PETER GOLDREICH
(b. 1939)

INTERVIEWED BY
SHIRLEY K. COHEN

March, April and November 1998

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the LIGO Series Interviews

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

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

Subject area

Physics, geology, planetary science, astronomy, astrophysics, LIGO

Abstract

Interview in five sessions in March, April, and November 1998 with Peter Goldreich, Lee A. DuBridge Professor of Astrophysics and Planetary Physics 1981–2003 (emeritus 2003), with joint appointments in the Division of Physics, Mathematics & Astronomy and the Division of Geological & Planetary Sciences.

He begins by discussing his family background and early education at Bronx High School of Science; engineering physics at Cornell; graduate work at Cornell with Thomas Gold on solar-system dynamics (PhD 1963). Postdoc with Donald Lynden-Bell at Cambridge; work on spiral density waves in galaxies. Friendship with Wallace Sargent. Assistant professorship at UCLA. Joins Caltech faculty 1966 as associate professor, with joint appointments in physics and geology divisions, becomes full professor 1969. Resident associate in Page House 1976–1980. Suicide of assistant professor Peter Young, 1981. 1987 presidential search committee.

Discusses his work on orbital dynamics, solar rotation, magnetospheres, pulsars, astronomical masers, circumstellar disks, solar oscillations, planetary rings, shepherd satellites, interstellar turbulence, white-dwarf pulsations.

Long discussion of LIGO [Laser Interferometer Gravitational-Wave Observatory] history at Caltech, including his involvement in conflict between LIGO's original leader, Ronald W. P. Drever, and Rochus (Robbie) Vogt, LIGO director 1987–1994. His support, with Sargent and Maarten Schmidt, of Drever. Comments on current state of astronomy and physics at Caltech. Closes with recollections of receiving National Medal of Science from President Clinton in 1995.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Goldreich, Peter. Interview by Shirley K. Cohen. Pasadena, California, March, April, and November 1998. Oral History Project, California Institute of

Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Goldreich_P

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH PETER GOLDREICH

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Peter Goldreich **by Shirley K. Cohen**

Pasadena, California

Session 1	**March 25, 1998**
Session 2	**April 2, 1998**
Session 4A	**November 11, 1998**
Session 3	**April 7, 1998**
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Begin Tape 1, Side 1

COHEN: We traditionally start these interviews with your own family background. Tell me a little bit about your mom and dad and growing up and your education.

GOLDREICH: My parents were the first educated members of their families. Their parents hadn't been educated. My father was born in Budapest. He came over, I guess, when he was around ten or twelve. His father was a chicken butcher—in the United States, anyway. I don't know what he did in Budapest. The family started coming over after the First World War, which they fought on the wrong side and all did badly. [Laughter] My mother's family comes from Lithuania, but she was born in Hartford, Connecticut. She grew up in New York. My mother went to Hunter College and my father to City College of New York, both of which provided free education. They were there during the Depression.

COHEN: Did they then teach in the schools?

GOLDREICH: My father started to teach, I think, fairly soon after he graduated. He did some other things. First he went to Wall Street.

COHEN: What did he teach?

GOLDREICH: He taught biology. My mother and father both majored in biology. I think they met through my mother's brother, who played baseball with my father in college. My parents, I guess, were separated for a while during the Depression, because my mother won some competition to get a job in Washington working for the Department of Agriculture. She came in first on the national test, and my father came in third. This was the only time, I think, that my mother ever bested my father [laughter] in intellectual activity.

COHEN: Did she remind him once in a while?

GOLDREICH: No. [Laughter] Anyway, my father taught high school, and my mother, when I was a kid, taught English to foreigners part-time. And then she became a lab assistant at the Bronx High School of Science after I had graduated.

COHEN: You graduated from the Bronx High School of Science?

GOLDREICH: Yes, and my mother eventually became a biology teacher there. My parents retired fairly young—age sixty-two or so. They sold their house, and for a while they didn't have any set place to live. They sort of ran around and spent some time in Florida, and then they settled in San Diego.

COHEN: They followed you?

GOLDREICH: Because of the grandchildren, mainly, I think.

COHEN: So when you were a child, both parents were teaching?

GOLDREICH: Yes. Well, my father was teaching. My mother was teaching English to foreigners. And I was not a very promising student for two intellectual parents. I wasn't very interested in school. My father used to worry that I was only interested in sports, even though I went to Bronx High School of Science.

COHEN: So it couldn't have been too bad.

GOLDREICH: No. I had the advantage of having educated parents and a reasonably stable home.

COHEN: Right. And they knew that that was where you should go.

GOLDREICH: Well, no, I don't think so. My father thought that Bronx High School of Science was doing a disservice to the city schools by taking the best kids from the different schools and putting them all in this one place. But when I was in junior high, I took the test, and I got in. And when I was at Bronx High School of Science I was one of the better, but not best, students. I was in the top ten percent.

COHEN: That was pretty good.

GOLDREICH: Well, it really wasn't. I always thought I could do better if I just tried. I just wasn't that interested—I don't know why.

COHEN: You were still playing baseball at this point?

GOLDREICH: Yes, I was just interested in sports. I remember when I got my SAT scores. The grade guide was very surprised at how well I did and said something—that even with this aberration he thought that I wasn't smart enough to be a scientist and that I should be an engineer.

COHEN: Now, who was this person?

GOLDREICH: His name was Alkali. He was rather sour, though. He was more acidic. I knew his son. Anyway, my father always told me that I should be a scientist. He said it's much more interesting than being an engineer. But when I graduated from high school in 1956, I followed my grade guide's advice and enrolled as an engineering undergraduate at Cornell.

COHEN: Now, your parents sent you to Cornell? Or did you have a scholarship? How did that go?

GOLDREICH: Well, I was almost certain I was going to go to City College. I applied to City College, Cooper Union, Rochester, and Columbia. I remember reading somewhere that you should know *Moby Dick* if you were applying to Columbia. I read *Moby Dick*, and when I got there, that's what they asked me about. I was absolutely amazed. I applied only in New York State, because they had a very good state scholarship, which I was almost certain to win, and which I did win. If you went into science or engineering, they gave you an additional $200. I think the state scholarship at that time was paying about $550 a year, with the extra $200. And tuition was much less than that. So there was a tremendous financial incentive to stay in New York. I never considered going out of state.

COHEN: By this time, of course, both parents were working.

GOLDREICH: Well, my mother was starting to work as a lab assistant. My father had trouble coming up with the money for my tuition. And I worked when I was in college. Anyway, I went to Cornell because my best friend since kindergarten, with whom I played on the high school baseball team, decided to go there. He applied to Cornell, and he got in on a scholarship. So I applied to Cornell late, and I got in. We went up to pre-frosh weekend together.

COHEN: Who was this friend? Do you still keep track of him?

GOLDREICH: Yes, his name is Alan Lippert. He's in Seattle now. He's retired from IBM, and he's doing some teaching in high school. He has a second family. So that's how I ended up at Cornell.

COHEN: Had you seen it before you got there?

GOLDREICH: I just went up for pre-frosh weekend. That was the only time I had seen it. And we lived in University Hall. Alan and I were roommates the first year, and then he joined a fraternity and I decided I didn't want to join a fraternity. He played on a lightweight football team, and I played soccer. Many of the kids in soccer were foreigners, and they couldn't get into most of the fraternities, and I decided I didn't want to be in a fraternity. So I went into a rooming house. Although I would occasionally play on one of my former roommate's fraternity teams, as a ringer.

COHEN: Now, you used to do wrestling, didn't you Peter? Did you do wrestling at this time, or was that later?

GOLDREICH: After I graduated I took up judo, when I was in graduate school. I stayed on at Cornell. I did judo very seriously for a few years. And then when I came to California, I got tired of charging downtown to these judo clubs at night—it was hard to prepare lectures and do that after I moved to Pasadena. While I was at UCLA, I still did judo. When I moved to Caltech, I used to work out with the wrestling team because it was convenient.

COHEN: I thought that was an early interest of yours. Let's go back to your years at Cornell. You were there quite a few years.

GOLDREICH: I took a five-year engineering course. It was called engineering physics. And it turned out I liked physics and math, but not the engineering. As I went along, I got disenchanted—mechanical drawing, welding, all the more practical stuff—some of which, later, I wished I had paid more attention to. But I didn't. I liked the math and physics, and so after a couple years I decided I would like to accelerate my graduation. So I finished the five-year course in four years.

COHEN: Now, were you actually in the engineering school?

GOLDREICH: Yes, I was in the engineering school. That had another big disadvantage for me. I had to take compulsory officers' training, since I was part of the engineering school, and I had a lot of trouble with ROTC, because I was very bad at following orders. I had three student courts-martial. I spent a lot of my spare time working off demerits by walking the colonel's dogs, putting up pictures in the drill hall, doing extra marching. I hated it. Nothing made me happier than when, after two years, I gave back my uniform.

COHEN: Was the draft finished then? Did that keep you out of the army?

GOLDREICH: No. I almost was drafted. I'll come back to that. That was after I got my PhD. Anyway, I had a terrible time in ROTC, and I knew that I should stay out of the army at all costs. I was in the air force ROTC. I was probably their best student in intellectual stuff, which was pretty Mickey Mouse, but I was a bad discipline problem. So I got rewarded, on the one hand, by being taken up in dual-control airplanes—since I had such good grades—but on the other hand I was always working off demerits. The happiest experience I had in ROTC was during the presidential review at the end of my two years, when a student officer who had regularly tortured me fainted while he was standing at attention with Washington representatives parading in front of us. Since I was one of the shorter people on the line, I had the privilege of pulling him by one of his legs with the back of his head on the ground all the way back along the line so we could stack him up against an old wrestling mat.

COHEN: One of the good moments.

GOLDREICH: That was one of the good moments. I didn't particularly like undergraduate school at Cornell. There were some good students in engineering physics, but by and large I didn't think it was a serious place.

COHEN: Were there any teachers or professors who stood out?

GOLDREICH: Oh yes, many of my physics and math professors, and even some professors in other subjects. But overall the atmosphere of the school—the fraternity atmosphere—was not very intellectual. I didn't care for the social part of it very much. I thought the idea of people living with groups similar to those they had come from was a real disadvantage to the place. But it was a very pretty place. It's not as pretty now. It's gotten more crowded. The elms have died. But it's still pretty nice.

COHEN: Where did you meet your wife, Susan?

GOLDREICH: I met Susan before I went to Cornell. In 1956, after I graduated from high school, my parents went to an NSF [National Science Foundation] teachers' program at the University of Colorado. That was my first trip west of the Hudson. We drove out to Colorado. I didn't want to go, because I was playing on a sand-lot baseball team, which was one of the best in the city, and we were going to play in the Polo Grounds, also called Giants Field, because we had won one of the league championships. We used to play four or five games a weekend. But my father was absolutely determined to get me away from the influence of these kids, most of whom went on to play minor-league baseball, none of whom made it to the major leagues. So he dragged me along. And when I was in Colorado, Susan was there for a similar reason. Her father was switching from managing a New York State unemployment insurance office to teaching math at a small college in New Jersey. So he was there. This was in Boulder. I was living in a dorm at the university, with a whole bunch of guys who were on the GI Bill, taking a course in Spanish. Susan was living with her sister and her parents. And all these guys on the GI Bill were chasing schoolteachers who were there—mostly grade-school teachers who were there for seminar programs. I had to do something too...

COHEN: So you chased Susan? [Laughter]

GOLDREICH: …with somebody more my own age. That was Susan. I had a lot of interesting experiences with these GIs. I taught them math. They taught me how to shoot automatic weapons, which they had stolen. I also climbed a little in the Rockies, which was something new for me, and which I loved. And I learned to drive. I went from sixteen to seventeen that summer, so I could get a license in Colorado but not in New York. I did a lot of the driving coming back from Colorado to New York. And then I was able to drive in New York, even though you had to be eighteen, so that was an advantage too. Then I went to Cornell. After four years, I got my degree in engineering to get a license there. But by the last year I was sort of being taken care of by Tommy Gold [director of Cornell's Center for Radiophysics and Space Research].

COHEN: Now, how did that happen? If you were in engineering, how did you come to Tommy Gold?

GOLDREICH: He gave me a summer job. And then, in 1960, when I took the Graduate Record Exam, I did well on everything, but particularly on the physics. So Cornell tried to—

COHEN: Keep you there?

GOLDREICH: Yes. They gave me a big scholarship, something like two times what the NSF paid. And of course they gave me a very good summer job. So I stayed at Cornell. Also, they admitted Susan for free for her last year of undergraduate school. She was a year behind me. They somehow fixed it for Susan so she could go there. And we got married. Instead of going to graduation, I got married, just before I was twenty-one. And I lived in Tommy Gold's house. I was there for three years. I got my PhD in two and one-half years, and then I was an instructor for one semester.

COHEN: What did you do your PhD in?

GOLDREICH: Well, actually I thought I was going to do particle physics. That's what I was studying. But since I lived in Tommy's house and he couldn't calculate anything—but he had lots of ideas—he used to ask me questions, and I would look at these things. And after I had solved three problems he said, "There's enough for a PhD. Why don't you just send it in and get a PhD?" So I said, "Tommy, I am always willing to take the easy way out." And that's what I did.

COHEN: Well, it must have been quite worthwhile. Would you allow a student of your own to do this now?

GOLDREICH: Sure, if I had a student like me. I have had some pretty good students. But anyway, that was very nice. And then, I guess, after that I had two job offers. One I got from the National Academy of Science National Research Council—the NASNRC fellowship—to go to Cambridge, England, to be a postdoc with Fred Hoyle. And I was also offered a job on the first proton-proton experiment at Brookhaven [National Laboratory], which would have kept me at Cornell. I would have had to fly back and forth to Brookhaven.

COHEN: Let me backtrack just a minute to your six or seven years at Cornell. Does anybody really stand out among the teachers and professors you had?

GOLDREICH: Oh, yes. I had a lot of professors who I thought were really terrific. Paul Olum was there. I had two classes with Hans Bethe. I thought he was great. And the fact that he always worked on his preparation. Even though he had a big sense of his place, he was humble about physics. He used to say, "This is not easy for me. I have never felt very confident about this subject." [Edwin E.] Salpeter, who was not a terribly good lecturer but was terrific to talk to, was another one I liked. And there was a mathematician named Lenny Gross. I took a functional analysis course with him. I liked him a lot too. He kept German shepherds—he and his wife bred them. And the Golds had a big, white German shepherd, who used to follow me to school. One day the Golds' German shepherd was outside the class, which was slightly below ground level, and the window was open. All of a sudden, he jumped in, landed on some woman in the class, and made a beeline under the professor's desk. The professor's German shepherd was in heat. [Laughter] There then followed a tremendous commotion. [Laughter]

COHEN: Was the woman hurt?

GOLDREICH: No. [Laughter]

COHEN: Oh, well. There were so many dogs on that campus.

GOLDREICH: I took Lenny sailing a couple of times. I put together a styrofoam board with a fiberglass cloth epoxy coating, and I used to sail it on the lake. The bigger the storm, the better. One time I took him out, and it turned over. As the boat was turning over, he told me he couldn't swim.

COHEN: Did he have a life jacket?

GOLDREICH: No. So I said to him, "Maybe you'd better hang on to the boat." [Laughter]

COHEN: Anybody else, Peter, who really stands out?

GOLDREICH: Well, Martin Harwit. I taught my first course with him.

COHEN: And that would have been in astronomy?

GOLDREICH: Yes. That was when I was an instructor, so it was in 1963 in the spring, right after I got my PhD. And then some of the visitors: Dennis Sciama, Ivor Robinson—for various reasons they stood out. Actually, I think the level of instruction at Cornell was, by and large, good.

COHEN: Well, these are all serious people.

GOLDREICH: I know. I think that in general people tried to teach well. Mostly the teaching was pretty good. I got very good grades, but I can't say I was a very good student.

COHEN: How does one get good grades without being a good student?

GOLDREICH: There were courses in which I did very well, for which I didn't really read the material thoroughly. I found out what it was that was easy to test on, I guess. And I was very good at that.

COHEN: Like *Moby Dick*? [Laughter]

GOLDREICH: Like some of the electrical engineering courses. If you were good at mathematics, on the exams, you could analyze Fourier signals very quickly and things like that. But I didn't have a real feel for the electronics. I never was interested enough. Later I was sorry. And I used to do very well on things like courses on strengths of materials. But I never really cared about the materials. Now I teach that stuff, so—

COHEN: So you had to learn it.

GOLDREICH: Yes. And I realized—well, probably I learn it in my own way now. But I think I could have gotten a lot more if I had had an interest in it.

COHEN: So Cornell was really a good experience for you.

GOLDREICH: Yes. I got to meet some unusual people, and I saw what a high standard there was. In fact, it took me a long time before I realized that people like Ed Salpeter and Hans Bethe and Tommy Gold were really almost singularities in the profession, and that the people I was going to be competing with mostly were very, very different. In fact, it wasn't until I went to a meeting in 1964—the summer of '64—that I really appreciated what the average professor in astrophysics was like. It was a real eye-opener to me. I remember telling Susan that it was not a very difficult subject. Whereas the people I had known before were so impressive, the average astrophysicist was not. And I realized that the people I had encountered at Cornell and Cambridge—where I got to know Fred Hoyle and Donald Lynden-Bell—were really special.

COHEN: So there is a reason to try to go to these kinds of places.

GOLDREICH: Yes. I don't think it's absolutely necessary, because you can do it later. And in my case, I didn't get much out of being in a special high school. I really didn't pay much attention. I didn't learn the material well. If I had been more conscientious, I could have learned a lot more. When I got to college, I knew that I had to do something, for once—I couldn't just sort of sit around—and I became, immediately, a much, much better student. Also, the students at Cornell weren't, on average, as good as the ones at Bronx High School of Science. Caltech is the first place I've been where the students are comparable to those at Bronx Science. So I went from being in the top ten percent to being at the top. That was a conscious thing. I realized that this was my last chance to perform in school.

COHEN: And you must have been enjoying it already.

GOLDREICH: Well, I enjoyed the math and physics. I was very interested in it, and I did extra reading and so on.

COHEN: I think it was an advantage for you to be with someone like Tommy Gold, who didn't care much about formal things.

GOLDREICH: The funny thing about Tommy is that the whole time I lived in his house, he used to tell me that there were other people just as smart who were working much harder and I was lazying around too much and spending too much time fooling around. He used to yell at Lindy, his oldest daughter, who would hang around with Susan and me. He'd say, "Get out of there!"—that she was going to ruin my career, and so on. He was such a slacker himself that it was amazing that he always worried about me. He was worse than my father. My father, by that time, was satisfied that I was doing OK, and now I had Tommy telling me I wasn't working hard enough and that there were all these smart people and that I should work harder and not play around so much—which was very surprising, coming from somebody who treated himself so well in every aspect of life.

COHEN: Well, maybe you were his conscience. [Laughter] So you went off to England for that year?

GOLDREICH: Yes, I went off to England. I was supposed to be Fred Hoyle's postdoc. It just seemed much more exciting than Brookhaven. Shortly after I got there, Fred had a falling-out with the head of the department, George Batchelor. Fred came in one morning and cut his nameplate off his door and never reappeared. So I worked with Donald Lynden-Bell, and that was very good—for both of us, I think. And we've stayed friends ever since. He's living in Ireland now. Susan went to England with me. We had a little baby—Eric was born a month before we went. It was hard on Susan, being displaced. We lived in Dennis Sciama's house. He was away at Caltech. It was an elegant house, except it didn't have any heat. And I bicycled in, every day. We didn't have a car. But we were quite well off, because the fellowship I had was very posh. It put me in the top-ten-percent income bracket in England. I thoroughly enjoyed that. I joined the town judo team and the university judo team. I would go work out at the gym and play basketball. And the academic life was interesting, too. I was in the Department of Applied Mathematics and Theoretical Physics. I was invited to tea by Sir Harold and Lady Bertha Jeffreys. I met [Paul] Dirac. I bicycled in with Pippard [A.B. Pippard], who was head of the Cavendish Laboratory. It was quite an experience. I was there just one year.

