



**THOMAS A. TOMBRELLO**  
(b. 1936)

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

**December 2, 1998**

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



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**Preface to the LIGO Series Interviews**

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

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

## **Subject area**

Physics, LIGO

## **Abstract**

Interview, December 2, 1998, with Thomas A. Tombrello, then chairman of the Division of Physics, Mathematics, and Astronomy.

Recalls arriving at Caltech in 1961 as postdoc with Tommy Lauritsen. Early work at Caltech on gravitational-wave detectors. Role of Kip S. Thorne, James Mercereau. 1976 committee to pursue gravitational-wave work. Arrival of Ronald W. P. Drever. MIT involvement under Rainer Weiss. Hiring of Frederick Raab and Jeff Kimble. Tombrello proposes Rochus (Robbie) Vogt as LIGO head, 1987; his relationship with Vogt. Vogt's dismissal as provost 1987 and lobbying effort for LIGO in Washington. Problems with Drever. LIGO's growing pains, late 1980s, early 1990s. President Thomas Everhart's lack of involvement. Vogt's difficulties leading an expanding LIGO. Formation of LIGO Oversight Committee under Lew Allen. National Science Foundation's involvement. NSF meeting January 1994 and dismissal of Vogt as director. Barry Barish becomes director. Earlier mishandling of Drever affair by Academic Freedom and Tenure Committee. Discussion of bad feelings on campus and within project.

Current promising outlook under Barish. How LIGO project is viewed at Caltech and at MIT. Raab's tenure problems. Tombrello as tenure committee head; role of Kenneth Libbrecht. Raab as Hanford site manager. LIGO's prospects. Fallout from dismissal of Vogt and Drever in early 1990s. Comments on new Caltech president David Baltimore, and on former presidents Marvin L. Goldberger and Harold Brown and former geology division chairman Robert Sharp. Comments on reengineering project begun under Everhart.

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**Contact information**

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

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**CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES**

**ORAL HISTORY PROJECT**

**INTERVIEW WITH THOMAS A. TOMBRELLO**

**BY SHIRLEY K. COHEN**

**PASADENA, CALIFORNIA**

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**ORAL HISTORY PROJECT**

**Interview with Dr. Thomas A. Tombrello**  
**Pasadena, California**

**by Shirley K. Cohen**  
**December 2, 1998**

**Begin Tape 1, Side 1**

COHEN: Good morning, Dr. Tombrello. It's good of you to come and talk to us about LIGO. While we're not really specifically talking about your own background, it wouldn't hurt if you just quickly gave us some sense of your education, when you came to Caltech, and so on.

TOMBRELLO: I did my undergraduate and graduate work at Rice Institute, which later became Rice University. I came to Caltech in 1961, as an NSF [National Science Foundation] postdoc, and with the exception of a couple of years I've taken off at various times, I've been here ever since. In some ways, I grew up at Caltech.

COHEN: And you went all the way from postdoc to, now, the division chief [chairman of the Division of Physics, Mathematics, and Astronomy].

TOMBRELLO: Yes, and it's not clear that I didn't start at the top and work my way to the bottom—in terms of research freedom and just having pure fun. Being a postdoc is probably the happiest time in one's life. What's interesting is that I knew this at the time.

COHEN: Whom were you a postdoc with?

TOMBRELLO: I was in Kellogg [Radiation Laboratory]. I worked with the graduate students and Tommy Lauritsen. I left once to be an assistant professor at Yale in the beginning of 1963. I didn't like it and came back to be a postdoc at Caltech, which caused a great deal of flak at Yale. I said that I wasn't mature enough to be an assistant professor, and they said, "That's true."

COHEN: [Laughter] Well, their loss.

TOMBRELLO: My gain. Would you like to hear about LIGO [Laser Interferometry Gravitational-Wave Observatory]?

COHEN: Well, I have in my notes that [Feynman Professor of Physics] Kip [Thorne], of course, had this bug about it. And I have that in November of '76 an informal committee was set up. You were part of that, weren't you, with Alan Moffet [professor of astronomy] and Gerry Neugebauer [R. A. Millikan Professor of Physics]? Can you tell me a little bit about that?

TOMBRELLO: I can start you even earlier, because Kip had a bug about this. There was an aborted attempt at the beginning in this area—as a collaboration between me and the low-temperature group at Caltech. Jim Mercereau [professor of physics] was running that then—and it would have been not on the laser-type interferometers but something like a supercooled bar-type detector. Remember, about 1970 was when the Weber [Joseph Weber, at the University of Maryland] observations began to come in. We all felt they were a bit fishy. So for a while Mercereau and I worked together on trying to decide what form a next-generation bar experiment would take. But it never really got anywhere; it was partly funding and partly that Jim and I were working at cross purposes. Kip was getting more and more frustrated with both of us, I think. And that led, eventually, to this informal committee that was formed in 1976, when we looked through the field to see what were the promising techniques and who were the promising people. And we found Ron [Ronald W. P.] Drever. We looked at a lot of people, Drever being somewhat nailed down at Glasgow. He had built a small interferometer there. So Caltech made an unusual arrangement with Ron. Ron was here part-time until the Glasgow thing was on its own feet, and also until the Caltech project got on its own feet. Here they were planning the 40-meter interferometer. So that was the early days, and that sort of extended until sometime in the mid 1980s.

COHEN: Then let me just correct an impression I have from talking to Kip. This all began a lot earlier than Kip's finally saying, "We really need an experimental group here." You had already been doing something?

TOMBRELLO: We were trying to, but getting nowhere, until we finally understood that Kip needed something to happen here. And it wasn't happening elsewhere—or maybe it was just beginning to happen elsewhere. So 1976 was the beginning of a serious project that was going to go somewhere. Getting Ron was something we all agreed on.

COHEN: Were there any qualms that perhaps he would not be effective in a large group?

TOMBRELLO: That never came up, because this was really small-group physics. This was the sort of thing for which you needed a very inventive person, and Ron had pulled off exceedingly difficult experiments and really had many of the bright ideas in this area. I believe the whole business of the laser-type detectors came from a man named Robert Forward.

COHEN: Over at Hughes [Research Laboratories]?

TOMBRELLO: Yes, over at Hughes. And that occurred, I think, in the early seventies. My memory is not perfect on this, but Forward never got as far as the stuff Drever was doing. Ron must have picked it up about the same time and contributed many of the very original ideas. So there was absolute unanimity on picking Ron, and no thought that this was going to turn into a big experiment at that point.

Now, by about 1986 it was clear that things were working—the 40-meter was something that was going to work. And the ideas were to go from there, clearly, to something bigger. MIT had their own way of going at it. Rai [MIT physics professor Rainer] Weiss was running that group, though Rai was [also] doing some other things. We had looked at Rai, back in the 1976 committee, but we felt that he was interested in a number of things and he might not focus on this in the same way as Ron. Though as it has turned out, Rai certainly put as much effort into it and provided many of the fundamental ideas in the field.

