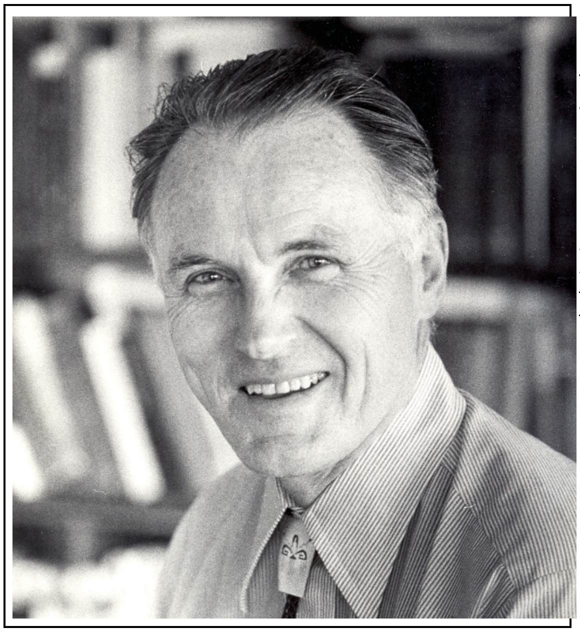

ROBERT B. LEIGHTON
(1919-1997)

INTERVIEWED BY
HEIDI ASPATURIAN

October 8, 1986 – February 12, 1987

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, astronomy

Abstract

An interview in seven sessions in October and November 1986 and January and February 1987 with Robert B. Leighton, William L. Valentine Professor of Physics, emeritus, in the Division of Physics, Mathematics, and Astronomy. Dr. Leighton received his BS in electrical engineering from Caltech in 1941, then switched to physics (MS 1944; PhD 1947). He joined the Caltech faculty in 1949, becoming a full professor in 1959 and Valentine Professor in 1984.

He recalls growing up in Los Angeles during the Depression; his early interest in mechanical things; his undergraduate and graduate years at Caltech; influence of W. V. Houston and W. R. Smythe; work on aircraft rocket launchers. Recollections of Willy Fowler, Charles Wilts, Paul Epstein, Carl Anderson, Fred Hoyle. Discusses his work on the mesotron (muon); cloud chamber experiments on strange particles; writing *Principles of Modern Physics.* Postwar rebuilding of Caltech physics: Robert Bacher; R. A. Millikan's attitude toward theoretical physicists; Robert F. Christy; J. R. Oppenheimer; Richard P. Feynman's first visit to Caltech.

Photographing the sun and planets with Mount Wilson 60-inch telescope; Fritz Zwicky's differential photography method; study of Zeeman and Doppler effects; discoveries of solar oscillations and supergranulation; search for a new solar observatory site; choice of Big Bear. Collaboration with Gerry Neugebauer on infrared sky survey; discovery of "dark brown" stars; work on Mariner Mars missions. Recalls his teaching; editing the Feynman lectures with Matt Sands; Feynman as lecturer; difficulties in editing Feynman's lectures; recollections of Feynman.

Discusses his instrumentation for millimeter and submillimeter astronomy; establishing Caltech Submillimeter Observatory at Mauna Kea; Owens Valley Radio Observatory. Concludes by commenting on his plans for improvements on Mauna Kea.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH ROBERT A. LEIGHTON

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

TABLE OF CONTENTS

INTERVIEW WITH ROBERT A. LEIGHTON

Significant discoveries of solar oscillations and supergranulation (ca 1960-1961): role of grad student Bob Noyes and others; how discovery of five-minute oscillations was made. Study of supergranulation: Project Stratoscope (Martin Schwarzschild) and Leighton research team's similar findings. Comments on nature of search for knowledge and problems of support. Aftermath of solar discoveries: opening up of new lines of research; search for a new solar observatory site (Big Bear). Conducting of site survey; problems of different sites and topography; choice of Big Bear. Role of Lockheed Solar Observatory. Building of Downs Laboratory to accommodate solar research. Arrival of Harold Zirin (1963). Introduction of computers into astronomy.

Session 5

Collaboration with Gerry Neugebauer on infrared telescope: building the reflecting dish and mounting. Conducting infrared sky survey: discovery of "dark brown" stars and other new light sources. First Mariner Project, 1964: assistance of Neugebauer and Bruce Murray in setting up Mars imaging experiments; limitations imposed by small payload. Discovery of Martian craters; determination of density of Martian atmosphere. Further Mariner projects, uncovering more aspects of the Martian terrain.

Session 6

Teaching at Caltech, beginning during World War II era. Mathematical physics, electricity and magnetism (Smythe's course); modern physics and the textbook *Principles of Modern Physics* (1959). The Feynman lectures: origins of the course; role of Matthew Sands; Feynman's resistance; Feynman as lecturer; Feynman's approach to subject matter. Leighton's role as coordinator and editor: difficulties in editing Feynman's lectures. Personal reminiscences of Feynman. Broad impact of the *Lectures* on the physics community. Feynman and Ralph Leighton; drumming; Feynman's "stories" and their publication.

Session 7

Instrumentation for millimeter and submillimeter astronomy: building a new, larger infrared dish and support structure with computer assistance; work leading to Caltech Submillimeter Observatory at Mauna Kea; Owens Valley Radio Observatory [OVRO]; funding and personnel for OVRO project. Funding patterns in physics; Bacher as chairman of physics division.

Conclusion: summary of achievements; current projects

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ORAL HISTORY PROJECT

Interview with Robert B. Leighton **by Heidi Aspaturian**

Pasadena, California

Session 1	October 8, 1986
Session 2	October 22, 1986
Session 3	November 18, 1986
Session 4	January 13, 1987
Session 5	January 20, 1987
Session 6	January 27, 1987
Session 7	February 12, 1987

Begin Tape 1, Side 1

ASPATURIAN: What were the circumstances that led to your early interest in science and who or what influenced you?

LEIGHTON: Defining offhand the areas that interested me as a kid, I think of four things. They come under the headings of electric train, erector set, merry-go-round, and clocks. The electric train is probably my earliest recollection; I must have been two or three years old. I don't know where this incident occurred. My parents moved to Seattle shortly after I was born, and it might have been there. Evidently I was known at a very early age as being interested in what was inside things and had started taking apart an electric train. I remember somebody saying the reason I couldn't get a particular piece back together was that the screw that held it had a left-hand thread and that I was trying to turn the screw as if it were right-handed. I learned at that point what this meant, and I don't recall that that ever bothered me again after that. But whenever I discover that I'm working with a left-hand thread, I remember the electric train.

At five or six, I started using erector sets to build cranes and vehicles and oil wells and things like that. That was a big influence, because later on when I was in junior high school, I was still interested in building things and would make my own pieces of train track layout by using the tin sheet available in the junior high school shop.

The merry-go-round has to do with a story my mother used to tell. When I was about six she took me to the Long Beach Pike—we had moved to North Long Beach very early in my childhood. I usually say I was born in Detroit, but I had the sense to move to California at an early age. [Laughter]

Anyway, my mother took me down to show me the sights, and she took me to the merry-go-round. But I didn't care at all about riding it; instead I got down on my stomach and looked at the gears underneath. That was what was fascinating me.

ASPATURIAN: Do *you* recall that?

LEIGHTON: Oh, yes, I recollect that. [Laughter]

I also used to take apart clocks. And that would lead to the same problem as with the electric train, except more so. Usually when you take a clock apart, you don't intend to get it back together because they aren't made to be put back together. But my mother always expected that I *would* put it back together. And my inability to do that caused her to be wary about my being left alone with a clock until I was about the age of twenty-five.

ASPATURIAN: What were your parents professionally?

LEIGHTON: My father was a toolmaker for an automobile company in Michigan. I was a war-time baby, born in 1919. By the time we moved west, I don't know what he did. Then they separated, and my mother was the breadwinner for some years. Most of my recollections have to do with school and that early time in North Long Beach.

ASPATURIAN: What did your mother do for a living?

LEIGHTON: She had done bookkeeping, but somehow or other did not want to do that again. So during these years she was a maid at a hotel.

ASPATURIAN: She must have encouraged your early interests, though, from what you tell me.

LEIGHTON: Oh, yes. While she herself may not have grasped very much of the significance of science, she did seem to know that Caltech was the place where I ought to be. She mentioned that many times in her life, but she never took any overt steps to put me there. But it happened, not through any planning, but just because things developed the way they did.

Another amusing experience having perhaps some premonitory significance had to do with Baby Ruth and Abba Zabba candy bars. For a nickel, one got to choose any bar in an open box of ten or twenty bars. Sometimes underneath the wrapper, there would be a little red paper ribbon, and if you got one of those, you got to choose another candy bar, for free. I remember going to the North Long Beach theater with some of my peers on a Saturday afternoon—I must have been eight or nine years old—and when we went to get our nickel candy bar before the movie, I propounded to myself a theory about the Baby Ruths. I could see that the bars seemed to be a little variable in size, and I hypothesized that they would put the red ribbons in the smaller bars. People would naturally check and choose as big a bar as they could see. I purposely chose a small bar, and it had a red ribbon in it! So I got another choice, and I chose another small bar. [Laughter] I think I wound up with about three or four bars that had ribbons in them, with a total mass much bigger than any one of the other bars.

ASPATURIAN: So, by the age of eight, you had moved on to theory.

LEIGHTON: Yes, I had made a hypothesis and I made a test. [Laughter] I don't know whether you would call it a mutation, but anyway, there it is. A few years later, I tried the same trick with the Abba Zabbas—the Abba Zabba company was nearby on the same street we then lived on, 12th Street in Los Angeles—and it worked!

ASPATURIAN: How did your friends react to this?

LEIGHTON: I think they were pretty impressed. I handed the bars around, naturally, and they must have thought I was really something.

ASPATURIAN: In school, for example, some of your special interests must have attracted the attention of your teachers and friends as well.

LEIGHTON: There was a Miss Franklin at Starr-King Elementary School. I actually don't remember whether she was the principal or my teacher. In third grade after school one day she sat me down on the steps of the bungalow classroom and gave me some little things to judge which was the heaviest and things like that—a little psychological and manual skills test. As a result of it, I skipped the third grade. I think I still have the little note that she wrote to my mother, saying that she recommended putting me in fourth grade unless my mother objected. But my mother didn't object, so everything was fine. There was, however, an amusing offshoot of that, namely, that in the fourth grade you were supposed to be able to sing "America the Beautiful" and a number of traditional works. The teacher begins by sounding a tone with a little pitch-pipe harmonica to establish "do," and then there were these strange looking "notes" on the sheet of music. I never knew what their exact meaning was. Nobody would ever say, "do" is the second line up from the middle, or the third line down, or anything like that. I always thought that this was something they had covered in the third grade that I'd missed. Only later did I realize they never covered it anywhere. [Laughter] Nevertheless, I've always had an inferiority complex about music.

From that elementary school I went to three different junior high schools. There was Compton Junior High School, and Sentous Junior High School in Los Angeles, which is now buried under the Harbor Freeway, several blocks south of a big interchange, and Berendo Junior High School, west of Vermont Avenue in Los Angeles. I was riding bicycles by this time, so transportation was easy. By the time I got to Berendo Junior High School, I was doing very well in science, algebra, shop and everything else, except music appreciation and Latin. We all took a language, and somehow or other I got talked into Latin when I probably would have done much better taking something more concretely useful, such as French or Spanish. I was worried I was going to get a "C" in the course, so I made a conscious decision—I can almost remember the time of day it was and everything else—that I was going to do whatever it took to learn what was presented. It wasn't that I worked so much harder, but I organized myself. I got a book on what was bothering me, and it often did some good. From that point on, I just kept getting better and better in school. It's kind of interesting that such a decision would hit me so hard. I just decided I was going to do it.