COHEN: You don't remember meeting us in Greece in August, do you?

GOLDREICH: I went to a meeting there. That's where I met Maarten Schmidt. We were driven in a bus all over Peloponnesus. I thought it was going to go off the road the whole time.

COHEN: And I think your mother came to take care of the baby.

GOLDREICH: My parents took Eric back to New York from England. And after a few weeks, when Susan came back, Eric was calling my father "Mom." When I came back he hardly knew me.

COHEN: So after that summer you came back to the U.S. But you had a job already?

GOLDREICH: Well, that's really an earlier chapter. When I got my PhD so quickly, I wasn't protected from the draft. I was called up for induction in October of '62, just before I got my degree. That really scared me. I didn't want to waste two years in the army. I already had friends who had had this experience of doing boring things, and also I had had this terrible experience in the ROTC, when I couldn't get along. I remember the first time they called me up. The Ithaca draft board took care of me, but the exams were in Syracuse, so they were busing everybody up there. Susan and I drove up instead, which was not what you were supposed to do. I went in and had my physical and told them that I had all sorts of sports injuries. I remember that when I was in the room, they asked how many people had a high school education—many of these people came from rural New York—and only about half had. And then they asked how many of us had graduated from college. There were a few. And then the guy snickered and said, "Are there any PhDs?" I raised my hand. So when I was taking the mental part, they kept coming around looking at me. [Laughter] I had an extra physical, because I told them I was injured in sports. And then I took a business trip to California.

COHEN: So that was OK? They made you 4F or something?

GOLDREICH: No, I passed the physical. But President Kennedy had issued an executive order saying that if you had a pregnant wife you couldn't be drafted. And I had a wife, so that was my chance. We went to California, and when we came back I had a pregnant wife. We were allowed a month business trip.

COHEN: [Laughter] To work on your deferment.

GOLDREICH: So I went and interviewed all up and down the coast at aerospace companies and at JPL [Jet Propulsion Laboratory]. And I went to UCLA and I saw Gordon MacDonald, who was a friend of Tommy Gold's, and Gordon said, "How would you like to be a professor at UCLA?" It was a beautiful day, and I had just left Ithaca in January, so I said, "Yes." He said, "Good, it's done then." Actually it turned out that he didn't have the power to offer me a job, but I didn't know that, so I said, "Yes." And then when I came back to New York, I had a doctor's note saying that Susan was pregnant. I came into the Ithaca draft board, and the woman there, who of course knew what was going on, threw her arms around me. She was an old woman—probably about the age I am now—and said, "You did it, son! You did it!" [Laughter] My draft board in New York was not amused, and they called me in and bawled me out. But there was nothing they could do, because I was protected by the executive order. Later, Kennedy expanded that and said that you only had to have a wife, but it was too late. We had Eric. So that's how I avoided the draft, and I'm proud of it. [Laughter]

COHEN: Does Eric know that?

GOLDREICH: Yes. I guess I'd have trouble becoming president of the U.S. The things that my friends who were in the military did were so boring. They were so misused.

COHEN: That was before Vietnam?

GOLDREICH: It was the beginning of Vietnam—'63. People were flying as observers on helicopters. Some were going on patrols. But it was the early period. The big thing the year before had been the Cuban missile crisis.

COHEN: So then you came back from England and you thought you had a job at UCLA?

GOLDREICH: Well, Gordon came to visit at Cambridge, and it turned out that I didn't quite have one, but in the end I did get one. I had a joint appointment between astronomy and the Institute for Geophysics and Planetary Physics. This was in the days when they were hiring professors off the streets. You didn't have to be special. It's so different now.

COHEN: Was that an assistant professor position?

GOLDREICH: Yes, I was an assistant professor. It was very nice. I was twenty-four years old. That's also so different now—assistant professors are much older. Of course, I was getting paid $8,000 a year [laughter], and it cost $5 an hour for a baby-sitter in Westwood. So we weren't rich.

COHEN: You lived over on the West Side?

GOLDREICH: Yes, and I'd bicycle to work. The Institute for Geophysics and Planetary Physics didn't involve teaching, and they had a very distinguished group of people, most of them in the National Academy of Sciences. The astronomy was not so good. They always looked with envy at Caltech and Palomar—

COHEN: Now, by this time, what was your PhD in, Peter? Was it in astronomy or physics?

GOLDREICH: Well, I had a PhD in physics—I think it was in theoretical physics. But the problems I worked on were solar-system dynamics. They were things that Tommy had suggested I look at. There were basically three of them, and each one he had suggested, I had solved. And they lasted, those things. It was easy in those days, because true discoveries were sort of on the surface. You didn't have to go very deep. There were a lot of problems that were easy to pose and not that difficult to solve that hadn't been looked at, or just looked at in a very cursory fashion. It's more difficult now, although it's still, in astrophysics, easier than most things, I think. It's sort of like in the old days of mining, when you could pick up the precious metals on the surface.

COHEN: But you have to have good eyes.

GOLDREICH: It still was a lot easier. And it was easier to get a job.

COHEN: So you were over at UCLA working with Gordon MacDonald?

GOLDREICH: I basically just worked by myself. I had an office. I didn't get bothered a lot. I attracted one student in physics whom I liked very much. In fact, it was a tradition that I continued—taking students that I just basically like. He's now well known. His name is Dick [Richard] McCray. He's a professor at Colorado. He's also a member of the National Academy. And we're still friendly. We climbed a 14,000-foot mountain last year.

I still wasn't really sure I wanted to do research for a career. I didn't really understand—I was very unsophisticated about what was expected. Again, I think when people were graduate students in those days, they didn't go to conferences. I never went to a conference as a graduate student. I didn't have a very professional view of what this type of job entailed. So it wasn't until, I guess, my second year at UCLA that I really started to get interested in a wide range of research. I thought maybe I'd rather teach at a small college. They have a different sort of life. Of course, there was very little pressure for money. There was always lots of money around. Anything you needed, you could get.

COHEN: Those were posh years.

GOLDREICH: Yes. And Gordon had lots of money. He didn't mind giving me what I needed. I never wrote a grant proposal or anything like that.

COHEN: But you worked by yourself, essentially?

GOLDREICH: Yes, I worked by myself. And then a year later I got a job with Stan [Stanton] Peale and I worked with him some. I got more and more interested in the research, and I got a lot of satisfaction out of it. I had solved a few problems as a graduate student and as a postdoc and now as an assistant professor. So I could see, in the first place, that I could do it, and second, that it was very satisfying.

COHEN: And you could just sit by yourself and do the mathematics and physics of these problems?

GOLDREICH: Yes, that's right. And I would poke around and try to learn about new things. I spent a month at UC San Diego. They ultimately offered me a job. I had a friend there named Bob [Robert] Gould, who had been at graduate school with me. And I already knew Wal [Wallace Sargent] a little from England. I had met him and Anneila Sargent in a punt on the Cam with Lynden-Bell while I was a postdoc. And I got to know Wal better, actually. I spent more time with Wal, in the end, than with Bob. It became a long-term friendship. Wal was uneasy about teaching physics at UC San Diego, and I was uneasy about teaching astronomy at UCLA, so we used to talk. There was a tie-line between the two campuses—a dedicated phone line. Wal used to help me with astronomy, and I would help him with physics. I got to know Anneila. She stayed with us when Wal went away a couple times. So I got more and more interested in astronomy.

COHEN: Now, how long did you stay at UCLA?

GOLDREICH: I was at UCLA for two years. I think it was during the first year that I was offered a job at Brown for about two and one-half times my salary, plus an associate professorship. That was also a big eye-opener for me, because I hadn't realized that my market value was much higher. I guess the faculty at Cornell, when asked who would be worth hiring, told Brown about me, and so they flew me out. My host was Leon Cooper, who had won a Nobel Prize [1972] for electron pairing in superconductivity. They showed me the new building they were building, and they offered me all this stuff. But it was raining all day, and when I flew back to Kennedy from Providence, the plane in front of my plane—also Eastern Airlines, but from Boston—crashed into Jamaica Bay. I saw these big flares as the Coast Guard searched the crash site.
[Tape ends]

Begin Tape 1, Side 2

GOLDREICH: My father thought I should go to Brown. It was so much more money than I was currently making, and it would be closer to my parents. We could get a beautiful house, because in LA we couldn't afford anything. We rented an apartment there that hung over Santa Monica Boulevard. It shook every morning and evening when the traffic went by. But I thought

California was paradise, and I still think it's paradise. I wouldn't have gone back if they had offered me ten times the salary. So I stayed at UCLA. Later that year, I was punched by a football player—actually the captain of the football team—in a dispute over priority on the squash court. When he was reinstated in school the following year, I decided to leave. I had already been offered a position at Caltech.

COHEN: And then Wal was offered one, too.

GOLDREICH: I went down to UCSD to see Wal, and we talked about it. First, John Bahcall and Wal and I and Bob Gould thought we'd all end up at UC San Diego. We also tried UC Santa Cruz. But then Caltech offered Wal and me positions. I went down to see Wal, and we decided it was worth coming here.

I came to Caltech in September 1966. I can't remember when the decision was made, but I remember calling up Caltech and saying, "If you make an offer, I will come." They said, "Sure," that they would. But then there was all this awkward business, because UCLA gave me tenure. And I remember Bob [Robert P.] Sharp at Caltech saying to me, "Well, Peter, we feel awkward about giving somebody your age tenure. How much is the tenure worth to you?" And I said, "$500 would be fine."

COHEN: [Laughter] Do you still feel that way about tenure?

GOLDREICH: And he gave me $5,000 more—maybe more than $5,000 more. I got a split appointment here too, just like I had at UCLA. I had half an appointment in planetary sciences and half in astronomy. At first my work was more planetary science; later it became more astronomy. I've had most of my students from physics, next most from astronomy, and third most from planetary science.

COHEN: So you moved to Pasadena in 1966?

GOLDREICH: We moved to Altadena. I stayed on the UCLA faculty through the summer, because they had requested that I keep my appointment that long. However, I worked at Woods Hole for the summer—I went there to attend a geophysical fluid dynamics school.

COHEN: Is there anything you remember as outstanding in the two years you were at UCLA—something in your research that's seminal, that you think back to?

GOLDREICH: I don't know. I don't think I've ever done anything really outstanding.

COHEN: Oh, come on. [Laughter]

GOLDREICH: No, I'm serious. The thing I did at UCLA that's best known was to work out how the planet Mercury came to end up spinning on its axis three times for every two orbits it makes around the sun.[1] I did that with Stan.

COHEN: And that was a mathematical problem?

GOLDREICH: Well, no. It was physical—all these things are a matter of touch. They're never purely technical.

COHEN: You're saying intuition?

GOLDREICH: Yes, somehow getting a feeling for what the essential aspects are and then being able to make a model problem that you can solve which captures that aspect. All my work is the same, in that sense. Sometimes it's technical for the field but not technical relative to what people can do in other fields. But somehow it captures the essence of something in a model that is possible to analyze. And that's nice. Pick the problem and then somehow isolate the guts of it. Anyway, I did that. I also wrote a paper on the evolution of the orbit of the moon, which has had a good life.[2]

COHEN: Did you do that yourself, or were you working with a student by now?

GOLDREICH: No, that one I did by myself. And my thesis. Almost all my other work has been done with only one other person—occasionally two—usually a postdoc or a student. I usually like to talk to somebody, so I like working with somebody else. While I was in Cambridge as a postdoc, I did a piece of work with Donald Lynden-Bell that characterized the amplifier responsible for exciting spiral density waves in disk galaxies.[3] That was becoming well known. Probably the best work I did as a graduate student was to show that the resonances among the orbits of satellites of the major planets are a result of tidal friction. And I showed how, as these orbits might have evolved under tidal friction, they could get stuck in these particular resonances. This was at a time when nobody knew if tidal friction was actually a significant process—or if it ever had been—in these satellite systems. And I estimated the rates of the tidal friction. They've been confirmed now, by space missions. So I had done three or four things by then that were well known in various astronomical subfields.

COHEN: Now, you were living up in Altadena at this time?

GOLDREICH: Yes, we bought a house in Altadena. It's a gorgeous place. We had a nice piece of land. But we didn't like taking care of it. Susan had said that she was going to take care of it, but she decided that together the kids, the house, and graduate school were too much for her. And neither of us liked pulling weeds. The grass kept growing. And after ten years we had had enough. [Laughter] Also, I had always wanted to be down at Caltech. And the kids, by that time, wanted to be down there playing sports. So after ten years, we sold the house and moved into a dorm.

COHEN: Where was Dan born?

GOLDREICH: Dan was born while I was at UCLA. He was born at Kaiser Hospital in Hollywood. When we came to Pasadena, we had two children—a one-year-old and a three-year-old.

COHEN: So you must have very quickly gone through assistant, associate, and professor stages in your life.

GOLDREICH: Yes, I was an associate professor when I came here, even though I didn't have tenure. And then I got tenure—I forget—a year or two later. And then I was promoted to a full professor in '69.

COHEN: Did you ever work with Wal, or was he just your good friend for talking?

GOLDREICH: Wal and I wrote one little paper together, that's all. But he taught me a lot. He was a source of information about all things in astronomy—also, occasionally, things in literature. But I knew very little astronomy. I used to learn quite a bit when I sat in oral exams. It took me a few years before I could answer all the questions, but that was only because my colleagues kept repeating the same questions. I taught my way through most of the courses. And now, with the sort of physical processes involved in astronomy, I think I probably know more than almost all of them, because I've worked and taught in more different areas. But Wal taught me a lot.

COHEN: So now we get to Caltech, which is…?

GOLDREICH: Caltech was great.

COHEN: Right from the beginning?

GOLDREICH: Yes, I think Caltech is perfect for me. It's small. People are serious here. I like the students, and I sympathize with them.

COHEN: Peter, you once told me that as far as you were concerned, your citizenship in the world of astronomy and science is done here, taking care of local students and local things, and not on a national or international scale.

GOLDREICH: That's right. I very seldom go to meetings—certainly less than once a year. And I try to avoid committees outside of Caltech. Since I'm a faculty member in two divisions, I get plenty of opportunity to do things here, and I find when you do things here, usually results come in. Whereas if you do something externally, it goes into some file and usually there's no result. You have to fool yourself into thinking you made a difference—whereas here, you really do. It's small enough. So, yes, I devote myself to this place—much more, I think, than most of my colleagues do, in percentage of effort.

COHEN: Of course, you're here because you're a theoretician and you don't go anywhere. I mean, you don't go observing. Of course, if you don't go to meetings or committees either, you don't travel too much.

GOLDREICH: Well, I travel, but for recreation. I travel to climb a mountain or canoe a river but not to sit in a meeting. Oh, I'll go give a talk here and there, but I really like being here. I like to get regular exercise. I love the facilities. Even the old facilities were great. The new facilities are unbelievable. I like, essentially, all my colleagues—even the ones I argue with.

COHEN: Now, tell me, how many years did you spend in the student dormitories?—speaking about your contribution here.

GOLDREICH: I spent four years as a resident associate at Page House. We sold our house in '76. At first we thought we would buy a Caltech house. And while we were going to wait for that house to become available—we had one in mind—I applied for and was chosen to be an RA. I moved with Susan into the Page House RA apartment, which was smaller than this room, and the kids had a dorm room across the hall, which they loved. And we stayed there for four years. Susan and I really did the RA together. And I played sports. The kids played sports with the students. I did math problems and physics problems. We took them on trips.

COHEN: So you liked it. They must have loved you.

GOLDREICH: Yes. We didn't have to cook, except on weekends. We ate the dorm food. It was fine. We had no social life, essentially, except for the dorm. It was wonderful. And I'm still friendly with lots of the kids—now middle-aged.

COHEN: Some of them are still around?

GOLDREICH: A couple of them are my colleagues. The best two students in one class were Sterl Phinney and Ken Libbrecht. They were roommates.

COHEN: I don't know Ken Libbrecht.

GOLDREICH: He's a professor of physics here. He did work on solar oscillations. Anyway, I met Ken—he was a transfer from North Dakota as a sophomore—I met him at freshman camp. I went to freshman camp five times. And then I also met Sterl at freshman camp, but he was in Dabney. After a year, I think, Ken wanted him to come to Page House. But Page House didn't want him, because he wasn't going to win any points in sports—just take up space, they said. But I said I needed somebody to talk to. Later I met the woman who would become Sterl's wife in China, and she came to graduate school at Caltech. So I provided service from cradle to grave.

COHEN: What do you mean by "met his wife in China?"

GOLDREICH: His wife is Chinese. I met her at summer school in China.

COHEN: And invited her to come to Caltech?

GOLDREICH: Well, actually, she was tagging around after my first student, Dick McCray, who was also there. Dick spoke a little Chinese. The Chinese loved him. After he left, Lin was crying and sad. I remember telling Dick, and he said to me, "Well, that's what you get for playing around with a married man." But she wasn't playing around with him—he was kidding. Anyway, he told her that she'd be better off at Caltech, and I told her that, too. So she came to Caltech.

COHEN: What year would that have been?

GOLDREICH: It was 1988, the year before Tiananmen Square. It was also the year [radio astronomer] Al [Alan T.] Moffet died. When Lin came here, we helped take care of her for a while. We gave her money to get better glasses. We'd invite her over for dinner. I'd take her exercising. I helped her through her first bad boyfriend experience. I kept telling her, "Only English-speaking boyfriends. You have to learn English." The first one didn't work out too well.

COHEN: Well, let's talk about you. [Laughter] So, of these student committees you were on, were any of them formal? Or were you just sort of available to talk to any kids that wanted to talk to you?

GOLDREICH: Well, at Page House I talked to kids all the time, of course. You know, kids would have problems. Susan did counseling.

COHEN: Did you feel the house system was all right?

GOLDREICH: Page House was fine.

COHEN: Did the kids throw food there?

GOLDREICH: No. They weren't much for throwing food. We had this dirty-joke night, which was sort of embarrassing for a lot of the students and for me. I talked to the leadership of the house, and I got that stopped. They used to announce that they were having dirty jokes and that people could leave if they wanted. Students were embarrassed to leave, so I said it would be better not to have it. It's awkward for people to leave. And they did away with it. That was one of my big accomplishments. [Laughter] One year, a lot of kids dropped out, and it sort of fed on itself. That was depressing.

COHEN: That was at Caltech altogether?

GOLDREICH: In particular in Page House. Well, they took leaves and came back. But most of the time it was an OK place. They had a lot of community.

COHEN: There were boys and girls there?

GOLDREICH: Yes, but there weren't very many females. We had ninety people and maybe seven females—not a lot.

COHEN: Was that a good experience for them? Or did they feel put upon?

GOLDREICH: It was different for each one. Some were very poised and could handle it easily. Others—there was one very young one, in particular, who became sort of the house madam. That wasn't so good.

COHEN: Because there's so much discussion on this now—about shared facilities.

GOLDREICH: Oh yes, the bathrooms were co-ed. But when I compare it to the dorm I was in at Cornell, which was all male, I would say it was a lot nicer. There was much more of a sense of community. Page House was socially less sophisticated, but I felt that people treated each other better, by and large, than at Cornell. I felt it was fine. You know, these kids are away from home for the first time, and all sorts of things happen. But, by and large, I didn't think it was bad. And our dorm was one of the better ones. I think Jim Mayer, master of student houses, put our family into—

COHEN: A good situation.