So what we had were two groups that were at the point where they had to do something. They had somewhat different technical directions they were pushing. And I was asked by Ed [Edward C.] Stone [Morrisroe Professor of Physics, chairman of the Division of Physics, Mathematics, and Astronomy, 1983-1988] to run the staffing committee in physics, something I ran until a few months ago.



COHEN: I see. This was in '86?

TOMBRELLO: This was in 1986. And we had several charges. One was to hire somebody in this field, and ultimately we did hire Fred [Frederick J.] Raab. We were to look for targets of opportunity. We found one—Jeff Kimble. That was, in some ways, the most interesting thing we did that year.

COHEN: Jeff Kimble was not hired for the gravitational work, was he?

TOMBRELLO: This is an interesting point. He wasn't, in a sense. We, and particularly I, felt that there was no culture of precision measurement at Caltech that all this would rest on—what became LIGO. Let me just call it LIGO, but it wasn't LIGO yet. I don't want to have to keep saying “gravitational-wave research into detectors,” so let me use the term LIGO as shorthand, but the name hadn't appeared at that time.

There wasn't any cultural base. And Jeff was working on things in squeezed light and precision measurement that I felt would enrich the culture. Kip felt that way, too. He felt that this was a target of opportunity, because the people at the University of Texas did not appreciate what they had. Kimble was an associate professor, and he was clearly a real star—a late-bloomer but a real star. And we had a good opportunity to get him. There was another name that popped up at that time, and I wish we could have gone after both of them. And that was Steve Chu, at Bell Labs—this was before he went to Stanford.

COHEN: Was that before he got the Nobel Prize?

TOMBRELLO: That was long before he got the Nobel Prize [1997], but it was very clear that these were two of the movers and shakers in a closely related field. We couldn't go after both, and everybody was already bidding for Steve, but Kimble looked like someone who was extraordinarily good. I still believe that Jeff is going to get a Nobel Prize. I think he's quite extraordinary. But it looked like Texas was not playing it well, and they continued to play it badly, in that they then offered him a chair after Caltech made him the offer. But Jeff, being

Jeff, said, “Well, but you didn’t want me before Caltech did. If you’re waiting for Caltech’s stamp of approval, I might just as well go to Caltech.”

But we had a third charge. Stone said—and it was generally agreed to, by Drever, Thorne, everybody—that we needed somebody to take over this job of pulling together Caltech and MIT, writing and getting funded a proposal for something bigger. That’s what became LIGO. And this is a funny story. You just have to take it on faith that I’m telling you the truth and not reconstructing it afterward. Robbie [Rochus E. Vogt] was provost then, and we were in the middle of searching for the next president; it was not out of the question that Robbie would be on that list. But Robbie’s name never came up in our committee. We looked at a bunch of people from other places, mostly high-energy physicists. I typically spent a couple of weeks a year at Yale, so I went off to Yale. D. Allan Bromley [then the director of Yale’s Wright Nuclear Structure Laboratory—ed.] was my host, and he decided to have a little party in my honor. Allan, at a later time, became President Bush’s science advisor. Anyway, Allan had a little party for me at the New Haven Lawn Club. People from the Physics Department were there, and we got to talking about what I was doing on this staffing committee. They said, “What are you doing about the gravitational-radiation thing? I hear you’re looking for a senior person to coordinate all that.” So I mentioned some of these high-energy physicists. One of the high-energy physicists said, “You see, any time anybody wants something run, you have to turn to a high-energy physicist to run it.” And I said, “I don’t know about that.” And then into my head came this idea: Robbie! And I started to laugh. I couldn’t stop. And Bromley looked over and said, “Well, we all knew someday you were going to snap, and I guess we’re just all here to observe it. Would you mind telling us what’s so funny?” And I said, “I can’t tell you, but it’s one of the world’s great jokes.” Because Robbie and I had always been. . . . When he was the division chairman [1978-1983], he and I worked at cross purposes. It came to very, very serious disagreements between us—I mean very serious! It pulled the president [Marvin L. (Murph) Goldberger, 1978-1987] in; there was talk of legal action. Robbie and I were enemies. So that’s what part of the joke was—I suddenly realized that this person who had been my enemy was someone I was going to try to offer what I felt was a great job. And nobody else would have thought of it.

So I got back and said something to the committee, and they said, “Yeah!”—including Drever. Drever was excited about it: “Oh, Robbie, yes! Exactly! But can we get him? How do

we do this?” So I spoke to Stone. Stone said, “I don’t know how to handle this. He’s probably a candidate for president, but the [search] committee is not going to tell us. If you can find out a way that we can get approval to go forward with this, I’ll take the offer to Robbie.” So I called Don Cohen, who was chairman of the faculty at the time and was clearly coordinating the [presidential] search committee. And I just explained the situation. I said I did not want to cut across their trail, but I really needed to know if we could go ahead with this. He said, “Stay by the phone. In no more than ten minutes, I’ll call you back and tell you the answer.” I assume he called Rube [Ruben F.] Mettler [then the chairman of Caltech’s board of trustees—ed.]. I don’t know; you can find out from Don Cohen. He called me back and said, “Go ahead.” And I thought, Well, Robbie’s not a candidate for president, then—that’s pretty clear—and we can go ahead. And Robbie may have a different story, but Stone managed to present this offer to him two weeks before Robbie was replaced—basically, fired—as provost.

COHEN: So there was a sense that he was going to be dismissed as provost?

TOMBRELLO: There was no sense of that; it came as an absolute, total surprise. It came, as far as most of us were concerned, out of a blue sky.

COHEN: There was no sense that he wasn’t getting along with Murph?

TOMBRELLO: Oh, in every sense of the word he was not getting along with Murph, but it was not clear which one of them the trustees preferred.

COHEN: [Laughter] Which one would go?

TOMBRELLO: Absolutely. In fact, you can probably get some people who will speak to that. From what I’ve heard—and these are rumors, and I do not have any inside information on this—the trustees were pretty divided over which one should go.

Anyway, Robbie got canned. But he had been presented with this offer before he got canned; it was not a consolation prize. Robbie may say that now. Certainly the negotiations hadn’t been finished, but Stone had gotten in there and presented it to him. Robbie had not answered yet. Robbie was clearly going to negotiate, and did. So that was my great joke of

putting Robbie in. And I think the results speak for themselves. I think no one else could have pulled those groups together and gotten that project funded.

COHEN: So at that point things were going, in a sense, smoothly. I mean, Ron [Drever] was cooperating as much as he could, and Rai [Weiss] was doing his part, and it was moving?

TOMBRELLO: Right. Everybody was swept up in this enthusiasm to achieve unity of purpose and get the money to build something. And Robbie put in a lot of effort in Washington on that, and with a lot of hostility. People like Allan Bromley were strongly against LIGO. It's basically small science with a big price tag. It had no real community behind it at that point. It wasn't like high-energy physics, where you have an enormous lobby of professors in every university. It was not even like astronomy, where there are a lot of astronomers. There were very few people in this field. So Robbie pulled off a miracle.