ASPATURIAN: When did the games with the Baby Ruths and the merry-go-round and so forth stop being playtime and start becoming a career path toward science?

LEIGHTON: I can't really say. It isn't clear to me now. I always enjoyed things like freehand mechanical drawing. I liked isometric drawings and perspective views and geometrical things.

One very important influence here was the Los Angeles Public Library. We used to live at Union Avenue and 12th Street in Los Angeles, which was within rollerskating distance of the library. I made a practice there of finding out where things were—the fiction and the philology, whatever that was, and mathematics and science and so forth. All through my high school days, even though I got to be farther away from the library, I knew how to get there. I would take streetcars or ride my bicycle. That was also eye-opening. I read a lot of things, and as I got somewhat older, I started reading astronomy textbooks. The Los Angeles Public Library, God bless them.

Then I had the good fortune to go to a single high school—John H. Francis Polytechnic High School—which was at that time at Washington and Flower Streets. It's now out in the San Fernando Valley; they moved it and turned the old school I think into an office complex for the L. A. Trade Tech College. At the time I was there, it was a great high school. They had courses made for everybody, including the geniuses. Carl Anderson went to that high school—I'll come to that later. It also had a big shop. It had been some kind of a central, technical high school in earlier years, maybe even a private one. When I went there, there was a big steam lab with steam engines and generators in it. Part of our electricity course involved running a 50-kilowatt motor generator set or something similar. It was great for kids of that age. The level of education in electricity that I got there was not surpassed until I got to the junior year at Caltech. They went into what is called complex notation—$e^{i\Omega t}$ 1; inductance and capacitance and phase lags and leads—things like that.

The chemistry was not surpassed until I got through the freshman-year chemistry I took at Los Angeles City College [LACC]. The high school had the same qualitative analysis book that they used at Caltech and at LACC.

The physics, strange to say, was very so-so for me. I didn't think much of it. It wasn't the teacher; it was just that I didn't really understand it all that well. But it might not have appealed to me—you know, strings and pulleys and balls on inclined planes, which I only came to later. The mental gymnastics and exercises that you get along with the strings and pulleys are helpful and even interesting and useful. But they didn't impress me much at the time.

ASPATURIAN: At what point in your education did you become aware that for a person with your interests, Caltech was the place to go?

LEIGHTON: Well, I was becoming dimly aware of things. It was in 1932 or so that I got interested in the sky. I ordered a 30-power telescope for five bucks or so from a place that's still in business, called Brown Scope. With that little telescope, I could see the moons of Jupiter, the Pleiades, and many other things from the backyard. I made a little clamp to hold it on the fence so it wouldn't wiggle. I remember Jupiter rising earlier and earlier in the evening over the apartment house down the block.

ASPATURIAN: Had you also read something that stimulated your interest?

LEIGHTON: I don't think so. I really don't know what did it, but I suddenly got interested. I saw Venus just about to be occulted by the moon, and I didn't know quite enough about the sky to realize that if I would just persevere for an hour or two, it would actually happen. So later on, I learned about the occultation of Venus—the famous one in 1932. So it was very willy-nilly. When I got to high school, I met a student called George Herbig, who became a well-known astronomer at Lick Observatory, where he spent most of his career. We were the same age; we were both at Poly High, and he and I got to be good friends. At the time, he was active in the Los Angeles Amateur Astronomers Association, which was on Eighth Street near Alvarado in the back of an old house. He got me interested in grinding telescopes; he took me to some of their meetings. The interesting thing is that, at that age, he knew Mount Wilson astronomers through, I think, having cultivated them at the meetings when they came to give talks. He had actually spent some evenings in the company of an astronomer, making observations at the 100-inch. He knew at that age exactly what he wanted to be and what he wanted to do; and he followed through right down the line. Our careers have seldom crossed, but we've collided now and then. I try to keep up with him in the literature, just in terms of what he's done. I owe a lot to him for awakening in me the desire to actually build things—that is, instruments of that kind—with my own hands. And that hasn't stopped.

I think the Chemical Society contest brought these interests to a focus. I took chemistry in the eleventh and twelfth grades, and at the time I was in twelfth grade, the Los Angeles chapter of the American Chemical Society sponsored one of their yearly contests. It was a substantial test that required a quantitative knowledge of chemistry. It just happened that a classmate of mine, Charlie Wilts—he's a Caltech professor emeritus now [Charles Wilts died on March 12, 1991. Ed.], of about the same vintage as I am—and I, and a third person who also wound up coming to Caltech, won the contest. Charlie came in first, and a couple of points behind him I came along, and the third guy, I guess, was a couple of points behind me. We were all very close. Charlie actually was a half a year ahead of me in graduating. I always told him that was why he did two points better on the exam, because I hadn't completed the senior course and he had. [Laughter] The prizes for winning the contest were one-year scholarships to Caltech, USC [University of Southern California], or Occidental—first choice to go to the winner, second choice to the first runner-up and so on. The pecking order at the time was Caltech first, USC second, and Oxy third. I showed up on registration day at USC, and it was quite clear to me that it was not the place where I should be. As a result of that, I'd lost out on registering for anything that year, so I went back to high school for a post-graduate year. I took solid geometry and some photography; it wasn't a full schedule, but it was enough to keep me going. But as it happened, later Charlie Wilts and I found each other again, at LACC. He had not gone to Caltech, I guess because of lack of money—I'm sure this is right—and the other fellow had not gone to Caltech either. Probably he went to Oxy, but later he transferred to Caltech. Anyway, we all wound up at Caltech.

I probably would have tried to come to Caltech then if I had known enough about it and gotten the first choice; but I think I too would have fallen by the wayside on the funding. You know what the tuition was? Three hundred dollars a year. The scholarship was for tuition for the first year, but what would I do after that? Furthermore, the scholarships didn't cover living expenses. And it was the Depression time still. That hit our family; we were pretty low. Although the funny thing was, it was never the kind of hopelessness that you see in the developing countries with overpopulation. It was just because the economic machine didn't work. So there was no great anguish about it; things would get better.

The mirror-making for astronomy—that technology transfer—began in the middle of high school. During my post-graduate year there, I took, as I said, solid geometry. Then, during the summer after that, one of my student friends came back from Caltech and described this marvelous mathematics called calculus they used for solving geometrical or kinematical problems. I got interested in this calculus stuff. So I spent some of my time at the Los Angeles

Public Library, looking in math books. One time I took out a thick, heavy, brown, slick-page book called *A Treatise on the Differential and Integral Calculus*, by no doubt the best mathematician in the world. I couldn't get past page one. I tried other books, and they didn't work either. Finally, I stopped in a used book shop on Fifth Street or Sixth Street, just below the library, and I came across a book called *Practical Calculus for Home Study*. I went through that in the summertime—through differential calculus and partway into integral calculus. So when I went to LACC, I just went through math like that. It all worked; it was really fine. So that was one of my better experiences.

I also took a course in photography, which helped a lot later on; it took away my fear of it. But as a matter of fact, I sort of flunked a job the photography teacher had found for me at the end of that year. It was at an advertising photography lab in Hollywood that made color magazine fronts, pictures, and ads. I was supposed to learn how to develop color film. The film came in what they called color-separation negatives, which means the blue, yellow, and red are separate. You make black and white images of those colors, tone each of them with dye of a complementary color, and then superimpose them—what they call a dye transfer process—so you wind up with a color picture from the three primary colors that you use. The blue negatives were very thin, because the light had to go through them, not only to make the blue picture but to make the other two pictures as well. I learned the process well enough, but my penchant for efficiency got the better of me, because it seems that they had some exposed but undeveloped film that had come back from a trip to San Francisco with pictures of a certain elegant steamship passing under the Golden Gate Bridge. And this expedition had cost them some hundreds of dollars. In the darkroom I mistook all of the blue-light negatives for little paper separators, which I threw out in the dark as I was developing the negatives. So most of the blue negatives went out to the incinerator. I got my walking papers for that.

Now, there was a real dichotomy, career-wise: If I'd been more successful at that point, I might have become a famous Hollywood figure. But as it was, I *had* to go to Caltech. [Laughter]

I think I should also mention that during my postgraduate year at Poly High, in connection with the photography class, I was the photographer in a group from the school paper that went over to Caltech to interview Carl Anderson. Carl had just gotten the Nobel Prize—this was 1936 or so—for work he'd done in '34. We went to Pasadena and interviewed the famous

Poly High alumnus, Carl Anderson. We saw his lab. He also showed us the optical lab in which the 200-inch mirror was being ground for the Hale Telescope at Palomar. One thing I didn't expect was that one day I would be tromping around in that same optical lab, under conditions that were so dirty and dusty that you couldn't keep a mirror clean, let alone polish one. At the time they were grinding the Hale mirror, they made a big point of the fact that the air was filtered and controlled so that it wouldn't carry specks of dust that would make scratches on this big mirror. Anyway, I used the lab to machine a 400-inch infrared mirror forty years later.

ASPATURIAN: What were your impressions of Anderson the first time you met him?

LEIGHTON: Very friendly, helpful. He didn't show his marvelous sense of humor; I guess it wasn't the occasion. I've heard him tell stories—you have some of these in the Archives, I'm sure—of how things were and the business of going to Pike's Peak. I heard a lot of those firsthand; and other ones that he either forgot or didn't tell at the time.

ASPATURIAN: Did you understand the significance of his discovery and his Nobel Prize?

LEIGHTON: No, nobody in the group did. I was only the photographer, so I spent my time flashing pictures as best I could. But the reporter from the paper, Dick Feinstein I think it was, asked Carl Anderson, "Well, Dr. Anderson, what good are these cosmetic rays you're studying?" [Laughter] Dr. Anderson laughed. I think all of us caught on except the guy who asked the question.

ASPATURIAN: Did you have a feeling of awe at meeting a Nobel laureate?

LEIGHTON: No. I didn't know what it was. I had never heard of the Nobel Prize.

At L.A. City College, I was in an engineering option, and I found myself taking calculus and physics. In physics it was actually the same textbook as they used at Caltech, and the calculus was similar. I had PhD's for instructors in math and physics and in chemistry, but in chemistry I had already had a PhD instructor at Polytechnic High School. I had a hard-boiled guy named MacIntyre for surveying. I don't know if he was a PhD or not. He was a real tough character. Outdoor surveyors tend to emphasize how tough they are. He made a point of beating into us that we should always be asking ourselves, "What are you doing that's stupid, what are you doing that's wrong? Stop and think. You're probably making a mistake." I remember a time when I was outside the classroom in the testing field, where they had some posts stuck up from the ground on which you'd check the actual length of a "hundred-foot" tape. You would suspend the tape from one post to the other, fasten one end, and pull on the other end with a known tension. That makes it take on the standard shape that you want, providing the line is level. I was pulling the string balance and I was watching the zero, and MacIntyre said, "Okay, what's the correction, what's the correction?" I said it was -.023 feet. He said—beating me on the head with his fist—"Stop and think, stop and think. What's the correction, tell me the correction." And I looked at it again and said, "It's -.023 feet." He said, "*Minus*?" then looked at it again himself. "My God, you're right." He took off his hat and bowed. [Laughter] So I had my day. Nevertheless, old MacIntyre had a lasting effect on my life. Whenever I'd be measuring or machining something, he'd still be in there fighting. "Stop and think. You're probably making a mistake"—and on many occasions it was true!

ASPATURIAN: Did you have a professional ambition at that time?

