be a culturally broadening experience. I would learn a little bit more physics, but I didn't want to stay there for a degree.

COHEN: Did you work with anybody specifically?

WHITCOMB: Yes, I did. I worked in the low-temperature physics group there. [Sir Alfred] Brian Pippard was, in a sense, the leader of the group.

COHEN: Was this was your first experience in leaving this country?

WHITCOMB: Almost my first experience. The year I graduated from high school, I was selected to attend a summer science school in Australia. Well, it's actually a winter science school for them, but it was summer for us. It was in July and August. As a result, I spent a couple of weeks in Australia and also made some other stops along the way.

COHEN: Was that Harry Messel's school [the Professor Harry Messel International Science School]?

WHITCOMB: Yes, it was. At that time, they chose five British students, ten American students, and five Japanese students. They all attended the Messel School at the University of Sydney and then traveled and made some stops. So that was, really, the first time I was out of the country. Cambridge was the second.

COHEN: At that time, had you already been accepted at the University of Chicago?

WHITCOMB: Yes, I had. Chicago had agreed to hold my spot in the class for one year. In fact, that was one of the main reasons I went there—that they were willing to do that. The other places I had applied to were less willing to do that—in fact, unwilling to do it.

COHEN: So, then you came back after this year in Cambridge and went to the University of Chicago?

WHITCOMB: Right. In 1974.

COHEN: And whom did you work with there?

WHITCOMB: I worked with two professors. My thesis advisor, and the person with whom I worked primarily, was Roger Hildebrand. In the first couple of years, I also worked a little bit with Valentine Telegdi. Telegdi was just getting started in a new experiment that involved optics and lasers. He was thinking about measuring parity violation using lasers and atomic spectroscopy, and he didn't know much about lasers and atomic spectroscopy, so I worked a little bit with him, trying to decide if that was the direction I wanted to go with my life. In the end, I decided not. I worked with Roger Hildebrand. Roger had been a high-energy physicist who had gotten involved in university administration. He had been the dean of the college at the University of Chicago, which is effectively a full-time job and took him completely away from research. He was the dean for five years, I think. When he got out of being a dean, he discovered that high-energy physics had changed so much that it wasn't fun and interesting for him anymore. So he was looking for a new area and picked infrared astronomy. He had almost no money and almost no support to do that—just a little bit of funding from the University of Chicago. So he was looking for free graduate students. I had a fellowship, so I was prime pickings. It turned out to be a great thing for me. Roger was a very, very honest and wonderful man. There are few people as unselfish whom I have met in my life—and as honest and straightforward and helpful. I could tell you all kinds of stories about Roger, but that's not really relevant for Caltech's oral history project.

COHEN: Right. Although, who knows? But at least we see, so far, that life has been smooth for you.

WHITCOMB: Yes. Very smooth. I had a good time. I was working in Roger's group. We were building infrared photometers, just getting started, just getting funding from the NSF [National Science Foundation]. We had enough results to start to get funding and to start to get telescope time. So it was a very exciting time. He and I were learning the same material at the same time. Roger was meticulous in teaching his students to write papers. We spent endless hours editing papers and manuscripts for submission. It was a very, very good experience. It was a long time I was there—until 1980.

COHEN: Did he work on your speaking, because you speak very well.

WHITCOMB: Again, it's something that probably rubbed off a little bit from Roger. Of all his graduate students, I was the one who took the longest to get a degree— [laughter] embarrassingly enough for both of us.

COHEN: Well, maybe you got more things done. So, never mind.

WHITCOMB: Well, it was partly because we were breaking into a new field and he didn't know quite how to—what to do.

COHEN: When you say long, how long were you there?

WHITCOMB: Until 1980. So it was six years, which isn't outrageously long, but Roger felt that it was very bad. He had always prided himself on getting degrees for his students in a relatively short period of time—it always took them about five years. He felt that was the right length of time for a PhD student. When I dragged on and on and on, he was concerned in a good-natured way. But it was a good experience. I wasn't in any particular hurry to get out. I was living comfortably. I met my wife, Laurie; we were married.

COHEN: So it was OK?

WHITCOMB: Life was OK.

COHEN: So then you finished. And then did you go directly to Caltech?

WHITCOMB: I did my thesis defense, and then I had a one-year postdoctoral position, which was funded by the NRC [National Research Council]. It was a National Needs Postdoctoral Fellow, which is really, I think, supposed to be an applied kind of fellowship. I was doing infrared astronomy and development of detectors. I think it stretched the intent of the fellowship a bit. But I had it, and so I—

COHEN: You could go where you wanted?

WHITCOMB: Well, I was at the University of Chicago. In the application for the fellowship, I had to say where I would go and what I would do. But it gave me some time to look around at other options. About that time, Robbie Vogt came to Chicago and gave a talk on the cosmic ray work he had recently done on the *Voyager* spacecraft. It turns out, by coincidence, that one of the detectors on the cosmic ray instrument on *Voyager* that Robbie and Ed [Edward C.] Stone

[Morrisroe Professor of Physics] had been principals on, was a detector that I had done the initial conceptual design work on, for my senior thesis.

COHEN: And Robbie was aware of that?

WHITCOMB: Oh, yes. Robbie had supervised my senior thesis. I found he was coming to give a talk on this, and so I went to his talk and invited him to come to my home for dinner and meet my wife, and we'd just sit and talk. Robbie came. He was just about to take the position of chairman of the Division of Physics, Mathematics, and Astronomy. And he told me about a new adventure, or endeavor, that Caltech was getting into: gravitational waves. He told me a little bit about the challenge, the physics behind it, the precision measurements, and so on. And he told me that they would be looking for a junior faculty member. They had already identified Ron [Ronald W. P.] Drever [professor of physics, emeritus, d. 2017] as the senior faculty member; but they were going to be looking for a junior faculty member, and he encouraged me to apply for that. And I did. They interviewed me and eventually offered me the position. So I cut short my fellowship at the University of Chicago. The National Needs Fellowship had a one-year term. I cut it short at about seven months and came out to Caltech in the fall of 1980.

COHEN: As an assistant professor?

WHITCOMB: Right.

COHEN: So you were coming back home, in some sense. This was a familiar place for you and you were also with people you knew.

WHITCOMB: Right. Of course, in detail, the gravitational-wave research group was completely new. Drever hadn't been at Caltech when I was here before. At the time I came back, he was on a half-time position here at Caltech, retaining a half-time position at the University of Glasgow. He was splitting his time back and forth—

COHEN: That was a special arrangement that was made for five years—

WHITCOMB: Right. And that started, I guess, really at about the beginning of 1979.

COHEN: Now, was that your first introduction to gravity-wave research?

WHITCOMB: Yes. Completely. On the other hand, I was interested in astrophysics, obviously, and I was very interested in the application of gravitational waves to astrophysical sources and what you could learn from them. So my interests were a little different from those of most people who were in that field at the time. Most of them were physicists whose specialties really were precision measurements. There were a lot of theorists who were very interested in what the gravitational waves could tell you about the astronomical sources, but very few of the experimentalists were interested. And so one of the things I tried to do when I was there during the early eighties was to keep the group interested in the astrophysics, so we'd develop some ties to the astronomy group here on campus.

COHEN: Who else was part of the group when you came, or who joined shortly thereafter? Who would that have been?

WHITCOMB: When I arrived, there was Ron Drever, of course. There was Siu Au Lee, who joined Ron as one of his first hires. Siu Au had been a postdoc working with John Hall at Colorado, at JILA [The Joint Institute for Laboratory Astrophysics], and had come to Caltech as a senior research fellow. We also had Bob [Robert E.] Spero.

COHEN: Now, these were postdocs or research fellows? What were their jobs?

WHITCOMB: Siu Au Lee was a senior research fellow. Bob [Robert] Spero [principal engineer, Caltech/JPL] was a postdoc research fellow. Bob had just gotten his degree at UC Irvine, and he arrived in January of 1980. Siu Au Lee arrived sometime after that, in the spring of 1980.

COHEN: So, in a sense, the three of you came almost together?

WHITCOMB: Right. I came in the fall, in September. We also had a graduate student, Mark Hereld, and another graduate student—

COHEN: Now, would they have been students of Ron's?

WHITCOMB: Right. They were Ron's students. So it was a small group. There were five of us.

COHEN: Mark Hereld and, what was the other name?