During this period—1987 to 1989—I had left Caltech to go run an industrial research lab. I was on leave for a while. So all this was happening in the background. I came back in '89 and discovered that Ron Drever was a very unhappy camper, a lot of people on campus were mad at one another, and everyone had sort of taken sides. I had been away from it, so I could look at it, I think, somewhat dispassionately and say, “There are problems on both sides.” And this is where Everhart [Thomas Everhart, president of Caltech, 1987-1997] and I really began to have trouble.

COHEN: So this is 1989?

TOMBRELLO: This was 1989. It became clear to me—and then in 1989 Barclay Kamb was sacked as provost and they put in [Paul] Jennings. I kept trying to tell Paul, “Look, you don't have the story right.” What I kept saying to Everhart and Jennings was, “Look, the Drever thing's a symptom. It's not what's really going wrong. It's certainly something that should be addressed, but the real problem is that the group is too small and too fragile to do what they're supposed to be doing next.”

And so I kept saying this. You have to remember that when Stone and I set this up, Stone and I knew Robbie. Robbie is a very volatile person. And Stone said, “This is fine,”—this is

now going back to 1986-1987—“but what do we do as an insurance policy?” And I said, “You’ve got to have some kind of board of directors or oversight committee or something that basically keeps an eye on what’s going on in that project.” But when I came back in ’89, there was no committee. I said, “Ed, what’s going on?” And Ed said, “I recommended it, Tom. I believe, as you do, that we may have to push the button sometime and get Robbie out of there.” Not that we saw any sign of it at the beginning. But Robbie didn’t want such a committee, and so they didn’t have it. And I kept saying to Everhart, “You’ve got to get this board of directors. You’ve got to get this group of people in there to provide a kind of a safety valve. If the director is having a problem, they can help him solve it, or they can get rid of the director.” So this went on. Finally, I think at the end of ’92—

COHEN: Let me come back a minute to Everhart. Do you think he really just had no vision, or he just wasn’t that interested?

TOMBRELLO: Yes, he had no vision at all.

COHEN: OK. It’s not that he wasn’t interested in the project.

TOMBRELLO: He’s not very smart.

COHEN: OK.

TOMBRELLO: I’m extremely candid. I’m going to tell you—and Everhart knows it, too.

COHEN: Good. You will not be the first person sitting here who has said that.

TOMBRELLO: After five years, I realized that they weren’t going to evaluate Everhart. You see, administrators have to be evaluated every five years. It’s something Harold Brown [Caltech president, 1969-1977] started. And Harold Brown said, “You have to do it to me after five years,” and they did. They didn’t do that with Everhart. So I just wrote Everhart my own evaluation of his performance; it was not a good grade. I sent it to him, and I got back an answer. [Gerry] Neugebauer was chairman [of the PMA Division] at that time, and he asked me,

“What did he say?” I said, “The part that probably scared me the most was that it began with “There are many things you’ve said that are probably right, Tom.” Neugebauer just rolled his eyes and said, “Well, it’s hopeless!” [Laughter]

I have to talk a little bit about the sociology of small groups, because I’ve looked at how groups grow up to run projects. Usually something starts with a dream, an idea. Then you assemble a small team, because one person usually can’t achieve this thing. And the first team, if successful, usually has to be made up of true believers. So this is tribal; you form a tribe. And the tribe is motivated, they’re excited—I’ve seen this in industry and in universities; it’s a human characteristic—and they really push for it. Now, if they’re successful, there’s a time when the tribe has to grow to make this thing real. They just had a project at Schlumberger that almost went belly-up; it’s almost a carbon copy of what happened at LIGO. The team has all the right feathers and all the right marks on their faces and the tattoos, and has been through the rite-of-passage/coming-of-age party. And now you’ve got to add people. And the team says, “These people don’t have the right feathers, they don’t have the right marks, and they weren’t there on day one. *We* are the founding fathers; *we* are the tribe.” That’s what happened with Robbie’s team. Robbie, basically, had created a very good team. He motivated them to some extent in a way that I think is counterproductive, which is, “They were all against us, boys. We can only depend on one another.” I’ve seen that happen at other places. It’s a great motivational tool, but it’s self-defeating, because it means you can’t grow when you have to. What happens is you have to be able to add people to make this dream real. And what happened with Robbie was—and I could see that, because I had just come out of industry, where you could just see this stuff happening—I could see it at Caltech. I could see that they had really good people who were really committed to this project, but they were too thin. And anything that happened sort of upset the progress in the group. So Drever was being Drever, which is sort of the mad-scientist mode.

COHEN: Working at night? [Laughter]

TOMBRELLO: Well, worse than that. I picked up a student just before I left for Schlumberger. He actually worked for somebody in my group; I kept the group alive. And I said, “Gary, you’ve been three years with Drever. He has a very high opinion of you.”

COHEN: Gary?

TOMBRELLO: His name is Gary Gutt; he's a staff member at JPL [Jet Propulsion Laboratory]. He said, "You don't understand what it's like to have your thesis advisor come in every morning and change your thesis. I'm never going to get out of here. Just the frustration of having to deal with this man, who has plenty of good ideas, but I can't change things every day." And it was really true. Gary, in two years, had a PhD. And he'd probably still be going crazy working with Ron. Ron is brilliant, but he's not focused or coherent or any of those things. So I began to think about how you use guys like Drever. I have to admit, this is stream-of-consciousness, but it's all sort of in sequence. I read something about that time called *Critical Assembly: A Technical History of Los Alamos* [eds. Hoddson, et al.: Cambridge Univ. Press, 1993—ed.], which had just come out. It was a book summarizing what happened at Los Alamos during the early stages of developing the first nuclear weapons. And I was interested in one particular question. There were two people in the project that were very like Ron: Edward Teller and Seth Neddermeyer. I wanted to know when [J. Robert] Oppenheimer had set those people up in their little projects. And I found out. They set them up essentially on day one. Neddermeyer was looking at a kind of crazy idea that you could increase the density of something quickly by basically using high explosives to squeeze it—implosion. They didn't need implosion; they had cyclotron-produced plutonium, and the weapon that would have been based on that was called Thin Man, a long gun-type weapon—like the Hiroshima bomb, which was a uranium-235 bomb. They didn't need implosion. But Oppenheimer started it—started a little project around Neddermeyer. Teller was working on thermonuclear reactions and was a true believer in that, and Oppenheimer set him up doing that. Now, as soon as they got the first plutonium back from the reactor at Hanford, Oppenheimer realized that they were never going to be able, because of the pre-ignition problem, to make this plutonium bomb a gun-type bomb. So immediately implosion was center stage. What do you do? Well, he didn't leave Neddermeyer in charge of that group. He made Neddermeyer senior advisor to George Kistiakowsky. Was Neddermeyer happy? No. But it was Neddermeyer's idea that had been crucial, and Neddermeyer wasn't thrown away. Oppenheimer knew how to play those cards. Once it became clear he had to have implosion, he put somebody in charge of it that would make it happen. If he had needed thermonuclear reactions at that point, I don't believe. . . . Well, actually that did happen; it

happened after the war. And he didn't put Teller in charge of that, either, which caused a lot of trouble for Oppenheimer afterward. He put [Hans] Bethe in charge of it, because he could depend on Bethe to carry it through. Although the original ideas, and actually the final solution, if you like, of the thermonuclear bomb actually came—it wasn't just Stan [Stanislaw] Ulam; Teller's idea was crucial, absolutely crucial.

