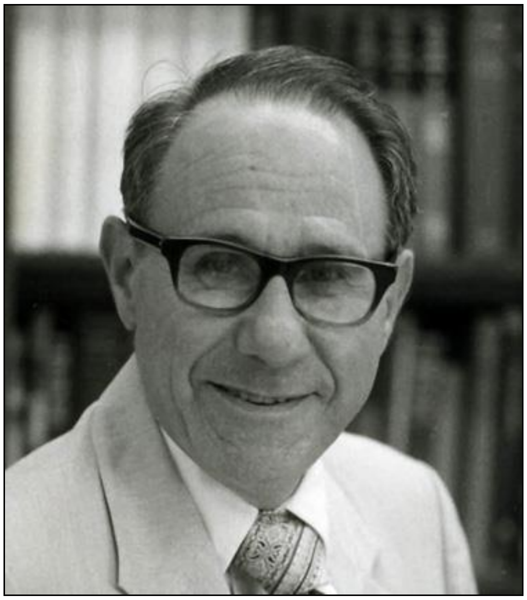

1979

MARSHALL H. COHEN
(1926-)

INTERVIEWED BY
SHELLEY ERWIN

November 1996, January-March 1997, and February 1999

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Astronomy

Abstract

Interview with Marshall H. Cohen, Caltech Professor of Astronomy, emeritus, by Shelley Erwin in six sessions and a supplement, 1996-1997, and 1999. He talks about his youth, family background, and education, and his early interest in electrical gadgets; wartime work for Westinghouse; higher education at Ohio State University: bachelor's degree, electrical engineering, 1948; PhD in physics, 1952. Reminiscences of the Ohio State Antenna Lab and Vic Rumsey, 1952-1954. Following appointment at Cornell in electrical engineering, Cohen describes his transition to the new field of radio astronomy. Recalls early participants in the field: J. Greenstein, F. Whipple, M. Ryle, B. Lovell, R. Hanbury Brown, E. G. Bowen, J. Bolton, P. Wild, W. Christiansen; British and Australian competition in interferometry; Caltech's early entry in the field.

Brief interlude recalls Richard Feynman both at Cornell and later at Caltech.

Recalls establishment of National Radio Astronomy Observatory (NRAO), Green Bank, West Virginia; later sites in New Mexico. Cohen's involvement with

ionospheric physics and building of Arecibo telescope in Puerto Rico. Recalls scientific work and political battles over Arecibo; colleagues E. Salpeter, T. Gold, B. Gordon. Cohen's move to UC San Diego, 1966, and soon after, recruitment to Caltech, 1968. He recalls the developments of the 1960s: first US interferometer in Owens Valley; competition for buildings very large arrays; the Greenstein decadal committee (1970).

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH MARSHALL H. COHEN

BY SHELLEY ERWIN

PASADENA, CALIFORNIA

http://resolver.caltech.edu/CaltechOH:OH_Cohen_M

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Pasadena, California

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ERWIN: Let's start and talk about your youth and your education. You were born in New Hampshire.

COHEN: Yes, in 1926.

ERWIN: What was it like growing up in New Hampshire?

COHEN: I don't much remember, because we moved away when I was six years old. We moved to New Jersey.

ERWIN: To a more urban environment?

COHEN: Well, we were living in Manchester, which was urban. But New Jersey was more urban, because we moved to a suburb of Newark—Bloomfield. I have very few recollections of Manchester. I went back there only once since we moved away. I drove through it fifteen years ago, and it didn't look particularly familiar. Everything looked very small and dingy.

ERWIN: So you don't think of yourself as a New Englander?

COHEN: No. Well, I don't know how I think of myself. If anyone asks me where I'm from or where I grew up, I say New Jersey. It's what I remember. But we moved around there also. When I was ten, we moved to Springfield, Massachusetts, for a year and then back to Bloomfield. And we lived in different houses in Bloomfield. We left Bloomfield when I was fifteen, and we went to Baltimore.

ERWIN: What were the reasons? Was this connected with your father's work?

COHEN: Yes. My father worked for an outfit called the Federal Tea Company, which was one of a number of companies that grew up in the Depression—or started up in the 1910s or 1920s—of delivering food and household goods to people's houses. He had a route and he would sell things to people at their door. There were crews of people who would walk up and down the street, knocking on doors and asking, "Do you want to join this organization?" And the delivery man would come the next week, and then he would come every week. My father was a good salesman, and he worked in various ways for this Federal Tea Company, which had its offices in Springfield, Massachusetts. He got to be branch manager, and he was opening up new branches, so he had to go to Springfield, and then he went back to Bloomfield.

When my parents first moved to New Hampshire, he did not work for that company. And I don't recall how he got connected with them.

Then later, when we went to Baltimore, he dropped out of contact with the Federal Tea Company and set up his own company—Prime Tea Company—which did not do well, because right after the war there was an explosion of supermarkets, and people had automobiles. During the Depression, it was easier to sell people things at their door.

ERWIN: Do you remember when you first became interested in science?

COHEN: When I was young, I was very interested in what you would call engineering things—tinkering, and radio things, making things. I did a lot of work with electrical gadgets. I used to do dangerous chemistry experiments, like making little bombs and things that would blow up. I used to take radios apart, and clocks. And then since part of what my father was selling was small household appliances—toasters, for example—he'd get broken ones back, and I would fix them. I would sit around his place and fix toasters and irons and whatnot.

ERWIN: Did your father have an inclination in that direction himself?

COHEN: No, none at all. But my Uncle Henry did. That's my mother's brother. He was quite interested in tinkering.

ERWIN: So you were alone in your immediate family as far as tinkering.

COHEN: Yes, I was totally alone. But I didn't get seriously into it. I studied partly with a friend. For example, there was an effort to get a radio amateur license, and I got up to ten words a minute in Morse code, but I never made it to thirteen, which was required at the time. So I gave that up—never really worked in amateur radio, although I used to take radios apart and try to fix them. I was not a real builder of things, like many people were, with elaborate basement or garage laboratories where they genuinely made mechanical and electrical gadgets. I was at most a tinkerer—but I was very interested in all that stuff.

ERWIN: Was there any point in your high school career when you were influenced by someone?

COHEN: Not really. I did very well in school. I was very young when I was in school. I started kindergarten when I was four years old instead of five. My sister, who was older, always claimed that my mother did that to get me out of the house. I was a bit precocious. So I was a year younger than everybody, and in addition I was put ahead in class.

ERWIN: You started in school in New Hampshire?

COHEN: I must have gone to kindergarten in New Hampshire, and probably first grade. At any rate, all through school, I was younger than everyone else in my class. Also smarter—that is, I was first in my class, or second, all through school. I had very few friends. I just couldn't deal with people. When you're eleven years old, say, and everybody else in your class is thirteen, it's very difficult. My closest friend when I was living in New Jersey was in a class behind me, because we were more of an age. So that was better.

ERWIN: You knew you were bound for college?

COHEN: Yes, I knew I wanted to go to college, although people in my family had not gone to college—neither my mother, my father, nor anybody else in my family, except for one uncle, who was always held up to me, my mother's brother Charlie. He was a model. He had gone to Boston Latin high school, and then he went to law school, and he practiced law in Boston. So he was held up to me as a great idol—someone who had gone to college and made a success. Most of the people in my family were small-time merchants or small-time salesmen—my Uncle Henry, however, was a machinist and fixed watches and did other odd jobs.

ERWIN: Was your mother's family from Massachusetts?

COHEN: Yes. My mother and father were both born in Boston. And their parents were born in Europe. My father's older brothers and sisters were born in Europe. They emigrated in the early 1890s. My father was born, I think, in 1895, and his older siblings were born in Europe.

ERWIN: And where had they come from in Europe?

COHEN: My mother's family came from Warsaw; and my father's family came from what is now Lithuania, which was then part of Russia—near Vilna.

ERWIN: For the record, what was your mother's family name?

COHEN: Epstein.

ERWIN: So are you related to the famous Paul Epstein?

COHEN: No, no. Epstein is a common name.

ERWIN: Well, he was born in Warsaw.

COHEN: Was he? That's very interesting. I'll have to look that up. Once I was in Warsaw, and we went to the Jewish cemetery and poked around, and I found a number of Epsteins who had been buried there. And several were in imposing big granite tombs. The inscriptions were all in Hebrew, but I could read them, sort of. But I had no idea who those people really were.

My maternal grandfather—it is alleged—left because he didn't want to get involved with the long-term, many-many-generation family tradition, which was to become a rabbi. In fact, I am descended from a long line of rabbis on my mother's side, who lived in Warsaw. And my grandfather refused to have anything to do with that, and came to Boston. And he also wanted to escape the Russian Army, I presume. That's why people left in those years—to get away from being drafted and sent to fight the Japanese or go to Siberia.

ERWIN: So when he came to this country, he went into business.

COHEN: Now that's interesting—I have no idea what my grandfather did. My sister will know. She was enough older to have known some of these people much better than I did. I was still young when many of them were dying.

ERWIN: Is your sister the only sibling?

COHEN: No, I have a brother, who's younger. We're all very close in age, but my sister is two years older, and that makes a difference. She has much more remembrance. For example, she remembers some of my grandparents rather well, and I don't. My maternal grandfather lived with us for a while during the Depression. My father had a job all during the Depression—that I remember—and a lot of other people didn't. So families took care of each other. My grandfather lived with us for several years, and then with others of his kids. For a time, my father's brother lived with us also, and he worked as a salesman with my father during that period. And my Uncle Charlie lived with us intermittently.

My father's father—I don't know what he did, either. But my father got involved with his older brother. My father's father had two sets of children. He had a wife who had a number of children, and then she died. And he married again and had another set of children. My father was in the middle of the second set of children. He and his two younger sisters were born in this country. All the rest were born in Lithuania. I met some of them. One of my half uncles—one of my father's brothers from the first family—had gone to Detroit and was in the jewelry business. My father went out there and worked with him for a while before he was married. Then he came back to Boston and married my mother.

My mother had graduated very early from high school. She must have been quite bright. Her mother died in the flu epidemic at the end of the First World War, when my mother was sixteen years old. And apparently that was a terrible thing for my mother, because she would occasionally talk about it. She went to live with her older sister, who was married, and had a bad time there. But sixteen-, eighteen-year-old girls didn't live by themselves. Her father—the one who later came to live with us—sort of put the children away and went off by himself. The kids were left on their own. And strange things happened. Her youngest brother was put into a mental institution, some place in Massachusetts. This was a very strange thing, because many years later, when my mother was in her seventies, she got a notice from this place in Massachusetts to the effect that this guy had died and she was the only person listed as a relative. She thought he had died twenty years earlier. Very peculiar. He had been lost, in a real sense, in this home in Massachusetts—although the people there knew my mother's name and address. I think things like that were much more common then than they are now—people getting put in homes and being left there for thirty years. And a father turning his kids out so that they have to fend for themselves while he goes off somewhere because his wife has died and he can't handle it. There are social services now that take care of situations like that.

Anyway, my mother went to live with my aunt. She had a lot of trouble there, but nonetheless they were fairly close.

My mother worked for the telephone company. She was an operator, and she did that until she got married. Then they moved to Manchester and three children were born. My mother went back to work many years later. She worked for the IRS in Baltimore.

ERWIN: Did she have particular interests or talents of her own that you recall?

COHEN: She used to play the piano. She would play popular tunes occasionally. Otherwise, did she have talent? I don't know.

ERWIN: Was she a force in your intellectual development? Or did you really feel you were alone?

COHEN: No, I was alone in all that. We were not an intellectual household. There was no classical music in our household, for example. We never discussed things.

ERWIN: So when it came time for you to go to college, how did that all transpire?

COHEN: Well, my mother, being from Boston, had lots of relatives in Boston, and my cousins, the next generation, were going to college. And MIT was the only place that anyone there had ever heard of for someone who's interested in engineering, which I claimed I was interested in. But the war was on.

I graduated high school when I was sixteen, in Baltimore—January 1943. And I went to work at Westinghouse. I didn't go to college. We didn't have much money, and also I wasn't urged to go to college. We had moved to Baltimore only six months before, and I wasn't involved with people. I got very poor advice, if any. So I went to work for Westinghouse, and I started working as a tester. I took a streetcar and a bus a long way across the city every morning, and I worked night shifts sometimes. It was mostly women who worked there, as I remember. It was an electronics factory, so there were machine shops and enormous assembly lines, with people assembling chassis and soldering wires. And I worked at the end of the assembly lines.

ERWIN: What exactly was being built?

COHEN: Gun-laying radar. They built in that factory a device called the SCR584, which had been designed at the Radiation Lab at MIT. I found out a lot about this after the war, and recently I've read some history books about it. It was called gun-laying radar, and it had just been designed the year before—maybe one or two years before. And they were being manufactured for military use at Westinghouse in Baltimore and by General Electric, I think in Pittsburgh. The components came in—resistors and capacitors and such—and I tested them to make sure they lay within the range specified: 44 plus or minus 3 ohms, for example. I put the good ones in one pile, and the bad ones we'd send back.

In the course of the year, I rose to become senior tester, and I did final tests on radar systems. I was quite bright, although I'd only had high school. But I had taken all the high school math and physics I could. I had trigonometry and solid geometry in high school. Calculus was not taught in Bloomfield. It was taught at Baltimore Polytechnic Institute—a high school—but I could not go there because I was so close to graduation. I did go to Baltimore City College—another high school.

I was paid a very handsome wage at Westinghouse. I had a couple people working under me, at the end. By then, I was seventeen years old. I was paid a dollar and eight and a half cents per hour, which was a good salary at the end of 1943. Westinghouse and the radar people were desperate for people who could test radar sets.

This equipment was in a military truck, and inside the truck was a whole big radar system and an antenna, which went up and down on a platform on the roof—it was S-band, I think, ten centimeters or eight centimeters, I'm not sure. The components—the transmitter, and so forth—were tested individually, but then all the stuff was put into the truck, and the truck, complete, had to be tested. I didn't drive the truck; I had no driving license—though I did by the end of it, because as soon as I got to be of age I got a driving license. So the truck would have to be tested. I didn't invent the test—there were engineers there who told me what to do. But I did all the tests, and checked things off, and signed my name. One of the tests, for example, was skimming across the top of a hill to the Baltimore airport and tracking airplanes. So I was tracking airplanes day by day, as a test of this gun-laying radar. It had an output that controlled anti-aircraft guns. It would track a target and a gun would follow and would shoot at the airplane. These things turned out to be very successful. By 1943, they were being sent off to Europe, and they probably were used also in Asia. They were supposed to be used to shoot down airplanes, but they were used for all kinds of other things. You could point them sideways and look at tanks and so forth. So they were used for many purposes beyond what they had originally been designed for, and they were very successful, as far as I know.

ERWIN: Was this secret?

COHEN: Oh, yes. I was not supposed to tell anybody what I was doing. I had clearance from the very first day I went in. The whole factory was secret. You couldn't get into the place at all without some special badge. I had to join a union. I bet I'm one of the few Caltech professors who joined an industrial union. I joined the United Electrical Radio and Machine Workers of America.

ERWIN: How long did you stay at Westinghouse—until the end of the war?

COHEN: No, about a year. Because I wanted to go to college, and my parents did encourage me in that. But what I was doing was interesting, and it was a lot of money for a kid. The other thing is it was an enormous amount of responsibility. You couldn't conceive now of a seventeen-year-old being in charge of the final test on a truck full of equipment that must have cost a million dollars in today's money. But they had no one else to do it. And I was smart enough to do it, although exceedingly young, I realized later. I had people who were supposed to do things for me, and I didn't really know how to tell people how to do things.

At the end of the year, I joined the army. I enlisted in the ASTP—Army Specialized Training Program. The idea was that they would send you to college. You had to be seventeen years old to join this program, and at age eighteen you'd be transferred to active duty. But I was never in the regular army.

So I joined the ASTP along with one other guy I had made friends with at Westinghouse. There were others, and we were sent to Lehigh University. I was there for six months. We studied electrical engineering. So this was my first college training—in the army.

ERWIN: Was this one of these specialized wartime programs?

COHEN: Yes, very specialized. I was not a normal freshman—I was in the army. There were programs like that at Caltech, too. Anyway, I was six months at Lehigh University, then three months at Virginia Polytechnic Institute. At that point, I was eighteen years old, so I was sent to Georgia, to a place near Macon, for regular basic training. And during that basic training, there was the Battle of the Bulge, in the Ardennes Forest—this was Christmas 1944. I was just finishing basic training, and it wasn't clear what was going to happen. There were a couple of people picked up out of the group I was with and sent off to Europe right on the spot, because they needed a lot of replacements in Belgium. But I and some others had done very well on the army tests, I guess. Anyway, we were shipped off to Columbus, Ohio, to continue this ASTP program. So after basic training, we're now on active duty in the army, and we got sent to Ohio State University to finish the ASTP program, which was eighteen months long. We had no engineering degree, but we had enough for the army's purposes. And we stayed in Columbus for nine months, but by then the war had ended.

But I didn't have many points. Do you know what points are? You get a point for each month you're in the army, and two points per month if you're overseas. And the people with the most points get discharged first. My points at Lehigh and VPI didn't count, so I had very few points.

ERWIN: So you had to hang around to be discharged?

COHEN: Well, first they sent me to a place called Camp Crowder, which was out in the southwest corner of Missouri. That was basic training, again, only by that time I'd already been in the army for a year. So we were big shots, and there were new eighteen-year-olds coming in, and we could lord it over them.

Anyway, after a short period of time—a couple of months—I was sent to Fort Dix, New Jersey, to be an instructor in an army school. Fort Dix had a big Signal Corps operation. People were sent there to learn about electricity. Then they became radar fixers—I guess it was mostly a repair school. So I stayed there for six months or so as an instructor. I was a private first class. I had sergeants and all kinds of people enrolled in my class.

We're now in 1946, so I'm twenty years old at this point. I was in the army a total of two years, nine months, and ten days—that's an important number, which will come up later—and I started in January 1944. So I got out toward the end of '46. It must have been in September. I had asked around about college, and Ohio State would give me more credit than other places.

Oh, I had applied at MIT, I forgot that. We have to back up. When I was working at Westinghouse, I applied for admission to MIT and was turned down.

ERWIN: Do you think that was because you were sixteen years old?

COHEN: The application period would have been in the early spring—that's right, I would have been sixteen still. I don't know why I was turned down. They don't say.

ERWIN: So you didn't have any particular sense about that, that either convinced you not to apply again or made you want to apply again later?

COHEN: I don't remember. I did not apply to MIT again. But at Ohio State I could get an engineering degree more quickly than anywhere else, because they would give me credits for all that time I had spent there.

ERWIN: It looks like, from the chronology, it took you two years to go through college.

COHEN: Yes, something like that. I went to Ohio State in electrical engineering in September 1946 and graduated with an engineering degree two years later, in June '48. But when I was there, I got much more interested in physics. And I then went to graduate school at Ohio State in physics.

ERWIN: So your PhD was in physics, four years later.

COHEN: That's right.

ERWIN: Well, in the immediate postwar period—Ohio State in the late forties—what were things like then? Did you feel that there was a kind of cranking down of things, or did you feel that physics and even engineering had been revitalized? What was the intellectual atmosphere like in this postwar period?

COHEN: I don't know how to answer that.

ERWIN: Was it a good time? Was it an exciting time?

COHEN: You read in history books or in biographies—"1925 was an exciting year. Erwin Schrödinger had just done such and such." I had no conception whatsoever of things like that. So in that sense, it was not intellectually exciting. I was a college student, a veteran, along with tens of thousands of others at Ohio State. The place was jammed. And a lot of people were much older than I was, and more mature. It was a serious bunch, particularly in electrical engineering. It was not a research-oriented place. The important people were the people who later on went into local industry. Ohio State graduates are all over Ohio, and they're the cream of the engineering profession there. And people there probably had never heard of Caltech; they maybe had heard of MIT.

I remember in 1945 when the atomic bomb was dropped. One of my classmates had a degree in chemistry, but he was in the same ASTP electrical-engineering program. The army needed electronics people—it didn't need chemists. So I remember that this guy—who now lives in Sacramento; his name is Sam Goldhagen—he told us about uranium and things like that. The rest of us had only the foggiest notion of what was going on, although by that time we'd had physics courses and we knew about atomic weights and so on. But he knew much more. And I remember him explaining what was going on. That was very exciting. But what was the intellectual atmosphere? I don't think Ohio State was very intellectual—certainly not like Caltech, and not like Cornell was when I was at Cornell.

ERWIN: Well, we can skip ahead. Maybe you can talk about the OSU Antenna Laboratory.

COHEN: Yes, I worked there for a number of years, before and after I graduated. Well, I got a degree in electrical engineering. And that was just beginning to be sort of newish, in the sense of getting away from the very strong emphasis on machinery. But we still had to do a lot of work in electrical machinery laboratories. That went out of fashion in the next decade, everywhere. But it was strong then, so I got a very heavy dose of rotating machinery, and at the time I knew all about three-phase motors and stuff like that.

Anyway, I remember talking to my first advisor in physics. He thought I should study physics in graduate school and not electrical engineering, because I was dissatisfied with what I was learning in electrical engineering. We had a course in thermodynamics, but we really didn't learn very much. Whereas in physics, it was clear that there were some interesting things to learn.

ERWIN: This was your perception of what was interesting?

COHEN: Right. Thermodynamics was interesting. Rotating machinery wasn't. Vacuum tubes were fairly interesting—I've had big, intensive study of vacuum tubes. But I finished all that before transistors were invented, and I have never understood how transistors work, let alone integrated circuits—well, vaguely I do.

So I started graduate work in physics. And the first thing I did was work with somebody named John Cooper, who was doing neutron scattering experiments. And I built a neutron detector, which consisted of a brass pipe full of wax. And at the moment I don't remember how it was supposed to work. But it did something. There was a counter and some sort of detector, I guess. And I had to build some electronic circuitry to go with it. So I built this stuff, and I compiled a very large list of cross sections.

ERWIN: Was this area of physics identified?

COHEN: It was nuclear physics. That took me a year.

ERWIN: Did you know at that time about any of the work that was going on at Caltech in nuclear physics?

COHEN: No, I had no idea. I had heard of Caltech and that's all. No, because I think the atmosphere at Ohio State, as I look back on it, was very different from Caltech. If I had stayed in nuclear physics, I might have gotten more involved with front-line research in one way or another and gone to seminars. But there was very little of that. I was taking a lot of classes. There was little notion of research as a long-term career—certainly not in engineering. People went out and got jobs in industry. And that was also true in physics, to a large extent.

So I was poking around the campus. And now I don't remember quite why I first went to the Antenna Laboratory. I think I knew somebody who was working there. Anyway, that was a place that had a lot of money. And I decided I needed more money, in addition.

ERWIN: Were they connected with the government?

COHEN: Yes, they were getting federal money. And a lot of the engineering work I had done was somewhat of a background for this Antenna Laboratory. The director of the laboratory was a guy named Vic Rumsey, a professor of electrical engineering, who's from England. There were research engineers working there, and a number of students doing different projects—the students were variously from physics and electrical engineering and mathematics, but mostly from electrical engineering. It was clear that I had a background that could fit in there—and I knew somebody there, and it seemed like a pretty cheerful place.

What that laboratory did, basically, was research on antennas. During the war, there was an enormous explosion in electromagnetic theory and how you deal with antennas. The lab did a lot of work for Wright Patterson Air Force Base—because airplanes were getting faster and faster. Old airplanes used to have wires that drooped around them, and of course for the high-speed jet airplane, that was no good—the wind would rip the wire off. So there was a great deal of work on so-called cavity antennas, where they would cut a hole in the airplane and put an antenna in the hole, and then put a cover over it. That was one aspect of the work going on there—studying those kinds of structures. How small could you make them and still have them work? And questions of bandwidth. And there was a lot of work there on radar systems—not the electronics but the electromagnetic effects—that is, the antennas for radar work, scattering and so on.

After I got my PhD, I stayed there for two years. I was studying radar cross sections. The radar set sends out a certain amount of power, and it gets an echo back. And so you want to build the signal up to be as strong as possible. The amount of signal you get back depends on how much power you put out. It depends on how far away the target is, but it also depends on what's called the cross section: that is, if it's a big flat plate looking at you, you'll get a big echo; but if it's a missile with a pointed nose coming at you, you'll get a very small echo. So the particular thing I did was study the radar cross section of missiles for a year and a half. That also was classified. And we had models of things like German V-2 rockets that were six and eight feet long, made out of wood with copper sprayed on it. And with fins. Some of them were built beautifully. And we had models of supersonic aircraft, and we measured the radar cross section of those in a simulated way. Instead of measuring at the radar frequency, we would use a corresponding high frequency and measure the cross sections, so that they could be factored into the radar equation to tell you the strength of the echo. The cross section, of course, depends on the aspect. We did a lot of that kind of stuff for Wright Patterson Air Force Base. [Tape ends]

Begin Tape 1, Side 2

ERWIN: You met your wife Shirley during the time you were at Ohio State?

COHEN: It was in the first year after I went back. She was a student at Ohio State, in her second year. And I was sort of in a third year, but it was mixed, because I was taking some early courses and some late courses. She majored in chemistry, and she was taking calculus. And I, as a job, was grading calculus papers. I was a teaching assistant when I went back to Ohio State. That two years, nine months, and ten days was the amount of time the Veterans Administration would pay tuition for me. I got, I think, $90 a month from them, but I decided I needed more. That was on the GI Bill.

ERWIN: So what year did you get married in?

COHEN: 1948. And we met in 1946, two years earlier. I knew her name, because there were very few girls in these classes I was grading. So I was at a dance, at Hillel. I went over to see what was up. And she had gone over to see what was up. While I was just standing there, I heard someone shout, "Shirley Kekst!" I said to myself, "I know that name," and I thought, That's an easy way to meet a girl. [Laughter] So I went over to her and said, "You must be Shirley." And one thing led to another, and we got married. That's how that happened—because she was in the class that I was a teaching assistant for.

ERWIN: So you were married the whole time you were doing your graduate work?

COHEN: Yes, that's right. And she had one year to go, at that point. She loves to tell the story of how she had been an Ohio resident and so the fees were only $20 a month—or maybe it was free, I don't remember. But I was from Maryland, and I had to pay out-of-state fees—well, the Veteran's Administration paid that. But after we got married, that somehow got registered with the university, and a little while later they called her in and said, "You've got to pay out-of-state fees, because you married someone from out of state. So therefore you're now from out of state, and you owe us a hundred and fifty dollars."

ERWIN: How perfectly unreasonable! It should have worked the other way around.

COHEN: Yes. I should have been considered a resident. But a woman follows her husband's residency. [Laughter] This was 1948, you understand.

So that led to a big row. Anyway, the upshot was that they let us off the hook—made a special exemption for us. They said something like, "We trust that when you get out and get on your feet, you'll see fit to make donations to the university." We never did. [Laughter] That's one of her favorite stories.

Anyway, I went into the Antenna Laboratory. And I don't remember quite what I did at first. I went there when I was in my second year of graduate work. What I did when I first went in, I just don't remember. Then I began to work on these cavity antennas. I wrote my thesis on cavity antennas. It was a theoretical study of some waveguide structure that opened up on a sheet of aluminum, and how wide a frequency range you could expect that to work for different sizes, and how small the aperture could be and what happens if you fill this thing up with polystyrene or something and make it work at a lower frequency. How can it be generalized? There were a lot of questions. There's a lot of theoretical calculations in my thesis. But I also built models. I built a number of these cavity antennas, with little stubs sticking into them, and made measurements on them. That was what my thesis was about.

ERWIN: I'm very curious to know how you got from there to radio astronomy. Did it happen at that time? Or was it later?

COHEN: People are always confused by that, because they think I was involved with John Kraus, who was at Ohio State and doing radio astronomy. I knew Kraus, and I knew some of the people who were working with him. And I'd go over and visit with them, because what I was doing was very close to what they were doing. But I never worked with them—never had anything to do, really, with radio astronomy.

After I got my PhD, I was thinking of going somewhere else. In fact, this was such an un-intellectual place that I was thinking of industrial places. But I didn't know what I wanted to do. I had had a very handsome offer to stay there at the Antenna Laboratory after my PhD—as a research associate or something, and it could be a permanent job. A number of people did that. In fact, that's where most of these senior engineers came from. Some of them became professors of electrical engineering, some didn't. Some of them stayed there their whole lives and never left that tiny little corner of work they were doing. Anyway, they offered me a very handsome salary—$8,000 a year—to stay as a research associate. So we stayed. And by that time we had two children. We lived in a nice place in Columbus on the other side of town. But I was looking for other jobs, and I didn't really know where to go. I had hardly heard of postdoctoral fellowships; they weren't in the air where I was. Had I stayed in nuclear physics, that would have been something I'd have known about. But where I was, in the Antenna Lab, nobody did that. People either stayed there, or they went out into industry, or they went to work for the military. Well, a few did become professors of electrical engineering, one at Syracuse University and another at Penn State. But I became dissatisfied with those prospects. They seemed too tame and too narrow a thing to do. I began looking for other jobs. I went to the Glenn Martin Company, in Baltimore, and I looked at their engineering facility, which seemed to be a one-acre-size room, it seemed, with a thousand people sitting in it. And I decided that wasn't for me. I left that area pretty quickly.

I then found out about jobs teaching physics, and I began to think of applying for physics jobs. But I was out of the mainstream of physics, so anybody who read my résumé would think this was a little bit crazy. But I thought maybe I should teach physics. I could teach electrical engineering; that would have been much easier.

Then my boss, Rumsey, came to me one day. He knew I was looking for jobs, because he'd been writing letters. And he said, "Here's an opening at Cornell University. They're looking for someone to be an assistant professor of electrical engineering. They have a radio astronomy program for studying the sun, and one of their chief instrument guys has just left, and they don't have the right mix of people to keep this thing going. They need someone who has some experience in antennas and in waveguides and an understanding of how these things work." I thought that sounded interesting.

So I went off to Cornell, and I got that job. That's how I became an assistant professor of electrical engineering at Cornell, and that's how I got into the radio astronomy business. It was entirely accidental, because I answered an ad. I had not done that before—I had not looked for radio astronomy, or astronomy. I did not start at age seven grinding lenses. And now I'm using the Keck Telescope as a professional optical astronomer. So I've come a long way.

ERWIN: So you moved to Ithaca, and you had an appointment in electrical engineering?

COHEN: That's right. I went there as an assistant professor of electrical engineering. I took a drop in salary to do it, so that shows you how eager I was to leave Columbus. Cornell is a research university, and I had already been writing papers at the Antenna Lab. People there wrote papers. We published papers in the *Proceedings of the IRE* [Institute of Radio Engineers], which since has become the *Transactions of the IEEE* [Institute of Electrical and Electronics Engineers], with many different subsections. So I have a number of publications in the engineering literature, having to do with antennas or electromagnetic theory. Only quite a bit later did I start publishing in astronomy journals.

ERWIN: One of the things I wanted to cover was how radio astronomy as a discipline took shape. It seemed to me from what I could gather that it was stronger in other places than in the United States. And I'd be interested in your personal slant on that.

COHEN: I was ten years too young to have been in on the ground floor.

ERWIN: But you must have known some of the people who were in on the ground floor.

COHEN: Yes, I knew some of the people who genuinely were on the ground floor. Not the people from the thirties—except for Jesse Greenstein. I knew Fred Whipple also; he was from the thirties. But I knew the people from the mid-forties who started things, like Martin Ryle—people in England. He was at Cambridge. And I knew Sir Bernard Lovell, who was at Manchester. I knew Robert Hanbury Brown, who was in England and then went to Australia. I did not ever meet Edward G. Bowen—Taffy Bowen. He was a big organizer. He wasn't a radio astronomer, but he was a very big organizer and a major player along with [Caltech president Lee] DuBridge.

ERWIN: We might come back to him later, when we talk about how it took shape at Caltech.

COHEN: Yes. Well, DuBridge and [Robert] Bacher and Greenstein were involved in that, with Bowen. And I knew others of the Australians. I knew John Bolton fairly well. I knew Paul Wild. I knew [Wilbur N.] Christiansen. Those are many of the early players. I knew people in France and in Russia.

ERWIN: When would you have met these people?

COHEN: I met some of them quite quickly. I met a number of them in 1957 or '58, when there was an URSI [International Union of Radio Science] meeting in Boulder, Colorado. Actually, I met some of them when I first went to Cornell. I was immediately dragged off to England to a radio astronomy meeting by Charlie Burrows, who was the director of the School of Electrical Engineering at Cornell. He had worked during the war on radio propagation things and knew all about radio astronomy, and he was the one who promoted radio astronomy at Cornell after the war. And he had gotten those other people to come. And then they left him, and he had to get some more, and that's where I came in.

ERWIN: You went to Cornell in '54. And very early in that year, there was the Caltech-Carnegie conference on radio astronomy, in Washington, which I know Burrows attended. His name was on the list. You would not have attended, because you were not in the field yet. But were you aware of the ferment that was going on?