WHITCOMB: I'm blanking on it, because he left us after about a year. He was working in the group there but was really trying to decide whether this was what he wanted to spend his time on. And then he left. Mark Hereld, actually, was the first PhD student from Caltech who stayed with the project. At that time, the group was occupying most of the third floor in West Bridge [Norman Bridge Laboratory of Physics]. They converted some rooms at the end into laboratories and offices along the hallway, and plans were being put together for the 40-meter [interferometer] lab around the CES [Central Engineering Services] building. One of the things they brought me in for was this arrangement, I think, with Ron being gone half of the time. They felt that this left the group without a clear spokesman. Ron tended to not delegate very readily—one of his character flaws. We all have them. And there was a feeling that the group was unable to make decisions when Ron wasn't around. In particular, the building people—the people who are in Physical Plant who are designing this laboratory—felt that they—

COHEN: Nobody was telling them what to do?

WHITCOMB: Right. Ron would be here, and they would get some instructions. Then suddenly he would disappear, and they would get nothing for two months. So one of the things I did the first couple of years was to work quite closely with Physical Plant to finalize the plans for the laboratory.

COHEN: Did you have much to do with the MIT people at this time? And how did Rai [Rainer] Weiss come into the picture? Or maybe he wasn't in the picture this early?

WHITCOMB: Actually, in the first couple of years—'80, '81, '82—the connection to MIT was very weak.

COHEN: So that was not something that one was concerned with at this time?

WHITCOMB: Right. Rai Weiss had, in the early seventies, basically independently of several other people in the world, invented the notion of using laser interferometers for measuring gravitational waves. He was teaching a class on relativity, and in that class he basically came up with this idea. As a result of that, he worked out the identification of many of the fundamental noise sources. Many of the very fundamental ideas go back to that. He wrote them up in an internal MIT journal [R. Weiss, "Electromagnetically Coupled Broadband Gravitational Antenna," *Quarterly Progress Report, Research Laboratory of Electronics, MIT* 105: 54 (1972)]. It's a paper that's almost impossible to get today, but it's very fundamental in terms of identification of noise source.

COHEN: So these laser ideas were really coming from there?

WHITCOMB: At that time, Rai Weiss wrote a proposal to the NSF and asked for funds to start to build a detector like that, or at least to start to develop the technology. I don't have direct knowledge, but what I've heard is that the NSF sent that proposal out to be reviewed. They sent it to, among other places, a group in Germany that was building bar detectors. They saw it, liked the idea, and decided that that was not such a bad thing to work on. They were able to quickly get some money and, in the end, the NSF was unable to immediately fund Rai's work—

COHEN: But the German work went ahead?

WHITCOMB: But the German work started and, in fact, was really a separate start to that effort. There was another group run by one of Joseph Weber's students, Bob [Robert L.] Forward, who independently had invented the laser detector. He was working at Hughes [Research Laboratories] here in Southern California and actually built the first interferometer. He did some experiments and then Hughes decided it wasn't interesting for a corporation, and Bob Forward got out of that business.

COHEN: It's always interesting where ideas come from. Who knows?

WHITCOMB: Yes, there were many independent inventions of about the same idea.

COHEN: When the time has come, they're there.

WHITCOMB: Right. So, anyway, Rai Weiss had not succeeded in getting money. And then these efforts started, first in Germany. And then Ron Drever, I believe, picked up that idea from Germany and invented several new improvements and started a group in Glasgow. Then Caltech decided that they wanted to get involved in this. [Feynman Professor of Physics] Kip [Thorne] would have much more detail about this—he was actually the person who sold the Caltech Physics, Math, and Astronomy faculty on the idea.

COHEN: Oh, yes. There's no question about that. Kip was the primary mover—*the* mover.

WHITCOMB: Yes. He helped identify Drever as the person to bring to Caltech to lead this. Caltech was willing to invest significant money, and that convinced the NSF to produce a grant for Caltech. The MIT group was then able to leverage the fact that Caltech was interested, and Rai Weiss submitted a proposal that was funded. But it was really a separate effort. The two groups were probably more competitive than cooperative in those early days.

COHEN: Now, were you already here when all that was going on?

WHITCOMB: The proposals were submitted to the NSF and both groups had funding before I came here.

COHEN: So when you came, this was in place—the two groups working?

WHITCOMB: Two groups working separately, pursuing somewhat different technologies. And then, in something like 1982—late '82 or the beginning of '83—there started to be a lot of pressure to develop plans for larger detectors. It had always been recognized from the beginning that these laboratory-scale devices weren't going to be sensitive enough to detect gravitational waves—that they were really test beds for building larger things. The MIT group may have felt themselves, I think, falling behind in terms of the laboratory work, so in a sense they turned more toward doing studies of these larger detectors as an activity that they could be in the lead on. The experimental work at MIT suffered enormously, because MIT the institution had no interest in gravitational waves. In fact, the MIT administration actively discouraged Rai Weiss from doing this. They only tolerated it because he was doing other kinds of research that they found interesting—microwave background things.

Anyway the MIT group began to do some trade studies on the costing and the design trades that would be involved in a large study. They engaged an engineering firm—Stone and Webster, which is local to Boston—to do detailed cost estimates and to start that activity. When Ron found out that MIT was doing that, he thought we needed to do similar sorts of things so that we wouldn't be at the mercy of MIT in regards to the larger detectors. And we, in fact, engaged JPL [the Jet Propulsion Laboratory] to help with that. It was probably a good move, but it was a reaction to what MIT was doing. The two groups were also receiving some significant pressure from the NSF to write a combined proposal to build a large gravitational-wave detector.

COHEN: The NSF was not going to support two?

WHITCOMB: I think we had indications that they would not support two large independent efforts.

COHEN: Was that clear to everybody right from the beginning?

WHITCOMB: I don't know that it was clear from the beginning. Certainly it was starting to be made very clear by late '82 or early '83. The NSF said they wouldn't accept proposals for engineering studies of large facilities unless they were joint proposals. And, in fact, there was a later stage when they'd really force the two institutions to collaborate. It was not an easy collaboration. It wasn't one that naturally evolved because the two groups recognized value in each other and wanted to collaborate.

COHEN: This was a fact of life?

WHITCOMB: Right. In particular, Ron was very resistant toward real collaboration. Rai was significantly less so. I actually thought it would be a great strengthening of the groups and that it would be beneficial. But Ron felt that he would lose independence and control and that, in the end, poorer decisions would be made by this larger collaboration than would be made by the Caltech group itself. And that led to some friction between him and me.

COHEN: You thought it was a good idea?

WHITCOMB: I thought it was a good idea. Neither group was really strong enough to carry off something like this project by themselves.

COHEN: Were you the only one here who felt that was a good idea? Or were you the only one in a position of authority who could say something? How about Kip? Did he have any say in this, or did he care?

WHITCOMB: Kip was actually very engaged. I think Kip also was trying to make a collaboration that worked. But Kip was very reluctant to make anybody unhappy. The organizational structure that eventually emerged when Caltech and MIT submitted a joint proposal for some engineering studies of large facilities was, essentially, a steering committee that consisted of Ron Drever, Rai Weiss, and Kip Thorne. And they would be the directors of this joint engineering study. It was doomed from the beginning. It would be difficult to find two physicists with styles as different as Ron's and Rai's. Rai is a very encyclopedic physicist. He knows something about everything in physics. He's very, very good in that regard. And he's very mathematical. He sits down and, if he has an idea, he'll work it out. He'll give you a solution that's got first- and second- and third-order corrections, oftentimes a solution that's much more precise than the initial conditions you have to put into the solutions. You might have a factor-of-ten uncertainty about what the initial conditions are, but he'll have a solution that's good to a tenth of a percent accuracy. So that's his style. He likes to see everything written down and worked through in very precise mathematical terms.

Ron, on the other hand, is a very intuitive physicist. He's actually, I think, not very good at doing the detailed mathematics. On the other hand, he's very clever at having little pictures in his head that outline how an experiment ought to go or how a situation ought to behave. And oftentimes those little pictures may get to the right answer faster than the more detailed calculations, because they can show the imperfections, perhaps, in a system that a more detailed mathematical approach might miss. The more complex the mathematics, the more approximations you make, and sometimes you lose the fidelity. So what would happen is that Ron and Rai would get together, and Rai would have this very detailed mathematical analysis of something that Ron couldn't, frankly, work through. And Ron would have this picture in his head of some geometrical relations that Rai would have a hard time comprehending and about which he would say, "Well, I have to go think about this. I have to go work it all out in detail." The two could never communicate. They would take almost opposing views on every topic. It was almost a matter of principle between them.