GOLDREICH: Yes. And also, they wanted us because we were interested in sports and could help the house win the inter-house trophies.

COHEN: So you really enjoyed those years?

GOLDREICH: Oh, I had a great time. And I still was able to work.

COHEN: Well, you didn't spend any time on transportation.

GOLDREICH: No. It was fine. There was a lot of interesting intellectual stuff going on. And during inter-house, the kids constructed amazing things.

COHEN: Are they still doing that? I don't hear much about that.

GOLDREICH: No. Inter-house was stopped, because the outside world invaded—there were incidents involving violence.

COHEN: Let's go on to more modern times, in some sense. What did you do as far as the institute went?

GOLDREICH: Starting, I guess, before Murph [Marvin L.] Goldberger, Caltech president 1978-1987] came, I already began to be on important divisional committees. I was on the provost-selection committee when Jack [John D.] Roberts was selected and when Robbie [Rochus E.] Vogt was selected. I was on the presidential search committee when Tom [Thomas E.] Everhart [Caltech president 1987-1997] was selected. I was on the divisional chair's selection committee regularly, in both divisions. I was asked to be the division chairman twice for geology and once in physics. I was asked to be provost once—or twice, if you consider being asked twice for the same round.

COHEN: But you were never interested?

GOLDREICH: I was the acting division chairman in GPS [Geological and Planetary Sciences] for a short period when Gerry [Gerald J.] Wasserburg left.

COHEN: Left?

GOLDREICH: Got dethroned. I never wanted to do administration. I spent a tremendous amount of time with Robbie Vogt when he was the division chairman [Physics, Mathematics, & Astronomy, 1978-1983] and after, when he was the provost. We had hours and hours of meetings.

COHEN: You mean, just talking over with him what he had to do?

GOLDREICH: Talking with him, yeah, and calming him down, and bringing him back up after he crashed.

COHEN: Now, this was when Murph was president?

GOLDREICH: Most of it was when Murph was president. I talked a lot to Murph. For a while I was in the administration building all the time. I'd either be in there because Murph wanted to talk to me or because Robbie wanted to talk to me.

COHEN: Do you mean that they would call you and say, "Come in. I want to talk"?

GOLDREICH: Yes.

COHEN: What were they talking about?

GOLDREICH: About how to get along with each other. Robbie thought Murph was trying to damage him, and he was reacting in a way that—well, it was just terrible. He was so convinced that Murph was out to get him that he was trying to get Murph first. And this went on for years. One time he quit, and then Murph was afraid the trustees were going to fire him.

COHEN: Now, Robbie always got on with the trustees, didn't he?

GOLDREICH: Robbie got on with the trustees. After he quit that time, I was at a meeting with a trustee—Shirley Hufstedler—who understood that Robbie had problems. There was no question about that. And when Robbie called some of the trustees to complain about Murph, a couple of them told him that he should get counseling. They weren't totally taken in by him. Some of them more than others. It was a very awkward business. I feel a little awkward even talking about it. But I spent an enormous amount of time over there.

COHEN: How do you think you got in this kind of position, where they called on you in this way?

GOLDREICH: Well, I have found that many of my colleagues use me that way, not just these. For example, even when I go traveling I often have quite senior people come up and ask me for advice on things like what they should do for the rest of their life.

COHEN: So you have some quality there that people are attracted to.

GOLDREICH: I look, I guess, like I know what to do. And I usually can give an opinion, anyway.

COHEN: Maybe it has something to do with not pushing yourself.

GOLDREICH: Maybe I look calmer than I am. I don't know. But I get asked a lot. When I was at Harvard last week, I gave a colloquium, and two full professors approached me. One of them asked me what he should do with the rest of his life, and he really wanted to talk about it. I couldn't believe it. The other one was even funnier. He asked me whether he should follow his mother's advice and have a family—if that would take too much away from his science. A full professor!

COHEN: And you didn't mind giving him the advice?

GOLDREICH: No. I told him I wouldn't have missed it for anything. But, I don't know. There are quite a few faculty here at Caltech who come to me when they have some problem—they don't get observing time on the telescopes, they are bothered by something. They come and ask me for advice. I always give it. I don't know if it's always good, but I have a whole crew of those, a group of individuals. I don't know exactly why it is. Maybe because I listen.

COHEN: That could be.

GOLDREICH: And I've had former students who would call up—even undergraduate students—even a kid I wrestled with in 1968, he calls up all the time.

COHEN: But, Peter, I remember—and I don't remember names particularly—several unfortunate times when we had some suicides, and you were very instrumental in dealing with the parents.

GOLDREICH: Well, Peter Young was not a great success story. Peter Young was the brightest student we've ever had here. And he was Wal's PhD student, but he and Wal had difficulty getting along together. Peter would have been difficult for anyone, and Wal's a difficult person to begin with. Wal asked me if I wanted to help with him. It's funny, because Greg [Gregory A.] Shields, who had been a student here, was a professor at Texas, where Peter had started graduate school. He told Peter he should go to Caltech, because I could physically and mentally deal with him. But actually I couldn't. Peter had lots of psychological problems. He was suicidal, and he had a history of suicide in the family. His mother had committed suicide. Her father had committed suicide. And he would get depressed. And he probably had sexual orientation problems. He certainly had sexual problems. And he got fixated on a couple of women here. It was very awkward.

COHEN: Wasn't he an assistant professor at this time?

GOLDREICH: Yes. After he got his PhD, we made him an assistant professor. He killed himself in '81. He was probably an assistant professor for a year. He was a mighty machine as a scientist. He used to call me all the time, and I tried to tell him that if he was depressed, it would pass—just remember that it's not always like this, don't rush to kill yourself. Susan had done suicide prevention, so I used to ask him all of the standard stuff. If he'd say he was going to kill himself, I'd ask if he had a plan. Supposedly if you don't have a plan, you're less likely to do it. He didn't always tell me the truth. But there were a lot of indications. He crashed his car at Palomar.

COHEN: Did you ever try to talk him into getting professional help?

GOLDREICH: Oh, yes, all the time. Actually I talked to both Robbie, who needed help himself, and Murph about it. And Murph called up a friend of his who was a psychiatrist. He recommended a professor at UCLA who dealt with suicide in young males, and this fellow called me to talk on the phone. But Peter wouldn't go see anyone. And then Murph arranged for me to see a psychiatrist.

COHEN: So Murph really took it very seriously.

GOLDREICH: Oh, we took it very seriously. I told the psychiatrist that this was for my friend—a sort of classic story. It took a little while before I actually could convince her that it wasn't me. There were terrible things. Peter took out a permit for a gun, and there were all sorts of veiled threats.

COHEN: All this time he was still doing his science?

GOLDREICH: Yes. I called up the gun store and told them that Peter wasn't stable, but they said that it was his right to have a gun. But, you know, there was this waiting period. I called up the Pasadena police, and they told me there was nothing they could do. And then Murph called a trustee, who called up the mayor, and that was the end of Peter's gun—although he didn't know it, and we didn't tell him. Eventually I bought that gun permit from him, because he had to forfeit the deposit. He never paid me back.

COHEN: So Murph was really good on all this?

GOLDREICH: Murph was wonderful in this sort of thing. The thing Murph was no good at was sticking to decisions and not doing things emotionally. But he was very good at this sort of stuff. He was terrific. Robbie wasn't bad on this, either. Everybody tried. Wal and I, before we found out that Peter didn't have the gun—that he just had the permit to get the gun—had talked him into taking a dance class at PCC [Pasadena City College] so he could meet women. Also, he joined one of these dating services that Kip Thorne had recommended, because Kip had belonged to them. He had quite a few experiences with women. Peter went out with one woman who cut hair, and she cut his hair, just like Samson, and conquered him. But anyway, the time he told me that he had bought a gun, Wal and I picked him up at PCC after his dance class. Peter always ate a steak before he went to bed. Wal and I had gone shopping and bought a steak. We took him to Wal's place to cook the steak. And then it turned out that he didn't have the gun—he had only put down a deposit. But there was one glitch after another. And then Caltech transferred the secretary Peter was pestering to JPL. And then Peter started in with my graduate student, Carolyn Porco. Carolyn's a pretty tough woman, but after a while she got frightened of Peter. She had encouraged him, partly because he had become a good dancer.

COHEN: So whatever he did, he did very well?

GOLDREICH: Yes. He could lift 350 pounds over his head, and he could run thirty miles. Carolyn liked backpacking and Peter had never done this. Scott Tremaine and I were going backpacking with Susan and Dan, and we told Peter we'd take him. We went out and bought him boots. We rented equipment for him. He came backpacking carrying his weights so that he would get extra exercise. Also special food. So we backpacked in the Sierras. We saw a plane crash on the morning of the second day, and Peter and I ran out twelve miles to get help. It took us a whole day to report this plane crash. We were flown back by high-altitude helicopter. It was a real adventure. We hiked over Forester Pass at 13,000 feet. When we came down, we had a good meal. Peter ate a steak. Shortly after we drove back to Pasadena, he killed himself.

COHEN: That was when he killed himself?

GOLDREICH: That evening. He went to see Carolyn, and Carolyn told him that she didn't want to see him. He said, "You won't see me anymore." He went back to the office and took cyanide.

COHEN: He had it there?

GOLDREICH: Yes. He had taken it out from the chemistry stockroom—potassium cyanide—weeks or months before. He was prepared.

COHEN: So there was nothing to do.

GOLDREICH: And that was that. I got a very nice note from Peter's father, thanking me for everything I had done, saying that he and his second wife were sure that Peter was going to commit suicide. They were surprised he had lasted as long as he had, because there had been incidents like this at Cambridge. That was very upsetting.

COHEN: That must have taken up a lot of your time that year. Was it one year or two years?

GOLDREICH: A couple years. I took him skiing. I took him to Owens Valley a few times. It was difficult with him, because I couldn't let him drive, because I was always afraid that he was going to do something nuts.

COHEN: What's interesting is that everybody progresses with their work here. I mean, with things like this at some other place, work would stop. Here it doesn't stop. You kept doing your work. He kept doing his work. Wal kept doing his work.

GOLDREICH: Even when Peter was depressed, he worked. But he didn't do much else. He moped around. He was just very badly depressed. It was so painful to him when he was depressed. He used to always say, "How long is it going to last? I can't stand it. I wish I was dead."

COHEN: Well, it's an illness.

GOLDREICH: The amazing thing was when we saw him dead in the office. Robbie came up and the body was there. And Robbie said, "I wish I had had the courage."

COHEN: All these crazy people.

GOLDREICH: That's the sort of people we have around here.

COHEN: Well, genius is not so far from madness, huh?

GOLDREICH: Robbie is a genius in his own way, too.

COHEN: Let's go back, Peter, and talk about your presidential search committee for Everhart. Do you want to talk about that?

GOLDREICH: Murph left a little early, because he got a job as the director of the Institute for Advanced Study, in Princeton. The search committee for his successor was chaired by Don [Donald] Cohen. I thought Don did an OK job, and it was a very good committee. We worked well together.

COHEN: Who else was on it?

GOLDREICH: It was Don, Bruce Cain, David Van Essen—both of them have left Caltech now—Steve Koonin, Ahmed Zewail, and Tom McGill. The committee was appointed, I think, by the faculty chair. And we had very little interference from the trustees—essentially none. Rube [Ruben F.] Mettler, who was chair of the board, could do anything that needed to be done. He seemed to have access to everybody. So when we needed something, he would do it. And he would give his opinions. But, basically, we were responsible for the whole thing.

COHEN: So you didn't have counterpart trustees on this committee?

GOLDREICH: There was a counterpart trustee committee, but they didn't do anything. I think the most recent committee did more. Now, we were very concerned about the administrative disarray, because of the Robbie-Murph business, so we were looking for a sounder administrator. We interviewed some people who were quite flashy, a number of whom later said that we had offered them the job, but nothing like that ever happened. I was surprised to hear through the grapevine that they were saying that they had been offered it. They were interviewed. We interviewed maybe ten people. We interviewed David Baltimore. He was one of the most interesting interviewees. He had an unsuccessful conversation with Mettler, I think. And also, he was viewed by some people as too arrogant. I found him very interesting, because he talked about AIDS at the time when I was watching Al Moffet die and spending a lot of time with him. But anyway, my favorite candidate was an Englishman by the name of Ron Oxborough. He is a geologist who had spent time at Caltech as a Fairchild Scholar. I thought he was terrific. He actually did move into administration at one of the Cambridge colleges. He was the chancellor, or whatever they call it. Then he became science advisor for the British government. He is now Sir Ron Oxborough, rector of Imperial College, University of London. In any case, he seemed just right. He flew over here and we interviewed him. But there was a split on the committee. A number of people felt he was too—well, first, he was not American. He couldn't understand the system, even though he had worked here. Others felt he was too flashy. I thought he was very good. And then we had to meet a whole bunch of administrative types, like Tom Everhart. There were a whole group of them. In fact, they would comment on each other being colorless. [Laughter] We also had some industrial types. They mostly weren't that interested. I think they felt the faculty would be too slow to change. They couldn't make enough moves. They got more satisfaction out of manipulating things at places like Bell Labs, I don't know. Anyway, there was a period in which a lot of our candidates disappeared. We were left with Oxborough and Everhart—pretty much those two. And we avoided some of the people, because we thought they would get into fights. They were forceful characters, and we were worried, after Robbie. So I basically concentrated my efforts on trying to get Oxborough to the front. And I had some help. Bruce Cain was particularly enthusiastic. In the end we got him even with Everhart. I have nothing against Everhart, except that when he met us he made some comments about astronomy and man-made versus natural science that I didn't agree with. He made statements about Caltech having too much astronomy and maybe too much natural science, compared to engineering. And I always thought that in a small place, you could never be really tops over a wide spectrum of engineering. It needs too much specialization. Our success was really in pure science. We shouldn't try to be like MIT. I had a completely incorrect impression of Everhart based on this meeting. The reason I was less than wild about him was based on this impression, which was totally wrong. [Laughter] I thought he would be stern and not very gracious and would push an agenda that I didn't agree with. And it turned out that he was almost obsequious. I think he never felt at ease here.

COHEN: He was certainly always pleasant.

GOLDREICH: Yes, but he had a lot of trouble making decisions. He didn't push anything. Basically, we were still waiting at the end, after ten years, for him to do something. He never felt the time was right to do anything significant. Not that anything terrible happened.

COHEN: He got on very well with the trustees, I think.

GOLDREICH: Some of them, not all of them. Some of them realized that he was fairly limited. I don't know. Look, who the hell knows? It was OK. But I completely misread him. The best reading we ever got on Everhart came from Korn-Ferry [International], a consulting firm. It was a report we received after the die had been cast. I remember we were having our dinner at the Ath [Athenaeum], celebrating being done, and we read this report. And Steve Koonin said, "Maybe we should start again," [laughter] but it was too late. There was some other little dirt. It turned out that Everhart had once been considered for a faculty position here, and his initial appointment had been turned down by Bob [Robert F.] Christy. McGill knew it and didn't even tell us, because he wanted an engineer. We gave McGill a hard time. He really squealed for one session. But then, Everhart's faculty appointment had gone through in the end anyway, but that time he didn't accept.

COHEN: Well, maybe next time we can talk about his tenure.

GOLDREICH: Yes. The trustees flew Oxborough to New York. From what I heard about it, both first- and second-hand, he wowed them. They were ready to offer him the job. And they asked him whether he would take it, and he said he would have to think about it. They decided that if they had flown him on the Concorde to New York and he still hadn't made up his mind, he wasn't their person. So they offered it to Everhart.

COHEN: I think it would be a little hard having someone who was not an American.

GOLDREICH: It would have been OK. He had worked as a professor at Cornell. It would have been much more interesting.

COHEN: Well, maybe that's enough for one session. That's good. **[Tape ends]**

PETER GOLDREICH

SESSION 2

April 2, 1998

Begin Tape 2, Side 1

COHEN: I'd like to backtrack a bit, because we talked about when you did the research and with whom, but we didn't talk too much about the research itself. You talked about stuff you did with Tommy Gold for your thesis. Where did you go from there?

GOLDREICH: Well, I tend to work in one or two areas for several years. Actually, as I'm getting older, the periods of time that I work on things seem to be getting longer. I'm not sure whether that's because I'm getting slower or because subjects are getting better developed and you have to dig deeper into more technical work.

COHEN: Maybe the easier things are solved already.

GOLDREICH: Yes, I think there's probably some of that. Surprisingly to me, the work I've done that's best recognized is often the simplest thing I've done in any subject. And then I go on and do a lot more, but it's really some bottom line that's come through some very simple piece. Anyway, I started off doing solar-system dynamics, because when I was living in Tommy Gold's house, he would ask me to look at dynamical problems. I worked on three problems there. One problem—the simplest one—took a fairly long time to be accepted by celestial mechanicians. This was possibly because of the way I solved it, which was non-standard, since I didn't know what celestial mechanics was. It was an explanation for how the satellites, the small satellites of Mars, managed to remain in the equatorial plane of the planet, even though the equatorial plane of the planet was precessing—not maintaining a constant orientation—in inertial space. I showed that the Martian moons were all locked into this plane by the gravitational oblateness of the planet—by the fact that since the planet was spinning and flattened at its equator, it had a nonspherical gravitational field. And these satellites were sort of locked into the equator by this field, even though the equator plane was moving around in inertial space.

COHEN: When you say "nontraditional," what do you mean by that?

GOLDREICH: I solved this problem in a non-inertial coordinate system, by setting up a coordinate system that was attached to the planet. The coordinate system was precessing in space. And I treated the fictitious, or non-inertial, forces—particularly the coriolis and centrifugal forces—as perturbations in celestial mechanics. Whereas traditionally celestial mechanicians would have worked in an inertial frame and gone through some god-awful geometry to solve this problem. It took a long time for this paper to be accepted in the *Astronomical Journal*.[4] It was rejected several times. And when I was a postdoc and kept getting rejections, I kept telling my wife, Susan, that the paper was correct. I think she began to wonder. But in the end, the editor of the journal, Dirk Brouwer, who was sort of dean of celestial mechanicians in the U.S., re-derived my results in a more standard way and accepted the paper. After that, he made up for my long wait for this paper by immediately accepting every paper I submitted to the *Astronomical Journal*, until he died. I think he must have just thought that I deserved this compensation.

COHEN: I see. Well, he was that impressed.

GOLDREICH: So that was very useful for me.

COHEN: Now, this paper was actually part of your thesis.

GOLDREICH: Yes, this was one of the three papers to come out of my thesis.

COHEN: Did you use somebody else's experimental data?

GOLDREICH: No, this was just sort of common sense. These satellites had been observed for a long time. Tommy just said to me, "Well, why do they stay in the equator plane of Mars, because that must be moving around in space?" And I estimated how fast Mars would precess—something like 150,000 years. Then I wondered about it. It didn't take very long to solve. Actually, two much more interesting papers were written on this subject years later by two students of mine—Bill Ward, who is now at JPL, as part of his thesis—and Jack Wisdom and a student of his. Jack's now a professor at MIT. They showed that the motion of the equator plane, or the precession of Mars, was much more complicated than I had assumed and was quite chaotic. This probably has profound implications for long-term climate variations on Mars. These are both very influential papers.[5]

COHEN: They just followed through from your initial work?