LEIGHTON: It was engineering. I don't think I had selected electrical engineering at the time; maybe it was even civil engineering. I didn't know what I was doing; I didn't know where I was going or anything like that. I knew that I liked what I was doing, that there was more to be learned, and I was entirely happy. I learned more calculus in the calculus class, and I began to learn how marvelous physics was in the physics class. An interesting thing, looking back on it now, is that I wasn't asked by my mother and stepfather to go out and get a job or earn some money somehow. Even though it was not then exactly the depth of the Depression, the times were still pretty tough, but my mother always encouraged me to go to school fulltime.

Up to that time, I hadn't known what physics was, and I didn't know exactly what it was even after I started taking it in college. But by the time I got into the second year, in the third and fourth semesters of physics, I began to realize what it was, and I was all the more sure that if you could make a living at it, I was going to do it.

ASPATURIAN: What was it particularly that excited you about it?

LEIGHTON: Well, we were starting to learn more optics, electricity, heat—things like that. Then there was the fact that in my first semester at LACC and afterwards, I was making all A's. It wasn't getting all the A's on the report card that got me; it was the fact that I realized I could do it. And so studying interested me—that sort of reinforcement feeds on itself.

By the end of my first year at LACC Charlie Wilts had gone to Caltech, and in my second year, the physics instructor at LACC, Dr. Ralph Winger, was instrumental in my taking the transfer exam to do the same. So I came over to Caltech and took the transfer exam for entry into the junior class, and as a result of that, I got a scholarship. I don't remember if there was living-expense money in it or not, or whether there was the possibility of borrowing money; but I kept on getting scholarships. In that case, I don't know why it was, unless it happened later, that I objected so much in principle to the fact they raised the tuition from $300 to $360 a year while I was in the middle of college. I think it's $10,000 now. I don't know whether it's still a bargain or not.

So I came to Caltech in my junior year, which was just at the start of World War II, the fall of '39. I realized that I was "home" intellectually, and there was just never any further doubt. And I decided I would take all the courses I could at whatever advanced level I could handle. Slightly to the anguish of the hydraulics instructor, I omitted hydraulics in favor of quantum mechanics in my senior year. I wish I'd taken it, actually, because later on I worried about how you do things related to hydraulics; but I worked them out all right. Anyway, he said, "I guess you know what you're doing," and he signed the petition, and I learned some quantum mechanics instead.

ASPATURIAN: Had you had any quantum physics before you got to Caltech?

LEIGHTON: Well, when I was at LACC, I was reading Weyl's *Space, Time and Matter*, and Sir James Jeans's books, and there were some popular books by Eddington and others, and I got to know about the Lorentz transformation and things like that. That was very influential, too, because it was what I liked, and I would just eat up all those things. I was teaching myself calculus. I would also take home astronomy books and make little outlined notes of the contents of each one. They were never really very different, but I wanted to be sure I understood everything. So I also gained a lot of astronomy sort of on the side.

I think my real soulmate—if you talk about pure influence—my favorite instructor by far was W. V. [William Vermillion] Houston. He later went on, poor guy, to be the president of Rice University. I have often thought of him and wondered what the world would be like if he had not moved away.

Begin Tape 1, Side 2

ASPATURIAN: Why do you say "poor fellow," to be the president of Rice?

LEIGHTON: It couldn't have been easy for someone of his interests to do that. I don't know why he did it. But I lost track of him then, in the sense that he was no longer so directly an influence on me. I'll come back to him later because he was my research advisor when I did my graduate work, too. But at the time I'm talking about, he was my professor in junior-year mathematical physics; and then in my senior year in quantum mechanics. I also remember taking courses from [William H.] Pickering and Fred [Frederick C.] Lindvall. I took a course from Carl Anderson just for the first term in the fall of 1941-42. I guess we got started, and then Pearl Harbor came. I had a course in electromagnetism from [William R.] Smythe when I was a senior. Traditionally, one hated the course, but my attitude of slogging through and learning it paid off, because I got a C, B, A as I went through. It was very worthwhile, although some of my compatriots on the staff here who also took Smythe's course might differ with that.

ASPATURIAN: Was Smythe well known in his day?

LEIGHTON: Oh, yes. Millikan brought him to Caltech and set him to putting together and teaching the course; it would be the go/no-go gauge for graduate students in physics—namely, you had to pass Smythe's course to go on in graduate physics.

ASPATURIAN: Had he been trained in Europe, where the real quantum mechanics ferment was?

LEIGHTON: Smythe was not a quantum mechanics type. He was a classical electricity man. He did experimental work. He and his students made an ion separator—a column separator for isotopes of uranium—just in the lab here, long after it had already been done, but more for studying the action of the separator than to get an output product.

The peer influences during my undergraduate years are sort of interesting in that I lived at home. I also had a job on campus because I was virtually the sole support for my mother and myself. And this was with the help of loans; a few hundred dollars did it, of course, in those days, because things were cheap. Since I lived at home, I had no distractions such as you have in the student houses. They were terribly distracting influences for somebody who wants to try to have it quiet. And I guess I was that type. Nonetheless, I had several good friends in my classes. I'd see them every day; we'd eat lunch together. Charlie Wilts was one of those. Few of the others left much of a lasting impression, other than that they are all Caltech alumni. So that, more or less, covers the undergraduate years at Caltech.

ASPATURIAN: What was it like from a social standpoint, being a student here?

LEIGHTON: I think I have to admit to being somewhat asocial. It wasn't a conscious matter of "the less contact, the better," or anything like that, but I didn't purposely seek out such contacts. I like people when I talk with them and if there's a particular occasion. But to this day, I tend to avoid things like that. I guess I spent a lot of time feeling guilty about my parents' situation and so forth. But it wore off.

When World War II came, I had a job over near MacArthur Park, which was then called Westlake Park, in a doctor's lab. He was an oncologist; and he was building, with the help of a Caltech graduate student, Charlie Robinson, an electrostatic generator to be a carbon copy of the small generator at Kellogg Laboratory at Caltech. They used the small generator at Kellogg for irradiating various tumors, and this doctor wanted one of his own in his lab. I learned more about machining and about lots of other technical things there. And then when Pearl Harbor came, Charlie Robinson was very vulnerable because he was just at the age group that they would want to take right away. So he quickly stopped that job and went to work on the Caltech rocket project. I stuck it out until the end of the year. At the time, I believe I was taking my first year of graduate courses in the mornings and commuting from Pasadena to the job in the afternoons. There were no lab sections or anything like that, so it was easy to do at the time. I was in charge of this electrostatic generator. I didn't know a Van de Graaff generator from a hole in the ground, but I was learning. I had done enough things that I knew pretty well what to do next, but I didn't know how to make electron optics; I didn't have any idea of the analytical approach to that. I had a qualitative idea of what you were trying to do, but how one would design an electron lens to focus properly, I just hadn't quite got there yet. Then, a year after Charlie Robinson left, I left. I guess the doctor got somebody from USC to help finish the thing. And I think it finally worked to some extent.

ASPATURIAN: I'd like to backtrack for a minute. You said they were using this generator for radiation therapy on tumors?

LEIGHTON: I'm not sure, but I think they used a gold target. My offhand impression was that they were using X rays, first from the X-ray tube in the big transformer. There wasn't the need to prove safety and stuff like they have now; and I don't know how many people it killed or how many people it saved. But presumably they learned something.

ASPATURIAN: Were you at all intrigued by the medical implications of what you were doing? Or for you, was it purely the technical questions?

LEIGHTON: No, I was becoming aware of the disease, and I was happy enough to be working on something like that. But I did not choose it because of that or because of any interest in the medical part. It would have been the machinery, the technology.

After I left that job, I got involved with designing and testing rocket launchers during the war, and in particular, aircraft-type launchers, things that were carried under the wings. You've seen pictures of aircraft with a lot of rockets underneath the wings. This was the Navy rocket—Defense Department—project with the objective of building usable ordnance. They did the experiments; they did the design; they did the pilot production. They got the first batches of rounds out. And they would do things up in Eaton Canyon, and now and then the powder presses that extruded the powder drain would explode.

In Kellogg, they were doing machining, God help us, in a little enclosed shop area. And they had sacks of open cordite or ballastite right in the room where they were making cuttings. This stuff does not explode when it's sparked, it starts to burn. Once it starts to burn, it burns faster; and once it starts to build up pressure, it burns even faster and that's what makes a rocket work. It's a self-limiting reaction, in that as long as you give it area to burn in, it burns. But it does not progress down through the rocket unless you confine it so much that the pressure goes very, very high, and you then get a detonation, a shockwave. So it's mostly a relatively slow-burning thing. Nonetheless, it got away from them. As soon as the fine powder started to go, then everything started to go. It was not an explosion as much as it was just an awful lot of hot gas that was given out. And people were caught in hallways. There were two people, I think, who died as a result of that. I remember seeing the smoke come out of Kellogg; I was carrying driveshafts for the Van de Graaff generator from the doctor's place over to a shop in Bridge Lab, because Bridge was where they had a lathe big enough to do whatever had to be done for the Van de Graaff. We were very friendly back and forth about the use of Institute shop facilities for private enterprise. I guess we still would do that if we had a chance.

ASPATURIAN: Can you tell me a little more about the naval rocket project and the effect it had on people at Caltech? Was a lot of the Institute caught up in it?

LEIGHTON: Oh, yes. There were several types of projects, all related to the war effort. There was the Navy rocket project, the jet propulsion project, and meteorology projects. Each project had a principal investigator —PI; that's where I learned the term—to be the responsible major domo and administrator. The PI for the rocket project was E. [Earnest] C. Watson. I didn't appreciate that man very much when I was a student, perhaps because he wasn't a front-rank physicist. But he took such a load off the "real" scientists by doing the administration, that he helped make the place work and was well deserving of credit and thanks.

The Navy project had the objective of making rocket-propelled ordnance, not rocket-propelled aircraft. When I was getting aboard, Charlie Wilts was just perfecting some big aluminum rails that would be carried under the wing of a PBY amphibian bomber. They were to be used on aircraft patrols for enemy submarines. These antisubmarine rockets would be mounted backwards, under the wings, so that when you fired them, they went backwards. The idea was to calibrate them with your flying speed so that when you pushed the button and they fired, they would wind up standing still right over a certain spot where your magnetic detector had said, "Here's the place," and they'd go straight down. It eliminated the problem of how to drop them so that they would hit the right place. That was very successful—what they called a retro-launcher.

C. C. Lauritsen and Tommy Lauritsen were in on the project's ground floor, although Tom, I guess, was a little late in getting aboard because he was in the East helping establish what they called the proximity fuse effort. He came along a little bit later; I didn't know him very well until after the war. Willy Fowler was the executive expert on all these things. Carl Anderson was in charge of the aircraft rocket launchers. On that project were also Fred Lindvall, Ralph Smythe, Leverett Davis, Charlie Wilts, and a student, Fred Thiele, who was one of my classmates. He graduated in the same year I did, in the same option, and went into private work afterward. Now he's the richest of all of us; he sold his business under very favorable terms just recently. At any rate, it was a time of learning. There weren't courses given in the things we were trying to do, but it was a time when you did what you could and hopefully stayed out of the draft.

Well, I was sent to Cherry Point, South Carolina, for a couple of weeks, to oversee the installation of some 12-inch air-to-ground missiles. And I'm afraid the launchers that we built for them were not of the very best design. Again, it's a thing where if you'd had some years of experience, you would have realized what would be the right thing to do. It turned out that they did not use our launchers; they used a different mode of launching, which had greater error but involved less machinery. That was probably all for the good. They sent a squadron of marine planes over to Europe to knock out the rocket launcher sites that the Germans were putting in across the English Channel from Britain. So things were being done that did relate to actual battle conditions.

