ROY W. GOULD
(1927-present)

INTERVIEWED BY
SHIRLEY K. COHEN

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Electrical engineering, physics

Abstract
Oral history interview in six sessions in 1996 with Roy W. Gould, Caltech Professor of Electrical Engineering, Physics and Applied Physics, 1955-1996 (emeritus 1996); Chairman, Division of Engineering and Applied Science, 1979-1985; and Caltech alumnus (BS, 1949; PhD, 1956). Gould describes his youth and student years at Caltech, beginning in 1944; Caltech during World War II and interruption of studies, resumption in 1946; courses in engineering and physics; BS in engineering. Graduate work begins at Stanford on microwaves with Lester Field; he returns to Caltech for a PhD in physics on microwaves and solar radio noise. Discusses microwave electronics community in the 1950s; J. Pierce, A. Haeff; recalls the "Tube Conferences." Job offers in industry but chooses Caltech, where he receives joint appointment in electrical engineering and physics. Recalls electrical engineering program at Caltech in the 1950s with C. Papas, G. McCann (analog computer), C. Wilts. Describes beginnings of his interest in plasma physics and thermonuclear fusion (late 1950s); connections with European plasma physics groups. Assumes directorship of Atomic Energy Commission [AEC] fusion program and moves to Washington; offered position of Deputy Science Advisor to President Nixon; returns instead to Caltech (1972). Builds tokamak at Caltech; fusion later becomes "Big Science." Birth of applied

physics program at Caltech; its history. He discusses engineering division at Caltech; its diversity; his tenure as chairman. Recalls the rise of computer science and roles of C. Mead, I. Sutherland and C. Seitz. Comments on Caltech presidents, especially Goldberger and Everhart; changes in Caltech over the years. Epilogue 1998: on Gould's return to earliest interest, amateur radio.

Administrative information

Access

The interview is unrestricted.

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Gould, Roy W. Interview by Shirley K. Cohen. Pasadena, California, March-April, 1996. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Gould_R

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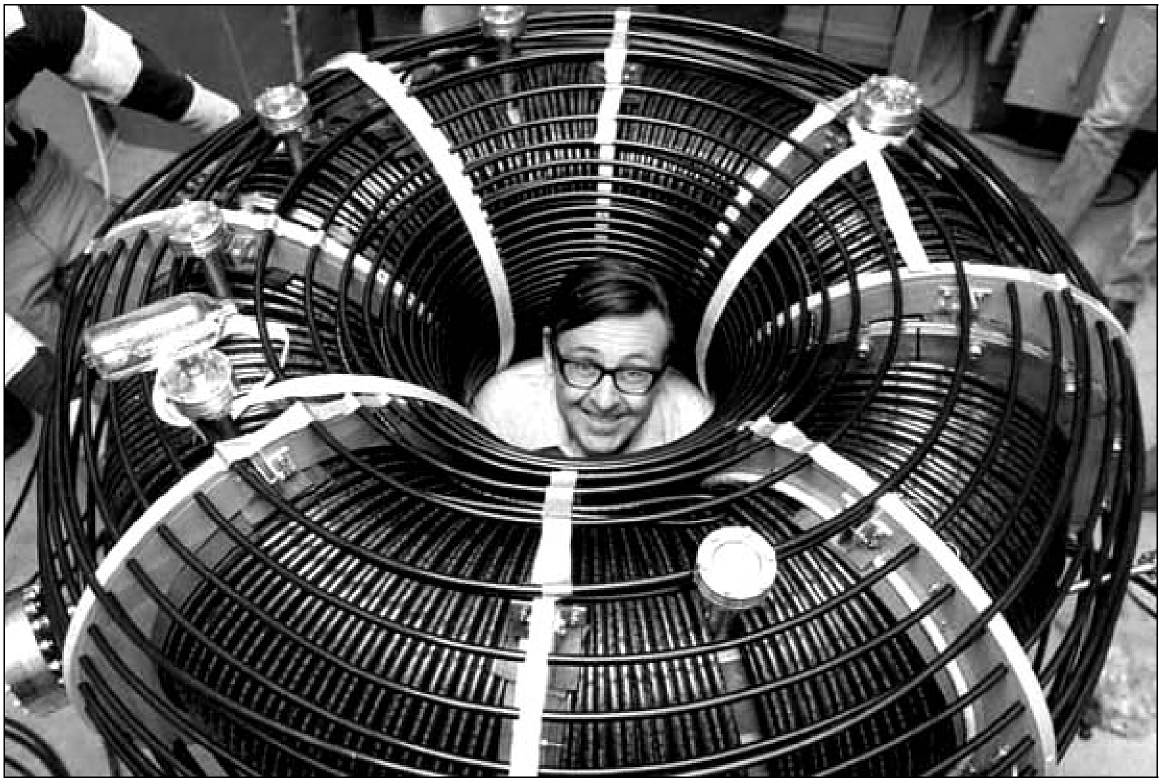

All wrapped up in his work, Roy W. Gould gets the feel of a newly constructed device, the *tokamak*, to be used for experiments concerning heating mechanisms in plasmas—work with applications for the production of energy through nuclear fusion. Coils of the *tokamak* are wound around a stainless steel vacuum chamber where the plasma heating takes place. These coils create the magnetic fields necessary to produce and confine the plasma. Photo *Caltech News*, February 1976/Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ROY W. GOULD

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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COHEN: Let's start with your childhood.

GOULD: Well, I was born in Los Angeles in 1927. My parents came from out of state in the early 1910s, or something like that.

COHEN: Where did they come from?

GOULD: My father came from Michigan, my mother from Colorado.

COHEN: Did they meet here?

GOULD: Yes, and they were married here in California. I lived for some time in Montebello, and I was in school there—I vaguely remember the school building being destroyed in the 1933 earthquake and going to school in a temporary building. But then early on I moved to Colton, which is about fifty miles east of here, and most of my education—grammar school, high school, and so forth—was in Colton.

COHEN: Now, why Colton?

GOULD: My father's mother had a small ranch in Colton. My mother's parents lived nearby, in Ontario. I'm not quite sure what happened, but at some point we ended up living in my paternal grandmother's house. My father eventually inherited it, and that was my home for a good part of the time—through elementary school, junior high school, and into high school.

COHEN: That must have been mostly country out there then.

GOULD: Yes, it was orange groves. In fact, the ranch was originally a small orange grove. My father eventually started a chicken ranch on some of the undeveloped property. He knocked around, had a lot of jobs.

COHEN: These were the Depression years.

GOULD: Yes, and we were not very well off. He did a lot of things. He drove a gasoline truck. He took over the ranch when my grandmother died. He wasn't college-educated; neither was my mother. So I was the first generation to go to college. I don't know how it happened, but somebody in the family had heard about Caltech and said, "You know, that's the place you ought to go."

COHEN: You must have been a very good student.

GOULD: No, actually I was not a very good student. [Laughter] I guess I was a satisfactory student. In high school, I was really not a very strong student grade-wise. I didn't really work; I didn't pay much attention. I had a physics teacher who was the track coach—not very good in physics. The chemistry teacher had taught my father chemistry, and I had a mathematics teacher of similar age—although she was far and away the best of the three. I had a junior high school science teacher who was very supportive and stimulating—much better than any of my high school teachers.

But what really got me going was that this ranch had a big barn, and my uncle—my father's brother—had left a lot of his old radio equipment and magazines from the 1920s and 1930s in it. I used to go up there and read these things and look at the equipment, play with it and stuff like that. I got started in electronics with crazy experiments.

COHEN: You worked by yourself?

GOULD: Yes, yes. But then as I grew up, I was serious enough to get into amateur radio. And I'm not quite sure how that came about. There were a couple of students in high school whose fathers were amateur radio operators, and I had some help from them. I built a receiver. And I met a man who lived about a mile from me who was an amateur. It also turned out that he was the chief engineer at the local radio station. I got to be very good friends with him. This was in 1940. And I got my amateur radio license just before the US entered the war, and just before they shut down all these amateur radio transmissions. The local radio station was having a hard time getting engineers, so I got a commercial radio license and went to work at the radio station, while I was still a high school student. So that's how I got started.

COHEN: And that was more interesting to you than what was going on in high school.

GOULD: It was *much* more interesting. Of course, I had a few friends who had similar interests. It was these friends, and also the people I met in the amateur radio fraternity, so to speak, that really got me going strongly. But my mathematical preparation, my physics preparation, and so forth, in high school was really rather poor, because I didn't work at it. I wasn't a good student. As I remember, I think I had a B+ average; today, that wouldn't get you into Caltech. [Laughter]

COHEN: So you were encouraged to apply to Caltech? Your family encouraged you to go to college?

GOULD: I don't think they knew what Caltech was, but they had heard somehow that Caltech was the kind of place where somebody like me should go.

I did have one very stimulating teacher in high school—the music teacher. She was probably the best teacher I had. I took up the clarinet and played in the high school band and the high school orchestra. Later on, I played in a little jazz band here at Caltech. My interest in jazz continues, and of course my musical interests have grown.

But I was just going to say that my parents really didn't have enough money to send me to Caltech; however, they managed to scrape up enough money to send me here for the first term. And, actually, the first term was not quite a disaster, but really pretty bad for me.

COHEN: Are we talking immediately after the war now?

GOULD: I graduated from high school in 1944. I had skipped a grade—or two half grades—back in grammar school, so I ended up graduating about a year younger than most people do. I was seventeen and a few months when I graduated from high school. I applied only to Caltech, but my fallback position was UCLA, because at that time UCLA's admissions extended all the way into the summer. So if I didn't get into Caltech, I could go to UCLA.

But I did go to Caltech, and I did very poorly my freshman year, because of my lack of preparation. In fact, I did so poorly that I took a term off to recuperate from the experience. This was still during the war, and the [navy] V-12 were here. I was a member of Throop Club and lived off campus, and went home on the weekends. In fact I lived right across from Dabney Gardens, on the second floor of a house on San Pasqual, which went all the way through the campus then. Some of those houses rented out rooms to Caltech students during the war years. I went one term, then took a term off. I don't think I actually failed any courses, but my grades were even worse than they were in high school.

COHEN: Did you have classes with the V-12 people?

GOULD: No, as I recall, they were separate. Throop Club was the center of the social activities, and I met a lot of the regular students at Throop Club. There was very little contact with the V-12 people. I think some of the regular students worked in the student houses, serving food and things like that.

COHEN: So the V-12 were in the student houses, and if you were a regular student you had to rent a room somewhere?

GOULD: Yes, that's right. The student houses were taken over by the V-12. That's why we had to live away. I don't recall, now, whether the classes were separate, too, but I was only a freshman and I think many of these V-12 students were more advanced students, so I had very little contact with them.

I came back in the spring term.

COHEN: Had it been suggested to you that you take some time off? Or was it just your feeling that you should do that?

GOULD: You know, I don't remember that very well. I think it was my own decision. I got Cs and a couple of Bs. And I got an incomplete, or a D, from Wally [J. E. Wallace] Sterling—later the president of Stanford—who was teaching history at that time. History was never a subject that appealed to me. But to do so poorly in a course with Wally Sterling [laughter]—I mean, the best teacher I could have had in the subject and I still didn't do very well! I had Foster Strong for freshman physics, on pulleys and mechanics and things like that. Electricity and magnetism—the things I really had some interest in and some slight knowledge about—came later. And at that time they weren't teaching calculus in high school, so calculus was new to me. I didn't do particularly well in mathematics. So I took some time off to sort of think about things. I went back to my amateur radio.

COHEN: And then you came back in the spring. Was it all right with Caltech for you to take some time off and then come back?

GOULD: It didn't seem to be a problem. I turned eighteen in April. It was 1945, and the war was still on. I didn't want to get drafted, so I enlisted in the navy about a week before I turned eighteen. At that time, a lot of the Caltech students were doing that; if they were eligible for the draft, they would enlist in the navy first. They were sent either to flight training or to radio training schools. I took some kind of test for flight training, because it would lead to an officer position, but I found out that I had a muscular imbalance in my eyes—they had a tendency to converge. And that was a no-no if you were going to be a pilot. It's never really troubled me. That was about the time I started to wear glasses, too.

I didn't get flight training, so I went into the radio-technician program. And they had so many people volunteering that it took them awhile to process us all. So I got to finish the spring term, and they still hadn't called us. Meanwhile, the war in Europe had ended. They got around to calling me in August, just after they dropped the first bomb on Japan. The second one was dropped while I was on the troop train to Chicago, to the Great Lakes Naval Training Center. Very shortly after that, they realized that all these people they had taken in they had to get rid of. I went through boot camp in Great Lakes, but then I volunteered to come back to the Los Angeles area, because my father was sick. He had been working on a construction job up in the Aleutian Islands and had gotten some kind of infection. They had given him a lot of sulfa drugs. Well, it turned out that the sulfa drugs were bad for his kidneys, and he died from kidney damage while I was still in the navy but stationed in San Pedro.

So I spent a year in the navy, and then the GI Bill put me through Caltech. That was important, because I really don't know how I would have managed it financially. The financial-aid situation then wasn't what it is today. And then there was a California GI bill that later put me through a year of graduate studies at Stanford.

COHEN: So after you got out of the navy, you came back here to continue your undergraduate education. And did better, I assume?

GOULD: Well, I did. By now, I had read several books on radio engineering. I hadn't studied any physics or mathematics, but I did a lot of reading while I was in the navy. And maybe I had just matured.

Another important factor was that Victor Neher was teaching sophomore physics. He taught electricity and magnetism and optics. And that's really where things turned around for me—with Vic Neher and that subject.

The next year, I took mathematical physics out of [William V.] Houston's book [*Principles of Mathematical Physics*—Ed.] from Bob Leighton. And that was a wonderful experience. We had George Trilling in the course—he's now at Berkeley; also Kent Terwilliger, who became a professor of physics at Michigan—and two or three other real hotshots. That was a wonderful section. It met every morning at eight o'clock, four days a week. And I lived in one of the student houses when I came back, and I used to get up for the eight o'clock class and then go back at nine o'clock and have breakfast. [Laughter] Bob Leighton's course on mathematical physics was really the heart of what Caltech was and still is to a large degree.

I was an electrical engineering student, and as a result I didn't take [Ralph] Smythe's course. [Electricity and Magnetism—Ed.] Smythe's course was the great terror for undergraduates at that time. It undid a lot of physics students, because it was a very tough course. I was spared that difficulty by being an electrical engineer. I did take a course out of a book by Ramo and Whinnery—Simon Ramo and John Whinnery, who've had some influence on me subsequently—taught by Bob Langmuir. And that dealt with electricity and magnetism, but more—well, the title of the book was *Fields and Waves in Modern Radio*. A year or two ago, I went to a party up at Berkeley celebrating the fiftieth anniversary of the publication of that book, and John Whinnery and Si Ramo were there, along with all kinds of people I've met throughout my career. But that book was almost the only thing in undergraduate electrical engineering that had an influence on me. Because after the war the electrical engineering program at Caltech was really not very good. Royal Sorensen was teaching electrical machinery and transmission lines, and those courses were obligatory. And although I've used some of that material since, and have been very glad I learned it, at the time it was very dull and uninteresting to me.

COHEN: Was that the consensus among the other students?

GOULD: Yes. That's why I went to Stanford as a graduate student. Frankly, the prospect of graduate study in electrical engineering at Caltech was dismal at that time.

COHEN: You didn't think of switching over to physics?

GOULD: Not at that point. Yes, that's a good question: how did I make that transition? Well, it turned out that as an undergraduate I'd had a lot of physics. There was Leighton's mathematical physics course, and I took a course in statistical physics from a low-temperature physicist who was here in the forties—not one of our superstars. But I certainly got interested in the subject.

COHEN: But you graduated in electrical engineering here.

GOULD: Yes, and though I was headed for graduate school, I was certainly not aiming at a PhD at that point. In fact, I must say, I have never really planned my career very much. It just sort of happened. Students today seem more focused on where they're going to get their first job, and they're planning and maneuvering to see where the opportunities are. Certainly that was not my style in those days.

COHEN: So you went to Stanford then?

GOULD: Well, Stanford and Harvard had radio research laboratories during the war. MIT had the Radiation Laboratory and radar. And immediately after the war, Stanford had assembled a very good faculty in microwaves. The Varian brothers were at Stanford, and there were a couple of really hotshot microwave people—Ed [Edward L.] Ginzton and Marvin Chodorow, who I guess were students of the Varian brothers. The Varian brothers were never on the faculty at Stanford, as I remember, but they were very creative people. And, of course, they started Varian Associates, which is now a very big company. Ed Ginzton eventually became the chairman of Varian.

All this microwave stuff was right up my alley. Since you've looked at my thesis, you know that one part of it had to do with microwaves and the other part had to do with radio waves—solar radio waves. So I was strongly oriented in that direction.

MIT was also very good. In fact, Hardy Martel, a very close friend of mine, went to MIT for a master's degree, and I went to Stanford.

When I got to Stanford, I got some bum advice from my advisor—namely, that these microwave courses that Ginzton and Chodorow taught were too hard for me. [Laughter]

COHEN: Who was your advisor?

GOULD: Hugh Skilling, who was subsequently the chairman of the Electrical Engineering Department.

You know, I should come back to my graduating from Caltech. I ended up graduating tenth in my class, despite my abysmal start. And if it hadn't been for my early years, it might have been even better. I was doing very well by the time I left Caltech. I applied to MIT and to Stanford, and that was it. I was admitted to both of them and chose Stanford. From then on, I was rolling. [Laughter]

But I did get bad advice at Stanford from Skilling, who was the departmental advisor for all incoming graduate students. So as a consequence, I didn't take those courses. And I'll bet if I had taken those courses, I would have stayed at Stanford, gotten my PhD at Stanford, and who knows what other directions I would have gone in.

But I did take some very good courses at Stanford. I took a course from Lester Field, out of the same book I'd studied from at Caltech, by Ramo and Whinnery. And Lester Field turned up at Caltech later on, when I was looking for a thesis advisor. He was such a charismatic teacher—just a wonderful lecturer, like [Richard P.] Feynman. You felt that you understood everything—until you got home. [Laughter] And he was a very nice person, personally. I also had a research assistantship in radio engineering with Mike Villard—Oswald G. Villard Jr., another radio amateur, incidentally. I believe his father was a well-known author, or editor. And that was a very enjoyable experience. But I didn't really want to go in that direction, and I think my interests were already moving toward physics. So, for some reason, I decided that I ought to try the outside world and see what jobs were like. I had not gotten hooked on Stanford.

I took a job at North American Aviation, in Downey, in nuclear reactor design. I don't know what possessed me to do that, but I did. This tied in to my later interest in nuclear fusion—and some physicists there were doing some very interesting work on nuclear properties and materials—but in the engineering-design section all we were doing were paper studies of nuclear reactors. And I was not a paper-study man; I was a hands-on experimentalist from the beginning. So this job was just not for me.

So I left that job after about three-quarters of a year and went to JPL. [In 1951—Ed.] And there I did missile guidance and control work—back to electronics.

COHEN: Now, JPL was a pretty small organization then?

GOULD: Yes. I think Louis Dunn was the director. [Dunn became director in 1947—Ed.] Bill Pickering was head of guidance and control, or head of a major division. He was also on the

Caltech faculty, but at that time people were doing both. [Pickering was professor of electrical engineering—Ed.] And Bob [Robert J.] Parks, a Caltech graduate, was responsible for the Corporal guidance. The Corporal was a short-range missile being developed by the army. I went to work with Hardy Martel, who had come to JPL after he got his master's degree at MIT. We did the reentry phase guidance for the Corporal missile. When it comes back in, it has to make a correction in the range. You send it a signal midcourse and tell it that it's a quarter of a mile short, or a quarter of a mile long, or whatever. And then there's a little maneuver it makes at the end to get back on track. We did the electronics for that. I'll never forget the first time I went down to White Sands to test our equipment. They had to destroy the missile, because as it went up, it went off in the wrong direction. [Laughter] And since we were involved in the terminal phase of the flight, we never got any results from that test.

COHEN: So how long were you there before you decided to go back to graduate school?

GOULD: Well, I was at North American Aviation for three-quarters of a year. Then I came to JPL. That would have been 1951. In '52, I went back to graduate school. I think one of the factors that had motivated me to get the PhD degree was working at North American. It was full of PhDs and I wasn't one of them. The things I was doing there were really kind of boring. If I had been in the physical sciences aspect of that project, it would have been a lot different. As I mentioned, I was doing engineering calculations on reactors, and North American eventually built a reactor but never became a player in the reactor engineering business. That was sort of a dead end for anybody working in that area. I think I was fortunate that I recognized, not that it was going to be a dead end commercially, but that it was a dead end for me professionally. And I decided to come back in physics at Caltech. At that point, Carl Anderson was very helpful to me. I still wasn't sure I could make it in graduate school.

COHEN: The long arm of Smythe.

GOULD: Well, that's true. And Carl Anderson worked out a special arrangement for me while I was still at JPL, where I came down and took Smythe's course on a part-time basis—only one course. And I did very well in it. I guess that was a turning point. I was always grateful to Carl for having let me come in as a special student and convince *myself*, anyway, that I was up to it. He was probably convinced already. [Laughter] But I wasn't.

I took quantum mechanics from Feynman. I remember being enamored of Feynman's style. I knocked on his door once and said, "You know, I'd like to do a thesis with you." And he said, "Well, go away. Don't bother me. When you've found a problem, come back and talk to me." [Laughter]

I had a course from Bob Walker in mathematical physics—a very good lecturer, very influential. Feynman was influential, too, but Feynman's unique style of quantum mechanics was not very helpful to me for the kinds of things I eventually did, which required a more practical, less sophisticated approach. But the important influences for me were Feynman, Bob Walker in mathematical physics, and Bob Christy in electricity and magnetism—and a course in special relativity.

I never took a course from Jesse Greenstein, but there were a lot of really great visitors coming through the astronomy department in that period. I met Ludwig Biermann. I took a course in plasma physics from Biermann when he was a visitor here from the Max Planck Institute in Munich.

COHEN: But were you registered as a student in both physics and astronomy?