COHEN: No, I wasn't. Although I did know some of the people working with John Kraus at Ohio State and they must have been aware. But somehow I wasn't close enough with them to pick it up. They were, in some sense, out of the true mainstream. John Kraus had a fixation on his own particular helical antenna, which he had invented. He made an array of them, and it didn't work as well as some other things did. And then he built a very big dish, which made the Ohio State Survey. But he in most ways was not a major player in radio astronomy and has not left much of a legacy, as a matter of fact, although I knew him and I always liked him as a man.

ERWIN: In your article about the Owens Valley Radio Observatory [*Engineering and Science*, Spring 1994], you do refer to DuBridge and Bacher and Greenstein. There are some interesting papers here in the Archives which show that DuBridge spearheaded this conference.

COHEN: That depends on who you talk to. If you talk to Greenstein, you'll find that Greenstein spearheaded the conference.

ERWIN: Well, certainly DuBridge was sympathetic.

COHEN: I think actually all these people tend to see what happened forty years ago according to their current view of what *should* have happened. That was true of John Bolton, by the way. It's true of Greenstein. And my guess is that DuBridge did have a very big hand in that—although Greenstein did all the work; all the organizing and the invitations and so on. But I have a feeling that DuBridge must have played a big part in that. Because I think DuBridge had been needled by Greenstein, and also by people from Carnegie, to the effect that because of Palomar, Caltech needed a collaborating radio observatory. We were a natural place to do it, but we'd have to get someone in from the outside. That's when they got John Bolton—that was a year later, after this meeting. But I think it must have been DuBridge who needed to be convinced, and somehow out of that came the idea for a big conference in Washington at the Carnegie Institution.

ERWIN: DuBridge seemed to be able to articulate the problem well—to say, here's the difficulty: the United States is lagging behind, and we need to try to assess the present state of radio astronomy, the future, and how we're going to be competitive.

COHEN: He did say a lot of things like that. He was the statesman in all this. And he was very close with this chap from Australia, Taffy Bowen. So he could get Bowen to describe what was going on in Australia and in England—because Bowen had been in England a lot. Bowen was part of the group that said we needed to beef up the US program—that we were falling way behind. And there were other people, on the East Coast. It wasn't only DuBridge who was saying things like that.

ERWIN: Was Bowen an Australian?

COHEN: He may have been an Englishman. [Bowen was born in Wales.—ed.]

ERWIN: But he actually worked at the Rad Lab [Radiation Laboratory at MIT] under DuBridge?

COHEN: I think he was no more than a visitor at the Rad Lab, because he was a major player in England in radar. Bowen worked in England as a manager of some sort in their radar development program. But he made a lot of visits to the Rad Lab. There was very close contact. He knew DuBridge. He had some position, probably not as high as DuBridge. I've lost track of who did what. But he certainly knew DuBridge, because there had to have been a very close liaison between England and America in this radar development work.

ERWIN: How is it that England took the lead in radio astronomy? Was it because of the radar development during the war?

COHEN: That's interesting. If you talk to Australians, they wouldn't put the question that way. They were always annoyed when people said, "Well, interferometry was developed in England, and Ryle got the Nobel Prize." Because it was developed in Australia, too. Nearly all the inventions that were made in England were also developed in Australia. England was closer to the center of the world than Australia is, and got much more publicity. And Martin Ryle was particularly forceful in having his own way of doing things. There was a famous fight that went on for a couple of years [mid-1950s—ed.], which Australia won—over the density of radio sources in the sky. And Ryle went down screaming, as it were.

ERWIN: Well, what were these Australians asserting?

COHEN: That Ryle was wrong. There were surveys of the point sources in the sky. There was the second Cambridge Survey, which found I forget how many sources—a thousand or whatever [1,936—ed.]. And the Australians made a similar survey at a somewhat different wavelength, but the two could be compared. And in the overlap region, the Australians claimed that many of Ryle's sources were wrong. And the Australians turned out to be correct. So Ryle actually lost on that score. It was a very famous fight.

ERWIN: There was definite competition here?

COHEN: There was certainly competition between the interferometrists in Australia and England. Another very fancy technique in interferometry was to use the rotation of the Earth. In Australia, this was developed under the name "Earth rotation synthesis," and in England it was developed under the name "aperture synthesis." But it's the same thing. There were some very bright, clever people who had been very close to things like this during the war, working on radar, and they were able to develop it. In Australia, they may still use the old Australian term. But all the rest of the world thinks of it as a British development. But that's not true. It was also an Australian development. A number of firsts come from Australia.

But then it's true that the British took off, with Ryle's telescope—the big one-mile array he built. He did lots and lots of work that eclipsed everybody else in the world, until the VLA [Very Large Array] was built in New Mexico. And then that wiped out the British.

ERWIN: So what you suggest is that it was kind of a combination of luck and strategy that put the English, in particular, ahead in the early years.

COHEN: Yes, right. Things were done in France at the same time. But that turned out to be at a much lower level and they didn't get anywhere near as much done. Also it wasn't supported as well. And the Russian work never got off the ground very well. The Dutch work did—the Dutch have always been very strong. They didn't try to make these elaborate interferometers, but they went straight for the 21-centimeter hydrogen-line development and would have been the first to discover the 21-centimeter line, but they had a fire and their receiver was damaged. But that doesn't matter so much in retrospect—they were there very early. Maarten Schmidt must have told you about that: the receiver that was about to be used to detect 21-centimeter waves got damaged in a fire and set them back some weeks.

In the United States, the 21-centimeter work was done by [Harold] Ewen and [Edward] Purcell. That was a Harvard development. And the Naval Research Laboratory [in Washington, DC] built radio astronomy equipment. Cornell had a solar radio astronomy program. But there was no place in the United States where grand, big developments were made, of the type that were going on in England and Australia. In the early 1950s, the Australians were way ahead in some areas. Work on the sun, for example. What we were doing at Cornell when I got there—I found out very quickly—was really quite primitive compared to what was going on in Australia. And in fact, part of the Cornell program was terminated not long afterward. So for solar work, Australia was clearly the world leader. For the big synthesis-type work, England was rapidly becoming the leader. Manchester—Lovell's group—built a series of bigger and bigger telescopes at Jodrell Bank.

MARSHALL H. COHEN

SESSION 2

November 26, 1996

Begin Tape 2, Side 1

ERWIN: I was wondering if we could start off today with any [Richard P.] Feynman stories you may have— especially connected to his legacy at Cornell. Though I know you went there after he left.

COHEN: When I was at Cornell, as far as I can recall I never heard the name Feynman. I knew of him—I mean, I knew who he was. But he had left Cornell before I ever got there.

ERWIN: And he hadn't made himself into a legend?

COHEN: Certainly not like he is here at Caltech. I was not in the Physics Department, so that might have made it somewhat different. But at Caltech, everybody in every department knows everything about Feynman. He's a cult figure. And that was not the case at Cornell. His name never came up.

ERWIN: Did you know him here?

COHEN: Yes. Not well. I didn't work with him at all. I would say "Hello," and he would sort of respond. I know he didn't know who I was, in some real sense. Because once, after he was sick, we were having some affair at our house—students were coming over—and he had come back from the hospital. So we thought, well, as a gesture of friendship and an expression of goodwill, we should invite him over, so he can take it easy for an evening. So he came. And as far as I could see, he had a good time. And Ed [Edwin E.] Salpeter from Cornell was there with his wife. And I remember Mika—that's Ed's wife—saying after the fact, "You know, that guy is tremendous! I hadn't seen him in fifteen years, and he started up a conversation by picking up where we had left off fifteen years ago. He said to me, 'You used to work on spiders.'" And that astounded her—that he remembered from fifteen years ago in a casual conversation that she worked on spiders.

The other thing I remember from that evening is that he spent a lot of time in a room off to the side with some students, regaling them with stories. He was chatting with graduate students for over an hour, and I presume they had a ball.

The next day, or some days later, we met out by the physics building or somewhere, and he thanked me for inviting him and said he had a good time. He said, “You know, I was really confused, because it wasn’t clear who was who.” He was confusing me, he said, with Marshall Hall, who was a mathematician here. So Feynman hadn’t really known who I was. Nor did he know who Marshall Hall was, and Marshall Hall had been here from long before my time. So although I say I knew Dick Feynman, in fact that’s not quite right, because you see, he hadn’t really known me.

ERWIN: So does that kind of imply that he lived in a world of his own?

COHEN: I think he did, absolutely. He was extraordinarily egocentric, and he had a very interesting public face. He put on quite a show.

ERWIN: He fascinated people.

COHEN: Oh, yes. Well, he was always the center of attention. He was a celebrity. He was a persona. He’d walk into a room and everybody would stop talking and look at him. He sat in the front row in the physics colloquia and would terrorize the speaker. And the speakers always talked to Richard. I mean, instead of generally addressing the audience, they almost always aimed their talk right at Dick Feynman. You could see that.

ERWIN: And you attended some of these?

COHEN: Oh, some fraction of the physics seminars.

ERWIN: Well, last time we talked about the beginnings of radio astronomy. And we talked some about the conference that was held jointly by Caltech and Carnegie in ’54. I thought maybe we could pick up again just a little bit of that thread. One of the things I wanted to ask you was, first of all, what some of the outcomes were of that conference—and particularly if it had a connection with the establishment of the National Radio Astronomy Observatory.

COHEN: Well, I must preface my remarks by saying that the 1954 conference was at the very beginning of that year, and that was before I had gotten into radio astronomy. I didn't yet know any of the people. I knew nothing about that conference until many years later.

My understanding of it is that that meeting acted as a catalyst. There were a lot of people worrying over the fact that the US was falling farther and farther behind, year by year, in radio astronomy, which in their view was a dramatic new development. And it was being taken over by people in England and Australia, and we should do something about it. The something that should be done would be the construction of a large telescope. We could do that and leapfrog over a lot of what had been done already and get to a commanding position. There was a general feeling that there should be some sort of National Radio Astronomy Observatory, and this meeting brought together all the principals—that is, radio astronomers but also people like DuBridge. There were people from the East Coast—astronomers, people from the science establishment and government. There were probably even people from AUI—Associated Universities Incorporated, which was a consortium of universities that ran Brookhaven National Laboratory. People got all fired up after this meeting and said, "This is great. We've really got to do it. We see these astounding new results that are coming in from overseas." And out of that grew the National Radio Astronomy Observatory, in some way that I do not understand.

ERWIN: Well, there was a National Optical Observatory.

COHEN: I don't know whether that predated NRAO or not. I think it came later. I don't know when Kitt Peak National Observatory was founded. [NRAO slightly predates the NOAO. Kitt Peak was selected as the first NOAO site in March 1958—ed.]

ERWIN: Who would have been in charge of the NRAO?

COHEN: Well, the model—I'm sure—must have been Brookhaven, which was operated by AUI as a nuclear physics laboratory. Brookhaven worked for the Atomic Energy Commission, of course. It got its money from the federal government, and the AEC defined the programs to probably a very large extent. But the operation was run by this university group, and I guess that was successful. That same group, then—the AUI—took on the task of setting up the National Radio Astronomy Observatory.

The first thing that had to be done was to find a site, a place to put it. It was decided in some way that the observatory had to be fairly close to Washington, in order not to offend the politicians or something. They did a site search in the mountain valleys. And after a couple of years [December 1955—ed.], they settled on Green Bank, in the Deer Creek Valley, in Pocahantas County, West Virginia. A very pretty rural area, very isolated. It's surrounded by many ridges of the Allegheny Mountains, to the east and west. The nearest town was Elkins, forty miles away. Green Bank itself was a village with a gas station and a store, and nothing else but farms.

ERWIN: Now, did that remain, and does it still remain, the only National Radio Astronomy Observatory?

COHEN: The headquarters of the NRAO moved from Green Bank to Charlottesville, Virginia, some years later. What happened was that a scientific staff was assembled. They went through a couple of directors at the beginning, and then Dave Heeschen became director. And the place really prospered under Dave. First they brought in an eminent American astronomer [Otto Struve], who retired a couple of years later, and then Joe Pawsey, from Australia, who never really did anything because he got sick and died. And then Dave Heeschen, who was a young scientist working there and had gotten his PhD from Harvard only a few years earlier, became director. And under his directorship, the place prospered.

But Green Bank is very isolated. And as the staff grew, it got very difficult up there. The nearest doctor was a long ways off, for example. And the schools were no good. And families were growing. So they moved down to Charlottesville, on the campus of the University of Virginia, and set up an office there. They were going to move a lot of the facilities and just leave the operating facility in Green Bank. But they were stymied by politics. So in fact a lot of the business end of the NRAO remains at Green Bank, because the senator from West Virginia [Robert C. Byrd] threatened to close them down and cut off their money if they tried to move everything out of West Virginia. And you have to do what the senators say. And so the politics of West Virginia entered in, and that made it cost more for the taxpayer—but that was immaterial, because West Virginia needed jobs. The senator from West Virginia has got all kinds of big deals in West Virginia. There are some big government offices there, and little ones, like the offices of the NRAO, which amounts to—I suppose—some fifty people. But the center of operations of NRAO in some real sense is now in [Socorro] New Mexico, in spite of the politicians in Washington, because that's where the main observatory is now—the VLA.

NRAO has many branches. They have the offices in Charlottesville, where the director is. They have the field station in Green Bank, which also has some of the financial offices. And that's now prospering, because the navy is doing things there. And then there is a branch station in Tucson, Arizona, with a radio telescope on Kitt Peak. And the Socorro office not only has the Very Large Array—which is up on the plains of San Augustine, about thirty miles away—but also the VLBA, the Very Long Baseline Array, which a few years ago became operational.

ERWIN: So these are all considered national facilities?

COHEN: Those are all national facilities. And right now, Green Bank is getting a very new, large telescope—the so-called Green Bank Telescope. That, again, is a political matter. They had this old telescope, which was built as a temporary telescope while they thought about how to build a big new modern one that was totally steerable. This was a 300-foot dish, which was a somewhat specialized device. It was supposed to be temporary, but it lasted a long time. And it fell down one night ten years ago, or whenever it was [November 15, 1988—ed.]. Nobody was killed—it just collapsed. The supports failed. And that was the end of it. And that was during a period when there had been a lot of discussion, sponsored by the NSF [National Science Foundation]. There were too many facilities and not enough money. Some of the old ones had to be closed down. I was part of that study. Our operations at Owens Valley were under discussion then. But it was pretty clear from these discussions that the 300-foot telescope in Green Bank was the least used and the least important of all of the ones we were talking about. I shouldn't say "least used"—all of these were used, all the time. But it got the least votes. So it was the least needed, even though some people swore it was vital. Probably it would have been closed down in due course, in some orderly way, to save a little money. But it collapsed, in the middle of these discussions. And immediately the same senator from West Virginia said, "You've got to build it up again," and produced $75 million to build a new one. Nobody had asked for it; he just demanded that it be done—and, again, if we didn't build a new one, they would close the whole place down.

Of course, people like to build new things. So this grand, gigantic new telescope is going to be ready in another couple of years—I don't know what the schedule is. But it was the result of an accident, and at the insistence of the senator from West Virginia.

ERWIN: Well, we should probably talk about the Arecibo Telescope. And I think chronologically that would come next.

COHEN: That's next chronologically. When I first went to Cornell, I got involved with the solar radio astronomy program. This was a program that was cooperating with other stations around the world to measure solar bursts, and the work had been going on for five years, or something like that.

ERWIN: Who was leading that work at Cornell?

COHEN: Well, Charles Burrows had set that up. He was the director of the School of Electrical Engineering. Bill Gordon was also involved with it. And then there were people who left before I got there. Ralph Williamson, a Canadian, who went back to Canada. But especially Charles Seeger, who was somewhat of a character. Nonetheless, he was competent, and he, I guess, built most of the equipment. There was also a guy there named Ed Schiffmacher, who left a few years after I got there and went to Boulder, Colorado.

But Charlie Seeger went to Europe, and the program was left without a person to really run it, because Bill Gordon and Charlie Burrows were senior professors in electrical engineering, and they couldn't do the day-to-day stuff. They had other research programs they were following anyway. So I was hired to do this, and I did. But that was already at a time when these solar programs were fading out. They got less and less useful.

I did get involved with a program that Cornell was starting at the Sacramento Peak Observatory. They set up a microwave receiver, in conjunction with the air force—this had been set up before I ever came to Cornell. And I went to Sac Peak a couple times in the first two or three years I was there. But it never got anywhere, and the air force closed it down. It was one of the worst sites in the country to build a station, because it was on top of a mountain, and it got radio noise from the White Sands Missile Range and other air force installations.

ERWIN: Was that in New Mexico?

COHEN: That was in New Mexico. The air force closed it down and instead built up a solar radio astronomy station at Fort Davis, Texas, which was run by Harvard. And that was closed down, too, fairly recently. They built an 85-foot dish there, and a solar astronomer named Alan Maxwell ran that for years. It shows up later in the story, because it was connected with VLBI [Very Long Baseline Interferometry] and Caltech.

I can't remember in detail why the air force closed down the Sac Peak site. It wasn't producing any data, and maybe they recognized it as a hopeless case. But it was also a very bad site. I don't remember fighting to keep it open.

We were getting money at the time at Cornell from, among other places, the Air Force Office of Scientific Research, and that money was coming typically through the Rome Air Development Center—Rome Air Force Base, near Utica. We were getting money for the solar work from them.

Kenji Akabane was my first postdoc—except I hadn't hired him; Bill Gordon had hired him. Kenji Akabane retired long before I did. In Japan, they retire when they're sixty. He stayed in Ithaca for about a year and a half, and we were doing polarization work at that time. It was my first foray into polarization. Then he went to the University of Michigan for a year, then back to Japan, and ultimately he became the director of the radio astronomy branch of the Tokyo Astronomical Observatory and worked at millimeter wavelengths. He also kept up solar work. He's since moved to the interior of Japan, and I think he's still teaching. He's younger than I am, but he retired a long time before I did. We visited him once in Japan, but I don't keep up with him.

One other thing about the solar radio work—I was trying to branch out a bit, and I had decided to build a Christiansen Interferometer—so-called—which was an array. This was my first foray into interferometry—in this case, an array of Yagi antennas of the type used for television reception. We bought sixteen Yagi antennas and put them on telephone poles at our field station near the Ithaca airport. And we followed the prescriptions that W. N. Christiansen, in Australia, had set down on how to make this thing work and what to do with it. We never quite got it to work, but that was something I worked on for a year or so. It was built with George Peters and others at the lab. It must have been a question of impedance matches. There's a lot of trickiness connected to that. It took either more diligence or more cleverness, but I was probably trying to do too many things at one time. I was teaching then and running the solar radio astronomy program. A few years later we set up a new lab at a quiet site, in Danby. I did the site search for it—we ran around Tompkins County in all the little hidden valleys. And there's many of them, because it's the north end of the Appalachian Mountains, and there's lots of hills—not mountains, but hills that are a thousand feet high—with tight valleys. I got to know Tompkins County. It's very rural as soon as you get a bit south of Ithaca.

But back to Arecibo. In 1958, Bill Gordon had the idea for building a large ionospheric backscatter system. While I was at Cornell, most of the radiophysics people were doing ionospheric physics, or atmospheric physics, and I got involved with that. So I knew a bit about what was going on in the ionospheric world. I never actually got into any of the experiments, but I went to all the seminars and did a lot of talking and read up on it. And indeed, I applied successfully some of the ionosphere theoretical ideas to the sun and wrote some papers a few years later, when I was in Paris. I'll come to that later.

So Bill Gordon developed this idea, and he floated it in seminars. It was talked around the department. The idea was to build a very large dish on the ground and shoot a radar beam up into the sky and get echoes from the ionosphere, which is up at a hundred kilometers or so. And then you analyze those echoes, and that can tell you something about the density and temperature and composition of the ionosphere.

Bill Gordon made a famous mistake in the calculation. He assumed that the electrons would all scatter incoherently, and that the net power in the echo would be the sum of all the individual echoes. That was correct, to within a factor of 2. But he also assumed that the echo would be controlled by the Doppler shift from the individual electrons, which gave a wide spectrum. That was wrong, as shown experimentally by Ken Bowles later that year, and shown theoretically two years later by a number of people. The scattering is actually controlled by the ions not the electrons, at least, in the normal regime of scattering in the ionosphere where the wavelength is much longer than the Debye length. The result is that the spectrum is much narrower than was calculated by Bill, and that translates into a much larger signal strength. The fact was that the ionosphere experiment could be done with a much smaller dish, a 100-footer would do the job. I recently puzzled over this situation. Within six months of Bill's calculation that 1,000 feet was needed, an experiment had shown that 100 feet would do the job, yet a year later ARPA gave the money to start construction of a 1,000-foot dish. Why? With this question in mind, I looked into the origins of the dish, and now I am writing a historical paper on it. I have made a lot of progress, but some questions remain.

The dish was sold as a general-purpose instrument, to do planetary radar and radio astronomy in addition to ionospheric physics. And, it would study the ionosphere in much more detail than was originally reckoned. This last was very important to ARPA, the new military agency to coordinate space research, because ballistic missiles traveled through the ionosphere, and everything about that environment was of interest. Remember, this was in the Cold War, just after Sputnik, and money was flowing freely.

So the 1,000-foot dish was built, and with regular upgrading, has been an extremely important feature of radio science for forty years. Bill's mistake was fortunate. Without it there probably would not have been an Arecibo Observatory. [Three paragraphs above written by M. Cohen to replace portion of original transcript.—ed.]

ERWIN: Was the Arecibo Telescope built as a result of research that was going on at Cornell specifically?

COHEN: Yes. It grew out of the ionospheric research at Cornell, not the solar radio astronomy work. It wasn't ever intended to study radio waves from the sun—it was going to be an ionospheric echo device. That was its original name—the Arecibo Ionospheric Observatory. But I suggested that it could be used to get echoes from the planets, and I was the first to calculate the echo that you might get from Venus or Mars. So that was a good thing. But it meant that the location had to be far enough south so that you could see the planets overhead. Originally, this was just going to be a stationary reflector on the ground, pointed vertically overhead to get echoes back. So if you want to study the planets, it has to be located in the south. And in order to do anything reasonable, you would also have to steer the dish. So the whole idea of it being down south and steerable grew out of the plan to look at planets—to do radar on planets. Then we thought, Why not do general radio astronomy? And that became part of the idea from the beginning also.

So, during the late 1950s there was a great deal of study at Cornell about how this telescope could be built. And Bill Gordon was in charge of the study. He was the PI [principal investigator]. And we wrote up various proposals and reports on scientific uses of it, and then a proposal was written and sold to ARPA—the Advanced Research Projects Agency—for a very modest sum of money, something like $8 or $10 million.

ERWIN: It had to be in the South. So who selected the site?

COHEN: Well, there was a site search. And there were professors from mechanical and civil engineering who were involved in this. So it was very substantially an in-house effort. In that sense it was like LIGO [Laser Interferometer Gravitational-Wave Observatory]. Well, in terms of its civil engineering, LIGO certainly had outside consultants, as did Arecibo. But I think in the beginning LIGO was all done in-house, by the people who are in Bridge—they did all the design and so on. Of course, for Arecibo, contracts were let for the civil engineering and for the construction of all the different parts, and for the radar set. But the receiving electronics were built in Ithaca.

The site search was very interesting. It settled fairly quickly on the Caribbean islands. Maybe Florida was looked at for a while. But the Caribbean islands—and the Philippines, I remember—have what are called limestone sinkholes. There are limestone caves, and their ceilings fall in. And it leaves a great big depression—typically with a drain at the bottom and an underground river or lake. There's quite a few of those sinkholes in the Philippines and in Puerto Rico and Cuba and I guess other Caribbean islands. The Philippines were immediately dismissed as being too far away. And then there was a discussion about Cuba versus Puerto Rico—this is pre-Castro. Well, Cuba's closer to us, of course, but Puerto Rico, being a part of the United States, seemed like the better deal. So Puerto Rico was chosen. It's good it wasn't Cuba. And several sites in Puerto Rico were looked at before they settled on the one near Arecibo. And the fact that it was a sinkhole meant that there were a million cubic yards of Earth or something that did not have to be excavated. It already was a natural dish. All you had to do was smooth it out. That's not exactly right, but it saved a tremendous amount of construction cost, to put the dish inside a sinkhole.

ERWIN: When you say on the ground, you mean literally on the ground?

COHEN: No, no, it's not on the ground. It's built on posts. It's sitting on posts and hanging from wires. The main dish itself, the reflector, was originally fairly open mesh, because the radar was at 430 megahertz. That frequency was picked because they were using the big klystron transmitters the military used on the DEW Line—the northern radar line that stretches across Greenland and Canada and Alaska and was supposed to pick up Russian missiles. The transmitters were already being built for the military, so we bought one—from Varian. So that set the frequency, and the dish had to be built to accommodate that frequency. The higher the frequency, the more it costs, and the price goes up rapidly. At the beginning, it was "hardware cloth," an open mesh. You could look right through it—but I now do not remember the size.

ERWIN: Well, was it reconstructed?

COHEN: Oh, it's been rebuilt and reconditioned several times. But the original was open mesh, hung on big cables that were 100 feet apart. This thing's 1,000 feet in diameter. There's ten cables going across it—bridge cables an inch in diameter. And then perpendicular to them there was a series of small cables that were maybe twenty feet apart. The mesh was laid on top of these small cables and just pinched down, so it sagged in between. But all the distortions were calculated, so that the sag would be tolerable for the highest frequency we were using. The cables individually could be adjusted at the ends, and they were all held up on posts and had tie-downs underneath. So you could walk around underneath it. Some places it was very close to the ground, a couple of feet. Other places, there was a 20- or 30-foot gap. It depended on the contour of the terrain relative to the spherical reflector.

ERWIN: And you said it was steerable.

COHEN: Well, the dish wasn't steerable, but an overhead structure held a moveable feed, and that steered the beam. The dish itself is bolted to the ground; it doesn't move. You can drive down underneath it—there was a dirt road. Right at the bottom was a hole, and when it rained, the water ran down that hole and disappeared. There's a well-known river under there that comes out above ground, about ten miles away.

ERWIN: It sounds like the site would have been rather dramatic. Just to see it would be an amazing thing.

COHEN: Oh, it is. The site was very dramatic. It was wooded. There were small farms. There was a trail that went through this site.

ERWIN: And this was the biggest radio dish built to that time, right?

COHEN: Still is the biggest radio dish ever built. There are other collecting systems that have a comparable collecting area, but they're arrangements of small dishes. No one will ever build a dish that size again, because with modern electronics you can synchronize small dishes and use them together. And a small dish is much cheaper than a big dish. And the electronics are cheap now.

Another major person I haven't mentioned who was involved in Arecibo was Henry Booker, who, when Charlie Burrows retired, became the head of the School of Electrical Engineering. And he was in charge of the group that was designing the Arecibo feed, or rather, in charge of the monitoring of the feed design. A commercial company designed the prime feed.

Construction began around 1960-'61. I had a sabbatic leave that year, and I went to Paris, on a Guggenheim Fellowship. And while I was there, I was doing theoretical work almost entirely. I wrote some papers on plasma physics and on the solar atmosphere—they came out later. One of them, for example, was on the escape of radio radiation from the sun, and another concerned the limiting polarization that you get on solar bursts. That, again, was an outgrowth of ionospheric work. The ionospheric physicists had studied the polarization of the signals coming back from the ionosphere, and their ideas were germane to the radio bursts coming from the sun. So I could take the ideas I had learned from the ionosphere people and apply them to the sun. One paper, which was published in about '63, still gets referenced today. It's quite remarkable. I saw a reference to it not too terribly long ago.

ERWIN: How much time did you spend at the Arecibo site?

COHEN: We lived there for a year. Well, my first visit was sometime after I came back from Paris. I made a number of visits, and then I decided I wanted to go down there and spend a year. So that was organized. They were encouraging people to go down there—graduate students and professors. Bill Gordon had moved down there to be in charge of things.

An interesting point is that Bill Gordon, besides being the PI, was also in charge of construction. He was on top of everything. There was no project manager, other than Bill Gordon. And that was too much. And in a way that I don't understand, ARPA forced Cornell to hire a project manager—not totally different from the LIGO situation, when Robbie [Rochus Vogt] couldn't be project manager anymore—the federal government forced Caltech to get a new project manager, because they weren't happy with what was going on.

What happened at Arecibo was that a professional guy, from Brookhaven I think, was brought in—a guy who had done a lot of big construction jobs. So after that, it operated the way the Keck Telescope construction operated—with a project manager, who was not a professor doing it part-time, but someone who had been a long-time construction boss.

ERWIN: And did that work better?

COHEN: Yes, sure it did. Well, I don't know. I was a lowly assistant professor—or I became an associate professor in there somewhere [1959—ed.]. And I don't know the ins and outs of it. I don't know how ARPA leaned on Cornell to force this. But ARPA was providing the money, so they could do it. So Bill was no longer the project manager, but he was still the PI, the scientific head of it.

I went down to Puerto Rico with Shirley and our three children—one a baby less than a year old. That was in 1963. We stayed there I think for nine months. The telescope was just being brought into play, and of course it wasn't working very well. But we were getting some real results with the ionospheric experiments. [Tape ends]

Begin Tape 2, Side 2

COHEN: So during that first year, we got some ionospheric results, and that was dramatic. And toward the end of that year there was some successful planetary radar work, I believe. There was no useful radio astronomy work. That first year, Elio Kazés from the Paris Observatory—someone I had met and befriended when I was in Paris—came to Puerto Rico with his wife. So we were working together. We realized that there was something wrong with the shape of the beam. The beam is controlled by the so-called illumination; the distribution of electric field over the aperture. Overhead there's a long line feed—the primary focal device—and it gets reflected off the big dish. And there was something wrong with the illumination. So I engineered a measurement of the aperture distribution by inventing a little cart with a transmitter on it, and we received its signal with the primary feed. With this we could measure the phase distribution on the reflector, and then calculate the phase over the aperture. We spent several days measuring this aperture distribution. And we found out that some piece of the aperture was out of phase. The dish is a gigantic thing, many acres, and some acres were out of phase with the rest of it. It's eighteen acres altogether, and my recollection is that seven acres were out of phase. That explained why we had low gain on the axis, and high side lobes.

Ron Bracewell, from Stanford—he's now a retired professor of electrical engineering—was chairman of a scientific advisory committee and spent some time in Puerto Rico. He took the data we had gathered on the aperture illumination and figured out what was wrong with the feed. The feed was a square-cross-section tapered waveguide, and it was like a needle in the sky pointing down. It was about twelve inches on a side at the top, and it tapered to something smaller at the bottom. It was a tapered, square waveguide, with slots on the four sides, and it generated circular polarization.

The problem was that the slots were too far from the axis of the spherical reflector. Bracewell figured all that out and decreed that the only cure would be a line feed with slots much closer to the axis. And I guess it was on the order of ten years before that was completely fixed, so the system operated at reduced efficiency for a long time.

Now, that was okay in many senses for the ionosphere. It was merely a low-efficiency system for planetary radar, but it was deadly for radio astronomy, because the beam was bad. Instead of getting a single trace, as it were, you would get multiple traces. However, as an intermediate step, a thin tapered waveguide feed was built, operating in the TE01 mode. The slots were close to the axis and it worked well for radio astronomy, but could not be used with the high-power transmitter and could not be used for ionosphere or planetary radar experiments.

ERWIN: So, with the original feed, you didn't know what the source was?

COHEN: Yes, that's right. With the original line feed you had terrible confusion, so for many kinds of radio astronomy work it was useless. But around that time, the phenomenon of interplanetary scintillations [IPS] was discovered in England. The IPS produced rapid time changes, which we could measure accurately. So for a while, most of the radio astronomy work that was done at Arecibo was a project I was doing on IPS. I don't now remember when that began; it must have been in 1964. It went on for a number of years when I was in Ithaca. And that was my first dealing with a large radio astronomy group. There were several postdocs working on that: Ellen Gundermann, Dan Harris, and Harry Hardebeck—who was a graduate student but also worked on IPS. We worked closely with Ed Salpeter, a very smart theoretical astrophysicist at Cornell. Ed wrote a theoretical review of IPS and explained a lot of the phenomena to us. We learned a lot from Ed—astronomical facts, of course, but also methodology, ways to think about and to organize an investigation. I also worked on IPS a little with Frank Drake, but that was not successful.

So the Arecibo experience was very interesting. We would run back and forth every day from the city of Arecibo to the observatory. If you talk to Shirley, you'll find out she had a bad time there and claimed it was a bad experience. But I learned a lot. Commissioning a telescope is a very interesting thing to do. You're on the spot when it's starting up, which is when the engineering problems are the most interesting. I did a little bit of that later at Keck and at Palomar, when I built my own little instruments and installed them. I had to commission them and find out why they didn't work. So I've always been a tinkerer in that sense.

ERWIN: Does it always not work the first time?

COHEN: Yes. [Laughter] Well, sort of. Something or other doesn't work the first time. There are at least adjustments needed, if not fixing.

We lived in Arecibo for about a year—nine months I think—and then we went back to Cornell.

ERWIN: I see that you left Cornell to go to San Diego.