ROBERT B. LEIGHTON

SESSION 2

October 22, 1986

Begin Tape 2, Side 1

LEIGHTON: Maybe it's a good idea to start with the end of the war, because life rather changed at that point. Most of the people with whom I was associated, both at that time and later, had been working on the Navy-sponsored rocket project. That would explain why I was unable to complete one year of graduate work. After the war, I had a couple of years left to go, and I got my degree in 1947. For my thesis, I picked up a problem that I had started on just at the beginning of the war under the sponsorship of W. V. Houston, who was a theoretical physicist. Shortly after the war, he went off to be president of Rice University. And that was responsible for one of the two non-Caltech job offers I got when I was looking. When I had my degree and was considering my career, I had the offer from Houston at Rice. I also had an offer from what was then probably Ramo Wooldridge—the company that became TRW, Thompson, Ramo, Wooldridge. Those two offers were probably not quite a factor of two different in salaries. What I accepted at Caltech was about what the Rice offer was. I seriously considered Rice, but I was a bit apprehensive about the climate. Having grown up in Southern California, I was a little hard to please. So I went to the public library and checked out a book on Texas. It had been written as one of the make-work WPA projects that many people participated in when they weren't in a job, early in the Depression. There was one section about the weather that started out saying—in paraphrase—"Many people complain that the weather in Texas is unpredictable. This is not true. The only thing unpredictable about the weather in Texas is, when are you going to have a Norther? And a Norther may appear at any time." [Laughter] The other question I asked of somebody was how many of the buildings at Rice were air-conditioned. I hate humid weather. It turned out that the only air-conditioned building or lab was for the Van de Graaff generator, which couldn't operate except in dry air.

My thesis problem was on lattice vibrations in crystals, which happened to be close to the subject matter of the course that Houston taught to juniors. One of the features of the course was modes of vibration of connected systems. Of course, when Houston moved off to Rice, I had to find another thesis advisor. So I dropped into the office of Paul Epstein, who was the Institute guru on theoretical physics at the time. He had done work on the Stark effect in the early days of quantum mechanics and knew all the classical subjects—electricity, magnetism, and thermodynamics. He had written a textbook and all that kind of thing. Epstein—"Eppy" —was somewhat of a character. He was described by somebody as being a person who spoke every language with a foreign accent. He was born in Russia; he was raised in Germany; he came to America. So there was a certain impediment to the communications channel. As an undergraduate, I took a couple of courses from Epstein, not realizing that he would become my research advisor. One of these courses was on electricity and magnetism; and I happened to know something about the subject. I'd taken Smythe's course; but you took Eppy's course for culture, not just to learn the subject.

ASPATURIAN: Culture?

LEIGHTON: Yes, scientific culture, because he would go back to the origins of things. He wasn't just writing down all the equations. He would take a whole lecture to describe something or to do something that everybody knew, but they had probably never known where it came from or had forgotten it. I remember three things in connection with my graduate work and my relations with him. Early in one lecture in his electricity and magnetism class, he did some algebra and came out with the wrong sign, plus instead of minus. I happened to notice it, scribbling down the notes, so I said, "Shouldn't that be minus?" He was hard of seeing, and he had little squinty glasses, and he squinted, so he couldn't really tell who was talking to him. He almost never looked around at the audience, but he turned around, startled, made a sort of generalized glance into the direction of the audience and sputtered a little bit. He said, "No, if you have no better thing to do than to comment in an ignorant manner, you should not come to this class." I shrank down in my shirt collar and dutifully took my notes. About fifteen minutes later, he followed the consequences of all this and he looked at the result and said, "Ja, something is here fishy!" Walked back and looked, walked back and forth. There was a steel plate over a trough in the floor where they ran wires when they used that room as a laboratory. Every once in a while he'd hit this steel cover and it would clunk. People closed their ears every time he was about to step on this steel plate. After a few clunks back and forth it finally sank into him what I had suggested. He said, "Uh, well, thank you."

ASPATURIAN: He sounds like quite a character.

LEIGHTON: People often learned how to imitate him. Another episode was in his thermodynamics class, which I took in 1940. I didn't register for that, but I knew he would be on my orals committee and he would ask me about thermodynamics. So one *always* read his book and attended some of his lectures on thermodynamics. It all worked out okay at the oral. I was his student by that time, and I guess he felt he couldn't really flunk me, so he didn't ask me anything hard. But there was one subject in thermodynamics that had been worked out in about 1925. Ever since Eppy had come to Caltech, which was also about 1925, he had included this subject in his course. So the years went by and every year he would finish up the topic, saying, "And recently it has been shown by so-and-so that such-and-such," referring to the 1925 solution. About the time I was sitting in these lectures—I forget whether it actually happened right then or not—he was talking, and he went back and forth. He got to the point where he said, "And recently, it has been shown—," and he stopped, got a faraway look in his eyes, and said, "Ja, it is some time, now." Fifteen years had passed, and all of a sudden he realized that he'd been saying that for the last fifteen years. [Laughter]

The only other memorable incident was when I handed in my thesis. I gave it to Eppy, of course. The protocol was that you handed your advisor your thesis in preparation for the oral exam. He was supposed to have it for a few days, and then you were supposed to pass it around to the other examiners. You were supposed to hand him a card at the same time, and when you went to pick up your thesis, he was supposed to hand you the card and have a couple of topics scrawled on it to give you a hint about what he might ask you on the oral exam. That's so people wouldn't be embarrassed by getting way off base. Well, he didn't give me a card, and I was a little worried about that. When he handed back the thesis, he said, "This looks like a very nice thesis. I would like to read it— [long silence] sometime." [Laughter] So that gives you an idea where you stand in the world.

ASPATURIAN: Did you kind of keep up connections with him after your academic association was over?

LEIGHTON: He was around, and I eventually got on the staff. Yes, he was friendly and all that. He was just that way.

I guess the thing I remember best about my graduate work was the 8:00 a.m., rain-or-shine course that Willy Fowler taught on nuclear physics. I had not had any nuclear physics really to speak of. He would get there at seven o'clock and fill the whole blackboard with equations, and his lecture was on the board when you came in at eight. The smart guys came in before eight o'clock and copied down the blackboard, so they could just listen. But seriously, the material in his course was up-to-date. It was just great. As a matter of fact, ten years later I was writing a textbook, and I used my notes copiously in the nuclear physics part. It was guys like Willy who made the place really jump.

I want to make one observation about my thesis, which was on lattice vibrations. It turned out that there was a certain integral I had to find. I had to evaluate some property of the vibrations of atoms for a very simple case of a cubic crystal, meaning it's symmetrical in all directions. The equations for the motions of the atoms—which you then have to analyze—boil down to a set of three equations in three unknowns, with x, y, and z as the variables. You have to find certain places in space where the frequency of the vibration is the same for the waves going these various directions in the crystals. Mathematically, it was a very tough problem, and the earliest work on this subject was done in 1900-1905, by Einstein, Born, and von Kármán. But it was still a significant problem in that there were certain experimental measurements of specific heats at very low temperatures, which seemed to have some anomalies in them. People didn't know whether these were the result of the approximations that were being made or due to coarseness in the sampling of the frequencies. Either way, they couldn't really get good curves or whatnot. I observed that, because of this cubic symmetry, there was only a very limited number of directions for the waves to propagate in a crystal that would give independent results. There'd be some direction over here, and it would be the same as the reflection on the plane, like a reflection in a mirror; because of the symmetry of the crystal, it boils down to a small fraction—like 1/64 or so—of the total directions surrounding a point. So solving the problem in that limited cone gave you the whole solution. On these symmetry boundaries, it turned out that the equation, rather than being a cubic equation, which is hard to solve, was a quadratic equation, which is very easy to solve. It also was true that because of the symmetry, you know how the constant-frequency surfaces behave as they intersect these symmetry planes, namely, the surfaces had to come in *perpendicularly*, because otherwise you'd have a cusp, and there was no physical reason for a cusp.

So I figured out that a good way to improve the accuracy of the measurements would be to use the data on these symmetry planes—and the boundary conditions of meeting these planes at right angles—to just "wing it" and guess how the surface would go in between, in the body. It was hard to find there, but you knew pretty well what it had to do on the basis of what was happening on the boundaries. So I went to the machine shop and got some plate brass and made a model of these three planes. It was arranged so that one of the faces was loose, so as not to completely enclose this whole thing. I used this model as a mold. I waxed the mold, mixed up some plaster of Paris, and cast the plaster of Paris in it, so that I got a block in the shape of the total space occupied by the problem that I was solving.

ASPATURIAN: You translated this into a geometry problem?

LEIGHTON: I translated it into a geometry problem. I had drawn up the shapes of these surfaces for various values of some parameters that there were, like about ten or fifteen different cases. I wrapped these graphs around this plaster of Paris and pricked through with pins on the contours, so I would get the frequency for one, two, three, four, and five units. I would have these shapes, which on the contours, matched exactly what they should be to the accuracy with which I could do it. So I had a whole bunch of layers; but the trouble was, what could I do about the region inside the plaster? I took the model to the band saw in the shop and sawed out every other one of these layers, got out my pocket knife, and carved the surfaces. I sculpted surfaces, which met the boundaries at right angles, did the right things inside and made a pretty good-looking approximation to what the whole thing was like. Then I boiled the pieces in wax so that they wouldn't stick to plaster any more. I carefully put the pieces back together in the mold, filled in the gaps with new plaster of Paris, and took the whole thing apart. I now had molds of each and every one of these things, each surface being counted twice: once when carved from one side, and once when carved from the other side. So if I had made some systematic errors, say by making them too fat or too thin—sticking out too far—I would find that, say, if they stuck out too far, the one in between would be too thin compared to the others. So I effectively had an error-correcting scheme in the process! All I had to do was measure the volume occupied by these blocks to find out how many modes there were in between two frequencies. So I dangled each piece, in turn, under water and weighed it while submerged, and then in free air, but still wet, using Archimedes's principle, and it came out! "Eureka, I have found it." I actually had, then, the volumes of these various things, plotted their curves; and that paper got published in *Reviews of Modern Physics* on the eightieth anniversary of Millikan's birth. I got more requests for reprints from that than any other paper I've written.

But what I learned from that experience was that I was not a theoretical physicist. The way to solve that problem is with computers. But the way I solved the theoretical problem was to go into the shop and build something concrete. I think my solution was good to about two or three percent.

I had a lot of fun doing that. I have thought about the problem several times from then on. Every time I run across some new wrinkle that would have helped if I had known it when I was doing this problem, I think, "Oh well, someday I've got to try that out on my approach and see if I can improve it some." So that was my thesis. I also passed the shop course.

My scientific life since then has been divided into a number of reincarnations. I came here first to be a theoretical physicist, and the next time you see me, I'm plotting the decay of mesotrons from the atmosphere, and then a little bit later, you find me doing something else, and then something else, and something else. I don't know whether you'd want to call it a problem, but I guess one of my properties is that I like to think for myself and I dislike organizing things and having a lot of people work for me. You'll find when we get to the period when I was chairman of the [Physics, Mathematics, and Astronomy] Division, I'm going to cancel out practically the whole thing. I wasn't very active; I was very passive as a division chairman. On the other hand, I've had a heck of a lot of fun doing some things.

Anyway, the next time you hear of me, I'm doing cosmic rays. The reason for that is I worked with Carl Anderson during the war on rocket launchers. He went to my high school, and we had a lot of things in common. After the war, he was looking for somebody to help with getting going again in cosmic rays. About the time I was handing in my thesis, he would keep asking me, "Hey, Bob, how are you doing? What are your plans?" I kept saying, "Gee, I don't know, Carl." I finally figured out what I was supposed to answer. So the next time he asked me, "What are your plans?" I said, "Golly, Carl, I don't really have any plans right now. What plans *should* I have?" He indicated that he thought it might be good if I got interested in cosmic rays.