GOULD: No. But, you know, at Caltech you can take almost anything you want. No, I was not a student in astronomy; I was a student in physics. And Reimar Lüst was here for a while. His name will come up later, because he and I got to be good friends, and later on he appointed me to a committee that was very interesting. So I did have a lot of contact with the astronomers, and I went to the astronomy seminars on a regular basis. I met Maarten Schmidt and the radio astronomers John Bolton and Gordon Stanley. But I decided I'd try high-energy physics, and I went to work with Bob Walker. I went over to the synchrotron, and I worked there for about six weeks and decided that big science was not for me.

About that time, Lester Field showed up from Stanford. [Field came in 1953—Ed.] He hadn't been at Stanford very long—four years, five years. I had taken the one course from him. He had a number of very illustrious graduate students while he was at Stanford. One of them was Dean Watkins, who is now probably retired—he was the founder and CEO of Watkins-

Johnson Company in Palo Alto. He's on the Board of Regents of the University of California. But in the technical area, Lester had a dozen students in the brief period he was at Stanford, and they were all exceptional. There was something about him that attracted people, and he attracted me. So I decided I wanted to go work for Lester, but he was over in electrical engineering. When I was an undergraduate, electrical engineering had been part of the Physics, Mathematics, and Astronomy Division. But I think it was right after the war that things were divided up differently. [In 1950, electrical engineering became part of the Division of Civil, Electrical, and Mechanical Engineering and Aeronautics—Ed.]

I remember going in to see Bob Bacher one day and telling him that I wanted to change. Bob Bacher was chairman of the Physics Division at that time. Things were very free and easy about taking courses in other divisions, but doing a thesis in another division was a different matter. And Bacher said, "Well, you know, you've got to make up your mind. Are you going to be a physicist or are you going to be an electrical engineer?" [Laughter] I don't think I've made up my mind yet, in a way. In fact, it's not necessary to make up your mind.

So I went over to Lester Field. He had brought a glassblower along with him from Stanford to make microwave tubes. And he also brought an ONR [Office of Naval Research] contract with him. And I was probably the first student he attracted at Caltech.

COHEN: So in truth, you became a student in electrical engineering at that time?

GOULD: Well, yes. But I got my PhD in physics. In fact, Bob Bacher said, "Well, you've got to have an advisor of record in physics." So they appointed Smythe as my advisor. [Laughter] I think he was really the closest person in the physics department. The physics department hadn't really grown a lot. I don't think Murray Gell-Mann was here yet, though Murray came while I was a graduate student. [Gell-Mann came in 1955—Ed.] Of course, Bacher had brought Walker and Feynman to Caltech from Cornell.

COHEN: Now, Willy Fowler had his group?

GOULD: Yes. I'd taken nuclear physics from Willy. I don't really know why I decided to try high-energy physics instead of nuclear physics, but I did. I liked Willy a great deal. In fact, later, when I got into plasma physics, Willy would send me reports from the Princeton Plasma Physics Laboratory, because he was on the advisory committee of the Atomic Energy Commission. He kept getting these plasma reports, and he wasn't all that interested in them, so he used to send them on to me. And I benefited from that.

So I worked with Lester Field in electrical engineering, but I was a physics student. And that's how I came to do a two-part thesis, in a way. Because he felt—and I felt—that because I was a physics student I'd better do something that was physics-oriented. His interests were in microwave devices, and the second part of my thesis—on the microwave oscillator—was really a microwave problem. I think either one of those two parts of the thesis would have been enough for a thesis, but I was working on both of these problems simultaneously. The idea for the first part, on solar radio noise, may have come from my contact with John Bolton and Gordon Stanley—and from my interest in radio noise. My amateur radio background came into this, too, because I knew about sunspot cycles and solar activity. So I was drawn to that subject. It was the marriage of two areas that interested me. Plus the fact that I had been auditing lectures in astrophysics, and Biermann had been around. I bought a copy of a big, thick book called *The Sun*. It was a good compendium of all the information available and I learned a lot from it during this period.

Now, there was another Caltech graduate, Andy [Andrew V.] Haeff, who worked in the microwave area. Being a very imaginative person, he came up with the idea of the interaction of two streams—two electron beams—for microwave amplification. He was at Hughes Aircraft Company at the time. He published a paper in which he proposed that a beam of charged particles coming out through the solar corona might stir up plasma oscillations and might be the source of radio noise from the sun. Lester Field, who was consulting at Hughes and very close to Andy Haeff, picked up on that paper, and he said to me, "You know, here's an interesting idea. Why don't you look into this?"

The microwave community was a field that came to fruition in the postwar era, and then faded away by 1960.

COHEN: So that community was Hughes and JPL and people here at Caltech?

GOULD: No, it was more centered at Stanford. But Hughes was one of the commercial companies involved; Varian Associates was another. There were a lot of them out on the West Coast—a whole microwave community. In the East, there was John Pierce of Bell Laboratories. Lester had worked at Bell with John Pierce before going to Stanford. John, of course, was a Caltech graduate. And Andy Haeff, who was head of the electron-tube laboratory at Hughes, was a Caltech PhD in physics. There was Si Ramo, who had worked at General Electric on space-charge waves on electron beams—sometimes called Hahn-Ramo waves. It was a very interesting, tight-knit community. Professionally tight-knit, but they were actually scattered all over the country.

COHEN: And they were basically either Caltech or Stanford graduates?

GOULD: A very large component of Stanford, Caltech, and MIT graduates. So Andy Haeff had this idea of beams of charged particles coming out through the solar corona; and he wrote this rather speculative paper about that as a possible source of radio noise from the sun. And Lester said to me, “Well, you know, you’re a physics student—you want to work in physics. Here’s a problem that’s really a marriage of physics, astronomy, and electrical engineering.” And he was prepared to supervise. So I worked on it, and it became one of the parts of my thesis. [“Plasma oscillations and radio noise from the disturbed sun. A field analysis of the M type backward wave oscillator.” (1956)—Ed.] And Willy Fowler and Jesse Greenstein and Smythe—and Lester, of course—were on my PhD committee. There must have been a fifth person, but I can’t remember who he was. Basically, it was a physics committee, with Lester as an outsider. And I think he was the only outsider.

Then Lester decided to leave Caltech, just as I was looking for a job. [Tape ends]

ROY W. GOULD

SESSION 2

March 7, 1996

Begin Tape 1, Side 2

COHEN: You're about to look for a job?

GOULD: Yes. I finished my PhD in the summer of 1955 and the degree was awarded in '56. At that point, I was thinking in terms of microwave electronics generally. That was the area Lester Field was working in. There was a series of conferences I had been going to, called affectionately by most people "the Tube Conference." [Laughter] Electron tubes. These were annual conferences held in June, and they moved all around the country. They were interesting conferences—heavily dominated by Bell Telephone Laboratories and, I guess at that time, General Electric—devoted to research on microwave and electronic devices. It was a clubby group—perhaps 100 researchers, 150 at most. And Lester Field, my thesis supervisor, was very well connected in this group. He was very good at pushing me and the other students at these conferences; he was in there fighting to get our papers on the conference program. So as a result, I had given a lot of papers there, and I was pretty well known, by John Pierce and others.

Anyway, I thought of myself, more or less, as a microwave-device person. So when I went looking for a job, of course I applied to the Bell Laboratories. Lester had been there, and John Pierce, whose parents lived in Pasadena, used to visit Caltech on a regular basis from Bell Laboratories. He would come to visit Lester Field, but he would always visit me, too. John was an interesting person. He invented the traveling-wave tube and also proposed orbiting satellites for space communication—though that came later. He was a very ingenious and clever person, and a Caltech PhD—very, very prolific and very bright. Also, somewhat unorthodox in his approach to mathematics and so forth; his books are hard to read for that reason. He was also somewhat unorthodox in his visits. He would just walk in the door and start a conversation where we'd left off the last time. [Laughter] He'd come in and talk for five, ten, fifteen minutes. Then he'd walk out the door without saying goodbye; nor would he say, "Hello, how are you?" when he came in.

Lester used to get internal memoranda from Bell Telephone Laboratories on his visits there, and also I think John would bring them to Caltech. Normally, these things are internal and not really for distribution, but there was a certain amount of trust there. And, of course, at that time, Bell Laboratories was certainly a very outstanding place. It was the obvious place for me to go.

I also applied to RCA and General Electric. And some outfit—whose name I have forgotten—in Madison, Wisconsin, which was willing to pay me twenty-five percent more than any of these other places would, but it didn't take me long to reject that.

COHEN: You didn't think in terms of an academic career at this time?

GOULD: No. Well, actually I had another friend, John Whinnery—somebody who's had quite an influence on my thinking about things over the years, and at one of the microwave tube conferences, I got to know John rather well. He put through a faculty appointment at the University of California at Berkeley, so I got an offer from there. And presumably I would have gone to work with him.

And by now Lester Field was working for Hughes Aircraft Company. Andy Haeff had been the director of the electron-tube and microwave laboratory at Hughes, and he was moving up. So they were looking for somebody to fill his position, and Lester decided to take that job. I had been a Howard Hughes Doctoral Fellow [Ed: 1953-1955], which was really a plum of a fellowship, because it paid you full time during the summer for work at Hughes and for one day a week during the academic year at rather handsome wages. John Whinnery had just left Hughes and gone back to Berkeley. So, of course, I had an offer from Hughes—in fact, from Hughes and one of the other aerospace companies—two offers that were significantly better than the offers from Bell Labs and General Electric.

But in fairly short order an offer materialized for me to stay at Caltech. I had not thought seriously about an academic position, but I did realize that if I were to go into academia sometime, I'd better do it at the beginning—because making the transition from an industrial job into academia would be hard, if not impossible. So I said, "What the hell, I'll try it."

COHEN: Now, this was 1955, and you were already married at this time, is that correct?

GOULD: That’s right. Bunny and I met in 1950. I was working at North American Aviation, in Downey, and I was living in Pasadena, because I knew Pasadena well. One of my friends [Don Westervelt], a physics student when I was an undergraduate, was working at North American, too, and he suggested I join their car pool. He and Bunny were driving alternately from Pasadena. She lived in Eagle Rock at that time. Bunny was born in Los Angeles. Her family’s home was in Eagle Rock—a very nice house in the hills north of Colorado Boulevard, just below what is now the freeway that goes through there. Anyway, she was one of the drivers in the car pool, and that’s how I got to know her. And every once in a while I would drive, and both of them would resoundingly kid me about the car I was driving. Not up to their standards. [Laughter] There was a fourth party [Bill Houghton] in this car pool, and he and I often had to ride in the backseat of the convertible, with the top down, on a cold day. [Laughter]

I was at North American about nine months before I decided to go to JPL. In the process, while in the car pool, I got to know Bunny pretty well. I don’t even know when our first date was. There was another Caltech graduate working there—a PhD in mechanical engineering, Leon Green—and another girl, Sue Samuels, who was a classmate of Bunny’s at Vassar. And it somehow happened that Leon Green dated Bunny first and I dated Sue Samuels first. And that went on for a brief period of time, before we got it right and figured out which was which. [Laughter]

Then in 1951 I went to JPL. And we were married in the summer of ’52. And that was just about the time I started graduate school full time. I think I mentioned that Carl Anderson had arranged a special dispensation for me to take Smythe’s course while I was still working at JPL. And 1952 is when I came back full time to graduate school, and we were married and got a little apartment over here on El Molino, within walking distance of Caltech. After about a year or two, Bunny began to get a little anxious about when I was going to finish my work—somehow, I’d given her the impression that I was going to be finished with graduate school in a year or so, and it went on for three more years. But we had a great time, and have had a great time. We bought a house in 1955, the year I joined the Caltech faculty.

COHEN: So you were actually rather financially secure.

GOULD: Oh, I think that by today’s standards we were in a great position. Bunny was working, and she continued to work for a number of years. Sometime in this period—before I finished my PhD—she went to work for Electrodata, which later became Burroughs. It was a computer manufacturer, one of the early ones. She had studied mathematics at Vassar—although I think her real love was art history. But she'd studied mathematics, and she was a very competent mathematician. She had gotten into programming at North American, and she saw a better future—and also it was closer, less commute—here in Pasadena with Electrodata. And through that connection, I met Joel Franklin, who's a member of our mathematics faculty now. In fact, a time came when Joel was thinking about moving from Electrodata, and I made a suggestion to Fred Lindvall [Frederick C. Lindvall, chairman of Engineering] that there was this great applied mathematician who was interested in numerical analysis, and maybe the Engineering Division would like to consider him for a possible faculty appointment. And it did. And he's been here ever since. That was in the mid-fifties. [1957—Ed.]

Back to Burroughs. Somehow, one of the Burroughs Datatron computers appeared in Spalding, where I worked as a graduate student—at least in the second part of my graduate years. I actually used it, and Bunny did some programming for some of the calculations that appear in my thesis. I've become much more interested in numerical computation since, and I do a lot now, but in those days I didn't do any hands-on numerical computation. I knew what I needed to compute, but I needed somebody to compute it for me. And in those days the person who was running the computer actually had to sit at the console and watch all the blinking lights. And there were only a few hundred or a few thousand bytes of memory and a big rotating drum and a paper tape to read the things in and out. It was really pretty crude by today's standards. And the average physicist or engineer had to do that work through somebody else, pretty much.

You mentioned our financial situation. Well, this Howard Hughes fellowship had paid me rather well while I was a graduate student. I think my income probably went down when I took my faculty position. [Laughter] In fact, the only reason it didn't take a dramatic drop was that part of the arrangement Lester worked out was that I would be a consultant at Hughes Aircraft Company. I can still remember what my starting salary was at Caltech. It was $6,000 a year as an assistant professor—this was in 1955. And I think that's about what Berkeley had offered me—it was sort of the going rate. But that first year I made $5,000 consulting at Hughes Aircraft Company. And Bunny, as I said, was working. So we were really very well off. We were able to buy a house in the fall of 1955. And our first child was born in the spring of 1956.

That was a big year!

COHEN: And that set your direction?

GOULD: And that really set my direction. But, again, I want to emphasize that my view of academic life was that this was a trial run; I was just going to find out whether I liked this sort of thing or not. Well, you can see what happened. [Laughter] I'd never lectured before. I'd never had any experience as a teaching assistant. But I took over the course that Lester had been teaching [Physical Electronics and Circuits, also third-term Ultra High Frequency Lab—Ed.], and I had good notes. When Lester left, he'd left the ONR grant, and four or five graduate students, and the glassblower. And what a wonderful way for me to start! I don't know many people who get to start like that. For the first couple of years, it was pretty smooth.

COHEN: Did you enjoy teaching?

GOULD: Yes. It was a lot of work, but I certainly didn't dislike it. I'm not sure whether I really enjoyed it at first. At first, it was kind of a struggle to get prepared.

COHEN: Was electrical engineering a very big department at this time?

GOULD: Well, I think I mentioned earlier that in 1949, when I graduated, it was not a very strong department. It was very small. And in the intervening six years, Charlie Papas had arrived and was interested in electricity and magnetism, an area that has been a continuing interest of mine. Lester Field was here, briefly. We didn't have any executive officers or departmental chairmen at that time, but Gilbert McCann functioned like a departmental chairman. Charlie Wilts, who started out with McCann in analog computers, eventually became interested in control systems. In fact, I had taken a very good course from Charlie in control systems and learned a lot. But when I joined the faculty it was still a rather small department. By the time I became an assistant professor, it had moved over to the Engineering Division, and Fred Lindvall was division chairman. As a department, electrical engineering hadn't really grown much. And to be honest, I don't think it was as strong as Stanford and Berkeley, which were the two big universities in California in electrical engineering.

COHEN: And did everybody have ties to Hughes or North American? Was that common?

GOULD: I don't think so. Gilbert McCann, of course, had started a company to manufacture analog computers. And he was still involved in that when I was a young assistant professor. Some of his former students were really, I guess, doing the major part of the work, but he was still involved in it and must have been involved in some of the decision making.

COHEN: So early on, there was a tradition for people to spin off their own companies?

GOULD: Well, I wouldn't say there was a tradition. The only one I know of is Gilbert McCann, unlike Stanford, which had a whole lot of companies surrounding it—Varian Associates being the primary one, Hewlett-Packard, and so forth, all Stanford graduates. I think I mentioned Dean Watkins of Watkins-Johnson, which came a little bit later. That kind of thing really was a tradition at Stanford, and also at MIT. But I think it's fair to say, not here. I'm not sure for what reason—it just didn't happen that way.

My tie with Hughes was more a professional tie. I learned a great deal of what I know about microwave devices from people at Hughes. From Ned [Charles Kennedy] Birdsall, who has just retired from the faculty at Berkeley, I learned traveling-wave tubes, out of John Pierce's book. I found Hughes a really good, rich environment. This was the peak of the period for microwave research, and they had some very good people at Hughes. I learned about wave propagation and periodic structures from a man who had done his PhD thesis at MIT on a tape helix. Dean Watkins was still at Hughes. Tom Everhart [Thomas E. Everhart, Caltech president 1987-1997] was there. Tom Everhart was a master's degree candidate at UCLA when I was a doctoral candidate at Caltech. And there was one other master's candidate. Hughes had a master's degree program and a PhD program.

COHEN: So people did their research at Hughes?

GOULD: Well, I wouldn't put it quite that way. We were very careful to draw a distinction. The problems I worked on for my thesis here had absolutely no connection with what I did at Hughes. What I did at Hughes was more in line with their own research program. And in fact, Lester and I together—he feeding me ideas and me executing them—played a rather significant role in developing what eventually became one of Hughes Aircraft's main products: the high-power traveling-wave tube and the particular kind of wave-guide structure used in it. Lester had come back from one of his many visits to Bell Laboratories with some internal memorandum from John Pierce—actually, it was a patent. And John Pierce had put everything but the kitchen sink into this patent. But like many patents the details were not there, and none of these things had been put into practice—they were only ideas. Lester thought that maybe Hughes could turn some of these ideas into something that would make a good high-power traveling-wave tube. So on my one-day-a-week visits over there, he set me to work building these things and testing them. The design has changed drastically from where we started, but it eventually evolved into something very close to what they probably still manufacture today.

I had subsequently a couple of Hughes doctoral students work for me, while they were still working over there. And they kept things pretty separate: they had their projects at Hughes and they had their projects here. So I think it worked pretty well. And we were all beneficiaries, because we got to enlarge our horizons and work on more problems.

COHEN: And also you got to live a little better.

GOULD: Exactly. [Laughter]

COHEN: So how long did you keep that connection with Hughes?

GOULD: Oh, quite a long time. Well, really until the microwave tube area faded as a prosperous research area. It had come about as a by-product of the war years. But it ran its course. I mean, this was basically an electron beam interacting with a traveling-wave structure. And people came up with all kinds of variations, and some of those things ended up as commercial products. Some of them are flying in satellites today; Hughes makes traveling-wave tubes for satellites. But the research era ended. It matured in a period of about ten years or so. I've noticed that that's more or less the pattern in electrical engineering. New ideas emerge. They get exploited as fast as they can. And then in five years, ten years, they run their course and the research people are off onto something else. If you're lucky, you can carry the same tools from one place to the other.

COHEN: You knew this area was finished, and you looked for something else.

GOULD: Yes, right. Of course, I had already been dabbling in plasma physics. In fact, I even had two graduate students, Gary Boyd and Al [Alvin] Trivelpiece, inherited from Lester Field, who had a foot in both camps—microwave devices and plasma physics, as I did. They were actually trying to combine some of these things.

I should say, first of all, that plasma physics was in its infancy at that time. Of course, Irving Langmuir wrote some very early papers in the twenties. But although there were some precursors, plasma physics had not really emerged as a subject, as a discipline. There was a famous paper by [Lev Davidovich] Landau in 1949, some aspects of which I still have interests in—still work on, in a way. But there were few papers in plasma physics. Andy Haeff had written this paper on two-stream instability. And the radio astronomers were talking about beam-plasma interaction as a possible source of solar radio noise. But as an experimental science, plasma physics was almost zero.

So, back when I was completing my thesis, Lester and I had the idea of doing a laboratory experiment with a beam going through a plasma, stirring up the plasma oscillations. It's actually something of a model for the solar radio noise problem—a beam going through the solar corona interacts with the electrons and generates radio noise. Well, the same thing can happen in the laboratory if you make a plasma and send a beam through it.

So we asked one of our students [Boyd] to look into this from an experimental point of view. There had been one experiment, very inconclusive, at MIT a couple of years earlier, trying to look for beam-plasma interaction. And since I'd studied it pretty thoroughly theoretically, in connection with the solar radio noise problem, I knew pretty clearly what to look for in the experiment. So we used some of the traveling-wave-tube ideas that I'd learned from Lester and at Hughes, and we did an experiment on beam-plasma interaction—I think probably the first definitive experiment—which showed that when you send a beam through a plasma, you excite oscillations at the plasma frequency and also get amplification. But we never really did tie it exactly to the solar radio noise problem. Actually, things don't scale; this device was only a few inches long, whereas there are kilometers and kilometers of plasma up there—nothing really to scale. But this was one of the experiments that was going on; it was already in progress at the time I became a faculty member. I mentioned that Gary Boyd was one graduate student I inherited. He's now at the Bell Laboratories, if he hasn't retired. And the other, Al Trivelpiece, is now the director of the Oak Ridge National Laboratory. Al and Lester and I tried to make a low-noise traveling-wave tube for Gordon Stanley and John Bolton. Low-noise amplifiers were the key to observational radio astronomy: the front end of a radio telescope could do you in, if there was too much noise. We never really succeeded. We never gave them anything that was helpful to them. But Al Trivelpiece found a PhD thesis in studying waves that propagate in this electron beam when it's going at very low velocities.

The point is, we were dabbling in plasma physics and microwaves at the same time. So as the microwave-tube era came to an end in the late fifties, it was a way to change adiabatically—slowly—into plasma physics.

The laser came along around 1957 or '58—though in the early years it was thought of as a solution in search of a problem. It was this wonderful thing that had been invented, but people didn't know what to do with it. But a lot of the people in the microwave area turned to lasers. And others—a very significant component—turned to plasma physics. And some turned to solid-state devices.