COHEN: And Kip was there in the middle trying to make peace?

WHITCOMB: Any time a decision had to be made, you'd have Ron taking one position, Rai taking another position, and Kip as the third party having in essence the deciding vote on everything. Kip recognized that he wasn't an experimental physicist and, as a result, he typically would not want to make those decisions, so not much got accomplished. I remember there would be visits. Rai would come out from MIT and the three of them would be closeted for two solid days and come out with no decision about a particular topic. It was horrendous, and very wearing on Kip during those times. And the joint project was really quite stalled in terms of any kind of action. These disagreements between Caltech and MIT were actually starting to eat into the time that other scientists could spend working in the laboratory. In fact, the group here had been fairly effective. We'd gotten the 40-meter laboratory built, we'd built up an interferometer, we got it operating. There was actually good work going on here. And then when we started this collaboration with MIT with this very high-tension sort of situation, it was distracting. Ron wanted to discuss these engineering issues and, in a sense, reinforce his position with the Caltech people endlessly. And so there would be long half-day discussions, and people were just unable to get things done in the laboratory.

COHEN: So he was just trying to reinforce his own ideas?

WHITCOMB: Right.

COHEN: That he had to, then, argue with Rai?

WHITCOMB: Exactly. And the same thing was happening at MIT. At that time Rai had, really, two senior people with whom he was working: Paul Linsay, who was the equivalent of a senior research fellow here at Caltech, and Peter Saulson, who was the equivalent of a postdoc and had just gotten his degree from Princeton. And those were the two people who would otherwise have been working in the interferometer lab at MIT, but they were—

COHEN: Having to listen to all this?

WHITCOMB: They were having to listen to Rai at the other end of this. The competitiveness between the two groups was just terrible at the time. I got quite discouraged and in 1984 I actually started to look for other things.

COHEN: You mean the atmosphere here was just so awful?

WHITCOMB: It was so awful. Ron was micromanaging the group. I was pretty well convinced that I would have nothing independent to show for a tenure review and that there would be no way to have something that was identifiably my contribution. Plus I wasn't getting any publications out. I was also pretty well convinced that, if things continued the way they were going, the project was going to fail—I mean, we'd just be unable to make decisions. I was very naïve about what it would take to do a big project. I probably underestimated by a factor of ten what it would take to actually carry out and build a big interferometer. So I was getting very discouraged. I actually applied in 1984 for some other positions. I had offers from a couple of places, including an offer to go back to the University of Chicago as an assistant professor. And Kip and Ed [Edward C.] Stone, who was PMA division chairman at that time—Robbie having taken over as provost by then—sat down with me. We talked through what my concerns were, and they said, "We'll help you identify something which is your piece. We'll help you in your relationship with Ron. We think all this stuff is going to get better. We'd like you to stay." In addition, my wife wasn't all that eager to move out of Southern California. She was starting to feel more comfortable. She hadn't been really wild about moving here, but by this time she was feeling reasonably comfortable and wasn't sure she wanted to pick up and move back to

Chicago. So I said, "OK, let's give it another try. This is still the most interesting physics around." And I turned down the position at the University of Chicago and stayed here. That was the fall of '84.

COHEN: Now, the Institute wasn't involved at this point? There was no public outcry as far as the Institute went? I mean, were the arguments all internal?

WHITCOMB: Largely. I mean Ron would go to Ed Stone after every meeting with MIT and say how terribly the people from MIT had behaved at this meeting and how it didn't make any sense whatsoever. I think he actually probably got some support from Ed.

COHEN: Was Robbie involved while he was provost?

WHITCOMB: He was in the chain of command, but in fact, I think, relatively far removed from any day-to-day interactions. And I think Ed took the point of view that "here's one of my faculty members coming to tell me how badly MIT is behaving, and I'm sure MIT is behaving very badly about this." So I think Ron got support for his positions from Ed at that time.

COHEN: So you stayed, but now you're going to tell me it didn't get better?

WHITCOMB: It didn't get better. In fact, if anything it got worse. Ed had promised me that he would work to identify something that would be my piece of the project. And I made a couple of proposals, and Ron basically vetoed everything I proposed, and I really didn't get much help from Ed. Ed viewed being division chairman as a half-time job. He worked extremely hard at it, but come one o'clock, he was out of his office and done for the day.

COHEN: What else did he do?

WHITCOMB: He was still very active in his cosmic ray research. Robbie had been very different when he was division chairman. He said that the division chairmanship is a full-time job and basically completely abandoned his research effort. Ed chose not to be a full-time division chairman and instead was a half-time one. Ed is very disciplined. He worked very hard. But if there was a problem that wasn't solved… [Tape Ends]

Begin Tape 1, Side 2

COHEN: Go ahead.

WHITCOMB: Ed decided every morning how late he would stay in the division chairman's office, and if there was a problem that wasn't solved when that time came, well, it carried over to the next day. So I felt I was getting limited support from Ed in my situation. Kip was completely wrapped up in dealing with Ron and Rai and trying to mediate between them. He didn't want to make either one of them unhappy. And so, in frustration I just left—I felt there was nothing I could do. It looked like a disaster for me if I stayed at Caltech.

COHEN: Where did you go, at this point?

WHITCOMB: Having really worked very hard for five years as an assistant professor at a very good research institute, I decided to try something completely different. I went to work at Northrop Corporation in their Electronics Division.

COHEN: So you wanted to be as far away from this kind of stuff as possible?

WHITCOMB: Yes. I decided, "Let's try something different, other than the academic life." So I went to this project. It was applied research, in a sense. It was very different from gravitational waves, but it used some of the same technology—lasers and optics and so on. The problems were sometimes different. They involved more personnel problems, and so on.

COHEN: And, of course, you didn't have to move then?

WHITCOMB: We didn't have to move. The job was down in Palos Verdes, so I was commuting a long distance.

COHEN: Were you living in Pasadena?

WHITCOMB: We lived in Pasadena. My wife was working here in Pasadena. So we stayed. And I enjoyed the new job. It was fun. It was a different set of challenges, a different set of tools that you use—

COHEN: Easier people, I would guess?

WHITCOMB: Different kinds of people. And the people problems were different. Companies make decisions very quickly and rapidly and move on.

COHEN: You have to meet the bottom line.

WHITCOMB: Right. In terms of their technical skills, the people in the aerospace industry aren't anywhere near as good as the average person at a place like Caltech. And that was a bit of a shock to me. Some of the best people in industry are as good as the better ones at a place like Caltech, but the average is much lower. So the problems are different. The focus is on how to get the most out of the people who are working with you and for you. I started off as a research engineer with minimal management responsibilities. I was just kind of, in a sense, almost a scientific guru. Within a couple of years, I was the project manager of the project I was working on. It was about a $20 million project. I had somewhere between thirty and fifty people working for me. Suddenly I was learning how companies do big projects: how you have to do the accounting; how you have to do the management and the reporting. These were skills that we would need to learn in LIGO but didn't even know that we needed to learn. And it was really in that environment that I recognized how naïve we had been in our early planning of the gravitational-wave project. So I got some very valuable training in industry.

COHEN: During this time, did you in any way keep an eye on what was happening here, or did you really just want to be separated? I mean, were you aware of what was going on here at Caltech?

WHITCOMB: Not in detail. I kept in touch with Mike [Michael E.] Zucker, who was a graduate student who had come in about 1983. At about the time Mark Hereld got his degree, Mike Zucker showed up. He was a fun guy. He and his wife got along with me and my wife. So we kept in touch on a social basis. Every six months, we'd get together for dinner or something. So I knew a little bit about what was going on, but not in detail. I don't think I ever came back and visited the lab, for example.

COHEN: Or came to seminars or anything like that?

WHITCOMB: Right. I had a full-time job. It's not easy, if you've got people depending on you, to pick up and go to a seminar offsite.

COHEN: So it was actually a good experience there? You enjoyed being there?

WHITCOMB: Yes, I did. It was fun. The challenges were different. The technical challenges were perhaps not as great as the engineering challenges.

COHEN: I would guess the remunerative part was good, too.