GOLDREICH: Well, Bill's was sort of built a little on what I did, but it was more just poking around and finding something new. I didn't really have anything to do with it, except that I was his thesis advisor, and after he got his PhD, it was sent to me to referee. Jack's was a subtle extension of Bill's work. It was really very interesting. Anyway, after the Mars paper, I did the first of a number of studies in which tidal friction played an important role. I investigated a suggestion—I think made by a man named Grove; I'm not sure whether it was Grove—that the tides raised by Earth on the moon would tend to damp or destroy the orbital eccentricity of the moon, which is nonzero. Small, but nonzero. I worked out a comparison between the effects of these tides and those raised by the moon on Earth, which had never been evaluated before.

COHEN: Tides on the moon?

GOLDREICH: Tides raised on the moon that have since been detected by experiments done on the moon. Tides raised by Earth on the moon tend to damp the eccentricity of the lunar orbit, and tides raised by the moon on Earth tend to increase the eccentricity. I tried to evaluate the relative importance of the two sources of tidal evolution on the orbital eccentricity of the moon. That was the second paper in my thesis. In the third paper, which was the most subtle and significant one, I evaluated the effects of tides on the formation and stability of orbital resonances among the satellites of Jupiter and Saturn.[6] In both planets, the satellite systems have a number of orbital resonances where the orbital periods of different satellites are related to one another by a ratio of small integers—two to one, three to two, four to three—things like that. These are very frequent. There had been a paper by [A. E.] Roy and [M. W.] Ovenden, two English astronomers, pointing out that these near resonances were more frequent than what you would have expected on the basis of chance. What I showed was that they could arise through the independent tidally driven expansion of the orbits of these satellites, which would expand individually until they hit off a near resonance. Then they would get stuck in the resonance and stay there. This hypothesis, and proof that it could work, enabled me to estimate the rate of tidal evolution in these systems, which previously was completely unknown. It still hasn't been directly measured, although we now know indirectly from the volcanoes on Io that I got the rates more or less right.

COHEN: Now, you had to wait some time before these were published though.

GOLDREICH: Yes. These papers got published within about a year. At the time I was going over the manuscripts for publication, I was in England as a postdoc. I was supposed to be linked up with Fred Hoyle, but since he had left the department I ended up working with Donald Lynden-Bell. And there I worked on spiral arms in disk galaxies. We wrote two papers, the second of which characterized what's now known as the amplifier for spiral density waves, sometimes called the swing amplifier for spiral density waves in disk galaxies.[7] It described the mechanism by which spiral waves in disk galaxies are amplified. It took a long time before the connection between this amplifier and the density wave theory of C. C. Lin and Frank Shu were connected together. Basically, our work is the Fourier transform of theirs. It displayed this amplifier very well, which they hadn't done. Similar work was done by Bill Julian and Alar Toomre about a year later. Julian became my postdoc and was a coauthor on probably the most widely cited paper I ever wrote—on a completely different subject—on pulsars.[8] Anyway, I presented the spiral-arm work in 1964 in Hamburg, at the International Astronomical Union meeting. I only stayed for a couple days. But before that, I had gone to Greece—to Thessaloniki—and I had met Brouwer and, I assumed, the referee of this first paper of mine that was getting rejected all the time. I got that straightened out. That was Lynden-Bell's suggestion—that I attend the meeting as I could explain what I had done to Brouwer. So that worked out fine.

After a year at Cambridge, I went to UCLA. The main things I did at UCLA, for the two years I was there, was, first, to write a paper on the history of the orbit of the moon, in which I showed that there were no landmarks in the past history of the orbit of the moon that you could point to that might have been obvious places where the moon was formed. And I did two papers with Stan Peale, who had been a graduate student with me at Cornell. He had also been Tommy Gold's student. When he got his PhD, I mentioned him as a candidate to the department at

UCLA, and they hired him. So the second year I was at UCLA, Stan was my colleague. We were there when the announcement came out that the planet Mercury wasn't spinning once per orbital period around the sun but three times for every two orbital periods around the sun. This was based on the Arecibo discovery by [Gordon] Pettengill and [Rolf] Dyce. We started to think about how this resonance could have been set up. We worked out that as the planet slowed down due to tidal friction of the tides raised by the sun in the planet, it would encounter a number of these spin-orbit resonances. And we formulated a theory that showed that there was a certain probability of capture at each one of these resonances. It was a probabilistic thing, because—depending on the exact phase details, which there was no way of recovering—it might be captured and it might not. And we showed that it was plausible that Mercury could have escaped all higher-order resonances and gotten stuck into the one in which it's found today. That was extremely satisfying work, because, first, it was a new observation that we addressed. Second, we formulated a new type of concept in dynamics—that of capture probability—that has since proved useful for many other problems. So that was really nice. We also worked on similar tidal effects in the spin of Venus. So I would say those are the main things I did at UCLA.

I also worked with another friend of mine—Gerry Schubert—who had been an undergraduate with me. I also recommended him to UCLA, and they hired him. He's still there as a professor. Gerry is a very bright guy who had studied engineering with me. On weekends, we used to go together to New York in his car to visit our girlfriends. Gerry was supported while he was an undergraduate by a naval ROTC scholarship. After he got his undergraduate degree, he taught at the Mare Island Nuclear Submarine School in Northern California. At the same time he also managed to attend UC Berkeley and get his PhD. And after that, they hired him at UCLA. He's quite a significant geodynamicist today.

Anyway, Gerry and I wrote a paper on the stability of differential rotation in the sun.[9] This also became quite an influential paper and the instability was named after us. It particularly became influential because around that time Bob [Robert H.] Dicke announced, while he was at Princeton—he just died recently; he was a great experimenter—that the sun was more oblate than previously had been thought. This indicated that it had a rapidly rotating core and that general relativity wasn't right, because this rapidly rotating core would make an oblate gravitational field that would account for some of the precession of the orbit of Mercury, which had been attributed to general relativity. Dicke then claimed that this was evidence in support of his scalar-tensor modification of general relativity. I went around the country with Dicke giving lectures in which I'd say the sun couldn't be differentially rotating inside because of this instability that Gerry and I had analyzed. I had a little experiment that involved salt and water and baby bottles, and so on, that I used to do at these meetings. We now know that the sun is not rotating differentially inside, as a result of helioseismology—a subject that was started here at Caltech by Bob [Robert B.] Leighton and his students Bob Noyes and George Simon. More about that later.

After that, I moved to Caltech. The first thing I did at Caltech that I was reasonably proud of was some work on the radio emission from Jupiter that was correlated with the position of its innermost Galilean satellite, Io.[10] This is work I did over a summer in England with Donald Lynden-Bell. I had gone back to England for a summer, and Donald had moved from Cambridge to the Royal Greenwich Observatory, which was then located not in Greenwich but in Herstmonceux. We lived on Boreham Street, in a little village nearby. I would pedal in across the sheep fields to the castle and work with Donald every day. We produced a theory that, in retrospect, is only partly correct. We missed the fact that Io was the source of a very large cloud of plasma that is orbiting Jupiter. The volcanoes spew out gas that goes into orbit around Jupiter and gets ionized. But the concepts we put in about the radio emissions were probably mostly correct. That was my first encounter with electrodynamics in a sort of natural setting, with the complex interplay between rotation, magnetic fields, and plasmas. And it put me in a very good position to do the work that I did with Bill Julian when pulsars were discovered. When pulsars were discovered, all the different possibilities of what they could be were very rapidly staked out. The winner in the sweepstakes was Tommy Gold, my old thesis advisor, who suggested that they had to be rotating neutron stars with magnetic fields and that only this could account for both the pulsation rates and the fact that these rates were slowly lengthening with time. What I did was look at the physics of a rotating magnet with the type of parameters that you would expect for a neutron star. And I discovered that a magnet that rotated, that had a big magnetic field and was as large as a neutron star, would generate enormous electric fields and that these would be great enough to tear charges out of the star and produce something that we had never encountered before in astronomy or anywhere else—a magnetosphere of surrounding plasma that was entirely maintained electrically, independent of any thermal temperature. And also that it would be impossible to short out these electric fields near the poles of the magnet and that, therefore, plasma would be ejected along the poles. These are now thought to be the active regions on these pulsars and the reason that they pulse. So this was a very significant paper.

I did that paper with Bill Julian, who had worked with Alar Toomre on the spiral amplifier, which I had also worked on with Lynden-Bell. Alar asked me to take Bill as a postdoc. Bill had done one postdoc already at Chicago, and he hadn't accomplished much there. Alar said Bill was very bright but mathematical, and I also found Bill to be very mathematical. He really wasn't very well suited for this work. The year after we did this, he got a job in New Mexico, where he has stayed and has done mathematics.

COHEN: So you were in your old role of working out Tommy Gold's mathematics.

GOLDREICH: Yes. I'd say that up until that time, and even in the next project I worked on, I was working on subjects that were either of interest to Tommy when I was a graduate student or became of interest to him after that time. The next thing I worked on was astronomical masers.

COHEN: We're in the seventies now?

GOLDREICH: The early seventies. I worked a bit more on pulsars. I wrote another paper with Bill Julian, which is OK but not as significant as the first one. And then I wrote one, by myself, which is still quoted—on the effects of the misalignment of magnetic and rotation axes, and whether the magnetic axis will tend to align with the rotation axis.[11]

But then I was always interested in astronomical masers. They seemed very mysterious. I started to work on them. It must have been about 1970. I was giving some lectures in the summer to small-college teachers at Stony Brook. We were staying in one of the apartments there. My family was there, and my parents came out, too. It was muddy as hell. That's what I remember. They were just constructing the campus. And I started working on the theory of polarization of these masers and also on the relation between their apparent sizes and their physical sizes. Here I was, again, working on a subject that Tommy and Ed Salpeter had done some work on while I was a graduate student. This work on masers went on for about three or four years at a high level and probably another couple of years at a lower level. I had a number of very capable collaborators, including two students—John Kwan, who is now at the University of Massachusetts in Amherst, and Doug Keeley, who isn't doing academic work. He works for a company called Science Applications. We all had lots of recreational interests in common. Then I finished off working on this subject with Nick Scoville, when he was a postdoc here—Nick later became a colleague. Nick was very good to work with. For an observer, he had an excellent theoretical understanding. You could really trust what he did and said—so I had a very high appreciation for him. I was sorry we didn't hire him then, though—we had an assistant professorship open. But we corrected that years later, when we brought him back as director of Owens Valley Radio Observatory, as a full professor. Anyway, in the maser work, the two biggest things I did were setting up the basic theory, which is still used to explain the polarization of the maser radiation and how it's related to the magnetic fields in the maser sources. This was, for astrophysics at least, quite a technical piece of work. And for years I don't think anyone paid attention to it. But surprisingly, after fifteen years, a number of people not only looked at it but started to extend it and improve it. Foremost among these is Bill [William D.] Watson, who is at the University of Illinois. In retrospect, it showed me that even if you do something very technical, if it's good and sufficiently far ahead of its time, it comes back. It has an impact—even though at the time it seemed difficult to explain to anybody what it was. Only a few people knew about it. But anyway, it's lasted well. And then Nick and I got together with Moshe Elitzur, who was a postdoc and is now at the University of Kentucky—I ended up having a rather low opinion of him, I must say—you can put that in.

I didn't trust what he did. And also he didn't do what he said he had done. But fortunately I had Nick to work with, even though we let Moshe be first author on one paper, through some craziness. We wrote two good papers, Nick and I, with Moshe sort of hanging on. Anyway, we wrote two papers in which we worked out the mechanism by which OH molecules are created in the outflows around evolved stars.[12] We proposed—and it's since been widely accepted and extended—that these molecules are created by the photodissociation of water. When the circumstellar flows become optically thin enough, stellar ultraviolet radiation gets in through the flows, photodissociates the water and produces OH. We worked out the means by which the maser, in this particular case, was excited. There were four lines that the OH molecule could emit in its ground state. One of them is always found to be much stronger than the others in this type of source. We really nailed this one; we showed just why this was. It was sort of a subtle argument in radiative transfer. We located how far away these masers were from the central star. They were about 1,000 astronomical units away from the central stars, which themselves were about an astronomical unit in size. They were enormous stars. And all of this was verified by later experiments. It was very satisfying. And it was also very good working with Nick.

COHEN: Do you do anything with Nick now, at all?

GOLDREICH: In recent years, Nick and I have been arguing about the direction of the department. It looks like, as Nick said, we're going to be friends again, because I've conceded.

COHEN: You let him have his way.

GOLDREICH: He now has his way, yes, as of this week. But anyway, that was a fairly substantial period of work for me with masers. First the pulsars and then the masers. In the mid-seventies, after the pulsars and masers, I got interested in solar oscillations. Henry Hill, from the University of Arizona, came to Caltech and gave a talk in which he announced that he had measured diameter variations of the sun. Henry Hill had been trying to shoot down the claim by his former mentor, Bob Dicke, that the sun had excessive oblateness. And this he claimed to have done. But he also made the much more startling claim that the sun was oscillating globally and that he was measuring these oscillations by seeing changes in the diameter of the sun. I found this very difficult to believe. So with my old student Keeley I made an estimate of how much you might expect the sun's diameter to oscillate on the basis that the oscillations would be excited and damped by interaction with convective motions in the outer part of the sun. We found that we could make an estimate for the amplitudes of these modes and it was much less than Henry Hill proposed. So we then went out to see Henry Hill while he was supervising students at the University of Colorado. We flew out with my first student, Dick McCray, in his small airplane. It was an exciting flight. I had to suck on oxygen most of the time to keep from becoming nauseated. We landed at night on an unlit runway in Aspen, Colorado. Dick got fined for that. But anyway, we met Henry Hill. And he showed us his data and we met his students, Tuck Stebbins and Tim Brown, who is now at HAO [High Altitude Observatory, in Boulder, Colorado]. The only thing I could think of when Henry Hill showed us his data was the old story "The Emperor's New Clothes," in which everybody stood around and commented on the emperor's new clothes until some little kid realized that he was totally naked. That's what I felt like, looking at the data—there was nothing there. But the students were all nodding, "Yes, yes, yes," and Henry was going on. So I was rather disappointed, and I just dropped the subject.

Then I went on to work on planetary rings. I did this work mostly with Scott Tremaine, who was a new postdoc at Caltech and asked me to give him a hard problem in dynamics. Scott had been a student of Jerry [Jeremiah P.] Ostriker. He came from Princeton. Jerry sent me a little note saying that this guy was very smart and worth paying attention to. The first thing Scott did was to get into some sort of a problem with Wal Sargent, by telling Wal something Wal felt he already knew. Scott came over to see me and asked me about a hard problem in dynamics—he was rather shy, actually—so I suggested we work on the gaps in Saturn's rings. This began a long and fruitful collaboration, in which we worked not only on the gaps in Saturn's rings but also discovered a mechanism that confines narrow rings—the shepherding-satellite mechanism. We predicted that a lot of spiral density waves, similar to those seen in disk galaxies, would be found in Saturn's rings, which they were. We worked out the dynamics that determined the random motions—the relative motions of particles in planetary rings. We did some work on spiral structure and disks in general. I found Scott a wonderful person to work with.

COHEN: You've continued to work with him, haven't you?

GOLDREICH: Yes. So we worked together for more than a decade. In the second half of the work, we were joined by a postdoc from France—Nicole Borderies. She's since been remarried; her name is now Nicole Rappaport. She works at JPL. You probably remember her, because we went to watch the grunions together.

COHEN: Oh, was she there?

GOLDREICH: Yes, she was there. She was, at that time, very skinny. That's no longer the case. Scott and I had a lot of interests in common besides work. We ran together. Actually, Nicole ran with us. She was a good runner. Sometimes we went skiing. She's also a good skier. And we went hiking. I still do these things with Scott—canoeing, hiking, skiing.

COHEN: He's in Canada now, isn't he?

GOLDREICH: No. Scott left Caltech after one or two years, I can't remember which. He was a prize postdoc here. Then he spent a year in Cambridge as a postdoc, and then he went to the Institute for Advanced Study as a long-term, or five-year, member. After that he went to MIT, where he became an associate professor. We continued working throughout this whole time. And I remember that when we did this work on shepherd satellites, I told him that this was going to make him very famous in astronomy. And he said that it would be too late for his tenure decision, because this work was based on observations made of the rings of Uranus, which were discovered in '77—by Jim Elliot, now at MIT but then at Cornell—but wouldn't be observed in detail until Voyager arrived, in 1986. Fortunately for us, when Voyager got to Saturn, it discovered shepherding satellites around the F-ring of Saturn. So Scott got famous sooner, and he got tenure at MIT.

COHEN: Is that where he is now?

GOLDREICH: No. He left MIT, against my recommendation, to become director of the newly formed Canadian Institute for Theoretical Astrophysics in Toronto. He really thrived there. He did a great job. He built it up. My advice was probably not so good. I thought it would be bad for his science, and I still think probably it limited his science. But it made him a happier person. He was very well suited to be a director, and I hadn't taken that into account, because it would not make *me* happy to be a director. Scott directed CITA for ten years, and he left it last year. He's now a professor and director-designate of the Astronomy Department at Princeton. I think as soon as he stepped down after two five-year terms as CITA director, he realized that his happiness depended on his being director somewhere. I haven't yet visited him since he's gone to Princeton. We used to meet a lot at the Institute for Advanced Study, even when he was in Canada. I spent several months in Toronto while he was director, and we would run in local ravines and go canoeing in the lakes in Algonquin Park.

COHEN: So he was a good partner for you in many ways.

GOLDREICH: Yes. Susan and I and Scott and Marilyn, who is now his wife, spent two weeks canoeing in the Northwest Territories. I look forward to doing many other trips with him. Every time he visits Southern California, he stays with us and we climb a mountain or bike to the sea or something similar. Our planetary ring work has held up well. It was also very satisfying, partly because some of it was predictive—the shepherd satellites, the density waves. We predicted that these things would be seen and how they would work. And they weren't generally accepted. And then they showed up just as predicted.

COHEN: But it took some time.

GOLDREICH: It took several years. Although we were never absolutely sure, we were pretty confident that we had gotten it all right. And then, during this period, I started to come back to working on helioseismology, because it became clear that, while Henry Hill was wrong, people who were measuring velocity fields on the surface of the sun were detecting global oscillations there. I went back in the late eighties and reworked the predictions of what the amplitudes in these modes should be and what their lifetimes should be—how rapidly they should damp. This work was done with a graduate student by the name of Pawan Kumar, who is now at the Institute for Advanced Study, and a postdoc by the name of Norm Murray, who had been a Caltech undergraduate in my dorm when I was dorm father and who is now a professor at the University of Toronto, where he just got tenure. And the data have gotten better and better. Our prediction that the modes should be stochastically excited, rather than being overstable, has now been well confirmed by the statistics of the variations of the amplitudes and phases of the modes. And helioseismology is a very, very active subject. There is a satellite devoted to it. There are six telescopes spread out worldwide that observe the sun twenty-four hours a day from clear sites.

COHEN: And all those researchers have to read that initial paper.

GOLDREICH: Yes. Our work has been quite influential. Before these big operations came along, the best observational data on the subject were collected at Big Bear Observatory by Ken

Libbrecht, who is now a professor at Caltech. Ken was also a student in Page House—the dorm that I was dorm father in. We hired him back after he got his PhD from Princeton, where Bob Dicke was his thesis advisor and where he—that is, Ken—and a fellow graduate student, Jeff [Jeffrey R.] Kuhn, basically redid Dicke's oblateness experiment and showed that the sun was not as oblate as Dicke had thought it was. They redid it and redid it. Dicke thought that maybe the oblateness varied with the solar cycle, but it always stayed very low.

COHEN: But he wasn't willing to let go of his idea?