COHEN: Yes, in 1966. That was about two years later.

ERWIN: How did that come about? Are we at the right place to tell this? Is there more you wish to say about Cornell?

COHEN: Well, there is. Somewhere in there, there was a big fight at Cornell between Tom Gold and Bill Gordon over who was going to control Arecibo, and ultimately Bill Gordon was forced to come back to Ithaca. Tom Gold had come to Cornell in the late 1950s as the chair of the Astronomy Department. He was building up an organization called the Cornell-Sydney University Center for Radiophysics and Space Research. His counterpart in Australia, Harry Messel, was as big a character as Tommy was. They were both very dramatic, extraordinarily egocentric. They wanted things their way. They were very clever, both of them, and big operators. They knew a lot of people and could produce money. Gold who was born in Vienna and went to England during the war and was an astrophysicist, was known for being involved with the steady state cosmology with [Fred] Hoyle and [Hermann] Bondi. He went to Harvard, where I gather he didn't get along very well. Then he went to Cornell. I forget what year he came to Cornell, but in the late fifties, maybe the early sixties. [Gold came to Cornell in 1959—ed.] Cornell had little attraction for him, other than the fact that he could build things up. It had a very good Physics Department, but it had almost no Astronomy Department. The Astronomy Department at the time was run by a guy by the name of Bill Shaw—an old cantankerous guy who would scream on occasion because he was paranoid and thought people were taking him over, which in fact they were doing. I was involved in the Astronomy Department because I was teaching some course over there on radio astronomy part of the time.

Gold thought the fact that Arecibo was being built was very advantageous. He was also interested in ionospheric physics. He was interested in a very many things and was a very clever guy, seriously disliked by the geophysicists here at Caltech for the outrageous things he has said.

Tommy won that argument with Bill Gordon. The observatory was put under the thumb of the center that Tommy was the director of. Bill Gordon got mad at all this in 1966, and he left and went to Rice University as dean of engineering and science. He retired some years ago. I think he's almost ten years older than me, so he's almost eighty now and still in good health. In later years, Bill Gordon became busy in the affairs of the National Academy of Sciences and was the Foreign Secretary of the NAS for five years, which is a big position. That was after he had retired from Rice.

After Bill Gordon left, Frank Drake became director of Arecibo. Frank also fought with Tommy. Frank left after ten years of being director. In many ways, he did a good job. But his problem—that's a whole separate matter—was a question of whether the observatory was doing enough for Cornell. I had left Cornell by this time. But at any rate, the result of all that was that Frank Drake left too, and went to UC Santa Cruz as a dean. So people had their difficulties with Tom Gold. He was a difficult man.

Anyway, when I came back from Arecibo, I transferred from electrical engineering to astronomy—to the consternation of some of my colleagues in electrical engineering, who claimed I was joining the enemy, because they regarded not the Astronomy Department but Tom Gold and his center and his connection with Australia as the enemy, because he was taking power and prestige away from electrical engineering; and that Cornell-Sydney University association brought people from Australia—postdocs—to Cornell. That was the main thing it did, as far as I was concerned. Tommy also made lots of trips to Australia, and Harry Messel would come to Cornell.

ERWIN: Was this your choice, to move into astronomy?

COHEN: Oh, yes, I engineered that. I had to talk to several deans. One or two of my friends thought it was a bad thing to do, but I did it. And why did I do that? The teaching loads were lighter there, and the people I was really getting involved with, because of working at Arecibo, were there. The whole center of my action was over there, it seemed. Tom Gold was chairman, which was okay, but it also had its difficult moments.

Henry Booker, who was head of electrical engineering, went to San Diego in 1965 to start a new department. UC San Diego had started with a great fanfare a couple years earlier. It was going to be the Caltech of the UC system. They had assembled two colleges already, and they had a world famous Physics Department and Chemistry Department. So they had these very powerful science departments already and they were setting up a new department, which was sort of electrical engineering but not exactly. So they haggled over the name. And they decided to set it up as the Department of Applied Electrophysics. The word "Physics" was difficult, because of the Physics Department. But if you put "Applied" in, it was okay. But the word "Engineering" was no good. Anyway, that name has changed several times.

So Henry Booker went there and brought in people, and I knew most of the people he brought in. There were computer and information sciences people—that was a fairly new subject then. They brought in a man, by the way, in that department who has since left and founded a company and is now a big entrepreneur—Irwin Jacobs, who founded several companies in San Diego and has done very well.

So one branch of the department was computer sciences and information sciences, and another branch was radiophysics, as interpreted by Henry Booker. Radiophysics at that time generally meant atmospheric physics as studied by radio means: ionospheric physics—the kind of thing they did at Arecibo—and it could include radio astronomy.

Radio astronomy, in the United States at least, was variously organized. At some places, it was in electrical engineering, some places within astronomy departments, some places it was separate. Caltech, of course, had started radio astronomy before this. OVRO [Owens Valley Radio Observatory] was already up and running. In the places where radio astronomy was organized within electrical engineering, it didn't prosper as well—at Illinois, for example. Cornell was another case; radio astronomy hadn't gotten very far when it was in electrical engineering, but some years later the astronomy part of Arecibo was run entirely from the Astronomy Department and was doing well. Arecibo was a big operation—it was a very big observatory with a big budget and lots of people in three divisions within it. Two of those divisions were run by the School of Electrical Engineering, and one was run by Astronomy—that was the radio astronomy side.

So at San Diego, Henry Booker already had Ken [Kenneth L.] Bowles, an ionospheric physicist, who had been working at the National Bureau of Standards, in Boulder. And there were other people—a very good group involved in information sciences.

Also, I was getting close to forty years old. I really liked Ithaca. It was a very nice town, and I liked the countryside. But if we were ever going to move and go anywhere, it had to be now, because in another ten years I'd be too old. There were the questions of unhappiness within the Astronomy Department—that is, it was hard to get along with Tommy Gold. In fact, I remember being called in by the dean of arts and sciences at Cornell as I was leaving. "Well, why are you leaving?" "Well, it's time to leave. I just felt that I should go, and go with Henry." Henry had gone a year earlier, and I was very close with him. "Well, were there any problems here?" And I said, "No." But of course there were, in some sense. I wasn't entirely happy.

Anyway, I thought it would be interesting to go to San Diego. I could start my own activity there and I could continue the work I was doing at Arecibo, which takes visitors, after all. So we decided to move to San Diego. I remember that Henry Booker made trips back and forth—presumably he couldn't come to Cornell and recruit. We were talking about it. And I went to San Diego to visit. And, of course, I went in February or something, when the sun was shining, and we saw nothing but clouds for three months in Ithaca. So that made a good impression.

At that time, I talked to my old boss from the Antenna Lab at Ohio State, Vic Rumsey, who by then had moved to Berkeley. I told him that Booker was setting up this organization, and if he could come also it would be great—there'd be him and me and Ken Bowles and a couple of others, and we'd have a large group, where we could work together. And it would provide a chance for a dramatic new beginning. Vic Rumsey took up that idea, and left his place in Berkeley and went to San Diego. And I wondered in later years whether he regretted that. Because he ended up having a fight—or at least a severe disagreement—with Henry Booker, which must have led to a lot of hard feelings and maybe recriminations, I don't know. He never expressed them directly, although I do know he was on the outs with the boss. Then he retired and moved up to Sonoma County, one ridge inland from the ocean.

So I went to San Diego, in 1966. And that lasted only two years. In 1965, I had spent a month or three weeks at Caltech. That was a time when they were looking to hire someone to be the director of OVRO, because John Bolton had left a few years earlier. Gordon Stanley was first the acting director, and then became director. But people weren't totally happy with that and thought there should be some well-known scientist as director. George Field had been invited out earlier, but I don't know whether they ever actually offered him the job or not.

So they were trying to build up the scientific staff. So I came and spent a few weeks here and also met the people in San Diego.

ERWIN: Were you a visiting professor here?

COHEN: Well, I don't remember what I was called—visiting associate or visiting professor [visiting associate professor—ed.].

ERWIN: Did you give lectures?

COHEN: Well, I certainly didn't give any classes. I probably gave a couple of seminars or something—talked about Arecibo. I don't remember what I did, to tell you the truth.

ERWIN: Did you know at the time that you were being looked over?

COHEN: Well, no. But what happened is that in 1967 the first VLBI results came in, and I was one of the prime movers in that. So I got some notoriety. And I got a letter one day. I remember hearing about it first from Alan Moffet. I was doing some things at the Owens Valley Radio Observatory—that is, from San Diego I had a connection with Caltech. So I had some stuff I was doing at OVRO, and I was driving up with Alan Moffet, who was an assistant professor at Caltech at the time. We got partway up Owens Valley and he said, "Well, you know you're going to get a letter from Carl Anderson"—Anderson was the chair of PMA [Caltech's Division of Physics, Mathematics, and Astronomy] at the time—"offering you a job." And I remember uttering some crude remark—I said, "Well, you know, it's only a year since I moved to San Diego. You could have done this a year ago and I would have come here and it would have been easy."

So I did get that letter. I'm not sure how that was engineered. Jesse Greenstein organized it, but I don't know the details of how I was hired. In those days, there weren't big selection committees and affirmative action and all that stuff that's done now. I don't think you even had to advertise. It was done very informally. They decided they needed someone where there was a slot, and they asked their buddies around the country who was good. So they picked on me. And Greenstein, I know, had asked various people around the department, because I talked to Hal Zirin about it. Zirin had come to Caltech only a couple years earlier. And I had worked with Hal Zirin a little bit, because I knew him from my solar physics days. I had spent some time at Boulder.

ERWIN: Where did he come from?

COHEN: When he came to Caltech? The High Altitude Observatory, in Boulder, Colorado.

So I don't know how it was all done, but somehow Greenstein went to Anderson, and Anderson said, "Well, that sounds good." And they talked to [Caltech provost Robert] Bacher. And Bacher said, "Are you sure this guy's okay?" And Greenstein said, "Yes, I'm sure." And that was the end of it, as far as I can see. Maybe there was more than that, I really don't know. I guess there was more than that. But you'd have to ask Jesse Greenstein or Bob Bacher.

ERWIN: Was it hard for you to disengage?

COHEN: Well, I got an offer. And I thought about it for a while. It seemed as if the opportunity here was tremendous. The point is, Caltech needed more radio astronomers, because Alan Moffet and Gordon Stanley had written a proposal for the so-called OVA—the Owens Valley Array—and submitted it to the NSF around 1967. And at the same time, the NRAO was proposing to build the VLA. And Bernie Burke at MIT was promoting a very large dish—a 400-or-so-foot dish. Bernie [Bernard F.] Burke was a professor of physics at MIT. So there were three rival proposals—big radio astronomy proposals—that were now floating up, and Caltech had one of them. And these were all coming to the NSF. It was clear that there was going to be a struggle over this. And Caltech was in a very weak position, because it had almost no people.

I was by now over forty, so I was a person the NSF could trust. Whereas Alan Moffet was quite a bit younger, and Gordon Stanley was a lightweight in these matters. MIT and NRAO had lots of people—so in order for the OVA to be credible in Washington, Caltech had to have a stronger radio astronomy group. So they hired me. I had some reputation, because of the VLBI business, I guess, which had made quite a stir in 1967.

ERWIN: On OVA just for a minute—the idea of building arrays and building large arrays, when did that occur?

COHEN: Oh, that had arisen already in England and Australia.

ERWIN: How far back?

COHEN: Oh, the first English array, at Cambridge, must have been in the late forties. And simple interferometers were built in Australia in the late forties. And then the Australians specialized in these very complex Christiansen-type arrays, the one that I couldn't duplicate in Ithaca. They built several of those. And it's only more recently that the Australians have gone to a more open array, where you have dishes that are widely separated. The British were the first to use separated dishes to any large extent—more than two at a time.

ERWIN: When did that first happen in the US? Was this the time when it was being considered?

COHEN: The first array in the United States—the first interferometer in the US—was at Owens Valley. It's the twin 90-foot interferometer, built by John Bolton and Gordon Stanley.

ERWIN: How many dishes do you have to have, to have an array?

COHEN: Well, I'm calling it an array—there were two. The thing that was built in Green Bank was two also, but then a third one was added. If it has two elements it is called an interferometer, but with three or more you can call it an array. The Owens Valley Array was planned to be eight. And the first of the eight was funded—Caltech got money from ONR [Office of Naval Research]. The ONR gave money to Caltech around 1964, and Westinghouse built the telescope at Owens Valley. And it was dedicated just after I arrived, in '68. I don't know when construction started, but from the beginning it was thought of as the first of eight—we'll build one and test it and use it and see how it needs to be modified; and then we'll build seven more. And that will be our eight-element OVA. The concept was very early, and it was developed by Gordon Stanley and by Alan Moffet. Moffet was brilliant.

ERWIN: He was at that time perhaps even a graduate student?

COHEN: He would have been a postdoc. Well, he might have had the idea earlier, as a graduate student. He had lots of ideas—a very difficult guy, very intolerant of people who were slow on the uptake. But brilliant, one of the smartest people I've ever known, with an incredible memory. He died, as you know, of AIDS, early in the AIDS epidemic. I remember hearing about that from Robbie Vogt; I was just totally blown away, I couldn't believe it.

So, when I came to Caltech, there was a single element of this proposed OVA. And the fact that they were promoting the OVA was one powerful reason, certainly, why I was hired. They had to get somebody on board to show the NSF that they had more than one young assistant professor to work on it. In fact, before I ever came, one of the first things I said to Carl Anderson the first time I spoke to him was, “What are you going to do with Alan Moffet? He’s got to get tenure and be promoted to associate professor. You can’t leave him like this.” And one or two years later, he was promoted.

ERWIN: So you had to work pretty closely with Moffet.

COHEN: Oh, yes, continually. I was very close with him for years.

ERWIN: And it sounds as if you got along reasonably well with him.

COHEN: He snapped at me occasionally, but I didn’t mind. He did snap at people. So that made him kind of hard to take. He was very difficult—really intolerant of people who were phony in some sense. And here, again, he got involved with the NSF, and NSF had to tell Caltech to get rid of Moffet as director of OVRO—this comes much later—because Moffet couldn’t tolerate the NSF people. And he was able to control himself less and less as time went on. Robbie Vogt did the same thing, I believe; he blew up at the NSF. Moffet blew up at the NSF. I don’t know what he called them, but anyway the NSF couldn’t deal with it. And there were a couple of people that Moffet was particularly annoyed with. When he became director of the observatory, he had to deal with the NSF, and it didn’t go down well. And the NSF sort of forced the issue. Robbie Vogt was the one who took him off, of course, but it was done because the NSF must have pressured him.

ERWIN: What time period are we in?

COHEN: Oh, that’s much later. That was after Gordon Stanley left—that’s a whole chapter of its own.

Now, what you need here, it seems to me, is the VLBI story. That starts at Cornell and it connects with Arecibo and Green Bank and San Diego, and then it moves to Caltech.

ERWIN: Well, just on OVA, there's a document here from the Greenstein collection—just a memo having to do with the fact that it was never funded, and it had to be reduced to a three-element array, which was then renamed OVI [Owens Valley Interferometer].

COHEN: Well, the OVA died, and we never built any more of those big dishes. And it died in some sense under Greenstein, because Greenstein was chairman of the decadal review committee, the Greenstein committee, which met in 1970 off and on for a year or two. And I was part of the panel which helped to kill the OVA.

ERWIN: Was this an internal Caltech committee?

COHEN: Oh, no, this was an NAS committee, which made a report on the state of astronomy and recommendations for the next ten years. Every ten years, the astronomy community gears itself up and does this. The first one was in 1960, and it was chaired by Al [Albert E.] Whitford—who's still alive and just moved back to Wisconsin from Santa Cruz. The 1970 committee was chaired by Jesse Greenstein.

ERWIN: Were you on that committee?

COHEN: Well, not on the 1960 one. But one way or another, I was involved with all the later committees. I've never been a member of the main committee, but I've been a member of one of the panels—there's always a panel on radio astronomy or something equivalent. So in 1970, '80, and '90, I was on these panels, and there was a good bit of squabbling. But the one in 1970 that Greenstein chaired was very bloody, because Caltech and MIT and NRAO were all pushing their own projects. The Caltech and NRAO projects were similar—large arrays, the OVA versus the VLA. And Greenstein, who was the boss, said that one of them has got to come out on top. We've got to recommend one as having priority. So that led to a tremendous amount of fighting and shouting.

ERWIN: How could Greenstein be disinterested?

COHEN: Well, he wasn't disinterested. However, it ended up that Bernie Burke and I gave in at the end. And the National Radio Astronomy Observatory won and out of that grew the VLA. But the thing that Burke wanted at that time is now being built. It's the replacement for the 300-foot dish that fell down at Green Bank. The new GBT [Green Bank Telescope] is the kind of device Burke wanted to build many, many years ago. The OVA—the thing I was fighting for at that time—never was built. In later years, I fought for the VLBA—the Very Long Baseline Array. And that *has* been built. So I was successful. And Owens Valley was successful, because they have the Millimeter Array—which never was fought for, in that sense. It grew incrementally, that's why it was able to grow. First there were two antennas, and then there were three, and then there were four, and now there are six. And over the years, they would sneak in the money from private funds or from the government. And they keep competing with Berkeley, but they stay ahead a little bit and they always got excellent ratings. So it never was such a big deal—never $50 million at a time. It never had the drama and the fight that the OVA had, or the VLBA, which was $100 million at a single crack. I'm not the one to give the history of the Millimeter Array, because I was really quite a bit apart from it. I even struggled with it for a while.

MARSHALL H. COHEN

SESSION 3

January 17, 1997

Begin Tape 3, Side 1

ERWIN: Today I'd like to get the VLBI story. Where does it begin?

COHEN: The VLBI [Very Long Baseline Interferometry] story begins in 1965. It actually was somewhat older than that, in some other countries, but the technology was not available. People thought about long baseline interferometry, but then gave up immediately.

But then in the middle sixties, the Varian Company, in Palo Alto, began marketing commercially a rubidium standard oscillator. And that gave us the idea that we could do something.

ERWIN: How were they thinking to market that oscillator, for what purposes?

COHEN: Well, they certainly didn't have the idea of selling to radio astronomers, because the radio astronomers bought only a few of them, and Varian had a whole plant running. But that's a really interesting question. I don't know the answer. Maybe the oscillators were needed for the Loran stations or for military purposes. I'm not sure.

So there was that technical development, the rubidium standard oscillator. Prior to that time, there were only laboratory devices for accurate timing, and the VLBI required very accurate timing. You needed a clock that kept extremely accurate time—to millionths of a second—and one that put out a very pure sine wave.

ERWIN: The reason you needed the timing was because the observations would be taking place at widely separated stations, so they had to be absolutely accurately timed.

COHEN: That's right. Well, there's a technical limit on what the accuracy has to be. I'll try to explain that in easy words later.

So that was one technical development, which my colleague Ken Kellermann knew about before I did. He may have found out about it by reading ads in magazines. He was an avid radio ham and was interested in that kind of stuff.

There was a second technical development around that time—broadband tape recorders for television, which we could adapt for our purposes. No, I'm wrong. We were using pure computer tape at the beginning. So in the middle sixties, the one technical development was the good clock.

The VLBI uses antennas that are separated on the Earth's surface by essentially the whole Earth's diameter. We've done California to Australia, for example, and others have done Australia to South Africa. These are 8,000- and 10,000-kilometer baselines—that's the straight-line distance through the Earth. So the separation between the stations is a large fraction of the Earth's diameter.

ERWIN: Is there a minimum distance?

COHEN: Well, not really, except it's not interesting to go very small.

ERWIN: At what point is it interesting?

COHEN: Well, it became interesting even at a couple of hundred miles. Another background matter—in addition to this technological development—is that people, especially in England, had been doing interferometry that was connected. That is, the antennas were connected with wires or radio links. This was done inside England, and England is only a couple hundred miles long. They had got up to 140 kilometers, and realized that they still had radio sources that proved to be unresolved; that is, they were very compact. "Unresolved" means that the interferometer response is independent of the length of the baseline. Let me explain.

We're looking at radio sources in the sky, and we want to know their diameter, which is measured in seconds of arc. And some of them were known to be phenomenally small, smaller than a second of arc. So the idea was to try to measure the diameter, or even the detailed shape, using an interferometer. As you move the elements of the interferometer apart, you get more and more angular resolution—that is, a higher and higher degree of resolution, a better measurement. The simplest interferometer consists, say, of two antennas connected with wires. And you can think of them as transmitting rather than receiving. They're connected with wires, so at the center I'd put a source, some kind of a signal generator, and I'd transmit out of both antennas. Radio waves would go out into the sky.

So you should imagine that the radio waves are going off at some angle to the meridian plane. The meridian plane is the perpendicular bisector of the line joining the antennas. So, I can aim this radio beam at various directions in the sky. The two antennas are pointing in the same direction—they're parallel. Think of it as two cones, aimed in the same direction.

Now, what will actually happen is that these cones will overlap. They are both transmitting almost over the whole sky, over some wide range of angle, and these two beams will interfere with each other. That is, you'll get maxima and minima, according to the delay. The signal from one of the antennas will be delayed relative to the other. That is, if the antennas are pointed off toward the right, then the antenna on the left will have a longer path to go in order to get to any particular point where you're making a measurement. So its signal can be out of phase with the signal from the right. If you're at a point in space where the two signals are in phase, they will add. If they're out of phase, they will subtract, and you'll get zero. So what actually happens out in space is that you'll get a pattern on the sky: in other words, if you ran around with a spaceship in space to observe these antenna signals, you would find bands—or fans, basically parallel to the meridian plane—which are alternating strong and weak. In fact, the strong bands would have twice the average signal and the weak would be zero. This is the interference pattern. And it's a fundamental concept in antennas that whatever happens on transmission also happens on reception, loosely speaking. That means that this interference pattern in some sense will replicate itself at the antenna on reception. So if I have an object which is very big, like the sun—the sun's a big object, it's half a degree in diameter—it could cover several of these fans, or fringes. That is, with transmission, you can think of these fringes on the sky as maybe a minute of arc apart, so the sun might cover ten of these fringes. And the fringes are alternating in phase, so the net result of pointing this interferometer at the sun would be that the signal is washed out. You would get no signal. But if you have an object which is very tiny, let's say a second of arc, and the fringes are separated by a whole minute of arc, then as the Earth turns—that's one way of doing interferometry; you leave the antennas stationary and the Earth turns—then the source in the sky will first be on a point of maximum and then it'll be on zero, and then maximum again, and then zero and so on. So as the Earth turns, you can get a sine wave, or some pattern, on reception in the interferometer. You won't get that from the sun, because the sun simultaneously has many maxima and zero, and you're averaging over maybe thirty of these fringes, so there's no change as the sun drifts across the sky. But as a very small point source drifts across the sky, you'll get maxima and minima. And the relative strength of the maxima and the minima tells you the source's diameter—it has to do with the angular spacing between the fringes. The angular spacing is given by a very simple mathematical formula. It's simply the ratio of the wavelength divided by the projected baseline—"projected" meaning perpendicular to the line of sight. The ratio of the angular spacing of the interferometer to the angular diameter of the source controls the relative strength of the measured maxima and minima. Thus, the measured pattern tells you the angular diameter.

ERWIN: So this was understood . . . ?

COHEN: Oh, this was all understood. It was known for a long time. Interferometry was invented in the 1880s by A. A. Michelson. And there were others. So by the late nineteenth century it was known that an interferometer of this kind could measure the angular diameter of a source in the sky. It was used at radio wavelengths immediately at the close of World War II, and it was found that some radio sources were very small. So they made the baseline longer. And what that does, according to our formula, is that when the baseline—the denominator—gets bigger, the fringe spacing gets smaller. The angle between the fringes gets smaller. So you're studying smaller and smaller objects as you make the baseline longer. That's what they were doing in England with the 140-kilometer distance between antennas. They also reduced the numerator of that equation: that is, they made the wavelengths smaller in addition to making the denominator bigger. That made the angle they were studying smaller and smaller, and they were getting down below a minute of arc. But even so, there were still radio sources in the sky that were unresolved. The interferometer showed the same strength with a baseline of 100 kilometers as it did at 1 kilometer. That means that the sources were smaller than the fringe spacing they were getting at 100 kilometers. So then you should try 200 or 500 kilometers. But, of course, you run out of space in England.

ERWIN: So how big were these objects that were unresolved?

COHEN: Well, they were way below a second of arc. We found that out later.

ERWIN: In human terms.

COHEN: In human terms? Well, yes, I remember the first time we wrote this up for the newspapers. We had to use analogies like "the size of a nickel on the moon," and so forth. The smallest of these objects are below a thousandth of a second of arc, which is one part in 200 million. And the moon is 240,000 kilometers away. So think of putting a yardstick on the moon and looking at it from the Earth. The angle that that yardstick subtends is the typical angle of these quasars we're looking at. So they are very small—smaller than a human being on the moon as seen from the Earth. If you took a six-year-old child to the moon and looked at her from the Earth, the angle that she would subtend is about the same as the angle of a quasar as seen from the Earth. It's a tiny, tiny angle. No single telescope could capture that. It takes these special interferometric techniques.

So there was the experimental fact that these sources were very small, and therefore you needed to go to longer and longer baselines in order to study them. And secondly, there were theoretical estimates at the time of what the diameter of these quasars should be. A number of people were working on that. And a consensus was developing that these objects probably were much smaller than anything that could be measured within the borders of England, or even within Europe, because they were thought to be 1,000 times smaller than a second of arc. That was the estimate—100 to 1,000 times smaller. And so you needed baselines that were 1,000 or even up to 10,000 kilometers long.

So there was a somewhat general appreciation that this long-baseline interferometry would be an interesting thing to do. In 1965, Ken Kellermann and I were at a meeting in Ann Arbor, Michigan—a meeting of the American Astronomical Society. Ken always claims that we discussed this over some beer at a bar, but I don't remember that aspect of it, and he may be just making that up. But I do know that it was at this meeting that we discussed how interesting this VLBI would be and the feasibility of doing it, using these new oscillators which were now available. And we talked about it again a little later. And we brought in some of our colleagues and friends to think about it with us.

Kellermann, of course, was a Caltech graduate. He got his PhD here in 1963, then he went to Australia for a couple of years, and now he was working at the National Radio Astronomy Observatory.

ERWIN: And you were still at Cornell?

COHEN: I was still at Cornell. So Ken got his NRAO colleague Barry Clark, another Caltech graduate of about the same era. And Barry Clark was a whizbang at figuring things out—very uncommunicative, but very, very smart. It was almost impossible to argue with him. He would occasionally be wrong, and you might even sort of know it, but it would be extraordinarily difficult to convince him of it. But he was nearly always right. So that's to say, whenever you thought he was wrong, the chances are he was right and you were wrong. So he was a really first-class person to have on our side. Then at Cornell, there was a postdoc who had recently come from Australia—Dave Jauncey. And he began to work on this with us. He had studied cosmic rays in Australia and came to Cornell as a postdoc under the auspices of the Center for Radiophysics and Space Research. He was working with me, and we were involved with Arecibo, and then the possibility of doing this VLBI stuff came up. He was very interested in that, and he joined the group. And we coopted a fifth person—an engineer from NRAO named Claude Bare.

Claude, unfortunately, died about twenty-five years ago. He was living in West Virginia at the time and developed cancer. And I guess it was a long time before it was recognized. As I mentioned, the medical facilities at Green Bank weren't so good—the nearest doctor was forty miles away in Elkins. So Claude Bare is gone, but the rest of us are still here and still involved with interferometry after all these years.

So in the autumn of '65, Ken and I jointly wrote a proposal, which we then gave to our respective bosses. Dave Heeschen was the director of the NRAO observatory. Ken talked about it with him. And I talked about it with Tom Gold, the chairman of the Astronomy Department at Cornell. And these guys both gave the project their blessing and also some money, which we needed because we needed to start collecting equipment and so on.

ERWIN: About how much money at this point?

COHEN: Well, there were salaries of course, which is hard to figure, because people were being paid anyway. But there was engineering time, which is something you *can* figure in. Claude Bare spent probably half-time for two years at it. And then there was equipment to buy—the oscillator, and various other bits and pieces.

ERWIN: The money came from your respective institutions?

COHEN: Yes, there was no federal money at the beginning—except that in some sense it was federal money because NRAO was supported by the National Science Foundation. This equipment might have cost as much as $50,000. I can give you another estimate, because three years later when I came to Caltech I got $70,000 from the Physics Division to set up a VLBI system in Owens Valley.

An interesting sidelight—since this is a historical record and we can put in comments about people: Tom Gold, I think, did not totally trust this operation. Whether he didn't trust me or whether he thought the thing wouldn't work, I'm not sure. But I do know that at one point Robert Hanbury Brown, a very highly regarded radio astronomer-physicist from Australia—originally from England—came by. He used to visit Cornell, and he came around and interrogated me. And I've always had the suspicion that Tommy had been talking to him and had prompted him to talk to me, so that he—Gold—could see if Hanbury Brown thought the thing was okay and was going to work. Because, you see, there was an interesting question at the time, having to do with quantum theory of photons, which goes something like this. The VLBI system is a standard 2-slit interferometer as studied in elementary physics, but with a wrinkle. The two signals are not combined directly, but are first recorded on magnetic tape, and the two tapes are combined later. Well, that suggests that the photons are identified as they are being recorded, and then the coherence will be lost, the interferometer won't work. That's naïve, of course, and the wrong way to look at it. Learned papers have been written about this.

An interesting facet of this is that neither Kellermann nor I—nor any other of the people associated with this, except maybe Barry Clark—knew much about quantum physics. I had a physics background, but I'd really been doing radio astronomy. And Kellermann and Clark had studied quantum mechanics, but quantum field theory was not part of what we were doing. We were involved with radio waves and radio boxes and receivers and antennas, and we knew that a radio system would work this way. We had no qualms at all about doing it. But depending on your degree of naïveté, you might question whether or not it would work.

ERWIN: Well, my degree of naïveté is extremely high at this point. Let me just ask—this might be a silly question—are the photons coming from the same source?

COHEN: Well, the photons are coming from the same source, but they're coming from different atoms. What we're looking at is thermal noise. The source itself—the quasar—is enormous; it isn't as if we had a true point up there. The photons are coming at us from something which is bigger than our own solar system. So radio waves are coming off this big hot ball—in fact, it's a *very* big ball. We're getting radiation that's arriving all jumbled together in a very sharply defined cone, because the source is so far away. But in reality, the photons come from a huge thing, a huge area.

ERWIN: So the question was, How would this affect your results?

COHEN: Yes. Is this VLBI thing going to work? I remember once when I first came to Caltech, I gave a talk at UCLA on this subject—because by this time we were getting a lot of publicity, we were getting good stuff. Someone in the audience—this was a technical audience—asked me something like that: How can this thing work when the receivers are so far apart and the photons are detected independently of each other? The antennas are many thousands of kilometers apart and the wavelength you're looking at is only a few centimeters. How can the photons over here and the photons over there have any interference effect with each other?

And the answer is that that's the way quantum physics works. It works that way.

ERWIN: So how long did it take to get this problem resolved?

COHEN: Well, we never had any trouble. We never thought about it. We just kept working all the time. I can't recall ever discussing it with Tommy. He was very smart in some ways, and very clever. I won't talk about him too much. Anyway, it was very difficult to argue with him. He was sharp, quick. He had a very quick mind, and he could catch the essence of an argument immediately and then rebut it. He always liked to argue and prove that he knew things that others didn't. But here was a case where I think he may have been confused. My suspicion is that he was confused on this issue and that he therefore got Hanbury Brown to talk to me.

The fact of the matter is we kept on working. We had to build some special receivers and synchronizing circuits, and buy a clock and build the circuits that connect the clock to the receiver. So all the timing and recording circuits, all that had to be built. Claude Bare, the engineer, was quite talented and worked on that. Barry Clark worked on that some. But Clark mostly wrote the software. You see, our first system recorded data directly on the old kind of computer tapes. Do you remember when computers had these big, round tapes? And you'd put them into a big box the size of a wall cabinet; and you'd close the door, and the tapes would spin? We were working with these old-fashioned so-called seven-track tapes. They had a limited data capacity. Only 720,000 bits per second could be put on a seven-track tape. We used one-bit sampling, and we ended up with a bandwidth of 330 kilohertz, which is small. The bandwidth is less than half the data rate, because we deliberately inserted gaps at regular intervals, for synchronization. And then a little later, the nine-track tapes came in. And then of course, later, broadband television recorders came in and we got involved with them. And now, for much of the VLBI, they're using the so-called instrumentation recorders, which record a much broader band for eight hours or more. Our first tapes ran for only three minutes at 330 kilohertz. So the sensitivity has come up enormously. There were many stages in which the sensitivity improved because the technology of tape recording improved.

Now, the actual sensitivity of the system depends on two quantities: The first is the bandwidth that you record—that's the 330 kilohertz on tape. Bandwidth is very much with us today; it has to do with television and how people can get video data into their houses in a hurry—they can't do it on a telephone line because it doesn't have enough bandwidth. Bandwidth is in the newspapers every day now. It's a common concept.