I said, "But I don't know anything about cosmic rays." He said, "Well, you know, none of us does, really, but you have to start somewhere."

So I got interested in cosmic rays. I think the first thing that I was involved with were questions concerning the decay of what was then called the mu-mesotron—everybody called it the muon or mu-meson, except people at Caltech; over here it was the mesotron. That was because when Anderson and Seth Neddermeyer discovered it in 1936 or so, Millikan was away, and they cabled an announcement to him that they were going to publish. They had called it a meso*ton*, because it sounds like pro*ton*; they thought "ton" was better than "tron," because "tron" sounded like a machine. But Millikan cabled back one word; namely, he cabled the "r" back to them, and they put in mesotron. [Laughter] At any rate, it's now called a muon, even at Caltech.

What was not known back then was how it decayed. It was pretty much known that a muon decayed into an electron, but it wasn't known what else it decayed into. It was thought to be like beta decay, and therefore you would expect a neutrino. But what came out funny was that the electrons that you saw coming from the muons didn't all have the same energy. If the cosmic ray stops in matter and decays to an electron and neutrino, the electron should have always the same energy, because it's kicking against the neutrino, and the two together have to add up to the mass of the muon.

Well, Carl used to say that if you figure out how to measure any given quantity ten times more accurately, you're sure to find something interesting.

ASPATURIAN: Was he right?

LEIGHTON: Sure. In this case, he had an idea, which was to put Geiger counters along the track, and actually *inside* a cloud chamber, and make it part of a coincidence circuit. This would select those events in which a charged particle came through some counters above the chamber and the counter inside the chamber, but did *not* get into counters that were spread out *below* the chamber—in other words, if it went straight through the chamber and out the bottom—it would cancel itself out and wouldn't trigger the chamber. But when a muon coming through hit one of the counters up above, and missed all the other counters that were around, it would trigger the chamber. The Geiger counters were shielded with enough material so that they wouldn't trigger on the electron that was emitted during the muon decay and thereby cancel it. Anyway, we triggered on all the genuine muon decays, and we got about a hundred and thirty-four muons that stopped in the wall of the chamber or passed through this counter, but didn't get into the other counters. On the basis of those events, we got a very nice spectrum that looked like what you would expect if there were two neutrinos rather than one. At that time, the nature of the muon was a very hot problem; it was being worked on by people at MIT and I don't know where else. Every month there'd be two or three muons measured. For a while it was thought that there were two energies or three energies because of where they were clustered. Anyway our results settled that. The hundred and some cases had a nice cutoff at the top end of the spectrum. If you want to talk about significant research, that was really one of the first things I was involved in. But I was more or less following my nose and having fun in a somewhat serious way.

That experiment led almost directly into what we now call the strange particles—they were called hooks and forks at that time. The people who first described them—[George] Rochester, Clifford Butler and Blackett at Manchester—detected them in a typical cloud chamber experiment. They hooked up their counters, and they got a charged fork and a neutral one. They published their observation, and it turned out to be true and all that, and everybody in the world jumped on the bandwagon. But after that, they spent months trying to adjust the coincidence circuits of their Geiger counters in such a way as to enrich the residual stuff that they photographed, in an effort to find some more of these particles. But it was a long time before they found any more.

Again, it was Carl who realized—from all his experience with protons, positrons and other things like that—that you had to go to high altitudes to get very many of these hooks and forks. We would use rather loose triggering on our counters; then just look at the pictures that resulted and throw away all the ones that didn't have these things in them. I designed and built a cloud chamber that had a magnet with it to let us measure momentum. It's possible, actually, that I had already built it for the muon decay spectrum experiment, because that had the same idea. So either I built or we already had this cloud chamber. It was very efficient in terms of the magnetic field strength that you got for a modest amount of power. We took it up to White Mountain—which at the time required a five-hour drive up a very poor dirt road to an altitude of about 10,000 feet—and parked the whole thing in a little meadow. I didn't get there to use it, but the graduate students had a great time. We had a radio communication link and so forth. They operated the cloud chamber and collected about two dozen of these hooks and forks. Actually, I think that White Mountain was where we found the neutrino decays, and that we went to Mount Wilson for the muon decays, because White Mountain was so tough to get to. To get to Mount Wilson, we took that same cloud chamber, put it in a smaller trailer and just drove up. We automated the experiment so that it would run overnight without any attendants, and people would go up once a day or every two or three days. We even hooked up a light on Mount Wilson, which stayed on if the generator was running, which would tell us that the magnet was operating. So when we'd be driving home from Caltech, I could look up at Mount Wilson and see whether everything was okay. If I didn't see my light on, I knew I had to go visit up there and turn the generator back on.

Eventually we got dozens of cases and were able to say some things about the masses. The strange particle publications from Caltech were not quite as notorious as those from Berkeley with regard to the number of authors—Berkeley's had fifty-two authors—but they got to have a dozen authors, lots of postdocs and students.

I forget exactly how late into the fifties I participated in the strange particle business, but I had graduate students and a research group. But I withdrew more and more from that as the years went by. The cosmic ray research was starting to get some tough competition from particle accelerators. The nuclear people at Berkeley had started making pi mesons, now known as pions; then the next thing you know, they're designing a machine that will make the tau meson, as it was then called, and some of the other strange particles. A lot of people realized then that the machines were getting bigger and more powerful, and that pretty soon they would also be used to study the V particles.

It's true that there are total energies in cosmic rays that are fantastically higher than anything that you can make in a machine today, even the superconducting supercollider, assuming that's built. The trouble is that they're random and difficult to measure. And the higher the energy you want to study, the less intensity there is.

So the cosmic ray work had come to a crossroads—a point of diminishing returns, as far as our own local approach was concerned. A lot of people jumped ship from the cosmic ray boat and went over to the machines, including many of Carl Anderson's graduate students. One was George Trilling; another was Ronald Rau, who went to Brookhaven. But I just wasn't built that way. I had to have my own motivation.

ASPATURIAN: So your V-particle research was preempted by the accelerators?

LEIGHTON: Yes. I think we died gracefully. Bud [Eugene] Cowan kept on for some years after that, and completed one more large step in terms of identification and characterization of the primary particles that produced the heavy showers of penetrating particles that you found in cloud chambers. He also had in his cloud chambers the capability of counting ionization levels of tracks, which permitted him to estimate the charges of the incoming particles pretty accurately. He had some tracks that looked like fractionally charged particles, like quarks. But even a triggered cloud chamber does have random particles going through it. If they come along after the expansion volume is made sensitive, they appear finer and with less ionization. If they come along just a little bit before, they're fatter and with less droplets because the ion column diffuses away more before the droplets form. If unconfined quarks had really existed, Bud Cowan would have gotten them, no question about it.

ASPATURIAN: In connection with your cosmic ray work, I have a note here to ask you about Seth Neddermeyer.

LEIGHTON: He's really a very clever guy. The last time I talked to him he was busy with extrasensory perception. [Laughter] I haven't seen Seth for years, but every time I was in Seattle, I used to stop and see him.

ASPATURIAN: One of the things I ran across in your file was a press release saying that at a conference in the early fifties, you'd presented a paper discussing experimental evidence for the internal structure of nucleons—that is, that protons and neutrons had some sort of substructure. This was at least a decade before the quark theory.

LEIGHTON: No, I didn't do anything like that. At that conference there were several papers with several authors. I think in one or more of the papers it was conjectured that strange particles, or at least some of them, might correspond to excited states of the nucleon. But it was not a theory; it was just a guess. And anyway, everything is an excited state of something else, these days. But at the time that idea was just to get our foot in the door, to say, "Well, we can think of things, too." Nobody had any real idea until Feynman came along with his interpretation, which had about the same amount of weight—namely, he guessed. Well, you have to call it insight at some point. Feynman had the insight to see that the excited states of the nucleon, if they involved high angular momentum, would be long-lived, because you can only change the angular momentum by a unit at a time, and so the particle would take a long time to decay and have a hard time getting very far.

Perhaps the last contribution in this field that I was involved with was in the study of the so-called Xi particle, which undergoes a two-step decay—first into a neutral Λ° particle and a meson, then into a proton and another meson. On the basis of four observed cases, in all of which the Ξ-particle sign of charge could be determined, the sign was *negative*.

ROBERT B. LEIGHTON

SESSION 3

November 18, 1986

Begin Tape 3, Side 1

ASPATURIAN: In the last interview you were talking about Willy Fowler's class in nuclear physics. I was going to ask you to describe that in more detail.

LEIGHTON: I'm afraid my memory isn't very good at details. I don't even remember whether I registered for it or—since he was giving the lectures and I was supposed to know some of this stuff—I just dropped in. It was more in retrospect that I appreciated what he covered, because there were a lot of things that I just had no idea about. And he was organized. He really transmitted the state of the art, the state of the science as it was at that time.

ASPATURIAN: Was he one of the few people then who was working in nuclear physics and astrophysics?

LEIGHTON: For that kind of nuclear physics—low energy nuclear physics—Kellogg Lab has been one of a handful in the country that have really concentrated on what you can say are the practical applications of nuclear astrophysics.

ASPATURIAN: What was Fowler talking about at that time?

LEIGHTON: Well, I'm a bit jumbled up plus or minus five years along in here. I wasn't very close to Kellogg; I had my nose in the cloud chamber over in Bridge. The hot topic during the early fifties was the isotopic abundances of the elements. In connection with writing my book on modern physics, I had gone to several of the seminars and other talks in which the origin of the elements and the question of where they were "cooked" was discussed. I took some of that material right out of those seminars and put what was appropriate in my book. It was really very well timed.

Around that time, Fred Hoyle was making visits to Caltech, and some people regarded him as a theoretical wild man, one of those idea people who has a million ideas—most of them wrong—but he doesn't bother to figure out which ones are wrong. If any of them turns out to be right, he can claim, well, he was correct there.

I remember that Hoyle on one occasion gave a series of seminars in Culbertson Hall, which was the auditorium for Caltech before we had Beckman. First he gave a couple of seminars on things that were considered only moderately wild. But then he got to the *really* controversial talk, and he opened up that one by saying, "And now I would like to turn from the relatively more completely verified views of such-and-such"—which people had actually been fighting with him about—"and discuss this other subject, which, while still somewhat speculative, shows promise." People hissed and booed. [Laughter] Some people thrive on opposition. Like Zwicky, for example. If he didn't have enemies, he wouldn't get anywhere.

ASPATURIAN: Do you remember anything else about Hoyle?

LEIGHTON: The major debate he got into back then was the steady state universe controversy. It's a funny thing; there's a whole school of people who seem to thrive on being in a minority. I guess maybe it's because if they're in the minority and they turn out to be right, it's a great distinction. After all, what point is there if everything turned out the way everybody thought it would? But some of these people are very, very tenacious. You would think that when the experimental facts change, or new data are added that continue to go against their idea and support the mainstream, that they'd say, "Well, we gave it a good shot; it doesn't seem to be that way; let's get on with something else." No, no, they look around at all the musty corners—could be this, could be that; you didn't take care of so-and-so. So the controversy just goes on and on. The same is true, you know, of things like the nature of the moon's surface. Before we'd sent anything there, the question was what it would be like. Tommy Gold, I remember, came and gave a seminar at Caltech in which he suggested that the surface of the moon was so powdery and puffed up from penetration of cosmic rays and particles into the upper layers that anything that sat on the surface would just sink out of sight. The rays supposedly knocked loose chemical bonds and made the surface very fluffy. It was the "fairy castle" theory of the moon's surface. He also thought that the surface would be so tenuous and filmy that if it started to flow in a landslide, it would flow down a slope as small as a degree or so. When the Surveyor spacecraft landed on the moon and went in about one inch and stood there ready to take pictures, somebody asked Tommy, "Okay, how is it now about the fairy castles?" Tommy didn't recall at all having ever said anything about the subject. [Laughter]

Regarding Fred Hoyle, I should mention the BBC program in which Feynman and Hoyle were interviewed together in England. This was the mid-sixties or so. During the interview, Feynman said something like, "Well, Fred, you know I've watched you have your ideas and things like that, and I really admire the way you can come up with interesting possibilities. Whenever I think of an interesting possibility, I can right away think of six or seven reasons why it can't possibly work." [Laughter]

ASPATURIAN: Was Feynman being intentionally facetious?