GOLDREICH: He had a hard time letting go of it. Dicke was a brilliant experimenter who made a mistake. He published something that was incorrect. He spent, I would say, the last twenty years of his career—a long time—coming to grips with that fact. Maybe the first ten years of that period were spent redoing the analysis of the data—trying to get more out of it to show that he was right. He spent the next ten years redoing the experiment—having his students redo the experiment. He found out that he had really wasted the previous ten years. He was a gentleman throughout. It was something his mind just couldn't accept. It's a lesson. Even the best people can make mistakes. Anyway, that was my involvement with helioseismology. I also worked out a theory, mostly with Norm Murray but also with Pawan Kumar, for the changes in the frequencies of the oscillations of the sun during the solar cycle and how the magnetic field, which waxes and wanes near the surface of the sun, affects the frequencies of the oscillations and what you can deduce from the changes in these frequencies about the changes in the surface magnetic field of the sun. That was also a very interesting study, but it remains to be confirmed or refuted by better data. OK, so we're in the nineties.

COHEN: That's right. Here we are, although we're almost done with the nineties. I'm very impressed with you, Peter, to go on this way without a note.

GOLDREICH: Oh, well, it's my life. I remember different parts of my life by what I was working on.

COHEN: I see. OK. Not by the ages of your children.

GOLDREICH: No, not by the ages of my children, because my children were there continuously, whereas these subjects changed. I spent plenty of time with my children. I don't feel they were shortchanged for time. They may have been shortchanged in some ways, but not for time. I used to take Danny out of school, essentially every afternoon, to play tennis with him. And even when he played on the high school tennis team, I would get permission from the coach to play with him, because I was better than the other players on the team. And I used to play basketball with Eric. Soccer, too.

COHEN: You're still doing it. **[Tape ends]**

PETER GOLDREICH

SESSION 4A

November 11, 1998

Begin Tape 5, Side 1

COHEN: OK. It's November 11. Perhaps we can talk about the work that you've done in the nineties.

GOLDREICH: I've basically worked on three separate projects during the last several years. And they're all continuing. The first one was a study of the twinkling of small angular diameter radio sources, mainly pulsars.

COHEN: Twinkling scintillation?

GOLDREICH: Yes, scintillation. You know about that. [Laughter] The scintillation of pulsars was discovered when pulsars were, but it had already been known from other sources. It occurs as radio waves pass through the solar wind. But this new discovery referred to scintillation in the interstellar medium. So it gave us information about the plasma density fluctuations in the interstellar medium.

COHEN: This was experimental data that had come in?

GOLDREICH: Yes. The discovery of pulsars was in '67 or '68. Within a year, there was a very nice paper by Peter Scheuer outlining the main properties of the optics of scintillation as applied to the interstellar medium.[13] Then there were lots of more technical papers, and there still are on the optics. But there was very little done on trying to understand the underlying dynamics that gave rise to these electron density fluctuations. I remember talking, when I was at the IAS in Princeton in 1972, with Russell Kulsrud about this and getting an initial education on plasma processes that cause density fluctuations and being interested in them. And then, during the second half of the eighties, we had a couple of very good postdocs at Caltech—Jeremy Goodman and Ramesh Narayan. They are now professors at Princeton and Harvard. They worked with

Roger Blandford on optics. I kept thinking, "Gee, we should really work on the dynamics." But I never knew quite how to get started.

COHEN: I'm not clear about the distinction between optics and dynamics.

GOLDREICH: Well, I guess I should say first why we think dynamics is involved. The particular spectrum of electron density fluctuation—or the magnitude of the difference in electron density between points separated by different distances—suggests that the electron density fluctuations are associated with some form of turbulence. Although we can't measure the velocity field of this turbulence, we can make a pretty good supposition that it's present. In the case of the earth's atmosphere, the scintillations are optical starlight. We can make *in situ* measurements and connect the scintillations with the spectrum of hydrodynamic turbulence. We can do this sort of stuff in laboratory experiments, too. But the interstellar medium is a magnetized, electrically conducting fluid. There isn't much work done on what turbulence would be like in that kind of medium. I was very lucky in the early nineties to have a postdoc from India named Sridhar. I told him about this problem, and I explained what I understood of the optics to him, which was more or less what Scheuer had put out in '68. The more sophisticated stuff that Roger and Jeremy and Ramesh did, I only vaguely knew about. And this really interested Sridhar, who is of a mathematical bent, so we would discuss it. He went to bed very late. His wife was at Princeton as a postdoc, and he used to go to bed just about an hour after I woke up and came into the office. Every morning between 6:30 and 7:30, we would talk about interstellar turbulence. He eventually came up with a really clever idea about how to approach the dynamics of disturbances in a conducting fluid, based on some earlier work that Robert Kraichnan had done, together with some quite different insights. He worked that out; I just advised him. In the end, we wrote a paper on which he was first author.[14] We wrote this up, even though the spectrum of disturbances it predicted was different from what was seen in space and we knew it couldn't be right. Also, we had treated the turbulences as composed of weakly interacting waves. Then for the next several months, we would meet and discuss this and how we could extend it to strongly interacting disturbances. One day I realized that there was a simple scaling argument, and when I described it to Sridhar, he got really excited. After about a year of messing around, we wrote it up.[15] I think this spectrum is probably the correct explanation. We devised a spectrum that matches the interstellar spectrum very well. Oddly enough, we still don't have a very good connection between this dynamical spectrum and the electron density fluctuation spectrum. It's something that has stymied us for years since, although just recently I think I've understood a crucial part of that, so that's made me feel much better. In fact, while I was ill this last week I did that. [Laughter] Just when I was getting better, I was able to think about it. I didn't have anybody bothering me. I tend to keep coming back to these phenomenological puzzles.

COHEN: You mean they're in back there and they're disturbing you?

GOLDREICH: I just cycle through year after year. I never let a year go by without at least trying to get some more done on them. Usually all I do is get back to scratch.

COHEN: Well, when do you do your thinking? I mean, this time you did your thinking when you were sick in bed.

GOLDREICH: I do my thinking any time I get a chance—committee meetings, if I wake up in the middle of the night, sleeping in a tent. Sometimes I even try to do it while I'm hiking, although that turns out to be inefficient.

COHEN: You have to pay some attention to your feet.

GOLDREICH: I can even do it while I'm driving.

COHEN: When you're detached from other things.

GOLDREICH: Yes. And then I do it in my chair—I can't part with my chair—while I'm writing letters of recommendation, reviewing papers, or talking on the phone.

COHEN: So you have this backlog filed in your head that you think of when you have the time.

GOLDREICH: I have a large backlog of things. Anyway, I'm very happy with this, because this is fairly technical stuff, in a way. And I think it's also true, to an extent, that if you do anything fundamental with astronomy, it's going to last. It's not going to be improved upon for a long time. I have a graduate student named Jason Maron who's doing simulations of this turbulence on one of Caltech's large multiprocessor machines, and the simulations look quite like the analytic scaling suggested they should. This part I'm happy with. Another project I work on is trying to understand the oscillations of white dwarf stars—different classes of white dwarfs are defined according to the composition of their surfaces. They pass through temperature ranges as they cool, within which they pulsate. These pulsations can be detected, and we can learn something about the structure of the stars: how fast they rotate; how massive they are; how thick their hydrogen layer is, the helium layer, and so on. The most common class is the one that has hydrogen on its surface. As far as we can tell, all hydrogen-rich white dwarfs pulsate when they cool to temperatures between 12,500 and 11,500 Kelvin at the surface. Over this 1,000-degree instability strip, the pulsation characteristics change tremendously. The periods of pulsations increase by about a factor of 10. Amplitudes go way up. The pulsations become much more time-variable. I had a new graduate student who came from China—Yanqin Wu—who I couldn't speak to very well at first but who had some mathematical and computational interests. And so I set her off on this, first to try to understand why the stars pulsate and then to understand why the pulsations change character so rapidly as they cool and, most important, to try to figure out why the pulsations never grow to very large amplitudes—why they always have rather modest amplitudes. To find out what stops the unstable oscillator from continuing to increase its amplitude. She worked on this with me for about five years and got her PhD. I thought she did really well.

It's funny, because I was very worried about her, because I didn't think she had very good sense some of the time. When she took her exam, I thought she was doing absolutely great, and she had a difficult committee—Judy [Judith Cohen], Sterl Phinney, and Tom [Thomas A.] Prince. Afterwards, when I came into her office to congratulate her, she was crying. I had never seen her cry—she's pretty tough. And she said, "I'm sorry. I did so badly." I said, "What do you mean? I thought you did great." She said, "You're just saying that to make me feel better." So I said, "Go ask Sterl," because I couldn't think of what else to say. Anyway, Sterl told me she did very well. Anyway, we're writing this up, particularly the amplitude-limiting process, which is called parametric instability, which is related to the way the pendula are excited in grandfather clocks.

COHEN: Where did this girl go?

GOLDREICH: She's a postdoc at the University of London, Queen Mary College. It's wonderful, because her boyfriend—now her husband—was a Dutch postdoc here, and he left a year before she did and went to Cambridge, so she desperately wanted to go to England. And we managed to arrange that through some grace of God. Chris [J. Christopher] Clemens, who was here as a postdoc, is an expert on the observational aspects of these objects and he, together with Marten van Kerkwijk, obtained some unique observations at the Keck telescope that provided us with further inspiration. It also advanced the field a lot.

COHEN: It's too bad Chris left. He felt badly about that.

GOLDREICH: I have a meeting with the optical astronomers today—what's left of them, because I'm head of the staffing committee. We just have to make some progress. But they're already pissing all over each other.

COHEN: [Laughter] Good to have this on tape. Go ahead.

GOLDREICH: A dysfunctional group.

And then the third topic I've been working on is with a graduate student named Eugene Chiang. Eugene was recommended to me by a colleague at MIT named Saul Rappaport, who wrote that this was a wonderful student and if possible, I should take him on. Eugene said he would come to Caltech only if I would take him as a student. And he was our best applicant. So I said that we could try it for a year and see how it went. It's going well. He's an extremely effective guy. He's very sensible. You just get him started and you don't have to keep guiding him back in the right direction. Eugene and I have done two papers on the structure and thermal properties of disks around young stars.[16] Anneila Sargent and Steve Beckwith have done a great deal of work on this subject observationally. We showed that these disks have very thin atmospheres in which the dust grains are quite a bit hotter than in their interiors. Many of the observational properties that were previously mysterious could be understood in terms of these superheated grains. The tops and bottoms of these disks are illuminated by starlight coming in at a slant angle. Previous workers had assumed that the surface grains behaved like cloud particles in planetary atmospheres—in their interactions with sunlight. The difference, in the case of the cloud particles in Earth's atmosphere in sunlight, is that molecular collisions in the atmosphere are so frequent that the cloud particles and the gas stay at the same temperature, whereas in these much less dense circumstellar disks the gas temperature and the particle temperature can be very different in the surface layers. So we had a very nice collaboration going on for a while.

COHEN: So that's where you are now?

GOLDREICH: Yes, that's where I am now. I have a new postdoc who seems to want to work with me and who is the sort of person I would like to work with. His specialty is gamma-ray bursts. So I think I'll probably go into that for a while.

COHEN: So you must have nothing but good to say about your position here. All these wonderful people come to you.

GOLDREICH: I have nothing but good to say about almost all the people who are around, actually. [Laughter] Including the ones who make trouble for everybody else. But it's too much work. It's not only from Caltech—it's some from Caltech, but.... For example, this week I've already gotten six requests for letters of recommendation. It takes a long time for me to write a letter of recommendation—I have to read something the candidate has done so I have something to say. I've got, in addition, a proposal to referee. All this stuff piled up while I was sick. It will probably take me a month to get through it.

COHEN: From four days of being sick, you have a month's work?

GOLDREICH: Yes, to catch up. I just can't go any faster and still do my work. The subject's gotten big enough, and I've worked on enough different things, that I can't respond to the outside pressures. You know, I'm pretty selfish, in that I don't do too much outside Caltech. I just can't handle all the pressure. Susan and I have talked about retiring.

COHEN: But you would still have that kind of thing.

GOLDREICH: I'd stop doing it. I'd say, "I've retired."

COHEN: So you would still do your work—what you consider your work?

GOLDREICH: Yes, but I wouldn't respond to all these outside people. That's why you retire. There would be less around here, too.

COHEN: You would still teach, of course? That you may like to do.

GOLDREICH: Take planetary science. We're having a planetary science curriculum meeting tomorrow. Now, I'm running the astronomy staffing committee for the second year, and that's an endless procedure, in which I have to listen to my colleagues attack each other—and they attack each other behind each other's backs but directly to me. Then Friday we're having another astronomy meeting, and then we have the PMA [Physics, Mathematics & Astronomy] division meeting and the physics staffing committee. It gets to be a little much. And there's a geology division meeting. There are an awful lot of meetings when you're in two divisions and you're also teaching physics and astronomy.

COHEN: There was a period of time, I remember, Peter, when you said you went away every year for two or three months.

GOLDREICH: Yes, that was very useful. I realized, around 1984 or 1985, that I hadn't gone anywhere during the summer or during the year, except for three months I took off in 1968. I went to Australia when the kids were little. And Susan has a job. So I asked if I could take a series of sabbaticals for two-month intervals. The administration said, "Fine." So I started doing that. And mostly somebody paid for me anyway, so I didn't even have to ask the institute for money. At first I felt pretty lonely. I wasn't used to being away from home.

COHEN: Well, you went to Boston. You went to Cambridge—Harvard.

GOLDREICH: I went mostly to the IAS, to Princeton. They provide an apartment. You just rip open the plastic bag and put the sheets on the bed—and the blankets. The first year, I made the mistake of not having any transportation. It's a long walk to get food if you haven't prepared ahead, or it's raining. I went to Princeton for two months the first time. Then I went to Toronto, to CITA, because Scott Tremaine had gone there as the director. I spent two months with Scott. I went to ITP [the Kavli Institute for Theoretical Physics] in Santa Barbara, to the University of Colorado, to Paris. I was at Harvard twice for two months.

COHEN: So you did this for quite a few years.

GOLDREICH: Yes. And then I went back to the IAS and spent another month in Toronto. But as my parents got older, I didn't feel right about it, so I stopped. For the last several years, I haven't gone anywhere.

COHEN: You didn't want to leave them for two months.

GOLDREICH: I used to take my parents for a few weeks. They would come and stay with me. Particularly at the IAS it was very convenient, because I'd get a big old place. They also came to Cambridge. Then I stopped. But my father has just died, and my mother is in no condition to travel, all as the result of an auto accident we had on April 12, 1998.

COHEN: You can start again?

GOLDREICH: Maybe. I've lost some of my desire. I'm not quite sure. It's hard to say. I get along better with Susan now, too. I'm not sure I would want to be away that long.

COHEN: Well, she used to join you for part of it.

GOLDREICH: Yes, Susan would come with me. But I'm not so sure whether I'm going to do that.

COHEN: That takes care of that period of time. OK, let me turn this off. **[Tape Ends]**

PETER GOLDREICH

SESSION 3

April 7, 1998

Begin Tape 3, Side 1

COHEN: Peter, I'm glad to see you this morning. I thought maybe we could go back a little bit to your nonscientific work at Caltech. We're very interested in the history of the LIGO [Laser Interferometer Gravitational-wave Observatory] project, of course. So could you tell us something about that and when your involvement started?

GOLDREICH: Well, of course everybody in the physics division became aware of LIGO when Kip Thorne proposed—I guess it must have been in the late seventies—that we have an experimental project started here to discover gravity waves. And the search was made for the appropriate person to be the director of the experimental program. There were really, I guess, three major candidates. There was [Vladimir] Braginsky in Moscow, who ran a group but whose work was mainly concerned with small-scale experiments. And then there was Rai [Rainer] Weiss at MIT, who had written a feasibility study on using laser interferometers for detecting gravity waves. And there was Ron [Ronald W. P.] Drever, who was running an experimental program in Glasgow. I wasn't on the committee to recommend these decisions, but a proposal came forth from the faculty, including me, to hire Ron Drever.

COHEN: You don't know anything about the discussion involving Rai Weiss also at the same time?

GOLDREICH: I know a little bit about the discussion involving Rai Weiss. He had a reputation for being very bright, but not being terribly productive. And Ron had a reputation for actually getting more done, in terms of building an interferometer that actually worked, even though it wasn't detecting gravity waves. Also, Ron had had experience both with bar detectors and with laser interferometers. It wasn't clear, when we got started, which was the more promising technology. The idea, starting with Joe Weber, of using resonant bar detectors had been around for a long time and was more advanced. In any case, I do remember the discussion among the faculty when we voted to offer Ron a job. Some of the letters from his colleagues in England praised his experimental skills. Then when he showed up, it was only as a part-time faculty member.

COHEN: There was a very special arrangement there.

GOLDREICH: He had a five-year, half-time appointment. He would go back and forth between here and Scotland. While he was here, he gave several inspiring colloquia. It was clear to me that he was a funny guy from the few times I met him.

COHEN: Funny in what sense?

GOLDREICH: Well, he was totally unworldly and totally involved with physics and very intuitive. You could tell, even from his talks, that he was not particularly technical or mathematical. He was a holistic type of scientist who was interested in how everything worked—mechanical things, electrical things. Quite different from the more specialized people who tend to develop these days. I remember also that we eventually pressed him to make a decision, and he only decided at the last minute to come here. Caltech provided quite a good startup for him in one of the engineering buildings, close to where they store the automobiles and other things. He set up this laser interferometer. I always had my doubts about this project. It seemed really far out to me. And I never thought of Kip as the most practical person, from my own scientific interactions with him. But it was certainly interesting stuff.

COHEN: So you thought that experimentally it was way out, in some ways?

GOLDREICH: Yes. I didn't know very much about it, either. But I remember we would occasionally discuss it in the physics department meetings, because there were various difficulties that came up. There were attempts to have a forced marriage between the MIT group and the Caltech group. Kip was trying to deal with Ron Drever and Rai Weiss. He was the head of this triumvirate and also its main publicist. The three of them put together a proposal for a few tens of millions of dollars—$35 million—to build a larger interferometer. Now it turns out that their estimate of cost was about a factor of 10 too low. So maybe it was true—the impracticality of the whole thing.

COHEN: Of the planners, you mean?

GOLDREICH: Yes, all of this was becoming apparent. But still, I didn't have much to do with it. The first time I really had a significant amount to do with it was after Robbie Vogt was fired as provost [1987]. I had been very friendly with both Robbie and Murph, and I had had a lot to do with both of them during the time that Robbie was provost. They had had a very rocky relationship. Robbie had become convinced that Murph was out to get him, so he did everything he could to get Murph. Well, I never saw any evidence that Murph was out to get him, and in the end Murph, in his last gasp, fired Robbie. And I also had something, unfortunately, to do with that. Because both of them told the then-chair of the Board of Trustees that if he wanted to know what was going on, he should talk to me. So he did.

COHEN: Oh, do you want to say something about that?

GOLDREICH: Well, that's a little awkward. I did talk to Rube Mettler in the boardroom.

COHEN: Rube asked you to come in?

GOLDREICH: Yes. And I told him what I knew—that Robbie was a tremendously skilled and visionary administrator but that he was paranoid, and that Murph was not a skilled administrator but that he was good in dealing with people, but he couldn't deal with Robbie. And Rube talked to Robbie, and Robbie promised to behave. But then he couldn't control himself. Robbie tended to blame Murph for things that happened in his life—even outside of work—that he wasn't proud of. He would somehow lay off the blame on Murph—sometimes in really incredible ways.

COHEN: Like what, Peter?