WHITCOMB: You mean, I got paid more? Actually, I did.

COHEN: Well, of course.

WHITCOMB: Although maybe not quite as much as you might have expected. But that's fine. It was working fine, and I was having fun. Unfortunately, after about four years the project I was working on got canceled. I started casting around for another position, either in the company doing something else—I could have stayed at Northrop—but also deciding I would look around to just see if there was something else a bit more fun. And at that time I got an offer from Loral Electro-Optic Systems. A person who had been at Northrop before taking over as the general manager at Loral here in Pasadena contacted me. He had heard that the project I was working on was going under. So he said he'd like me to come in and interview with them. They were doing some fun things and it would be a much-reduced commute, and that was attractive. So I started to work at Loral. That was in 1989.

COHEN: And meanwhile things were probably coming to a head here.

WHITCOMB: Well, actually, things had changed radically here at Caltech. I think that my leaving may have focused a little bit more attention on the fact that maybe there were some problems in the group here. I don't think that the administration had recognized the severity of the problems here. In fact, the NSF recognized, within a year or two after I left, that the management structure of this three-person committee that couldn't make any decisions was a disaster. And they basically went to the Caltech and MIT administrations—I wasn't involved directly, so this is just hearsay, but probably reasonably reliable—and said, "We have to make a change. You have to put a single principal investigator in charge of the project. This management troika isn't working." I think Rai and Ron each thought that he would be that principal investigator. MIT, which still at this time had no interest in LIGO whatsoever, wasn't really interested in putting somebody forth as a principal investigator. However, Caltech was looking around. They were interested in finding somebody who would meet those requirements. I think they agreed that Ron was not the right person to lead a big construction project. Tom [Thomas A.] Tombrello [Goddard Professor of Physics, d. 2014] was the chairman of the committee that was charged with finding a principal investigator for LIGO. Tom tells me that they had decided that the best person for that job was Robbie Vogt.

COHEN: Now, at this time, he no longer was provost?

WHITCOMB: Well, Tom claims that they had identified him as the right person while he was still provost. They just didn't quite know how to approach him and ask if he would take the job. They were sort of sitting on this as their first choice but not knowing how to act on it when Robbie was—

COHEN: When things blew up there—

WHITCOMB: Then things blew up, and Robbie was fired as provost. And so, within a week or two after that—or, at least, within a short period of time—they approached Robbie about LIGO.

COHEN: But according to how you heard it, they really wanted Robbie?

WHITCOMB: That's what Tom has told me. You can ask him yourself. You may or may not find out any more of the truth than I did. But that's what he told me.

COHEN: And also, people remember things in different ways.

WHITCOMB: Of course.

COHEN: So then, this all was happening while you were at Loral?

WHITCOMB: Yes, while I was at Northrop and Loral. I knew from Mike Zucker that Robbie had come in as principal investigator. There were probably some expectations there. Rai Weiss, I think, was very suspicious of Robbie in the beginning, for obvious reasons. Robbie was new to the project and from Caltech, and MIT had been unwilling to support Rai in trying to find somebody at MIT who could take that role. The Caltech and MIT groups had evolved rather different concepts of what would be the right detector, and Rai was concerned that Robbie would immediately adopt all of the Caltech ideas and that Rai's ideas would not get a fair hearing. I think Ron thought that Robbie would be more of an administrator than a manager—someone who would take care of the books and manage the money and so on, but would largely defer to Ron on technical issues. So I think there were a lot of expectations in that. From everything I have heard, those first couple of years with Robbie were a very tense time. First of all, both the Caltech and MIT groups were out of money. They needed to write a joint proposal for the next phase. The NSF was pushing them to write a proposal that would fund a preliminary design and lead to a construction proposal. So they wrote a joint proposal, and they were funded. Robbie, I think, pulled that together rather well. Indeed, Rai was right. Robbie adopted a majority of the designs favored by the Caltech group, possibly because they were Caltech ideas but I think more likely simply because the 40-meter was a more successful instrument than the MIT equivalent instrument. The ideas behind it were a little more mature. The group out at Caltech was larger and had gotten more done in their prototype work than the group at MIT. I think that even Rai would now say that it was necessary that Robbie made the decisions that he did. But I suspect that it was quite discouraging to the group at MIT. They lost some of their people. Some people who were working in that MIT group felt they were second-class citizens and had been shunted aside.

COHEN: They didn't want to work on it?

WHITCOMB: They didn't want to work on it. They left. Paul Linsay, for example, left. It was a fairly turbulent time. But the two groups got the joint grant, and they started writing a construction proposal. It was, again, a very tough time. They were having to commit to making certain decisions about the design, and they were very hard decisions.

COHEN: But you were just watching this from the outside? I mean, you were not involved in any of this?

WHITCOMB: I wasn't involved. I was completely on the outside.

COHEN: So tell me, when did you come back into the project? Or when did you come back to Caltech? How did that happen?

WHITCOMB: It was, oh, about mid-1990, in fact. I got a rather funny phone call from Jocelyn Keene, Tom [Thomas G.] Phillips' [MacArthur Professor of Physics, emeritus] wife. Jocelyn and I had been graduate students together at the University of Chicago, and we were good friends. Jocelyn called me, and was clearly very embarrassed by this, and asked whether I would hang up on Robbie immediately if he called or would I be willing to talk to him. I said that of course I would be willing to talk to Robbie. Nothing happened for a couple of months. And then Robbie did call me.

COHEN: That's a very peculiar phone call—from Jocelyn.

WHITCOMB: Yes, it was. She had been asked to do this, and she was really very embarrassed about it and very uncomfortable with the situation. So then Robbie called me and invited me up to his house for a beer and a chat. And he told me about LIGO and talked to me about what was in the proposal, and he gave me a copy of it. He was looking, at that point, for a deputy director and he asked if I might be at all interested in that. He wanted to know a little bit more about my situation, and so we talked. We had a couple of conversations in that general vein, with me going up to his place and having a beer and talking—mostly he would talk and I would listen.

Every once in a while I would nod. If you've ever talked with Robbie, you'll know that. Toward the end of 1990, it was clear that all this was going to materialize into an offer from Caltech to come back.

COHEN: Now, was Ron Drever out of the project by this time?

WHITCOMB: No, no. He was still in the project and very active. It was clear that there were difficulties between Ron and Robbie. But there had been difficulties between Ron and everybody who had worked with him. He'd had difficulties with me. He'd had difficulties with Siu Au Lee and, ultimately, more or less let her go under not very happy terms. Ron's had difficulties with most of the people he's worked with; there are very few that would be considered very happy relationships. And so it was not surprising that Robbie and Ron, both being strong personalities, would have difficulties. Robbie viewed Ron as technically a good resource and managerially as a disaster. You would never put him in charge of anything. But as a guru, he was quite valuable. That was the role Ron was fulfilling more and more. In a sense, he was being forced into it. It was probably a more appropriate use of his talent than putting him in charge of something.

COHEN: So he already was in this position, or at least being pushed into this position?

WHITCOMB: Right.

COHEN: So you came back?

WHITCOMB: I came back. My formal offer came from Caltech in the beginning of February of '91 and I was back by mid-March or so.

COHEN: You were the deputy director of the project under Robbie—or with Robbie—whichever preposition we may use?

WHITCOMB: Yes. It took a little bit of time for me to readapt to some of the changes, meet the people, and get a feeling about what the engineering state of things was. The physics was still the same, and I was able to get back on top of that pretty quickly. Back in December 1989, the group had submitted a construction proposal to the NSF for $200 million. That had been peer-reviewed by the NSF and had gotten high marks. The NSF had given, I guess, a little bit of money toward that. But the LIGO budget was cut from the NSF budget by Congress in 1990. It was put in again the following year—the initial construction funds. And in the fall of '91 there were congressional hearings on the NSF budget.

COHEN: This would be a good place for us to stop.

WHITCOMB: OK.

STANLEY E. WHITCOMB

SESSION 2

March 14, 1997

Begin Tape 2, Side 1

COHEN: It's nice to see you again. I think we were going to discuss your starting to work on the LIGO project the second time around.

WHITCOMB: The second time. I'd actually, if possible, like to kind of change things a bit. First of all, after reflecting a bit after our last discussion, I felt a little bit that it had gone into a discussion more of the politics and personalities, and I felt that we were, in a sense, missing some of the technical underpinnings that in fact actually have driven a lot of the later things. And secondly, after a great deal of reflection, I'm not sure I feel comfortable talking about the things since my return to LIGO the second time. At this time, I don't feel real comfortable talking about those things.