Another one of the threads that runs through my career relates to General Atomic and fusion energy. Thermonuclear fusion—fusion energy—was a classified research program until 1958. It was started in this country by [Princeton astrophysicist] Lyman Spitzer, primarily. There were a few others, but he was the one who pursued it most vigorously. And I think it was around '52 or '53 that there was a rumor out of South America that somebody was thinking about fusion energy. And it stirred people to thinking—in the Soviet Union and at Princeton and at a few other places, like Harwell, in England. People began to think about the fusing of nuclei and realized, of course immediately, that this would be plasma. Thermonuclear fusion research started up in the US in the middle fifties—totally unknown to me, because it was classified. The first inkling I got of it was in 1956, when I went to the New York meeting of the APS [American Physical Society] —the first APS meeting I'd ever been to—and gave a ten-minute talk on my beam-plasma interaction that I did for my thesis. And there was an undergraduate colleague of mine in the audience who'd come up from Princeton—Tom [Thomas H.] Stix. After the talk was over, he said, "You guys have some interesting results." I'd known Tom Stix at Caltech. He had lived in Fleming and I had lived in Dabney, and we used to throw oranges across to each other. [Laughter] Anyway, he said, "We've got an interesting project going on down at Princeton. It's classified, but maybe if you could come down sometime, we could tell you a little bit about it." And I said, "Thanks, but no thanks." I'd had my taste of classified stuff. The work I did at North American Aviation in the reactor division was all classified. And it took me two months to get my clearance before I could start. That may have turned me off a little bit. The things we were doing at Caltech were all very fundamental and stayed away from the classified stuff, but I knew enough about classified work that I decided I didn't want it.

But in '58, by agreement with the Russians, the fusion research program was declassified. There was a big international conference in 1958, at which everybody who had been working in the field on a classified basis came to Geneva and released all of the technical information. I was unaware of all this at the time, but I know in retrospect.

But in 1959, there was a project going on at General Atomic, in San Diego. [Tape ends]

Begin Tape 2, Side 1

GOULD: Donald Kerst was heading the General Atomic program in fusion research. General Atomic was a new company in 1959—it was part of General Dynamics. They had formed this program almost instantly, by bringing in a huge number of consultants. Well, I had been dabbling in plasma physics, and Don Kerst and John Malmberg came up to visit me at Caltech that year. They invited me to go down to General Atomic and work with Don Kerst on one of the plasma pinch problems, as a summer consultant. So I went down in the summer of 1959. And John Malmberg was along. John Malmberg was a physicist with that program—later at UCSD. He's a person with whom I've collaborated over the years since that time, until his death just a few years ago.

COHEN: Were you driving down one day a week, or something like that?

GOULD: No, I went for the summer. And that was another classy deal. They found a house for us. We had two kids by 1959—our daughter was born in ’56 and our son in ’58. And they found us a nice cottage in the La Jolla Shores area, only three blocks from the beach.

It was really a great era for plasma physics at General Atomic. Marshall Rosenbluth was there, and Al [Albert] Simon. I could come up with lots of names that would be immediately recognizable to plasma physicists. Freddie de Hoffmann was the head of the company; he was known for throwing lavish parties for his consultants. [Laughter]

So that’s the beginning of the General Atomic connection. I spent the summer there, working on one problem. I learned a lot about instrumentation for plasma physics. Much of it was similar to what I had learned in the microwave area, but some of the pulse-power technology was really quite different, and I had not been exposed to that before. After that summer, I made periodic visits to San Diego, to General Atomic, but I honestly can’t remember whether I became a regular one-day-a-week consultant. I don’t think so, because I was still consulting for Hughes. My consulting for Hughes continued until their program ran out of steam—probably into the early sixties.

COHEN: Now, what was happening here at Caltech? Was the electrical engineering department developing?

GOULD: You know, I was an untenured assistant professor. I had taken the job kind of as a trial. I had my hands full; I had lots of things I was interested in. I think most people at Caltech tend to look outward; we don’t look so much at what goes on around us. I’ve spent more time talking with my colleagues away from Caltech than my colleagues at Caltech. And I would say, one of the blessings, and at the same time one of the weaknesses, of Caltech is that some of us have very few colleagues at Caltech whom we can talk to, so we go out and find our stimulation outside.

COHEN: Perhaps that’s because it’s a small place and there isn’t a critical mass of people doing the same thing you’re doing.

GOULD: Well, that’s true. And at that time, that was absolutely true for me. There was nobody else doing plasma physics—although I could talk to the people in astronomy about some of their problems. Later, there was a period in the sixties when the aeronautics people—Hans Liepmann and one or two others—got interested in plasma physics, more MHD [magnetohydrodynamics] oriented. And there we developed a really good relationship. Hans had a number of very good students in that period. I sat on their thesis examinations, occasionally helped them with problems. They were doing MHD problems, which is a little bit different from what I was doing.

COHEN: MHD?

GOULD: Magnetohydrodynamics—fluid flow in magnetic fields. MHD generators were proposed—taking hot ionized gases and making them go through magnetic fields, and using them for electrical power generation. This is an idea that has been around for a long time, but it's never been made commercially viable. It's been worked on for a long time, but there are still so many problems—as there are with fusion power.

You asked me what was happening at Caltech, and I think I was not really paying too much attention to it.

I should mention that I had a lot of visits from people at other universities, who came to see my work in plasma physics. So, even though I had not really fully emerged as a plasma physicist, I was nevertheless attracting some attention. So I became more and more connected with that community—the Princeton people, in particular. I remember a couple of Princeton people [John Dawson and Carl Oberman] also coming to visit in 1959, just after things had been declassified. It was kind of a revelation to me that they'd been working on some of the same problems I had been interested in, in connection with the solar radio noise. So by about 1960 I was fully into plasma physics.

COHEN: You said something about Biermann having had some significant influence on you.

GOULD: Yes. Let me just go back a little bit. The first trip I made abroad was in 1958, I believe, to give a paper at an electrical engineering conference in London—a paper on some of Al Trivelpiece's work. And in conjunction with that trip, I visited Munich. This was 1958, and I was still unaware of the fusion program. It was basically Biermann's being there, and I wanted to visit the Max Planck Institute for Physics and Astrophysics, which had moved to Munich at that time from Göttingen. And I actually probably wanted to do a little sight-seeing, too. So I decided, Gee, I'd better go to Munich. And I'd heard from Biermann, I guess, that people were dabbling in some of these plasma physics problems. And indeed they were. I think they had been doing some work in the classified area. Anyway, whatever I saw was not classified, obviously. I spent a couple of days there. Reimar Lüst and his wife, Rhea, were there, both astrophysicists. I don't recall whether I saw him at that time or not, but certainly the two of them were responsible for my knowing about the place and what it was interested in and my desire to go there. Then I went back, I think, in 1960. And that started a long connection.

I began suggesting problems in plasma physics to my students about 1960. Let's see, I was promoted to associate professor in 1958. And I guess in 1960 I got an offer from Marvin Chodorow to join the Stanford faculty. It looked very appealing to me, because Stanford was a very appealing place. They came up with an offer of a joint appointment in electrical engineering and physics. I was an associate professor of electrical engineering here, though I'd got my PhD in physics, and, frankly, the idea of having an appointment in physics sounded attractive to me—particularly the way my research was going. So I asked the people up there if it would be OK if I switched into plasma physics. At that time, there were several other people interested in plasma physics there. And it was OK.

But I turned the job down. And the reason I turned it down was that I decided the appealing thing about going to Stanford was this joint appointment. And really, things were going pretty well at Caltech for me. So I thought to myself, well, you know, maybe I should bring the subject up here and see if I can have a joint appointment here. If I could, I'd be happy to stay here, because things were going just fine. So I did bring the subject up, and a joint appointment was forthcoming almost immediately. So that's why I didn't take the job at Stanford.

COHEN: Did you get tenure at this point?

GOULD: No, I didn't get tenure then. But that was in 1960, and I'd been on the faculty for only five years. I got a tenure letter in 1962, so I can't complain.

COHEN: So after 1960, you were a professor both in electrical engineering and physics?

GOULD: From 1960 until 1979, when I became division chairman of Engineering and Applied Science. Then I felt I should relinquish my appointment in physics. I'd like to talk about the relationship between physics and electrical engineering, and how applied physics came in—when we get to that point in time.

So, that was '62. I'd been on the faculty for seven years, and I began to think about taking a sabbatical. I figured, well, if I'm going to be a university professor, I ought to take advantage of all the fringe benefits. And, of course, what did I think of first? Munich. [Laughter]

Actually, by that time I had learned a bit about the Culham Laboratory, in England, outside Oxford, and I wanted to fit them both in. So I went nine months to Munich, to the Max Planck Institute for Plasma Physics, which had just spun off from the Max Planck Institute for Physics and Astrophysics. And they had just moved to Garching in 1963, when I arrived.

So I applied for and got a National Science Foundation senior postdoctoral fellowship, which would pay my existing salary up to $15,000 a year maximum. And I went in and told Carl Anderson about these terms, and he said, "Well, we'll raise your salary to $15,000." [Laughter] Actually, I think they raised it a bit more. But I got the full benefit.

So I went off to Munich, with the whole family. In fact, I had had a skiing accident that spring and had broken my leg—a spiral fracture in my right leg—and I was in a cast. We were set to go to Munich in July. And Bunny had dislocated her shoulder a couple of times and decided that before going off to Europe for a year she'd better have her shoulder repaired. So she had had an operation on her shoulder, and she had her arm in a sling. And I had my leg in a cast. [Laughter] And we went off to Munich. We had arranged to pick up our car in Paris and drive to Munich, and I had charted the scenic route, through Switzerland. And Bunny had to drive, because I couldn't drive. [Laughter] And our kids were, I think, five and seven at the time. And we set off from Paris to Munich.

We arrived in Munich, and they had found a nice apartment for us in Schwabing, right across from the Nordbad, very well located. I used to have to take the bus out to Garching, which was a bit inconvenient with my leg in a cast. Also, I was worried about what would happen when the winter came. I was in a cast for five months; it was quite a nasty break. And when winter did come, I was still on crutches, and I was worried about slipping on the ice in Munich. But we found a doctor there who could treat both Bunny and me, and we were both back skiing that year. [Laughter]

It was an interesting year. I made a lot of friends at Garching, though at that time there wasn't much fraternization between the visitors and the people there. I met a student there who was working on a problem very closely related to what my students were working on here in Pasadena—waves in electron gas. And I was able to help him. Eventually he turned in his thesis at the University of Munich, and got his PhD, and basically I was his informal advisor for the work. His name was Ben O'Brien. And somehow I missed his name when we were introduced. It was a German laboratory; everybody was speaking German, but he spoke English to me. And I said, "You know, your English is really very good. Where did you learn to speak English?" [Laughter] Well, he was an American, but he had been in Munich so long that he had acquired a German accent, even when he spoke English. Anyway, he and I became very good friends. He's now living here in Southern California, and we still see him quite regularly.

COHEN: Was it a profitable year for you?

GOULD: Oh, yes. Actually, with these National Science Foundation proposals, you have to pick the people that you intend to work with. And, frankly, I picked them out of a book, because Biermann and Lüst were not doing exactly what I wanted to do. So I picked somebody who seemed, from the description, to be doing the closest thing—and it turned out not to be a very good choice. But I soon latched onto the theoretical group. Arnulf Schlüter was the director of the Garching laboratory and also head of the theoretical group—a very outstanding physicist.

I ended up writing a paper on a subject I had picked up here. And I remember that when I was leaving I wanted to publish it. I had done all the work while I was in Munich, and I wanted to publish it there so that my byline would read "The Institute for Plasma Physics in Garching." And that required the approval of the director. So a few weeks before I was about to leave, I took him a copy of the manuscript. I didn't hear from him until the day I was ready to leave. So while saying goodbye, I said, "Well, I'll wait a little longer, and if I don't hear from you I'll assume it's OK to publish the paper." [Laughter] And sure enough, I didn't hear from him. And I published it. The request was a formality, I think, more than anything else.

COHEN: Now, did you learn German that year?

GOULD: That's an interesting question. I learned enough German to get along—in the shops, the restaurants, train stations, things like that. One of my colleagues from Los Alamos [Fred Ribe], who was there at the same time, insisted that he would not speak a word of English in the institute. And he made it stick, with some difficulty. He became pretty fluent in German in the year he was there, but I didn't.

Then in the spring I moved on to Culham, in England, just outside Oxford. And there, somebody had arranged for us to live in a house whose owner was also on sabbatical. I didn't know him at the time, but we've become very good friends since then. His name is Sydney Hamberger—he's now living in Australia. So I spent about four months in England.

Oh, I should say that we put our children into the German schools cold turkey. Our son was in kindergarten, and our daughter would have been in the second grade, but we put her in the first grade in the German school. Fortunately, a Swiss couple, Edgar and Marianne Luscher, had just returned to Munich from the University of Illinois at Champaign—he was a physicist. And they had two kids the same age, who spoke German fluently. They took our kids under their wing, and our kids did OK. When we got to England, of course, the kids had no problem; in fact, our daughter acquired an English accent in about four months.

At Culham I had picked out somebody by name, too, who turned out to be a delightful character—Roy Bickerton, with whom I've had a lot of contact since. He had a very lively group. Sydney Hamberger was on sabbatical from that group.

COHEN: Did you live in Oxford?

GOULD: We lived in Abingdon, a little bucolic village right on the Thames. We weren't more than half a mile from the Thames, and one of our favorite weekend pastimes was to go down to the locks, where the kids would be allowed to help open the gates on the locks for the boats to go through.

I managed to do one minor paper while I was at Culham. But that was where I was really exposed to fusion research. Garching was a collection of little dukedoms and fiefdoms; it was a funny kind of place at that time. It had a lot of good people, but they were all doing their own thing. It was a rather large laboratory but totally fragmented—a lot of infighting, actually. And when I eventually ended up on the Garching institute's advisory committee, many years later, that was one of the problems we helped them face up to.

ROY GOULD

SESSION 3

March 15, 1996

Begin Tape 3, Side 1

COHEN: Was there something you wanted to add to the last session?

GOULD: Well, I did forget one important occurrence that happened in 1959. Paul Vandenplas visited Caltech from Brussels, on a NATO fellowship, to work with Milton Plesset. Milton was very well known, of course, in Europe. And somebody—[Ilya] Prigogine, or one of the people at the university in Brussels—must have suggested, "Go to Caltech and spend a year. And Milton Plesset's a good person to work with." And it just happened that as Paul was poking around, he came over to see me one day. And he was more interested in the problems that I was working on. I got him involved in one of them, and in about half a year he got into this problem well enough. Then he went back to Brussels.

COHEN: Was this a plasma physics problem?

GOULD: It was a plasma physics problem, yes—the fluid dynamics of an electron gas. He took it back to the university at Brussels and worked on it another year or so and turned it into a PhD thesis. And that was the beginning of a collaboration that continues until this time. Although I must admit it's now less technical and more of a personal friendship. But in the sixties, it was a real collaboration. He was here again in 1966, I think, for a summer, or something like that. And then he would visit periodically. And he still visits periodically, though his technical interests are more with people at UCLA right now.

COHEN: Have you gone to Belgium to visit him?

GOULD: Oh, yes, in fact, about four or five years ago. I should say, first of all, that he was a member of the Belgian military, I guess. And he took a position as an instructor in the École

Royale Militaire—the Belgian military school, which is like our West Point. He got the job partly because he was fluent in French and Flemish—and fluent in physics, too, of course. [Laughter] He worked his way up. He had sort of a parallel career, and he's still with the École Royale Militaire, in Brussels. When I get to the tokamak days, I'll talk a little bit about how our interests intertwined in that period—that's in the 1970s.

But the laboratory that he formed in Brussels celebrated its twenty-fifth anniversary about five years ago. And I was in Europe at the time and was invited to come to their celebration. In fact, I was one of three principal speakers—a speaker from his past, and in some sense what the Germans call the "doctor father." Paul and his wife and Bunny and I are good friends; we were back there last year, in the spring. We went to Paris and to Brussels. And it turned out that the École Royale Militaire was having its annual ball. [Laughter] So we went to the grand ball, and it was just absolutely delightful. There was a dinner ahead of time. We were all announced with the people in their fancy uniforms, and so forth.

COHEN: All right. Let's proceed now. You're back after your sabbatical, and getting into things.

GOULD: Yes. Let me put this a little bit in perspective, because the 1960s was really a glorious period for the development of plasma physics. As I mentioned before, the fusion effort was declassified in 1958. Most of us didn't know what was going on. The work was very goal-oriented. And the underlying science of plasma physics, except for the work that had been done in astronomy—in astrophysics, primarily—was pretty meager. There were almost no laboratory experiments, just a lot of theoretical work from astrophysics. Fusion experiments didn't work very well, and nobody knew why. So it was just a great period for the science of plasma physics: the money was available; new people had been flooding into the field after it was declassified. People like me. People from MIT. This was a new field that had suddenly opened up. It was a period of very rapid development. I think it was probably—if you go back and include the microwave work that I did, starting in 1953 or '54—that fifteen-year or eighteen-year period was probably the most productive scientific period I've ever had. I did my best work, probably, because I was young and I was in this new field.

The other thing that happened in the early sixties, before my sabbatical, was that my consulting work with Hughes began to diminish, and I became affiliated with General Atomic, in San Diego, as I've mentioned. But another group that I'd failed to mention was the Space Technology Laboratories, an aerospace group. When plasma physics emerged as a good, solid scientific discipline to work in—particularly from the experimental standpoint—the Space Technology Laboratories created a plasma physics group. And I believe Milton Clauser, Francis Clauser's twin brother, was the director of that plasma laboratory. I think it began as Space Technology Laboratories and eventually became Aerospace Corporation. I've forgotten the exact details.

COHEN: Where was it located?

GOULD: In El Segundo—in the same area as the old Hughes research laboratories, before Hughes moved to Malibu.

And I met a theorist there—Burton Fried—and he and I got interested in quite a few problems, and that's another collaboration that has continued for a number of years. We worked out the theory of ion acoustic waves. I think you can see that these consulting contacts were a source of great stimulation.

COHEN: Now this was not necessarily what you were working on here at Caltech.

GOULD: No, these were almost always separate problems. But it seemed to me—I think it was certainly true at Hughes—that the fun was in expanding your horizons and working on new problems. One of the problems that Space Technology Laboratories was interested in at that time was the plasma that surrounds a reentry vehicle. When a missile reenters the atmosphere, it ionizes the gas around it, and there's a radio blackout period, during which you can't communicate with it. So there was a lot of interest in plasma problems like that—very practical problems. They were really big problems for the military. So these companies like Aerospace, Space Technology Laboratories, Hughes, later TRW, could justify having a plasma physics group because there were these enormous technical problems to solve and they didn't have very much fundamental data.

Also, satellites were beginning to be put up in those days. The first satellite, the Russian

Sputnik, was in late ’57. I remember we had a radio receiver in the laboratory on the fourth floor of Kellogg, and we listened to it. But that was one of the motivating factors for the aerospace companies to go into plasma physics.

And then—I’m jumping ahead a little—in 1969 the plasma physics division of the American Physical Society held its meeting in Los Angeles, and Burt Fried and I were the co-chairs of the meeting. And we had to make the arrangements. We had to do the site selection; we had to go around and look at the hotels and decide which hotels looked best. Burt and I and maybe one other person formed the site-selection committee, and we settled on the Los Angeles Hilton. One major duty was to deal with the hotel. The other major duty was to put the program together. And after a few times of dealing with the hotel, Burt said, “You know I can’t stand this sort of thing. You deal with the hotel, and I’ll take care of the technical program.” [Laughter] And so that’s how it was done.

One other thing happened at that meeting. There had been, unbeknownst to me, an evening activity that subsequently became a part of my life. It was referred to as “the probability seminar.” And what it was, a small handful of people—some very illustrious people in the plasma physics business—would retire to the largest room, maybe a suite, for a poker game. [Laughter] That’s why it was called the probability seminar. Amasa Bishop, the director of the Atomic Energy Commission’s fusion program, was a member of this group. One of my former students, Al Trivelpiece, was a member, and John Malmberg and Harold Furth. And this meeting was my introduction into the poker game. That poker game continued for some twenty-five years, always at this particular meeting. Although there were also other times, when I was at the Atomic Energy Commission—at Los Alamos there were a lot of people interested in “probability.” [Laughter] We played wild-card games, which provided a lot of opportunity for individual expression and personalities to emerge—bluffing, and so on.

I had some very good graduate students in that period of time, too.

COHEN: Would you like to mention some of them?

GOULD: Yes. Trivelpiece was one of the earlier ones. Paul Vandenplas, though he didn’t get his PhD with me directly, he certainly started it with me. There was Jerry [Jerold V.] Parker and John Nickel who worked on a problem that got a lot of attention. There was [Donald] Gary

Swanson and Bob Hertel. Gary Swanson, who got his PhD in 1963, stayed on a year as a postdoc, and he ran the lab while I was on sabbatical in Europe. Gary is now a professor at Auburn University. He did a very nice experimental PhD thesis. And now at Auburn, he's turned theorist and quite good.

COHEN: And what kinds of problems were your students working on?

GOULD: Well, mostly looking over the kinds of problems that I've worked on—waves in plasmas. The work I did in electron devices was waves on electron beams. In fact, plasma physics was really kind of an ideal field to carry over the methods and tools I had used in the microwave era. You know, I learned an awful lot from Smythe, too. A lot of people were unhappy with that course, because it was really a tough course and it separated the men from the boys, as they say. But people who took that course learned a lot about problem solving, because it really was a problem-solving course, not a course in the fundamentals of electricity and magnetism. You had to get that somewhere else. And people used to say that whoever took Smythe's course could go out and make a living applying the methods they learned in his course to solve problems in different areas. I see now that Smythe's approach and the problem-solving methods I learned in that course from him have run through my life. Once you pick them up, they're part of you.

COHEN: So this was really a very rich period for you, the sixties?

GOULD: It was. In fact, there were a few rewards that came along in the sixties also. I got tenure in 1962, if I remember correctly—and I was full professor by 1962. That was kind of the fast track in those days. It's not the fast track anymore, for some people. The fast track is even faster now for some people. And I was elected a fellow of the IEEE—Institute of Electrical and Electronics Engineers—for my work in microwave tubes, which I was no longer doing. [Laughter]

COHEN: So your work was being recognized.