COHEN: So what you're saying is that you need someone to step in to direct the whole thing?

TOMBRELLO: Somebody has got to say, "Look, it was your idea, but I can't let you run this project." And that's what we had done to Ron. And at some point late in 1992, what happened was that we did get our oversight committee.

COHEN: Was that people outside of Caltech?

TOMBRELLO: No. Well, it included some MIT people. What happened is that Everhart finally realized. . . . In '92 I went to Everhart's Christmas party. My wife had been back East for months, because her mother was dying. So I thought, "Well, I'll go to this Christmas party. I need a little cheering up." Lew Allen [former JPL director] and Everhart met me at the door, grinning at me. I thought, I don't like the look of this. "Do you want something?" I asked. And they said, "Oh, we're so glad you're here, Tom. Guess what?" And I didn't want to know what. "We formed your oversight committee, and you're on it." Oh, how nice! Just what I needed.

COHEN: Now, this was some years after you suggested that they do this?

TOMBRELLO: This was five years after I suggested it, almost six. So anyway, that's when people began to look hard at Robbie's performance. But Lew Allen [head of the oversight committee] just would not admit that we had a problem. Gene [Walker E.] Giberson, from JPL, was putting in a lot of time trying to get the LIGO project on kind of a—

COHEN: Was he on this committee?



TOMBRELLO: He was put on this committee, but he was also working behind the scenes to try to organize things without rubbing Robbie the wrong way. The LIGO project was just chaotic, because there weren't enough people. It was growing fast, but not in people. And Robbie, at the first oversight committee meeting—I mean, this is a perfect example: [We asked him], “What’s your biggest problem, Robbie?” [and he said], “NSF’s not giving me the money fast enough.” Well, Neugebauer and I were sitting there sharing the balance sheet. Neugebauer ran his finger down, and I followed the moving finger, and he said, “Robbie, you seem to have \$20 million you haven’t spent yet.” And Robbie just went into a tirade. It was clear that Robbie was not spending the money. They didn’t have enough people to spend the money. All these big contracts were supposed to be let.

Now, by the way, the first thing I want to tell you is that all the prototyping work done by that team was superb, and the stuff still exists. If you go up to [the two LIGO sites], Hanford [Washington] and Livingston [Louisiana], you can see it. Those guys made beautiful technical decisions. They were a brilliant and effective prototype team; the problem was that they weren’t able to grow beyond the prototype stage. But the decisions they made were brilliant—innovative, cheap, absolutely wonderful! All of them deserve an enormous amount of credit. LIGO wouldn’t even exist without those people, including Robbie. But then this thing went on until—more and more trouble. Lew Allen didn’t like the way I was behaving on the oversight committee, because I kept saying, “This project’s in deep, deep trouble.”

COHEN: And Lew Allen didn’t want to believe that it was in trouble?

TOMBRELLO: He wanted to try to fix it.

COHEN: Was he a big fan of Robbie’s, by the way, because Robbie had been so involved with JPL?

TOMBRELLO: It wasn’t clear. Well, I tried to explain it to him: “The problem is, you’re a general.” He said, “Yeah, I was a general. Why is that a problem?” I said, “Well, you know, I think I understand generals. They’ve got a commanding officer, and the commanding officer says to the general, ‘Go take that hill.’ The general says, ‘Yessir,’ and goes to try to take the hill.

I'm a scientist-professor. If somebody says to me, 'Go take that hill,' my reaction is, 'Why? Why do you want to take the hill? Are you sure you've got this right? Maybe you don't need that hill. Maybe you should go around that hill.' That's the difference: A general says, 'Yessir, Boss,' and the professor says, 'Why? Give me a good reason. Let's think about this.'" And I said, "There are places where each is needed, but right now you're trying to take the hill with what you've got, and I'm telling you, 'You ain't never going to take no hill with what you've got.'" "

COHEN: How did Allen take this kind of criticism?

TOMBRELLO: He laughed. He has a great sense of humor.

COHEN: Yes, he's a nice man.

TOMBRELLO: So Charlie [Charles W.] Peck then came into the job of chair of the PMA Division. And I wrote a little white paper for Charlie describing what I thought was wrong with the project, which was basically what I told you: Basically, they were not spending the money. That was a symptom. The Drever thing was also a symptom. But the group was so flustered and running around trying to do too many things that they considered Drever just to be off the wall, and in the way, and keeping them from doing these fifty different things that they weren't making any progress on. Everhart never understood that. He never could understand Drever as a symptom. The money was another symptom. So it rested there. I was sort of at odds with [Lew Allen], the head of the oversight committee. He kept telling me, "You're pontificating," and probably I was, but I thought I had the answer and nobody was listening.

So Charlie came in. I had written this white paper, and Charlie ignored it. That's OK. But another thing happened. Neal Lane became head of the National Science Foundation. Now, I had known Neal a long time, but I knew the guy he had as science advisor, Karl Erb, even better. He had worked for Allan Bromley when Bromley was science advisor, over at OST [Office of Science and Technology]. Bromley continued to be the science advisor and planted Karl at the NSF to provide kind of a slightly lower level of science advice. Karl was a very bright fixer. So Karl came out. He said, "Tom, I really need to know the story of this." I said,

“Karl, my lips are sealed. This is Caltech stuff, Caltech business. I can’t say a word about any of this stuff.” He said, “That’s too bad. The new [NSF] director, whom you really admire, is in there, and he signed this big IOU and he’s done it on faith that Caltech and MIT are going to do this right. But it doesn’t look like you’re doing it right.” And I said, “Well, there’s not much I can do. But that is really too bad, because sitting in the middle of this table in a manila folder is a draft of a report I wrote for Charlie Peck analyzing the LIGO situation.” And he said, “Boy, I’d sure like to see that report.” I said, “Can’t do it, Karl. But I’m going out for a cup of coffee. Would you like to join me for a cup of coffee?” And he said, “Oh, no, no. I think I’ll just sit right here.”

COHEN: [Laughter] Bad, bad, bad!