COHEN: OK.

WHITCOMB: The time may come, in three or four years, when circumstances will be a little different and I'll feel better talking about that. But, at the moment, I don't feel it would be wise for me to talk about that.

COHEN: You mean the politics since you came back to LIGO to the present time?

WHITCOMB: Exactly.

COHEN: OK. Can you talk about the science that's gone on in that time?

WHITCOMB: I could talk about the science, probably. We talked last time, I think, a lot about the politics of what was going on at Caltech and MIT and between the institutions. And I'd like to at least talk a little bit about the science and the technology development that was going on at both places. At the time I first came on, in 1980, there were really two major ideas—big ideas—about how you would build large gravitational-wave detectors. The original idea, and the one that was being pursued by the group in Germany at the time, and which was outlined at least in part in Rai Weiss' early work, was to build what we call a simple Michelson interferometer. The concept behind that is that you bounce the light back and forth between mirrors with well-defined spots on the two arms, bring it back to a beam splitter, and recombine the light. And that has certain technical advantages. It has simplicity, which is nice. It requires somewhat larger mirrors, because you are bouncing the light back and forth perhaps a hundred times. So you have to have a mirror large enough to accommodate a hundred spots.

There are a couple of different ways to do that. The German group had pioneered a way that used a particular type of delay line called a Herriott delay line, in which the spots form a circle on the mirror. Drever, in his early work in Glasgow, had done the same sort of thing using something called a White cell delay line, which involved separate mirrors operating in a rectangular pattern. It was kind of a clumsy thing, in the sense that it required aligning a hundred different mirrors relative to each other.

But as a result of that earlier work, Ron had come up with an idea that was in fact was quite innovative. And that was to bounce the light back and forth in the two arms but to overlap the spots, effectively forming the arms into Fabry-Perot cavities. That makes for much more compact systems, in the sense that you just have to have small mirrors. But it's a slightly more complex system in terms of maintaining the resonances and so on. And the group in Glasgow had started to work along those lines.

When I arrived here in 1980, because the group here was really an outgrowth of Ron's group in Glasgow, it was already decided that we would use this Fabry-Perot technology. Rai Weiss at MIT didn't feel as comfortable with the Fabry-Perot technology. He had received funding from the NSF at about the same time as the group here at Caltech and had decided to pursue a Michelson delay-line approach. So he had put together a group that was focusing on that: building a 1.5-meter interferometer. Here at Caltech we had both Institute and NSF funding, enough to build a 40-meter lab and a 40-meter interferometer. There was some Caltech money for the startup and NSF money for the actual experimentation—actually quite a bit more money than Rai Weiss had available at MIT. On the other hand, we didn't have the lab. The lab was being built.

And so, for the first couple of years we had a space that would accommodate about a 10-

meter-long interferometer on the third floor of West Bridge. And we actually built it. There was a vacuum system there, with two perpendicular arms. Initially it was built with optics that were on fixed platforms, not suspended. So it wasn't really a gravitational-wave detector, but it was a test bed that could be used for stabilizing the laser—for looking at certain types of optical noise sources. And so it developed with that goal. I mean, basically, there was a very big effort here at Caltech of stabilizing lasers, developing control systems—the servo systems necessary to operate things. There was much less emphasis on the kinds of noise sources that come from external disturbances, such as seismic noise and what we call thermal noise—the internal vibrations of the test masses. But there was a lot of looking at optical configurations.

The group at MIT, in a sense, took a different approach. They were concentrating on fundamental sources of seismic noise and thermal noise but had less emphasis on and less experience with these other optical configurations and optical noise sources. And so, in a sense, there was a lot of complementarity in the two groups. I should emphasize that the group in Glasgow was similar to the one here at Caltech—obviously. It was led by the same person.

COHEN: Was there talk back and forth between all these people?

WHITCOMB: There was a lot of communication between us and Glasgow. Again, at that time Ron was commuting back and forth every couple of months. So with every trip, there would be a big infusion of information in one direction or another. The group that he had in Glasgow had more experience than the one here at Caltech. It was also somewhat larger. And in fact, I think, over most of that period of the early 1980s, if you really were to look at it critically, I would have said that the German group—the one in Garching—was the strongest and achieved the best results. It was an experienced team of people. They had four or five senior people who had worked together for a number of years. They all held staff positions at the Max Planck Institute. They were a very stable group.

COHEN: Also, I guess, their funding was pretty good?

WHITCOMB: They had stable funding, stable staffing. And also they were just very good. They were clever. They had good ideas and were able to carry those through. They didn't have lots of teaching responsibilities to distract them. So they were able to make very strong progress. I would rate the group in Glasgow as the second strongest group. In general, I would say that the group here at Caltech lagged behind the group in Glasgow. There were things that were done in Glasgow that we ended up copying. But, in fact, I would say that we gained much more from that interaction than the group in Glasgow did.

COHEN: Well, you were a new group, just starting.

WHITCOMB: We were a new group, starting out, and still playing a lot of catch-up. I'll be quite honest. The group at MIT, I think, in a sense was even somewhat behind the group at Caltech. They were more poorly funded and even newer. They didn't have the benefit of a close interaction to help them catch up, such as the one we had with the group in Glasgow. They didn't have that same kind of relationship with the group in Garching, which was pursuing essentially the same technology as at MIT. Here at Caltech, we were already building up this 40-meter interferometer. By the end of 1981 or the beginning of 1982, we were able to move into the 40-meter laboratory by the Central Engineering Services building. We began to assemble a vacuum system and ultimately got the interferometer operating.

COHEN: Did all these systems depend on vacuums?

WHITCOMB: Oh, they all have to operate in a vacuum. That's absolutely critical.

COHEN: So was that a problem or a challenge?

WHITCOMB: It's a challenge, and it means that as you build these things, you have to evacuate it and see how it works. It increases the cycling time to make it work. At the time, I don't think we appreciated how important having a very clean vacuum system was. And so by the end of 1982 we had the 40-meter interferometer operating here at Caltech with, actually, a little bit poorer performance than the corresponding 10-meter interferometer at Glasgow. But still good performance. Because it was longer, it was a little bit better as a gravitational-wave detector, although no one would have expected it to detect gravitational waves. I consider one of the high points in those early days the fact that we were able to do a run looking for a gravitational-wave signal. We didn't expect to see one and we didn't see one. But it formed a part of Mark

Hereld's PhD thesis. He was our first graduate student—I mentioned him last time. And by the end of 1983 or the beginning of 1984 we had that experimental run. It was several days of operating the interferometer, more or less continuously taking data. Mark went and analyzed the data afterwards. It was a challenge, because we didn't have much in the way of computational resources. We weren't set up for it. But he was quite clever in doing that.

COHEN: So, in a sense, he had a non-result? I mean, it was a thesis with a non-result?

WHITCOMB: It's an upper limit.

COHEN: Yes, OK.

WHITCOMB: Which is the kindest way of saying a non-result. By the time Mark finished his thing, Siu Au Lee, who had been here and had been responsible for a lot of the early laser stabilization, had left and gone to Colorado State University. We picked up a new postdoc, Klaus Ziock, who was here. Bob Spero continued with the group and is still with LIGO today. But at the end of 1983, he came to the end of his three-year stint as a research fellow, the term they used then for a fresh postdoc. Bob was quite a solid scientist and good in the lab, and we wanted to keep him. We proposed him, actually, for promotion to senior research fellow. I think it was Ed Stone who nixed that. It was a bit of a blow to us. The reasons were simply that there hadn't been very much in the way of publications coming out of the research, and Ed just didn't feel that it was a package that could be justified to the Caltech administration. We still wanted to keep Bob, so we moved him into a staff scientist position as a member of the professional staff.

COHEN: And he was willing to stay on and do that?

WHITCOMB: He was willing to stay on in that capacity. So we kept him on. But it also, I think, set the stage for a lot of the later things and shaped the way that LIGO was staffed. There was recognition that publications in this field were not plentiful enough to assure somebody of a promotion along the traditional academic research or faculty tracks here at Caltech. And so, later on when Robbie took over, he made the decision that most of the people hired into LIGO at that time would be hired into these research staff positions.