If you're dealing with thermal noise, the more bandwidth you have in one of these receiving systems, the more sensitive it is. In fact, the signal-to-noise ratio goes as the square root of the bandwidth. So if you can increase the bandwidth ten times—which we did, and more—then your sensitivity goes up by about a factor of three. The second sensitivity is the integration time; that is, the longer you look at an object, the more signals you can pick up out of the noise. And so the sensitivity goes as the square root of the length of time you can observe. Now that's a complicated business, for it depends on the clock and also on the atmosphere. At the beginning, we were typically limited by the length of tape we could record on. The tape would hold up to three minutes of data at full bandwidth. But another limitation is set by the clock. The clocks are independent: there's a clock in California and a clock in Europe. They're both very high-quality clocks—the best money can buy—but they're not perfect. The clock is basically an oscillator, and it's putting out a nice clean sine wave, except it's not a perfect sine wave. There are fluctuations, and the frequency slowly drifts around—connected with jitter in the oscillator. The jitter is random, and different in each clock. The receivers are of the superheterodyne type. We take the sine wave coming out of the clock, and we multiply it up to some high frequency and record in the normal superheterodyne way. And therefore the jitter that's on the clock is transferred to the signals that we record. So when the clock makes a jump, the bits that we record on the magnetic tape are a little out of phase. And that's happening at random on the two sides of the ocean. And the net phase error gets bigger as time goes on. After some minutes, the clock has lost all memory of exactly where it was at the beginning. So you lose what's called coherence. You can't multiply the signals together anymore after some length of time. And there, again, there's been an improvement. We went from rubidium clocks to crystal-stabilized clocks, which are stabilized with a cesium oscillator. And then we went to hydrogen maser oscillators. And then we went to better hydrogen maser oscillators. And each improvement increases the time period in which we could make our observations.

But then you run into another limit, which is the atmosphere. The radio waves come through the ionosphere and the atmosphere. And that puts a jiggle in the phase, as the wind blows, or little clouds come, and the cells of air move around. This is related to the phenomenon of jitter you see in an optical telescope: stars twinkle because of fluctuations in the refractive index of air. You get the equivalent phenomenon at radio wavelengths.

So that sets a limit, because the atmosphere in different places is different. It may be raining in Europe and clear in California, or vice versa. Or it could be sunset in one place and the middle of the day in another. So depending on how rapidly the atmosphere is changing, that will also set a limit on how long you can observe. And that limit remains with us.

ERWIN: What's a really good time?

COHEN: Well, three minutes turned out to be fairly easy at the beginning, using fairly long wavelengths. The first experiments we did, at fifty centimeters, were not terribly successful. The first successful ones were at eighteen centimeters. Then we did some successful ones at fifty centimeters. And it was easy to maintain the three-minute coherence. Then we went to six centimeters. The shorter the wavelength, the worse the problem. And the three-minute tape was okay at six centimeters, too, it turned out—provided it was not sunrise or sunset and there were no heavy clouds and it wasn't raining. You could get trouble, but you didn't have to.

Then when we went to better oscillators and better tape so we could record longer, we could do experiments on coherence. And my recollection is that usually you run out of coherence at about five minutes. Although sometimes people tried to go longer at the longer wavelengths.

ERWIN: When did you feel you were starting to get a really good return?

COHEN: The very first proper experiment gave a good result—we got new data.

ERWIN: And how long did it take to get that?

COHEN: It took two years of development. That's an interesting story, also, because there were two other organizations involved in VLBI. There was a Canadian group, which started this for the same reasons—they knew about the oscillator. Instead of using computer tape, they went to what was then a very bulky and awkward television studio tape recorder that had just come out. It was very, very hard to use. So there was the Canadian group. Their electronics guru was a professor of electrical engineering named Jui Lin [Allan] Yen from Toronto, who died recently. There were a number of others, and I was friendly with all of them. And we found out about halfway through our business that they were doing it—that is, we were in competition with the Canadian group and didn't know it for the first year. When we found out, that put a little more fire under us. But it was hard to speed things up.

In addition, there was a group at MIT led by Bernie Burke. He was working in conjunction with some people at the Haystack Observatory, and had a number of students who were interested in interferometry. And they were doing interferometry on short baselines with wires. They knew what we were doing. Our work was public. It was being done at a national observatory. We couldn't keep it under the table, and we had to let other people in on it, which I thought at the time was unfortunate, because we got scooped in some measure.

So the people at MIT-Haystack became interested in this and wanted in on the action. And Bernie Burke is another interesting character—he's another guy who's domineering. He's very bright. He wrote one of the early papers on the quantum physics of why VLBI should work. So he's no slouch. He's won prizes and has done some very nice things. But he also is a very hard man to deal with—perhaps not now but he was then. And he had to be in on things. Ken and I used to complain about that. And our term for what he was doing was that he was muscling in on our act. It was very difficult to deal with Bernie—though he's a friend, and I think of him as a friend.

ERWIN: You had served on a panel with him.

COHEN: Oh, yes—the question of what happened in 1970 in Chicago, when Bernie Burke and Dave Heeschen and I had a violent argument. We didn't quite kill each other, but we thought about it, I guess. It was terrible. That was at a meeting of the Greenstein committee.

ERWIN: Yes, you talked about that at the end of our last session.

COHEN: Yes, well, I don't want to get into that now. Let's talk about the beginnings of VLBI and how the first experiments went. That's more interesting.

So these MIT and Haystack guys were interested and were muscling in on what we were doing. We made some first experiments in the early spring of 1967. Kellermann and I wrote up a history of VLBI and it appeared in a Canadian journal [*Journal of the Royal Astronomical Society of Canada*]. We did a local experiment—sort of right on the bench, as it were—at Green Bank. Everything worked so well that we skipped a test of true independent antennas that are, say, a mile apart—something we could have done at Green Bank. We went straight to the big experiment, which was to try to do VLBI between Green Bank and Arecibo. [Tape ends]

Begin Tape 3, Side 2

COHEN: We went to the big experiment, which we carried out at forty-nine centimeters, which was our original design frequency. It's about 600 megahertz, which was the highest frequency at which Arecibo could operate at that time. And the experiment was a failure. We didn't get any fringes. And we never understood why. We went over and over it. There were questions of software and questions of hardware. The software by that time seemed to be working, although we were using it in a regime where it had not been tested—that is, for rapid fringe rates. We knew it worked for low fringe rates; that had been tested locally.

In retrospect, it appears that the multiplier—the device that multiplied the rubidium clock's 5-megahertz signal up to 600 megahertz—must have been noisy. Later we bought a commercial synthesizer and just plugged it in. Those synthesizers had been available a year earlier, from Hewlett Packard. But sometime in 1966 we had independently developed our own multiplier box. I think we finally decided that it wasn't doing its job. Anyway, it was a big effort to move this equipment around. It had to be shipped from Green Bank to Arecibo and back, and we did that twice. This involved one of these gigantic tape recorders. It was half a ton of equipment. The experiment failed and the equipment was sent back to Green Bank, where it did seem to work.

Then we did a short experiment to the Naval Research Laboratory telescope at Maryland Point, which was fairly close by—a few hundred kilometers away. And that was successful, and then we were on the right track. And we went to Arecibo a year later. But the experiment to Maryland Point in a sense was a local experiment. The distance wasn't long enough to get us into an angular diameter range that was scientifically exciting. It was only twice as long as the long baselines in Europe, but it was at a longer wavelength. So it had less resolution than some of the experiments in Europe.

But along about that time, Burke and the Haystack people developed their own receivers at eighteen centimeters, because they were interested in studying a particular phenomenon that happened at that wavelength. We were doing thermal-noise measurements, and they were doing OH—the hydroxyl radical. So they were studying the OH spectral line. They built receivers and multipliers, and the chain of follow-on electronics that would go with that and make a VLBI system.

Then they, in a way I don't totally understand, managed to get our equipment before we had really finished testing it and using it. But it was public equipment, because it belonged to the National Radio Astronomy Observatory—it was built at NRAO. Well, Burke for years and years was a trustee of AUI—the Associated Universities Incorporated—which runs NRAO. So he carried a lot of weight at NRAO, and may have thrown his weight around in Green Bank. Later he made remarks about how we were incompetent and couldn't get our stuff together, whereas his group was able to do it. That's what started that big fight in Chicago, by the way.

ERWIN: You were unlucky, because at this point you had not…?

COHEN: You can use the word "unlucky." That wasn't the word he used. He said we were incompetent or something like that. And that started the big fight.

So they organized an experiment at eighteen centimeters, between Green Bank and Haystack, which is a distance of 800 kilometers. And this was now scientifically very interesting, because of the small angular size. And the compromise that was worked out was that we both did an experiment, first our group looked at the so-called continuum, or thermal sources—quasars—and they looked at the OH sources.

ERWIN: Is that what you were looking at originally, quasars?

COHEN: Yes, that was our original idea. So we did the 18-centimeter experiment, and it was a success. And that gave a very interesting result, because it directly showed that the speculation about the small size of these objects was correct. We still had unresolved sources; they were still smaller than we knew about. So the first really scientifically useful experiment was done jointly with the Haystack-MIT people.

ERWIN: Did you write a joint paper?

COHEN: No, we never wrote a joint paper. We never collaborated with them except in this equipment way. And in the event, it turned out that the Canadian group had done some interesting measurements a few weeks earlier. A lot of this was announced at an URSI meeting in Ottawa or Montreal in the spring of 1967. We had all gotten some results a few weeks before this meeting, but the Canadians beat us. They had done a transcontinental baseline. It was a real tour de force—from Ottawa clear to Penticton, which is in British Columbia. That was 3,000 to 4,000 kilometers, so it was a big successful experiment. And they had a longer wavelength even than we did. They were first by a week, and they published first. Our experiment to Maryland Point—which in some sense was comparable—was something like a week after theirs. The 18-centimeter experiments were a little bit later. But we talked about it publicly together at this meeting in Canada.

But there is another interesting point. In Florida, there was Tom Carr, who had been studying bursts from Jupiter. Earlier in the sixties, he had developed an interferometer with independent oscillators. He was trying to measure the angular size and fix the position of these bursts from Jupiter. The thing that's interesting is that the oscillators were independent, like ours, and he was using them in Florida before any of our experiments came to fruition. So he, in a sense, was the first to do this independent-local-oscillator business, but he was at a very low frequency, and so it was technically easy. You could do it with parts you would today buy at Radio Shack. So there was never any question of his system not working, because the bursts from Jupiter are at about 30 megahertz. So the difficulty, the constraints, on the accuracy and the timing and the bandwidth—those constraints were very weak. As I say, nowadays you would buy a few parts at Radio Shack and build this stuff up and do it. But at the time, of course, it was not so simple, and I don't want to make light of their work.

Tom Carr never got any recognition for that work—or very little—even though he should have. For example, it would not have been out of bounds for him to share in the prize we got a couple years later—the Rumford Prize. But he and his group didn't publish until quite a bit later. In fact, they weren't really sure that their independent-oscillator experiment was working until they heard of our results. When they did, that fired them up, and they saw that they had a positive result also.

ERWIN: Since we referred to the Rumford Prize, that came several years later. Did that mark a point at which—obviously it marks a level of recognition that you and the MIT people and the Canadians all received together.

COHEN: Yes, there were some twenty people who got this prize jointly.

ERWIN: Up to that point, could you summarize some of the scientific achievements?

COHEN: Well, immediately after the successful experiment between Green Bank and Haystack, we did experiments from Green Bank to Sweden, and to California. So we very rapidly made the baseline longer. This was just to test this question of the small size. And at some point, we began resolving some objects, and we knew that some objects were about the size that had been predicted. So the first scientific result was the verification of the theoretical prediction that these objects were very tiny. And once we had the size pinned down, we could then calculate the magnetic field and work on the physics of these objects. And that sparked a whole lot of work. We developed a list of the compact quasars and galactic nuclei, and we were cataloging them. What we were doing was finding out what the relative sizes were, and we were doing it at different wavelengths, to try and get a handle on the spectrum of these small objects.

ERWIN: And what were these objects, in laymen's terms?

COHEN: These are clouds of hot gas in quasars. Well, not only in quasars but in radio galaxies also. And a couple of years later it was found that these things are moving. That's a whole separate story—the discovery of the superluminal effect, which came in 1971.

ERWIN: So these are not interstellar objects?

COHEN: No, they're far away. It's only in the past two years that objects of this general type have been found in the Milky Way. It took all that time to find local objects that are roughly the same kind of thing—weaker, but much closer. The things we were looking at are generated in quasars. A quasar presumably has a large black hole, and it's spinning, and has a magnetic field. And that combination, in a way that is not understood, will generate a jet that will collimate the plasma that's around it—ions and electrons, and possibly positrons—to go rushing out at high speed. And now it's virtually certain that those jets go relativistically—that is, there's a relativistic jet that's organized by this big black hole. We see the jet. It's very bright, and it must be pointing nearly at us. Some of this radiation is relativistically enhanced—that's another topic. So these objects are very bright at radio wavelengths.

Within the Milky Way there are analogs of this, which must consist of stars that have imploded. You get the same kind of jet, in a very much weaker way, from these stars. These stars are very much closer, so you can study them, because the radio sources have a bigger angular size. So studying them will presumably shed light on what's happening in the quasars, even though the quasars are a million—or maybe 100 million—times stronger than the stars in terms of radio power. The more distant quasars and radio galaxies are a billion parsecs away—a million times farther away than the local objects. Therefore, their angular size is very small. But the fact that we still see them in strength comparable to the local sources gives you some hint of how much more energetic they are intrinsically.

I remember one day—I was at Caltech, so it must have been in '69—we did the first experiment to Australia. And this was at six centimeters. It was somewhat of a record: the baseline was 100 million wavelengths long. We could resolve a fraction of a second of arc, because a second of arc is 200,000 wavelengths. We could tell what was going on at a level well below a thousandth of a second of arc. We were reducing the data here. We had the tapes from Australia—they came by mail. We got them and we took them over to the Booth Computing Center and put them on the computer there, and some days later we got the result, and we saw that we had strong fringes from a number of sources. And in particular, one of them was 3C 454.3. And I remember sending a telegram to Dave Heeschen, the director of NRAO, saying that we had success at 100 million wavelengths. And I heard a number of years later that there was a meeting going on at NRAO at the time, and Heeschen was called out and given this telegram. When he walked back in with it, he astounded everyone by reading it out loud. So that was a famous day at the time.

This whole business was regarded as very dramatic, because the notion of using independent oscillators and jumping from 100 kilometers to 10,000 kilometers, and the technology involved—recording independently on tape and bringing the tapes together into a computing machine—it was very jazzy and high tech for the time, and it really struck people's imagination. We had a big VLBI program here that was very strong. Our group was the strongest one in the world for a whole decade.

ERWIN: Let me ask you this at this point. Once you jumped to these long baselines, you needed the goodwill and help of the people who were running those other facilities. How did that work?

COHEN: Good question. That worked by arm twisting and doing favors and promising things.

ERWIN: You mentioned Sweden. Now Sweden wouldn't have been a hard one, perhaps like Russia, for example.

COHEN: We did Russia twice, and that was interesting. We did Russia because it was far away and because it was exotic. We went to Russia for the first time in '69, and then in '71. I went there for the '71 experiment.

The Sweden connection came mainly because I knew Olof Rydbeck, who had come to Cornell. He worked in atmospheric physics and did some radio astronomy. He was a regular visitor at Cornell and had spent a year there, and I had talked a lot with him—maybe not actually collaborated with him, but we got along very well, and he knew what we were doing. He had his telescope in Sweden and wanted to collaborate. He wanted to be part of this, so it was easy to set up an arrangement with him. And there's still a lot of work going on with Sweden. Well, there's a whole new arrangement these last few years, but up until the new arrangement started, there were regular routine experiments with Sweden.

ERWIN: Did you go to Australia in 1969?

COHEN: No, I didn't. Usually, there was not much travel.

ERWIN: Was it necessary to have someone from your group go?

COHEN: No. Well, sometimes it was, because there was a lot of changing of tapes. It was a very hectic twenty-four hours, because every ten minutes you had to change a tape. We had students who did some of the traveling. There's a guy named Dave Shaffer who was a student here, who for years held what he claimed was the world's speed record for rewinding the tape and changing it. Students vied to do things like that. And in those early days, we had to do things like take our clock to Vandenberg Air Force Base, or else to Goldstone, and get it synchronized with the Naval Observatory Clock, and then drive it back to Owens Valley. And the people at the other end would have to do something similar, so that we could be synchronized around the world. Because if it had been some months since the last test, you could be off by ten or twenty microseconds. And that's okay—we could do a search and find that. But if we were off by 100 microseconds, it would blow the experiment.

A few years after we started, the Loran system came into full operation, and then we synchronized our clocks with Loran. You can do a synchronization with WWV to a millisecond. That's not good enough for our purpose, but that's easy. And we had a WWV receiver at Owens Valley to get that started.

ERWIN: What is WWV?

COHEN: It used to be the Bureau of Standards transmitter. They'd transmit standard times and frequencies. Now it's run by NIST [National Institute of Standards and Technology], I guess, in Boulder, Colorado. That station transmits standard time and frequency signals. They've done that for years and years. It used to take a big piece of equipment to get the signal in to derive a clock from that. Nowadays for a couple hundred dollars you can buy a battery-operated clock you can hang on your wall, and it will receive WWV, and will then be accurate to a millisecond.

We used Loran as soon as it became available. That was better and more convenient than carrying clocks around. Russia was difficult before Loran, because the Russian telescope was on the edge of the Black Sea at Simeiz, which is near Yalta. That's a whole long story of its own. We had thought we'd use a dish near Moscow, but it turned out—we found many years later—that it was in a forbidden area, not far from an atomic energy plant. So they sent us to this telescope down south.

The whole business of arranging this with Russia in the middle of the cold war was quite interesting, because both sides were worried about the other side learning something about coordinating the map grid around the world. So if they sent rockets up, they would have a better idea of where the rockets were coming down. But that was nonsense, because the map grid was already good enough for nuclear bombs. As we said, it doesn't matter what side of the street you're on—if the bomb goes off, you're going to get killed anyway, so it doesn't matter if you get ten yards more accuracy. The CIA and the military weren't of that opinion, however, because they had an error-probability estimate, and everything that contributed to it was of interest.

Anyway, it took some doing to do this business with Russia.

ERWIN: Clearances?

COHEN: Not really clearances. But we had visits from the CIA, and they wanted to know what was going on. And there were some limits on what could be done. Because a Russian came over to work on the data, and the agreement was that he could have the astronomical data but not the geodetic data. But that didn't work too well.

It turned out that the exact location of the telescope is very important in calculating the rate at which the fringes are coming along. And by using the best maps, by searching around, we found these fringes to Russia. And according to the best maps, the telescope's location was in the Black Sea, out in the water. And of course, it wasn't, so the maps were wrong—that is, the latitude and longitude on the maps were wrong. The maps we were using were British Army maps of some kind that were quite old. And I found out many years later that the Russian astronomers knew that you couldn't use those coordinates. And what they actually did, they knew accurately the coordinates of an observatory that was up above Yalta. And they made a measurement from that station down to the telescope. And they quietly said to the people in Green Bank, "Try these coordinates." So they did something that was probably highly illegal at the time. But there was no other way for the experiment to be done. So there was some sleight of hand by the Russian astronomers to help us get this thing coordinated. [Tape ends]

MARSHALL H. COHEN

SESSION 4

January 23, 1997

Begin Tape 4, Side 1

ERWIN: We'll start today with Caltech. We talked last time about the VLBI and its origins and the first experiments. This was happening first at Cornell, but then in the middle of all this, you started moving around. You went to San Diego and then you came to Caltech.

COHEN: Yes, that's right. I came to Caltech in the late summer of 1968 and immediately started to work at the Owens Valley Observatory. There were two things I had in mind to do when I came. One was to build up the VLBI program, and the other thing was to work on the Owens Valley Array. I had talked about that with Alan Moffet and Carl Anderson and Jesse Greenstein that summer. There was the dedication of the new 40-meter telescope in, I think, November '68. That was quite a nice occasion—John Bolton came and gave a very good talk. It was built on a little section of railroad track, because the whole eight-element array would be on railroad tracks, and the antennas would roll around to get into new configurations. Engineers at Westinghouse did the detailed design of the telescope, with a lot of help and general guidance from Caltech people. Alan Moffet and Gordon Stanley both had a hand in it. Also Bruce Rule had a very large hand in the general layout—that is, the general sense of what the 40-meter telescope would look like.

ERWIN: Let's just take a second to say who Gordon Stanley was.

COHEN: After John Bolton left and went back to Australia at the end of 1960, Gordon Stanley became the acting director. Gordon was a very good engineer. Immediately after the war, he and Bolton had worked together at CSIRO, the Commonwealth Scientific and Industrial Research Organization—at that time it was just called CSIR. And when Bolton came to Caltech, he brought along Gordon Stanley. It must, in some sense, have been a condition of his coming—that he could bring his number-one associate to build antennas and electronic equipment and be a general all-purpose worker. He and Gordon had done some very good work and made some seminal discoveries about radio sources, and they had published papers together. So Gordon was more than just an engineer—he was a scientist. He came here and was in charge of all the electronics, I believe, but various other aspects of the work, too. Incidentally, if you're interested in that, you should interview Gordon Stanley. He lives now in Carmel Valley. He has some bitterness against Caltech, because, as I'll describe, he was eased out some years later. Anyway, after Bolton left, Gordon became the acting director.

So I came here, and got a grant of $70,000 from the Physics Division, from Carl Anderson. I made very few requests of Anderson when I came. I realized only much later that people got these million-dollar propositions when they came. I got $70,000, and I got something like a promise—or at least an agreement—that Alan Moffet should be considered for tenure very soon. He was one of the brightest people I had ever met and obviously needed to be a tenured professor here. He was an assistant professor at the time, so I discussed that with Anderson.

Actually, I discussed the $70,000 with Gordon Stanley, who by then was the director. They had given up, I believe, trying to get an outside director. Gordon had been promoted from acting director to director sometime in the mid-sixties [1965—ed.].

ERWIN: So the $70,000 and the promotion of Alan Moffet were, in a sense, conditions?

COHEN: Well, that's too strong. I would have come anyway. But the promotion of Alan Moffet probably was a condition, because I needed him as a colleague and he was an assistant professor and deserving of more. Without him, the place would be very weak. It was known—I had discussed this with Anderson before accepting the job—that I wanted to build up VLBI, and of course they wanted me to. VLBI was very dramatic. This was something that was in the newspapers. So everybody was in agreement that it was a good thing to do and that it would cost money. But the specific sum wasn't arranged until after I came. I wrote up some sort of budget and discussed it with Gordon Stanley, and Gordon went over and promoted the money. It was division money; it was not ONR money.

ERWIN: Was VLBI a very costly undertaking?

COHEN: No, compared with other systems it was cheap. The $70,000 was spent on the rubidium standard oscillator that I spoke of and various other electronic devices: synchronizing circuits;

and we had to buy, or get built, the circuits that took the signal that came down and converted it to digital form to be put on magnetic tape. So that entire circuitry of writing on tape had to be built. The observatory did not have a good low-noise short-wavelength receiver. They had been building extremely stable and very highly regarded receivers for the two-element interferometer, but they were not state-of-the-art low-noise receivers. So I bought an expensive parametric amplifier for six centimeters. Those are totally gone now; nobody uses them anymore, because they're outmoded—also, they were touchy. But we bought one, and it had to be cooled, so we had to buy the cryogenic system that went with it. And so on. And that's where most of the money went—in the so-called front end, the receiver.

It took on the order of a year to get all that stuff bought, assembled, and put together. I spent a lot of time at Owens Valley. And we spent a lot of time talking about the proposed Owens Valley Array.

Then about that same time, the Greenstein committee was formed. And the committee issued a report—it must have come out in 1971. It's a decadal astronomy report, organized under the sponsorship of the National Academy of Sciences. The idea was it was supposed to make recommendations for big projects. This was aimed at NSF-sponsored work, though it was not funded by the NSF; we were independent of the NSF. There were a number of panels, and I was on the so-called radio panel.

ERWIN: Did Jesse Greenstein ask you to be on it?

COHEN: I don't remember. Someone did. It was either Greenstein or the chair of the radio panel—who I believe was Dave Heeschen. I've been on all the panels since then—in '70, '80, and '90. But the one in '70 was the most interesting, in some sense. The later ones were more complex, because the 1990 one included both the National Science Foundation and NASA-supported projects—both space- and ground-based astronomy—whereas the earlier ones dealt only with ground-based astronomy.

What happened in that 1970 committee, as I mentioned earlier, was that there were three big radio astronomy proposals. There was the Caltech proposal for the Owens Valley Array—eight 40-meter telescopes connected up in modern style. There was the National Radio Astronomy Observatory proposal to build what they called the VLA—the Very Large Array—

which was twenty-seven antennas. Originally, I think it was thirty-five, and then through further studies they saw they could get the same results, almost, with twenty-seven. Anyway, it was very large, and they would build it for the entire astronomy community. The array we would build at Owens Valley was a Caltech proposal; the community would be invited in, of course, but it wouldn't be for the community—it would be for us. The community would get some fraction of the time.

Then there was a proposal from MIT, and the spokesman for the MIT proposal was Bernie Burke, whom I spoke of last time. And Burke's proposal was for a very large, single-dish antenna to be built in Green Bank. And there were tremendous arguments. Burke for years had been promoting a super large dish, to be bigger than the dishes in Europe. There was the 76-meter dish at Jodrell Bank, which has been upgraded continuously over the years. There was the 64-meter Parkes Telescope in Australia; Bolton had gone back to be in charge of the later phases of its construction and to be the director of that dish. The 100-meter Bonn Telescope was at least under construction; I forget the details of that. Anyway, there were these other big dishes in the world, and the US was way behind, according to Burke. He'd been promoting that for years. That was his pet.

ERWIN: What was the virtue of having a very large dish?

COHEN: Well, it has more collecting area. It's more sensitive. But the argument raged over the large dish versus the array, which was in some ways less sensitive but much more versatile in terms of mapping. The large single dish was more versatile in terms of doing radio spectroscopy, because it had one focal point, and with one set of receivers you could sort out all the wavelengths, do spectroscopy. Whereas to do spectroscopy with an array at Owens Valley would take eight receivers, and the NRAO would take twenty-seven, or thirty-five. So it would be very expensive and there wouldn't be much spectroscopy done with the array. So this argument went on for a year, including this big fight in Chicago. And it was an interesting collection of fights, because Dave Heeschen, who was the chair of the panel, of course wasn't neutral about what was going on. He was the promoter of the NRAO plan.

ERWIN: It seems that this is a classic case of conflict of interest.

COHEN: Yes, because he was the chair and so he could steer the discussion. I recall that at one point in the Chicago meeting, Dave got mad and said he would resign as chair unless we went along with his idea. We didn't. So we had a break, and then picked up the discussion, and of course Dave didn't resign. Such threats are always hollow. Greenstein came to some of these meetings, too, and he was trying to get us to settle on something, because if we tried to recommend everything, we'd get nothing. If we recommended one thing, we had a chance at least of getting it. So we ended up recommending one thing, which was the NRAO plan.

Incidentally, the radio astronomers, after these big arguments, have always been able to combine forces and get behind one plan. So the NSF has actually given lots of money to the radio astronomers over the years, and there have been lots of big productions. The optical astronomers have been much more parochial, and they refuse to cede to each other's plans. The net result is that they try to promote too much, and so for years they got nothing, or very little. The radio astronomers for thirty years were more successful. And that bred lots of jealousy and ill will. Well, ill will is too strong. Jealousy, certainly. And a certain amount of ill will, which showed itself once in a while.

ERWIN: Well, you seem to have been more adept at teamwork. Perhaps this was part of the nature of your discipline.

COHEN: That's right, it's the nature of the discipline. That's interesting. I'll come back to this one of these days. As you know, six or eight years ago, I switched almost entirely to optical astronomy. And I've been working at the Keck Telescope, and talking and dealing with and collaborating with optical astronomers instead of radio astronomers. And the sociology is different. The optical astronomers I've been dealing with are much more jealous of their prerogatives than were the radio astronomers. Now, it's true that I was dealing with a select group of radio astronomers for twenty-five years—it was the VLBI world, where a lot of help and collaboration was necessary. You really worked with other people, because there were so many telescopes involved that you had to get involved with lots of people in order to do anything.

ERWIN: Didn't you have to cross over traditional disciplinary lines, too? I mean, you had more engineering people in some sense?

COHEN: Well, in that sense, yes. The radio astronomers, many of them—this was certainly true in the sixties; it's not true now so much—had a background in radio engineering, or at least in amateur radio work. Ken Kellermann was a very avid radio ham and very familiar with radio engineering. I worked, for example, also with George Swenson, who was in electrical engineering. The Canadians we were competing with—but also collaborating with—many of them were radio engineers. And Allan Yen, who was their chief electronics guru, came to spend a sabbatic at Caltech [1972-1973]. So there was much more cross-fertilization between the engineers and the radio astronomers than there is in optical astronomy.

Now, maybe that's not quite right either. Let me correct what I just said because I thought of a good counterexample. Jerry Nelson, who's a well-known Caltech graduate in physics, designed a great deal of the engineering and watched over the construction of the Keck Telescope. But he, I think, is not called an astronomer. He's not a scientist in the sense of using a telescope and looking at the sky and working up images of the sky or theories of how stars work or something like that. He's a builder or physicist.

ERWIN: Is he a physics graduate?

COHEN: He's a physics graduate from Caltech. And he's on the astronomy faculty now at UC Santa Cruz. There are a few people like that—Bev [J. Beverley] Oke, who was on our staff, and Jim [James E.] Gunn, who was here and is now at Princeton, Judy Cohen, and various other people build things and also use them. But it's rare, and it's also more rare in radio astronomy than it was thirty years ago. In the fifties and into the sixties, nearly every radio astronomer was also a radio engineer. They built their own equipment.

So that's a difference in the culture. It's also true that in the fifties and sixties there was one subculture of radio astronomy in which there was a great deal of jealousy and backbiting, and that was molecular spectroscopy. There was a race to discover new interstellar molecules. I know a number of people who were involved in that work, and they had secrets from each other, and they worked hard to get their papers published first, and in some sense talked against each other. That was a different sociology from the interferometry people, very different. And I'm glad I wasn't in that line of work, because I don't like that. But some of the people involved in that were Phil Solomon, Pat Thaddeus, Ben Zuckerman. I knew all these people, and I was friendly with them. They operated in a cutthroat way, and the interferometry people mostly didn't. But I'll come to some of that competition also. "Cutthroat" is too strong, but there certainly was strenuous competition. In interferometry, there was not as much competition where Bernie Burke was concerned as there was with Irwin Shapiro. His name hasn't come up yet. It'll come up a lot. Let me save that.

ERWIN: Maybe we could talk about the VLA.

COHEN: Okay. So what happened in Chicago was that sometimes Heeschen and I would argue against Burke. We would argue that interferometers were superior to single dishes. But then at other times, Heeschen and I would argue with each other, over the relative merits of our two proposals. But not only the technical merits. There was also the philosophical point—that the NRAO, if we didn't watch out, was going to take over the whole world of radio astronomy. That argument has gone on for a long time. And indeed, it has come to pass, more and more. The university radio astronomy groups have been squeezed out, one by one. We still have one, and Berkeley still has one. And there are a few others—Massachusetts and so on—but there used to be many more. And they were run as sort of physics experiments, but they were expensive, and they were squeezed out because the facilities at the NRAO were much better. So if the NRAO got the VLA, that meant we wouldn't have an array, and that would squeeze out the work we were doing with the 90-foot interferometer—except it's still doing solar work.

ERWIN: But you knew you would be able to use the VLA. And had the site been chosen by this time?

COHEN: Not at that time. They had been looking at sites. Green Bank wasn't big enough. I knew it had to be in the Southwest, where the weather was better. And it would have to be up high, to get above a great deal of moisture. So Socorro was pretty good.

I remember a big argument we had in Charlottesville, at some NRAO meeting, where a millimeter telescope was being discussed. And I remember saying, "No, if you ever build a telescope like that, it's got to be in the Southwest." And they all laughed at me, because I'm from California. They thought I just wanted it in my backyard. But it's perfectly obvious that you can't build a thing like that in West Virginia, say, where it's damp all the time. You've got to build it in a desert, basically. And in fact, now the NRAO is talking of going to a high desert in Chile to build their millimeter system.

So they accused me of being parochial, whereas I thought I was dealing only with the objective scientific facts. Well, self-objectivity is not easy.

Anyway, in Chicago, what happened is that ultimately…

ERWIN: This was at a particular meeting?