GOULD: Yes. And I began to serve on committees. I became an associate editor of one or two of the journals and began to serve on some advisory committees.

COHEN: Did you enjoy that?

GOULD: Yes. Well, I regarded serving on advisory committees as an opportunity to see what laboratories were doing, with all the details stripped away. You could see the goals and objectives, because on these advisory committees, you know, they often lay on very careful presentations, in which they give you the big picture and spare you a lot of the gory details. Whereas if you go around and talk to the individual scientists, you're likely to get the gory details, about what *they're* doing and you don't see the big picture. So that was very useful.

But scientifically, the next really important thing that happened, aside from the work that was going on, is that my consulting with General Atomic began to pick up. John Malmberg, whom I had met down there in 1959, and another colleague of his, Bill Drummond, got some money from DASA, the Defense Atomic Support Agency, to build a plasma physics machine to study turbulence in plasmas. And John called me in as a consultant to help him design this machine.

They had an ambitious program. Bill Drummond and David Pines had done some very basic work on turbulence in plasma, and they wanted to test these ideas. It was an experiment that overreached most anything that had been done up until that time. It was very ambitious. One small contribution I made—although I'm sure it occurred to a lot of other people, too—is that they should do an experiment on Landau damping. That's a concept that had been written—I think I mentioned Landau's pioneering paper in 1949. Every plasma physicist knew about Landau's paper and the Landau-damping concept. I won't go into the details of that. This was a phenomenon that was thought to be very important in plasma physics, but it had never been seen experimentally. So I made the obvious suggestion.

They were on a fast track; they had to get results in one year, and they were very anxious to get on to this turbulence experiment, which they had sold DASA on. But somehow things always get stretched out and take longer. They did manage to sandwich in this Landau-damping experiment, and it became a classic experiment in plasma physics. All plasma physics experimentalists look on that experiment as an experiment they wish they had done. *I* didn't do it, mind you, but I was involved in watching and kibitzing on the sidelines—I got to know their experimental apparatus very well.

There was another discovery by a group in one of the aerospace companies up in the Bay Area, concerning a thing called cyclotron echoes. And the person who was doing it was a student of Erwin Hahn, who was a physicist who wrote a pioneering paper on spin echoes in the late 1940s. I had attended some lectures here by Ed [Harvard physicist Edward M.] Purcell on nuclear magnetic resonance, and one of the topics he covered was spin echoes. He must have been here for several months, and he gave a series of lectures. I remember it was in the Bridge lecture hall, rather well attended. [Nuclear magnetic resonance] was a hot subject in those days, and still is. The chemists have exploited it tremendously, but in those days it was the domain of the physicists.

But I learned about spin echoes, and when they discovered cyclotron echoes in the middle sixties in plasmas.... One thinks of a cyclotron as an accelerator of charged particles, but in fact in plasmas, whenever you have a magnetic field, all the charged particles go in little circles, and that motion is called the cyclotron motion. And the cyclotron echo has to do with that. Well, it got me thinking about echoes. This was an experimental discovery, and they didn't have an explanation of it. But I had heard Purcell's lectures ten or fifteen years earlier, and it finally dawned on me what was going on. And I published a short two-page paper in *Physics Letters*, which outlined the explanation—which I think still holds today.

And that got me thinking about echoes again, in another context. It got me back to Landau damping. Because it turned out that these problems have some features in common, although they look entirely different. The underlying physics is exactly the same. There are these free particles that don't collide very often, and when they get hit by a pulse, they can remember what they saw before. And then, if you're clever, you can rejuvenate some of the effects that occurred earlier. An echo . . . you apply a pulse and the system responds, and then you apply another pulse and the system responds. But then you also see an echo come back at a much later time. And this happens in spin systems. It had also been shown that it happens in molecules. People in the Bay Area [R. M. Hill and D. Kaplan] had shown that it also happens in plasmas, in the cyclotron motion. And I thought, Gee, you know, this Landau damping—everybody knew it was reversible, that if you could sort of turn time around and make time run backwards, the damping that occurred would actually be reversed, and the response to the original pulse would all come back together again.

So I got this idea to do an echo experiment. It was kind of a half-baked idea—it was on sound ground, but I had not worked much of the mathematics out. But I went down to John Malmberg in the San Diego group and said, "Look here, you guys have just been doing the Landau-damping experiment. I've got this clever idea. Let's reverse the Landau damping and do a plasma-wave echo experiment in your machine."

Well, John Malmberg was always skeptical of these sorts of things, so he wasn't convinced at first. But there was a young theoretical postdoc there who had just gotten his PhD at UC San Diego—Tom O'Neil. Tom was intrigued by the idea, and he grasped the basics quickly. I had the fundamental idea down, and I'd done very rudimentary calculations, but that wasn't good enough for John. So Tom O'Neil stepped in, and he and I in a period of two or three months fleshed out the theory and then we convinced John that the experiment was worth doing. [Laughter]

So John said, "OK, we'll take one week. We'll set the machine aside." There were a few changes that had to be made in the machine—they had to put some more probes in. He got all that done, and after a few months I went down to San Diego. They had set aside one week to do the experiment, and we found the effect on Monday morning. We started on Monday morning, and we found the effect. And I always used to kid John that we found it when he was down the hall in the men's room. [Laughter] So he wasn't really there when it was discovered. Then we spent the rest of the week just measuring every conceivable property of the echo we could think of. By that time, we had a number of predictions as to things that should happen. And we spent the whole week, then, just in sort of a romp. That's a week that most of us will never forget. Because few people, I think, have the kind of experience where you go through something as fundamental and as nice as that, and it all happens in one week's time. They had done the damping experiment, and this experiment showed that that it was actually all reversible—something the theorists had taken for granted for years. It was part of every plasma theory of the time, but it had never been shown experimentally. So that was a fun time.

And we had an interesting experience with the *Physical Review Letters* on that work. We had published the theoretical concept—O'Neil and I and Malmberg. We had published it in the summer, I think, and then we did the experiment in the fall. And we submitted the experiment for publication to the same journal. And they said, "No, you can't do that. You've already had one publication on that subject. This is serial publication, and we don't allow serial publication." Well, actually, the experiment was, of course, the best part, in some respects. But in about a month they relented, because two other groups submitted papers doing the same experiment. [Laughter]

COHEN: Where did they get the idea?

GOULD: From our theoretical paper. Our theoretical paper had been out there for about six months. Two other groups picked it up and did the experiment. But we had *our* experimental paper in first, and within a month two more papers came in on the same subject. And *Physical Review Letters* said, "OK, we're going to publish all three of them. And we're going to publish them in sequential issues, and we'll publish them in the order in which they were received." So ours came first.

But we really were mad, because we knew this was a good piece of work, and they wanted to reject it because we'd already published one letter on the subject. They didn't seem to know the difference between a theoretical paper and an experimental paper.

COHEN: So, now, was this in the late sixties?

GOULD: This was '67, something like that.

Now there was another interesting thing that came from that. That work led me to submit a proposal to the National Science Foundation to continue in that line of investigation. There was a man in the physics section of the National Science Foundation whom I talked with. And he was very excited about the work and said, "Send me a proposal and I'll fund it." And at that time, the National Science Foundation was very stuffy. They looked on the front cover page to see where you were. And I had carefully camouflaged things and had "Physics Department" on the front page. But when you looked through the whole rest of it, and you looked at my biography and all that kind of stuff, you could see that I was basically an electrical engineer. So they sent it to the engineering section of the National Science Foundation. And the contract monitor for that area was an old fuddy-duddy, and he drug his feet. He would neither fund it nor would he give it back to the guy in physics. There was a little turf war going on there. So I never got any money from the National Science Foundation to carry on this work.

On the other hand, word began to get out. I was grumbling to people. And Amasa Bishop, the director of the fusion program at the Atomic Energy Commission, picked this up. And he said, "Well, I'll give you some money." [Laughter] It came to me here at Caltech, and that's what started me with the Atomic Energy Commission. Things were pretty free and easy in those days. I can remember when we did some of our early plasma work in the fifties; we were supported by the navy. But earlier I had people come from the Army Research Office and say, "Do you need more money?" And I had people from the Air Force Scientific Research Office come to me and say, "Do you need more money?" Of course you need more money! You can always use more money. But those days are gone.

Just by comparison, there's a minor flap going on right now. I have navy support again, from ONR, after a long period of no navy support. And a memo just came from my grant monitor saying that there's a memo from the chief of naval research, or somebody like that, saying, "We want you guys to spend all your money. And we want it to show on our books as spent; because if it is not shown on our books as spent, it'll get taken away from us." That just arrived in the last two or three weeks. And there's a flurry of activity going on, because, you know, investigators tend to shepherd some of their money. How things have changed!

COHEN: And many more people are competing.

GOULD: Oh, that's true!

COHEN: So you then proceeded to have a relationship with the Atomic Energy Commission?

GOULD: Yes. At first they funded my work. Amasa Bishop, the program director, was another Caltech graduate—a little before my time. I didn't know him at Caltech, but I think he got a bachelor's degree at Caltech and a PhD somewhere else. He stepped in and funded the work.

There's also another thing in the sixties. There were the conferences. I mentioned the American Physical Society. But there was another series of meetings in Europe called the Conference on Ionization Phenomena in Gases. And those conferences used to be held in the Eastern European countries. They were sponsored by [Alfred Hans] von Engel, who was at

Oxford, but he was a world traveler and liked the Eastern European countries, and used the conferences as kind of an excuse to go there. A number of us latched on to those conferences and presented papers there. My colleague Paul Vandenplas and I had a paper, I believe, in the ’61 conference, in Munich. The next one was in Dubrovnik—actually, the conference was in Belgrade, but everybody ended up in Dubrovnik afterward. Then there was one in Vienna. And then in 1969, just before I went to the Atomic Energy Commission, there was one in Bucharest. Good conferences. I got to meet the Eastern Europeans, because some of them could come to these conferences.

I had been corresponding a little bit with a group in East Berlin. They had sent me reprints of some of their work, which was very close to some things I was doing here in Pasadena. So I finally wrote them a letter and said, “I’m going to come to this conference in Vienna. I’d like to come visit you.” I wrote a couple of months in advance, and I never heard a word from them. So I went to the conference sort of disappointed. But they were at the conference, and they said, “Well, sorry we couldn’t write to you. But if you want to visit us, you can come visit us.” And they outlined the way it had to go. They said, “Go to West Berlin. Go through Checkpoint Charlie. We’ll meet you at a coffee shop in East Berlin, and we’ll drive you to our lab out in the countryside, and we’ll host you for the day.” And it turned out to be an old Luftwaffe laboratory. [Laughter]

COHEN: I think the idea was no paper trail.

GOULD: No paper trail, absolutely. And they couldn’t have been more delighted to have visitors! I mean, they were really impoverished from the scientific standpoint. They did get to go to meetings in the Eastern European countries. And they had a collaboration with the Soviet Union, but they said it was really not very productive.

Before I did all this, of course, I checked with my West German friends that this was a wise thing to do—or at least that it wasn’t unwise—to go visit them under these circumstances.

COHEN: Your West German colleagues were doing the same thing?

GOULD: They were. And they said, “The one thing to be sure of, don’t talk politics with them—

because there might be some people there who are really sensitive to that. So just stay away from politics." Then, some years later, ten years later or so, the National Academy had an exchange with East Germany, and I was on it. Sam Epstein [Caltech professor of geology] was on it. There was a delegation. And I saw these same people again. The Eastern Europeans had good basic plasma physics, no fusion.

At the end of the sixties, I was beginning to think about another sabbatical.

COHEN: That's interesting, because many people at Caltech do not take sabbaticals.

GOULD: You know, for me it's been very important—because I'm a Caltech graduate and I've spent my whole career at Caltech. And I think even at the time, I looked at it this way: I've just got to see some of the things I'm missing and see what life is like elsewhere. Caltech is really a very pleasant place. People leave you alone. You chart your own course. If you don't set a good course, then your days are numbered, but if you have good ideas and things you really want to accomplish, it's a wonderful place to be.

Anyway, I thought about a sabbatical. [Tape ends]

Begin Tape 3, Side 2

GOULD: Of course, I'd had funding from the navy for a long time, and I was aware of these newsletters that came out of ONR in London. They had a liaison office in London. And I had this friend from Stanford [Professor Hubert Heffner] who had spent a year in that liaison office. And the job was to travel around to the European laboratories and write reports, and write contributions for the ONR newsletter, which was circulated to American scientists. I think they have an office in Tokyo now. I don't know if the office in London still exists or not; I think not. But it sounded like a real boondoggle, and pleasant. [Laughter] But I must say I viewed it with mixed feelings, because, first of all, I'm not a very good writer. It's painful for me to write technical things. And I didn't have any real experience writing nontechnical things or popular articles. But I had gotten a fair way into the negotiations for taking this job. And then Amasa Bishop, the director of the AEC fusion program, decided it was about time for him to move on. He had lived in Europe on a number of occasions, and he wanted to go back to Switzerland. So, suddenly the AEC fusion program was going to need a new director. I was not particularly aware of that, but somehow the call came. And they said, essentially, "Would you think about taking this job in Washington?"

Last night, Bunny was asking me, "How did these things come about?" And I tried to reconstruct it. The man who turned out to be my boss at the AEC, the director of research, was basically a lifetime political man, very skilled and very good—Paul McDaniel—but he wouldn't know whom to turn to for a program director for one of the programs in his area. But they had an advisory committee, and I think it was probably the advisory committee that did it. Willy Fowler was on it, and Sol [Solomon J.] Buchsbaum, whom I'd met when he was a graduate student at MIT in the late fifties. He was just a year or two behind me, and he had gone to Bell Laboratories and had risen rapidly there. And there was Melvin Gottlieb, who was the director of the Princeton Plasma Physics Laboratory, and there was the director of the Livermore [Lawrence Radiation] laboratory. All these people sat as an advisory committee, and I have a feeling that's where my name got tossed into the pot.

I struggled with that offer for a while, but I thought, you know—I must have been forty-two by then. I thought, "You know, this is an opportunity for a midcourse correction. Maybe I want to go in some different direction. What a wonderful opportunity to find out whether I want to do that or not!" So I was finally persuaded. I had absolutely no administrative experience, and here I was going to step into a job that was only administration. I'm not sure why they wanted me, except I know that there's a lot of infighting in these programs, particularly in this program. The big laboratories—Los Alamos, Livermore, Princeton, and Oak Ridge—were very competitive with one another. They surely were trying to find somebody that they thought they could get the best deal from. Maybe a neophyte in administration would be an advantage. [Laughter] I don't know. This is all speculation, of course.

So I did think about it, and I finally accepted the job. I negotiated a two-year leave of absence from Caltech—actually it ended up being two and a half years. I left for Washington in the spring of 1970—left the family behind until school was over, and found an apartment there and finally found a house to rent. The one saving grace was that I did inherit this wonderful advisory committee, with Willy Fowler and Sol Buchsbaum. That was really a major force in the shaping of the fusion program. It met quarterly, and more often if necessary.

There was something that happened just before that which is important, too. The

Princeton work on stellarators was going badly. This is what Lyman Spitzer had started. Stellarators: It's like "stellar," from "star." Lyman Spitzer invented the term. It's a technical takeoff for a fusion device that makes energy the way the stars do. Willy Fowler, of course, had been worrying about nucleosynthesis and the burning of hydrogen and helium in the stars, and Lyman Spitzer, in the early fifties, saw that it might be possible to do the same thing in the laboratory. And that's how the Princeton fusion energy program got started, and their main line of investigation was called the stellarator.

You know, the idea of these fusion devices is that you're supposed to get hot plasma to react and make energy. And you have to contain the plasma with a magnetic field. The trouble was that the hot plasma leaked out of the magnetic containers. And it leaked out too fast; if you don't keep it contained long enough, you end up putting all your energy in to get it hot, and you don't get any energy back. The stellarators were really very poor in that respect. People didn't understand why the plasma was leaking out, and it was getting rather discouraging. The late sixties were a period of discouragement, and the budgets were beginning to suffer a little bit. So in some ways, it was a bad time for me to go in. Except that the Russians were having some success with their tokamaks.

Now, "tokamak" is a Russian acronym for *toroidal'naya kamera aksial'nym magnitnym polem*—toroidal chamber with an axial magnetic field. There was a Russian scientist named Lev Artsimovich who came to the United States in 1968 and was making these great claims for his tokamak device. He was in the Kurchatov Institute, in Moscow. He visited MIT—he couldn't get near the Atomic Energy Commission laboratories, because of the security—so he visited MIT, and he gave a number of lectures at MIT about the experiments done in their tokamak in Moscow. And the temperatures and the confinement they were getting were really quite good—significantly better than any of the stellarator results from Princeton or any other results in the American program. At first, people didn't really believe him.

COHEN: Well, they didn't think Russians could do things back then.

GOULD: [Laughter] Well, I've subsequently seen some of their laboratories, and I wonder how they were able to do some of these things, because the circumstances under which they had to work, the equipment they had, the grimy laboratories… There was a long period of disbelief.

The British put together a diagnostic team to go in and check the Russian results with lasers. They actually took laser apparatus to Russia, got it through customs, did an experiment on the Russian tokamak. And they found that in fact Artsimovich was right, that what he said was true. They *were* getting good results.

So immediately the American community reacted, and they formed a committee—naturally. [Laughter] I forget what it was called, but it came to be called the Kerst committee, after Donald Kerst. He's the one who invited me down to General Atomic in 1959, and I knew him very well. He was back at MURA—the Midwest University Research Association—in Madison, Wisconsin.

The object of the Kerst committee was to study the tokamak results and make a recommendation as to what the US program should do. And I was appointed to that committee. It had all the top fusion people on it, and then they had a sprinkling of people like me who were from the outside—plasma physicists, but not really in the heart of fusion. We ended up recommending that the US program be reoriented and that we take up the tokamak line of investigation in the United States.

That was the background when I went to the Atomic Energy Commission. Budgets were declining, but on the other hand the tokamaks looked very promising. Even before I arrived—about half a year before I arrived, or maybe a little more—the advisory committee had approved the construction of five tokamaks in the United States. And they built five. I can still remember the first congressional budget hearing that I participated in. In those days, there was the old Joint Committee on Atomic Energy, a joint House-Senate committee. And they really ran atomic energy. I mean, when you have a joint committee of both houses, they pretty much have their say—they aren't competing. All the difficulties were ironed out in the committee before a proposal ever saw the light of day. We used to have hearings once a year on this program I directed. I'd never been to a congressional hearing before, so the first one was sort of a trial by fire. And the only thing they really wanted to know was why were we building five? [Laughter] And actually we tried our best to defend it. We brought along all the people who were building the five tokamaks. And we argued that we were trying to do catch-up and also leap ahead of the Russians at the same time.

COHEN: That's a good argument.

GOULD: Yes, and in those days it worked. Each of these five experiments had quite different objectives; they were quite different from one another. The Princeton people fought us tooth and nail at the beginning. But when these five tokamaks were approved, and they could see the handwriting on the wall, they said, "OK, we better get busy. We'll take our stellarator, which isn't working very well, and we'll turn it into a tokamak. And furthermore, we'll do it in four months and we'll beat all those guys." [Laughter] And they did. They got the first results. And indeed they got some very good results. But I must say, I've had a lot of experience with Princeton since then. They have a special not-invented-here syndrome—basically, if it isn't invented there, then it doesn't exist.

Those days in Washington were very interesting. The hearings were fun—well, in retrospect, they were fun. I was really quite nervous, although I think by the time we had our first hearings I had already been doing briefings of congressmen and their staffs. I remember I had a whole hour once with Senator Howard Baker, for example. Howard Baker was from Tennessee, and the Oak Ridge Laboratory was in Tennessee, and Howard Baker had quite an interest in fusion energy at that time. And I must say, he was a delightful person, and very sharp.

Anyway, that was the fun part of it. The bad part was the office routine, which was grueling. We had these things called "impact statements" we had to generate. Every once in a while, Congress would threaten to cut your budget—cut out a piece of the program, or something like that. And the upshot was that the program office would write an impact statement, which was a statement saying how bad it would be if they did that. My boss used to refer to those as "Damn fool requests"—DFRs. [Laughter]

Incidentally, about those congressional briefings, I couldn't go alone on these briefings to the congressional people. They always sent somebody from the Office of Congressional Relations along with me. The fission reactor program distrusted the fusion people, because the fission people thought that these young upstarts would go over there and sell the fusion program at the expense of the fission program. At that time, the fission program wasn't in the kind of trouble it subsequently turned out to be in. And they were somewhat paranoid, and maybe partly correctly, about the kinds of claims we might make for fusion energy—claims that would reflect adversely on the fission program. So every time I would get a call to go over to Congress and give a briefing to somebody, they'd send along a man from Congressional Relations—my intellectual bodyguard [laughter]—to make sure I didn't get out of line.

COHEN: What happened to your lab here at Caltech? Did you just close up shop?

GOULD: No, I left three or four graduate students, and I paid periodic visits to Caltech—as I did when I was on sabbatical in Munich. I had three or four students, who kept it going, and I had some very exciting work going on—some of the nicest work in my whole career was done just before, and then continued during my absence.

COHEN: So you never closed off things here?

GOULD: Oh, no! In fact, I think it would have been devastating if I had let it shrink away.

COHEN: Because you weren't doing any experimental work in Washington.

GOULD: No. I think you've caused me to focus more clearly on what I didn't like about that job—it was a desk job. But I managed to avoid some of that by travel. I would visit all the laboratories. I spent thirty to forty percent of my time traveling. And those were the good days, because then I was out in the lab, and I was talking to people who were doing the experiments, and I was functioning like a scientist again. But then I had to go back to Washington and I had to sit at that desk, and there was all this paper flowing over. You know a congressman would get a question from a constituent about atomic energy—about fusion, let's say. Well, congressional staffers can't answer those questions, so they funneled them over to me. That was my job—to write replies for the congressmen, and it would go back through channels. One of the channels it had to go through, of course, was the fission office, which was called the Reactor Division. They wanted to see everything we were saying about the program to make sure we weren't saying something out of school.