COHEN: So there wouldn't be a question of tenure or publication or things like that?

WHITCOMB: Right. Again, to try to have stable staffing. It was really, in a sense, attempting to work—although I don't think explicitly—along the lines of the very successful German group at the Max Planck Institute.

COHEN: So you can see where people got their ammunition that perhaps this was not a project that belonged to any university but rather to an institute.

WHITCOMB: Sure. But again it's just a different sort of thing. There are other activities here on campus that are staffed with predominantly research scientists, and I don't think they are as strongly attacked in the same way that LIGO is. The cosmic ray group, for example—the Space Radiation Lab.

COHEN: And when we get into biology, I'm sure—I mean, they have stables of research groups.

WHITCOMB: No, not biology. Actually, I have heard that in biology they staff people predominantly with research faculty and postdoc positions, because they can pay them much less. [Laughter] Not to say anything bad about the biologists.

COHEN: OK. It's always the market.

WHITCOMB: But, actually, radio astronomy is another area that also uses quite a few of these research staff people. Dave Woody and most of the people up at Owens Valley [Radio Observatory, OVRO]—I think these are highly respected people. I think the world of Dave Woody [assistant director, OVRO], actually.

COHEN: They're lucky to have him.

WHITCOMB: Yes. But it's another area that has a lot of staff people and, in fact, I don't think it's inconsistent with how LIGO operated.

COHEN: Those radio astronomy people do publish.

WHITCOMB: Right. So, with the initial 40-meter interferometer, the sensitivity, I think, wasn't very good. It was built up in a configuration different from what we are doing now. It actually had three test masses. Technically speaking, there were the end test masses, which were rather complex structures, with lots of things bolted together; cut-outs, conventional mirror mounts holding mirrors on those things. And then we had what we call the beam-splitter test mass, which was a place that had a beam splitter plus the two input mirrors for the Fabry-Perot cavities all on a single structure, bolted together again—a rather loose and floppy structure. If you look at that now, in the light of current designs and what we now know about these interferometers, you would say that that's an absolutely dumb design. It's guaranteed to have lots of mechanical resonances and poor performance, particularly at low frequencies.

Here at Caltech, we were concentrating on high frequencies and looking at the sensitivity in the region between 1 kilohertz and 10 kilohertz. This was in spite of the fact that the theorists were by this time starting to tell us rather strongly that the best and most important sources would be at lower frequencies, and that around 100 hertz was probably our best chance of detecting a source. I think that as a group we were a little slow to recognize this and to assess the instrument's sensitivity at lower frequencies.

COHEN: Were they doing this at MIT back in the early '80s? Were they checking the lower frequencies there?

WHITCOMB: At MIT they had designed this thing with very good compact test masses. You'd look at them and you'd say, in light of what we're currently doing, that it looks like the right kind of design. These are structures that are compact, without a lot of things bolted onto them. Very rigid, high-resonant frequencies. From the point of view of having low noise, this should have been very good. The difficulty they had was that they decided to control those with an electrostatic controller, which doesn't offer very large forces or very much dynamic range. As a result they had difficulties holding everything in alignment and being able to control their interferometer—adjust the pointing of their test masses, and so on. And it was on a 1.5-meter-long base line—much shorter than the one at Caltech. And so the results they got were also, I think, really not very interesting over most of the frequency range. A second problem they had is that the MIT environment is a very noisy environment. I'm talking about the physical environment as opposed to the—

COHEN: [Laughter] Oh, OK. Actually, the other was quiet for them, because nobody had an interest in it there, I believe.

WHITCOMB: Well, that lack of caring on the part of MIT was a big handicap for them, in the sense that they couldn't get support. But the physical environment—it's in an urban area that has a lot of vibration and man-made noise. Their lab was right next to a river [the Charles] and the ground was very soft. It's not compacted very well and transmits vibrations. They're located right next to a busy street with a typical Boston road surface, which is 80 percent pavement and 20 percent potholes. So it was very difficult for them to get good results; and they didn't get very good results. That's my assessment.

COHEN: So you proceeded then with the 40-meter?

WHITCOMB: We proceeded with the 40-meter, and then there was recognition that the design that we had for these test masses was really not a suitable one. We were starting to see the effects of all of these mechanical resonances. And there was a decision made to move in the direction of installing much more compact test masses, partly driven by looking at the Glasgow, Garching, and MIT results. And we began to do that, but it was by retrofitting our existing interferometer. And after we made those changes, things improved. We initially did that on the end masses. Eventually we installed nice, compact test masses on the vertex. We did a little bit of a trick of installing two nice, compact masses on this rather complex beam-splitter arrangement. We were still operating that 40-meter interferometer at that time in what we call the non-recombined mode—actually looking optically at the two arms independently, as separate Fabry-Perot cavities. We would stabilize the laser to one cavity and use the second one as an analyzer cavity. That has a big advantage in terms of simplifying the control systems and making it really a very flexible diagnostic, but it also is a configuration that had no practical extrapolation to a large interferometer. You just couldn't build a large, sensitive interferometer with that configuration.

We recognized that, even as early as 1983 or 1984. We started to think about other configurations. Around that time Ron came up with an idea called recycling, which was quite a good idea, but it wouldn't work with the particular configuration that we had in the 40-meter. At the same time, it would be kind of moving into uncertain territory to change the 40-meter, although it needed to be done.

COHEN: Let me just ask you about the Russians. Were they still just doing bar stuff at this time? Or had their ideas come into play at all?

WHITCOMB: At that time Vladimir Braginsky's group was still doing bar detectors. It wasn't, I think, until 1984 or 1985 that Braginsky really started to see the advantages of interferometers. And they've never really tried any interferometry in Russia. I think there was a group in Kazakhstan that was attempting to do some interferometry. It was really not done.

COHEN: So they were not really part of the scene at this time?

WHITCOMB: No, at least not the experimental effort. Braginsky was an occasional visitor to Caltech, and always an interesting visitor, and he played a key role with Kip and Carl [Carlton M.] Caves in defining the fundamental limits of measurement. But he and his group were not really players in terms of being influential in the day-to-day direction that the experimental research was going.

When MIT and Caltech combined to try to arrive at a joint proposal, they had had radically different experiences with their interferometer prototypes. We had worked with the Fabry-Perot cavities and had some familiarity with them. We weren't scared by the control problems, and so on. The group at MIT had worked with this Michelson interferometer but hadn't worked with Fabry-Perot cavities, and were a little afraid of the control issues. Rai Weiss hadn't worked out in gory detail all of the mathematics for a Fabry-Perot cavity, which is what he needed to do in order to feel very comfortable with it—and the people in the MIT laboratory didn't feel very comfortable with it. And they had worked an entirely different set of problems from the ones that had been worked at Caltech. And that, I think, contributed to some of the difficulties. The groups didn't merge very smoothly in 1984 and 1985, in large measure because we had to make a decision: What were we going to propose and build? It broke entirely along institutional lines.

COHEN: Were you really supposed to be cooperating at this time? Or were you just still running in parallel?

WHITCOMB: Well, by 1984 we were starting to write joint proposals for the engineering design of the facilities. And as a result of the two groups being unable to find common ground on the design for these things, it was always viewed as a shoot-out. And in fact, much of the work on the 40-meter and on the 1.5-meter at MIT was sort of driven by the sense of having to prove the technological superiority of one design or the other to win this competition that was supposedly going on.

COHEN: Well, it could be constructive as well as destructive.

WHITCOMB: Possibly, but it led, I think, sometimes to some shortsightedness. There was a continual drive to make the 40-meter more and more sensitive. But at the time it was operating in this un-recombined mode, and it would have been a big job and a big risk to convert it over to a recombined mode—although that probably would have been the right thing to do for the long term. If you're going to build a big interferometer, you should get experience with the configuration that you're going to operate. But, at the same time, there was a pressure to continually make it more and more sensitive so as to win this competition with MIT.

If you say that converting it to this recombined mode might take six months to make the optical and electronics changes and get everything working together—just to get it operating—you don't really know if it's going to be more sensitive, because you don't know what all the problems are going to be. And there's going to need to be a period of time to shake it down. And if you're in this sort of competition, this shoot-out, you're very reluctant to shut things down for any length of time to make that kind of a conversion.

COHEN: You'd lose your place.