COHEN: It was a two- or three-day meeting, which was held at the Chicago airport. And Burke and I finally acceded, and saw that we weren't going to get anywhere and we had to get behind the National Radio Astronomy Observatory. Because we were being parochial, Burke and I. And it wasn't only Heeschen who was pushing for the VLA to be built by NRAO—there were other people on the committee who thought that doing it in a national way, at the National Observatory, was the right thing to do. It would benefit the most people most directly. Also, there was some worry, I think, that Caltech was too weak. Caltech had a powerful reputation, but we had very few people to carry off such big projects. MIT was okay on that score, because they had the backing of the Haystack Observatory and Lincoln Laboratory. So MIT had plenty of people. But there was worry about whether Caltech really had the group that could do it. At the time, of course, we had nowhere near it. And as evidence for that, you can look at the group now that's running the Owens Valley Millimeter Array, which is smaller—although it's technically more advanced—than what we were talking about. We were talking eight telescopes. The current array has lots and lots of people working on it—many postdocs and students, and several professors, and all of them are stretched.

ERWIN: Well, do you think in fact, objectively, that the decision in 1970 was a good one?

COHEN: Yes, I think so. It came up again ten years later, when we were talking about the VLB Array. We put in sort of a feeler proposal to build it jointly with JPL, and again, that was blown away by the NRAO. This would be telescopes scattered all over the country. You would have the operating center and the headquarters here at Caltech and we would run it. So that was a parochial thing we had in mind. And some of us were gung-ho for that. The people who were pushing it were me and Tony [Anthony C. S.] Readhead and Marty [Martin S.] Ewing. These were the three main people. Ewing has gone to Yale University since then. We had this delusion of grandeur, if I may say so, thinking that we could run such a thing. We could have run it, but then we would have turned into administrators and done nothing but run this thing. And it's a big deal to run a big operation, and spend many millions per year, and be in charge of ten telescopes scattered across the country with a hundred and fifty employees and so on.

Anyway, so we had something like a proposal for a VLBA. We were going to do this. And then NRAO was going to do it. And they won; in fact, we conceded. The NSF awarded the VLBA to NRAO. And then we had a fight with them over whether we couldn't do part of it. The answer was no. And now, in retrospect, that was a good thing, because we were left to be university scientist types. And the NRAO can run all these big administrative matters. Although at the time we felt like we hadn't been listened to enough.

ERWIN: Well, you were already perhaps feeling this squeeze, being pushed into the background?

COHEN: Right. The university radio astronomy facilities were being squeezed out, and everything was being organized by the NRAO. So we would become very much like the optical astronomers. The radio astronomers wouldn't have their own facilities. There'd be one central place that would run it. And we thought that was bad for several reasons. One is that many of us—the older ones among us—had a background in radio engineering, and we wanted to keep on dealing with equipment. We didn't want everything to be run by some remote place far away. And it was bad for our students, who wouldn't get experience working on equipment.

But in fact, we have stayed in good condition at Caltech, because we have the big millimeter interferometer at Owens Valley. And there's the solar work still going on, on the twin 90-foot interferometer. There's Readhead's new work building this cosmic background imager, which will go to Chile. So there's still a lot of experimental radio astronomy work going on here. But we're one of the very few universities in the country where that's still going on, and students still participate in it, so that's very good. Although the Millimeter Array is now at such an advanced level—so high tech and modern—that I think students cannot get involved in any of the electronics stuff. It's all professionals doing that. But students can get involved in the software.

The thing that Readhead is doing, of course, can have students involved with it. I'm not sure of the level to which students get into the millimeter system. They have a number of graduate students, but I think they're all doing astronomical observations. And the NRAO right now is hoping to get money from NSF to build a very large version of the Millimeter Array at Owens Valley. It'll be much stronger and be somewhat different: it will be at a high site. But one wonders how many years after the NRAO machine is built and operating will NSF continue to support Owens Valley, with its high cost. Well, that's a whole separate question.

ERWIN: Is this a prototype for what is popularly known as big science? This tendency to centralize and to concentrate resources on…?

COHEN: I think that's one aspect of big science—to concentrate on very large instruments that are used by the whole community. LIGO is big science in that way. These big nuclear accelerators are big science. The VLA is certainly big science; it cost almost $100 million, and it's got a staff of a hundred people or whatever to run it, and a big administrative group, and so on. And the VLA is used by hundreds, if not thousands, of scientists from around the world.

ERWIN: So is this a good thing or a bad thing?

COHEN: Good. In retrospect, it was very good. The VLA is one of the premiere scientific instruments in the world. It has been, for fifteen or twenty years.

ERWIN: Is anything lost in this movement toward these big collaborative ventures?

COHEN: It was for me. One of the reasons I switched from radio to optical astronomy is that the work I was doing was getting too remote. The VLBI business had been taken over by the VLBA—a dedicated ten-station system with headquarters in Socorro and run by NRAO. There are former students, of course, running these national telescopes; many were people trained at Owens Valley. But instead of going to the telescope and doing things to get the data, you wrote proposals and sent off a piece of paper to Charlottesville, Virginia, and then six months later you would get a magnetic tape with the data. You never went to a telescope. You don't use your hands in any way except on a computing machine. And that got to be too remote and not really to my taste. And I'd been doing the same VLBI business for twenty-five years, and it was time to do something else.

Anyway, that's why I joined the optical astronomy group here and got involved first at Palomar, building a device for the 200-inch telescope, using my own two hands to measure things and working with an engineer to design it. And then I did the same thing—or something very similar—for the Keck Telescope. These were very satisfying, because I was actually building something myself and had to make tests. And that, I find, is a very interesting aspect of the work—making sure something works and fixing it and so on. That's my engineering background—but ever since I was a kid, I liked to work on gadgets and fix things, and this is still my nature.

ERWIN: I've often wondered if this is an intrinsic part of being a scientist. Maybe not for everybody. Feynman used to say that all he needed was a pencil. But there are so many who seem to have this inherent, in-built desire to use their hands and to make things.

COHEN: Yes, that's right. There are very many people like that. And, of course, the radio astronomers that were arguing with NRAO were all of that type, because they were all my age or somewhat younger—or even a little bit older. All of them had been involved in building things themselves and had this urge to use their hands and design things—to build things and make them work. It's very satisfying, I can tell you, to think of something and to figure out how to build it, and then build it. And it doesn't work. And then after a while it does work, and you can actually use it to get scientific data. It's very satisfying. Some people stop at that point and go build something else. Jerry Nelson does. Whereas I, and many others—typical experimental physicists—then go on and use it to get some scientific results.

ERWIN: Should we talk about the VLBA project?

COHEN: Well, that's somewhat later. Let me talk about the VLBI network—that is, the organization, because I was very much involved in that. That was a fairly large part of my life for quite a few years.

There's also the business of discovering superluminal sources. That's all written up in detail. [Tape ends]

Begin Tape 4, Side 2

COHEN: Let me talk first about the discovery of superluminal motion, which for many years was the chief result from VLBI. The first big result was the confirmation of the theoretical predictions of the small size. That was quite remarkable, and everybody was very impressed with that. Then there was the superluminal motion, which happened in 1971. And that was the leading discovery made by VLBI, in my opinion, for a decade.

Superluminal motion refers to the fact that many of these quasars as seen at radio wavelengths—you make a map at radio wavelengths, and you get components strung out along a string, like sausages. Sometimes there'd be three or four components, or maybe two. They would have different spectral indices. That is, if you did this at different wavelengths, you would see that there'd be a flat spectrum component at one end, and that component would be very compact. We would call that the core. Our picture was that this core was in the center of the quasar, and these other blobs had been ejected, or there was a collimated jet.

Then in 1971 it was found out that the blobs in the jet are separating—and we assumed that the core was stationary and the blobs were moving away. That was actually shown for one of these objects. And then when you calculate in a naïve way the transverse velocity of that separation, you find that it's greater than the velocity of light. So that meant you were too naïve, and you had to think of reasons why you had such a high apparent velocity. There were lots of explanations, but the one that took hold and that everybody believes is that the motion of the radiating material is due to relativistic shock waves. The common present picture, which is not too different from the picture of fifteen or twenty years ago, is that there's a narrow jet of high-speed plasma that's ejected by the quasar, and there are shock waves in it. The shock waves make material pile up and heat it, and so you get extra radiation from those shock waves. Meanwhile, you also see what's going on at the base of this jet, or near the base of the jet, where the optical depth to synchrotron radiation is near unity. So there you have the core, which is compact. Then farther out, you have these shock waves, which are compressing material and compressing the magnetic field so that you get a lot of extra radiation. The shock wave is relativistic, and moves at near the speed of light. And if it's aimed near your line of sight, then two things will happen. The fact that the material is moving relativistically means that there's a so-called relativistic boost—the Doppler boosting of the radiation makes it very bright, and you get this extremely bright searchlight effect, so you will preferentially see these objects. That explained one problem: there seemed to be lots of these objects, but if the angle was very small, you should see them only rarely; on the other hand, if they were brightened up because they were at a small angle, then you would preferentially see them. So that explained the statistics. But besides being relativistically boosted and therefore extremely bright, the timescale would be squeezed, because the material moving relativistically can nearly catch up with its own radiation. That's because the velocity of light is finite. So if you work that out, it turns out that you get this superluminal effect. It's tricky. In order to measure velocities, you need to look at something twice and see that the separation has changed. In some cases, the separation changed in only a few months, and so you have an angular expansion rate of so many arc seconds per year. And you have a distance, calculated from the redshifts. And from that, you get the velocity.

I invented the word "superluminal" one day in the Robinson Library. We thought of various terms. And I had been talking with [Caltech astronomer] Roger Blandford. And I think actually Roger was the first one to mention that word—although when I talked to him about this a year or so ago he said no, it was me. But I was in the library talking to the librarian, Helen Knudsen. We pulled out the big dictionary—we were looking for words to describe this phenomenon. We had simply been calling it "faster-than-light," but we needed a better word. So in a review paper I wrote, I said, "Well, call it superluminal." In Greek, we could have called it "hyperphotic," and we talked about that. I remember talking about that word with Helen Knudsen. But that was a very awkward word. It's not melodious. So, superluminal.

I remember Fritz Zwicky, who was a retired professor of astronomy at the time. He lived beyond me, farther away from Caltech. He would walk to Caltech, and I would go on my bicycle, and I would pass him at eight o'clock in the morning as he was walking into Robinson. He was quite a character. And he would shout at me, "Hey, how are all those goddamned 'faster-than-lights' doing?" And I would shout back, "They're doing great," and keep on bicycling. That was before the word "superluminal" was invented. But I remember that—his shouting at me.

ERWIN: Was this just his way of talking to you? Or did he have some doubt about…?

COHEN: No, he always swore when he talked. He talked about "bastards" and "sons of bitches" and all kinds of things. He never spoke without cursing and complaining. Well, I exaggerate, of course. But his language was very spicy and very derogatory of other people.

But Zwicky was intrigued by this effect. He thought it was great, because he was always interested in extremes. And if you get something maybe going faster than light, that was just wonderful and keep working at it was his idea.

Most of the people I worked with were very conservative. I am myself very conservative. Einstein says things can't go faster than light. And there's the theory of relativity, and it works in thousands of different cases. So it's got to work here also. So we invent this other picture, where there's a squeezing of the timescale.

Not everybody was so conservative: There were people at the time who were thinking, "Ah, we found things that are *actually* moving faster than light," in an absolute sense. And there are these schemes. In Einstein's theory, there are particles that can move faster than light, but you can't cross the boundary. And Ken Kellermann, my good friend, is much more open-minded. He's much less conservative than I am. And he always liked to entertain the idea that these things really are going faster than light. They weren't accelerated through the speed of light, but they were born somehow faster, in the strong gravitational field of the quasar. I certainly never joined any written paper where anything of that type was expressed. But Ken used to talk about that, and other people did, too—the people who were less conservative.

ERWIN: In writing?

COHEN: I think Ken may have, in some of his review talks, at least spoken of it rather than putting it in writing. But I think nearly everybody takes the conservative view—that it's an effect of special relativity—I mean, squeezing the timescale—because it's very rapid material moving toward you at *almost* the speed of light, so it catches up with its own radiation. And you can't see that effect at low velocities. However, there were skeptics and the people who like to break idols—Geoffrey Burbidge and others—who have used this superluminal effect to try to prove that these objects are really much closer to us than we think they are. Instead of saying that we have these interesting relativistic effects, they'll say, "Well, Cohen and those people have found things that they think are moving faster than light, or that seem to be moving faster than light, and all that proves is that the objects are much closer than they think they are." But we had the distance: you have an angular scale and you have a distance, and from that you get the transverse velocity.

Burbidge and others—Chip [Halton] Arp is one—said that the redshift, from which we calculated the distance, was not a good measure of distance for these objects. And they still say that. It's a very small group—there's only a half a dozen people who are listened to at all now who say things like this.

ERWIN: Were you the discoverer of superluminal objects?

COHEN: Well, I was a codiscoverer.

But here's what's interesting about that. We were the first ones, with the Canadians, to look at these very small sources. These are the ones that show the effect.

ERWIN: Are these all quasars?

COHEN: Well, no, there are some galactic nuclei that do this also. We had been very influenced by an article by [British astrophysicist] Martin Rees which appeared about the time we first were successful—maybe a year earlier. I think his first paper was in '66 [Rees, M. J., 1966, "Appearance of relativistically expanding radio sources," *Nature*, 211, 468], where in conjunction with the theory that predicted the small size, he was saying, Well, we can explain some of what goes on—this refers to luminosity and so forth—if we have a relativistically expanding sphere. So he calculated what would happen if you had a ball of hot gas that was blowing out sideways very, very fast in all directions. Think of a balloon expanding relativistically. And seen from a distance, it's going to have very interesting properties, and you can get the superluminal effect from it; that is, the apparent diameter will expand superluminally, even if the true expansion rate as seen from the center is, say, ninety-nine percent of the speed of light. As *you* see it, from far away, it can be five or ten times the speed of light. So we were very familiar with Rees's paper, and we were thinking in terms of expanding balloons, which would have a certain look as seen from a distance. Everything we did at the beginning was interpreted in terms of circles. We even looked for expanding circles.

ERWIN: Who is "we"?

COHEN: Me and Kellermann. And Barry Clark. That is, our early group. And Alan Moffet had joined this, although he was somewhat separate. He was working with people in Australia. As soon as we were successful, he was very, very clever and began to use equipment at JPL at the Deep Space Network. So he built up some special equipment to use the 64-meter telescope at Goldstone in conjunction with the telescope in Australia. Very shortly after our first experiments, they began to do experiments, and Moffet was the first one to publish something having to do with relativistic expansion. It turned out he was wrong on that first one. But he also—because he was talking to us and was looking at the Rees article—was talking in terms of expanding spheres. They had very limited data, which they interpreted that way. Then they retracted it, because they couldn't properly interpret what they had. Then they spoke about another object, which did show it, although they were tentative.

But there was a Russian article [1969] by two people, V. N. Sazonov and L. M. Ozernoj, who talked about separating blobs. Unfortunately, we didn't pay too much attention to that. They talked more about what you might see. We even had hints of separated structures—things that weren't circles. We talked about concentric spheres of things. Our own data had hints of separated structure, but that hint comes from a minimum in the visibility—that is, a low fringe visibility. And the reduction procedures were not good. The noise was high and we had lots of reasons to suspect low points. So if we had a low point, we generally ascribed it to experimental error and didn't quite believe it. And so we didn't then go on and make further tests based on that. And what we did not do was look at a single object all day long to see what its shape was. Essentially all of our experiments were looking at a list of sources to try to do cataloging: Which ones are small; how small are they? We looked at some more than once, and at different wavelengths, and got different sizes. And we were even able to estimate an expansion velocity for a couple of these, and it was below the speed of light. But those were for objects which we now know are expanding slowly—one of them is; the other one is fast. Those are both nearby galaxies.

The Canadians had done so-called tracking experiments. But they also had lots of trouble getting their results out correctly. And they were suspicious of their low points, too, but they published them. And we were suspicious. We didn't follow up on the Canadian results, which showed that there are strings of blobs rather than spheres. There were separations, but we didn't follow up on them, because we were interpreting everything in terms of spheres. And we didn't do the detailed tracking experiments that would have shown the stringing-out.

Now, Irwin Shapiro and his colleagues from MIT and Haystack and the Goddard Space Flight Center had been doing relativistic bending experiments. One of the predictions of Einstein's theory of general relativity is that light rays are bent as they go by the sun. That's also true of radio waves. So if you have this very high-precision measuring technique, you can look at radio sources when they're near the sun and see if they're bent—see if the relative orientation in the sky of these distant objects is different. So you measure six months before the source is near the sun on the sky, and then when it's near the sun, and then again six months later. And you measure relativistic bending. And they were successful in that; this was in 1970.

And Dewey [Duane] Muhleman, here at Caltech, a professor of planetary science, had done some experiments of that type, too. And Ron [Ronald D.] Ekers, who was a postdoc here, had done some of that. So there had been a number of experiments of that type. But mostly what they did was just take single snapshots—or brief looks at these objects. And from that, you can calculate their position. And indeed, Dave Shaffer and I—Shaffer was a graduate student here—based on work we did at thirteen centimeters on an Australian experiment, we had lots of sources and we found their relative positions on the sky. That is, we could catalog their positions to a very high accuracy based on a single five-minute shot on each one—or maybe a few shots.

What Shapiro and his gang did in 1970 in the autumn was to look at two sources that were near the sun—sources they had looked at earlier. These are objects which we originally had found to be compact, and they had been written up, and we had some kind of size for them. So Shapiro et al. did a tracking experiment. They were not looking for the shape of the thing—they, too, thought it was a very small sphere. They just wanted to get a more accurate position. So they measured—these are close together on the sky, these two quasars—so they alternated measuring 3C 273 and 279. They did that all day long in October, 1970. That's when 3C 279 is near the sun. So they were getting the effect of the sun's gravity, and in order to get high precision they measured all day long. And then when they reduced their data, they saw that the fringes were not steady but dropped down and then came back up, in a very steady way. And you plot those up as a function of baseline length, and you get a cosine, and that's a sign of a double source. What you measure—the interferometer output—is the Fourier transform of the shape. And if you measure a cosine, then you take the transform of that and you get a double—an equal-point double. That was what 3C 279 was. It was obvious from looking at their data that we had a beautiful double structure—that is, we had independent blobs. The source wasn't all compact, and it wasn't circular or elliptical. It was two separated blobs. 3C 273 was similar, but the cosine was not as clean. Well, in fact, because we had one-dimensional, not two-dimensional data, it could have been something like an expanding circle, but we began thinking in terms of an expanding double.

Well, Irwin Shapiro was visiting here at the time, and so we were talking about that. He showed us his data before it was published. He was too excited to contain himself, I suppose. We were totally blown away by it. So we found out, some weeks after his experiment, that these two objects in the sky had multiple components. We were scheduled to observe on the Goldstone Telescope between Goldstone and Haystack in February of 1971. That is, our group—me, Kellermann, and company. We already had a schedule to observe. And we knew from what they had done that we had to repeat their experiment to see if there were any changes in size or shape. We caught on instantly to the fact that there might be expansions.

But, of course, Shapiro knew that, too. As soon as he saw his own data he knew that he had to do it again. But we were the ones with the observing time in February. So we were going to beat him to it. But what he did was persuade NASA and JPL and Haystack to give him observing time at the same time. So we couldn't beat him to it, and we all observed at the same time.

Well, we didn't literally share the data. We did a series of experiments, and then they did a series of experiments, but all within the same couple of days. Then we reduced the data and we saw that both 3C 273 and 3C 279 had expanded. So we discovered superluminal expansion in February of 1971. Shapiro also discovered superluminal expansion the same day or a day or two later. But it was all based on what they had done four months earlier. We would not have looked in that way had we not seen their data, which they had shown to us before it was published.

So this now raises a question: Who discovered superluminal motion? I have always claimed that we discovered it together. I was one of the codiscoverers. Some people who are less generous say that the Haystack group discovered it, because they were the ones who found the preliminary result from which you could discover it. I don't know how you count that. I claim that I was a codiscoverer. We published later than they did, because we did a big experiment. In addition to doing tracking experiments on 273 and 279, we did a whole bunch of sources, and it took us quite a few months to publish that. We could have published a letter immediately on that, but we didn't. They published a letter immediately. But to me it is obvious that the discovery of superliminal motion was made simultaneously by two groups. Every experiment or observation is based on previous knowledge. Maarten Schmidt is credited with the discovery of quasars. But he would not have looked at 3C 273 had not Cyril Hazard given him the coordinates, which he measured earlier in Australia.

Now what happened is that about that time, the American Academy of Arts and Sciences awarded all these groups of people the Rumford Medal, for the invention and bringing to realization of Very Long Baseline Interferometry. It was not awarded because of this superluminal thing—it was for the original production, although the superluminal thing had come along by the time we all got together in Boston. There were twenty people: everybody who was in each of the three original groups—that was our group of five people, the Canadian group, which was seven or eight people, and this big MIT group. We all jointly shared the Rumford Prize, which comes with a medal. And it comes with money. But there were so many people they couldn't really give money. It's the first and only time they've given it to a big gang of people like that. Each group got two medals—a gold medal and a silver medal. I have the silver medal here; Kellermann has our gold medal.

For many years, Ken and I thought this was a bit unfair, because the original realization was from our group and the MIT people got in on it a year after we started. But they did build most of their own equipment and got some of the original data at the same time.

MARSHALL H. COHEN

SESSION 5

February 27, 1997

Begin Tape 5, Side 1

ERWIN: Today we'll pick up the matter of the VLBI array and how it developed.

COHEN: You mean the network. You have to keep the words correct. There's an interesting anecdote there. In the 1970s, we used the word "array" to refer to what then became the VLBI Network, and even used it in proposals to the National Science Foundation. We deliberately used it because the word "array" means a concatenation of devices. But the NSF, in the person of Bill [William E.] Howard, who was the director of the astronomy section of the National Science Foundation, forbade us to use the word "array," because the word had already been patented for the Very Large Array, which was under construction in New Mexico. And he didn't want Congress to hear of a second array being discussed when there already was one array. We had to use a different word. So we used the word "network." So there was the VLBI Network, and that was started in the mid-1970s. It's discussed in that yellow report there.

ERWIN: The report is titled "A VLBI Network Using Existing Telescopes," dated December '75.

COHEN: Right. And a couple of years before that time, it became clear that we were too fragmented and disorganized to be effective, because we needed to schedule many telescopes simultaneously and not only get the time correct but get all the operating characteristics correct and make the telescopes' recordings compatible. This was quite difficult, and lots of mistakes were made.

ERWIN: So this was a combination of technical and management issues, right?

COHEN: Yes, that's right; it was both. And after a lot of discussion, which was mostly started by me but stimulated by lots of others also, we set up an informal association. There were the telescopes at Owens Valley and at the National Radio Astronomy Observatory in Green Bank and the Haystack Observatory, run by MIT and Lincoln Laboratory. We used the 85-foot telescope of the Naval Research Laboratory at Maryland Point, off Chesapeake Bay. And there was a telescope in Illinois—the Vermilion River Observatory, in Danville—which was not terribly well suited for this purpose but had an enthusiastic director, George Swenson, so it was used occasionally. That telescope no longer exists. And there was a small telescope in Iowa, the North Liberty Observatory run by Bob Mutel at the University of Iowa. The person at NRAO who was chiefly involved was Ken Kellermann. And at Haystack, there were a number of people, but there was always Bernie Burke, either in the foreground or background. But there were other people at Haystack, especially Alan Rogers—a very bright guy who's been at Haystack for many years. He's a technical person—something like the second in command, at least for technical things. He's smarter than anybody else there; indeed, he's smarter than most people everywhere.

So that group of telescopes was organized. We were users, and we talked to the managers. And, of course, at Owens Valley, Gordon Stanley was the director. At the beginning, he didn't think much of the project, but nonetheless went along with it, because I brought in the money and I was basically in charge of the 40-meter telescope. So I could do what I wanted there. And at the other telescopes—well, George Swenson could do what he wanted with his telescope, and so could Bob Mutel in Iowa. But at Haystack, for example, and at NRAO especially, there was a hierarchical structure that controlled the use of the telescope. So we had to make a deal with the management. And the deal we made was that we would use the telescope for a week every two months. The deal changed from time to time, and it got to be two weeks every few months.

Oh, there was another telescope, which was in Fort Davis, Texas—a very important telescope. It was operated by Harvard University. Alan Maxwell was in charge of that. And we had both a lot of cooperation and a certain amount of trouble with him.

ERWIN: There's one other telescope mentioned in the report—Hat Creek?

COHEN: Yes, that was another one. Hat Creek is up near Redding, California. It's a University of California telescope operated by UC Berkeley and by the Radio Astronomy Laboratory at UC Berkeley.

At Hat Creek, when we first started VLBI, the director of the Radio Astronomy Laboratory was Harold Weaver. And Jack Welch then became the director. He was very enthusiastic about VLBI, so he did a lot. Earlier, there were times when it was difficult. But mostly, we've had very good cooperation—very good data from Hat Creek. They've often operated on a shoestring. The problem I'm referring to has to do with changing the front ends, the box up at the prime focus of the antenna. The people at Fort Davis and Hat Creek who were in charge were used to doing things a certain way, and they would set up to do something for many months, and we would want to come in and interrupt it. And we were rather brash, I suppose. We weren't insistent, but we kept pointing out how valuable our thing was and how important it was, and how they could get extra money for their telescope if they would go along with us. And that didn't always sit too well, especially with Alan Maxwell.

ERWIN: But finally you wore them down.

COHEN: We wore them down. Well, we wore Alan Maxwell out. He got eased out as director partly because of our pushing.

ERWIN: So it took initiative to pull these people together?

COHEN: I also did the same thing with Gordon Stanley. I was responsible in some measure for Gordon's getting eased out when he did. Now, there had been difficulties, and the administration was down on him for a long time—Bob Christy, who was provost, and Greenstein, who was the EO [executive officer] of astronomy. But at one point, when there was enough difficulty, I talked to Christy about it and Christy said, "Well, do you think he should leave as director?" I had to make an instant decision, and I said yes. And then Stanley left very soon, too.

ERWIN: What was the principal difficulty with Gordon Stanley?

COHEN: It's a little hard for me at the moment to reconstruct the principal difficulty.

ERWIN: Was it a general lack of willingness to participate in the larger organization?

COHEN: I suppose I was brash, and I felt I wasn't getting as much cooperation as I was due. In neither case was it a scientific argument. It was a managerial argument. Gordon always suspected that I had had a hand in his removal. And he was right, of course. And I see him very rarely. Gordon was offered another position here. He was removed from the directorship on fairly short notice, and offered a position as something like chief engineer or senior research engineer. But he didn't want that, and he promptly left and went up to Santa Barbara. He had already been living in Santa Barbara. He now lives in Carmel Valley, and I've seen him only once in the last fifteen years.

ERWIN: Back to your users' group.

COHEN: Yes—it was called the Network Users' Group—NUG.

ERWIN: You weren't subject to the oversight of any other…?

COHEN: We were a self-organized group, and we had no boss, we had no board. There was nobody on top of us. We did what we wanted. We organized ourselves. And incidentally, another one of the telescopes was in Europe—the big telescope at the Max Planck Institute in Bonn. It's the biggest steerable telescope in the world—100 meters—and was very valuable for VLBI work. And that came into the picture very early on and was one of the telescopes we dealt with on this one-week-every-two-months basis. And there were people from all over the world, of course, who were users. So quite a few people from Germany, and then a lot of Italians, came into it also. People from Sweden. Another telescope that was part of this informal group at the beginning was in Sweden. And then later telescopes were built in Italy. And a new telescope was built in England, just for VLBI work. One in Poland, more recently. And in China.

ERWIN: So this has continued to grow.

COHEN: Yes. It has continued to grow. Let me come to more recent developments. We'll talk about the Network Users' Group—the NUG. I in some real sense organized the NUG—I mean, first promoted it, with that yellow report. I was the first chairman. We had regular meetings. And since we had no oversight and were fairly democratic, we had a lot of people who were frustrated. This had all been built out of frustration. People were expecting things to get much better, and they didn't all get better. Many things were better, but not everything. So the meetings got very raucous, and people would shout and practically throw things at one another, especially at the chair—"Why didn't you do this?" and "I got cheated out of that." Lots of shouting and complaints. And factions would argue about which was the best receiver wavelength to promote. You see, the group could get together and after some argument it could make recommendations, through the chair, to the telescopes: "We now want to expand our operation. We want to do two weeks every two months instead of one week. We want to next operate on a new wavelength. And will you please see if you can build yourself a new receiver so that we can do the following." Or, "We find that such-and-such station's clocks are not very good. Do you think it would be possible for you to do this? We would be glad to write to the National Science Foundation and tell them that the network is getting much better, but one of the weak links is the bad clock at the Fort Davis Telescope. But they need money. Will you please give them money to buy a new clock?" I don't know if we really did that in particular—but we did things *like* that. And the thing that was most difficult was getting help at the telescopes. For a number of years, that was very difficult. We fairly early on began to nag the telescopes to provide more manpower to help us. It was very labor-intensive work.

ERWIN: Would this have been a dedicated staff member?

COHEN: Well, that's what we wanted. But it wouldn't be full-time. It would be a staff member who spent about a quarter of his or her time working for the network—seeing that the system was set up and running properly and that the formats were all compatible with the standard. So we were a standardizing group and made sure everything was up to standard. And then we assisted in the observations, which went on around the clock for twelve or fourteen days at some telescopes. At the busier telescopes, it really went on around the clock for two weeks. And you know, that's very difficult; one person can't do that. It's very hard even for two people. Typically, though, two people did it. And the only place where life was easy was at NRAO, because they were a big organization and they had helpers at the telescope anyway. Haystack developed into that, but at the beginning we typically sent people to Haystack to help them also. At other places, like Owens Valley, the tradition was that if you wanted observations you did it yourself, so it was very difficult. And that caused some tension. I myself spent a lot of time up there, changing tapes and working for others, but I didn't get the reciprocal of that, say, from NRAO, because the people there had salaried staff members to do that. So that was a cause of a certain amount of tension at times.

ERWIN: So this was an issue that NUG wanted to try to attack?

COHEN: Yes. And over the years, things did work out. But it took a lot of time, because hiring another person is a big deal if you have an observatory with five people and you want to hire another one. The director wants to hire someone to do *his* work, not someone who's going to spend a good quarter of the time basically doing something else. On the other hand, we were promoting the money for it, so the only way they could have hired that person in the first place was because we went to the NSF and said, "Look, we need more money." But in fact, during those years, there typically was no more money; we were very bad in promoting more money for people. We could do small things, like get small instrumentation. But for a number of years, we weren't able to move things very much.

ERWIN: Well, it seems to me that that was a tough time for funding, the seventies.

COHEN: Yes, through the seventies into the eighties. But then there was a period of time when my grants from the NSF and JPL for VLBI work here were very handsome. They were getting up toward $1 million a year in the late seventies and eighties. The NSF grant specifically included postdocs, part of whose job was to go to Owens Valley and spend these weeks when help was needed. So there were times when we had as many as three postdocs on that project. That doesn't sound like much in biology or chemistry, but it was an awful lot in astronomy. Usually it was two postdocs, and even that's a lot in astronomy—to get money from the NSF to pay for two postdocs. But those people were assigned part-time to be at the telescope and do things. And whenever we had graduate students—we never had many students typically; one at a time, or maybe two, but they also had the job of working at the telescope during the network sessions.

ERWIN: Well, Caltech is small. But at the other academically owned telescopes, was it a similar kind of situation?

COHEN: Yes, it was very difficult everywhere. At Berkeley it was very hard. Now, the Berkeley telescope had a limited wavelength coverage, so it wasn't used as much as Owens Valley or Haystack or Green Bank or Germany. Germany's no problem; it's a big, massive telescope, and they have lots of people working around the clock. But whenever we were scheduled at Berkeley, we almost always sent someone. Whoever wanted the observations would go to the telescopes. And sometimes you'd get your friend to go somewhere else, and then you owe that friend a favor.

So it was very labor intensive, and the reductions were very labor intensive. They're much better now. Handling the data was very hard. It's much more automated now—much much better. And the correlations were different—that's another topic I'll bring up, together with JPL. Correlating the tapes as they arrived from the different stations, that was very difficult. It's gotten much easier; in fact, it's trivial now, because someone else does it for us. We used to do it for ourselves. And there's been an improvement in the hierarchy of tape recorders. The technological improvements have been enormous over the last twenty-five years. And clocks have gotten better everywhere. Altogether, things have improved a great deal.

So we kept expanding the network and what it would do. At the beginning, we thought that if we could get three telescopes, or maybe four, to work together at one time, we were doing very well. The first three that we tried to get to work together, we called the FOG Array. And then the word "array" was not allowed anymore. FOG meant Fort Davis, Owens Valley, and Green Bank. Two across the country and one in the middle. You need geographic spread at different baseline lengths. Then we had FOGB. The "B" stands for Bonn, in Germany. That was four, and that gave us a lot of data. We also had FOGA, with "A" standing for Algonquin Park in Canada. But it was clear essentially from the beginning—from 1971 on, or even before that—that we needed to use many telescopes, and we rapidly went into the world of many telescopes.

ERWIN: How many is many?

COHEN: Well, experiments have been done with eighteen telescopes used simultaneously. And this is not the modern VLB Array—we did eighteen with the informal network.