LEIGHTON: Oh, yes, I'm sure he said it intentionally, in the nicest possible way and everything. Fred knows how people regard his theories. Halton Arp's theories about redshifts and quasars are another case. You have to say that whenever you have a really unknown phenomenon—where you have some evidence and you don't really know what's going on—that you have to distinguish between whether the thing you're looking at is some very unlikely but possible event in the ordinary scheme of things, or whether it's a highly likely outcome, but working according to a set of rules we don't yet know. In the second case, nature is doing what it should be doing, and the reason it's unexpected is that we don't really understand the substrate on which it's based. You have to say for Chip Arp that, up to a point, his counting the quasars and galaxies and their galaxy elongations, and associating a quasar with a galaxy and so forth, was a worthwhile thing to do. The problem was that eventually he used up all the data, because once you've gone through a complete catalogue and picked out those things that meet certain criteria, and you come up with your wild theory, there's no way to check up on it, except maybe some billions of years hence, when a lot more data will be produced. To insist, then, that you're really looking at new physics rather than at ordinary physics under conditions that we aren't completely familiar with yet is pushing things farther than they should reasonably go.

ASPATURIAN: What prompted you to write your book, *Principles of Modern Physics*?

LEIGHTON: Tommy Lauritsen left town for a year to go to the Bohr Institute in Denmark, and somebody had to teach the senior modern physics course. Bacher, who was the division chairman then, said, "Okay Leighton, why don't you try that?" I forget just how I looked at it at first, but when I started looking at textbooks, I didn't like the selection that was available at the time. I thought that they were the remaining dregs of older books—updated, to be sure, as time went on—but based mainly upon the transition between the Bohr theory of atomic structure and the quantum mechanical theory of atomic structure, and all the phenomena that go with that. The emphasis seemed to be more on the mysterious aspects of the Bohr theory than on presenting quantum mechanics. I felt that it was time that quantum mechanics and relativity theory, on which modern physics is largely based, be presented for their own sakes in connection with this course, which discussed the properties of atoms and molecules and things. So I tried to bridge that gap. To some extent, I think it worked, because right now there are so many quantum mechanics books that we might no longer need the kind of book I wrote anymore. I started to revise it, but I'm not sure it's really going to go all the way.

ASPATURIAN: Has it been revised before?

LEIGHTON: No. I realized fairly soon that it would eventually have to be revised. But I wanted to be involved with it myself, and I was overpowered with a lot of other things.

ASPATURIAN: I understand it was quite a big seller in its day.

LEIGHTON: Yes, it was a pretty good seller; I've forgotten the exact number of copies. I think McGraw-Hill printed 5,000 copies to start with; I think that was their standard supply. One of my friends said, "How many copies did they make?" And I said, "5,000 copies." He said, "You've got about a hundred students in your class, haven't you? So they printed a fifty-year supply." [Laughter]

ASPATURIAN: Did you write it in the process of teaching the course; was it based on the lectures?

LEIGHTON: The first thing I did was to review how we were teaching electricity and magnetism. At the time there was rather a big hint that students should have learned that in their junior year and shouldn't be bothered by it as seniors. That was fine for the senior physics majors, but the graduate students from other fields had some trouble with it. Some of the chemistry and geology graduate students may not have been quite there. So I reintroduced it into the course. Somehow, with the help of the mimeograph machine and the secretary down the hall, I actually got enough stuff done to hold me through the first year. I didn't cover everything I wanted to cover; but I did a better job probably than I ever did again, because I could do it more fundamentally. I was writing it down and distributing the notes at the same time.

ASPATURIAN: You actually wrote a text concurrent with teaching the class, and distributed it?

LEIGHTON: Pretty much, yes. I was not at that time committed to publishing it. I did not really realize I was writing a text; it was course notes.

ASPATURIAN: What was it that made you decide to publish?

LEIGHTON: At a certain point I had to face the fact that if I revised this material once more, went through and trimmed it up once more, I was going to publish it. So I stopped saying no when the book dealers kept coming around. I think I had given copies out to a few book publishers. They had it reviewed; they offered to pay people to review it, which I appreciated. Plenty of those reviews came back—I still have them in my folders. Prentice-Hall sent me a polite little note saying they'd had it reviewed and it was well done, but they couldn't figure out where in the curriculum it was supposed to fit. They said that perhaps at a place like Caltech, where you have especially bright undergraduates, and for an occasional graduate student, it might be just what they need. But otherwise it might be hard to fit. It turned out that that was exactly why I had to write it, because there wasn't anything on the market then that did quantum mechanics well enough to pass as quantum mechanics, and yet had the facts about spectroscopy and all the applications, too. So they hit the nail right on the head. [Laughter] Rights to publication turned out to be a fight between Addison-Wesley and McGraw-Hill. I don't know what made me choose McGraw-Hill, but it was okay; they treated me right.

ASPATURIAN: In what year was this published?

LEIGHTON: 1959. Afterwards, the problem was that I'd written everything I knew, and then some, in that book. Everything I could lecture to the students on was in there. And I wasn't about to go look at things from a different point of view. If I revise it, I could put in other subjects. The trouble is I'm afraid it's going to increase in length. I don't know how it can decrease in length or boil down and stay the same. I have to think about that; I don't know what I'm going to do. I'm actually starting some writing. As a matter of fact, I have a collection of orange-covered folders which were my dismal attempt to present some of this stuff at the sophomore level three or four years back.

ASPATURIAN: Did that work?

LEIGHTON: Yes and no. I warned the students that it was going to be a kind of an esoteric—and I didn't know how successful—approach, but interesting. All of the freshmen that I remember, and about 20 percent of the sophomores, did very well. But most of the sophomores didn't do very well, because they didn't come to class very much. They weren't plugged in; they really didn't care about that subject. So it was a bit of a failure in terms of the mainline, if it was to be a mainline approach. But I surely learned a lot; I read a lot of interesting material. Anyway, finally the book got written, and then got forgotten—namely, other people taught the course.

ASPATURIAN: It's no longer used?

LEIGHTON: McGraw-Hill sent me a box full of copies, which I took to be the last copies of their stock, along with a nice little note saying that the sales had dwindled. I didn't offer to revise it. But they gave me the copyright, so I can do whatever I want with it.

ASPATURIAN: Last time I was asking about changes that had taken place in physics after the war. And you mentioned Bob Bacher, who I think had a major impact on reinfusing the division.

LEIGHTON: He showed up, and he really ran it. With Millikan around, the division had not had a real head, because Millikan was unofficially in charge. There was the administrative head; that was Earnest Watson, who was fine doing what he did, but he was not the person who planned for new staff and things like that. Anyway, Bacher must have come in about '49; DuBridge brought him in. With Millikan out of the center stage, Bob brought a lot of bright young people from some of the places he'd been—especially Los Alamos and Cornell. You see, Caltech had always had trouble with theoretical physics. We had a few theoretical physicists; one was Paul Epstein, maybe von Kármán also—that is to say, if you looked around the Institute, you'd find people who were strong. Another was Richard Tolman, who was technically in chemistry but did all those things in relativity and statistical mechanics. But Millikan never trusted theorists. Did I tell you about the time when I was teaching a senior course in the third term, close to the end of the school year? Rather than have a final examination, I'd bring in a few lecturers who had been involved with some of the material we'd covered, and let them tell the students what they knew. Robertson came and gave a nice talk on relativity. Paul Epstein was really flattered to be asked. I guess he talked about the early days of the quantum theory—Planck and so on. I was part of the audience, and I learned quite a lot. Another lecturer was Millikan. He gave a talk on "Some famous physicists I have known, and the mistakes they made." I've often wondered what would have happened if Gell-Mann had been on the staff back in those days, and the quark idea with the one-third and two-thirds charges had come up. Because Millikan, you know, fought and fought with experimentalists who got variable results on the charges. Of course, since quarks exist, or at least we think they do, and they are important in nuclear physics, in retrospect it was bad for Millikan to be so adamant about the idea that charge was quantized into full units, because here it is. It turns out that in a cloud chamber, a one-third charge would be barely visible; its track, being the square of the charge, would be about one-tenth the strength of a full unit charge. As I mentioned last week, Eugene Cowan, my former colleague here who built cloud chambers, had a way of counting ions. He could actually evaluate what the ionization density was, and he found some strange-looking things. But they could have been just random particles that went through before the chamber was triggered and the ions had recombined or something. You see, unless you find a real peak on the graph of the distribution of effective charge, you don't believe it. But a real two-thirds charge would give about a half-density track—that equals the square of the charge. You could distinguish those tracks from electrons', for example. So it wasn't so foolish to talk about fractional charges. People have actually spent time looking for them. But Millikan had it all figured out and wasn't about to admit anything else.

ASPATURIAN: How do you account for the fact that a man who was so close-minded in many respects was so successful?

LEIGHTON: Oh, he was good.

ASPATURIAN: I think of a scientist who's that accomplished as having a greater breadth of imagination.

LEIGHTON: There are people who are before their time; there are people who are after their time. [Laughter] And there are people who are right at the right place at the right time. Millikan was at the right place at the right time. The kinds of things there were to be done involved glass-blowing, getting better vacuums, using new ideas about spectroscopy, and things like that. He staffed the Institute with the very best people in those fields. When quantum mechanics came along, he wouldn't fight about quantum mechanics because the chemists embraced it. Tolman embraced it; it was the thing. But Millikan still couldn't bring himself to bring really high-powered theorists here. He didn't know any; and I think at that point that was part of the reason; his friends were not the ones.

ASPATURIAN: When people like Feynman and Christy began arriving in the fifties, how did they change the complexion of the department?

LEIGHTON: Christy came first. A lot of us thought it was fine. At last we had somebody who could explain to us what was going on in theoretical physics. He was certainly the right choice; this was a good place for him to do what he wanted to do, and for him to teach the theoretical aspects of things that had gone on at Los Alamos and so on. Willy [Fowler] took one approach to that, and Bob Christy taught more of the theoretical aspects. What Willy did would have been called theoretical physics ten years earlier; but it was the hard nuts and bolts of nuclear physics when it came out in his lectures, because it was what you needed in the laboratory to get along and to understand what was going on.

ASPATURIAN: He was teaching about the applications at that point?

LEIGHTON: The applications, the practical. But there was a lot of phenomenology, where you have semi-empirical formulas for things, but the formulas work. You don't know exactly why, but it is not absolutely out of the blue that there's a basis for it. You use that as a way of planning experiments. You have to get to know a lot about how matter works.

ASPATURIAN: What exactly was Christy teaching?