GOLDREICH: Some of it I'd rather not talk about—but improper behavior on his part. He would somehow blame Murph for it, and sometimes even publicly. So, while I felt sorry for Robbie when he was fired, I thought it was quite justified. I knew it was coming. And I had been mourning his life, because I knew Robbie had somewhat suicidal tendencies. Anyway, after Robbie was fired as provost, Caltech was looking for something for him to do. The idea came—various people take credit for it; I know Tom [Thomas A.] Tombrello does—to make Robbie head of LIGO. And I, unfortunately, had something to do with that, too. Although it certainly was not my idea, I was sympathetic to Robbie. And I talked to Ron and told him that Robbie would be an inspirational leader and a good organizer. Ron was very nervous about giving up any control, but in the end he did. I also talked to Robbie about taking the job. He was very hurt that we hadn't all resigned en masse when he was fired. He was living down in a room next to the men's john in the basement of the physics building, sulking for a long time, and refusing to accept this position. And one of his demands was that his salary remain at the provostian level, and also that it not be allowed to suffer from inflation and that he get regular raises. And I had long discussions with the then-provost, Barclay Kamb, about this. Barclay eventually caved in and agreed to us, which was clearly a mistake, but who knew at the time? Anyway, Robbie became head of LIGO and organized things better, as far as I could tell. But the project also became almost classified.

COHEN: What year are we talking about now, Peter?

GOLDREICH: It was right at the end of Murph's tenure—probably the spring of '87. Anyway, once Robbie agreed to do this, the project really became a secret project. Robbie took Ron's group, which was not so happy with Ron, I gather, from what I found out later.

COHEN: How big was the group at this time?

GOLDREICH: I don't know. There were a half-dozen postdocs maybe—something like that. Robbie organized it and built up a good spirit, partly, I'm afraid, by telling them that the whole world was against them and that everybody was their enemy and that they were in a state of siege. And we heard very little about LIGO after that. But they produced a very well-written proposal, and within a year or two they were funded by the NSF at a level of about $230 million. I can't remember exactly when the funding came through—maybe '89 or '88. It was a big surprise. All of a sudden we had this big project with a large amount of money promised. And then the fact that nobody knew what was going on, and also that a number of us felt this might not be possible to do scientifically, made it more serious. But when we would try to discuss it in the physics division, we could hardly ever get any information about it. Robbie didn't come to too many of the physics meetings. He was still annoyed at his colleagues and hurt because we hadn't stood up for him more forcefully. Kip was just happy that the project was going along. And often what I saw then, when I would be over in the theoretical astrophysics wing of Bridge [Norman Bridge Laboratory of Physics], was Robbie yelling at Ron Drever and browbeating him.

COHEN: But that was his manner, and you didn't think anything of it?

GOLDREICH: Well, Robbie never did that to me. He never yelled at me. I would have killed him if he had yelled at me. And I remember Ron telling me one time, "This is awful. This is awful." Robbie would be yelling at him. And I'd say, "Why don't you just walk away if he starts yelling at you?" Ron said, "Could I do that?" And I said, "Of course you can do that. You are a professor here." And Ron said, "I'll have to ask Gerry [Neugebauer]." Gerry was, by now, the division chairman. I couldn't believe that Ron was so naïve. But still, I didn't have very much to do with the project until May or June of '92, when Ron came to see me. He told me that he thought he was going to be thrown out of the project and that he had been forbidden by Robbie and the provost to go to some scientific meetings, and that he had no funding—no independent funding. Not only could he not present papers on LIGO, he shouldn't even go to the meetings. So I called in Kip, and I told Kip that this was unacceptable—that no professor should be treated this way and that his academic freedom was violated and that I felt terrible that I had been part of talking Ron into accepting Robbie and that this was a case for the Academic Freedom and Tenure committee. And I thought Kip behaved very badly.

COHEN: Did Kip acknowledge that that was the case?

GOLDREICH: Oh, yes. He acknowledged that that was the case—but that he didn't have anything to do with it and that I had to understand that the people in LIGO were behind Robbie and not Ron, and that they couldn't let Ron go to these meetings because he was expressing doubts about whether the project would succeed. Ron's reservations were well founded, in my opinion, then and now. And he would present alternate possibilities for detectors when they had already settled on one. And while Kip said it was true that all the big ideas had come from Ron, the project had gotten too complicated, and Ron couldn't understand things anymore. So I actually talked to one person inside LIGO who described some of what was going on, and he also confirmed that Ron was on the verge of being thrown out. But he thought Ron could still do a lot, even though he was in many ways a nuisance. So I went to see the division chairman and complain to him. I received a sour reception from Gerry Neugebauer, which didn't surprise me too much, because that had happened to me before—although basically Gerry and I have always gotten along well. And then Ron told me that Gerry had told him he should go to a meeting that was coming up in Argentina if he wanted to. So Ron went. When he came back, they threw him out of the project. Gerry called me into the office and told me that Ron was no longer a part of LIGO. I said, "What did you do that for?" He didn't say he did it, but that that was just the way it was. And then, within a day or two, I got a memo that Robbie had written to the LIGO community, saying that as of whatever day it was, 1992—June 6, or something [date was July 6--ed.]—Ron Drever was no longer a part of LIGO and that he would be permitted to remove his effects from the LIGO office, provided such acts were witnessed by a member of the LIGO staff. This really annoyed me. So I went and made an appointment with the provost—Paul Jennings, by that time. And I told Gerry, and Gerry came along. This must have been two days after this happened. And I said to Jennings, "This is unacceptable. We can't have this at Caltech." I remember Gerry telling the provost that if he had written a memo like this as director of Palomar—because Gerry was director of Palomar—that the provost should have fired him. On the other hand, I don't know whether Gerry was just saying that for effect or not.

COHEN: This letter was written by Robbie?

GOLDREICH: Yes. It was sent out to all LIGO people. I had gotten hold of it from somebody inside LIGO—somebody who was not such a fan of Robbie. Then I talked to Jeff Kimble, and

Wal, and Maarten Schmidt. Maarten said that he and Wal were upset about this also. I can't remember whether it was Maarten and Wal or whether it was the three of us who sent a letter to [President] Everhart complaining about what had happened and saying he should pay attention to it. A reply was received, which was interesting, because later Everhart denied that he was around when any of this happened—a straightforward lie. I thought the administration as a whole behaved terribly throughout this whole business.

COHEN: When you went in to see Jennings, did he support this action?

GOLDREICH: Oh yes. I went to see Jennings several more times, once with Ron. And once Jeff went to see him with Ron. And when I went to see him by myself, he told me that he couldn't believe that Ron had ever done any science, but that he respected my opinion. So I said, "Why don't you read the letters when we hired him? He was considered the best experimenter in England." He was a much better scientist—and still is, in my opinion—than most of the Caltech physics faculty. Clearly what had happened was that Jennings was in awe of Robbie. He had been Engineering and Applied Science division chairman when Robbie was provost. Robbie must have been telling Paul for years how worthless Ron was, and Paul had just swallowed the whole thing. He decided that Ron was just absolutely a piece of shit and that they could tell him anything. And in fact Ron is so unworldly that you *can* pretty much tell him anything and he accepts it. So that really horrified me: The accomplishments of somebody who Kip had always told me had all the main, big ideas—the experimental setup, the whole thing—being treated like he'd never done anything. And this whole project was based on Ron's ideas. So I told Jennings that this was a case for the academic freedom committee. He tried to keep it in the division. He said, "Well, we have division grievance procedures." I said, "Yes, but the grievance is against you and the division chairman as well as Robbie." And he was very upset by this. He didn't want to hear anything about that. But I said there was no way it was going to go through the PMA division. Meanwhile, I collected some letters from people who knew Ron. Carlton Caves had been a student of Kip's—he's now at the University of New Mexico. But he had been at USC [University of Southern California] and worked with Kip for years and done the sort of work that's relevant to some of the experimental aspects of LIGO. John Hall was a famous scientist at JILA, working with lasers. And I got letters from other people who had worked with

Ron. I sent them to Everhart. Nothing was ever acknowledged. Gerry Sussman, who was a professor at MIT and who had spent a year here and had observed LIGO, tried to have something to do with it. He was rebuffed. Gerry also sent a letter in support of Ron. Sometime at the end of July, I think, the president took off for vacation. About a month later, he acknowledged the internal letters but never responded to the external ones. And we spent the whole summer, basically, preparing a case for the Academic Freedom and Tenure committee.

COHEN: Now, that was you and Wal and Maarten Schmidt and Jeff Kimble?

GOLDREICH: Yes. Jeff and I interviewed people in LIGO and chased down various things. Robbie accused Ron of many things. He believed that Ron had badmouthed him to [Albrecht] Rüdiger, a prominent member of the German gravity-wave team.. I sent an e-mail message to Rüdiger, and he replied immediately and said, "This never happened." He said that he had criticized Robbie in a meeting, but that was because Robbie's behavior had annoyed him. Robbie had stated that LIGO was his project and that he was going to change the face of physics, while Ron and Rai Weiss, who had been the initiators of the science, were sitting in the audience, and Rüdiger couldn't take that and he had made a critical comment. Robbie blamed this on Ron. I was concerned, first, that I had talked Ron into accepting Robbie, and second, that once Robbie had taken over, Ron had been deprived of funds and had been forbidden to go to meetings—not just forbidden to present papers but actually to go to the meetings.

COHEN: Now, was this Robbie's behavior with anybody, or was that specific to Ron?

GOLDREICH: Well, Ron was the only faculty member in the project other than Robbie. But it was very clear from the pattern of behavior that Robbie, with Kip's approval, was trying to make Ron disappear. They were trying to basically make Ron a nonperson. They didn't want his pictures in the paper. Whenever there was a group picture, they would try to stick him in the corner. They didn't want him to go to meetings. They didn't want him to be seen as a part of the project. They thought that they could go on without him. Enough of the science was in hand, or the people they already had could do all that needed to be done. They could go on without him. And Kip even told me that, as early as three years before that, Gerry had said, when Robbie was complaining about Ron, “Why don’t you just get rid of him?” And Robbie had said, “Not yet.” So this was a long time in coming. I also had a long talk, one or two days after Ron was fired from the project, with Robbie—about a four-hour talk in which I told Robbie in no uncertain terms.... Well, first Robbie called Ron a windbag, a liar, a crook. I had already heard him make similar remarks about other people, like Bruce Murray when Robbie was chief scientist at JPL and Bruce was the director; about Murph when Robbie was provost; about Jack Roberts when Jack Roberts was provost and Robbie was division chairman. This is how Robbie gets.

COHEN: Some people have trouble with authority.

GOLDREICH: But I had never seen him do this with somebody beneath him. On the other hand, in some ways Ron was way above him, because Ron had had all the ideas. And I think what Robbie resented more than anything else was that he, Robbie, this great engine of progress, was managing the project and this sad-sack little round man, Ron, was going to get the credit—and maybe even a Nobel Prize. And of course, then nobody would appreciate Robbie—he always had a problem with things like that. That’s why he didn’t like Maarten Schmidt as division chairman. He thought everybody felt that Maarten was such a good guy and that they despised Robbie. I didn’t really understand that, because we all admired Robbie. So I told Robbie at the end that he ought to resign, because he was going to be fired eventually. He would never win. He couldn’t survive this type of thing. In the end, he would be fired. It took a lot longer than I thought it would, probably because of the lack of courage on the part of the Caltech president. But in the end, it happened.

COHEN: They were finally pushed by NSF, weren’t they?

GOLDREICH: Well, it was a combination of things. Jeff and I decided the best thing to do was to advocate a supervisory committee for LIGO. We wrote a letter to Everhart saying that with a project of this size, with this exposure and uncertainty, it was unthinkable that it be run in secrecy and that there should be an oversight committee set up. We figured that ultimately that would sort things out. It did, more or less, as far as Robbie was concerned.

COHEN: So there was a committee appointed?

GOLDREICH: The committee was appointed, yes, even with the person we suggested as the head of it—[former JPL director] Lew Allen, who turned out to be less effective than I had expected. But nevertheless, ultimately that was the end of Robbie. He fought with the committee; it was intolerable. He got bogged down trying to defend his actions, and the project didn't move along. It didn't help Ron that much, though. And then the case was taken to the Academic Freedom and Tenure committee. Robbie filed a grievance against me, but it was thrown out. But Ron's wasn't. The committee found that the provost and Robbie were guilty of violating Ron's academic freedom. But they only advocated that Ron be set up as an independent researcher, not that he be put back in the project—which I thought was a mistake, because then the administration just started dilly-dallying and making excuses. We had an institute-wide faculty meeting that fall. When I asked President Everhart what had happened, he said that while he was out of town, the director separated the principal from the project. Of course, he had not been out of town. He said that he didn't really know what was going on—that when he came back, the campus was in turmoil, and so on and so forth. I remember, at the end of this, getting up for the third time and saying, "At the risk of repeating myself for the third time, I'd like to know what you're planning to do about this violation of academic freedom." And he sort of fluffed around. This was the night the institute had a party for Rudy [Rudolph] Marcus in honor of his winning the [1992] Nobel Prize [in chemistry]. After that, it was just very unpleasant. There was a committee set up by Neugebauer with [Barry] Barish and [Roger] Blandford and Tom [Thomas G.] Phillips. They were supposed to look at what had happened. Their report recommended all sorts of things that turned out to be impractical. They explicitly avoided the issues of academic freedom. They talked more about the project—like they were some sort of project organization, which maybe is how they think, but it's not how I think. My opinion is that the whole project isn't worth what happened, even if it succeeds, which I doubt it will. It was a difficult time for me, because I tended to judge my colleagues by what side of this issue they were on. Most of them just didn't pay attention to it. We had a physics meeting that winter to which the president and provost came. I don't remember exactly what the point of it was—perhaps to tell us that we should all be good boys. And I told them they were incompetent, that this was an example of gross incompetence on the part of the administration. Some of my colleagues congratulated me;

others didn't say anything. By that time, we were in a trench warfare mode, where you needed to be organized and systematic and so on. My role in it was diminishing, and Maarten was becoming the head figure, because he had the patience to write memos and keep records, whereas I was just good for getting up and shooting off my mouth in public, which I did a few more times. Once in an institute faculty meeting I got up and said that there had been no apology to Ron for what had happened—until the president did come out with the stingiest apology I had ever seen—and that while the administration didn't have to agree with what the Academic Freedom and Tenure committee said, they should at least have the decency to resign if they didn't agree with it, because the committee was elected and they were just appointed. People just stood there and took notes. It didn't really have much of an effect. We started a petition to have a special faculty meeting to discuss this, and we quickly got the number of signatures that are needed to call one. Then the president called me up when I was in the shower one night and said that he'd agreed to go to arbitration on this. I went to his office and agreed to call off the faculty meeting. Wal and Maarten were furious at me. They said we were going to be double-crossed, and we were. There was no arbitration. There was some sort of kangaroo court that Everhart fixed up. I hardly blamed him. He was a weak president. And it was a real mess. His division chairman and provost were central players in it, and he had to deal with Robbie, whom he couldn't handle. There was just nothing you could do. In the end, we settled for getting Ron some money. Even that didn't come out quite the way it was supposed to, but Maarten negotiated a written agreement in which Ron was promised a couple million bucks for his research. So that was the end. In my view, it was a very sad example of academic behavior. The institute was very ambitious about this project and didn't understand how risky it was. It probably still doesn't understand completely. Since Barry [Barish] has taken over [LIGO], I think the big engineering part has been going very well—the construction of the vacuum systems, and so on. As far as I can tell, there has not been much real progress on the detector development. They'll come up with some sort of detector that probably won't detect anything.

COHEN: Is most of that original group still working on it?

GOLDREICH: No. Essentially all of the people who worked first with Ron and then with Robbie have left. They mostly left after Robbie was fired. And it's interesting, because when Ron was thrown out, his main complaint was that even though the management of the project was going better—that the design was going better—the science was going too slowly and they wouldn't be ready with a good detector. This was his criticism of Robbie. And when Robbie was fired and then became in charge of the detector development, that was his criticism of the project—that the science wasn't going fast enough and that all the attention was being paid to engineering. When Robbie separated himself from the project, the original scientists left—the postdocs who had by then become staff scientists. Many of them went to JPL to work on an optical space interferometer. They all complained that there wasn't enough attention being paid to the science of LIGO. But I don't know very much about it. Things are much more open now, of course. The claims that are being made have been muted. And even history is being rewritten about what had been promised. Now if LIGO 1 is delivered on time and on cost and meets the initial goal of sensitivity, I think it would be considered a success, even if it doesn't detect anything. Initially, much more was promised. I actually hope it works, because it would be terrible for Caltech if it doesn't. But I have my doubts.

COHEN: Do you think that Caltech never should have started this project?

GOLDREICH: No, I think they should have paid more attention to it. And they should have had a more sober look at it. Gravity-wave astronomy is not an established field. The experts are experts in only one or two pieces of the technology. You'll get laser experts. You'll get materials experts. And they all think it's wonderful, because it's pushing their own technologies to their limits. But when you make an overall assessment of how likely it is to work, I think it doesn't look very good—even to some guy who is not terribly knowledgeable about technical things, like me.

COHEN: Now, tell me something. I see him [Ron Drever] here, and he is set up in his lab. Is he doing anything that you know of?

GOLDREICH: Yes, Ron is working on new technologies for advanced detectors. It's probably a good use of his talents. Of course, he's being kept out of everything except the open scientific meetings of the project. I think that's mainly because of Stan [Stanley E.] Whitcomb these days.

I think everybody else who resents Ron is gone, but not Stan. Stan doesn't want Ron around, so Ron isn't allowed in. He can just come to the science part.

COHEN: Yes, I know. He's very unhappy about that. I have talked to him about that.

GOLDREICH: Ron will be mostly written out of the history of this project if it succeeds. If it doesn't succeed, maybe he'll be lucky. [Laughter] My view is that the project was ill-conceived. And that's not Robbie's fault, because it was before his time, although he succeeded in getting it funded. It was ill-conceived by the three principals—Ron, Rai Weiss, and Kip. They were too ambitious. They went for much too much. They should have done much more research on smaller-scale detector development before they proposed such a large facility. The argument for jumping to such a large facility was mainly that the country was running out of money for big science projects. It was now or never.

COHEN: I thought that was something that the NSF pushed on them.

GOLDREICH: Well, some people in the NSF felt that this was a very good thing for the NSF to do—that it was really a bold new thing. If you are an administrator at the NSF, mostly pushing paper, and you have a chance to promote something that's significant in your field, it makes you feel like you've done something. So, yes, the NSF was enthusiastic. But I think they were also sold a bill of goods about how likely it was that what was promised could be produced. But we won't know until—

COHEN: Until it goes. Well, they're certainly building it.

GOLDREICH: But anyway, for me that wasn't the main issue. For me the main issue was the violation of Ron's academic freedom—and even more, my own role of getting him into this situation by urging him to accept Robbie, when I knew from previous experience that Robbie was capable of developing irrational hatreds for people and was very effective in convincing other people that the objects of his scorn really deserved it. That's what really made me feel bad. The project—well, that's not my responsibility, any more than it is for any other physics professor. But what happened to Ron, I feel responsible for. And the fact that he was treated so badly—that it was acceptable that he have no funding.... He was even forbidden to apply for grants while he was part of LIGO on topics that were different from the LIGO topics. So he was really completely controlled financially.

COHEN: Now, Peter, don't you think this is sort of an aspect of Caltech, where people are involved so deeply in their own work that they can't find time to look at somebody else's problems? People are so pressured here, it seems to me.

GOLDREICH: Oh, yes. People are very pressured here. That's for sure. I've become more philosophical about what happened, but while it was happening I resented many of my colleagues. And I no longer have quite the same cozy feeling about Caltech that I once did.

COHEN: So it was bad for you?

GOLDREICH: Yes. It was bad for me.

COHEN: How about all the other players in this thing? Do you think they have just gone on?