WHITCOMB: Yes. Ron was afraid that, if we made a change like that, suddenly MIT would spurt ahead of Caltech in sensitivity and we would be behind the curve.

COHEN: So, in some real sense, this competitive environment was not a healthy thing?

WHITCOMB: It was not a healthy thing. I really felt that it was quite important to start to look at the issues of doing this recombined thing. When I left in 1985, we had been, for about a year, just always on the verge. We had been talking about recombining the light, because we knew we had to do that. But it was always something that we had to do but couldn't do quite yet.

COHEN: How many people were working on the project at this time—1984 to 1985?

WHITCOMB: The group, when I came you may recall, was Siu Au Lee, Bob Spero, myself, two graduate students—Mark Hereld and Eugene something-or-other; and then Ron. In 1982 we picked up another postdoc, Dana Anderson. He had worked with ring laser gyros. He was actually Herbert Anderson's son, from the University of Chicago. I forget where he did his PhD. He came as a research fellow—a fresh postdoc who had just gotten his degree. He was a very energetic guy—a little undisciplined. Every time he'd hear about something new, he'd want to go off and try it. Actually, he was very fun and exciting to work with. That kind of undiscipline is great in a normal research group, but maybe not quite as appropriate in a project where you're really focusing on a single activity. He learned about phase conjugation after a couple of years at Caltech, and he wanted to go into the lab and build optical cavities using phase conjugation, which really didn't have much to do with the gravitational-wave effort. But a very lively guy. I liked Dana a lot.

So he came in 1982, overlapping with Siu Au Lee for about six months, I'd guess, before she left, because her position wasn't renewed. Then there was another research fellow who came: Klaus Ziock. He came from a low-temperature group at Stanford, where he had just gotten his degree. He stayed about a year—I forget whether it was one or two. He and his wife weren't all that pleased with living in Pasadena. They were having difficulties monetarily—surviving as a family on what a postdoc made here. And maybe he just wasn't really that happy with the work and as suited for the position. He left.

Dana stayed the full three years as a research fellow. In 1984, in his third year, Ron was trying to decide if we should make him a senior research fellow or just let it lapse. He had a very difficult time making that decision. I told Dana at the time, because I remembered the experience with Bob Spero, that it wasn't clear that, even if we wanted to get that kind of a position, that we could. And frankly, I wasn't sure whether Ron was going to push it or not.

Ron had mixed feeling about Dana. He thought Dana was a very lively, energetic guy but oftentimes working on problems that Ron didn't really want him to be working on. And I encouraged Dana to protect himself by applying for some other positions. In the end, Ron finally came down and decided to push for a senior research fellow appointment, and actually got it through. And the offer was made to Dana almost coincidentally with his getting an offer as an assistant professor at the University of Colorado with a very nice startup package. And he went to the University of Colorado and has done very well there. He is a tenured faculty member. In the meantime, we had also picked up some graduate students.

After Mark Hereld left, we still had about four graduate students. Mike Zucker was one. He had worked as an undergraduate at the University of Rochester in gravitational waves. A very, very good guy. He eventually got his Caltech degree around 1987. Another graduate student, Sherry Smith, joined about the same time as Mike. She wasn't quite as experienced in the laboratory as Mike was, and so was a little slower to pick up. But she was really very sharp—maybe better academically than Mike was—and so that maybe made her life a little easier. There were two other graduate students: Gary Gutt and I forget the fourth one.

COHEN: That's a pretty good-sized group.

WHITCOMB: It was actually. The latter two graduate students didn't really catch on as well as Mike and Sherry did. They had a hard time focusing. Ron wasn't giving them much in the way of supervision. They tended to need a lot of day-to-day guidance, and we were so wrapped up in things with MIT that they oftentimes went for relatively long periods of time without that. It was pretty clear to me that it wasn't a good fit between their personalities and the research group. And I encouraged Ron to cut it off with those students at an early enough stage so that they could go to another research group and still get a thesis done in a reasonable amount of time. Ron, again reluctant to make a decision like that, let it just kind of drag on and on. Ultimately, I think, both of them did leave the group without getting degrees.

Gary Gutt, I know, went to Tom Tombrello's group and got his degree. The other student I think got a degree—maybe with Tom. I'm not absolutely sure. Mike and Sherry, who were the younger two students, stuck around. And in a sense they supported each other. At that time—this was after I had left, in '86 or '87—Bob Spero and Ron and the other post-PhD scientists, were so focused on the large interferometers, the designs, and the battles going on between MIT and Caltech, that Mike and Sherry basically took over the 40-meter lab. They were so much on top of the things going on in the 40-meter lab that they intimidated the rest of the group, and were able then to make a lot of progress in the day-to-day work and actually made a lot of improvements in the interferometer. [Laughter]

COHEN: That's an interesting story in itself! [Laughter]

WHITCOMB: There are more cases than you might believe of graduate students who take over a lab, shut their thesis advisor out, and actually do good work for that reason. It oftentimes makes some of the best researchers. It demonstrates a level of initiative and self-reliance.

COHEN: Independence?

WHITCOMB: Independence. A really good graduate student doesn't need a thesis advisor. Thesis advisors are more of a handicap than help if you've got a really good student. Ultimately Mike Zucker stayed with the project here. Sherry Smith went to Bell Labs.

COHEN: So it sounds like these people went off to good jobs.

WHITCOMB: Oh, yes. People who have gotten degrees with gravitational-wave detection—I mean, they learn a lot of experimental physics. And they learn it in a very broad and a very fundamental way. And so they tend, oftentimes, to be very adaptable to different kinds of things. Sherry Smith isn't doing gravitational waves. She's working in optical things at Bell Labs, and doing quite well.

COHEN: So, in that sense, that's a plus for being in this environment?

Whitcomb: Sure. It's actually very good for graduate students. A good graduate student learns a little bit of general relativity and some deep mathematics and electronics and mechanical design and optics and computation. And suddenly they are very flexible and very adaptable. For instance, Mark Hereld went to the University of Chicago and worked in near-infrared astronomy, again developing instruments there. Very, very successful. So the students who have stuck it through have done really very well. And the same thing can be said of the students that Rai Weiss had at MIT. Again, they are scattered all over in different places.

COHEN: Well, that's how you can judge the success of something. And in a university you're watching the clock tick.

WHITCOMB: Right.

COHEN: This is the science that was going on in that period.

WHITCOMB: Right. And it was predominantly focused around these instruments and really trying to prove the technology. It was entirely instrumentation development at that time.

COHEN: And were you having weekly meetings? How did this progress? I don't want to get into politics, because you don't want to do that.

WHITCOMB: There weren't formal, regular weekly meetings, but people were interacting on a day-to-day basis. The physical environment was as good as could be expected. Everybody had their offices on the third floor of West Bridge, so we would see each other on a very frequent basis. People went to lunch together.

COHEN: Was there interaction with the theoretical group? With Kip Thorne and his people?

WHITCOMB: Some. Kip was kind of a periodic visitor to Caltech at that time. For one year, he was on sabbatical. And so there was almost a year when we didn't see him at all. Other times you'd see him fairly frequently. He was starting to work off campus more and more at the time, because he found that he could concentrate better if he was off campus rather than on campus. We were, as a group, interacting quite a bit with the astronomy community here. I tried very hard to integrate the group into astronomy. And I worked, actually, rather hard to make the astronomy community on campus aware of gravitational waves and their astronomical applications. We were on the regular circuit for the Astrophysics Journal Club on Tuesdays.

Two or three times a year, I would have to do a journal paper. And I typically would pull out something that had to do with gravitational waves and the astrophysics of that. So the group was actually interacting with the astronomers on a fairly regular basis at that time.

COHEN: So then you left. And when you came back—we're talking about the science again—did you see a lot of progress having been made? How many years were you away?

WHITCOMB: I was away for almost six years. Five-and-a-half years.

COHEN: So you came back. And, scientifically, what presented itself to you?

WHITCOMB: They had done some very important things in terms of the instrumental work on the 40-meter. I came back, and they had broken up the central test mass. They, again, had recognized that it was still one of the things that was limiting the performance. And they had gone through a rather lengthy process to rebuild the vacuum system—expanding it and breaking apart the central test mass into two individual test masses for each arm. Each arm now consisted of a near test mass and a far test mass, and then a separately suspended beam splitter.

COHEN: Did the interaction between MIT and Caltech seem any different—better or worse?