Now, why did we need a consortium? Why couldn't we stay totally informal? The consortium was in some substantial part an MIT push. And I should mention that there's always been some competition in the VLBI world between Caltech and MIT—MIT connected with Haystack. There's a lot of good cooperation, but there's also been competition. The competition was reflected also in competition between me and Bernie Burke—and to some extent, between me and Irwin Shapiro, but Shapiro left MIT and went to Harvard quite a few years ago. Bernie liked to be on top of anything he was connected with, and at the very beginning he didn't think much of the NUG—at least that's my impression. It was the case that there was competition to get things done, or to make improvements. We thought we would make an improvement—and Haystack would rush in and try to do it first because they wanted to be first. They wanted to have the leading role in the VLBI world.

Haystack Observatory had a different role from Owens Valley. Owens Valley is an academic place, and I always felt that, well, if we ran out of things to do in the VLBI, we could always just sort of shrink the observatory and do something else. And in fact, that's what we've done. But at Haystack, they couldn't conceive of something like that. They had a big organization, and they weren't about to let it shrink. In fact, their idea was always to grow. And they had a hundred or so people working there. And a lot of what they did was for the military. It was not a university-type organization. The VLBI was a little piece of it; it provided a very nice chunk of money for them. But they had to promote themselves as much as possible. They had a big engineering staff that they had to keep busy. So whenever there was a development to be made, they wanted to do it and they wanted to get the money. And if we were doing something, there were cases when they would do it first. That's inefficient, and we tried to control it with an engineering committee, but that didn't always work.

On the other hand, we wanted to keep our engineering operation going, too, because it's good training for students. We also had people that had to be kept busy part of the time. In fact, they were always too busy, of course.

Anyway, there was this level of competition. It was not so much direct scientific competition as organizational and engineering competition, and competition maybe to be seen as the leader—at least as far as Washington was concerned. And I attribute that to what JPL would call the "programmatic imperatives" of operating at a place like Haystack. JPL is even more impelled that way. And the competition was also partly due to Bernie's personality. All those things. And maybe to my personality, too—I don't know.

But with MIT's pushing the consortium, we got together and decided that we needed more organization. The totally informal organization that we had wasn't good enough. Because we needed the universities themselves to back the network more strongly—put a little of their own money in, but also give it a stronger footing. It would carry more weight at the NSF if the university presidents signed a document and said, "Okay. We, Caltech, are now part of a consortium for long baseline interferometry." That gives the network much more stature. And MIT produced a very good person. The vice provost of MIT, Joel Orlen—he now works at the American Academy of Arts and Sciences—had engineered a marriage of sorts between different components of MIT and some other Boston organizations. He drew up a charter for us. We met a couple of times—once in San Francisco and once somewhere else—and he was very smooth.

ERWIN: Were you, at this point, in favor of this consortium wholeheartedly? Were you compromising, in some sense?

COHEN: I don't think so. I think by that time I was beginning to lose interest in a lot of the organizational things. I have realized over the years that I'm not a natural-born manager-organizer. Well, I am, except that I get tired of it. It took an enormous amount of energy and time over the course of very many years, and I was beginning to think I had devoted enough time to it.

ERWIN: So the consortium would take some of this burden off you?

COHEN: Yes. Well, I had already begun paying very much less attention to the organization, because other people—like Don Backer at Berkeley and Jim Moran at Harvard, who were very good and seemed to have an infinite capacity for doing these things—were running the network. Backer became the chair. And I didn't pay too much attention to it after the first five years or whatever. I have difficulty remembering why it is that we needed a consortium, or why people thought there should be a consortium. But anyway, it was invented. So this was a formal piece of paper that was signed by Caltech and Harvard and MIT and the University of California and the University of Iowa. I think by that time, the Illinois telescope was out of the picture.

ERWIN: But were there just American participants? Or did you have foreign?

COHEN: Well, these were American universities, but we had associate members of various kinds. The Naval Research Laboratory was an associate member; they had a telescope that had been in VLBI from the beginning, the Maryland Point Telescope. And the Max Planck Institute was a member of this consortium; it wasn't a university, but nonetheless it was a member. But maybe it had a slightly different status; I don't remember. And the National Radio Astronomy Observatory already was part of a university consortium—it's the AUI. That is, the grandfather of NRAO was the Associated Universities Incorporated, which is a consortium of about twenty universities—mostly all in the East. So there was some question about the legalities of the NRAO joining. From their organizational position, could they join a different university consortium? They became some sort of associate member. I mean, they *had* to belong, at some level. We had to talk to them; they had the most important U.S. telescope.

May I make a diversion? I half-promised Jane Dietrich [Editor of *Engineering and Science*] that I would write chapter two of my history of Owens Valley Observatory. That's because next year is the fortieth anniversary of the Owens Valley Radio Observatory, and we're trying to plan some kind of a meeting, and it would be nice if I could get this thing written. If I do get it written, it will contain a coherent story of how Caltech and OVRO fit into the VLBI picture.

ERWIN: It's also the fiftieth anniversary of Palomar. It's going to be a big astronomy year—1998.

COHEN: Big astronomy year next year, right. There have been a lot of changes at Owens Valley—more than at Palomar, in a real sense.

Let's get back to the main thread. So now there is this consortium, which got a few thousand dollars a year from each university, and it was put into the account that the treasurer, Bob Mutel, controlled. It wasn't a lot, but it in fact was useful, because it bought standard filters, for example—that is, one person bought a whole bunch of identical pieces and gave them to the telescopes, so we were all standardized and more compatible than we were before. So things did get a little bit better in some ways because of that. There was more uniformity. And at that point, I think there was much more pressure on the observatories to provide what we called remote observing. That is to say, if you wanted to get a certain experiment done, you didn't have to go simultaneously to six telescopes, because the telescopes began to get more of their own local observers. So it got finally to the point where there was much less travel and aggravation and trouble. And that, in turn, was overtaken by the national VLBA, the Very Long Baseline Array, and after that started working, the network and the users' group and the consortium were all disbanded.

ERWIN: When was that approximately?

COHEN: Three years ago. There was a lot of disbanding before VLBA actually got to work, so there was quite a problem for over two years. The disbanding came about because the National Science Foundation was providing money to NRAO to build up this national array, which was going to replace our informal ad-hoc network, now managed by the consortium. So the NSF cut off the money as soon as it could; it had planned to do so for years, as soon as the VLB Array started working. At that point they would stop supporting these other telescopes. So they did stop supporting the other telescopes, but of course the VLB Array wasn't working, because the correlator didn't work yet. So there was a gap, and that caused quite a bit of trouble. And then there were emergency repairs and start-ups of the network and so on.

Meanwhile, I should point out that for a long time there had been both an American network—basically managed by the consortium—and a European network, the EVN, the European VLBI Network. And the EVN started off simply; there was Sweden and Germany, and then there was Italy. But then as more telescopes developed in Europe, they organized their own network. And the EVN is quite formal. It has a charter, in the sense that it has gotten money from the European Science Foundation, which is a branch of the EC in Brussels. And they're building a European correlator in Holland. So they're getting millions of dollars in concert from the European Community. Now, it's not organized like CERN [European Center for Nuclear Research, in Geneva]. There is no treaty which says you have to pay such-and-such a fraction of some budget. But they have been able to get money for equipment in the million-dollar class, and they have gotten money from the EC for special fellowships. The countries involved now are Germany, Sweden, and Italy—the Italians have two telescopes now that are part of this European network. Also there's Holland, England, Spain, Poland—they built new telescopes in Poland—and Finland, and Russia. France is an interesting exception. France has never really been in this game.

ERWIN: Who built the telescope in Poland?

COHEN: The Poles did—my friend Stanislav Gorgolewski started that. But it was taken up by his student, who's now the director.

ERWIN: Where did they get the money?

COHEN: You know, I asked him that, too. How do you get a million dollars in Poland? And this was a dozen years ago. Well, Gorgolewski knew parliamentarians and such. He was quite a promoter. He's a little bit crazy. I met him in Poland when we were there eight years ago, in Torun, a university town—Nicolaus Copernicus University. And he showed me his field station, which they'd been running for years and years. But the new telescope was going to be built there. Anyway, I asked him where they got all this money. Well, it's a European thing. The Poles wanted to be part of Europe even then. And the idea was that the design, everything, was going to be done in Poland. The steel would be steel beams from Poland. Instead of building a bridge, they would build a telescope. And it would provide work for the Polish electrical engineering institutes, because they could design something and see it really work. They would have to spend hard cash buying some of the electronic equipment. The hydrogen maser would come from Switzerland, and for that you need the equivalent of gold bars. The government provided the money to buy those things they had to buy abroad, and everything else was built in Poland. It provided experience for engineering students and engineering professors. It was a big thing in Poland. And a telescope like that costs between $1 million and $2 million in this country. Of course, in some sense, it cost much less there, because the people who built it were paid very little. But it's now a working telescope.

The telescope in Finland is used occasionally. And in Russia, from the beginning, there have been telescopes. The Russians some years ago built a network of very big telescopes, equivalent to the Deep Space Network. We've used them, too.

ERWIN: Let me just ask about Australia at this point.

COHEN: Oh, yes. They've been in this quite a bit.

ERWIN: But they're not part of the European network?

COHEN: No, no, because they're in the Southern Hemisphere. But the telescopes in Australia have been used a lot for special purposes. Also in Japan. And China—new telescopes were built in China. I made a big trip to China some years ago, where I promoted VLBI. I gave a series of talks in Shanghai. People came from all over the country. I thought I was just going to lecture to the local people. It turned out that about a hundred people had come from as far away as Xian and spent a week in Shanghai. I gave a talk every day on the science and the organization, and some of the technical questions. And that was the start of promoting VLBI in a big way in China. And they've now built two telescopes.

ERWIN: When was this? In the eighties?

COHEN: 1983, I think, in China. It was either '83 or '85. And the director of the Shanghai Observatory was a woman named Wu. She's a very interesting person. She used to travel a lot and come here, and we'd see her.

ERWIN: Did you at that time have students from China?

COHEN: One student from her place did come over. He got his PhD here—supposedly with me, except I left town at that time and he did it with Tony Readhead. And, like all Chinese students, he didn't go back to China to work in their group. He stayed here, and he's now working at IPAC [Caltech's Infrared Processing and Analysis Center].

Incidentally, along that line, some students came from Israel, when Yuval Ne'eman was the chair in the Physics Department at Tel Aviv University. There was a period of time when he was very interested in promoting VLBI—or at least radio astronomy—in Israel. This was twenty-five years ago, more or less. And he rounded up a couple of Israeli students and sent them to this country. He was dealing with three people in the United States—me, Ken Kellermann, and Arno Penzias, who then was at Bell Laboratories. One of the Israeli students came here—Manny Cimerman. He got a PhD at Caltech, and I think he is now working in Israel in some electronics company.

So that, too, was an attempt to try to get something started by sending students to the United States. But it didn't work in Israel. They don't have any radio astronomy program. It did work in China, but there they had an indigenous group, big observatories, and they wanted to do it themselves. And I kind of helped that along. I catalyzed it, by being there and giving all those talks. There's a lot of interchange, I believe, between China and Japan. The Japanese have been doing this for quite a while. So there was something like a Far East Network, but they often attach onto the European Network. [Tape ends]

Begin Tape 5, Side 2

COHEN: So the EVN is very highly organized compared to the way we were organized here. EVN has something like a director, and the directors of all the different telescopes meet a few times a year at different places. They essentially have their own laboratory in Holland. And they have an institute called JIVE—the Joint Institute for VLBI in Europe. And an ex-postdoc from here—my first postdoc here, Richard Schilizzi, who's from Australia—went to Europe, and he is now the director of JIVE. He's a professor at Leiden. I think he lives in Dwingeloo, which is where JIVE is. JIVE runs the VLBI observations in Holland. They are also building a very large European correlator. They got money from the EC in Brussels, some millions of dollars, to build this correlator. JIVE has a big budget: they have their own engineering staff and they have some postdoctoral positions. And they have some positions at other telescopes, through this. So this is a big organization, with a lot of money.

ERWIN: Should we take a moment to summarize some of the scientific results of VLBI? I might also ask you if there's been a difference in the directions taken between the Americans and the Europeans—since we've been contrasting the organizational aspects.

COHEN: Yes, there's a difference in the scientific direction—has been for a dozen years—between the European network and the American network. The European network includes a couple of very big telescopes—the one in Bonn, 100 meters; and the one in England, 76 meters. That one doesn't go to the shortest wavelengths, but for about six centimeters or longer you have that very large collecting area. And the interferometer at Dwingeloo has a large collecting area—it has either ten or twelve telescopes, each sixty feet in diameter. They have had more sensitivity than we in the United States, so they often would look at fainter sources. And we often concentrated on longer baselines—that is, higher resolution. Particularly California to Europe—we would almost always tie Europe in with the American network. So there was somewhat of a difference. But, of course scientists from both sides did both things. It was largely the same, let me put it that way.

But from the beginning, there has been one split in VLBI, between the people who use it for astronomical purposes and those who use it for Earth studies—that is, geodesy. Well, geodesy and astrometry. Geodesy has to do with fixing locations on the Earth, and astrometry has to do with fixing locations on the sky: the relative positions of the stars—the grid of stars on the sky. And those two studies are tied together: in order to fix positions on the Earth, if you're using stars, you have to know where the stars are. People at Haystack, for example, and Goddard Space Flight Center and JPL, were very interested in that; that was sort of their line—crustal motions, things connected possibly with earthquake studies, fixing exactly distances between places and tying that to exact positions of narrow radio sources in the sky. And there are people in Europe who did that. In fact, that's the official excuse, as I understood it, for why there is VLBI in China. It's because they're interested in earthquakes, and VLBI can study earthquakes, so they built it for that purpose.

But of course, in this country—and probably also in China—the majority of the time of these telescopes is spent not on geodesy but rather on studying other objects. To me, the exact location of an object on the sky isn't as important as its shape spectra. And indeed, even at

Haystack and at Goddard and at JPL and in China, there's lots of interest in what I would call the astronomical aspect, too.

But at Caltech, here within our department, there's been very little interest in geodesy or astrometry. I wrote a paper with a student once on the coordinates of stars, but that was just dabbling. I've never been seriously involved in that.

So there were those two main directions. And one of the early results—well, it took ten years, because you have to accumulate data for many years—had to do with plate tectonics. That is, you could see that North America really is separating from Europe at the rate of a few centimeters a year, and there are all these other motions. Now those studies have been overtaken, as I understand it, by the Global Positioning Satellites; they're much easier to use and probably as accurate. Except that the VLBI programs continue, too, at a reduced level. That's a management thing: when a government laboratory has a program, it doesn't stop just because it's been superceded. Well, geodetic VLBI now does only long-range experiments—North America to Europe, for example—where it still has an advantage.

Within the astronomical world, the main scientific result at the very beginning was the confirmation of the notion that many of these sources were tiny, very small. And then a couple years later came the discovery of superluminal motion, which was a very interesting discovery. And then more recently, the statistics of superluminal motion and what that says about cosmology—the scale of the universe. There are relativistic jets in these objects, and studying their statistics can tell you something, we think, about cosmology. And I'm involved in a program that's still doing that, with the VLBA.

A very recent—two-year-old, or maybe three-year-old—result from the Harvard group was one of the very first results from the VLB Array. There's a galaxy called NGC4258 that contains molecular clouds near its center. Molecular clouds contain, among many other molecules, water vapor and disassociated water—that is, plain hydrogen and the OH radical. And the OH in the water and some other compounds form masers. It's physically analogous to the kind of laser you have inside a CD player, except they're operating on the large scale in these astronomical clouds. So you have these very concentrated beams of light—that is, of radio radiation—at the wavelength of some transition in the OH or water molecule. And that gives you a very fine location mechanism, and you can also tell velocities from that. So you get a great deal of three-dimensional information. And the people at Harvard have been in the forefront of this for twenty some years. They've concentrated on that and have done very good work. That was even more labor intensive than the kind of stuff we were doing.

Anyway, they found out that this NGC4258 galaxy has these clouds in it, and they were able to analyze that and show that there's a warped disk in there which is rotating at a certain speed. And from the speeds they can calculate the mass at the center. And it doesn't genuinely prove there's a black hole there, but at least there's a very large mass concentration in a very small volume. And that struck the world. Many people were excited by this. It was a remarkable result. And they continue to follow that up—and there are other such objects. And VLBI makes that possible. And there's a tremendous amount of data analysis that goes along with it. The VLBA makes life much easier for people now, because the correlating is done in Socorro by professionals, and the calibrations are much easier than they were a dozen years ago.

ERWIN: So it seems to me that perhaps radio astronomy has been able to look at a broad diversity of objects—more so than optical astronomy.

COHEN: Well, yes and no. In fact, one of the important things to do if you see a radio spot in the sky is immediately go to an optical telescope and see if there's something optical there. That is, the word "identification" is always used in reference to the optical thing. Something like "Seeing is believing." Because you want to know if this is a galaxy or a star or a cloud. And the same holds for the X-ray people. They get an X-ray source from the sky and they immediately want to "identify" it, meaning "I want an optical picture of that, and I want to see if it's a galaxy; and what does the optical spectrum look like?" So the optical work is at the center. They all go together.

Radio astronomy opened up people's minds to the notion that the universe was not stodgy, but rather was exciting and full of high-energy stuff. It was in turmoil, and things were happening on a short timescale, and there were explosions, and concentrations of things, and relativistic jets. And according to optical work, prior to the development of radio astronomy, things were pretty quiescent. There were galaxies and there were clouds in the sky. There were supernovae, of course, which were spectacular things. But otherwise the universe was on a very slow and even placid timescale. Things moved, and they went around in their stately orbits. Once in a while a star exploded, but that was the end of it.

And then radio astronomers found out that there were such things as quasars, which had black holes in them and were really violent. And things happened on a short timescale. And it was even shorter than that when radio astronomers discovered pulsars, which are spinning neutron stars.

ERWIN: We didn't really talk about those discoveries.

COHEN: Well, I had very little or nothing to do with that. The only thing I had to do with pulsars is that I was one of many people who didn't discover them, and so therefore I was one of many people who didn't win the Nobel Prize. I could have discovered them, because I was well located for that, but someone else did it.

Since then, of course, the X-ray people, especially, have provided even stronger evidence of violence, of things happening on short timescales, and relativistic effects near black holes. And then there are gamma rays coming from some of these things. They don't know what those gamma-ray bursts are due to.

Mostly, the infrared stuff is fairly quiescent, because it's radiation from dust. But it shows that some galaxies are dominated by dust. There are many galaxies you don't see optically, because they're shrouded substantially by dust. And so there's this tremendous outflow of radiation from the dust. I mean, all that energy is coming out of the stars. It goes to the dust, it heats up the dust, and then the whole thing is a great big oven. And it just radiates slowly at infrared wavelengths. [Tape ends]

MARSHALL H. COHEN

SESSION 6

March 11, 1997

Begin Tape 6, Side 1

COHEN: In the seventies, Alan Moffet and Bob Leighton began developing what has since turned into the Owens Valley Millimeter Array—it was their idea. At the beginning, it was just an expansion. Alan was always very interested in interferometry and always wanted to see an expansion of that. He got into the VLBI business also. He was not so much associated directly with what I was doing; he had his own empire, which he was running in conjunction with JPL. They used JPL radio telescopes and did a number of experiments to Australia. But he worked with my group also.

Then in the late eighties, this millimeter interferometer became the dominant business up at OVRO. And it has been the dominant force up there for the last ten years, certainly.

ERWIN: Was that new at the time?

COHEN: Well, when they began developing it in the seventies, there had already been some tests by Bernie Burke at MIT, which did not turn out very well. And in fact, it appeared that Burke had a fairly extensive program going for a long time on the roof of a building at MIT, and never got very far with it. He kept being supported, though.

Jack Welch, at the Hat Creek Observatory of the University of California, made the first working millimeter interferometer in this country. And there's always been some combination of cooperation and competition between Caltech and Berkeley on that score. I was never involved particularly with that, but I watched it from the sidelines. Moffet built on what people at Berkeley had done with the electronics. Bob Leighton went off in a totally new direction and designed an appropriate dish—a radio telescope for millimeter wavelengths.

ERWIN: And that was new?

COHEN: Yes, it was a new design. Well, people had done things like that, but he made one that was very light and elegant. There are six of them in use now at Owens Valley, and there's one in Hawaii. The best of the lot was put on Hawaii and is used as a submillimeter antenna. So Leighton got into the dish-building business. And even while he was division chairman, which must have been in the early seventies [1970-1975]—I remember seeing him when he was division chairman; he always had a computer on his desk. And whenever he didn't absolutely have to take care of division business, he was busy calculating the stresses in these dishes, finding the proper location of the bars behind that strengthened them, and so on.

ERWIN: It sounds almost like he was a reluctant chairman.

COHEN: I think he was. Leighton was enthusiastically promoted for that job. He was a great physicist, but I think he was not a great manager. I could say something about every one of the division chairmen. I'm not convinced that any of them were ideal—of the ones I was involved with. Leighton was a reluctant division chairman, I think, although I never had any specific problems with him. Other people grumbled a bit about what he was doing. But one always gets that.

Anyway, at the beginning, there was a tremendous amount of trouble with the millimeter interferometer. Things didn't work, and there were money troubles. And NSF wasn't going to support it. And at one point finally—and I forget what year [1980]—Robbie [Vogt] fired Al Moffet as the director of the Owens Valley Observatory. Moffet had become director in 1975, after Gordon Stanley was eased out. Moffet did not tolerate fools easily. In fact, he didn't tolerate them at all. And he finally had trouble with the NSF—serious trouble. He was brilliant, but he was a little too brilliant for the millimeter interferometer, because it required detailed careful engineering, and he was much more of a quick inventive genius who would do something and then it would lie there in a bit of a shaky state, and other people couldn't make it work. And it turned out that he had ignored some facets of it. As a detailed engineer, he was not so good. But as a brilliant inventor of things, he was very good—one of the smartest people I've ever known.

He got on the wrong side of the NSF and on the wrong side of the administration here. And the place was floundering. Robbie Vogt, the division chairman, fired him—eased him out, whatever the right phrase is—as director of OVRO. Then there was the question of who was going to be the director. And that went in circles for a while. Robbie was the temporary director, or acting director. Tony Readhead became director a year or so later. And during that period, Robbie, who was an organizational genius in the way that was opposite to Moffet, could bring people together. And his method of management, of course, was a combination of cajoling and flattering and shouting and beating people with a club. So it was an interesting combination, which was difficult. He never did that with me, as far as VLBI was concerned. But at one point there, I was executive officer for astronomy [1981-1985—ed.]. So I had plenty of occasions to be beat on by Robbie. He was a very strange person—still is a strange person. I guess he always has been. He shouts and screams and talks and talks and talks. He's a very interesting person.

There were other troubles when Robbie was the acting director, because there was some competition for funds. And that goes back to something that had happened some years earlier. Let me pick up that thread, and then we'll come back to what was going on when Robbie was acting director.

Some time in the middle seventies, the National Science Foundation forced us to split our funding. Originally, the Owens Valley Radio Observatory was funded by the Office of Naval Research. When I came here, I got a grant from the National Science Foundation. Moffet probably had a small NSF grant, too. But there was a big ONR grant that supported the observatory and the telescopes and paid the engineers and others up there, and the Pasadena staff. And there were these small grants that did things. I had a modest grant from NSF to build up VLBI. Of course, the institute put quite a bit of money into OVRO also, and these monies kept increasing. And the reviews would say, "This thing's great, and that thing is very shaky, or no good." Still, the NSF would give a big pile of money, and the director could still funnel the money to the part that the community thought wasn't very good. And in the middle seventies, the millimeter interferometer was in a bad way, and the VLBI was in a very strong, growing mode, and was recognized as a world leader. So the National Science Foundation, instead of giving a block grant to the observatory, forced us to break up the proposal—break up the observatory into little duchies. So that I would be in charge of a VLBI empire, and Moffet would be in charge of a millimeter empire. There would be a director, but the director didn't have any money—these people down below had the money.

ERWIN: That's a curious sort of arrangement.

COHEN: It's very curious. And managerially, it's a nightmare. Managerially, how can you have a director who has no money, who does not control anything? And when I say we were forced, that's not quite right. The NSF also told UC Berkeley that the Hat Creek Observatory was going to have to run that way, too, but the University of California was smarter than we were. They refused to do it. They simply said to the National Science Foundation, "No, that's no good. We will not do it." And they sent them a combined proposal, as always. And ultimately they won. We didn't even think—or at least I didn't even think—of doing that. Moffet and I and the authorities here just said, "Well, we've got to go along with it." And actually, I didn't mind, because my part of the organization was much the best, and I hadn't been getting what I regarded as my proper share of the money. Moffet had been funneling a good bit of the money sideways, into what was a weak operation, the millimeter interferometer. After they broke us up, he would still borrow my engineer, my technician, and use him for long periods of time. And he was up there a lot, and I wasn't. And he was also the director, so he could tell people what to do. I had to be quite firm at some point and say that certain people were not allowed to work on this. And that caused a certain amount of friction.

So things were broken up, and that has given us trouble ever since. And for twenty years there has been aggravation up there. It depends on who the director is. For the last ten years, Nick Scoville has been the director, and he is also the PI of the NSF grant that supports the Millimeter Array. So he can take his directorial money that he gets from Caltech and do what he wants with that, pretty much. But he's also in charge of the biggest program. So he, as director, is in charge of the biggest program with all its biggest money. And the other guys have to either shift for themselves or try to get money from him.

But when Tony Readhead was director for a number of years, there was quite a period of time when he had no authority, in the sense that he didn't have much money. I had all the money for the VLBI grants—more than a million dollars a year. And Tom [Thomas Gould] Phillips or someone had all the money for the millimeter interferometer. And the director had some Caltech money and arranged for things—like making sure the buildings were kept painted and the sewers didn't clog up—things like that, whatever had to be done. But the director had little authority over how to allocate the money—allocation of money and promotion of certain lines of instrumental development. He had *something* to say about that, because he had some control, but not very much. Most of the control was held by these little duchies. So it's a case of a weak king with powerful dukes. And managerially, it's very bad, and the observatory has suffered all this time, sometimes more than others. It hasn't been so bad lately, although if you ever get to interview Readhead, you'll find—I trust—that he will grumble about the bad treatment he had from Scoville. Because while Scoville was director and also in charge of the millimeter interferometer, Readhead—he thinks—did not get his share of the money that was supposed to come, say, from Caltech. Caltech put in a couple hundred thousand dollars a year, and the director is supposed to allocate that evenly, to promote good things. And we weren't getting our share. And if you ever interview Hal Zirin, who for a number of years has been using the old 90-foot interferometer at Owens Valley, he also will grumble, because he claims he's been charged too much and can't do things.

So there have been managerial troubles at Owens Valley. Sometimes it's been very difficult. And I presume this is the only observatory in the world that runs this way now. It doesn't make sense. It's run like physics projects—that is, in the physics department here, each professor gets money to run his project. There's a division chairman who can kind of push things around a little bit, but the projects are run by the people, and they get their own money. That's what's happening at the Owens Valley Observatory—the projects get their own money.

ERWIN: If you were to contrast that with the Palomar operation?

COHEN: That's totally different. In fact, when I went into the optical astronomy business, I was really amazed at how Gerry Neugebauer, the director, could control what went on and keep things secret. He had a budget that was totally secret. Whereas up at Owens Valley, everything was totally out in the open; everybody knew everything.

ERWIN: It was so democratic, in fact, that it just didn't work? [Laughter]

COHEN: Well, it was difficult. Anyway, during this period, a great deal of the VLBI activity was done on campus—it wasn't done at the observatory. That's this correlator development that I was talking about. You have these widely separated radio telescopes, and you make a magnetic tape recording of the signals at different stations. And then these tapes have to be sent by railway express or parcel post or air freight, or whatever, to some central place, where they are put on a machine called the correlator. Then some mathematical operations are done on the signals coming from the tapes. There's a cross-correlation, and from that the images of the sky are made.

ERWIN: What sorts of images? What do they look like?

COHEN: Well, radio images. We make pictures—contour diagrams. We make colored pictures. They're false color, of course, because the signals are coming at radio wavelengths and don't have color in the normal sense. But we use color to show intensity. Bright yellow is hot and blue is cold.

ERWIN: So actually these are correlated and then converted into some visual . . .

COHEN: Right converted into images. Or into graphs that show certain things. But the correlation comes first. Our first correlator for this purpose was built at NRAO in Charlottesville. And it was a two-station correlator.

Now, there's an interesting point. We found out very quickly that we needed many telescopes to get a good image of the source. So suppose you have three telescopes—A, B, and C. The tapes have to be played against each other—that is, you need A times B, and B times C, and A times C. They're all independent, so when you go from two to three telescopes you go from one baseline to three baselines—you get three sets of information. So by increasing the number of telescopes by one you get three times as much information. It's very efficient, so we quickly would go to more telescopes.

Now there's some kind of an inverse rule that goes with the correlators. If you have a two-station correlator—which is what they had at NRAO—then you can do A times B. Then you take those off and go back and do A times C. Then you do B times C. So it takes three times as long to do the correlation as to do the telescope work. If you go to four telescopes, it takes six times as long. And then very quickly we went to five telescopes—and that takes ten times as long.

So the first thing you know, you run out of time. If you observe for a week, using five telescopes, then you need ten weeks on the correlator. But before the ten weeks is up, you're observing again. So you've fallen behind. And it's worse than that, because correlators are a very tricky business. And there were some technical mistakes we made early on. We used very poor tape recorders, and it was even worse than that. Because for a number of years, the only way to get this done was to go physically to Charlottesville and sit there around the clock and nurse this business. So the correlator in Charlottesville fairly rapidly fell a year behind. And people would sign up. You'd go there for a week and try to do your thing, and sometimes it gave good data and sometimes it didn't. And by 1972, which was only a few years after all this started, we and the people from JPL were so disgusted by this that we started to develop our own correlator program. We were going to build one here: Caltech plus JPL—it was a joint development project. And for the first ten years or more, it was all done under the umbrella of my NSF grant. Well, I also had a grant from JPL, which grew to be quite large. The JPL money grew to be a couple of hundred thousand dollars a year, plus some JPL engineers who were assigned to the project. And that was very expensive, because we would sometimes get very high-quality engineers who had large salaries and large overhead. So we were getting the equivalent of half a million dollars a year from JPL—and of course another half a million or more from NSF.

So we built a correlator here. And because we were building the correlator, the local group could expand. That is, NSF not only provided for the observations at Owens Valley—the telescope support and the scientific use of the observatory and some students—but an engineering staff in Pasadena, and programmers to build the correlator.

Our correlator was, to some extent, a copy of the NRAO system, but an improvement. And it was built with an expansion capability, so that instead of having just two stations we could have three, and then five. And it took quite a while to make it work. We never kept to a schedule.

We actually started in '72. And the main designers were Dave Rogstad from JPL—he was a Caltech graduate in radio astronomy, working at JPL—and Martin Ewing, who got his PhD in physics at MIT and came here as a postdoc in radio astronomy. He was very interested in engineering and computers, so he gravitated toward that side of things. He was a senior engineer and a real member of both the scientific and technical staffs—although he didn't do much in the way of scientific programs himself. He wrote an occasional paper, but very few. But he was more than just an engineer who did what he was told. He left Caltech some eight years ago, and is now at Yale as the director of their science and engineering computing facility. He's a very good guy. Dave and Martin Ewing worked very well together. Dave did most of the hardware design with some other JPL engineers. He still works at JPL.

ERWIN: Was it your idea to do this?

COHEN: Well, yes, my idea. I initiated it, and I got the money. Arthur Niell, at JPL, also had the idea. We were very close with the JPL people. It's a little hard to know who was the first person to have the idea; it might have been Marty Ewing. We did a lot of playing off against each other. For a half a dozen years, six or eight of us from that group would go to lunch four days a week—we were very, very close. We usually would go to the BC—Burger Continental, on Lake Avenue. We really were close scientifically and from the construction point of view.

How long did it take to get the first working correlator—the first two-station system? Probably it must have taken three years, or maybe more. I can look all that up. And, of course, when it was working, we got data. But then it turned out it wasn't working, because there was some bug in it, and we'd have to redo things. That even happened at NRAO, two or three times.

So these things are very difficult, and they take much longer than you'd think. And you end up with much more time and money spent on the software, on the computer end of it, than on the hardware. And that's the universal experience with these correlators.

ERWIN: Did you get to the point where you felt that the whole thing had worked and been worthwhile?

COHEN: Oh, yes. And we then very rapidly went from two to three to four to five stations. We had a five-station correlator, so if we did a five-station experiment we could put the five tapes on at one time, and play the whole thing back. So if we had an experiment that took a day, we could do all this correlation in a day—well, actually it took longer.

Then we set up a whole production scheme. We ran the correlator twenty-four hours a day, seven days a week sometimes—sometimes only five days a week. We hired professional operators and students to work on that. And for a number of years, we had this whole big production system going in the basement. And it kept being improved. We kept getting better and better tape drives. And we finally went to the VHS tape drives—the home VCR. The people in Japan made millions of these things, and they had spent millions of dollars perfecting them. And they were very much better than the expensive ones we'd been using.