TOMBRELLO: And I said, “Well, Karl, it might take me about fifteen minutes to get back. I’d really like to have a cup of coffee.” And he said, “Oh, no. That’s fine. Just take your time, pal.” I got back and Karl’s just sitting there with a deadpan look. I said, “Well, Karl, it’s been a pleasure, but I wish I could help you.” He said, “I’m sorry you can’t help me,” and just smiled. And then the next thing that happened was that in January they scheduled a meeting at the NSF.

COHEN: And what year are we in now?

TOMBRELLO: We are in early ’94. I don’t remember the exact date, but the date is a big date, because I turned on the news and looked at it, went across the street from the hotel and got some breakfast and came back—it was early in the morning—and got this phone call from my wife. She said, “I’m all right.” I said, “Of course you’re all right! Why are you up so early in the morning?” She said, “You haven’t been watching the news.” I said, “I did watch the news—nothing.” She said, “Well, we just had one hell of a big earthquake.” [Laughter] The Northridge earthquake [January 17, 1994]. She said, “There are no problems here. I just wanted to call you while I could still get a line and tell you I was OK.” Then I turned the TV on, and of course there it was. [In Washington,] we had just had an ice storm. The streets were covered with glare-ice. But the meeting just ground down into a . . . It was clear that Robbie was losing it, and he knew that somebody was going to pull the plug.

COHEN: Now, at this point was he yelling at the NSF people? Was that happening yet?

TOMBRELLO: He had yelled at the NSF before that. At this meeting, he was much more subdued. He realized that he had gone to trial. And all he was waiting for was what was going to happen next. It was very clear that what was going to happen at this meeting was that Robbie was going to get sacked. And this was never said. It was interesting: Lew Allen and Ned [Edwin L.] Goldwasser—who was brought in from outside, from Fermilab [Fermi National Accelerator Laboratory]—the three of us set out for the airport, driving zero miles an hour, because the ice was very slick. And Lew said, “All right, Tom, I pushed that old car to the top of the hill, and I thought it would start on the way down. And then I found another hill, and I pushed it to the top of that hill. I’ve pushed it to the top of a lot of hills now, but we’ve gone to the top of our last hill with Robbie.” I said, “I really admire the patience you’ve shown, Lew. But we can go back to my original comment about the difference between generals and professors.” He really tried. He and Giberson put everything they had into trying to make that project successful, and they just couldn’t do it.

So then we sat there—the plane had its wheels frozen to the runway, so they were de-icing our plane. So we sat in the bar and watched our plane being de-iced. And Goldwasser said, “Well, who’s going to run LIGO? You?” And I said, “I could run it, but I’d have to spend the first year explaining to people that I could run it.” And I said, “We’ve got a very good choice. Because the SSC [Superconducting Super Collider] has been canceled, I think what we should do is just get [Barry] Barish to run it. Barish has the credibility to run this project, and he’d do a superb job. I think everybody will want that, and he won’t have to spend one minute explaining that he can run a project. Besides, I don’t want to run the project. My specialty is running things like that. I’ve been there and done that on a smaller scale, and it’s not what I find interesting.”

So we got back, and that weekend Barish was presented with the thing, Robbie was presented with the opposite, and Robbie was kept on to run the detector group. And subsequent history, which you can get from a lot of people, is that Robbie gradually. . . . [Sigh] Robbie runs tribes well, but he doesn’t run teams well.

COHEN: But when did Ron [Drever] get taken off the project?

TOMBRELLO: Ron got taken off the project probably in '92 or '93—somewhere in there [July 1992—ed.].

COHEN: Because that's what started the campus turmoil—they weren't knowing these details.

TOMBRELLO: Well, then we can go into what the Academic Freedom and Tenure Committee did wrong the first time. Ron supplied them with a series of memos from Robbie and Neugebauer that, if you looked at them, were an absolutely damning case for Jennings, Neugebauer, and Vogt. And the AFTC [Academic Freedom and Tenure Committee] found for Drever in the situation. Now, Neugebauer cried foul, Robbie cried foul, because what had happened was Ron had selected the memos. There were memos in between that he left out. And the AFTC didn't bother to check. I've been on the AFTC subsequently, with people who are just much more careful, like Roger Blandford and Jennings, some of the old pros around here. And I'll tell you, when we have a case, it is done very carefully. You start a timeline, and you start filling in the dates on that timeline: Who said what to whom? Was that before this or after this? How do you know it was before or after? If somebody was supposed to have made a phone call, whom did he call? We should call that person and see when that phone call was made and what was said, and fill in this diagram. But what happened with that first AFTC thing was they had gaps in their timeline and didn't bother to check. The next AFTC thing, which also proposed a solution, I think did a much more careful job.

It really was a bad show. It was a very bad, bad show. There were many things that. . . . As I say, I had no qualms about getting Robbie out of the project, and I had none about putting him in, but I knew when we put him in that there was always a chance that we'd have to take him out. As I say, I've been in industry for a few years, and you have this in industry. People will get to some point that other people couldn't have reached, but they can't go farther, and they usually find somebody to carry it to the next stage. I don't think Barish could have gotten that project together or gotten it funded. But I do believe that Barish is the guy who can build it—and has. What will happen if Barish ever comes up short? Well, I hope he doesn't, but if he does, the oversight committee is perfectly capable of pulling him out.

COHEN: Yes, and he would appreciate it, I believe. [Laughter]

TOMBRELLO: Barish is sane. He's coherent. He's very constructive. LIGO has become the model for big projects on the Caltech campus. If I could rent them out to solve the problem we're having with reengineering, they'd solve it. They're a really good bunch. They're very orderly minded. Would this be the group to have the original dream? No, you need a Drever. You had to have the mad scientist—the man with the dream. Could they have gotten it funded? No, you had to have this driving personality. Robbie had enormous vision. Robbie could paint that vision for people in Congress. Robbie could go to Washington and basically sell hostile agencies [on the idea that] this thing was the future. And I truly believe it is the future for Caltech. LIGO and the Keck Telescopes are, at the moment, the big-ticket items for twenty-first-century physics at Caltech.

COHEN: Let's just go back a bit, Tom, to some of the bitter feelings of people. How do you attribute this? I mean, it's been said that some of these people have had such bad doings with Robbie that they never looked at the case; this was just a chance to get him. I've heard that, too.

TOMBRELLO: I think, having been there and having many scars from having to deal with Robbie—as I said, things got really nasty between Robbie and myself. It really was bad. Murph [Goldberger] was dragged into it. Murph was ineffectual. I eventually solved the problem in my own way. It turned out to be a standoff, considering I wasn't holding any cards. These things happen. But Robbie did make enemies. He had a bunch of enemies. And Robbie would sometimes think he knew the answer to things and would cram it down people's throats. Robbie has two states. One state is brilliant, visionary, and extremely incisive about how things are put together. I use Robbie a lot these days to dig into things for me in an investigator mode. He can get answers no one else can get. He can get people to talk to him. A brilliant, brilliant man. But he does have his black moods, and in the black moods everybody is Robbie's enemy. And if you ever get to be Robbie's enemy—and people have—you see another side of him, a very different side. And I think a number of those people became convinced that Robbie was just doing to Drever what he had tried to do to them, sometimes successfully and sometimes not so successfully.