COHEN: So you spent your two years there doing that sort of thing and became convinced that you should go back to Caltech?

GOULD: But see, things were changing, and at first I wasn't totally convinced I should go back. I must say—on this travel thing, the high point of my travel was when I went to Madison, Wisconsin, for lunch. [Laughter] I mean, that's the kind of traveling you did—you'd go somewhere for a day. Of course, we don't think anything of that nowadays. But once I went to Madison, Wisconsin, for lunch, because one of the electrical utilities was giving the university $100,000, or something like that, for their fusion program, and the Madison people wanted somebody to be there to say good things about the program. But I really enjoyed the travel and getting out in the labs. That was the saving grace of this whole thing.

COHEN: It must have been a little hard on your family, though.

GOULD: I guess it was. But I was always home on the weekends. And that's a policy I still have today. I very rarely will travel on a weekend, and of course now it's very popular to have weekend meetings, because of the airfares. Sometimes I'll do it, but very seldom. And now Bunny travels with me a lot.

The fusion budget turned around, for one thing. It had sunk to about $26 or $27 million, and toward the end of my term it went up to $35, $40 million. And the projection for the following year was going to $50 million. Just to jump ahead a bit—in 1974, when the oil embargo hit, the fusion budget just took off. But I could see already that the tokamak work was coming along very nicely. We not only reproduced the Russian results but did much better than they did. So money was beginning to flow again, because things were beginning to look a little more optimistic, we started a reactor-design study—about one or two percent of our programmatic budget. We began to study what a reactor would look like, if fusion power ever came to pass. That was kind of interesting for me, because I had left a reactor-design program at North American because I didn't like it. [Laughter] But nevertheless, you have to do those sorts of things, and that's one of the things we did. So, the lure of the increasing budget, and the fact that things were going to change, and that there were going to be things built, tempted me to stay on. And another thing was that the fusion program was reorganized and made a division in and of itself; that is, it was elevated in the AEC hierarchy. This was when Glenn Seaborg was the chairman of the Atomic Energy Commission. I remember we used to have to go to the commissioners' meetings, and we had to sit out in the hallway, waiting for our turn to go in and make our spiel.

Also, I was elected to the National Academy of Engineering while I was in that position.

And this turned out to be a nice thing to happen, because there aren't many people in government agencies who are academy members. There are a few, but not many.

One other thing I ought to mention, because it's kind of amusing: one time when I was sitting in my office, Paul MacDaniel, my boss, came down and made some funny inquiries. I later realized he was inquiring as to my political affiliation. And I didn't know what that was all about. But a couple of weeks later I got a call from Ed [Edward E.] David who was then the president's science advisor. This was in the Nixon Administration. Well, I am a Republican, so I passed the test. [Laughter] It was a test I didn't know I was taking at the time. But the word filtered back to the White House and Ed David. And I got a call from him saying he'd like to talk to me. I knew him. He used to come to Caltech recruiting for Bell Laboratories on a regular basis, and I knew him quite well from his recruiting trips. Bell Laboratories had a very aggressive—and, I think, enlightened—recruiting policy. They would come and spend several days, learn who all your new students were, and then they would follow them carefully through their career as graduate students. And when the good ones started to come out, Bell would make a very aggressive move to get them. And that's how I got to know Ed David.

He was sounding me out as to whether I would take a job over in his shop as deputy science advisor and, I guess, deputy for OST—Office of Science and Technology. Boy, was I flattered by that! We had lunch. His parting statement was, "Well, if you think you're interested in this job, the next step will be to go over and meet [H. R.] Haldeman and [John] Erlichman because they have to pass on you." [Laughter]

COHEN: Of course, you didn't know then what you know now. [Laughter]

GOULD: Oh, no. But he did say, "I can only promise you a job for nine months, because there's the election coming up." The '72 election was coming up, and this was before Watergate broke.

So I was very flattered by that, and went back and thought about it for a while. I finally decided that I had come to Washington to see whether I had any talents for administration, or could cope with it. I came in a program that I knew very well, and a field of science that I knew very well, and he was offering me an opportunity to go in and look at science as a whole. And frankly, I am not so much on the big picture; I like to zero in on things. And I thought, for one thing, I might not be very good at the big picture. And secondly, I had really come to do a job and I was already beginning to think about my return to Caltech. I didn't want to get diverted into something I thought I might not be very good at. Maybe I would have been—I'll never know. But I already had an inkling that I wasn't going to use this as a midcourse correction. So I guess that's when I must have realized that I'm basically a scientist and engineer at heart. [Laughter] And I like to do my own thing. I really didn't want the responsibility. It was a big enough job for me to take on the administration of the fusion program. I was familiar with the physics of it, but the politics of it and all that stuff I had to learn on the spot.

But I was tempted to stay another year, and there's an interesting little story there. Caltech has a rule that you can take a leave of absence for two years maximum, and I had already stretched it to two and a half years. That was OK, because it involved an extra summer, which didn't seem to worry Caltech too much. But then I thought, well, maybe I'll stay one more year and see the fruits of this increasing budget, and things are beginning to turn around, and I'll have some say in the directions that it takes while the money is increasing. I wrote a letter to either Francis Clauser or Bob Christy—Christy was provost, and Francis was division chairman at that time. Anyway, the word came back, "Well, we have this two-year rule. We can't bend it. On the other hand, if you really want to do this, you can resign, but before you resign, we'll give you a letter of reappointment." [Laughter]

Now, there was a reason for this maneuver. There was another person from the Engineering Division on leave at that time, and he had also run his two-year course and wanted to do the same thing. And they didn't want him back. So that's a little glimpse into the inner workings of Caltech and the kinds of arrangements that are sometimes made.

COHEN: Did they get rid of that person?

GOULD: Well, that person decided he would move on. And I decided not to avail myself of this arrangement, because I realized that our children—our son was just starting high school, and if I stayed another year, our daughter would come back just for her senior year, and that was not a very good arrangement. So I decided I had better come back and get on with it, and I did.

ROY GOULD

SESSION 4

March 21, 1996

Begin Tape 4, Side 1

COHEN: We've got you back from Washington. How did you find Pasadena and Caltech when you came back?

GOULD: Well, while I was away, I had made frequent visits here. A number of things had changed, though. There was this Aims and Goals Committee report, which recommended, among other things, that the division chairmanship become a five-year term, with an option to renew for a second term. So, one of the things that had happened was that Francis Clauser had become the new division chairman after Fred Lindvall. Fred Lindvall had been division chairman forever, but now they wanted to make it not a lifetime job.

Francis served out his five years—in the course of which he had a heart attack—and then we had to look for a new division chairman, and I got appointed the chairman of the search committee. This must have been around '73, shortly after I came back—within a year or so. Another thing that happened, which I'll come back to, is that the applied physics program was off and running by the time I got back. But I would like to talk a little bit about what led up to that.

I'd left some students here. One or two finished while I was away, but I still had a few students. So I went right back into the groove for a couple of years.

COHEN: You moved back into your same house?

GOULD: Same house. We had leased it out. We've lived in that house now since 1960, and it's only the second house that we've owned in Pasadena.

We had a very pleasant interlude in September of '72, when I left Washington. We came back to Pasadena by way of Yugoslavia. [Laughter] We picked up a car in Milan and drove over to the Yugoslavian coast—had a wonderful time! Went down to Dubrovnik. It was just a delightful country, and it's such a shame when you read nowadays what's happened to it. We also went to Sarajevo, and to a lot of places, mainly along the coast and almost all the way down to the Albanian border. So we had a great little respite between Washington and getting back to Caltech.

Maybe I ought to talk about my research interests for a while and then come back to applied physics. They're actually parallel, time wise, but I think it's a little bit complicated to intertwine them.

COHEN: Now, during the period you were in Washington, while you kept up your connections here, you were no longer consulting for these other places?

GOULD: Oh, no. In fact, I also had to sign an agreement—as all government employees do—that I could not represent Caltech in any way to the Atomic Energy Commission on any matters before them having to do with Caltech. But the interesting thing is that when I left Caltech I had a contract from the Atomic Energy Commission for my Caltech research. And that contract continued all the time I was away; it continued to pay my students. It didn't grow, but it didn't decline, and it was here waiting for me when I got back.

As I told you, I had toyed with staying an extra year and decided not to. And when I did come back, everything just sort of fell back into place—except that, of course, I had been exposed to all these fusion-oriented problems. By the time I came back to Pasadena, the five tokamaks that had been authorized were all operating and producing some very interesting and good results, by comparison with the earlier days. So I began to think about whether there was something I could do in these areas in my own research. I had met a young UCLA graduate named Robert Taylor. He's actually Hungarian, and I'm sure "Robert Taylor" is not his real name—it's a new name he took. He'd gotten his PhD at UCLA and then had gone to MIT. I think we even made a feeble attempt to hire him here at Caltech at the time he was leaving UCLA with his PhD. Anyway, he was at MIT. He was an enormously clever guy, and unbeknownst to the Atomic Energy Commission, he built a tokamak at MIT without their knowing that he was spending their money on it. But it was a very small device, which required only $10,000 or so to build. He scrounged an awful lot of stuff—a lot of old stuff that wasn't being used. And when the Atomic Energy Commission found out about it, they were hopping mad. [Laughter] At first, I think they wanted to take the money back, but then they finally realized that here was a very clever guy who had done something really rather ingenious.

I knew about all this. And one of my former students—the one I mentioned from Brussels, Paul Vandenplas—was at the same point in his thinking about plasma physics research, and he happened to be visiting me. This was probably in ’74. And we made a trip to MIT to talk with Bob Taylor, both with the idea of finding out how you build these things and with an idea of coming back and building one ourselves and reorienting our research.

It turned out that Bob Taylor was willing to manufacture these things on the side. He knew all the machinists in the Boston area. So he actually built one for Paul Vandenplas, and they exported it to Brussels—and until relatively recently it was functioning in Brussels.

I, on the other hand, came back with the idea that I wanted to make some changes, and I wanted to do it myself. There were some particular problems I wanted to pay attention to, that I thought I could do better than Bob and improve it a little bit. So I came back, and I did build one. I probably started it in ’74, and by ’76 it was finished. So we had a small tokamak right here.

The goal of the program was to look at the propagation of waves in plasmas in toroidal geometry. The tokamak is a torus, a doughnut-shaped container. Heating was one of the problems that had to be solved—getting it very, very hot, up to a million degrees, or something like that. One way to heat the plasma is to put radio-frequency waves in and stir up the particles with the waves, but this was an idea that had not yet caught on in this country. There were a lot of people dabbling in it, but the big laboratories were not doing this. So I thought this would be a great area for us to go into, and we did. Two or three students did PhD theses on the propagation of waves around the ion cyclotron frequency, and it was a pretty productive time. Except that finally the laboratories got on to this idea and decided that there actually were some advantages to this sort of heating. And almost overnight, the big laboratories started doing the same thing. So our position really was untenable, because the university programs in those days were running on a few hundred thousand dollars a year, and the big laboratories were running on $10 million or $15 million, $20 million a year. So when they decided to do something, they could put a great deal of effort, money, and people onto a problem—and because their objective really was simply to get the plasma hotter, often they didn’t study the fundamental problems that needed to be studied. Nevertheless, it was hard to compete with them. And it’s a little annoying to have them move in on you like that. When we were doing the work, they hardly paid any attention to us; they always used to say, "Well, these little tokamaks—they're really not like the big ones, so the results you get don't transfer very readily." So we had two problems, working in that area. One was that when we got a result the big laboratories didn't believe the result, because our plasmas were not as hot and not as big. There was always that problem of credibility. And there was also a tendency in the big laboratories to think that if it wasn't their idea, then it wasn't a good idea.

So we turned to another area that the big labs seemed uninterested in, and that was the study of fluctuations and turbulence in these plasmas. There were a lot of back-of-the-envelope calculations that suggested that these plasmas were noisy and turbulent and that was why they were leaking out of their magnetic container, so I had a couple of PhD students do measurements of the fluctuations and turbulence.

So we studied these problems for three or four years. It was very hard to prove that turbulence was responsible for the leaky magnetic bottles, but we got some very nice measurements of the kind of turbulence that existed in these plasmas. Turbulence, of course, is a phenomenon that's been around for years in ordinary fluids, but it was just beginning to get some attention in plasmas.

Then again, in just a few years, we learned that several of the major laboratories were taking this up. So, again, it seemed hard to keep ahead of them. And even when you did manage to keep ahead of them, it wasn't clear, when they finally did move in on an area, that they paid much attention to what had gone before.

COHEN: Did they come and visit you? Or did they just read the literature? Of course, your students had to go somewhere. You seemed well blessed with a lot of good graduate students.

GOULD: Yes. But not many of them went to Princeton. And Princeton was *the* major laboratory for these things at that time. One of my students who worked on ion cyclotron heating did go to Princeton. One of the reasons they wanted to hire him was that they had gone into this field in a big way, and here was a man who already had expertise in the field. So they wanted him because of what he knew, not so much because of what he had done.

It was just hard to keep ahead of them. That period started in 1975 and went to 1985, roughly. I'll come back to the division matters a little later. But during this period—the last part of this period, 1979 to 1985—I was division chairman. One thing that caused matters to go downhill is that I didn't take on any new students during that time, and it was really hard to keep my research program together. It was kept together by two people I had had as postdoctoral fellows. One was a very smart woman, Paulett Liewer, who had gotten her doctoral degree at Maryland. She's now at JPL, in numerical computation. She was a theorist, and she wrote a marvelous review paper on turbulence in plasmas. She actually got some good PR from that article, and by that time it was a really hot problem.

The other postdoc was Stewart Zweben. I got a call one day back in the middle seventies from somebody at Oak Ridge National Laboratory who said, "We've got this student from Cornell who's been doing his thesis down here, and he's spent all of his time at the Oak Ridge National Laboratory doing his PhD thesis. And we think he ought to get a little university experience." [Laughter] Unfortunately, I didn't pick up on that one fast enough, and Bob Taylor, who had come back to UCLA, got him. And Zweben stayed at UCLA for a couple of years, and then he got a little bit restless and was looking around. And then I got him. He was here for maybe five years—the five critical years when I was division chairman. And he really kept the program together—he and Paulett—and I just sort of kibitzed during that period of time. But eventually Stewart left and went to Princeton, where he is now a major figure. And that was the beginning of the end of the tokamak era, as far as I was concerned. Some really interesting work and half a dozen good students.

The other thing that was happening is that the fusion devices were getting bigger all the time. They had some major successes, and every time there was a success, people said, "Let's build a bigger one." And when they built a bigger one, it was even better. There's a basic reason for that: the leakage from the bottle depends on the extent of the surface; if you make the container bigger, there's less surface to volume, and the core gets hotter and hotter. So it was a very simple explanation. When I was in Washington, we were projecting that we'd get as much power out as power in, in something like ten years. And of course those projections were not met. They have now been met—it's just taken about twice as long as people thought it would take, and it's taken a lot more money, because they had to make the devices so much bigger. And, of course, as the devices got bigger, our little devices at Caltech became less and less relevant.

Frankly, I got to a point where I really couldn't see interesting yet relevant problems that we could do on a small scale that the fusion community would pay any attention to. The field had really become big science. The budgets mushroomed, from the order of $30 million a year when I was in Washington. The oil embargo of 1974 precipitated just a great expansion, and the budgets got up to about $500 million a year at the peak. We're talking about the late seventies and early eighties.

Now, of course, the situation is changing. I think, politically, energy is not regarded as a serious problem anymore, so all the energy research programs that were started in the late seventies, early eighties—solar energy and so forth—have all been stopped. Budgets are going down. But we know that time will come again; we just don't know when. Meanwhile, oil is cheap, so no one cares.

Now let me go back to my return to Caltech from Washington, and talk a little bit about another thing that happened. I also was elected to the National Academy of Sciences around '74. I got a letter from Reimar Lüst, whom I mentioned earlier, who had been a visitor to astronomy here at Caltech in the fifties and whose lectures I had attended. I had seen him off and on through the years. He had become president of the Max Planck Society in Germany, which is the society that runs many of the German research laboratories. He felt that the Garching Institute for Plasma Physics, which I had visited in '63-'64, was in need of a visiting committee. It had not had one. Their term for a visiting committee is a *Beirat*—it's basically an advisory committee.

This was a very appealing idea, because not only had I spent that year, or nearly a year, in '63-'64 but I'd also spent a summer at Lüst's Institute for Extraterrestrial Physics in '67. That was also in Garching, just next door to the plasma institute. They were setting off barium clouds in the upper atmosphere to look at magnetic field lines and the effect of the solar wind. The barium clouds enabled you to visualize basically what was happening up there.

So about '73 or '74, I got this letter from him, saying they were forming an advisory committee and would I like to be a member of it. And he sent me a copy of the by-laws. They're a lot more formal there than we are here. The term was seven years, and they were going to meet annually. And I thought, "Gee, what a great opportunity! I can go to Munich every year and visit my friends." [Laughter] So I accepted. I got there for the first meeting, and about five minutes before the meeting was to begin, Reimar sidled over to me and said, "Roy, I'd like you to be the chairman of this committee." [Laughter] Well, I've come to understand why

Reimar has gotten where he is. In addition to being a great scientist, he's a great politician and knows how to get you to do something. He knew I would bite on being a member of the advisory committee, but he probably wasn't sure whether I would agree to be the chairman. There were only two non-Germans on the committee, and I was the only American. The other non-German was a plasma physicist [Bohlenert] from Stockholm. All the rest were Germans from other institutes, and there was only one other plasma institute in Germany, so they were drawn quite broadly. There was the head of what here we would call the National Institute of Standards and Technology, and a couple of very well-known physicists—not plasma physicists but well-known in other fields. It was really a very good committee.

The problem was that the Garching institute was more like a university than like a fusion laboratory. And it was the biggest Max Planck institute in all of Germany: it got about half of its money from the Max Planck Society and the other half directly from the German government for energy research. So it really wasn't quite like the other Max Planck institutes, and it was under a lot more pressure than some of the other institutes. There really weren't any plasma physicists on the university staffs in Germany. There was the Technical University at Munich, and the University of Munich, and anybody there who wanted to work in plasma physics got sent out to the Plasma Physics Institute to do it, and then a university professor would act as a nominal supervisor and sign off on the thesis.

But the problem was that the Plasma Physics Institute was a place sort of like Caltech, where there are literally dozens of small groups of two or three people all doing their own thing. And while there were a few serious fusion experiments, they, too, were pretty small, and didn't attract a lot of attention in the larger scheme of things. And Reimar Lüst felt that this laboratory ought to step up to the big experiments and become a real fusion laboratory for Germany. I'm not quite sure who was the director when I first started on that assignment, but I think it was Arnulf Schlüter, whom I mentioned before—an eminent scientist and very well known, with an excellent reputation. He'd been director for quite a long time. But in one way or another there came to be a new director—I don't know whether Schlüter decided it was time to step aside or what.

The new director, Rudolf Wienecke, was an experimentalist and not as well known. I think that choosing a director of a laboratory there is really like choosing a division chairman here. It's consensus politics and you have to find somebody that everybody will more or less agree to. And I think Wienecke was kind of the lowest common denominator. I don't mean to denigrate him, because he turned out to be a very good leader. But he didn't have the confidence of some of the people there. And one of our jobs as a committee was to try to back him up on a lot of the things he needed to do. And that's what really made this assignment very interesting. I think Wienecke was director for only about five years, but it was probably the middle five years of my term as chairman of this committee. And I got the impression that they really paid attention to what we said, unlike what happens with most visiting committees. Most visiting committees are kind of pro forma, and if you recommend something that the place wants to do anyway, that's just fine. In our case, Wienecke was in the position of trying to whip this laboratory into shape and get it to do certain things, and there was a lot of dissension among his people. The various group leaders—there were probably about ten of them—were distinguished scientists in their own right; they were like full professors around here, and they did what they damn well pleased. [Laughter] He really didn't have much power, except that the handwriting was on the wall and they had to change the laboratory. He was trying his best to do that, and we on the advisory committee tried our best to help him. So it was really a very interesting period. He took quite a beating, I think, in this transition—and it was a successful transition, I might add. But he really got burned out, so he went back to Stuttgart to a university position.

Then a new director came along—Klaus Pinkau, who's now the director, who is a superb science politician. He was a very good physicist and commanded a great deal of respect. But the transition to a fusion laboratory had already been made, and he stepped into a going laboratory. Maybe if he had been available earlier, he would have been a good person for that. Actually, he had been director of Lüst's Extraterrestrial Physics Laboratory, so he just moved across the street.

COHEN: How much time did you spend in Munich—a week?

GOULD: Well, the meetings were only two days. But I would always go a week early and poke around. I found that the only way I could stay on top of what was going on was to go poke around—because otherwise you sit in the conference room and listen to various people give their thirty-minute spiel. First of all, it's so concentrated it's hard to absorb, and secondly, you only get what they want to tell you. So I used to go and poke around. And I could do that, because I knew everybody there. Normally, that's kind of hard to do.

We tried writing the first report by committee, and that didn't work, so I ended up writing the reports. Then I would spend another day or two in Munich to write the report, because I knew that once I got on the plane and started back, it would all evaporate. So I would stay, overall, nearly two weeks.

And then somewhere along the line Bunny started going with me. On one trip, I took my daughter along. And on one trip I went to Russia. I hadn't been to the Soviet Union at all when I was in the Atomic Energy Commission, though we had a lot of dealings with them; there was an international committee of all the directors of all the national fusion programs, including the Russians, the Japanese, and so forth. We used to meet regularly in various places, but I'd never been to the Soviet Union. So I took my son along with me on that trip.

The plasma institute advisory committee activity went on for twelve years, and it met every year. Gradually they broadened it to include members from other countries. First I think they got another American—Harold Furth, from the Princeton Plasma Physics Laboratory. I remember Pinkau agonizing over whether they could put their number-one competitor on their advisory committee or not. [Laughter] And I assured him that I knew Harold very well and had dealt with him a lot when I was in the Atomic Energy Commission, and I thought he could be fair. And he was. Indeed, he turned out to be my successor as committee chairman. That was 1987. But I stayed on the committee for another five or six years and I finally had to twist their arm to get off.