WHITCOMB: It was a lot better, actually, when I came back. Getting back to the other changes, let me talk a little bit about the technical changes here at Caltech.

COHEN: OK.

WHITCOMB: So, they had made that one change on the beam splitter test mass, to split it into a beam splitter and separate input test masses. The other big change that they had made was in the way in which they were stabilizing the laser. The way that we were stabilizing it in the early eighties was a convenient way that involved putting a Pockels cell inside the laser. That allows you to change the length of the cavity electronically, which you could do very rapidly. It makes it easy to stabilize the cavity, but it reduces the amount of power that you can get out of the laser. And so the most power we could get out of the laser in the early eighties was a few tens of milliwatts, when what we really needed were, in the end, watts of power. In order to get watts of power, you have to take that Pockels cell out, put piezo-actuated mirrors on, and reconfigure the laser. [Tape Ends]

Begin Tape 2, Side 2

WHITCOMB: The stabilization of the laser was one of those things where, again, you would shut down the interferometer for a period of perhaps six months, make this change to the laser, and eventually bring it back up. And you don't know exactly what you're going to get when you bring it back up again. Most likely the performance, at least initially, is going to be worse. And, in fact, it did shut down the interferometer for about six months, and when they turned it back on, the performance was worse. It took them some length of time to get that back up. It was not a uniformly liked decision, to make that change. Robbie insisted on it, because he said that the old laser stabilization system could not be scaled up to LIGO and it needed to be changed.

A lot of people didn't like the idea of turning off the interferometer, making that kind of a change, and then turning it back on. But Robbie's point of view was that otherwise you were solving the wrong problems. You were building an instrument and testing something here on campus that wasn't what you would be doing in LIGO for the full-scale interferometers. So not a popular decision, but one that in the end worked out. Both of those changes were really good, and in fact, the interferometer performance was much better when I came back in '91 than it had been in '85 when I left. That was good. And it had been pushed to do better at low frequencies, in particular—another very positive thing. I think the biggest disappointment I had when I came back in 1991 was that we were still operating in this un-recombined mode.

When I left in 1985, everyone was saying that the next step really ought to be to recombine the arms and operate in a mode where you're interfering light from the two arms. And six years later I came back and people were still saying that it was really something we ought to do and that it ought to be high on our list of priorities and that it ought to get done. I think it was crucial to do that. It was very important and it hadn't happened. I was a little bit disappointed to find that that aspect of it hadn't.

COHEN: Had they not worked on it? Or did everybody just have a different priority?

WHITCOMB: It was, I think, just too scary for people to really say, "Let's do it." There were big pushes to make some of these other changes that were maybe a little less disruptive to the interferometer. There was pressure from the NSF to improve the sensitivity of the interferometer so that we could push for a big construction project. Also, it was maybe just that other people didn't think it was as important as I did. We finally did that, basically, a year ago. So even after I came back it still took us years to actually get around to doing it.

COHEN: I don't understand something. You're doing something experimentally here, but you've already broken ground to build this thing somewhere? That's hard for me to understand.

WHITCOMB: It's "concurrent R & D."

COHEN: That's normal? I mean, I don't know.

WHITCOMB: That's not too surprising. If you look, for example, at the Caltech submillimeter telescope, Tom Phillips was developing more sensitive detectors at the same time that they were building a big telescope. The Superconducting Super Collider was developing the capability of building magnets and testing that at the same time they were digging tunnels in Texas. Maybe we were a little less mature than those projects, but nonetheless it's still—

COHEN: So that wasn't really something crazy?

WHITCOMB: I don't think it was completely crazy.

COHEN: "Scary" is maybe a better word.

WHITCOMB: Sure. This approach put a lot of pressure on us to get results from the R&D program. There was always the possibility that we'd run into some kind of a showstopper, although I was pretty sure that we wouldn't. But there's always that possibility. But the other reason for doing that is, if you waited until all this technology was done and then started a construction project, you would have a hiatus of four or five years from the time you had the technology developed to the time you were ready to install it at the sites. And there was, I think, a very real question: Could you keep a group of scientists sort of hanging around twiddling their thumbs on campus and playing with small-scale things, waiting for this big construction project to finish?

COHEN: OK. So I guess that's sort of normal.

WHITCOMB: It's scary. A lot of people thought it was very frightening. And that was, I think, a lot of the controversy in 1991 and 1992 after I came back. It revolved around this question of whether or not the construction project was ahead of the development. I think we'll show, in the end, that in fact it wasn't—that those two things will come together about the same time. I might be wrong. But we'll know the answer to that question three or four years from now.

COHEN: So, the science just develops and you just continue to solve the problems that need to be solved for the finished instrument?

WHITCOMB: Right.

COHEN: And the group is at peace in some way now? Is that a good word? I mean, are they functioning well?

WHITCOMB: Well, I think during the early eighties, certainly the group functioned well. They were close socially. It was a relatively small group. I mean, it had a significant number of people. It was large compared to a lot of physics groups on campus, but not outrageously so. It had a dozen people who were quite sociably interacting.

COHEN: Would you feel comfortable saying something about the climate that you're in now?

WHITCOMB: I'll come back sometime in the future and talk about that, if that's all right.

COHEN: So, you're optimistic of course, and you think we're going to detect all these waves one of these days?

WHITCOMB: Yes, I think so. There have been very few examples of cases where people have done really innovative things in developing significantly more sensitive detectors in astronomy and not found pleasant surprises and puzzles—the kinds of things that drive the science. Radio astronomy itself was kind of an accident. It wasn't thought that there would be such wonderful discoveries. It's been tremendously successful. But if you look at it, so was gamma ray astronomy. And so was infrared astronomy. And so was submillimeter astronomy. And on down the list. I think the only—if there is any—kind of astronomy that was a little bit of a disappointment in the sense that people saw just more or less what they expected to see, might be ultraviolet astronomy. But in general the initial discoveries have always been more interesting than the predictions. The universe has produced surprises. And I can't imagine that that's not going to happen with gravitational waves. I think, even if you just say, "Well, what are the things we're almost assured of seeing and what are we going to learn from that?" it's still interesting and it's still worthwhile. But it's really the surprises that are going to be, I think, the fuel. I think the detectors will actually come together, and they will be as sensitive as we predict they will be. It will be challenging.

COHEN: And so you see a good future for all this work?

WHITCOMB: I do.

COHEN: OK. Well, maybe in a few years you'll talk some more.

WHITCOMB: OK. Thanks.

AFTERWARD: 2017

The interviews here were taken in mid-1997 but have sat unreleased for nearly twenty years. I received transcripts of them in the months after they were done to make necessary corrections and to approve them for release. I didn't do that for a couple of reasons. The work of installing the initial LIGO detectors at the two observatories was just about to begin, which led to increased demands on my time, including a heavy travel burden. Moreover, I was not entirely satisfied with my tone in the interviews and concerned about the impact that their release might have on my career. Consequently, they were set aside, and largely forgotten.

With the announcement of LIGO's discovery of gravitational waves last year, there seems to be increasing interest in how the whole enterprise came about. My most recent encounter with this came when Heidi Aspaturian from the Caltech Archives contacted me to ask if I would be willing to give an additional set of interviews on LIGO. For better or worse, I have agreed to do so. She and I agreed that the first step in this process would be to finish off the earlier interviews, i.e., for me to make the changes necessary to feel comfortable about releasing the text.

With Heidi's admonition that "a major aim of the oral histories is to capture your personal perspective on events and to record how history unfolded from your point of view" in mind, I have tried to step as lightly as possible on the original transcript. The majority of the changes were aimed at clarifying a word, phrase, or sentiment. Sometimes I simply crossed out gibberish phrases, ostensibly for clarity, but in reality to not appear so tongue-tied.

The substantive changes were of two varieties. First, there were instances in the original interview where I ascribed motives to others' actions. Sometimes, I had a real reason to believe those motives (i.e., the person told me why they were doing something), and I have left those in the text. In others, I had just tried to guess why, as people are often wont to do. When I recognized those instances, I changed the text to just report on what the other person did, and not to include my guess at why. Second, there were a few cases where I spoke about a situation in a narrow context, and I felt that a typical reader might interpret my answer in a broader context. In those cases I have added a sentence or clause to either address the broader context or to make it clear that my answer should be interpreted narrowly. My wife described this to me as softening the text, and I suppose, as usual, she is right.