ERWIN: And was it just your data that was being processed?

COHEN: No. We became a center for the whole world. And of course, by the time we had the five-station system going, people were making eight- and ten-station experiments. So our system was still too small, because it took many passes through the system to make it work. Even before it was finished, the five-station system was recognized as being much too small. Meanwhile, NRAO had thrown theirs away and built a new three-station system. And there were new broadband systems that had been built at Haystack by MIT and also at the Naval Observatory and in Germany. And in Russia they even built a copy of our thing. People in China wanted to build one. They sent people over to work with us for a while, but they never got one working in China. In Japan, they've since built a different one. A lot has happened in the last ten years.

ERWIN: Is this correlator still going today?

COHEN: No. What happened was that about the time this was finished, the people at JPL, on their own initiative, began developing a big new correlator—the so-called Block II Correlator—broadband. And we joined that project, Caltech being a junior partner. And this was a very expensive development, because they were using these big tape recorders that cost $100,000 apiece or something.

ERWIN: Now, was this related to their planetary missions?

COHEN: Well, yes and no. Their excuse for doing it was engineering development, in part. But by that time they were also doing routine VLBI experiments with the Deep Space Network, as a means of defining the location of some of their reference objects. Those reference objects then were used for guidance of interplanetary probes. They had to have precise coordinate systems. So it was a navigational thing, but it was a science thing also. It was multipurpose. And it was clear that they would use it only a fraction of the time and we'd be able to do science on it from the NSF side. So we've had various memoranda of understanding—and I guess there was another one connected with the Block II system.

The broadband correlator they built was big enough to correlate sixteen of our narrowband tapes—the VHS home-video-style tapes—sixteen at one time. So that was built and began working in the late eighties. And there was a lot of tension. It was a JPL project, but it floundered, and it had low priority at JPL. And then it turned out that the people working on it weren't quite up to the job of getting it done right. They had made a fundamentally wrong decision at the beginning, of going to very big boards. A board is an electronic plate on which you plunk down all these chips. You've seen computer boards—well, this thing is three times wider and five times longer than a computer board. And the problem was, there was so much on the board that there was a lot of crosstalk—interaction from one wire to the next—which wasn't recognized originally. We couldn't get answers out, and people didn't understand it. The software wasn't working. Ewing was working on this full time, but Rogstad wasn't, because he had been taken off to work on supercomputer projects. So they had what I called the second team on the hardware. And we had some fights—me and a man named Bill Melbourne, especially, who was the manager at JPL. Melbourne's a Caltech graduate, by the way, in astrophysics. I kept saying we weren't getting the people we were supposed to get, and they kept saying, "We'll take care of it."

Finally they did. That was a very aggravating couple of years. It was extremely aggravating, because we couldn't get things moving. And we had commitments. We had committed ourselves to using the four-station broadband Block II system they were building. And this had to do with competition with MIT, again, at Haystack. We had commitments to process experiments for NASA, and I got drawn into that. And the people back east said, "No, you guys aren't going to get done in time. You don't know what you're doing." And it turned out they were right; we didn't get done in time, and we didn't know what we were doing. It was very aggravating. And I think it's the only period in my life when I didn't sleep well. It got so bad that the guys at JPL said, "Well, we can't control this. And the person who's supposed to be watching this can't do it." So finally, in some meeting over there, I said I would do it. So I became the manager, doing JPL's job of being in charge of the construction of this thing. And we had weekly meetings and diagrams, and people came from JPL, and we sat around a big table. People had to report to me on what progress they had made. But it didn't do any good, because the people didn't know what they were doing.

ERWIN: So how did that resolve itself?

COHEN: It resolved itself when, as I like to put it, JPL changed what they were doing—their priority, in some sense—and we got a first team. It was a management decision over there.

Finally we got some good people. And I had even gone and complained to Bruce Murray [director of JPL], at least once. I would threaten the people underneath. I'd say, "Well, you know, I'm going to have to go and complain to the top management about this," and I did. And, of course, when I'd go and complain to Bruce Murray, what he'd do is tell one of his underlings to please take care of this. And then that message would get amplified as it worked its way down to the guys I dealt with. And they'd just fall all over themselves to do things.

But it never lasted, until a guy named Elliott Sigman was put on the project. He worked for JPL, and I had the impression that he was not in the classic JPL mold of following exactly in the line of what he was supposed to do. He didn't fit into their administration mold.

Anyway, he became part of this project. And he was really very smart. He found out very quickly what the fundamental problems were and began fixing them. They also hired a guy named Dave Fort—a radio astronomer from Canada who was into instrumentation. So those two guys worked for a number of years, with some others. And they began clearing up the problems. And ultimately we had a sixteen-station correlator working. For a number of years, six or eight years, it was the world's biggest correlator. People came from Europe and from the East Coast and from Japan and Australia and such places to use our machine. So we were a center for quite a few years. We were operating on what was called a fraction of a shoestring, but we did get enough money out of the NSF to hire a couple of full-time operators. And they would sit there and grind out these tapes and so on. And when it got very busy, we would hire students and make them work on weekends. So for years we went on in this fashion. For a long time, the daytime shift was reserved for engineering, and the JPL guys would work on it. And then we would do production correlation from five o'clock at night until eight in the morning. That went on for a number of years. An awful lot of work got put through, because they kept making the system more stable, with better and better calibration. I spent a lot of time on that, as the manager of that whole operation. It took a lot of my time for I guess fifteen years. And eventually it got to be much more of a chore than it was interesting. And that, also, is part of the reason why I left radio astronomy.

At some point in there, we got Steve [Stephen C.] Unwin, who's now working permanently at JPL, to be the manager, and he took care of everything—seeing that the guys who worked at night were there and had their vacations on time, and we had enough chart paper. Whatever had to be done, he did it. And Marty went to Yale, and the thing became very much more routine.

So that's the correlator story. And it's part of radio astronomy. But, of course, the people at Owens Valley didn't recognize this, or didn't think much about it. When Nick Scoville or Robbie [Vogt] was the director of OVRO, they didn't care much about what was going on in the sub-basement of Robinson. And I could never, for example, get any extra support for this. That wasn't the case, of course, when Tony was director, because he was deeply involved in the correlator also. But in fact, there was a long period of time when, since I had the biggest piece of money in the whole department—except for Palomar money, I had more money than anyone else—I was able to support things. And particularly when computers were first coming into Robinson, I did a lot of support of computers, because computers were central to VLBI—well, now they are central to all branches of astronomy, but twenty-five years ago you could still do optical astronomy with minimal computer support, whereas VLBI could not exist without computers. Anyway, the VLBI group spent a lot of money on computers we brought into Robinson, and other people benefited from them also. And that was occasionally a cause of resentment, because the others would rarely admit that anything like this had happened. And then in later years, I could never get them to contribute to what I wanted to do. So that was not entirely always simple.

This was particularly true when Robbie was the acting director of Owens Valley [1980-81]. He wouldn't pay any attention to the correlator. He was interested only in what was going on at Owens Valley. And he also was really concerned about the millimeter interferometer, because it needed managerial attention. The other telescope was left to its own devices, and we had money for it. So we had a very good thriving project on one side [VLBI], and on the other was a project [mm interferometer] that was sinking and under a lot of attack. And the only way to keep it afloat was to put a lot of resources into it—money and people. And so there was tension, because they were always taking more than their share, I felt… [Tape ends]

Begin Tape 6, Side 2

COHEN: So VLBI had very strong support from NSF and JPL. And apart from a couple of years when we couldn't seem to make progress in building the correlator, it all went very well, into the late eighties. We led the world in multistation correlator construction; we led the world in science programs in a certain area. VLBI had grown to encompass many kinds of work.

But since the early seventies, there had been a push by radio astronomers—especially by NRAO, and by us also—to build a dedicated VLB array. And that was eventually approved, but came on very, very slowly. The idea was, there would be a dedicated array, run by one organization. The array would consist of all new telescopes. The calibrations would be easy. And it would be run around the clock, and only for this purpose. A lot of the problems with the network was that the VLBI would be fired up only for a few weeks every two or three months. And that was very difficult: for the first week, there would be mistakes made, everywhere, so a lot of bad data came in.

Well, there was a fairly strong opinion here that we—we and JPL—should build and control this new, dedicated array. It was me and Readhead, Marty Ewing, and the people at JPL—David Rogstad and the management at JPL—and the management here. And Robbie was pushing the notion that we would be in charge of this thing. We even wrote a proposal [1980], which I think I mentioned last time—for what we called the Transcontinental Radio Telescope.

ERWIN: That was another name for the VLB Array—the Transcontinental Radio Telescope?

COHEN: Yes. Well, that was the name we put on the cover of our proposal. This wasn't a true proposal for money, but it was an indication of what a real proposal would look like. And we floated that. Of course, we sent copies to the NSF, to NRAO. NRAO, at this time, was occupied with the VLA—it couldn't get that finished. It was very late. And the director, Dave Heeschen, was taking resources out of other places in the observatory and putting it into the VLA. All of their effort went to that. They couldn't begin to think of a VLB array—especially because there was another group of people who wanted to build a large millimeter telescope at NRAO, and they thought that had to come first, before any VLB array. Well, we decided the VLBA would take nearly a hundred people, and we showed how it could be done. And that fired up NRAO, which began to think that they should and could do it also. So they put out a counterproposal.

Well, in 1980 there was the Field committee—like the 1970 Greenstein committee. So at the Field committee, the Very Long Baseline Array was promoted, and it got a good recommendation in the committee's report to the National Research Council. And NSF liked the idea. NSF, if it can get the money, always like to push new projects. NSF and most of the community thought it would be better to give the VLBA to the National Radio Astronomy Observatory. That was a national observatory; they were used to doing this kind of thing. A university would not be well suited—even doing it with JPL. You're going to have to run on the order of ten isolated systems. It isn't as if you would be running one observatory in Hawaii. The VLBA would be ten observatories, scattered all over the United States. You have to run all of them. It's a tremendous managerial and logistical problem. And we kind of backed out of it, because I, at least, and others, got cold feet. I think that had Caltech actually won that contract and built the VLBA, it would have been a big deal. It would have had to be done JPL-style. And nearly ninety-five percent of what went on would be for the benefit of other people—it would not be done for Caltech's scientific purposes. But whatever science you could get out of it, you would also get if NRAO ran it. And you wouldn't have the managerial headaches.

ERWIN: So when you said "cold feet," what you meant was that you had second thoughts.

COHEN: Second thoughts, yes. I did—JPL always thought it was a good idea. But there's also the point that outsiders continually hounded us for being together with JPL. JPL had a bad reputation, because they're so expensive. And it's true; costs at JPL are much more than they are at a university or at NRAO. They have a very heavy management structure, compared with a university observatory or NRAO. It's just a heavy-handed operation. NRAO can and does build a satellite receiving system for a couple of million dollars, whereas JPL builds the same thing for $15 million or something like that.

Anyway, second thoughts. The construction itself, by the way, would have been a big thing. We would need JPL to do that. It's a big contracting effort. You'd have to spend $100 million. It's like LIGO. LIGO at least had Robbie Vogt pushing it. We wouldn't have had anybody like Robbie. Some JPL person would have had to be the general manager.

But we thought we could, and should, build or design or operate some aspects of it. And Haystack wanted some piece of this action, too. And we regretted the continuous drift of radio astronomy to the national observatory. In the end—and I had some fights with NRAO that I lost—NRAO basically built and designed the VLBA. Haystack did some design of parts of the correlator—things connected with the tape drives. They had real expertise on that. But they operated as a contractor. And people from here—Tim Pearson and Marty Ewing—were on various advisory committees connected with it. So there was a national group on top of it.

And now there are ten new telescopes in the world because of this. But not one is on a university campus. One of them is at Owens Valley, but it's not on the land of the Owens Valley Radio Observatory—it's a hundred yards outside. They are independent operations, wherever they are. The plan originally had been to try to co-locate them with university facilities to make life easier. And indeed, up in Owens Valley, I think those people must occasionally use the machine shop and other facilities. I know for a while that there was some tension, because the NRAO technicians at the VLBA site felt they weren't welcome—and I think that probably was the case.

ERWIN: So there truly didn't develop cooperative efforts. It's truly gone in a different direction.

COHEN: Yes, it's gone in a different direction.

ERWIN: Was JPL ultimately involved?

COHEN: No. Although I guess Rogstad and maybe some others were a part of this advisory system. And the whole thing is now run from Socorro, and it's working very well. I mean, the calibrations are good, and the whole system works better now than the VLBI Network ever did. But, of course, as soon as the correlator at Socorro began working, the National Science Foundation stopped funding university efforts. So we lost all our funding. When we were running the correlator for the world, we were getting a lot of money from JPL and NSF—a few hundred thousand dollars a year. And because of that, we had a big science group. They supported postdocs and students. Well, nearly all that money evaporated over the course of a year or two. All the universities were cut off, because the national observatory was now running the VLB business. If you had a good scientific program, they'd give you money for students. But you don't get any money to run a telescope for this purpose, or to run a correlator. And you don't need engineers. So all of that funding disappeared.

Some years ago—I guess when I turned fifty—I thought, probably somewhat arrogantly, "Well, half my working life is over" and maybe I should do something else. But I couldn't think of anything I would rather do, so I just kept on doing what I was doing. But when I got closer to sixty, I thought again the same—maybe I should really do something else. And this is apropos of why I switched. In the VLBI world, the opportunity to work with equipment was disappearing, because NRAO was taking over. And it was already the case that you couldn't go to all the telescopes and do things.

So the opportunity to work at and with telescopes was vanishing rapidly. And the other thing is, that I'd been doing this VLBI stuff for over twenty years and I was getting kind of tired of it. And I had heard of this new development at UC Santa Cruz about the polarization in Seyfert galaxies. And we had the world's biggest telescope at Palomar, and we could be doing that same kind of work. Not only that, but the Keck Telescope was under construction and pretty soon would be able to do things like that also. So I got a sabbatic leave and went to Lick Observatory to work with Joe Miller for a while. And then I went to France. And I studied optics and polarimetry and things like that, and came back and built a polarimeter. I got a postdoc, a very clever guy by the name of Bob Goodrich. For a couple of years, I basically gave up the VLBI business. I'm back at it, working with the VLBA—not with people locally, actually, but with my pals from the old days, Ken Kellermann and some former postdocs.

So I guess there were three reasons why I quit. One is that the sense of working with telescopes had vanished—it was all computers. Whereas in optical astronomy, you still go to an optical telescope. That's disappearing, too. Now when you go to Hawaii to use the Keck, you don't go to the telescope; you stay down below, mostly. You don't actually go up to the telescope. At Palomar, you still need to know what you're doing. So that's still interesting.

I go to Keck quite a bit. And, of course, I went up to the summit and worked at the telescope itself for quite a while. When I was sick, I couldn't go the summit. But now, more recently, they don't want people to go to the summit. It's much more convenient for everybody if you stay down below and use the facilities there. They have this good link, and you're sitting in a room like this with a bunch of computers. And for all you know, you're at the telescope. That's not quite right, but it's almost like that. You tell an operator how to move the telescope. And there's also a spectrograph control, so that you can control the taking of pictures. And if you can do it down below, you can do it in Pasadena—that will come, too. The problem is, it's very expensive to get the broadband link across the Pacific. But I'm sure that will come. In five years, people will not go to Hawaii to use the Keck Telescope. They'll do everything from the basement of Robinson—that's my guess.

Anyway, there was that. And there was the disenchantment with what I had been doing. It was going stale. And so I'm really very pleased that I made the change, because I built with my own two hands the polarimeters for both Palomar and for Keck. The Keck one works very well. It's an improvement of the one that is at Palomar. When I say "with my own two hands"—I didn't do the machine-shop work. There were other people who physically used their fingers. But I was involved and I tested it and I talked with the engineers who did the detailed design, and so on.

ERWIN: So this is instrumentation for the Keck and Palomar, and other people use it?

COHEN: The system at Keck is used by several other groups, including people who are competing directly with me on the same science project.

ERWIN: How does that work, by the way? You made the instrument.

COHEN: I made the instrument. But it's a public instrument. Keck gave me money, and it's a public instrument. And I knew that going in. And it was worse than that, because if you build a big spectrograph, you've got time ahead of everybody else. You had a lot of time for engineering time, which you could use to do science also. I didn't get any of that. My project was too small.

ERWIN: So you don't get extra time, necessarily?

COHEN: No. But I have had a lot of Keck time, and we have done a lot of work and written a number of papers. A couple of big papers will come out in about a year.

ERWIN: But you don't feel as if you ever were scooped, then, on…?

COHEN: Well, in one way I was. But I managed to get enough time. I don't feel terribly bad about that. I think the scooping that we had in the VLBI with the MIT people was worse. Well, I have never been exceedingly competitive, like the people who eat themselves up because somebody else got there first or something like that. Having made the polarimeter, and having it work well enough for other people to use it and publish scientific papers with the data they get—I think that's a comfortable feeling. I can take some satisfaction from having made something that works, and I have reaped some benefits, to a reasonable extent.

But it is the case in radio astronomy over the years that people would build special receivers, and these would be their own private instruments, and they would take them to some telescope and use them, and then they'd take them away. And they were the only ones who could use it, and get that kind of information. That kind of thing did happen, but I never did that. That's quite close to a physics experiment, where you do a particular experiment in your own lab and it's your experiment.

So I worked at Palomar; and I've been working at Keck. My interest was in looking at these galaxies in the same way that Joe Miller did, and collaborating with him. He studied the polarization of Seyfert galaxies. And he showed that the light is polarized by a reflection, a scattering by electrons or dust. And there are various deductions you can make. And the conclusion is quite startling: in these galaxies, there's something like a donut of dust around a very bright nucleus. And so here's this donut, and there's a bright spot in the middle. And if the donut is facing sideways, you can't see the hole, but the light can escape out the side and get scattered off electrons or dust. So some of that radiation gets scattered toward you. And if you can study that polarized light, you can get a look from the outside at what's going on in the middle. Then you see that what goes on in the middle is very much like what happens in some other galaxies. There are Type 1 and Type 2 galaxies, with characteristically different spectra, and they were thought to be different beasts. But the polarimetry showed that they were the same; they just differ in aspect. In some, the nucleus is visible through the hole in the donut, and in others it isn't. Thus, we—along with others, of course—are changing the taxonomy of galaxies. Instead of there being these different types of objects, they were the same, just looked at from a different direction. Joe Miller and his students first did this "unification" for Seyfert galaxies, then there was the question of extending it to the much more powerful radio galaxies. And that's what I've been doing at Keck. We've really shown that unification works in radio galaxies. I have the data now to write a big paper, and it's been started.

The fact that we had this new capability at Palomar meant that we could look at other things, too. We looked at broad absorption line quasars, and there's a student now who's finishing a thesis on that. He's working at both Keck and Palomar. Another type of object in the sky which is highly polarized and is very interesting is a magnetic white dwarf star—a certain type of star that has a strong magnetic field on the surface. And the radiation from that star can be polarized. So we studied a bunch of them. And a young woman—Angela Putney—wrote a PhD thesis on those magnetic stars using the polarimeter at Palomar.

So all of that, I think, was quite successful. And that work is still going on.

ERWIN: You're still active in this field. So you have a group of followers?

COHEN: Well, that was the case with VLBI, of course. Tony Readhead came as a postdoctoral fellow in 1974, and then he went back to England for a while. Then he came as a senior research fellow or whatever it was called. And then after a while he was professor, and then he was the director of the observatory. So he came way up. Actually, we were working mostly separately, although we were together in the NSF grants. Then I began to drift off and do other things, and I remember what happened one day. We were talking. I was going away, and I asked him if he would take care of things for a while. Well, he wasn't so sure. Then I said, "Well, maybe you should be the PI." This all happened in a flash—Principal Investigator of the National Science Foundation grant—that's a lot of money. So being PI shifted from me to him. Readhead always liked to be in charge of things. He's very imaginative. And in addition to doing his VLBI stuff with me, he was doing, on the side, this microwave background stuff which he's now gotten into. And then I went off on sabbatic leave to Santa Cruz, and he picked up the torch. He had already had the torch, in fact—he just wasn't the PI. But he had been a co-PI on the grant.

Then I was the co-PI on the grant, although I really was doing very little. Since he was doing so many things at one time, he needed another name on the NSF proposal, in order to show that there was enough manpower. And, of course, for quite a while my salary recovery still came from that grant. I worked on the VLBI stuff a little bit, but not much—worked with a postdoc for a while. Anyway, all of that has stopped, because NSF cut our funding off. I guess there's still one Caltech student doing VLBI stuff with Readhead.

Tim Pearson was another postdoc who came from Cambridge. I guess Tony identified him first; he stayed close to the Cambridge folks and knew what was going on there. Tim worked on my grant, mostly as a programmer, and over the years he has worked more with Tony than with me. There have always been a lot of British people here in VLBI, because there's a lot of interferometry in England. Tony is the one who's made the most progress here. But Tim Pearson's been here since the mid-seventies. Steve Unwin came here in the late seventies, and now he's working at JPL. And he was from Cambridge. And there's another guy, Martin Shepherd, who's here as a programmer now, and he's from England, too—Manchester.

ERWIN: We talked early in the interview about the differences in culture between the radio astronomers and the optical astronomers. Do you want to add anything more about that?

COHEN: My experience in the world of optical astronomy is that the optical astronomers are more competitive, more concerned, and maybe more secretive about what they're doing than the radio astronomers. And that's related to a number of things. One is that at optical observatories, there are these independent programs, so you may have some idea of what other people are doing, but you don't really know; whereas at NRAO, everybody knows everything that's going on. It's all public. You can see the schedule for the telescopes; for the next six months, you see who's doing what. There's much more openness. To be sure, there is competition in the VLBI world—people who are trying to do the same kind of thing. But there's very much more selection—that is, you're assigned to do a particular thing and you do it. It's difficult to change what you're doing. But at an optical observatory, people can change what they're doing. You write a proposal to do a certain thing; then when the time comes, you can do something else if you don't want to do what was proposed, because of whatever reason. That's because you're controlling the telescope yourself. You do whatever you want. At NRAO, with the VLBA, and the VLA, and the big new telescope they're building at Green Bank—there, the management is running everything. They set the telescope, and they have the schedule that you made months ago, and you've got to follow it. If you want to change the source you look at, they will first check to see if anyone else is looking at that source, and make sure that you're not repeating something other people have already done or are planning to do. But at an optical telescope, we do whatever we want: if there's something hot, forty people can do it all within the same month, and then there's a big scramble to see who gets credit for it.

And in radio astronomy papers—apart from some sensitive people—radio astronomers have a rather more laid-back air as to who gets references and who your co-authors are. Whereas the optical astronomers are very worried about the niceties of all that.

ERWIN: Isn't there some tradition here, maybe relating to the fact that radio astronomy is a newer field and it developed in a kind of cowboy atmosphere?

COHEN: That's right. Frank Drake and Fred Haddock, two old-time radio astronomers, used to speak of radio astronomers in general as ruffians. And that's true of people my age or somewhat younger. But there's no cowboy version of it anymore because the facilities aren't being built privately now. If you have an idea, you go to the national telescopes. The National Radio Astronomy Observatory for a long time has had a very good reputation—a far better reputation than the National Optical Observatory. The Kitt Peak Observatory has been fighting with private observatories, like Palomar and all the other big private observatories. And everybody tries to stay ahead of everybody else. But the only places in the US that were big enough, after the beginnings of things, to compete with NRAO were Caltech and Haystack.

ERWIN: So there's been a trend toward a kind of nationalization in radio astronomy. Or is that too strong a word?

COHEN: Well, it's not a word that's normally used, but you're right.

ERWIN: And that hasn't happened in optical?

COHEN: No, no. It's going the other way. Look at Keck Observatories. They just spent $200 million or something for the world's two biggest telescopes. And there's a lot of resentment toward people who use Keck. That's partly a problem for me, because I'm an upstart using the Keck Telescopes.

ERWIN: Resentment by whom against what?

COHEN: Well, for example, I wrote a piece in some conference book. It was a three-page paper, very short. I had limited space. I put in a few pictures and a small amount of text, and not many references. And I got a very serious complaint from a guy who said that I didn't make a reference to his work. So I said, "Look, I only had a couple of pages. It's only a conference report. And anyway, we had thought of the thing also. It's true that you had this publication, but a lot of people had publications, and I didn't have any space to put in a lot of references. And also, three or four years ago, we had thought of doing this."

Well, I think I would not have gotten that from a radio astronomer. Anyway, I left this guy out, and he got very annoyed about it. And I've had similar kinds of situations where I don't give enough credit to other people. And partly that's because people who are not at Caltech are jealous of people who are at Caltech. It's always been that way. They were jealous of Palomar—now they're even more jealous of Keck. And I presume they resent me as an upstart who gets a lot of time on the Keck Telescope and can do things they can't. I jumped into optical astronomy and got to do things immediately, that they can't do because they don't have access to this huge new powerful telescope, even though they have been in the field for twenty-five years. And they finally accumulate enough data to show a point. And then I immediately override and blow them away because I get data that's ten times better. I've done that to a number of people, and I don't think they like it. And I understand that. So that's a problem. And that's part of the sociology.

ERWIN: I'm going to bring this one out of the blue. Would you care to comment on women in astronomy? And in your career, do you feel that there are any issues with women in astronomy? Or do you think women have just been able to pursue their careers right alongside the men?

COHEN: My experience is nearly entirely here. I've had several female postdocs. Ellen Hardebeck—her name then was Gundermann—was a postdoc of mine. And I had a couple of others. Joan Wrobel, who's now working at NRAO at Socorro, on the VLBA. Ellen Hardebeck gave up astronomy and is now something like the chief officer of the Inyo County Air Resources Board. She's fighting the Los Angeles Water and Power Department. And then there was Ann Wehrle, who's now working at IPAC.

ERWIN: What about the women who've been on the staff here—Judith Cohen? Sandra Faber?

COHEN: No, Faber never came. She's working at Lick. She was offered a job here but ultimately didn't come. It was because of her husband, who's a lawyer. Even though Caltech arranged for some job for him downtown, they ended up not coming. She's a very powerful figure.

Astronomy has always attracted a lot of women—maybe not as many as biology. But it's traditionally been a subject where there are quite a few women. If you go to Europe to an astronomy meeting, you see that the audience is half, or maybe a third, women. In the United States, it's ten percent. There are far fewer here.

I've been accused of not understanding women's problems, by a woman astronomer from Hawaii who spent some time here—Ann Boesgaard. She was a visiting professor here [1987-1988]. I talked with her about some issues, and she always accused me of not understanding. Because I claimed that at Caltech, at least, we would lean over backwards to try to hire women and promote them. What I didn't understand, she said, was that women were being discriminated against everywhere. And I kept saying, "Well, you know, in astronomy or physics, when we get a list of candidates to appoint for an assistant professorship, we always try to cut it down to a small number of the very best people, and then pick one. And there's never a woman who ends up at, or very close to the top." That's not quite right, because there are a few women. Judith Cohen is one. And there are two women now in physics at Caltech—one was just promoted to tenure, and one is an assistant professor in physics. There aren't any in mathematics. But our division is slowly, slowly getting better. I guess there are three women professors in PMA.

Well, Ann Boesgaard didn't believe that. She said there were hidden biases. And it's certainly true that Jesse Greenstein was anti-women as professional astronomers twenty-five years ago. I've heard stories about that. He thought that it was a waste of time to have a woman as a graduate student, because when she got out she would only get married and have babies. And that would be the end of her astronomy. So why go through all that trouble—it was a waste of time. He never said anything to the effect that women were inferior. It's just that they wouldn't stick it out. They wouldn't stay in the field, so what was the point?

Nowadays, one doesn't say anything like that. But it isn't just women that don't stick in the field—a lot of people don't stay in the field. There are not enough jobs for the numbers of astronomers who are being trained. So they go and do other things. Women are in the same boat that men are in. In fact, I think women have an easier, or a bigger, safer boat than men. Because there are many places that will specifically pick a woman. If there's a woman who ends up somewhere in the top group, they will pick that woman to be on the faculty, because then they can show that to the dean and say, "Look, we're doing what you told us to do." Bob Goodrich couldn't get a job—he worked with me, and he's very, very bright and talented. He couldn't get a job.

But you know, there are other talents you need. It's more than being smart. You have to have a certain ability to present yourself, and maybe Bob wasn't so good at that. But he was very resentful. This was a few years ago, when there was a strong move toward making sure we got more women on faculties. He's now working at Keck as an instrument specialist. He enjoyed instrumentation, so maybe he'll be happy at what he's doing now.

ERWIN: But there are women in astronomy who do have intellectual attainments on the same scale as the men?

COHEN: Well, Sandra Faber is above nearly all men. I mean, she's at a very high level. She's busy with her scientific programs on the Keck Telescope. She's an intellectual leader and the PI of some programs at Keck and on the Hubble Space Telescope.

ERWIN: Did you say she's at Santa Cruz?

COHEN: Yes, Santa Cruz. She has refused national positions, because she's too busy and doesn't want to get into administration, or spend all her time being an advisor in Washington. So there's an example of a woman who's at the very top of things. And there are others—Andrea Dupree at Harvard is a very prominent woman astronomer.

ERWIN: So it seems that women are finding their way?

COHEN: Yes. Not as many as one would hope, or think. There's been a lot of discussion about women in astronomy. There's the Baltimore Declaration, or whatever it was called, where a bunch of young women at a meeting were having difficulties getting jobs, getting promotions, and they put up a Bill of Rights, or whatever it was—and that's referred to a great deal. And I think some of the women who are involved in that and who can't get jobs and are claiming discrimination are perhaps second-rate scientists rather than people who are truly being discriminated against. Because there are a lot of people applying for every position in astronomy. [Tape ends]

Begin Tape 7, Side 1

ERWIN: You were close to [former Caltech president Marvin (Murph)] Goldberger personally.

COHEN: Yes, I would say so. I still see him once in a while.

ERWIN: Would you like to comment on his presidency?

COHEN: Well, I guess it must be true—and I agree with the general characterization of him by the faculty—that he was a weak president. He was nowhere near forceful enough with Robbie Vogt when it was necessary, for example. And I gather there were other occasions, which I never really understood—but nothing anywhere near as dramatic as that. That was the most dramatic thing, his fight with Robbie.

ERWIN: And that ended up with his firing of Robbie as provost.

COHEN: Yes. But it had gone on for a long time. I don't understand the dynamics of what happened, but he somehow couldn't, or didn't, force Robbie to do what he wanted early on. At an exceedingly reduced level, I understand that. I was executive officer in astronomy—a very low position. You know, executive officers at Caltech are at the bottom of all administrative levels.

ERWIN: I thought they were second to the division chair?

COHEN: Yes, but there's nobody below them. [Laughter] So I had to go listen to Robbie. And he would shout at me. And it got so that I would shout back. He's almost the only person I ever really shouted at. We really had shouting arguments. He was quick to shout. And I got so I refused to take it.

Harry Gray quit as division chairman [of Chemistry] when Robbie was provost, because he just couldn't take all that abuse. And Robbie is abusive—that's one of his techniques. And sometimes I think it's deliberate. He can be extraordinarily charming also. But sometimes he loses his temper. And that was his trouble with the National Science Foundation. That's why he had to quit being boss of LIGO—in part, anyway.

But when I was EO, there were occasions when . . . I guess, like Goldberger, I'm not a dynamic, forceful person. It's very hard for people to analyze themselves. Robbie is a dynamic, forceful person. He gets other people to do things his way. I don't do that very often. In scientific groups, we do a lot of talking. If people want to do something else, I won't push them. I don't push people to do things. So in that sense, I guess I'm not a natural leader—or not a forceful leader. And there were occasions, when I was executive officer, when people wanted something done a certain way and I was very reluctant, so I wouldn't go along with it. Then they went and did their thing anyway, in spite of me. That happened a couple of times, and that was very annoying, but I think that's probably the kind of thing that Murph must have run into with Robbie. Robbie, being very self-confident and opinionated and a better organizer and manager than Murph, could see how things had to be done. If Murph said he didn't want Robbie to do that, Robbie would do it anyway. And that's what happened to me.

ERWIN: So if Robbie hadn't been there, then Murph's presidency would have been different?

COHEN: It would have been different. Whether it would have been better or worse, I don't know. In my particular case as executive officer, the things that other people did were of no consequence. So I ignored it. But a more domineering person, or a person who was maybe more nervous about his own position—a little more worried about whether he was in charge or not—would have blown up and been infuriated and insisted that people stop doing whatever they were doing. Robbie Vogt used to threaten to quit continually. That was his method of dealing with other people. "If you don't let me do this, I will quit."

ERWIN: Well, was he ever allowed to quit?

COHEN: Yes, I think that happened a couple of times with Goldberger. And Robbie didn't quit, of course. So it was an empty threat—if you didn't carry it out the first time you made it. But I'm not quite certain of this. I was never in the office when he was shouting at Goldberger. But I know that there were shouts occasionally that came out of the central offices.