GOLDREICH: Well, it's obviously been bad for Ron, because a large part of his life was taken away from him. It certainly didn't do Robbie any good, but probably Robbie would have careened around from one high to another low no matter what. He had a big opportunity here and he blew it, just as he blew the provost position where he had a big opportunity—although he accomplished things before that final explosion. That's just Robbie. He has a history of that. I think it probably soured Maarten on Caltech. It certainly soured Gerry. Gerry lost everything in the end, partly because of this, partly because he became more dictatorial and power-mongering, and eventually the optical astronomers didn't want him as director. But it obviously contributed to his resentment about the institution and about some of us, like me. Yes, I think it was a very bad thing. I don't admire Kip as a person at all. I even have a certain amount of difficulty with Roger [Blandford], who I felt was pussyfooting around this. And it carries over when we have to cooperate on things that are not easy to cooperate on—appointments that aren't perhaps as good as we would like to make, and things like that. **[Tape ends]**

Begin Tape 3, Side 2

COHEN: OK, Peter, let's continue with your involvement with the two divisions in which you have appointments—Geological & Planetary Sciences and Physics, Mathematics & Astronomy. Where do you consider your main commitment?

GOLDREICH: Well, it depends. When I first came to Caltech, intellectually I was split very evenly between the two divisions, because about half of my work was in planetary science. But in the last ten years, it's been much more in astrophysics. My teaching has been more in physics and astronomy always, although I've always taught a course—pretty much every year—in the geology division. And I'm doing that now. It's much more like a family in the geology division. It's much warmer.

COHEN: Do people like each other?

GOLDREICH: Well, some of the people don't like each other, but that's also part of a family dynamic. But we meet together, usually in the division chair's house. People know more about each other. It's smaller.

COHEN: Is that a legacy of Bob Sharp, maybe?

GOLDREICH: Partly, but I think it's also just a difference in the subject. For example, many of the people in geology and planetary science do most of their work on campus. And they do it with a few other people. They don't have enormous groups, so they interact with each other. I'm not that central to a large part of it. I've become less so as I've gotten older. But my main office is in GPS. So in terms of family, I'm more a part of the geology division. But in terms of what I understand intellectually, I'm more a part of PMA. I know more astronomy than most of the physicists, and more physics than most of the astronomers, and probably comparable amounts of math to some of the physicists. So I'm more at home intellectually in the physics division. And I've had many more students from that division. And most of the money I raise gets spent in that division. My postdocs and graduate students are there. I buy more computers there. [Laughter]

COHEN: But you've never had an office over there?

GOLDREICH: Well, part of the time I've had an office I can use in Bridge Annex. And I did use it. So it's split. A fair number of physicists do their main work outside of Caltech—all the high-energy experimentalists, for example. Also, many of the physicists consider their colleagues at other places as their principal intellectual companions, rather than their colleagues at Caltech. They teach at Caltech and they get their salaries from Caltech. But their involvement at Caltech is less than that of their colleagues in the geology division.

COHEN: That's the nature of high-energy physics, perhaps.

GOLDREICH: Not just high-energy physics but other parts of physics, too. But high-energy experimenters are a particular case.

COHEN: You know, there used to be geniality in physics—all that friendliness, with William Fowler and his group. Are you saying that that has disappeared?

GOLDREICH: Well, nuclear astrophysics doesn't really exist at Caltech anymore, except in a very minor way. The nuclear physicists are doing intermediate-energy physics, which is what high-energy physics was not too many years ago. So they are using facilities outside of Caltech. Also, the groups are bigger, and the amount of money required to do something significant is usually greater in physics than in geology. So people can be more concerned with their own enterprises and their own groups. They are working harder to make a living.

COHEN: To get the grants?

GOLDREICH: To be significant players in science—a narrower and narrower science. It's a very different feeling in the physics division, although it's still not bad. Mostly we do OK when we get together. There's also a difference now in the way that divisions are being run. In geology we have very strong leadership with Ed [Edward] Stolper, who has a very clear focus on what's important. Whereas in physics, we have a very hard-working division chair, Charlie Peck, who has a much bigger job than the geology division chairman but has much less focus and enjoys the job less and treats many items as though they have equal importance, where only a few of them really matter. So in my opinion we're not doing as well. We also have a lot less success using peer pressure to get people to retire in physics than we do in geology. So in geology we have lots of appointments. It's partly the age distribution, but it's also partly that most of the geologists realize that it's their duty to retire and make room for new people. In physics, that sense of community is less. As a consequence we have very few appointments.

COHEN: Now when you say physics, are you talking about astronomy also, or is that separate again?

GOLDREICH: I'm talking mostly about physics, which includes some astronomers. Astronomy is sort of special, and it's becoming more isolated from the rest of physics. The astronomers have their own problems. They have a big empire to run now—a bit like how the British Empire was. They are not really capable of running it. There are certainly not enough people who are capable.

COHEN: You're talking about the Keck telescope?

GOLDREICH: The Keck telescope, the Owens Valley Radio Observatory, the CSO [Caltech Submillimeter Observatory]—there's just too much. And also there are the space missions. There's too much for the number of people. The astronomers also try to separate themselves from physics, because they don't want to teach physics courses. Astronomy is a pretty small business within the physics division as a whole, but it's the most successful part of it. Physics has slipped a lot. Our physics faculty has slipped a lot. If you look at the number who were in the National Academy of Sciences or who are likely to get into it, it's rather small compared to what it once was. When you look at the number of people in astronomy who are getting this type of distinction, it's much larger. It's a bit easier in astronomy. And we have more advantage. And when we make appointments, we can get people who are closer to the top of their profession than we can in physics, where we're just now one of the top handful of schools, but not the top one or two.

COHEN: But that's not true in astronomy? You think we're still at the top?

GOLDREICH: Well, we're at the top partly because of the people but more so because of the facilities. Also the competition is easier. There aren't as many talented people who have gone into that subject. So I say we haven't been doing very well in physics. Although recently, the staffing committee has been energized by the leadership of Tombrello, who takes it as a personal burden and I think does pretty well at it. He gives it a lot of energy and attention.

COHEN: He's head of the staffing committee?

GOLDREICH: Yes, he has been for a few years now. It's a major activity for him. He does it quite well. So maybe we'll do a little bit better. But right now we don't have appointments, and that's serious, because we have opportunities and we can't fill them. You tend to become very conservative and even stagnate. If you can't do something after a lot of effort goes into identifying people—if you can't even make an appointment—it's not good. And in geology it's very different. We have lots of appointments.

COHEN: You have people coming? Young people?

GOLDREICH: Yes, so we're doing a lot better.

COHEN: Well, people are nicer to each other.

GOLDREICH: Mostly. Well, as I said, nobody likes everybody else. But it's more like a family. But again, it's easier. There's less competition. The fields change more slowly. There's less startup cost of moving to a new area. In parts of physics, to change from one thing to another requires a tremendous investment in time, which is very hard to come by if you are already busy. It also requires a lot of money.

COHEN: Well, moving yourself a little bit further out of this—because you've been so involved in things at the institute—how about biology and chemistry coming onto center stage?

GOLDREICH: Well, I don't know that much about biology. My impression is that we're good in biology but not where we should be—much less than MIT, for example. Of course, we're also much smaller. In neurobiology, we are one of the few players, but in molecular biology we're one of quite a few players. And in nothing are we really absolutely tops. We're doing better in chemistry, where we are close to the top. The two things that we're considered the top in are earth sciences and astronomy, which are the two things that I'm part of. [Laughter]

COHEN: It so happens. [Laughter]

GOLDREICH: I don't think I had too much to do with that.

COHEN: Well, the fact that you're willing to be here.

GOLDREICH: But I think in chemistry we're quite close to the top. In little bits of engineering we're very good, but overall I don't think we're particularly special. I think there's a lot of room in the institution for pruning. With appropriate pruning, there would be many positions available for growth areas. But it's very difficult to prune. I hope that our current president [David Baltimore] will recognize the opportunities and start doing some pruning. Because some of the appointments that are made seem to be really only made to keep fields that should be let go continue. We should just let them go.

COHEN: Can you give me a for-instance?

GOLDREICH: Well, it's a little bit awkward, but I think parts of applied math are like that. Once it was great, but I think now—

COHEN: Well, those people are all ready to retire or have retired.

GOLDREICH: But they're hiring new people—new computational people, new people of various sorts. And I think there are better people doing computational astrophysics. They would be better off getting rid of some of the classical parts of engineering. In physics, we've probably carried nuclear physics too long.

COHEN: Just let it go? But there's such a history of it here. That's difficult.

GOLDREICH: Yes, well there's always that problem. I think also that in the earth sciences Caltech's going to have to make a choice. I think a big theme in the next couple of decades is going to be a search for planets, and ultimately life, around other stars. That's not going to be done in astronomy here. Or at least it won't be unless there is a tremendous pressure put on astronomy from above. It's a natural extension of planetary science, but it's certainly way outside what we do now, and also what the GPS division as a whole thinks of as its general mission. But somehow within Caltech we have to find a place to do it.

COHEN: Now are you talking about things like SETI [Search for Extraterrestrial Intelligence]? I mean, looking for life in the universe?

GOLDREICH: Well, less that. I would say sort of the things that NASA is supporting. It's the type of space interferometry that they're supporting at JPL, and the preliminary stuff that they're supporting at Keck. These are very big opportunities, and they involve facilities that we control. It's a perfect opportunity for us, and we have to find a home for it. At present we only have one faculty member who's interested in it—Shri Kulkarni. He thinks he can do all of it, but he can't. We should have a real presence in that, because it's something we have an advantage in that's going to be a very interesting, long-term intellectual journey.

COHEN: So those bright graduate students and postdocs—which is how you can judge this program—where would they go if they wouldn't come here? Well, they have the instruments here, though.

GOLDREICH: We are doing OK with getting astronomy postdocs, but we need senior people who are interested in this type of adventure, which is a little different than staying in astronomy or planetary science. It involves more technological use of the instruments than most astronomers make of them. That is something I see as needing to find a place at Caltech.

COHEN: Do you think our new president is aware of this? Or is he just looking?

GOLDREICH: I was on the committee that found the previous president. As part of that, we interviewed the current president, who wasn't very interested.

COHEN: That's ten years ago now.

GOLDREICH: That's more than ten years ago. And I've met him once since, but I've not had a real discussion with him. I think I will, within the next couple of weeks, as part of the astronomy staffing. My guess is that he'll be a lot easier to talk to than the previous president, because he has opinions which he expresses. So you can have a dialogue, whereas I found with Everhart that he just listened and you never got any feedback. It looked like he was paying attention, but you couldn't tell what he was thinking. And after ten years, I still couldn't tell. So I'm hopeful that the current fellow will be better. From what people who have had dealings with him have said, it sounds like that's probably the case. Everhart was extremely cautious, and that's not what this place needs.

COHEN: Have you had any dealings with the current provost [Steven E. Koonin]?

GOLDREICH: Oh yes. I've had lots of dealings with the Koonin about all sorts of things. He's a very capable person. He thinks well. He writes well. He speaks well. He needs a personality transplant, if he wants to progress. If he wants to progress as an administrator, he has to develop a more positive approach towards dealing with people. He also has to display a little bit less of a programmatic or bureaucratic face when he talks to faculty. But he has a lot of ability, and I wish him well. It's very difficult being provost. It's much harder than being division chairman.

COHEN: Well, you're on the front line.

GOLDREICH: Well, you have to say no. You take all the blame for this sort of thing, even when ultimately it's the president who has said no. So I think we have a good administration. The Institute Administrative Council must be much livelier now than it was a few years ago, both for the people who are division chairs and also for the president and provost. The short time I was acting division chairman and went to the Institute Administrative Council, it was so bureaucratic that it became boring after one meeting. Once you knew what went on, there was no more interest. Whereas now, I imagine, the issues that are being discussed are really discussed. So I would think it would be much more rewarding to be a division chairman now than it was then. We might hope for better leadership in the next few years.

COHEN: So you're optimistic, at least about this.

GOLDREICH: Yes. Actually, I'm fairly optimistic about Caltech, because no matter what we do to screw up, the places we compete with are doing at least as much. So our problem is to decide how, with a limited size, we are going to use our resources. But per faculty member, our resources are considerable. We just have to decide how we're going to use them. And I think we can use them better.

COHEN: So looking back—I mean, you still have a lot to look forward to, Peter, of course—do you sum up your stay here as good?

GOLDREICH: Oh, great. I was very lucky. I could have been somewhere else. I worked very hard here, and on things other than my own science—very hard. But it counted. In almost all cases it had an effect. And even in the miserable LIGO business, it wasn't that it was all ignored, it was just sort of a mess. But in other places, there wouldn't have been any point. I would have written reports that just got lost or had second-order effects. Not here. The fact that the place is run by the faculty extracts a cost—in time, in particular. On the other hand, it's a very rewarding place.

COHEN: Now, Peter, you talked a lot about your interaction with the faculty and this and that, and of course with your own students and graduate students. But you've really had a big track record of contribution with the undergraduates here.

GOLDREICH: Yes. I had two major interactions with undergraduates when I first came here. I liked to practice judo. I used to visit Japanese clubs downtown and on the West Side and work out. There was even one in Pasadena for a while. But it was very hard with little kids and teaching, so I started wrestling with the undergraduate team. For four years, I worked out with the wrestling team, and I made lots of friends, some of whom I still see.

COHEN: This was in the first years that you were here?

GOLDREICH: It was '68 through '72. I had been here two years before I started wrestling. Then I hurt my left knee and took up tennis. Also my younger son liked to play tennis. In 1976, after I had been here ten years, I moved into Page House with my family. My wife and I had a tiny apartment, and our kids had a dorm room across the hall. We lived there for four years. We had ninety students. And I loved it. I would go back and do it again. We ate the dorm food during the week for lunch and dinner. We played on the intramural teams. I would go around the dorm looking for unsolved physics and math problems before I went to bed. I couldn't solve all of them, to tell you the truth. I wrestled with the kids. On vacations, I would take the kids that were still around in the dorm to Owens Valley to go hiking or skiing.

COHEN: You didn't have the same ninety kids over the four years, did you?

GOLDREICH: No, no. They changed. Typically, kids would stay only a couple years in the dorms. Some stayed all four. But we saw a whole class graduate. And after that class left, I still had quite a bit to do for another three or four years with the ones who had been freshmen during our final year in Page. Up until a couple years ago, most of my undergrad advisees came from Page House. I would have five advisees from Page House. They didn't even know why they chose me. They just knew from previous Page undergraduates that they should. And I still see the students we lived with, all over. Two of them are my colleagues here—Sterl Phinney and

Ken Libbrecht; they were roommates at Page House. I have worked a lot with another one, Norm Murray, who is a professor at the University of Toronto. I've been to a lot of weddings. I thoroughly enjoyed the experience of living in Page. At first I was worried that I wouldn't be able to do any work, that it would be noisy, that I'd be bothered all the time. But it turned out to be fine, partly because my kids played with the students. We all went out and played together.

COHEN: Do you think it was positive, as far as your kids were concerned?

GOLDREICH: Yes, I think it was positive for all of us, including the students. I didn't do that much, because RA's are not in charge of anything. They're just meant to be available.

COHEN: And you had no huge crises or anything?

GOLDREICH: Oh yes, we had some crises. Fortunately, while we were there, we didn't have any suicides or anything like that. But we did have kids who disappeared or got in trouble in one way or another. One year we had quite a few students drop out. It seemed like an infectious disease. A few would give up, and then others would sit around the hall talking about how miserable it was. But the other three years, Page was a fairly happy house—not too many who dropped out. Many of the students seemed to have a very good time there. It was one of the more successful houses when we came into it. That's why Jim Mayer, who was master, put us there—because we had young children.

COHEN: Was that a unique thing? Was any other Caltech family ever in a dorm like that?

GOLDREICH: Well, subsequently Kerry Sieh went to Dabney House.

COHEN: But did he have his family with him?

GOLDREICH: They had a child, I think, or a baby. My kids were ten and twelve when we moved in, so that was different. I was also much older—Kerry was just a beginning faculty member. I don't think he had even been a postdoc. After Kerry left, Don [Donald J.] DePaolo was in Dabney House. He's now a famous geochemist at Berkeley. We just tried to hire him, unsuccessfully. It was different to have an older couple who, at that time, weren't that much older [laughter], not like now. But I played on the intramural tennis team and the intramural soccer team. I wrestled in the discobolus wrestling match against some kid from Ruddock House, who thought he was going to wrestle my son. He was very surprised to find out he was wrestling me. By the time he woke up, I had pinned him. Overall, I really enjoyed it. I still see these students. They drop by, and I hear about them. Some of them have become fantastically wealthy over the years. The third baseman on our softball team was one of the founders of Stac Electronics, which recently was awarded $120 million in a suit against Microsoft, which stole their disk compression algorithm. We had a very interesting time. But it was our whole social life for four years.

COHEN: That's what you did.

GOLDREICH: I was working, too. Some of the kids used to spend a lot of time with us. They also spent a lot of time with my wife, who had done some counseling.

COHEN: Susan really worked with them a lot, too?

GOLDREICH: Many of them wanted to talk to a woman.

COHEN: Well, you had, of course, girl students also.

GOLDREICH: Yes, but not very many. Typically there were about ten. I got along pretty well with most of them. But it was not a great environment for most females. It was OK for some, but there were too few. But most of the students seem to have turned out OK afterwards.

COHEN: Do you think that the students who continue to come to Caltech are good? What is your feeling about that?

GOLDREICH: I think we get excellent students. And we're fairly uncompromising in how we select them. I did recruiting visits two years ago for merit scholars—in the east and northeast. That was an interesting experience also.

COHEN: And they're certainly supportive of the institute when they leave.

GOLDREICH: Most of them look back fondly upon their time here, partly because they were in a climate with lots of other kids very similar to themselves. It's something they had never experienced before they came here. A lot of our kids come from rural areas. We tend not to get sophisticated kids. So we don't get kids who come from big urban centers of intellectual activity. They go to Harvard or Princeton or Stanford or even Berkeley.

COHEN: I think Berkeley's the main competitor.

GOLDREICH: I've heard that the average income of the parents of kids who go to Berkeley is higher than those who come here. We tend to attract kids who are unsophisticated and whose parents are not particularly well educated. They often come from environments where there is nobody else like them. And then they come here and find soulmates. So that compensates a bit for no longer being the best, or sometimes the superstar, intellectually, which is a shock for many of them.

Yes, I think Caltech is doing OK with students. I know there's a push for more diversity. And for a little while, when we had an admissions officer who came from MIT—Dan Langdale; I played basketball with him—we did much better attracting black students.

COHEN: Was he black?

GOLDREICH: No. When I first came, we had some black students, but they came from the LA area, and they mostly were very poorly prepared. And we didn't do them any great favor by bringing them here and having them fail. But then there was a period when we got middle-class kids, who happened to be black, from all over the country, whose parents were newspaper editors or teachers, and so on. They did fine. But all we were doing was taking them from Harvard or Princeton [laughter] or Stanford. It wasn't any great accomplishment, except that it better represented this particular segment of minorities. Recently we haven't been doing so well again. I don't know what to do about these issues. These are very difficult issues. They are less difficult with female students, but they are somewhat related.

COHEN: Everhart started to make a big thing of it, I think. Baltimore maybe started, but I understand he's dropped it—the diversity issue.

GOLDREICH: Well, I think it's worth trying for diversity. The problem is that there's a lot of competition for those kids who can succeed in this type of environment. Unfortunately, it's probably too late for the others—the ones who aren't prepared. They would be better off going to a less demanding place, where they have a chance to catch up. So that's a big problem here.

COHEN: But it's a problem in many places.

GOLDREICH: It may be less of a problem in regular schools, where the pressure is not so high and the coursework isn't so specialized.