COHEN: Now, you're really blaming this on Everhart. Is this because he wouldn't take a look at it?

TOMBRELLO: Everhart is not a deep man. He's just not a deep man. He didn't ever want to look at the details. And I think that's true of a lot of things around Caltech during the Everhart years. He never looked much below the surface. I can't blame Everhart for the trouble. The troubles really were Robbie's. Robbie had done brilliantly up to a point and failed to get to the next step. It was Everhart's job, as somebody over that, to help recognize that, or at least to understand that this was one of the things that goes wrong with big projects. Places like Caltech have a lot of big projects. And we cannot afford to look stupid in the eyes of the world when we do them, because we want to do more of them.

COHEN: Now, that raises another question, because there are people who feel that a project this big doesn't really belong on a university campus—that it's a JPL-type project.

TOMBRELLO: Sure, JPL can build it for three times as much money.

COHEN: But they wouldn't have the scientific insight?

TOMBRELLO: They wouldn't cut the corners. It would be very different. It would be very much more expensive. And it wouldn't be driven by the dream. . . . **[Tape Ends]**

**Begin Tape 1, Side 2**

COHEN: I see you're a believer.

TOMBRELLO: I'm a great believer in Caltech having visions that are bigger than other places and being able to carry them out.

COHEN: OK. So I've been calling this period of three or four years "the controversy." It's interesting. People pick this word up from me, because it's a good word—the controversy. So we get over this, and Barry comes in. I have already spoken to Barry, and I have an appreciation of how wonderful he is. How do you think the institute feels now? Or are they just glad not to think about it?

TOMBRELLO: I think there's certainly an element of that. I think they're all waiting for something to spring out of LIGO and embarrass us, but it's not going to. I think people are joining the project, voting with their feet. Ken Libbrecht has joined the project. Alan Weinstein has spent some time with the project. Tom Prince has joined it. This makes Caltech very different from MIT.

I have to tell you a little MIT story. This was in 1995. We had our last visiting committee with the PMA Division. I was invited to the dinner at the end of the first day. Millie Dresselhaus [MIT Institute Professor of electrical engineering and physics], whom I've known for years and who is one of our trustees—

COHEN: She works hard.

TOMBRELLO: Yeah. Oh, god, she works hard! Sometimes not to the right purpose, but she works hard and means well, and I like her.

COHEN: Just as an aside, we saw her on the way to [Caltech President] David Baltimore's inaugural party. She was holding books and looking in the dark, wondering how she was going to cross California Boulevard. Well, she's just got so much work to do that she can't put it down for a minute. [Laughter]



TOMBRELLO: She's on two of my committees at Livermore [Lawrence Livermore National Laboratory]. She's tireless. Some people might say "tiresome," too [laughter], but she means well. She really wants to get it right. But anyway, at the dinner she says, "You're going to buy me a drink." "Yes, Ma'am!" So we go over to the Hayman Lounge [in the Athenaeum].

COHEN: Now, what year was this?

TOMBRELLO: This was spring of '95. Barish was running the project. I want to show you the difference between MIT and Caltech in LIGO.

Millie says, "Let me see if I've got this right. Caltech, especially you, seems to think LIGO is the centerpiece of big twenty-first-century science. Caltech is totally committed to it. You have faculty members who want to join in on this. This is a big community thing that you're all behind." And I say, "Well, maybe not everybody, but it really does have strong backing at Caltech from the top to the bottom of the place." And she says, "Well, let me explain about MIT. At MIT, LIGO is Rai Weiss's project, and that is all it is. Whether you agree or disagree, I just want to make sure you understand that that's all it is at MIT. It is not some big centerpiece of twenty-first-century science. It is not something that we've sworn on our sacred honor to defend and make work. It is Rai Weiss's project, and don't forget it."

COHEN: That's exactly right.

TOMBRELLO: I said, "Ma'am, I had already figured that out, but you have now put an exclamation point at the end of my sentence. We understand one another," I said, "but don't ever assume that Caltech is in the MIT mode. We're true believers in this project. We're going to make it work. And that may be hard to do." But by 1995 we realized we were on the road to recovery.

COHEN: It's interesting. From Fred Raab, I got some sense of how people in the project felt—I mean those nameless people who were there. I mean, you only heard what all the professors had to say.

TOMBRELLO: Well, I have to talk about Raab’s tenure committee. All this is sealed for a while. In 1993, Charlie [Peck] came in [as chairman of the PMA Division]. Raab [then an assistant professor of physics] had been reappointed, with some difficulty, and Barish had been chairman of that committee. This was now the fall of ’93, so Barish was not part of LIGO at that point. Charlie said, “I want you to run the Raab committee.” And I said, “Barish can run it. He ran the reappointment committee.” And Charlie said, “Barish doesn’t want to do it. Barish will be on it, but he wants you to run it.” And I said, “Let this cup pass. This is not a winner. The physics faculty is totally polarized.”

So I got the committee together. I had no idea what I had in front of me. I said, “I want a secret ballot on what you feel now about this case.” Barish rolled his eyes; he did not like this. I said, “I don’t care if you don’t like it, Barry. I want a secret ballot. I want people to write on these little pieces of paper I’ve made up that you’re either for, neutral, or against [tenure for Fred Raab].” This was before we had done anything.

COHEN: How long had he been here?

TOMBRELLO: He had been here for six years at that point. This was a tenure committee. He had just sort of squeaked through on the reappointment; mainly [he was] not being very productive. And I looked at it, and I only got one negative vote. This was like a bridge hand, where you had to figure out where the missing trump was. I called it wrong, because only much later I realized—I was told—that I had called it wrong. But I knew I had a committee that was mostly for tenure, at least at that point. There were a few that were neutral, including me, and one against. I thought it was Bob McKeown; it turned out to be Ken Libbrecht.

That committee worked their tails off. I said, “I want this to be a totally defensible case. We do not have a lot of publications from [Raab] at this point, but he has written a lot of stuff for LIGO. He has done a lot of things. I want a detailed analysis of everything that he’s done.” And I jokingly said, “I want you to read the stuff that’s stuck to the bottom of his wastebasket.” And Libbrecht in particular did that. Libbrecht really worked harder than anybody I had ever seen. And I would go to Raab and say, “Look, I’m on your side. I don’t know what the answer to this thing’s going to be, but I’m going to tell you to do things and you are going to do them, because I don’t have papers I can show them. If I want something on dual recycling or power

recycling, I want you to write me something on it. And I'm going to circulate it to the committee." And so Raab did a lot of work. We basically took a vote, after we had worked for months. It was a good thing Barish wasn't chairman, because now he was running LIGO. We were unanimous for tenure. OK. Then I went to Peck and said, "Here's what we got. Here's the report. I think you should circulate it with all his backing material." It had appendices. I said, "I want it circulated to the faculty. I want an informed faculty. And you have to tell them that they are expected to read this stuff, and when they come to the meeting they're not going to just come there and vote—they're going to come there knowledgeable about what's gone on." And Charlie is made of good stuff. Charlie said, "They'll do it." And they did. We scheduled two meetings with, I think, a week between them. And now my committee began to come apart. Libbrecht came to me and said, "I have got to change my vote for the following reasons."