Anyway, that's as much as I know about Goldberger. I never had much to do with any president.

ERWIN: So your life was really more tied up with your division [Physics, Mathematics, and Astronomy]?

COHEN: Oh, yes. I was never the division chairman. I was on various division chairman and provost search committees. And I know that I was considered—when Maarten Schmidt became division chairman [1976], I was probably the number two candidate at that time. I don't know if I would have been a good division chairman or not. It would have been totally opposite from Robbie. Maybe not, because I'm not a very effective arguer. I listen too much to other people's points of view.

ERWIN: Who were the most effective, in your opinion, of your division chairmen?

COHEN: Well, who were the division chairmen? There was Carl Anderson when I arrived. And then he retired. And then Bob Leighton, whom we talked about and whose heart wasn't in it. And then Maarten Schmidt briefly. And he's a very cool customer, but he was division chairman for only about three years.

ERWIN: Did he prefer doing his scientific work to being a division chairman? Did you have a sense that these people had to make a decision, in a sense: "Now I'm going to give up my science"?

COHEN: Well, Maarten may have done that, and Robbie did that. But other people didn't. Ed Stone didn't. And Gerry Neugebauer—they kept their labs running all that time. It must have driven them crazy, because it's two full-time jobs. Gerry was doing three full-time jobs. And it must have been very, very difficult. I remember Maarten saying once—he treated the chairmanship as a full-time job pretty much, and then he was director of the Mount Wilson and Palomar Observatories—that he had difficulty getting back into research after that. I think he was pretty much out of research. And Robbie was pretty much out of research. I guess he's a full-time manager, or administrator, now. Ed Stone, of course, even though he is director of JPL, keeps a hand in his research. And, of course, when he was division chairman, he still was involved in the Space Radiation Laboratory. And Gerry kept up his three full-time occupations the whole time, as far as I could see.

I had difficulties with Ed Stone when he was division chairman. [Stone was div. chairman 1983-88—ed.] I was EO under Robbie Vogt, and then Vogt went on to become provost, and Stone became division chairman. And Robbie—although I complain a lot about Robbie, he was very good in many ways. He was probably the most effective division chairman. He was brilliant in terms of organizational things, and could understand what could be done and what couldn't be done, and how you had to organize people and jobs.

Ed Stone was very highly and tightly organized, whereas Robbie left me pretty much alone in the astronomy department. Ed Stone took literally the notion that he was division chairman. There are no department chairman in Caltech; there are executive officers. And all they do is whatever little chores have to be done. And so—depending on the division, the specific balance between these people—you can do more or less. And Robbie was very busy with big deals. He didn't want to worry about the astronomy department, so I took care of all kinds of things. But Ed Stone worried about everything—every little thing that went on in astronomy, all the way through the division and the institute, and so on. And so all I was doing was what almost could be called clerical work. And I finally told him, "I've only signed on for three years. And I will not do anymore. And you're going to have to find a replacement." And that was the end of it. I just quit. Because I didn't see any reason to be an executive officer when Ed Stone was division chairman. There was no executive work of any kind that was left, except getting people to agree to teach certain classes. That was the only thing there was to do.

But Robbie Vogt, for example, treated me as a person who could speak for astronomers. There are the observatories, Palomar, Owens Valley, and there's also an executive officer for astronomy. And we should all get together. And he tried to bring about discussions, and treated me as if I knew something about astronomy.

ERWIN: Would it be fair to say that Robbie used intellectual capital and knew how to employ that?

COHEN: Yes, that's right. He was very good at that.

ERWIN: One would have to recognize that as a very good characteristic in a manager.

COHEN: Yes. I presume that Ed is very successful at JPL, because he is exactly suited for that style. It's a big organization, so he can deal with a modest number of people and assign things, and he can go to Washington and do whatever has to be done. He's in Washington a lot. But he also keeps involved with his own experiments on campus. And he's the chairman of the board of CARA [the organization that manages Keck]. He's involved in space telescopes. He's involved in all these things. He assigns each of them so many minutes a day, I assume. He's very highly organized.

ERWIN: So you've been at Caltech for thirty years. 1998 is the fiftieth anniversary of the Palomar Telescope. Are you going to participate in it?

COHEN: Well, I'll go to whatever affair there is. But I'm not involved. Wal [Caltech astronomer Wallace] Sargent is the person who's been promoting that. And there'll be a committee of some kind to make up a meeting, I suppose. And I will participate in whatever goes on. But I'm not otherwise involved.

ERWIN: Are you writing the second chapter of your history of the Owens Valley?

COHEN: No. I think about it. But I haven't actually started writing—or I haven't started assembling the pieces of paper that are needed. Next year is the fortieth anniversary of the dedication of Owens Valley. So I've been involved in that.

ERWIN: That's closer to your heart than the Palomar?

COHEN: Yes, it is. Sure. And over the years, I've been to several anniversaries of things connected with NRAO. I go to them whenever there's anything to be done.

ERWIN: Are you interested in history now?

COHEN: A little. Not very much. I'm still working hard on my science. And it's worse than that, because I started taking piano lessons a month ago. And now I have to practice an hour a day. So there's not enough for any of these things.

MARSHALL H. COHEN

SUPPLEMENT

February 10, 1999

Begin Tape 8, Side 1

ERWIN: Today, Marshall Cohen is doing a supplement to his oral history, largely now on the subject of his postdocs and some of the work done by the people that he has worked with over time. Is that correct?

COHEN: Yes, that's right.

I noticed in reading this big pile [i.e., the first six interview sessions] of paper that I didn't say much about postdocs. There was a lot of detail about some things, but I always have felt that I was working with an exceptional group of people most of the time. And I just wanted to get them into this record.

ERWIN: So you're going to start from what time period?

COHEN: I'm going back to when I was at Cornell University. Let me say first that in some areas of biology and chemistry, professors are normally dealing with half a dozen postdocs every year, and so over thirty years they can have maybe a hundred postdocs. In astronomy, it's not that way. And in most of engineering it isn't—at least not where I've been. At Caltech, I've been involved with a number of postdocs, though, and many of them have gone on and done good things. And I've been involved more with postdocs than I have with students. That is, I've not had very many graduate students—there's not much about them in my interview—although some of them have been very good, too. But I'm going to talk about this list of postdocs. I think I've already mentioned a few of them. Some of them are already mentioned in the manuscript, but I'll just go down this entire list.

The list starts at Cornell University. I went to Cornell in 1954. So the first name here is Kenji Akabane, who was from Japan. That would have been in, I think, 1958. He wasn't a postdoc in the normal sense of the word, because he had been invited to Cornell by a man named Takeo Hatanaka, a Japanese solar physicist, who'd worked at Cornell University. Hatanaka had a young student that he wanted to come to Cornell and work with the Cornell people, so Akabane came to Cornell. And I had been there for a while, and I'd been building the polarization measuring equipment that we've already talked about. He and I worked on that together and wrote several papers. He worked for a year, and then he went to the University of Michigan, I think for a year or a year and a half. And then he went back to Japan. He had a wife and two daughters at the time. We were quite friendly with them, and visited them in Japan. I saw him twice in Japan over the years. We were even invited to his house once, which was very unusual, very pleasant.

ERWIN: Unusual in the sense that this doesn't happen too often in Japan?

COHEN: That's right. People in Japan live in tiny little houses, and there's no room. He had a somewhat bigger place than most people. He rose to be professor at Tokyo University. He was an associate director of the Tokyo Observatory—director of the radio astronomy branch. And he drifted away from studies of the sun and worked on millimeter astronomy. He retired at age sixty; as far as I know, in Japan, you have to retire at age sixty. Then he went on to teach physics at some college on the Sea of Japan. That lasted a few years, and then I think he had to retire from that. And then he went on to teach at a small girls' school in the middle of the country. He just didn't want to stop. As far as I know, he has now stopped. I've been out of touch with him now for about five years. I think he has completely retired.

Anyway, he was very good. He worked very hard and very diligently. I remember that he had a bicycle. He would bicycle out to the observatory, which was near the Ithaca airport, and we would have a good time. Akabane was the only postdoc—or equivalent—that I was involved with while I was at Cornell, until the Arecibo program started. Then once that was working, I got—"hired," I guess, is the right word, because once Arecibo started, I was in charge of the radio astronomy program there—I hired a woman named Ellen Gundermann, who had just graduated from Harvard [1965 PhD]. She came to Cornell and worked with me on Arecibo data on interplanetary scintillations.

ERWIN: May I ask you a question here? You hired her. Did she apply to work with you? How did that work? Was it any different from other disciplines? You'd have a graduate student, or even a postdoc, write to you and request to work with you? Or did you seek her out?

COHEN: She probably applied for a position. That is, the Arecibo Observatory had real postdoc-type positions. There were three areas of work at Arecibo—ionospheric physics, radar astronomy, and radio astronomy. And each had its own allotment of people that could be hired. We hired Ellen.

ERWIN: And at that time, how long had she…?

COHEN: She had just finished her PhD. She came straight out of a PhD at Harvard. She lived in Ithaca, but she spent a lot of time at Arecibo. At the time, there was a graduate student named Harry Hardebeck. And what happened after a while was that Ellen and Harry got married. First Harry had to divorce his wife—I mean, this went on for a number of years. He got divorced, and then, after a while, Ellen and Harry got married.

There was another postdoc who was hired in a somewhat different way, named Dan [Daniel Everett] Harris. He was a Caltech graduate [PhD 1961]—he was the first radio astronomy graduate from Caltech. He had graduated in the early sixties and had worked in a number of places.

ERWIN: Whom had he worked with here at Caltech? That was before you were here.

COHEN: Yes. He worked with John Bolton and Gordon Stanley. Dan Harris was early enough to have worked with the small dish at Palomar, and then he went to OVRO. Anyway, after he graduated, he worked at several other places. He was somewhat peripatetic. He and some others decided to sail from England to Australia in a trimaran. Along the way, they stopped in Puerto Rico. At that point Dan decided he had had enough and quit the boat trip. He got off, and I gave him a job. And then he lived in Arecibo for several years.

So for a while there were three people working on this interplanetary scintillation study. There was Dan Harris and Ellen Gundermann—and at the beginning of that period, Harry Hardebeck, who was a graduate student, although his thesis was on a different topic. When he graduated [1965], he went to the University of Pennsylvania, to the Electrical Engineering Department. He got in assorted troubles there and didn't get tenure. Then later he came out to Caltech, because by that time he had married Ellen Gundermann and I had hired Ellen here [at Caltech] as a postdoc [1969]. So Harry came out as a husband; he had no job. Then he got a job as an engineer at OVRO. So they moved up to the Owens Valley after Ellen's position as a postdoc finished, and Harry worked as an engineer at Owens Valley until about two years ago, when he retired.

Ellen Hardebeck did not stay in astronomy. She did other things. She had a child, Jean, now a graduate student in planetary sciences at Caltech. And then she got a county job. For a number of years, she has been the chief operating officer of the Great Basin Air Pollution Control District, and she fights with Los Angeles. She was in charge of the Inyo County end of cleaning up Mono Lake. She was involved in a lot of legal battles, and she's still doing that.

So Harry and Dan and Ellen were hired in very different ways, but they all worked on this interplanetary scintillation business. And that lasted even after I left Cornell and went to San Diego. We wrote a number of papers that were very highly regarded—very nice work. And I really had very good experience working with them. That was the first time I'd worked with a group that way, and it was very pleasurable, because they were smart and they worked hard and they knew how to enjoy themselves. They had a good time. It was a very good group, a very good team.

ERWIN: It seems to me that's very important.

COHEN: Yes, that's important. None of us—including me, if I may say so—was so egocentric that we had to push our views or be first on a paper. There were a number of papers that came out of that work. In that program, it was a matter of studying fluctuations in the solar wind. We were looking at small extragalactic sources as they came near the sun, and we would see what the solar wind did to make their radio signals jitter. It's the kind of jittering you see when you look at a star; we could see the same effect with radio wavelengths. From that, you can say something about the diameter of the star, or the radio source. We did a number of surveys, and that work was superseded within a few years by VLBI, which could measure the same kind of thing but much more precisely, or easily. Some of the people who were either our competitors or our collaborators—people in England and at UC San Diego—have continued over the years to work on these interplanetary scintillations. So there's still a little bit of that going on.

Anyway, Harry Hardebeck is retired. Ellen Hardebeck is working up in Inyo County. Dan Harris is working at the Harvard-Smithsonian Center for Astrophysics.

Then, still at Cornell, now getting into VLBI—I was doing the interplanetary scintillation business and VLBI at the same time—Dave Jauncey was hired as a postdoc. He was hired because of a connection that Tom Gold had made between Cornell and Sydney University. The Cornell-Sydney University Center for Radiophysics and Space Research. Jauncey was an Australian who was brought over under the aegis of that program.

There's another one whose name I should mention—Les [Leslie E.] Sharp, who was brought over under the same program. Those people weren't hired explicitly to work with me, but Dave did work almost entirely with me. Les Sharp did partly. Les was a plasma physicist, and we were involved still in this interplanetary scintillation business, which involves plasma physics. Dave Jauncey had studied cosmic rays in Australia. He and I were the ones who started the Cornell end of VLBI, as I mentioned earlier, along with our colleagues Ken Kellermann and Barry Clark and Claude Bare at NRAO. That was in 1965, and Dave Jauncey came just about that time. And then I went to UC San Diego and then to Caltech. Dave stayed at Cornell for a long while, but eventually he went back to Australia. He's been very successful in Australia these last years. I'm not sure exactly what his job is now. He lives in Canberra. But he works a lot with the Deep Space Network. I'm not sure if he's hired by JPL to work in Australia, or whether he's hired by the Australian National University to work with the Deep Space Network. It's one or the other. But he's been very successful.

ERWIN: Did you write papers with him?

COHEN: I wrote papers with him. I wrote papers with all these people—without exception.

So Dave was hired at Cornell, and then the rest of this long list I have was hired at Caltech. And, of course, once I got to Caltech and got the VLBI program started, I began to get a lot of support from the National Science Foundation. And then I got more into the normal Caltech mode—having my own grants and my own program and people that I would hire to come and work for two or three years, although even that got mixed up a bit.

The first person who came that way was Richard Schilizzi [in 1973], who was from Australia. He stayed here for I think three and a half years and was in on the early days of VLBI. And then very quickly, after Richard, I hired a couple of other people, both from England: Peter Wilkinson and Tony Readhead. Tony, of course, stayed on. He went back to England for a bit, but he's still here—he's a professor here. And then Martin Ewing, who was an MIT graduate, came shortly after that. And there was a period of time when Tony Readhead and Peter Wilkinson and Marty Ewing and I were all working on VLBI. Over the years, especially in VLBI, we had a very prominent group. We had what generally were regarded as the best postdocs. That is, the people at NRAO and at other places would complain, because some very bright person would graduate from somewhere and was clearly on the market as a postdoc, and we would hire them. They would get offers from other places, but nobody else could hire the best people, because we had the biggest and best program, so these people were attracted to Caltech. We had the telescope here, and we were building the correlators, which I've talked about. So there was a lot going on. And we had a lot of money. It all feeds on itself: you get money if you're successful, and you're successful if you get money. The fact that we were building the correlators was important in terms of getting money from the NSF.

Readhead had been a student at Cambridge; Wilkinson had been a student at Manchester; Ewing at MIT. After his three years here, Wilkinson went back to Manchester, where he's stayed ever since. He is a reader in radio astronomy and until very recently was acting director of MERLIN—that's an acronym for Multi-Element Radio Linked Interferometer Network, which spreads over England. He has a major role in the European VLBI program. So he's become a major player in Europe over the years.

Tony Readhead returned to England for a year, and then he came back here as a senior research fellow [1976]. And then after a few years, he became a professor and was director of the Owens Valley Radio Observatory [1981-86]. And he's been running his own big independent program for a long time. In fact, the later people, some of the names I will mention, were working both with him and with me. To some extent, it was mixed up for quite a few years, in the sense that we had one grant from NSF, which supported both Tony and me and an assortment of postdocs. There was somewhat a division of labor, but not a very precise one.

ERWIN: Will the lineage become clear as we go along?

COHEN: It gets a little more mixed up later, as a matter of fact. I've left off some names of people who were working a lot more with Tony than with me.

ERWIN: In a sense, then, Tony Readhead split off a little bit and he acquired his own group.

COHEN: And then he got some of his own money, but for a long time there was only one grant. Then ten or twelve years ago, that changed. He became PI. At about that time, I went off on a sabbatic leave and started working in optical astronomy, and pretty much dropped my connection—not a hundred percent, but to a large extent. And Tony became the sole boss of the VLBI program. And the VLBI program now is pretty much wiped out here, because I'm not doing it and Tony is working on the cosmic background business and not much else.

To get back to these postdocs of thirty years ago. So Readhead and Wilkinson have done very well, evidently. Martin Ewing, who was from MIT—and ended up going to Yale—worked with us, and he was also fairly independent. He became an engineer; he was really more interested in engineering than science. Then he worked with us building correlators—he was the designer. As I mentioned in an earlier interview, he was on the Caltech side of this, with Dave Rogstad on the JPL side. They were the main players, over a dozen years. Marty and I were very close. And during that later period of time, he would have been called a senior research engineer, not a postdoc—but he started as a postdoc. And then, after a while, the place was too small for him. There wasn't enough going on. He went to Yale as the director of computing services for engineering and science. His job was not just to be an administrator—to see if the bills were paid—but to keep on top of what's new, and to see that the engineering and science faculties had the right sorts of computing and could get a handle on it. He's still there, and I'm not sure exactly what his position is now. [Director of Information Technology, Faculty of Engineering—ed.]

Then there are two other people who came from England—from Cambridge: Tim Pearson and Steve Unwin, who came as postdocs in the late seventies. They worked with me, and they worked with Tony. Tim then began to work much more with Tony; in more recent years, he and Tony have worked very closely together. And he's still here. He's now a senior research associate, and he is one of the main workers with Tony on the cosmic background imager. But, of course, he was involved with Tony for many years on the VLBI surveys, and toward the end of that period he worked much more with Tony than with me.

Steve Unwin worked with me a great deal, and after he finished his first years he stayed on as a staff scientist for a while and was manager of the VLBI correlator. More recently [1996], he has gone to JPL, where he now is something like—not the manager but a principal scientist for SIM, the Space Interferometry Mission. That's a program that JPL is involved in where they're studying and designing the spaceborne optical interferometer, which is to fly in another half a dozen years. [Present launch date is June 2005—ed.] So he's got a reasonably strong position at JPL.

And then there's Dayton Jones, a Cornell graduate [PhD 1981], who had a lot of independence, but he also worked with me and with Tony. He worked on VLBI. He's at JPL now, too. Dayton Jones is, as far as I know, working in what must be—it's officially an engineering division to support the Deep Space Network. But he's doing VLBI experiments in support of that, and so he's publishing in the astronomical literature also.

Two other names—somewhat more recent—are Peter Barthel and Tony Zensus. They were both very highly recommended when they were graduating. Both were sort of the whiz kids of their decade. Tony Zensus—Anton is his name; J. Anton Zensus—is a German. He was a PhD [1984] from the Max Planck Institute in Bonn. As soon as he got his PhD, he came here as a postdoc. Peter Barthel got a PhD [1984] from Leiden and, again, got exceedingly high recommendations. They both came and worked very hard for three years here in the mid-eighties. Both went back to Europe.

ERWIN: It seems that many of your postdocs were Europeans.

COHEN: Yes, more than Americans; that's right. There was a long period of time when there were many more students being trained in Europe in radio astronomy and interferometry than there were in the United States. So there were more Europeans to choose from. There were a few Australians also—Ron Ekers, who's very famous now, was a postdoc here, and I worked with him, although he didn't come directly under my aegis.

Anyway, Barthel went back to Holland; and he now is a professor at the University of Groningen. Tony Zensus went to NRAO, where he became a staff scientist. And then he went to Germany a year ago—maybe now it's two years—as the director of the Max Planck Institute for Radio Astronomy, in Bonn. That's a very high position. He was quite young when he got that position—he was just under forty.

ERWIN: Are those Max Planck institutes equivalent to our national laboratories?

COHEN: In some sense, yes. But of course they're very small as compared with Los Alamos or Brookhaven. They're about the size of Beckman Institute. There'll be a big building and there'll only be about a hundred people in there.

ERWIN: Yes. But it's quite prestigious to be involved with one of them.

COHEN: That's right. They're very well funded—mostly by the federal government. And the people who are directors—well, they're Germanic jobs. That is, the person who is in charge—and Zensus will become a professor at the University of Bonn; he hasn't been there long, but that's expected in due course—so Zensus is the director, and he's in charge of everything. He really tells people what to do, in a way that you don't do in the United States. He can really set the direction. Now that's a pretty big institute for radio astronomy, so there are several co-directors. There are three or four directors, and there's a leading director; and that rotates, and I'm not sure who is the leading director at the moment. That person has to go to Munich and report to the Max Planck Gesellschaft. But these co-directors each has his own empire, and they carve up the money. When you're an MPI director, you have a lot of money and you really control things—although you can't fire someone who isn't performing, because there is no such thing as getting rid of a person. But you're very careful about whom you bring in. There's a lot of money, and you have a lot of positions.

Barthel and Zensus, when they were here, did very good work. We wrote a lot of papers together.

ERWIN: Do you think it's rare to find that combination? People who like to tell people what to do and are good at that, along with the scientific acumen?

COHEN: No, I think not. When Zensus first came, it was clear that he had a bureaucratic mind. I don't mean "bureaucrat" in the sense of being in charge of paperclips, but in the sense of being organized; he was a good organizer and understood how a program should run and what the management should be doing. And it was obvious even then—he was twenty-seven years old when he came—that he already understood all those things, and he worked very well in that kind of a situation. And I had the sense that he was destined to become a director. He's very smart, and wrote very nice scientific papers.

Then a little bit more recently, there's [Robert] Craig Walker, who is another graduate from MIT [PhD 1977]. We got good students from MIT, by the way, who came as postdocs. I haven't mentioned all of them. Fred [Kwok-Yung] Lo, for example, who didn't work very much with me, was an MIT graduate [PhD 1974].

ERWIN: So we can consider MIT a feeder school?

COHEN: MIT is definitely a feeder school for postdocs in radio astronomy; that's certainly true.

Joan Wrobel is Canadian, but she did her PhD [1983] at NRAO. And she had Canadian money.

ERWIN: Oh, you can do that?

COHEN: Yes, if you make a special arrangement. I think Mort Roberts at NRAO was her advisor in Charlottesville—although she actually was a student at the University of Toronto. Anyway, she got a degree in physics there, and she worked with radio astronomy. And she got a grant from the Canadian government to spend a couple of years doing research, and she asked if she could come here to Caltech with her money. So we said yes, of course.

ERWIN: I see. So that's another way it can work. They can come, bringing money.

COHEN: She brought her own money. Nowadays, it's much easier in astronomy, because there are other ways for people to get their own money. There are these Hubble Fellowships—there's a dozen a year, or whatever it is. But in those days—this was fifteen years ago or more—it wasn't so easy. And the fact that she wanted to come here is a sign that she thought of this as the best place she could go; she could have gone anywhere, since she had her own money. She stayed here for a few years, at about the same time as Craig Walker, and then they got married. And they both went to work at NRAO, and they still are working at NRAO. Joan had different, assorted jobs there. But now she is the manager of the VLBA headquarters there, and Craig Walker is a staff scientist of some kind—I'm not quite sure what his title is. But Craig was an exceptionally smart guy. He is the son of a retired Caltech physics professor—Robert Walker. Craig was here as a postdoctoral fellow, working with me and with Tony.

ERWIN: Did you say where he had come from?

COHEN: MIT.

ERWIN: So he was not Caltech educated?

COHEN: No. I don't know where he went as an undergraduate. [UC San Diego—ed.] Anyway, Craig really was smart. And he also was the kind of person who could accommodate enormous experiments that had tables full of scientific data that had to be organized correctly in the computer. He could do these very big computer programs that could assimilate very large amounts of data and put out answers. And that's what he's been doing at NRAO, and he's also been working a great deal with the shakedown and the commissioning of the VLBA. In fact, for the last ten years he's been doing that. We wrote some papers together, too.

Ann Wehrle was a student at UCLA [PhD 1987]—one of the very few UCLA students who've come here as a postdoc. We always thought it would be nice to bring some UCLA PhD graduates in. Another one was Dick [Richard M.] Crutcher. He didn't work with me, he worked in spectroscopy at Owens Valley. But Dick Crutcher was from UCLA, came here [in 1972], and he's now a professor at the University of Illinois. So he's done fine.

Ann Wehrle was a UCLA PhD. She answered one of our many ads and came and worked here for a few years. And she is now working at IPAC. She went to JPL, and was working as a programmer. But then she was able to move to IPAC and get a job as a scientist, where her job is to spend I guess roughly fifty percent of her time helping visitors. IPAC has a lot of visitors who use the data center. And she's an independent scientist.

All of these people were working with me on VLBI. In fact, Ann came at about the time when I was quitting the VLBI business. And then I went off on a year's leave. And she continued one of those big programs that we had been doing and wrote a couple of papers. We wrote some papers together. And she is still in the astronomy game, although in some sense she went out of it by being a programmer.

And the last name on here in the VLBI world is René Vermeulen. René is from Holland—from Leiden [PhD 1989]. We hired him [in 1989]. He was an exceptionally smart person—smarter than many of these others. He was gifted mathematically. Actually, I think he must have been hired by Tony Readhead, but he ended up working a lot with me. So I put him on my list.

In the late ’80s I began trying to derive cosmological information from superluminal velocities, with statistical analyses of the distribution of velocity with redshift. The data were too sparse for highly significant results, but a paper I wrote in 1990 with Tim Pearson, Tony Zensus and Peter Barthel did get a little famous. A few years later, René Vermeulen put this on a much sounder basis, and he and I wrote a long elaborate paper in 1994; he was the first author. We allowed for different pattern and beam velocities, and I think this paper will provide the scheme for analyzing larger samples of data when they become available.

René is talented, but at the end of his postdoc here, he couldn’t get the kind of job he wanted, although I wrote him very glowing recommendations. The job market runs in waves—some years it’s quite hard to get jobs, and other years you get lots of jobs. Anyway, he was having trouble. But he’s now back in Holland and has been for a couple of years. He’s working at the Dwingeloo interferometer, and developing methods of using it at low frequencies. But he’s doing other things on the side. He and I and Ken Kellermann continued through the years to work on the VLB Array—Kellermann is really in charge. And Tony Zensus is part of that, too. And all of that started about six or eight years ago. And that was fine, because Zensus and Vermeulen were both available to do all the work. I was doing mostly optical work, and Kellermann was doing lots of other things. So there were sort of two chiefs and two Indians. But then Zensus became a director in Germany, and Vermeulen went to Holland and is now working where he is sort of obliged to do other things. So not a lot is happening on that program these days. It still goes on, but not as effectively as it could if we had a student working on it, for instance. I’m not involved with graduate students. Zensus is, so he can produce some of the work.

Anyway, I like René very much. And he’s now in Holland. He’s exceedingly smart, and I think he will end up in a stronger position.

Then there’re two more postdoc names I haven’t told you about. I’ll do this quickly. More recently, in optical polarization work: When I came back from that sabbatic leave, I hired Bob Goodrich as a postdoc. I had got a grant from the National Science Foundation to build a polarimeter for Palomar. And I also got some Palomar money. Goodrich had been an undergraduate at Caltech, got his PhD [1988] at Santa Cruz with Joe Miller, and he had been a postdoc at Texas. He came here after Texas, and he stayed here for three years. He, too, left here at a time when it was very hard for people to get jobs in astronomy. He's very smart, but he had a great deal of difficulty getting a job. He couldn't get a job as a professor, which is what he was trying to do for a long time, and he got very despondent over that. He had a couple of other postdoc jobs. And then a year ago—I think it's two years now—he took a job at the Keck Observatory in Hawaii as an instrument specialist. So he has a small fraction of his time available for free scientific research. He and I are collaborating. He's another very smart guy. He was a Caltech undergrad, and like all Caltech undergraduates he could be quick. And he had experience in polarimetry, which I did not. And so he had a great deal to do with the design and development. It wasn't a hundred percent correct, everything that was done, but it worked. And then, when we built the Keck polarimeter, we had some advantage, since we had built one at Palomar. Anyway, Bob and I wrote a lot of papers, and we continue to work together now. Bob Goodrich has been a part of the work I'm doing right now on polarimetry, even though he has been working in other areas.

After Bob left, I hired Hien Tran, who is also a Santa Cruz graduate [PhD 1993]. He came here directly after graduating from Santa Cruz. He also had trouble getting a job three years later. He went back to Santa Cruz for a year, and then he went to Lawrence Livermore National Laboratory. They have astronomy programs, and he's been working there, but he also has been working partly with me, at a low level—he and Goodrich. So the people that I've been working with for the last eight years, I continue to work with, but at a low level. I have no students now, no postdocs here. And work goes at a very slow pace, because these other people are doing lots of other things.

ERWIN: How do you swing that? Do you just chat on the phone?

COHEN: E-mail more than anything else. Well, we get observing time at Keck Observatory. We talk when we're in Hawaii. And Hien Tran comes down here. For a while, he had some relatives that were living in Orange County, so he would come down and we'd talk.

ERWIN: Is that a Vietnamese name?

COHEN: Yes, he's Vietnamese, from Vietnam.

ERWIN: He was born there?

COHEN: Yes. He moved to this country when he was fourteen. He went to the University of Texas, and then eventually to UC Santa Cruz. And he just got married and lives in San Jose. But he'll go wherever he can find a job.

I'm also involved with another set of people right now, in Europe. There's Bob Fosbury in Munich and Andrea Cimatti and Sperello di Serego Alighieri in Florence, and Bob Fosbury's student, Joël Vernet, who's French.

ERWIN: Bob Fosbury sounds rather English.

COHEN: He's English. And then part of this group is a former student of Fosbury's named Montse. That's short—her actual name is interesting; she's from Spain and her name is Montserrat Villar-Martin. She's now working in Paris.

ERWIN: Where is this Munich group located?

COHEN: They are in the ESO building—European Southern Observatory. But they are not members of the staff of the European Southern Observatory. They belong to ST-ECF, the Space Telescope-European Coordinating Facility. They actually work for the European end of the Hubble Space Telescope, for the European Space Agency. And there's a little group of them, and they're housed in Munich in the ESO building.

ERWIN: How did you gain contact with them? Through your meetings?

COHEN: Well, I got in contact with them originally because right after the Keck Telescope began operating, and Goodrich and I and others were working, we had new data on radio galaxies that were very much better than what was in the literature and what other people had. And right away, we saw that other people were wrong in some things they had said. So there was a meeting that Ken Kellermann and I organized in Irvine on extragalactic astronomy and some other things. It was a National Academy meeting. And it turned out that Fosbury and Sperello came to that meeting. We were talking there about this new Keck work. And a few weeks before, I had said that we should have—for all the people who are around who are interested in this polarimetry—a meeting at Caltech immediately after the Irvine meeting. And that made a very big splash… [Tape ends]

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COHEN: Well, that made a big splash. And as a consequence of that, Fosbury and di Serego Alighieri, who had been doing this kind of work, saw that they were totally outclassed by the new telescope. And there was no point in their observing anymore. So they then came to me and said, "Can we not collaborate?" And that was just at the time when I was losing some of my other connections. Students were graduating. I had no more money for postdocs. So I said, "Well, I'm involved with these other people"—I'd also been collaborating with Joe Miller at Santa Cruz. So I had to talk with those people. And the end result of that was that since that time, I've been collaborating with this European group. But I've kept up my connection with the others also, and we're right now still writing papers based on the work we did three or four years ago—with Goodrich, Tran, and some students from here, and Miller. So there are two groups, which partly intersect.

ERWIN: So this is an interesting example of how one just keeps going forward, keeping up contacts, developing new interests.

COHEN: Yes. It's difficult.

ERWIN: The institutional support is not as big as it was?

COHEN: Well, I get a little money every year from the Division [of Physics, Mathematics, and Astronomy]. And I can travel and I can pay page charges—although sometimes it's not enough, and I have to go back and see if I can get more. And then [the division] helps me with support. They paid a little bit toward postdocs when I ran out of money. But I could get much more done if I was connected to graduate students in the normal way. They would do their work, but I would be connected with it. Or, if I had money for postdocs; you need a fortune to pay postdocs.

ERWIN: But these people you're collaborating with now are all independent and employed? So it's a different kind of collaboration?

COHEN: It's a different kind of collaboration. They're independent. And the one who's actually working most out of all of that is Joël Vernet, the student of Bob Fosbury. He is working on data from Keck that we have taken. He's officially a student at École Normale Superieur, but he's working mostly on data taken in the United States—that is, in Hawaii. So I see Vernet occasionally, because he comes here about once a year and stays in Pasadena and works on the data for some period. He was here a year ago, and he stayed for awhile.

ERWIN: Does that do it?

COHEN: That does it.