Easing Garching's transition into a fusion laboratory was a great success. I think they're now probably the best—well, there are two other very good world laboratories. Princeton's still very good, although I don't know what this bunch of changes is going to mean, and General Atomic in San Diego is also very good. And there's a Japanese laboratory that's very good. But of the European laboratories, Garching is now *the* laboratory. They have the most diversified program and they have done some really outstanding work in that ten, fifteen years. So I feel really quite happy to have watched all that happen, and to have had a small part in it.

COHEN: Let's come back to Caltech. Do you want to say something about applied physics?

GOULD: Yes, I think so. Let me go back a ways. First of all, the applied physics program came into existence in 1970, when I was away. When I came back, it had emerged full blown.

COHEN: Whose effort was this?

GOULD: Let me go back and explain what led to it. There were several factors. Over the years, the Engineering Division, and particularly electrical engineering, had acquired faculty who were interested in solid-state physics and solid-state electronic devices—lasers and masers and things like that. And these fields required people with physics backgrounds. Plasma physics, I should add, did too—although plasma physics was here pretty early on. But the physics department was concentrated in high-energy physics, astrophysics, low-energy nuclear physics, and of course theoretical physics, which was superb—but not in any of the applied areas—quantum electronics, solid state, things like that. I remember that when Bob Bacher was provost I used to have regular conversations with him about this: Why couldn't Caltech get into some of these fields, which were clearly technologically very important and yet very good physics? And Bacher's view was that it would be hard for a place like Caltech to compete with Bell Laboratories. But I didn't think he was right.

One person I had a hand in getting here was [professor of applied physics] Amnon Yariv. In fact, I can remember, when I was in Munich in 1963, making long distance phone calls to Fred Lindvall to please get going and make this appointment. Those were the days when you didn't have to have a search; you didn't have to have dozens of letters. I had known Amnon from his days as a graduate student. Amnon did his PhD at Berkeley with John Whinnery; he got his PhD a few years after I did. He went to Bell Laboratories, was at Bell for a couple of years, then went to the Watkins-Johnson Company in Palo Alto. I got word that he wasn't terribly happy there, so I leaned on Fred Lindvall, saying, "Here's this bright young guy; get a hold of him." Then when I went off to Munich, it nearly went under, because things stopped happening—but I do remember those phone calls.

So Amnon came to Caltech. He was interested in solid-state physics. And other people came. Floyd Humphrey came in magnetism. Charlie Wilts changed from control systems to magnetism. So we were building a small activity in solid-state physics in the electrical engineering department.

These were rather abstract discussions I used to have with Bob Bacher. I enjoyed my discussions with him very much; I wasn't going in complaining to him, but we had philosophical conversations about directions in the institute and where I thought the institute could go. And these conversations never seemed to hit a responsive chord, although every once in a while I would hear him repeat back to me something I'd told him in a previous discussion. So he was listening. He was absorbing.

COHEN: Did these people whom you mentioned all have physics degrees?

GOULD: No. Amnon got his degree in electrical engineering with John Whinnery. Floyd Humphrey got his PhD here in chemistry. Charlie Wilts, I think, simply moved into magnetism partly because of Floyd Humphrey, and they made a team. We hired Nick George. Nick George did a PhD thesis here—he was interested in lasers. So gradually these activities began to build up in electrical engineering.

At the same time, there was a related interest in aeronautics. Hans Liepmann and one or two other faculty were doing magnetohydrodynamics. I think I mentioned that earlier. Hans was basically interested in the physics of fluids. And he is a first-rate physicist—in some sense "aeronautics" may be a bit of a misnomer for him, though he has made wonderful contributions in aeronautics as well. He was doing what one might call applied physics in that period. Later on, he went on to do low-temperature fluids, which was more physics than engineering. So all these things were happening in the sixties, and they were mostly happening in engineering. I was really trying to get the physics department to move in this direction, and nothing much happened.

Meanwhile, a lot of students were applying to do graduate work in physics—very good students—and the physics faculty didn't want them. These applicants wanted to do, say, solid-state physics. You know, a lot of these students didn't know what Caltech did, what its strengths were, but they would apply to Caltech, I presume, because they had heard a lot of good things about Caltech. They probably didn't know that in the physics department there wasn't a field they were interested in. So the Caltech admissions committee was turning away a lot of very good students.

So I got on the physics admissions committee, and we tried to funnel these applicants over to electrical engineering. There was a lot of applied physics going on, and it was all under the aegis of the electrical engineering department, and we were missing some really good students. A lot of these people—Humphrey, Wilts, Jim Mayer [James W. Mayer, professor of electrical engineering], who had come to do ion implantation, and Marc Nicolet [Marc-Aurele Nicolet, professor of electrical engineering]—wanted students with good preparation in physics. And I was the one person who had a foot in both camps; I was a member of both divisions, and I was on the physics admissions committee—mainly to make sure that these applications didn't sit around in the bin until it was too late. In fact, finally, the other members of the committee caught on to this, and they would say, "Here's one you ought to look at."

But then we found we had a hard time persuading some of these students to come. Some of them wanted to be admitted into physics, and we had trouble talking them into going to graduate school in electrical engineering instead. They wanted to do physics, but they wanted to do a kind of physics that wasn't being done at Caltech. And this was one of the reasons behind the move to establish an applied physics program.

COHEN: So when did that finally come in?

GOULD: Well, it came in 1970. There was a committee. I was involved, but most of it happened while I was away. The prime movers were, first of all, Hans Liepmann, and secondly, Floyd Humphrey. I think the program first appeared in the 1971-72 catalogue; there were some new courses introduced.

COHEN: What division was this in? Was this in Engineering?

GOULD: It was an interdivisional program. It was patterned after applied mathematics. Applied mathematics started when Gerry [Gerald B.] Whitham came here. Gerry Whitham was originally brought here with strong support from Bob Bacher. Bacher was really very influential in getting applied mathematics started; I think maybe he was worried about mathematics in that period and thought that bringing in some applied mathematics would be a way of strengthening things. I don't know exactly how he got onto Gerry Whitham, but when Gerry Whitham came [1962—Ed.], he was a professor of aeronautics and mathematics, and then within a few years the applied mathematics program materialized. Besides Gerry Whitham, there was Julian Cole and two or three other people. Joel Franklin was in the Engineering Division at that time. I don't remember what his title was [professor of applied science—Ed.], but there was as yet no applied mathematics. Gerry Whitham was the catalyzing element that brought all these things together into applied mathematics. It was a joint program between the Engineering Division and the Physics, Mathematics, and Astronomy Division. So the applied physics program was supposed to operate along those same lines. [Tape ends]

Begin Tape 4, Side 2

GOULD: David Goodstein was interested in low-temperature physics and in statistical physics. Jim Mercereau was doing low-temperature physics. And Hans Liepmann was doing low-temperature fluids at that particular point in time—he had moved out of the MHD and plasma area. And I was joint of course, in physics and engineering. So there were three people from physics, one from aeronautics. And then there were Mayer, Nicolet, Wilts, Humphrey. Amnon Yariv had been here seven or eight years already. And I think one of the things that Hans promoted—certainly something we all wanted to see happen—was to get somebody who was really right in the middle of solid-state physics. And that turned out to be Tom McGill. So Tom McGill joined the faculty [1971—Ed.]. I think Tom, if I recall correctly, was a PhD student of Carver Mead's, but had been doing solid state. Carver himself has done a lot of solid-state physics—he was actually doing the physics of electronic devices and semiconductor physics, and that's what Tom McGill worked in—but he was interested more broadly in solid-state physics.

David Goodstein created a new course called "States of Matter," which was really a course in thermodynamics and statistical physics. That was an area of graduate study totally lacking in the physics department. There used to be a course in thermodynamics and statistical physics in the physics department, but it hadn't been given for years—kind of a glaring omission, I think, in the graduate program. Anyway, the applied physics program offered that, and it became one of the requirements for all graduate students. Many of the requirements could be met by courses in other departments—requirements like quantum mechanics could be taken in physics. Now, they're taken either in physics or in chemistry, because there are quantum mechanics courses in both departments and it depends on what perspective you want to get on the subject. So the applied physics program was actually built around things that were already in place.

There were two other courses already in place. When I came back from my sabbatical in 1964, I had started a course in plasma physics, and I decided to offer it in the Physics Division, because that's where it would draw the largest audience. I had already given some lectures in aeronautics the year before I went away on sabbatical, on plasma waves—that was a period when several of the aeronautics faculty were interested in these kinds of things. And then, after Amnon had been here for a couple of years, he started a course in solid-state physics. And for the same reason, he started it up in the Physics Division, even though he was a professor of electrical engineering. I think we all felt that these courses would draw from people all over the institute much better if they were called physics courses than if they were called electrical engineering courses.

You can see that all these factors were intertwined, and all kinds of things led up to the formation of the applied physics program. And it's been quite successful—its real core of people included Goodstein and Mercereau and Liepmann, of course, and others in Steele, the electrical engineering building. Noel Corngold was associated with the program of engineering science, which was kind of a predecessor of applied physics. It had some of the same objectives, but it was a free-standing academic program completely in engineering. In fact, I got three or four really good graduate students through that program; I was part of it, in that sense. There were only a few courses offered, but they would farm graduate students out to whoever would take them, and I got some really good ones through the program of engineering science. That's where nuclear engineering was when we had nuclear engineering. We all talked to Noel and asked him if he wouldn't like to become part of applied physics, which he did. So that further augmented the applied physics program.

Bill Bridges came, too [1977—Ed.]. Bill Bridges had been here as a Fairchild Fellow and despite the admonitions against hiring Fairchild Fellows to our faculty, we did in fact hire Bill Bridges, and that was a great catch. Bill came to Caltech from Hughes Aircraft Company. He had a PhD in electrical engineering from Berkeley and was one of our really outstanding appointments. We were lucky to get him.

So Steele was just bulging at the seams. For a brief period, things were really awful in Steele, and we had what Tom McGill likes to refer to as "Space Wars." [Laughter] Bob Cannon by then had become division chairman—he was chairman of Engineering from ’74 to ’79. He was the person we were dealing with, and we finally managed to get some space in Keck—a fair fraction of the second floor of Keck—and McGill and Bridges and a couple of other people moved over there.

Let’s see. Harold Brown was here from ’69 to ’77. Harold Brown had reached some kind of agreement with Tom Watson [Thomas J. Watson, Jr.] to build a building—the Watson Laboratories of Applied Physics. And my understanding is that it was kind of a handshake or verbal agreement. And when Harold Brown left, it got off the track, because it was really a deal between Harold Brown and Tom Watson, Jr.

By that time I was talking to Bob Christy about things like this. Christy was the provost, and when Harold Brown left he became acting president. Anyway, some of those dialogues I’d had with Bob Bacher continued with Bob Christy. And one of the things I mentioned was this problem of space. And Christy, I think, really was the one who managed to get the Watson building back on the track. And it did get back on the track, finally—not without some problems. Tom Watson wanted a big fancy building with a great impact on the campus, but it was only a $4 million project. He hired an architectural firm, HOK—Helmuth, Obata & Kassabaum. Obata was the architect. He visited the campus a number of times, and he liked the Athenaeum. [Laughter] Don’t we all! So he wanted to build an Athenaeum-like building, but it couldn’t be done for that kind of money. There were a few abortive attempts to make something that was like the Athenaeum. There were actually three or four dry runs on building designs, and it looked like the project would go down the drain. Finally, he came up with the design we have now. And I remember—by then I was division chairman of Engineering—that Dave Morrisroe [vice president for financial affairs] and I went to New York for a meeting with Tom Watson, Jr., and the architect met us there. And we were all kind of quaking in our boots as to whether Watson would like this new design or not. The architect had a scale model, drawings, and renderings—and Tom Watson didn’t seem to be interested. Well, it turned out that one of his dearest friends had died several days before, and one of his architectural friends had died a few months earlier. Other things were on his mind. By that time, he really didn’t care, and we all heaved a sigh of relief, because at least the building was approved. And that’s how the Watson Laboratories came into being. I guess it was actually completed in ’82. And that relieved the space problems I mentioned earlier. All the people who had moved into Keck moved into

Watson. I had been in Steele all during this time, and I moved into Watson. So did Paul Bellan, who had come [in 1977—Ed.]—another person in plasma physics. So we actually had a group now of two people in plasma physics. Over the years, a number of other people have joined the program, as fractional participants. Tom Ahrens in geophysics takes students—I don't know whether he changed his title. But Goodstein and Mercereau changed their titles to "physics and applied physics." And for a while Hans Liepmann was professor of aeronautics and applied physics. I changed my title to professor of applied physics, and I became executive officer for applied physics in 1973. Francis Clauser and I felt that the program needed more visibility, and we thought that maybe having an executive officer would do that. I think it did help some, although an executive officer around here has absolutely no power—some responsibilities, but no power. [Laughter]

COHEN: They can make trouble!

GOULD: Oh, yes. In fact, by not doing some things they should do, they can make trouble, and also they can be incompetent. Hans Liepmann used to have a good phrase for that. He said, "People can either be statically incompetent, which means they don't do anything, or they can be dynamically incompetent, in which case, they could do a huge amount of damage." [Laughter]

I was going to mention a few others. Bill Goddard got his PhD in Engineering in materials science. And he has moved over the years—obviously, to the Chemistry Division, but he's still materials-oriented. And so he has always had a foot in the applied physics camp, albeit just a fraction. And then when Ahmed Zewail came [1976—Ed.], his field of interest was chemical physics. Brad Sturtevant, in fluid dynamics, and later Paul Dimotakis. They would take applied physics students, they would attend faculty meetings, they would try to look after the program and see that it served all of our needs.

COHEN: Well, at least then you weren't losing these good students.

GOULD: That's true. And occasionally we would feed other departments good students. I think the flow has been more out of applied physics. I mean, Zewail would get a student from time to time who wanted to work with him, as would Goodstein—or Mercereau, when he was here. So it grew to be quite an activity. I ceased being executive officer, and Noel Corngold does those chores that need to be done, although he doesn't actually have the title of executive officer.

ROY W. GOULD

SESSION 5

March 28, 1996

Begin Tape 5, Side 1

COHEN: We have you coming back from Washington. And so I think you can now speak about your life at Caltech from then on.

GOULD: I talked last time about how applied physics had gotten started in my absence. When I came back, it was a going concern. In fact, Tom McGill had been appointed as the first new faculty member, to get things off to a good start. Since my interest was squarely in the middle of applied physics, I decided to ask that my title be changed from professor of electrical engineering and physics to professor of applied physics—although it would continue to be professor of applied physics in both divisions, because it was a joint appointment. I think I talked already about some of the people who changed their titles.

Then in 1973, Francis Clauser asked me to be executive officer for applied physics, to give it more visibility, and I gladly undertook that job. I think I spent most of my time worrying about space problems. At that time, John Pierce was executive officer for electrical engineering, and we were both occupying the Steele building, and John didn't have any taste for dealing with space problems. So that fell to me.

COHEN: How about teaching? Were people teaching in all the other departments they were in?

GOULD: Well, there was a core of applied physics faculty, of which I considered myself one. And Tom McGill, of course, was the first faculty member who was *only* applied physics. And Dave Goodstein started a new course in states of matter, for which he wrote a book. [*States of Matter* (1975)—Ed.] He was really core, as far as the teaching was concerned. And Amnon Yariv's course in solid-state physics was moved over from physics to applied physics. My course in plasma physics was moved over into applied physics. Floyd Humphrey was involved in laboratory courses for undergraduate students, and he created a projects lab for undergraduate students in applied physics. So it was put together piece by piece. Hans Liepmann at one point was very active in teaching the undergraduate course in thermodynamics. Thermodynamics had always been given in mechanical engineering, and there were a lot of discussions about how practical it should be versus how theoretical it should be. At one point, Hans was complaining about this, and somebody said, "Well, why don't *you* teach it, then?" You know, the usual reaction, when you complain about something. So Hans ended up teaching thermodynamics. It was a wonderfully popular course. In fact, it was popular campus-wide.

I also taught the graduate course in electromagnetism in the Physics Division a number of times, in alternate years—the course that had replaced Smythe's course when Smythe retired. Mine was a totally different kind of course. It was actually a course that Bob Christy had given in earlier days, and it was more theoretically oriented, more conceptually oriented, whereas Smythe's was a problem-solving course. Actually, everything that was being done in applied physics was already being done in some way or another, with at least one exception, and that was Goodstein's course on states of matter. That had been a big gap in Caltech's teaching, and he stepped into that gap.

COHEN: And he's such a good teacher.

GOULD: Right, absolutely. I did a section of freshman physics this last term, and I attended a lot of his lectures. And he's very good. I enjoyed the lectures a lot.

Let's see. I had been chairman of the search committee for the division chairmanship that preceded mine. We picked Bob Cannon. Bob Cannon came in '74 and was here for a five-year term. He made a number of really good appointments. There was beginning to be some turnover in the division, with retirements and that sort of thing—mostly retirements. Because, you know, there was a great expansion of the Engineering Division right after the war. The whole institute was expanding right after the war. So some of those people were now nearing retirement age, because they hadn't been just out of their PhDs when they were appointed originally, in the fifties. So by the time the seventies came along, there were some new appointments to be made. Bob Cannon made about ten or twelve new appointments, and they were really quite good.

COHEN: Where did Bob Cannon come from?

GOULD: That's an interesting question, too. He had most recently been with one of the government agencies—I think it was the Department of Transportation. But basically he was a Stanford professor of aeronautics who had taken a government job—perhaps something like I did with the Atomic Energy Commission. I don't know how long he had been in this position—probably not more than three or four years. [Cannon was Assistant US Secretary of Transportation, 1970-1974—Ed.] But we had a search and we finally zeroed in on him. I don't remember the details exactly, but Harold Brown had admonished the search committee to look at outside candidates before we looked at inside candidates. So we took his admonition pretty seriously, and in fact did decide to go outside. It's kind of hard to find division chairmen for Engineering. Engineering and Applied Science is a rather diverse group of faculty. When you think about it, there is some attempt to cover all of engineering in some sense—mechanical, civil, electrical, computer sciences, and aeronautics and applied physics and applied math. Compared to other institutions that try to cover the same scope, the faculty is very diverse and rather small, and it's hard to find a division chairman from inside. We all go our own separate ways. There's a saying—I don't know who first told me this—but most of the people you'd want to be division chairman don't want to do it, and the ones who want to do it are not acceptable for some reason or another, or at least not optimal. And I think the Engineering and Applied Science Division has more trouble with this than other divisions. I suppose that Physics, Mathematics, and Astronomy must have some parallels, because that division has three major subgroups. But in Engineering and Applied Science, there are something like ten groups, and even within those groups there is diversity; they're not coherent groups. Applied mathematics has six or eight faculty—some are numerical, some theoretical. So there's really not a great concentration in any one area. That's probably a characteristic of the institute as a whole, though in some areas there are concentrations. But it's hard to find somebody who has even a rudimentary understanding of the whole field; plus the fact that there are the inevitable little personality quirks and so forth that you usually encounter.

COHEN: There's another thing, too. You're dealing with the growth of new fields all the time. You have fields coming in that didn't exist before.

GOULD: That's true. I know electrical engineering a little better than some of the other fields, but I think it's true of the other fields as well. They're in a constant renewal process. Electrical engineering has changed enormously since I was a student. The subjects that are studied, the kinds of things that are done in electrical engineering, are totally different from what they were forty years ago. But I think it's the same in the sciences, too. Some new subject comes along, and people recognize that it has potential importance—some earlier than others—and they jump into it. So if we choose our faculty well, they will lead us into these new areas.

COHEN: Of course, Caltech is unique in this.

GOULD: That's certainly the objective, and we seem to succeed at it very well.

COHEN: So who were some of these appointments that Cannon brought in?

GOULD: Well, I'll have to think about who they were. But let me just describe one problem that I inherited from Cannon. With all these new appointments, of course, you have tenure decisions to make. Or if not tenure, then there's a reappointment decision to make. Normally, appointments are made for either three or four years—the initial appointment of a young PhD without previous academic experience would be for three or four years. And then there would be a review and a reappointment if work is satisfactory. And in more cases than not, it is. And then near the end of the sixth year, or something like that, there's a review for tenure.

So these new appointments were mostly in this situation, where they either had to be reappointed or considered for tenure. It was a mixture of both, actually. And I remember a meeting where the senior division faculty met in one of the conference rooms, with the big blackboard full of names, organized according to some kind of logic I'm not sure I really understood. But we now speak of it as taking one from Column A and one from Column B and one from Column C. And that kind of procedure doesn't work at Caltech. These are all individuals.

Bob Cannon left before these decisions were made. He decided to return to Stanford—I'm not entirely sure what all of his reasons were. But that problem was left unsolved for the next division chairman.

COHEN: You mean the problem of how to give people tenure?

GOULD: Yes, to conduct these reviews. This meeting was to have been a discussion. The Engineering and Applied Science Division has functioned by consensus politics, more or less—unlike the other divisions. Though I think it's probably true of PMA, too, at least in those years that I'm familiar with. There were meetings to discuss these cases, but votes were never taken and recorded. So it was up to the division chairman always to determine what the consensus of the faculty was, and oftentimes he would have to listen to inputs made privately, too. And there's a risk connected with that, because some people might make private inputs that they weren't willing to make publicly, and some of these inputs might be thought by others to be unduly influential. But it's basically up to the division chairman to listen carefully to these comments. And customarily what used to happen at the division faculty meetings when we were considering promotions, reappointments, new appointments, and so forth was the emergence of a sense of what the faculty wanted. And then the chairman would summarize what he'd heard, and say, "Well, based on what I've heard, this is what I'm going to do." And then, of course, there was a period in which, if people objected, they could come and talk to the division chairman. That was the way things functioned.

It's recently changed. The current provost wants the Engineering and Applied Science Division to vote on its appointments. Part of the reason is that the attendance at these meetings is somewhat sporadic. You certainly can't get a hundred percent of the faculty at any given time. You're lucky if you can get half.