COHEN: OK, Peter, is there anything else you'd like to say? I think we've got a good record.

GOLDREICH: I think, overall, it's hard to imagine a better place for me.

COHEN: You'd be surprised how many people in that chair say the same thing.

GOLDREICH: Well, it's a very good place. Also, it's incredibly luxurious.

COHEN: That's true.

GOLDREICH: It's incredibly luxurious. I don't take advantage of a lot of that, it's just—

COHEN: Very nice living in Southern California?

GOLDREICH: Yes, it's paradise.

COHEN: Well, you only go one block and you're walking through trees and flowers together.

GOLDREICH: The campus is beautiful. I like outdoor living. I can boat. Every time I go to the mountains, like I did a week and a half ago, it just amazes me that in a few hours you can go to a totally different environment on roads that are uncrowded. I can put on skis and tramp up to 11,000 feet in twenty feet of snow with no other people.

COHEN: It's very good here.

GOLDREICH: It's very lovely. And the gym is so nice. Most of the students aren't so good at sports, so I can still play with them.

COHEN: And you think it's going to continue to be a good place?

GOLDREICH: I think it's going to continue to be a very good place. But it's been around for a long time, and probably it needs to reevaluate a bit more where its resources go.

COHEN: You've had very little to do with JPL? I mean, you don't mention it at all.

GOLDREICH: I have students who work at JPL, and some of my success in science has come either by predicting or explaining phenomena that JPL spacecraft have observed. But I've stayed away. I've deliberately avoided team science, which is what mainly goes on, at least for faculty who get involved with JPL. Yes, I've avoided it. That's not true of my colleagues. Many of them in planetary science have profited by being on spacecraft teams. But I've never been. In fact, the only spacecraft involvement I've ever had was for the ISO [Infrared Space Observatory] spacecraft, which was pretty minimal involvement. And I only did that because Anneila Sargent was joining up with the people at UCLA, and she wanted somebody else from here so that she wouldn't be too badly outnumbered. We were part of the prime project. I don't like to have more meetings than I already have. I'd rather sit in my chair and think about things. I'm not so concerned about getting credit for something that's discovered by a spacecraft just because I'm on the team that had something to do with it. So I stay away from that. That's very deliberate, because at times I've been asked to participate.

COHEN: Do you have any feelings in general about how Caltech operates JPL—that it's such a big appendage?

GOLDREICH: Yes, I have a feeling about JPL, but not so much about the spacecraft missions, which, as far as I can tell, are done well. I think what JPL does in that line of work is probably better than that at any of the other NASA centers. Maybe that's because JPL pays competitive salaries, whereas at the other centers the salaries are set by the civil-service scale. Caltech is missing an opportunity, because we don't have enough science at JPL of the sort that takes place at Lawrence Livermore and at Lawrence Radiation Lab in connection with the faculty at Berkeley, particularly in physics. When we look for faculty members in space sciences, which we've done several times now over the past few years, we invariably find that a majority of the top candidates had connections with those labs—Lawrence Radiation and Lawrence Livermore. And somehow the Berkeley faculty who want to do projects that are larger than those that they can handle on campus use these laboratories as places to operate from—to have people attached to. And I think we haven't done that well. For example, I think LIGO should be in something like that. It's not a good idea to have something as big as LIGO on campus, taking up a significant fraction of a physics building and some of the library. Most of the LIGO staff are not intellectually connected to the campus, and so they dilute what goes on here. They should be in a JPL-type environment. And I think it's also a bit of false advertising to have this look like a Caltech project, because there really aren't many Caltech faculty involved in the guts of LIGO, except as management. So from that point of view, I think we haven't gotten what we can out of JPL. It would be better if we could have other types of science there—maybe a little far out, imaginative science.

COHEN: Now, are you saying it's because of the people who work at JPL?

GOLDREICH: I don't know why. I don't know enough. I've suggested this before, when we have had discussions about JPL at Caltech. And I don't know why nothing has happened. I don't think there's anything in JPL's charter that makes it impossible to do more than space missions there. A few years ago, JPL was looking for projects, and they embraced a certain amount of military work, which I didn't think was a good idea. And now it's dying away. So I think they can do other types of work. Of course, it has to be able to secure funding. But if Berkeley and the Lawrence Radiation and Livermore Labs can manage this, we should also be able to do it.

COHEN: Well, maybe those places are bigger, so that the people—

GOLDREICH: Well, there may be different funding. I know the University of California also gets a big fee for running the weapons labs. And I'm sure they hold some of that and use it to compete with big, private institutions. I know that even though, in general, their salary structure and perks are perhaps not as good as ours, if they really want somebody they can be very competitive in terms of resources. And they may use some of this in good ways. But we should be able to do something like that, too, and we haven't.

COHEN: Well, there are some people who work over there with groups. I guess if they want to they can. Andrew Lange is doing work over there.

GOLDREICH: JPL is not growing the type of people whom top universities covet as faculty members in the way the Lawrence Labs are. You see, Lawrence Labs are doing things like microwave background radiation, of course. Maybe that's partly Andy's [Andrew Lange's] model, because he was with Paul Richards, and he ran things out of those labs. And Luis Alvarez did some of his most imaginative stuff with this kind of connection with the Lawrence Labs. **[Tape ends]**

PETER GOLDREICH

SESSION 4

November 11, 1998

Begin Tape 4, Side 1

COHEN: I know you've talked on other tapes about the fact that this has been a great place for you. But there have been several other things that have been important in your life. One of them has certainly been your sports. How do you do that? Do you set apart a time, or is it just part of your day every day?

GOLDREICH: I try to make it pretty much part of my time every day. When I wake up in the morning, I always think to myself, "What am I going to do for exercise today?" Fortunately, I don't have a tremendous preference, except I don't care to swim very much. I'm not very good at it, and I'm pretty good at the other things. And I like variety. So some things I schedule, if I can find people who like to play at the same time I do and if they have a similar ability. I'll schedule a tennis match, or maybe a squash match, or racquetball. I also like to go to aerobics. For a while I did a very high level of aerobics. I can still do the advanced aerobics.

COHEN: Do you do it at Caltech?

GOLDREICH: No, I mainly go to a club. Susan and I started going twenty years ago. For a while I was able to do two advanced high-impact aerobics sessions one after the other. The club has since dropped that, fortunately for me, because I couldn't manage them any more.

COHEN: Has Susan continued to do aerobics?

GOLDREICH: Susan still goes.

COHEN: What club is it, particularly?

GOLDREICH: It used to be called Nautilus, it's called Bally's now. I go occasionally to the gym, which has gotten much, much better. They have a group of people who are much better now. The hours for aerobics are a little awkward for me. We wake up early—5:00 to 5:30 a.m.—and eat dinner around 6:30 p.m., and the good classes are mostly 6:00 to 7:00 p.m.

COHEN: Where? At the gym?

GOLDREICH: Both at the gym and at the club. Susan provides dinner, so I conform to her schedule. Andy [Andrew] Ingersoll started morning basketball, which lasted for several years. I played that—7:30 in the morning, two or three times a week. I used to play regularly, especially when my grandson was available. He played with us. I still enjoy basketball, when my legs are OK.

COHEN: That's right. You had a leg problem for a while.

GOLDREICH: I had a terrible sciatica problem for a couple years. I like to hike, and I used to run a lot. Since the sciatic nerve problem, I haven't. Lifting my right leg behind me seems to bother it. It's funny about these things. In aerobics, lifting my knee in front of me doesn't bother it at all. I like to ski, but Susan doesn't like to ski, unfortunately. But she'll go cross-country skiing with me. Last summer I did a 120-kilometer canoe trip. I like to go to remote places and live in tents. I've done that with a mutual friend of ours the last two summers, but Susan does not like to come—too many bugs and no showers.

I went to the Yukon and the northern part of British Columbia the last two summers. By and large, I like Canada. It's inexpensive, lower key, and empty. [Laughter] The Canadian summer is just gorgeous. It's very inexpensive to get to and to live in. So I like to do that.

COHEN: I always think in terms of tennis when I think of you, but maybe I just see you on the tennis court.

GOLDREICH: Well, I like to play tennis. I began to play tennis after I gave up wrestling. And my youngest son became a tennis player. I played with him all through his high school career. He was the number one player at Blair High School for two or three years.

Mainly I just like exercise. I like the competitive part. Mostly the people I exercise with are people I like. I like to ride bikes. In fact, I can see myself doing more of that. Again, I used to ride back and forth to work when we lived at the top of Altadena. I rode my bike up there all the time. We only had one car. And occasionally I'd ride up the mountains. I could see doing that again. I've ridden my bike up across the Angeles Crest, over Yosemite. The problem with bike riding is that it's easy on your body, as long as nobody hits you or you don't fall off the bike. [Laughter] Unfortunately, you can get killed by being hit by a car. That part's not so good.

COHEN: OK, Peter. The first thing I have written down is this 1981 California Scientist of the Year Award. Was that your first major honor, or did you get something before that?

GOLDREICH: Do you count the National Academy of Sciences? I was elected to the National Academy.

COHEN: OK. And you signed the book? Did you ever go?

GOLDREICH: No. I think I was elected to the National Academy in '72. I remember I was very surprised. Ed Salpeter called me up and congratulated me.

COHEN: But you've never gone to a meeting?

GOLDREICH: I've never gone to a meeting. I gave a talk at one of their symposia at the annual meeting. So I was there for a day.

COHEN: Did they make you sign the book?

GOLDREICH: I just came in and gave my talk. It's a waste of time.

COHEN: Well, it's actually a good place to see friends.

GOLDREICH: Yes. We did something unusual this summer. We went to a National Academy reception in Cape Cod. I was already there, giving lectures at Woods Hole, and Willem Malkus, an old friend, told us about it. We went and had a good meal. [Laughter] It's the only place I've ever been to where they've had lobster sandwiches—as many as you could eat.

COHEN: Who was the person you mentioned?

GOLDREICH: Willem Malkus. He's a fluid mechanician in the Math Department at MIT. My biggest honor, which I received before the California Scientist of the Year, was the Henry Norris Russell Prize of the AAS [American Astronomical Society]. That was in 1979. I gave a lecture at a meeting in San Francisco. The award is for lifetime achievement. I thought that was pretty good, since I wasn't even forty. I was recently on the committee, and most of the committee members voted against anybody under sixty on the grounds that they couldn't have had an entire career yet. I don't know about the National Academy, because of the peculiar manner in which members get elected to it. But on the Russell committee—on all of the AAS committees I've been part of—there's at least some appreciation of what the candidates have actually done. So if you get an AAS award it means something. The California Scientist of the Year—I don't know how they decide that, but it was perfectly fine to get it. I've probably told you the story—I went to the award ceremony in tails. The dinner was really a fund-raiser for the science museum. They also have a California Industrialist of the Year. That year it was Armand Hammer, who dramatically flew in from Moscow for the dinner. But anyway, I had a good time that day. First, I remember it was very, very hot. I rented a tuxedo. Jerry Ostriker was giving the Wednesday astronomy colloquium on campus, and before I left, I introduced him. Jerry always wears a jacket when he lectures, and this time I could trump him. I should have worn shorts with the tux, but I wore all the regalia. I was sweating like a pig. Anyway, I introduced Jerry and stayed for most of his talk. Then I took Susan and Barbara Niles, who was the GPS division chairman's secretary, to the dinner. I guess Robbie had to go, and Murph. I think Barclay, who was the GPS division chairman, was there too. They sat Susan and me at a table with some Libyan oilmen [laughter] and with a woman who was on the Coastal Commission. Her date was a developer, an obnoxious guy. Some of the more interesting things happened on the way in. I came early for pictures—and when we drove into the parking lot, the attendant asked me if I was with the catering staff. [Laughter] I was driving our old white Plymouth Valiant. I met the California secretary of state, March Fong Eu, the mother of Matt Fong.

COHEN: The guy who just lost the senate race against Barbara Boxer?

GOLDREICH: Yes. Then I had another very interesting experience. Arnold Beckman came up to me. He told me all about the San Francisco Exploratorium and how enthusiastic he was about it. Then he said to me, "It was all the child of Frank Oppenheimer," who of course was the real leftist in the Oppenheimer family. But this didn't bother old Arnold, who was about as conservative a person as you could find.

COHEN: Well, they compartmentalize.

GOLDREICH: Yes. He was so enthused. I had never visited the Exploratorium. The next time I had a chance, I went up to San Francisco and spent half a day there. It's wonderful. And then when Dan was in graduate school at UCSF, we used to go all the time.

COHEN: So, then the National Medal of Science. I bet there was something in between.

GOLDREICH: Oh yes, there were other things. I don't know if I can remember them all. I got the Chapman Medal from the Royal Astronomical Society. I had gotten the Brower Award of the Division of Dynamical Astronomy of the AAS. Each one of the different divisions has an award. I got the Kuiper Prize of the Division of Planetary Science of the AAS. That was in Munich. Susan and I met in Geneva, spent a couple of days in Zermatt, and drove to Munich. The award ceremony took so long that by the time I got into the line for the buffet dinner that followed, all that was left was Spam. [Laughter]

COHEN: Spam?

GOLDREICH: I was the guest of honor, but the roast beef ran out before I got to sit. I also got a gold medal from the Royal Astronomical Society.

COHEN: Oh, so that's not the same thing as the Chapman?

GOLDREICH: No, no. The Chapman was a separate one. That time, I had a hole in my pants pocket and was bleeding guineas while walking across the park on my way back to the subway to the hotel. [Laughter] And then there were a couple more. Then I got the National Medal of Science.

COHEN: And what year was that?

GOLDREICH: I think 1995. Clinton was president. I was very happy to get it from Clinton rather than from Bush or Reagan. Clinton and Gore put on a good show for us.

COHEN: What did that entail? And how did you know you got it? Did they call you on the phone? What happened?

GOLDREICH: I got a call from the president's science advisor. Also, there is a National Medal society. They're trying to make it—

COHEN: A bigger thing?

GOLDREICH: Yes.

COHEN: Well, it is a big thing.

GOLDREICH: Once a number of people get the medal, then there are more people trying to try to make it a big thing. It was sort of after the fact—like the Crafoord Prize. First the Crafoord Prize was given—somewhat similar to the Nobel Prize—but nobody had ever heard of it. But once there are enough Crafoord Prize laureates, they will try to make the Crafoord a bigger deal. Most of this stuff is a big waste of time. For the National Medal ceremony, you are allowed to take a limited number of guests. I took two secretaries and Susan and Dan and Deda. Eric didn't want to go. There was a fancy dinner the night before the award ceremony.

COHEN: They put you in a nice hotel, I assume.

GOLDREICH: It was a hotel—I wouldn't say it was so nice. It was OK. I don't like hotels too much. This was the day after the Million Man March [October 16]. There was plenty of activity in the hotel. It was a good location—within a few blocks of the White House, in the right direction. So it was a nice area. There was a formal dinner—I borrowed a tuxedo from one of my graduate students—at the Union Railroad Station. Some congressmen and senators attended.

COHEN: How many people at one time get this medal?

GOLDREICH: I think it is, on average, about six people a year in all fields who get the National Medal of Science. There have been years when there have been none, probably because the committee didn't do its work. There may have been years when there have been somewhat more. One time Ronald Reagan just decided to give a medal to Edward Teller.

COHEN: I see. So there's an interesting history to this. [Laughter]

GOLDREICH: On the award day, I had breakfast at the NSF, which was touching. I hadn't been to the NSF headquarters, so that was interesting enough. I met the monitors of my grants, which was nice. I also met the head of the NSF and complained a little about LIGO. [Laughter] I went there with a couple of other medal winners—we were in the same hotel. They were interesting guys. Meanwhile, Susan, Dan, Deda, and the secretaries took a tour of the White House. After lunch, we all went to the White House. Susan and I went to the Blue Room, and President Clinton and Vice President Gore entered and each one of them talked individually to each one of us and also to our companions. Clinton spent most of his time with Susan. He clasped her hand with both of his hands, and he looked directly into her eyes. Susan seemed to melt. He looked absolutely, genuinely entranced. It was amazing. We have this in pictures taken by White House photographers.

COHEN: Well, he has a large capacity for women, and Susan's good to look at. Why not?

GOLDREICH: It was just amazing. I couldn't believe it. But anyway, there it was.

COHEN: This may be the best part of this tape. [Laughter]

GOLDREICH: Yes. I talked to Gore and asked him about how much in control of his schedule he was—did he know what he was supposed to be doing, and when. [Laughter] Because it seemed to me it must be an awful life. Gore was very pleasant and self-effacing, and not nearly as wooden as he appears.

COHEN: I think he's getting a little better.

GOLDREICH: He's trying. It's pretty difficult. [Laughter] And then we went to the East Room where Clinton normally gives his speeches or press conferences. First Gore gave a speech on technology and science, which wasn't too bad. Clinton was taking notes, and his speech had all sorts of ad-libs in it. It was really clever. However, it didn't show much understanding of science. In fact it showed pretty much the opposite. But you could see he is a clever guy. And then they brought us up individually, and the vice president hung the medal around our necks and someone read our citations out loud. Clinton said to me, "You did so much," because all these little things I had done were listed in the citation. And I said, "I even voted for you." And he said, "God bless you," and gave me a hug.

COHEN: Politics. Did this award come with money or just a medal?

GOLDREICH: No. I've never won more than a few thousand bucks. Not that I need it. I guess I can look forward to winning a pile some day. After the award ceremony, there was a reception in one of the other rooms with really good food—fresh orange juice, great shrimps—really good stuff. We were allowed to wander out in the gardens. We were given free run of the halls on this floor of the White House. We could just wander around and take a look and see how well they were keeping up the upholstery and things like that. And I talked to other people. I talked to some of the butlers and some of the aides. Since I was wearing my rather big medal, I was a momentary celebrity; it was my five minutes of fame. I had had some attention the night before, too—sort of Washington power meetings. We'd get a message, or some undersecretary would come up and tell you, "The boss wants to meet you. Come meet the boss." We'd give each other manly stares and then this official, whoever he was, would tell me what he was all about, and I would be duly impressed. Then you'd go on to meet the next one. [Laughter] I had a hilarious meeting with Dan Goldin, the head of NASA, who is a little nuts. He sent over somebody—

COHEN: They had their people they sent to get people to come to them?

GOLDREICH: Yes. To have me come over to shake hands. He said he always wanted to meet me and that he had read all my papers. He's not a scientist; I thought, "My God! He didn't read any of my papers! Either he's confused or he's just trying to puff himself up into something he isn't."

COHEN: That is weird.

GOLDREICH: Yes, although I didn't say anything. Then he told me about how he was revamping NASA, most of which I agreed with. I couldn't think of anything to say to him, so I asked him how France Cordova was doing, because I knew she was working for him as liaison. He told me she was doing really well. [Laughter] And then, actually a couple months ago, I mentioned to one of my colleagues that I could call up my friend Dan Goldin and ask him about such and such; and my friend said to me, completely correctly, that Goldin wouldn't know who the hell I was. [Laughter]

COHEN: He comes out here once in a while, I think, to look at JPL.

GOLDREICH: Yes, but I don't have anything to do with that.

COHEN: Well, he's a politician.

GOLDREICH: He has some technical background. He is a technical administrator. And I think he's probably pretty good—although a bit nutty, like some other people we know.

COHEN: So, of all these things, that sounds like it was the most fun.

GOLDREICH: No, the Russell lecture was the most significant for me.

COHEN: That's the one that you hold in most value. OK. So anyway, Peter, you've had a good run. We'll have to do another interview in ten years or so.

GOLDREICH: If I'm still around here.

COHEN: You'll be around. **[Tape ends]**

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