LEIGHTON: He was teaching the stuff that [J. Robert] Oppenheimer taught when he was dividing his time between Berkeley and Caltech, modified and updated to include what happened during World War II. As a matter of fact, before Christy showed up, Oppy came and gave some lectures. I guess he came down from Berkeley; he was just picking up after the war. He gave a series of lectures covering a term or two's worth of theoretical physics. That meant radiation formulas and hyperfine structure and plasmas, and a lot of other things that some of us knew something about but didn't know the theory of. Oppenheimer would come in and write down the equation that he'd finished with the last time. "Now you remember, we were very much in the middle of things. $\frac{\hbar}{mc}$ 2, $\frac{n^2 e^2}{mc^2}$ 3, and $\frac{\hbar^2}{ne^2}$ 4. And I think we need a factor," and so on. "Well, let's see; does that about do it? Yes, I think that's about it." And then he'd go on, and it got stickier and stickier and stickier. Finally came the time when if you were going to finish the term, you had to fish or cut bait. Some of us got kind of uneasy about what was going to show up on our final exams.

A couple of us at the same time decided we would bail out. We took our drop cards up to Oppy, and he said, "Oh, I'm so sorry. What's the difficulty? Is it too simple? Too difficult?" He had no idea. [Laughter] So I went and told Carl Anderson what I'd done, and he laughed, and said, "The same thing happened before World War II when Oppy would come down to Caltech. Some of us wanted to learn about quantum mechanics, so we'd go and listen to his lectures. When I signed up for the course, I went around at drop time and gave him the drop slip. And Oppy said, 'Oh, Carl, you can't do that; you're the last one left.'" [Laughter]

ASPATURIAN: Did he stay?

LEIGHTON: Well, sure. Oppenheimer would have a few graduate students who'd follow him from Berkeley down to here, so he always had an audience who more or less knew what he was doing. He would pick up one or two students from here, whom he would take back to Berkeley when he returned there. So there was information being transferred. I'm only telling you about the subset of us who were not really ready for it.

ASPATURIAN: What was he like personally?

LEIGHTON: He was the mildest of men. He was erudite. He spoke very, very well; his sentences were well formed. But the content was another story. He understood it, I guess, but he couldn't understand why some other people didn't understand.

ASPATURIAN: I understand, though, that he was very conversant with a whole lot of things.

LEIGHTON: Oh, yes, absolutely. He was the Gell-Mann of his time, the complete scientist.

At any rate, everybody was relieved when Christy came, because they knew that he would give us solid instruction on what Oppy was supposed to have been teaching.

Begin Tape 3, Side 2

LEIGHTON: When Bacher came, he realized first, that we needed to get new staff, and more staff in theoretical physics, and second, that we had to bring the people here up to speed on what was going on outside Caltech. We had a lot of important things going on here, but we didn't have a really good cross-section. Elsewhere, high-energy physics was taking off, making pions and new particles and things like that. We had to get associated with some of those things and bring people here who were interested in such things. So Bacher brought people here to lecture, partly to update us, and partly for the purpose of looking them over. Nothing was ever said about, do we want this person on the staff. The approach was, "Here's somebody who understands something about what's going on and has interesting results. Let's have him come and give three or four lectures." He put us through some very nice, broadening types of lectures. One of the topics was quantum electrodynamics, which was not quite signed, sealed and delivered at the time. It was still a very active, important and ongoing, uncompleted thing. Bacher asked Oppy to come and give some lectures about some new aspects of quantum electrodynamics. And shortly after that, he invited Feynman to come out from Cornell and talk about it.

When Feynman came, he started out by saying that he was very pleased to be at Caltech to talk on an interesting subject that he was still pushing very hard on, and he couldn't wait to see how it would all come out. He started writing some equations on the board, and then he said, "I understand that Professor Oppenheimer has just been here and has given a series of lectures on the same subject. And therefore, I won't define the symbols—either." [Laughter]

It was a funny kind of thing; it tells you something about the person, to realize that when Oppy would come and lecture, the place would be jammed to the rafters at the first lecture and then about three-quarters full the second lecture, and then one-quarter full. And at the final lecture there'd be only a couple of rows filled—that was the department; they would stick it out through to the end. But when Feynman came, it was the reverse. That is, people didn't know him very well, and for the first lecture, the hall was a quarter full; then at the next lecture it was three-quarters full; and at the last lecture it was overflowing—right to the ceiling. [Laughter] Feynman was really great to have around. You just have to look at the Feynman book, *Surely You're Joking, Mr. Feynman*, to get some of the other aspects. It's the true Feynman.

ROBERT B. LEIGHTON

SESSION 4

January 13, 1987

Begin Tape 4, Side 1

ASPATURIAN: I'd like to ask you about your research in solar astronomy, which seems to have started in the mid-fifties, while you were still involved in the cosmic ray research.

LEIGHTON: That's right. You'll remember that in connection with the cosmic ray research, we had some apparatus on top of Mount Wilson. I had several friends from the war project who were astronomers working there—Horace Babcock, Olin Wilson, and others. Olin Wilson was in charge of the 60-inch telescope and knew I was interested in astronomy and photographing the sun and planets. Every once in a while, when he could find nobody who wanted to use the telescope, he would call me and say, "Why don't you come use it, Bob?" So I'd say okay, even though it turned out that the times when nobody wanted the telescope were days like Thanksgiving or Christmas Eve.

I got interested at some point in the possibility of making a guider for the 60-inch that would hold the planetary images steady so you could take good pictures of the planets. These things often started as just bench-top, home-shop, or physics-shop activities, more or less as sidelines to research and teaching, but now and then something more interesting would show up. Anyway, very much on a shoestring basis, I built this guider. It automatically "shook" so as to keep an image of a planet centered, because it turned out that I needed to use long exposures, usually from a second to half a minute or so. I was taking time-lapse movies in order to see the rotation of Jupiter. Since Jupiter rotates so fast, in one evening you can virtually photograph an entire cycle. About ten years later, this planetary work paid off with respect to the Mariner missions, because I was probably the world's expert on stabilizing images of planets. At the time, I didn't learn that much about the planets; I guess I was mainly interested in the technical aspects of getting good planetary images.

I did have in mind—if I got good pictures of Jupiter—to use them stereoscopically and see if it was possible to detect cloud layers on the planet. In view of later developments, it was not a very promising thing to do. But some of the things that showed up, say, on my images of Mars, were things that other pictures had not shown. That was also a challenge, since Mars can only get to be twenty arc seconds in size, even in the closest approaches. But with all the fantasies that people have had about Mars and the supposed nature of the surface—with canals and civilizations and things like that—and the seasonal wave of darkening, which was an accepted effect at the time—I was interested in these things. People would stare at Mars for long periods of time, and there grew up a cult of amateurs and professionals who devised terminologies for things and talked to one another. And their mutual interaction led to group recognition, or at least pseudo-recognition, of effects. They would talk about how the polar cap was "well-developed" this opposition, and one guy would say to another that the canals were "quite a bit fainter this season, weren't they?" And then another guy would say, "Yeah, yeah, they sure are," and so on. A whole terminology as to the history and topography of Mars grew out of this kind of mutual reinforcement.

I did feel a little uncomfortable about some of these things, particularly the planet guider, because planetary astronomy for practical purposes was an arcane art. Spectroscopists could do good things with the planets, but the people who just gazed at the planets and then wrote up what they saw or thought they saw were fairly widely disbelieved. And yet there was some substance to what they said, mainly regarding how big the polar cap was this year—you could make a measurement of that. Anyway, the fact that I was trying to get more accurate pictures of the planets was, in a way, a little tainted, I thought. But it was fun to do because I had the technical problem of how to hold the image steady, because that was the thing that you needed to make progress.

ASPATURIAN: Did any of your colleagues indicate to you obliquely that they thought you were wasting your time?

LEIGHTON: Not at all. As a matter of fact, Bob Bacher, who was then the PMA division chairman, met me in the hall one day and said, "Say, Leighton, I understand that you're using the Mount Wilson telescope to take pictures of Jupiter and other planets." I sort of shrank down in my collar a little bit and said, "Yes, that's right." He could have said, "Well, look, Leighton, you're supposed to be measuring the decay spectrum of so-and-so; why do I find you going up to

Mount Wilson using the telescope?" Instead, he said, "I want you to know I think that's a great idea. I think that a lot of people keep pursuing the same thing, and pretty soon it is no longer interesting. And others can't stay more than three weeks on the same path without diddling off somewhere else." I didn't know whether he was talking about me at that point or not. [Laughter] But he thought that originality and a little freedom of motion, of operation, was a great idea. And since he was the division chairman, I took that as a pat on the back. If he had said, "Well, look, you're in physics, and that's astronomy," I think I would never have kept on studying the sun. As it was, he said, "I think you refresh yourself by doing things like that. I like to hear about people extending themselves in an unfamiliar field." So I walked away a mile high.

It was great, because I like to have about four or five interesting problems to work on at any given time, on which I feel that I can make some progress, and yet not one of them so urgent that it has to be done at all costs at the expense of everything. I find it refreshing to be able to turn from one thing to something else and not have to feel that I'm giving up.

When I got my Kodachrome pictures of Mars, in '54 or '56, I showed them to Dr. Al Wilson, who had participated in the first Palomar Sky Survey. He looked at the pictures and said, "Oh, those are good pictures—they seem to be just on the point of revealing something interesting." [Laughter] Meaning, he didn't see anything that was on the recognized list of things that he would expect to show up on such a picture. I thought that was very amusing.

That work at Mount Wilson led eventually to my working on solar astronomy and also to my work on the Mariner missions in the 1960s. Let's take the solar astronomy first.

At that time, the 60-foot tower telescope at Mount Wilson was used only for a few minutes daily by an observer who was hired to take a daily picture of the total disk of the sun to show the sunspots, and to take a smaller image of the sun in H alpha and calcium K-line spectroheliograms.

About that same time, I had some contact with Fritz Zwicky. Fritz was hard to live with, a very interesting man. He was all hot on differential photography. He was taking pictures of galaxies in different colored light, using the principle of cancellation. He would take a negative transparency of one of the pictures in one color and a positive transparency at the same scale and contrast as the other picture in another color, and then superimpose them. If they were the same picture, they would cancel out to a neutral grey. But if there was a preponderance of red light coming from certain things in the galaxy, and a preponderance of blue light coming from elsewhere, you'd get the blue and the red showing up as light and dark on the composite image. Fritz gave a seminar on this subject; it was a very contentious seminar as usual. If one of his talks didn't start out contentious, he'd make it that way by making bad remarks about all his competitors. [Laughter] "Well, I told those guys," was one of his favorite phrases. [Laughter]

During this particular talk, he showed a picture he had taken of a great big heap of tin cans that had been dumped in some remote canyon. Then he had thrown on one more can, and taken another picture within a few seconds—it looked to us the same as the first picture. But then he showed the cancellation picture, taking the negative of one and the positive of the other, carefully superimposed. The third picture was all grey except for the final tin can that he had thrown on the pile, and it really stood out. So his approach was a way to find out things—to bring out some essential thing that you may have a qualitative inkling about, but making it quantitative.

I was thinking at that time about whether I could study the magnetic field on the sun. It had been found, just a few years after the war, that during solar flares—eruptions on the sun—neutrons are emitted that come to Earth. Cosmic ray particles were also emitted. This was evidence that some high energy particles were being generated somehow—that nuclear reactions were going on in connection with the eruptions seen on spectroheliograms. It seemed to me that there might be an opportunity to study the relationship between the solar eruptions—that is, to look at what would make such an energetic eruption on the sun that it would emit mega-electron-volt-type particles. The answer evidently had to do with the decay of the magnetic fields embedded in rapidly changing sunspot groups. It was a naturally occurring accelerator: they called it a synchrotron or solartron. I thought it would be interesting to use the 60-foot Mount Wilson tower to study this. I was interested in the question of whether you could take enough high-resolution pictures of sunspot groups and their surroundings to be able to study the changes in the magnetic field pattern and the geometry of the sunspots during solar flares, using Zwicky's techniques of differential photography.