Anyway, I think the main job of the division chairman is to manage this appointment and reappointment and promotion process, and to represent the division to the president and the provost—and to the Board of Trustees, occasionally. We have visiting committees that come around and offer advice, and we take problems to them. Again, the division chairman's job is not much different from the executive officer's job, except that it applies to a much larger group of people. The Engineering Division faculty has typically run between seventy and eighty faculty. And back to the other point—it covers a diverse field. Seventy or eighty would probably be the number of professors of electrical engineering and computer science at Illinois or Berkeley or MIT, where the interests are more homogeneous.

But I think the job is basically managing the appointments. The initiative for new directions really comes from the faculty. Caltech is really a remarkable place. The faculty runs this place, in a way. They are responsible for the new ideas; they're responsible for the new directions that the institute goes in; they're responsible for getting the money to do it. And I think being a member of the faculty carries a high degree of responsibility. You've got to do things; you've got to be good at what you do. But basically, you do what you please. And if you don't do well at it, you know it, and the rest of the division—or at least a small subset of the division—knows it, fairly quickly. And that's the way the tenure decisions and the reappointment decisions get made. More often than not, faculty members will leave, recognizing that they're not doing so well. So that spares you from having to turn them down.

But back to the matter of reappointments and tenure—that occupied a good part of my first year as division chairman. And I think all of that went very well.

COHEN: Who was provost at this time?

GOULD: Well, Bob Christy was the provost when I became division chairman. Harold Brown had left in 1977, and Christy had become the acting president; and then when Murph [Marvin L.] Goldberger came in 1978, Christy went back to being provost. So Bob Christy was provost for a year or so. Then he stepped aside and Jack Roberts became the provost for about three years. And then Jack stepped aside and Robbie [Rochus] Vogt became provost.

One of the things a division chairman has to do is assess the various ideas and proposals for new appointments and present them to the administration. The most critical things are the new faculty appointments, because in the long term that dictates the future. So the main thing is to get the new appointments and the promotions right. Not that this is necessarily the thing that takes the most time, but I think it's the most important thing that can be done. And Bob Cannon had done a pretty good job of identifying people—or the search committees did. You know the process, basically? The division chairman tries to assess from the faculty where the promising new directions are, and which areas should get new appointments—where the ideas are good, and where there is some turnover. There's a strong element of continuity: I haven't seen many departments phased out around here. What happens is that they change. A faculty group is likely to remain intact, but as people leave and new people come, the orientation and the interests of that group change. So you've got to be able to assess where you can sell a good appointment to the provost. You've got to get a search committee authorized by the provost. And the division chairman has to constantly be listening to all the inputs he gets. He has to assess where the best ideas are coming from, where the needs are the strongest, and then go convince the provost that that's really where the next appointment ought to be made, and negotiate with the provost and discuss funding.

COHEN: So what was going on when you were division chair? Which were the areas that you felt you had to…?

GOULD: Well, but a more important question from my standpoint was, Why did I take the job? Because I belonged to the category of people who didn't particularly want the job. But on the other hand, I knew—from the previous search committee that I chaired and subsequent ones that I wasn't on but was familiar with—that it's hard to find somebody who's willing to put aside what he's doing and undertake this.

COHEN: It's a full-time job.

GOULD: Yes. And particularly someone that the search committee feels will be at least halfway good at the job, or satisfactory. The number of people willing to take on the job is relatively small, and when you eliminate the ones you think wouldn't do as well as you'd like, the number is even smaller. And I guess I fell into that group. I think I mentioned earlier that I had left Caltech as an undergraduate because the electrical engineering program was rather poor. In the intervening years it had changed a lot, but I thought I could do a lot to help strengthen electrical engineering.

Also, computer science had come on the scene in a big way. This was 1979. Computer science had been around for a long time, but now it was really beginning to flourish. Gilbert McCann had an analog computer, and when that era ended he was sort of a nominal leader of computer science, but it hadn't really developed very much. And Bob Cannon had brought in Ivan Sutherland. And Carver Mead started out as a solid-state-device electronics man, but his interests had slowly changed to computer science—or to certain aspects of computer science. So computer science was beginning to grow at Caltech, and it looked like it needed a push and it would be ready to take off. Plus applied physics had pretty well jelled. Much of applied physics has something to do with electrical technology, electrical sciences of some sort, and electrical properties of materials. And these three areas—electrical engineering, computer science, and applied physics—constitute what might be called the electrical side of the Engineering Division's activities. So I thought maybe I might be able to give that side of things a little bit of a boost; certainly that's what I spent a lot of time on.

Well, first, as to electrical engineering: there were a number of new appointments in electrical engineering, and things did get strengthened a bit there. I was division chairman from '79 through the calendar year '84, so this was in the first half of the eighties. I remember Bill Bridges and I went on the road a number of times. We went to Illinois, and we went to Michigan. We went to Bell Labs and IBM. We were trying to identify both areas and people—find out what areas looked interesting and also where there were good people. And we went to Georgia Tech, I guess, which had a very strong program in communication theory. And MIT. We really did a lot of digging. There were searches authorized. And I think our efforts turned up some good appointments—the electrical engineering department is stronger than it was before. But the big difference occurred, of course, prior to that time. In the fifties, there were a lot of new appointments. So it was on its way, and this was kind of an incremental thing.

COHEN: But you were looking at computer science at this time. Had you identified that? Or was that part of your…?

GOULD: No, I think everybody knew it. Everybody in the division recognized that computer science was a promising area and that we needed to do more. I don't think I ever got any really substantial arguments on that point. But there were a couple of problems. Computer science, as a discipline, is a little different from the sciences. The clearest way that the problem manifests itself, I think, is when you try to judge an applicant for a position. In most fields, you look at an applicant's publications and you try to assess the content of their publications, and what kind of reputation they have, and so forth—and what kind of impact their ideas have had, if you're looking at somebody who's senior. Well, in computer science it always turned out that when we had a new appointment there was no paper trail that said, "This guy is really good." Ivan and Carver would swear up and down that this person was the best that could be had, and that he was very good. But it was hard to make a persuasive case in the usual way. The problem in computer science was that the best work was being done in the industrial laboratories, and the work wasn't published, or even publishable. There were a few academic departments doing very good computer science—Carnegie-Mellon was certainly one—but it was hard to find who the new young people were.

And it was complicated by the fact that both Carver Mead and Ivan Sutherland were rather strong actors in this thing—both strong personalities—and they didn't always agree. They weren't always fighting, but they were sometimes fighting, and they didn't always agree. And I was kind of an outsider, so it was somewhat difficult. But we sure tried, and there were a few appointments. Chuck Seitz had come as a visitor [in 1977—Ed.], and Ivan said, "Let's make him a faculty member." Well, as you probably know, it's very hard for somebody who comes in a research position to make the transition to an academic position. There's a certain amount of suspicion, I suppose, that that person is maybe not as good as somebody who is already a faculty member. But I think it's certainly not true in Seitz's case—and it's not true, probably, for a lot of computer science people. And so finally that case was made, and now, regrettably, Chuck has chosen to leave. He seems to be a person who moves somewhere else every ten years or so, and he's gone off to some new and I'm sure very exciting endeavor.

So some progress was made. I spent a lot of time on computer science and electrical engineering. The other parts of the division—I don't want to minimize them, because there were appointments made. There were a couple of appointments made in earthquake engineering—Jim Beck and John Hall—and some in applied mathematics. Appointments were going on throughout the division. But what led me to say yes to doing this job was that I thought maybe with my perspective I could do something in the areas where I had some knowledge.

COHEN: So did you enjoy those years—though they sound as if they were a bit stressful?

GOULD: Yes, I think so. They were certainly interesting. But I tried to keep my research going at the same time. Fortunately, I had a few students—three or four—when I took on the division chairmanship. Most of them finished up during the time I was there, and I didn't take on any new students. I had two postdocs who were really carrying on the bulk of the work. But by the time I left the chairmanship, things had dwindled to the point where I had only one student left.

And both of the postdocs decided to leave. Stewart Zweben decided to go to Princeton, and then a few years later Paulett Liewer decided to move to JPL. I must say, it was a bit of a low period for me, coming out of that position.

I decided basically that I didn't want to stay in the chairmanship for another five years. I figured that if things had dwindled that far in five years, there'd be nothing left in ten. [Laughter] And I would have been in my early sixties. I remember running into John Baldeschwieler one day, shortly after I had finished my term as division chairman. He had been division chairman of Chemistry and Chemical Engineering. And I said, "You know, five years is just enough time for your research to really go downhill. Fortunately there's a little bit left." He said, "That's the good news. The bad news is it's going to take you five years to build it back up again." [Laughter]

Actually, it was kind of tough getting going again, because I had also realized that the tokamak era and the fusion physics era was over for me. The successes in the big machines around the country and around the world were really very good. There was no way that a small, one-person operation could compete in that arena, and I did not want to become an appendage to the Princeton Plasma Physics Laboratory or the General Atomic Laboratory—that is, to be a user who goes down and does something on the side. My style is really hands-on, small experiments. I'm not big-science oriented or big-project oriented. So it took me a little while to dig my way out of that hole.

There were two other things I want to get back to on the division chairman business. I talked about the computer science appointments, but I want to say one more thing: during that period when I was division chairman, Carver Mead had a colossal success. Carver started out doing semiconductor devices, and I think he was right, square in the middle of semiconductor-device physics at the time the integrated circuit was invented; he sort of grew up with the integrated circuit. And it was clear that integrated circuits were going to get more and more complicated, and there were going to be literally millions of transistors on a single chip. The original integrated circuit probably had five or ten. Carver, I guess, perceived the problems there very quickly and readily, and he came up with a design philosophy for very large-scale integrated circuits—probably hundreds of thousands of transistors then, but now there are some millions of transistors. He came up with design tools—computer-aided design tools, rules, and so forth. He wrote a classic book, which came out of notes from a course he gave here.

[*Introduction to VLSI Systems* (1980)—Ed.] And it made a really big impact. First, it made an impact on other schools, and then—I think somewhat reluctantly—the industrial people began to realize that there was something to these things.

And along with that, he introduced the idea that the students could design their own chips—they could design these very large-scale integrated circuits. And then he arranged for a few industrial places to manufacture chips to the design of the students. So in this course he was offering, the students would design a chip, and before the year was over it would be fabricated for them, and they'd get to test it.

COHEN: Could they turn this into anything commercial?

GOULD: Well, I'm sure a lot of Carver's students have gone out and become great successes. The students who went through Caltech's computer science at that period of time, I'm sure, were in great demand. I don't know in detail where they are now, but I'm sure that a lot of them are heads of their own companies and in very prominent positions around the country.

Carver is another one of these people whose ideas change periodically—and as he began to change his interests to other things, Chuck Seitz was in the wings. He was doing parallel systems, the precursor of the parallel processors we have now. So Seitz stepped in and did the design course for a number of years and remained quite involved in parallel processing. So the computer science department actually did get going. But still, I don't think they had more than about five or six faculty at any given time. There was a certain amount of coming and going, too. And they relied an awful lot on industrial visitors—people that Carver or Ivan could persuade to come.

COHEN: Did they use the Fairchilds, or they had other…?

GOULD: They had Fairchilds and they were also generously funded. Carver and Ivan both had very good reputations with the Defense Department. So I don't think funding was the problem.

COHEN: Those were generous years anyway.

GOULD: Yes, they certainly were. This was in the early eighties, and computer science was still growing and hot. It is now, also, but the problems have changed.

I might say that one of the specific areas where it was hard to judge how good somebody was, in terms of new appointments, was one in which Ivan was personally interested—the area of computer graphics. Ivan Sutherland has done a number of things, and he was responsible for the founding of Evans & Sutherland, a company that was preeminent in computer graphics.

COHEN: Sutherland's not here anymore, is he?

GOULD: No. He left Caltech—I'm not quite sure when. [1982—Ed.] He is also the kind of mobile person who moves on. And also, I think, quite frankly, he wasn't terribly happy about the rate of progress around here—the rate at which he could get new appointments and other things done. Caltech is a little stodgy in that respect. We don't do things at the drop of a hat. I think Ivan was disappointed that he couldn't go in with a proposal and have it accepted on the spot. Maybe it isn't quite that simple, but that does happen, I think, in these fast-moving computer organizations; they have to make quick decisions and gambles, and move. But they're also not lifetime careers, either.

Also, there's one other aspect of the division: The division had these various pieces, and one of the major pieces is GALCIT—the Graduate Aeronautical Laboratories of the California Institute of Technology. That entity predated the Engineering Division; it has the glorious history of the von Kármán era and the early days of aeronautics. But by this time, of course, I think I would characterize it more as an institute of fluid mechanics than an aeronautics laboratory, although the wind tunnels continued—at least, they were still active but they were doing mostly outside work. There was a manager, and the wind tunnels did testing for people, essentially on a contract basis. But with a few exceptions, our faculty was not very much involved. There was a little bit of friction, I think, between GALCIT and the rest of the division, because GALCIT had this longer history, and a very glorious history. And they also had a degree of budgetary autonomy.

COHEN: But they're not strictly in the Engineering Division?

GOULD: Well, they are. But they have a director, you see. I can remember in the early eighties, when Jack Roberts was provost; he felt very strongly that Caltech is not a place where we want to have directors of this or that; it just creates another layer, I guess, and a slightly different status… [Tape ends]

Begin Tape 5, Side 2

GOULD: I did find that the autonomy—particularly in the budgetary area—in aeronautics made aeronautics more equal than the other departments. And I tried slowly to make some changes in the budget. That's another aspect of a division chairman's job. There is an institute budget, but it generally covers teaching and some secretarial support. And if you look at it from the faculty members' point of view, the bulk of the money we need to do our work, we go out and get ourselves. So the institute budget is a very small part of what we want to do.

COHEN: But there are little bits and pieces in there that can get things going.

GOULD: There are bits and pieces. And there are some common facilities. And there are technician pools, machinist pools, things like that, that people can call on—services that are nice to have. But in my view, these items were inequitably distributed. My opinion was that the Engineering and Applied Science Division was kind of on the short end of things for a long time—maybe still is, for all I know. Fortunately, it doesn't make that much difference, but these small amounts of money, as you say, can often be very important.

One of the things that has dwindled over the years, to my dismay—the president and provost used to have discretionary funds. There used to be a Sloan Fund, and there used to be a this fund and a that fund. You could make short, one-page proposals, and the division sometimes had gift funds of this sort. A faculty member, who wanted to start something new and didn't have grant support for it, could go to one of these funds and say, "I want to build this new thing that's going to take me off in a new direction. I need something to start with." Seed money. My impression is that now we don't have much of that. We probably have almost none. I dipped into that well on two occasions as a faculty member, and in both cases it allowed me to go off in a new direction. The money was not a very great amount, but it was money for something I wasn't then doing, and I would probably have had a hard time getting it from funding agencies—at least on the timescale that I wanted to move.

So budgeting is another job of the division chairman, along with managing the promotions and being attuned to how things are going. And a lot of this, I think, has to be done by one-on-one conversations with a very large number of people. You've got to be sure that you don't listen to just a small trusted set of advisors. And quite frankly, I think we've had a few division chairmen who didn't do that. I think Bob Cannon perhaps didn't do that as much as he should have. Among other things, it helps you to understand the place. And of course, our more recent division chairmen do.

The way Caltech runs and the administration of it—or lack of administration of it—is really quite different, in my perception, from what it is particularly at the state universities but also at other private universities. There's very little authority here. Most of the initiatives and decisions rest with the faculty. In order to get something done, you have to convince your colleagues—that you need more space, say, or that you need another faculty member. I tried to exert a little leadership by going out and looking for electrical engineering faculty—Bill and I did—and I think with some modest success. But leadership is not something that a division chairman exerts—except to listen to what his colleagues say are the right things to do. You have to be able to assess what you hear. And therefore, you have to understand your colleagues to some degree. And then you have to understand what's needed well enough to sell the provost. So it's really quite a time-consuming job, especially for Engineering and Applied Science, where the fields are so diverse.

COHEN: But you had your five years. And can you say you enjoyed those five years?

GOULD: "Enjoy" is not really the proper word: It was interesting, and I'm very glad I did it. I'm personally very devoted to Caltech. I was paying my dues. I didn't look at it quite that way at the time—though I probably did subconsciously. But I had a feeling that somebody had to do these things and that I owed so much to Caltech for what it had given me—it's been such a great place to work. But after five years, I decided, Well, I've done it. Five years was enough. Some people can do it for longer—there are a few who have. Barclay Kamb was geology chairman for ten years. [From 1972 to 1983—Ed.] I really don't know whether it's any easier in the geology division than it is in Engineering and Applied Science. But there have been a few who've done it for longer.

The original idea was proposed by the Aims and Goals Committee, and Harold Brown implemented it. A division chairmanship was to be five years, with a review at the end of that five years, and then offered by mutual agreement—you could continue or not. I can remember Murph one day calling me on the phone and saying, "Hey, Roy, you know, your time is coming to an end. What do you want to do? I'll appoint a committee to review your progress, or I'll appoint a committee to find a new division chairman. Tell me which you want me to do." [Laughter] And I thought about it for a few days, or maybe a week, and I said, "Murph, I think you ought to appoint a committee to find a new division chairman."

The division chairmen met monthly as a council; they met with the president and the provost. Things have changed a little bit since then: there are apparently two different groupings. But I remember even at the time there were two different groupings. One was the division chairmen and the president and the provost. They sat as a committee to pass on all promotions, new appointments, and so forth; and I think their role was to keep each other honest. You had to sell the recommendations of your division to the other division chairmen and to the president and the provost. But mainly the provost was the point man on that. The president, of course, was present at all the meetings, and spoke up from time to time. But it was really between the particular division chairman and the provost, with the other division chairmen listening to see whether you could make your case or not, and asking pointed questions. There were a few who conscientiously asked pointed questions—and I think it was a process that worked pretty well.

Then there was a larger group, which included the various vice presidents and deans. And they would discuss all kinds of institute matters—things that were essentially action items, or about to become action items, and some information items. And I think we were a sounding board for the president and the provost—and probably the vice presidents—who eventually made the decisions.

COHEN: Were there any trustees involved in these discussions?

GOULD: No. The only interaction that the division chairmen had with the trustees was at the annual meeting of the Board of Trustees in Palm Springs. I can remember on at least one occasion making a presentation to the Board of Trustees.

We also had a visiting committee, and a few trustees were members of the visiting committee. I think Si Ramo was a member when I was division chairman.

But I want to come back to the provost, and a special problem that we got into, which I really want to express myself on. We had a problem near the end of Murph's term as president.

When I was division chairman, Robbie Vogt was division chairman for Physics, Mathematics, and Astronomy. And you know Robbie. In these meetings, his style was very intense; he's sometimes an argumentative person. But I must say if you cut through the rhetoric, my impression is that Robbie made very good comments on things that were going on in these meetings. My impression is that Robbie was really on the mark most of the time. I've heard a lot of things—that people were unhappy with Robbie and his style and the way he worked and so forth—but I saw a different side of him.

The problem I wanted to mention was this: Murph is a very easygoing, likable person, but he did get into a bit of trouble, in my opinion, at the end, when he made promises of a lot of appointments in the Humanities and Social Sciences Division. I don't know how widely this was known, but it certainly came to the attention of some people, and it came to Robbie's attention when he was provost. I don't know the numbers, but there were a large number of promises made, probably one-on-one. So the expectations were very high for a lot of new appointments in Humanities and Social Sciences. And when Robbie came in as provost, he brought that whole process to a halt. He declared a moratorium on new appointments, which is almost unheard of around here. But I think he had to do it. And there was a six-month study of what the needs of the various divisions were, and I think that was Robbie's way of solving this problem—first of all, call a halt to it, and then study the problem and see where we really need to make new appointments. And that was done by the division chairmen. Each division chairman had to present his plans, where his division was going, and what the priorities were. Robbie asked for this study and ran it.

The net result was that all these hunting licenses—informal as they were—for new appointments in Humanities were withdrawn. Robbie ended up with a plan of five new appointments for the next year, and he parceled out three or four of them. And the Engineering Division was fortunate to be awarded two of them. And one or two of them were kept kind of in his hip pocket for special situations that might arise.

Now, I’m not saying this because the Engineering Division came out a little better than the others, but I think Robbie did the institute a great service in reining that problem in—at least as I saw it and as I heard it.

There must have been a not very friendly parting of the ways between Murph and Robbie. I don’t know the details, but I know Robbie is very bitter about things. When I see him walking across the campus he’s still scowling. But I do feel very strongly that Robbie’s efforts on that particular problem—and on a lot of lesser problems—showed that he really had the best interests of the institute at heart. The trouble is that his style sometimes got in the way of what he thought ought to happen. But on balance, I have to be very grateful to Robbie for having straightened out what could have been a very difficult problem. As a member of the administrative council and a division chairman, he could be vociferous and argumentative. But when he went into the provost job, I think his approach seemed to change—at least as I saw it.

I wanted to put that on the record because I really feel strongly that Robbie deserves thanks, which he probably hasn’t gotten.

Now, back to other matters. As I told you, I didn’t do very well after getting out of the division chair. Things had faded away, and I had lost interest in the things I was doing, for the reasons I described. I was really at sixes and sevens, I guess. I really didn’t know quite what to do. And I said, “Well, it’s time to take another sabbatical.” [Laughter] Which I probably should have done immediately after I finished. Bunny and I did take an around-the-world trip immediately after I finished with the chairmanship. We had some free miles on TWA, and we used them up and went around the world. But we took only six weeks. We went westbound, and we spent the first five and a half weeks in Australia, Hong Kong, Singapore, and India. After Bombay, we were supposed to go on to Europe and see some friends, but we said, “No, we’re going home.” So we flew all the way back to Los Angeles from Bombay.

Several years passed before I realized that I should take another sabbatical. And then I began to think about where I might go. The European laboratories that I knew were all fusion laboratories, and that was not the direction I was going to go in. There was this group down at UC San Diego in the Physics Department—John Malmberg and Tom O’Neil—with whom I had worked at General Atomic in the sixties and then off and on over the years. So I decided, well, I like those two people very much, and we get along very well, and we’ve had some very productive collaborations; so I'll go down and spend six months with them.