"Fine, Ken."

Then he said, "You know, I was never in favor of this."

"Oh," I said. "It was you, it wasn't McKeown."

So then we presented the case. Several more of the committee bailed out, not having informed me, which I didn't entirely approve of. It's one thing if somebody comes to you and says, "Look, I've worked hard on this. I really understand it. Yes, I voted. Yes, I signed this thing. But I'm going to have to pull back." I have no problem with that. This was a complicated case.

So it got presented. There was a week between, when people had a chance to ask more questions and fill in blanks and things. And people were extraordinarily responsible. There were no crazy remarks. There was no invective. It was all handled in a very collegial way. It was impressive. When push comes to shove, we really can do it. And then at the next meeting, Charlie said, "I want a secret ballot on this." Nobody argued with him. And it came out at about two-to-one against Fred.

Fred came to me afterward and said, "I have no complaint with the process. I feel I've been dealt with fairly. I wish it had come out differently." I thought to myself, That's all I was trying for. In fact, I really would have voted against [tenure]. But I felt if I had voted against it—if I had started the committee off with that—they wouldn't have worked as hard and we wouldn't have had as much discussion.

COHEN: Did it come out that way just because of his publishing record? Because he had some strong feelings about that, which he has told me about, actually.

TOMBRELLO: That certainly was a part of it. I can't analyze to what extent there were votes on the basis of his being part of what they considered to be an evil empire [LIGO]. I can't measure that. All I know is that everybody behaved, at that time, very, very well. But it was not just counting his publications, and he knows that. We really worked. As I say, Libbrecht was down in the bottom of [Raab's] wastebasket, reading everything Fred had done. And Libbrecht was the first one to bail out, saying, "I can't support this case." It was close. On the committee, [the outcome] was much more positive than in the division. But it wasn't that ten people out of fifty were against it, it was thirty or thirty-five out of fifty who were against it.

COHEN: But was it at that time, then, that they asked him to [stay with LIGO]? Or was that later?

TOMBRELLO: No. Barish realized that [Fred would] make a good site manager, but I think it took a while to work that out. And I think he's going to be a very good site manager.

COHEN: Oh, he loves it.

TOMBRELLO: And Mark Coles [head of LIGO site in Livingston], I think, is going to be extraordinary in Louisiana. I don't know if you've talked to Mark.

COHEN: No. I don't know him.

TOMBRELLO: I knew Mark at Schlumberger. Schlumberger is one of these big global—

COHEN: I don't even have his name written down anywhere.

TOMBRELLO: OK. You need to talk to Mark.

COHEN: So now, let's get into the actual project itself. Evidently Barry is really running a marvelous project. Do you think it's going to work?

TOMBRELLO: Oh, well, that's a different question. Do I think it's going to detect gravitational radiation right off?

COHEN: Oh, I think even Barry isn't expecting that.

TOMBRELLO: No, I don't think so. I think we're a decade away.

COHEN: A decade?

TOMBRELLO: Yeah, that's what I believe. And that may be optimistic. I think they're going to have to sharpen their tools, unless they're lucky—unless the gamma-ray bursts carry a lot of gravitational energy, they come often enough, and you can correlate them in time with something somebody has measured. That's sort of the wild card, and maybe we'll draw that wild card. But do I think it will operate in a manner that we'll find to our credit? Yeah, I think it will. I really do. If I didn't, I would, by god, start jerking it around until I could have that feeling.

COHEN: Now, given that this controversy is now five years behind us, do you think it left some permanent scars on the Caltech community?

TOMBRELLO: Sure. All these things leave scars. There are people who don't feel as warm and friendly toward Peter Goldreich as they used to. There are people who really don't have much respect for Steve Koonin, because Koonin was the chair of the first AFTC committee, which did, I think, a really bad job. They felt he was somebody who just was in a hurry and didn't do his homework and that he took a convenient, superficial look at this. And the AFTC shouldn't be superficial, and it shouldn't be convenient. And I think Robbie clearly has—

COHEN: Well, he still acts wounded much of the time.

TOMBRELLO: Yeah.

COHEN: He refuses to come in and talk to me about this, although he was happy to talk to Jane Dietrich [editor of *Engineering & Science*]. [Laughter]

TOMBRELLO: Robbie's a very complex man, and a very interesting man. It's an interesting relationship that Robbie and I have. I think, having fought each other to a standstill, there's a certain amount of mutual respect. We also tend to think of one another as people who know a lot about other stuff that other people don't bother to know about, and some of those things are important to know—for example, things connected with the weapons labs.

COHEN: So you've never really been bothered by the fact that suddenly Caltech and the physics community is burdened with this huge, high-cost project that wasn't supposed to be that way?

TOMBRELLO: Well, I never thought it was going to be built cheaply. I mean, I never thought it was going to be built for \$100 million. Do I feel burdened by having the Keck [Telescopes]? Absolutely not. I feel like a hog in clover; I'm just worried about the competition building their own versions if they can. Oh, no! I think that Caltech has got to pick and choose wisely. I don't think we should pick every damn project that comes along. Some projects are on a scale that Caltech can't handle, like a major high-energy physics project.

COHEN: Well, no one gets to—

TOMBRELLO: No one can do that anymore.

COHEN: No one country. [Laughter]

TOMBRELLO: I remember when [Robert F.] Bacher [chairman of PMA Division, 1948-1962] turned down the design-study thing that Matt Sands was running, and that was the machine that turned into Fermilab. That's the reason Matt Sands left Caltech [in 1963, to become deputy director of the Stanford Linear Accelerator—ed.]. His nose was out of joint. He was mad. Bacher had looked at it carefully and said, "This is too big for a university, or even a couple of

universities, in Southern California.” And that was sort of the 200-GeV version of what became the Fermilab machine. No, I think Caltech should pick and choose wisely, but we’re going to have to keep our project.

COHEN: So you think that’s OK?

TOMBRELLO: I think it’s the future. Caltech can either choose to be Amherst or we can continue to be Caltech.

COHEN: Now, how do you feel about the new president [David Baltimore]? Does he have an appreciation for this project?

TOMBRELLO: Yeah. The man has style, he’s brighter than hell, he has vision, and he wants to make things happen. It is such a pleasure to have a president that you don’t drag. But I think we may occasionally have to restrain him, because he gets so optimistic. We’ll have to say, “David, David, I don’t know if that will fit at Caltech.” It is such a pleasure to do that rather than try to wheedle him into things. David is probably always going to have an easy time. David’s very busy. He’s a strong man. People claim he’s arrogant. Well, if he is, he’s—

COHEN: Well, that wouldn’t show at Caltech. [Laughter]

TOMBRELLO: It wouldn’t show at Caltech at all. [Laughter] He’s somewhere on the curve at Caltech, but he’s not at the top.

As far as I’m concerned, the [search committee] did extraordinarily well. I despaired that they would find somebody like that. I had suggested him in ’87, which would have been a disaster, because that was before all this other thing [accusations of scientific misconduct made against Baltimore’s collaborator Thereza Imanishi-Kari—ed.] blew up, and Caltech would have had to suffer through ten years of that. But I never thought they would go back to him. A friend of mine worked closely with David for years—in fact, they were roommates at Rockefeller, in graduate school—a guy named Harvey Lodish, who is now a professor at MIT. He’s at the Whitehead [Whitehead Institute for Biomedical Research]. In fact, he and David started the Whitehead. He had convinced me that David—after Rockefeller [University] and the trouble

with [Representative John] Dingell [chairman of the oversight subcommittee of the House Energy and Commerce Committee]: “David running a university? Oh, no, David’s much too smart to do that again.” [Laughter]

COHEN: This is a very good place for him, I think.

TOMBRELLO: It’s sort of like Harold Brown’s situation. Where else could Harold Brown have gone in 1968-1969? Only Caltech.

COHEN: Now, you know, Murph [Goldberger] was terribly enthusiastic, too, but that was different, evidently.

TOMBRELLO: I’ll give you my candid analysis of Murph, because Murph is very bright and very enthusiastic: Murph was a gifted amateur president. What he wanted to do, he did exquisitely well. But if he didn’t want to do it, he didn’t do it, and he didn’t pay attention to it. That was Murph’s problem. Also, his people skills were highly variable. They were mostly extraordinarily good. He had a good political touch—going and riding the elephant for the undergrad stuff.

COHEN: Well, the staff loved him. He’d go with them to their parties.

TOMBRELLO: Murph had style and he was smart, but he was not a full-time president. And Baltimore has trouble. It’s not because he’s an amateur, it’s because he’s a professional at too many things at once. He’s got Caltech, he’s got his own research group, and he’s got this AIDS vaccine program in Washington. Any one of those would probably keep most people occupied full time. But as Harvey, my friend at MIT, said, “Well, he’ll probably pay about the same amount of attention to the research lab at Caltech as he did at MIT.” [Laughter] I said, “Well, that was his undoing once. I think he’d better be careful.” But I like him. I like Alice [Huang, his wife]. I think they’re going to fit here. They do fit here.

COHEN: Yeah, when we see them. He wasn’t at the party for retiring physics professors last night, which was quite nice. Was he there for a moment at the—



TOMBRELLO: He said he would come at the beginning and that that was the best he could do, but that he would show the flag. I said, “David, that’s all I need. Sometimes I need you to show the flag.” And he does that. If he has a failing, I think it’s that he’s not getting out and walking the halls.

COHEN: Do you want to know what I think? He can’t see. I think it’s hard for him. He has walked by me several times, and I’ve realized—he has done this to Bettyann [Kevles, the wife of Koepfli Professor of the Humanities Daniel J. Kevles]. I don’t think he sees.

TOMBRELLO: That could be.

COHEN: Have you seen the thickness of his glasses?

TOMBRELLO: He needs to go out and meet professors one-on-one. Harold Brown used to do that. He’d go to see a junior faculty member. The junior faculty member would look up and there was the president of Caltech:

“What do you want, Harold?”

“I want to come in and talk to you about what you’re working on.”

“What have I done wrong, Harold?”

“Not a thing. I want you to tell me about what you’re doing.”

So the faculty member would tell him. Brown hadn’t been a physicist in years, but he’d ask you the one question that you could have told him to ask you if he wanted to nail you to the floor. He knew. The guy had just been there and done that, and he knew how to ask those impossible questions. He’d ask you a question, and you’d sit there and think, “Yep, you put your finger on it, Harold. If there’s anything wrong with it, that’s it. I’m going to have to dig my way out of this one.” Harold was one of the quickest human beings I have ever met.

COHEN: Smart, smart.

TOMBRELLO: No. I’ll give you my declension of smart: quick, smart, and wise. Quick: Harold could do it [snaps his fingers] like that. Smart: sort of knowing some of the time that you shouldn’t be so quick. And wise: people like Bob [Robert P.] Sharp [chairman of the Geology

Division, 1952-1968]—cagey and can see way down, many moons deep, on anything. What Sharp had in his favor, too, was that he could play that good ol’ boy thing. Eventually you learned that you had to be very careful when Sharp was good-ol’-boying you, because something was going to happen, and you might not want to be swept away by Sharp and his folksy stuff. The measure of Bob Sharp and how good he was was that he built a division here that, years after he took his hand off it, continued to move in a straight line. Granted, after twenty or so years it began to run off the track. And Ed [Edward M.] Stolper [chairman of the Division of Geological and Planetary Sciences] is putting it back on the track. But Sharp really planned for the long term. Bob Bacher did a lot of that, too.

COHEN: But when [Robert A.] Millikan [Caltech’s head, 1921-1945] used to trot around looking at people in their labs—because I’ve heard that in some of these interviews—they’d look up and here was this man and they didn’t know who he was.

TOMBRELLO: It’s very important to do that.

COHEN: So anyway, you’re optimistic that LIGO’s going to go.

TOMBRELLO: I’d be happier if I knew it was going to detect something in a decade, but I can’t expect that.

COHEN: So you think Caltech, at the moment, has come out strong in spite of all this commotion?

TOMBRELLO: The LIGO commotion? Yes. We’ve come through that and come out the other side without too many scars. Whether we can get rid of the bureaucracy that built up under Everhart, which is just a total mess—particularly the financial side of things—that’s. . . . See, Everhart never really. . . . My one complaint about him, to him, was “You don’t supervise the people who report to you. You don’t know what they’re doing, and they’re building these bureaucratic fiefdoms, and it’s just an impenetrable bureaucratic mess, and Caltech is not used to that.” And Everhart left that as part of his legacy. He did some good things, too; they weren’t all bad. But Baltimore has now got to address this question of a reengineering project that has

gone belly-up and burned tens of millions of dollars. And I don't think anybody has any real conviction that it's going to work. It was run in a very nonprofessional way, and those of us who said that at the beginning were told to get out of the way, we were disturbing the troops.

COHEN: We can't even get a new personnel directory. [Laughter]

TOMBRELLO: Oh, god, yes!

COHEN: OK. Well, thank you. [**Tape is turned off**]