Malmberg was working on something totally different from what I had been working on. He'd started up a new line of investigation. Malmberg was the experimentalist; Tom O'Neil was a theorist. And John had built up this small basic plasma program involving pure electron plasmas—no ions. Other places, like the Bureau of Standards, were doing ions—confining ions in traps and studying the properties of these ions. At Boulder, they wanted to do this for making time standards. At San Diego, John wanted to do basic plasma physics. And, of course, probably one of the reasons I enjoyed working with him so much was that we had very similar interests. We were interested in basic questions. He was always good for an argument, never believed what you said the first time, and had a persistent way of zeroing in on issues. I really enjoyed him. So I decided to go down there and just hang around and see what was going on.

He had several things going on. And he had one thing he wanted me to work on, because of my knowledge of traveling-wave tubes and microwaves. And I took a look at that for the first three or four weeks.

COHEN: Did you actually move to La Jolla, or did you commute?

GOULD: Well, that's a good question, because it was our intention to move to La Jolla. I went to their housing office and they said, "Well, we can find an apartment for you somewhere." We had a big dog [laughter], and an apartment didn't sound like a very good solution. But we had a house in San Clemente, so somewhat reluctantly I decided we would live in San Clemente and I would commute. It's about a forty-five-minute commute—it's gotten worse since then. But it's beautiful driving down the coast in the morning, particularly through Camp Pendleton.

COHEN: You could take the train.

GOULD: Well, I did on some occasions. But with the hours I kept, it really wasn't easy. And also, you could only go to Del Mar, and from there you had to take a taxi or something like that in order to get to the university.

So we lived in San Clemente, and it worked out just fine. I shifted my hours so as to avoid the commuting traffic—went in late, came back late. And I went in every day. I think I probably came up here a few times, too, but not much more than I would have if I had been across the country somewhere. I don't think I had any students at that time, so I was really very free. I think this was '87, after I was out of the chairmanship.

But it turned out that I wasn't interested in the one project John wanted me to work on because he thought I could help him. [Laughter] It was kind of a complicated problem, and it was something I didn't really identify with. But on the other hand, he had another, larger activity, in non-neutral plasmas—pure electron plasmas—which he had just done three or four really fine experiments on. The apparatus was all going. So I started kibitzing on those things.

Marshall Rosenbluth was there at the time, and we had a number of conversations. Marshall was kibitzing, too. So it turned out to be a really fine experience, and I got thoroughly enmeshed in the things that were going on there. Actually, I reread the proposal I had made for my leave of absence, and I noticed that Paul Jennings, who was then division chairman, had written that this leave would allow me to learn new techniques and bring them back and benefit Caltech's program. Well, I wasn't really quite sure that that was going to happen when I made the proposal, but in fact that's exactly what happened.

So I came back to Caltech, and the first thing I did—I'd gotten involved in some numerical computations while I was down there, to explain one of the experiments that they didn't have an explanation for. And that worked out pretty well, and I continued that here, and I began to get into computational plasma physics. Then John called me one day and said, "You know, the navy, which supports a lot of my work, wants to expand this program. And I'll let you in on a little secret. I'm going to recommend you to the grant monitor." Actually what he said was, "I would rather have you as a competitor than somebody else." [Laughter] Which was a nice compliment.

So I wrote a proposal, and a couple of students have come and done work on it. I'm now working in that area. I now go fairly regularly to San Diego. Unfortunately, John died a year and a half or so ago, unexpectedly. He had cancer; it showed up in his spine. He went quickly. But John had an entourage around him. And one experimentalist—Fred Driscoll, who was very good and who had been a UCSD student—kept on. John had been trying for several years to get Fred Driscoll a faculty appointment. And, again, as I've mentioned, it's very hard to make the transition from postdoc to faculty. The question there was: If this guy is so good, why is he still here? Why is he just a senior postdoc? And the answer—in my opinion—was that if he was interested in that particular problem, then that was absolutely the best place for him to be. Of course, John protected him from some of the things that he would have had to deal with. But he has now stepped into John's shoes, and he's doing a fine job of running that program, and now I go down and consult with him.

COHEN: So you're very involved in this? And you have students working on this?

GOULD: I had a student who finished his PhD last year. He stayed on as a postdoc for a while, until he found a job. The job situation is not so good right now, so I told him he could stay as long as he wanted, within reason. He was lucky; he found a job in three or four months. But he helped me get something else started. So now I have another PhD student, and this experiment is still running.

COHEN: And you've taken the retirement bit also?

GOULD: Yes, as of January 1 [1996]. I assume that for a while that experiment will stay running, and the ONR grant continues. I had a telephone conversation this morning with my grant monitor, Charles Roberson. I don't know if I mentioned this, but he is really one of the finest grant monitors I've ever had, because he's a PhD in plasma physics from the University of Texas, and he's very good; he manages to do work at the Naval Research Laboratory on a half-time basis.

There's one other interesting little tidbit that I ought to mention. It's also a reflection on Caltech and the way things work, or don't work, around here. I did have support, not only from the ONR in the early days, but also from the air force and the Army Research Office, so I got to know a lot of those people. And there's a program called the Joint Services and Electronics Program. It's a block-grant type of program. Most of the big universities—Stanford, Berkeley, MIT—have these block grants. It's for electronics, and it has the advantage that the money comes in big chunks and there's a lot of autonomy about how you spend it. Caltech had never had one of those, and prior to my becoming division chairman we had three or four people—Jim Mayer, Tom McGill, Marc Nicolet, Jim McCaldin—who had money from various Defense Department agencies. And the Defense Department agencies said, "Why don't you put in for a

Joint Services and Electronics Program? It will make funding a lot easier for you, and you'll get this money. And you can have some freedom, and it's easier to start new things."

COHEN: Were there security checks on this?

GOULD: Oh, no. There's a hierarchy in the Defense Department of research money. And the 6.1 money is for basic research. But it's basic research in the areas of interest to the Defense Department. And that's why electronics and computer science would certainly be in there, and fluid mechanics. But then there's money for hardware development; very few universities get that money. And, of course, there's acquisition money for buying things.

Interestingly enough, I ended up being selected to be the principal investigator, because, as you know, at Caltech people go their own separate ways, and disagreements can arise. And I was identified as kind of the lowest common denominator. [Laughter] I could talk to all of them, because I wasn't in their fields. I don't think any of them were willing to have the decision making in the hands of any one of them, but they were willing to give it to me. So we did propose that, and got it. It wasn't a lot of money—it was probably around $300,000 or $400,000 a year—but this was nearly twenty years ago. So that was a substantial amount of money, with some flexibility. But unfortunately, it sort of fell apart after about five years.

And we had another experience like that, while I was division chairman. They were constantly trying to identify areas where they could get support from industry. And Tanya Mink in the Development Office and I together cooked up what finally came to be termed a Program in Advanced Technologies, which we thought we could sell to a lot of the industrial companies. Aerospace was one; General Motors Research Laboratories was another. And in fact, we succeeded—again, in a block-grant form. Originally the idea was that the division chairman would administer this, as kind of seed money for various projects. And I thought that that was one more thing to add to the division chairman's job, but fortunately Bill Bridges was willing to take that on. And it lasted, again, for about five years, and I think in this case it died not because it didn't work for the faculty members but because the industrial people felt they weren't getting very much out of it. Because, you know, we all sort of go our own separate ways, and when it comes to getting money, we're willing to change our arguments—try to make the arguments in terms of how what we're doing will benefit the company. But when you finally come right down to it, we want to do what we want to do. And after a certain period of time, these industrial companies felt they really weren't getting their money's worth.

Fortunately, the navy has been much more enlightened about that, I think. The ONR, of all the research funding agencies, has had, over the years, one of the most enlightened approaches to the funding of basic research. They supported radio astronomy in the early years, and that, I think, was originally based on its navigational potential, which probably was not a great potential, but it was sufficient.

ROY W. GOULD

SESSION 6

April 10, 1996

Begin Tape 6, Side 1

COHEN: I was hoping you would have something to say about some of our presidents, or some of our appointments here.

GOULD: Well, I think I did talk about some of the presidents. I didn't say much about Lee DuBridge. Lee led us through this golden era of expansion, which I would like to talk a little bit about when we're finished with this. I think it's difficult to judge the impact of a president, because first of all I think the traditions of Caltech are well established.

Tom Everhart, our current president, I have known from the early years, when we were both at Hughes as students. I think Tom is the first engineer we've had as a president; they've all been physicists before.

COHEN: Do you think that there was a deliberate decision to appoint a non-physicist—say, an engineer—at that time? Do you think that was deliberate, or did it just happen?

GOULD: No, I don't honestly think it was deliberate. And I say this based only on what I know about searches. I think the problem always is, when you're searching for a new president, new provost, or new division chairman—the problem is to find somebody you think will do the job well. And I think you have to look wherever you can to find such a person. I have no reason to think it was deliberate.

And the final choice must come down to just the two or three or four viable candidates who the search committee and the trustees feel best fit the needs. Normally, there's a faculty search committee and a trustee committee. The faculty committee makes recommendations to the trustee committee, and there has to be a lot of back-and-forth between them. But finally you come down to a short list. There used to be a saying that if you drew up a list of specifications of what you wanted for any given position, the odds of finding anybody who fit those specifications would be zero. And if you did find somebody, that person wouldn't want to come. [Laughter] So I think it's a matter of casting the net as widely as you can, and then sifting through all the candidates and narrowing it down to a short list. Now, I was not on the search that led to Everhart. I was on the search that led to Harold Brown's appointment. That's my understanding of the search process. I doubt if it's changed very much.

COHEN: Do you think that Everhart's being an engineer has helped the Engineering Division?

GOULD: I don't think it's made any difference at all. You know, from a distance, Caltech is really an institute of science, not an institute of technology—and I think Caltech's reputation is based mainly on its science. I think the Engineering Division is very good and places well; it's consistent with the other departments and the other divisions. But I think that anybody who comes here as a president has to acknowledge where the center of gravity of this place is: it's in the sciences and not in engineering. And I see no evidence that there's been any special treatment—in either direction.

COHEN: Maybe not special treatment, but perhaps disregard at other times that maybe should have been remedied.

GOULD: No. I have the feeling that engineering has always had to struggle a little bit—but reasonably successfully—to hold its own in the scheme of things here. I think it's done well, but certainly not exceptionally. I don't see that there has been much shift, actually, one way or the other.

COHEN: So, when Tom Everhart came, what was your position then?

GOULD: I was no longer division chairman. I left the division chairmanship at the end of 1984, when Murph was still president. And Barclay Kamb was provost at the time. He was the transition provost, between Murph's presidency and Tom Everhart's. I was already back as a regular faculty member.

COHEN: So you would have seen the impact as any other professor?

GOULD: Yes. We all care very deeply, I think, that Caltech remains the kind of place that it is. A good president realizes that. But Caltech is rather unusual in the very small, lean administration it has. There's just a president and a provost, and six division chairmen. And there's such strong direction from the faculty about research interests; at big universities, there's not as much initiative from the faculty. The administration can create new departments and chart new directions, but here it comes from the faculty. And I think it's the job of the president to maintain good relationships with the trustees and with the faculty—to be the bridge between the trustees and the faculty. We did have a very successful fund-raising campaign under Everhart.

COHEN: Of course, seed work is done for that over many, many years. It doesn't really happen with one person.

GOULD: Yes, that's true. It's hard to tell—except with the passage of time—what the impact of a president is, unless something goes wrong. And so far I haven't seen anything like that.

COHEN: Well, then, the one thing that comes to mind is the neural network push here. That had electrical engineering involved also—Carver Mead, of course.

GOULD: Yes, Carver Mead. And John Hopfield was a very strong part of that.

COHEN: Well, that's why he came, didn't he?

GOULD: Well, I didn't know all that much about it. John Hopfield worked in solid-state physics at Princeton, so when he first came to Caltech it wasn't clear to many of us exactly what he was going to do—although it was unusual that he came not into the physics department but into chemistry and biology. But my recollection is that it took a few years for that new program to evolve. John may have been brought here by Murph and other people with the intention of starting in this direction. But the way programs evolve is that existing faculty get their heads together and think that there's an opportunity to move in a certain direction. Some of them are already moving in that direction, and they want to formalize it. And they want to make a program—a PhD program, or something that has some visibility outside—so that they can recruit students. And I think that's the way this neural network program started. And it was Hopfield and Mead who were the two key players, who were already here.

Then I remember, about the time I left the division chairmanship, there was a special committee appointed to find a new faculty member who would allow this program to grow. And that also, I think, is fairly common when you're trying to start a new program. There's a nucleus of people who are already here and who want to go in this direction, and they say, "OK, we need somebody who's right in the middle of this—somebody to round out our existing activities." And they go out and in most cases successfully propose new faculty appointments in this area. That's what happened in applied physics; Tom McGill was the new appointment that was made. In some respects, probably the same thing happened in applied mathematics. In this case, this was a joint search committee of Engineering and Applied Science and Biology, which is a little unusual. And I happened, as an ex-division chairman, to be appointed to that search committee. The end result of that search was to recommend Christof Koch as a new faculty appointment in this area. And then other people came. I think Demetri Psaltis, who's now executive officer for computation and neural systems. It sort of drew in more people, and now other universities are imitating it. It's not all centered in one division. But it was recognized that from time to time the people whose interests coincided might change, and there might be a different combination. And maybe that will happen, because Carver's moved out. I understand John Hopfield is leaving. So there'll be space here. Maybe some new group will come together.

But that's the way new groups tend to nucleate around here. They form out of the old groups and then make a couple of key appointments. It puts the faculty clearly in control of generating these things. It's a way to evolve—to not stay stagnant.

I'd just like to say something about some of the changes that I have observed. I've been here I guess now more than fifty years, including my years as an undergraduate. I've felt the necessity to get away from time to time. And I must say that I found, over and over again, that Caltech is a great place to come back to. It was small when I arrived—smaller than it is now. There's been a lot of growth in this period of time. And I think most of that was in the period immediately after the Second World War—let's say the fifties and sixties. There was a great growth in funding. As you know, there was almost no government funding of science before

World War II, but in the postwar era there was a realization that science and engineering had a lot to offer to the country, so we entered into a period of great expansion and growth. It's been a very interesting time to live through. I don't know what the faculty size was at the end of the war, but it must have been a good deal smaller than it is now. There were no buildings north of San Pasqual. I've described some of the evolutionary changes in the divisions and in the programs, but it's an ongoing process.

Caltech was a unique place in the thirties, but in the fifties and sixties we acquired a lot of competition. Science and engineering programs at other institutions grew. Almost all the states wanted to have their own research universities, and now do. So I think it's a much more competitive environment now than it was in those days.

It's also more competitive for the young faculty who come in now. The expectations are enormously different. Earlier I was never really aware of faculty committees for promotion. I can remember one appointment I was involved in in the sixties, when I was spearheading the attempt to get Professor Yariv here. Much later, when I was division chairman, I looked through the files on that appointment, and it was remarkable how different the appointment process was in those days from what it is today. You could get a few phone calls; maybe you got a letter or two. But in those days the division chairman had the power and the responsibility to sift through the suggestions of the faculty and act. It was a very informal process, compared to what it is today.

COHEN: Of course, you're forced into some of this by the legalities. I mean, you take federal money, and therefore you have to do federal…

GOULD: That's right. I think a lot of this change has been forced by the federal government. And affirmative action requirements have done a lot of that—and legal changes that allow people to see their files and to look into matters of that kind. So, yes, I think a lot of it is just a consequence of the change of times and the laws. The good part is the growth in support of science, and the bad part is probably that more control has come from Washington.

I guess this has led to more bureaucracy at Caltech; I've seen a growth of the bureaucracy, and I think it's dictated in large part by the federal government. There are more vice presidents, for example. [Laughter] I guess the notion of a vice president didn't exist back in the earlier days, when I arrived. The provost was the provost. There might have been a vice president. But that was it. And there was a dean of the faculty. So there's been a growth there.

One thing, I guess, that has not grown very much is the administrative control on research funds. That seems not to have grown very much. That is to say, grants and contracts come in, and aside from rules about travel and about how you may or may not expend funds—which are all very reasonable and rational—I don't see much growth in bureaucracy there. But there has been a great increase in the bureaucracy for personnel, business services, and student administration.

COHEN: Although there is approval along the way on people putting in grants.

GOULD: Oh, yes. The division chairman has to approve all proposals that go out of here before they go to the sponsored research office. And the main purpose of that, I think, is to see that the proposed research is appropriate to what the division is doing. And my impression is that most faculty know very well what's appropriate and what's not. I don't know of many cases where there have been objections by division chairmen. Only rarely do things get proposed that are not acceptable. There are a number of questions on the division approval forms that tend to keep things in line. We don't want research proposals that don't involve students in some way or another. And we don't want a faculty member to get completely disconnected from the educational role of the institute. But by and large, I don't think there are any real restraints, unless you do something that's out in left field. I'm not sure that any proposal that came across my desk as division chairman ever got sent back to anybody.

COHEN: So it's a group of people here who really take care of themselves and police themselves.

GOULD: Yes. Everybody understands what the institute's goals are. In fact, the goals are largely set by the faculty, and I don't think the goals have changed very much over the years.

The money that's come in has changed a lot; the size of the faculty has changed. The undergraduate student body has not changed much. It was 180 for each class, new admits, in the fifties—that was the target. It was allowed to grow somewhere up to about 225—limited more or less by the considerations that the educational philosophy not change in any significant way, and also by the physical plant requirements. You couldn't double the class, for example. I think that now it probably runs 225, 230. So that leads to an undergraduate student body of something under 800. Now, the thing that has grown is the graduate student body. It's grown rather significantly. And part of that, I think, is a consequence of research funding and the style in which research is done throughout the country. There are many more graduate students now. All of these research appointments are supported out of research grants, most of which come from the federal government—a small part comes from the state and private funds. And it's the growth of that funding, I think, that has led to a larger number of postdoctoral fellows. There has been a strong pressure from the administration—and rightly so—to keep that research population on a turnover basis, so that you don't have many people who aren't on the academic ladder who have been here for long periods of time. There have been isolated exceptions—some of whom were spectacularly successful in their fields—but really very few.

COHEN: Of course the institute has benefited, too, because all these people are paid out of grants, and grants pay overhead.

GOULD: Yes, that's right. I don't know what the number is exactly, but roughly fifty percent of our annual budget comes from sponsored research of one kind or another, and it's also probably one of the factors that make the faculty so autonomous. You know, you get an office and lab space, for which you pay overhead if you get grants. But then you're pretty much left to your own devices, to chart your own course. And if you do well, you're promoted and get salary increases and so forth.

I want to mention a couple of other changes. The students are not coming in with as much hands-on experimental feel as they had in the forties and fifties. And one way that manifests itself is that the physics program has had to put in another track of physics, which gives people a chance to get hands-on experience in electronics and that sort of thing. Now they have hands-on experience with computers instead, and of course that's good, in its own way. But they don't have so much hands-on experimental experience.

The other thing is that there's not as much interest in science and technology as there was in that golden period. The high school students are really not as gung-ho about science and technology. They're not as well prepared—perhaps a failing of our high school preparation. But

I also think there is an anti-science, anti-technology feeling in the country, which seems to be a factor in why the students who are coming are not quite as motivated toward science and technology. They have broader horizons. You know, Caltech is a very specialized place. And if high school students have broad interests and come to Caltech and find out that those broad interests are not really met by our curriculum, it's a tough place for them. And there's no graceful way out for them. Normally, their grades go downhill and they lose interest. So it is tough for those people.

Finally, I'll just say that my experience has been a very rewarding one at Caltech. I've really enjoyed the experience. I've ridden this wave of postwar expansion in science, and I found Caltech just a wonderful place to work, because of the freedom it allows. You set your own course. Where else could you have a job and do what you would want to do anyway?

COHEN: And you've made your contribution.

GOULD: Well, I hope so. That's what makes the place—all the individuals and the contributions they make. You can't bat a hundred percent on your appointments, but you have to maintain a high batting average in order for Caltech to maintain its position. And it is really up to the faculty and their accomplishments to do that.

ROY W. GOULD

Epilogue

June 4, 1998

Shortly after the completion of the taping of this oral history, I got back into amateur radio, the hobby that got me started in engineering and science as a boy. I learned from Professor Bridges that there would soon be an opportunity to reacquire old call letters. He checked and found that, over the years, mine (W6UKX) had never been reassigned. I had let my license expire in graduate school when I was very busy. Thinking that it would be amusing to get them back now, I studied a few weeks for the examination. I found that the Morse code was like riding a bicycle—it comes back fast. The technical questions were easy for me, so all I really had to study were the rules and regulations.

After I got the new license, I pondered whether or not to actually use it, and if so what equipment to buy, what bands to try, and what kind of antennas to put up. It was certainly not like the old days, when I built everything, the receiver included. That challenge was the most interesting part, and it was what had led me into electrical engineering. Now the technology has advanced so much that it would be virtually impossible to build a receiver and transmitter (the current transceivers include both in the same small package) with the technical performance and the many features and options (bells and whistles) that one can buy today. This enormous change in equipment was not a total surprise to me, because exactly the same change has occurred during this period in the scientific instruments we use in the research laboratory. Partly because of these changes, we find that applicants to Caltech no longer have the same kind of hands-on electronics experience that many once had. Now the experience is in using computers.

After a few months, I did decide to use the license, acquired new equipment, and put up antennas. I have had quite a bit of fun reengaging in this old hobby. Of course, we now have cell phones, the Internet, and satellite communications, but there is still an air of excitement in talking to operators in Papua, New Guinea (a missionary), Croatia, South Africa, countries of the former Soviet Union, and the Cook Islands (on the beach with antenna strung between two coconut trees). And some of these operators become friends through repeated contacts. There are also the contests, the “field days” (preparation for civil emergencies), and the Caltech Radio Club, which I belonged to as an undergraduate. In addition, I again find myself trying to understand the vicissitudes of radio propagation due to the influence of sunspots and geomagnetic storms on the ionosphere, a subject that has advanced greatly over the intervening years.

—R.W.G.