Up to that time the sun's magnetic field was studied with a magnetograph, which recorded the local magnetic fields along linear segments or "slices" going across the sun—a lattice of linear traverses. One could see fragments of weak fields here and there. But I wanted to get something with two-dimensional pictorial resolution so as to be able to study large areas in fine detail, rather than simply a series of slices across the image. I thought of doing this with the spectroheliograph—using a beam-splitter to split out light of two different polarizations and treating that result à la Zwicky so as to bring out the Zeeman effect and then looking at the light of a certain spectral line that happens to have a big Zeeman effect. That would, then, give an effect of looking at an image in one direction of polarization, and then an equivalent image taken at the same time but in the opposite polarization.

ASPATURIAN: Does the Zeeman effect have something to do with magnetic field splitting?

LEIGHTON: Yes, I should explain that. It has to do with the fact that when electrons go around atoms in a magnetic field, the field adds to or subtracts something from the electron's angular velocity so that the light from that atom comes out with a different frequency. To look at the magnetic field on the sun, you pick out what's called a very pure line—one that does not have a lot of anomalous components—but only the classical *three* components, and, hopefully, amplified in separation, because the Zeeman effect is very small.

I did all this from 1957 to 1959. Although this cancellation approach was not a very sensitive method of doing things, it had much better photographic resolution on the sun in terms of kilometers than before, and was valuable for studying the relationship between magnetic fields and other processes that were going on. One of these was the brightness and the darkness on the surface that are related to what are called the chromospheric lines of the sun. These are lines that are produced by atoms well above the surface of the sun.

That all worked very nicely. And one thing led to another. I was pretty soon stymied by the fact that the image size available to do this was not very big. The Mount Wilson astronomers made spectroheliograms of what they called a small image done with an all-reflecting system. They used three-and-a-half-inch-wide film. But the lens that was used to take the early-morning pictures of the sun was a twelve-inch lens, which yielded an image seven inches in diameter in the focal plane at the entrance slit of the spectroheliograph. Part of the joke here actually was that in order to get optimal *average* results, the lens was stopped down to only four inches aperture. This beautiful twelve-inch, nicely corrected lens available at the 60-foot tower was seldom if ever used at its full aperture, because at four inches you got the best all-around average results. When the seeing was bad, it was much worse with the twelve-inch aperture than with the four-inch aperture. When it was very, very good, the twelve-inch aperture was far better. So I got permission and some support from Ira Bowen, the director of the Mount Wilson Observatory, to modify the spectroheliograph in order to make larger, better monochromatic pictures.

One of my early graduate students in solar astronomy helped build a big camera which could record three-color pictures of sunspots, eruptive phenomena and other disturbances on the surface of the sun with the seven-inch image operated at full 12 inch, f/60 aperture, using fine-grain aerial camera film.

Returning to the spectroheliograph—as soon as things got rebuilt to the point where we could get the big images through, the project really took off, because we could then measure and photograph the magnetic fields under very good seeing conditions at two-times better resolution. Of course, you could also do the same kind of thing with the velocities—the Doppler effect shifts the wavelengths also. So, there was a period of four or five years when two or three students and I were using the machine up there all through the summer, when we didn't have classes.

The really significant outcomes of this Doppler and Zeeman program were the solar oscillations and the supergranulation. I have a hard time here, because I can't remember exactly the order in which various graduate students came aboard. We all shared the observing at Mount Wilson during these periods, especially in 1960 and 1961, which were the peak years of our study of the oscillations. Shortly after I had done the first Zeeman study with the small image in 1957, I took aboard Bob Noyes as my first solar graduate student. The circumstances of the discovery of these oscillations are sort of interesting, but I have a hard time judging to what extent one ought to recognize Noyes in connection with the actual discovery. I don't know that it makes any difference, but if it ever did, I'd have to do a lot more looking back to see what was going on.

Bob Noyes and I were both taking solar photographs. We had to obtain these pictures very early in the morning, when the sun is low, and the atmosphere is very quiescent and not "boiling" because of atmospheric turbulence. Then, as the sun was coming up, we would take spectroheliograph plates or a series of camera images until the atmosphere "boiled" too much; then we'd go to have breakfast. We took a number of Zeeman and Doppler plates during this period, and dozens of large camera film images.

As a practical matter, we didn't yet have a darkroom that was set up properly and useful for the purpose of obtaining the difference-signals of these plates. I had a darkroom at my house in Altadena, and I had developed the procedures for the Zeeman case there. So, as a matter of course, we eventually adopted a procedure where different combinations of people would go observe and keep the notes about who was up there with whom and who ran the machine.

I think Noyes began studying the fill-disk camera high-resolution pictures of the sun, not for any pure spectral color but in bands of red, yellow, or blue. I worked mostly on reducing the Zeeman and Doppler plates in my home darkroom. Noyes would take the exposed aerial camera film down the mountain to a darkroom in Bridge Lab, where he and two or three other students had built a developing machine, and after the film was developed he would scan it to find the images having the best resolution and tag these for further study. I concentrated on perfecting the new spectroheliograph configuration and carrying through the image-cancellation procedures. The fact that the spectroheliograph has to move past the image in order to make a spectroheliogram has a consequence, which at some point I recognized: if you cancel one Doppler image against its mating image taken at the *same time*, you'll get the velocity field for that time. But in most cases, the spectroheliograms produced a velocity field difference at *different times* which vary from essentially zero at one edge of the image up to several minutes at the other edge.

In order to amplify the effect at first, we thought it would be nice to get a double signal by immediately putting in a new plate, reversing the direction, and coming back over the image the other way—in effect reversing the polarity of the picture as well to be able to doubly cancel the picture. Then we would cancel one picture against its mate, another picture against its mate, and finally cancel one cancelled picture against the other cancelled picture. Thus one should get *four times* the signal and only one-half the noise of doing it any other way.

I had this all arranged in my darkroom. I made the enlargements in a one-to-one enlargement, like a contact print. But I would then have to make enlargements of the new image, and then I'd have to cancel those images against each other. I had procedural machinery in my darkroom for doing all that.

One of the things that I had also worked on a bit in the early fifties was the lifetime of the cells in the sun that do the overturning, that produce the granulation of the sun. I had found out by using correlation procedures that the turnover period was about five minutes. So ten minutes is of some interest in this. I thought I could measure the lifetime of the velocity pattern by canceling these images against one another and having the time-difference go from zero to twenty minutes—ten minutes going, ten more minutes "coming back," with the Z or D fields reversed in sign. I almost at once noticed that, as I should expect, we got a big signal near the zero-time boundary. As I looked at the image for later times, the signal went away. It was a time effect. That is to say, I was ignoring, at first, the fact that the signal would change. I just wanted to amplify the thing. But then, bad news—I have to accept a certain time interval, whether I like it or not, so I can't get the *same* field signal all the way across the canceled plate. All right, let's use that for something: let's find out *how* the pattern decays. So I expect the pattern to decay; the *correlation,* in other words, to go to zero, but the *contrast* to go up. Instead, the thing went to zero correlation, and then the contrast *came back* if more time went by. And this at first puzzled me. I was in my darkroom one of the evenings after dinner, when I was doing these things. All of a sudden, it struck me that the velocity field is going away and coming back. That is to say, the solar atmosphere is in oscillation. We had taken pictures—I don't know who took the pictures. It was not organized; we were just still trying to adjust things. We were trying out different spectra, so we used a sodium line and a calcium line, and so forth. I tried one of these things and got a certain result. I had a plate from a different line and tried it again. I then predicted where that band would be, where it would go away. The parameters were different. The speed of the spectroheliograph was changing, because the sun was rising. After calculating in seconds when the signal should come back, I then predicted that another would come in the same number of seconds, and it did. It was one of the few cases where I'd actually seen something and realized, "Aha!, I now know something about the sun that nobody else knows."

So at breakfast the next day, after Bob Noyes and I had taken our plates, I told him, "I think I know at last what you're going to study for your thesis. The sun is an oscillator with a period of 300 seconds." He said, "No!" And that was the actual occurrence of things.

The other big problem we worked on was the nature of the supergranulation on the sun. The five-minute solar oscillations involve parcels of gas that go radially up and down at the surface. There's some lateral component, but it's not systematic, so it doesn't show a significant signal. The supergranulation is due to a process like you see in a boiling pot—material comes to the surface of the sun and spreads out. These are huge convection cells. The phenomenon itself has to do with the energy transport mechanisms out near the surface of the sun and similar stars. It's an interesting problem, but in terms of its astronomical importance, it's about the equivalent to the lifetime of fruit flies to a biologist.

The nature of the supergranulation was getting a lot of attention at the time in connection with Project Stratoscope. This was a balloon project, in the fifties, to fly a telescope on a high-altitude balloon and have that telescope point at the sun, with an image-guider, taking pictures. It was sponsored by the NSF and Princeton University. Martin Schwarzschild from Princeton was the principal investigator. In anticipation of results from the various flights, he had some of his young colleagues come out and use the Mount Wilson solar telescopes. The convection cells make sort of a network field on the sun like the surface of a dish of rice. They showed up when we took pictures and they showed up also on the full image of the sun. The big question was, whether the rice-grain structure was topologically polarized. Visual observers had looked at it for a long time, using telescopes with high magnification and with filters to bring the intensity down to where you can look at it with your eye. They swore that the rice-grain structure was not a random pattern of light and dark features, but consisted of bright elements surrounded by dark boundaries. In other words, that it *was* polarized in the topological sense.

This work was a source of controversy, and Schwarzschild wanted to get photographic data that would settle it one way or the other. It was an interesting thing and since I was already busy taking high-resolution pictures of planets, I naturally got interested in the solar problem. So I built a film-transport camera system to take 35mm pictures. We would film very early in the morning on days when the seeing appeared to be very good. There was a solar observer at Mount Wilson who was available to do that. And this camera was all set to go. All you had to do was put in a light beam, pull the dark slide, and turn it on, and it would then click away. The observer would be doing his other observing—there were two telescopes there—so a lot of data came out. Without question, some of the pictures that we got showed very clearly bright grains within a dark boundary. I wrote a short paper about it.

Schwarzschild, interestingly enough, was unconvinced. He and his theoretical colleagues were analyzing the rice-grain structure as if it consisted of a set of random elements, light and dark, running around on the sun, and rising and falling again and so forth. It wasn't until his own telescope came back with pictures that showed the rice-grain structure with this granular polarization that he changed his mind. It was his own data, and so he became convinced that that was the nature of it.

ASPATURIAN: What interested you, or what did you find more rewarding about this? The actual observations or the success of the instrumentation?

LEIGHTON: Well, in this case, clearly the observations. But to know how to get the observations, that was just fantastic. I think that in almost any new experimental discovery there are phases. You don't just buy something off the shelf and say, "Let's run it," and then find something new that people hadn't seen before. You generally either buy something off the shelf and modify it so it can work ten times better, or you gin up something yourself that you have the confidence will tell you something you might be interested in. We didn't realize that we would find oscillations at all. We didn't know we would find the supergranulation. And the funny thing is, almost all the procedures that we used to do the job were absolutely available to George Ellery Hale perhaps twenty or thirty years earlier!

ASPATURIAN: Why do you suppose they had not been uncovered at that time?

LEIGHTON: Interesting question. It just goes to show that the search for knowledge is consistently, almost automatically, undervalued. It's hard to get grants to do things. People will usually ask you, "What do you expect to find?" You can't tell them what you expect to find, because they automatically assume that if you have something to say about what you expect to find, it means that it's already known. On the other hand, if you say you don't know what you'll find, the assumption is that the project can't have much value because your imagination isn't good enough. So it's hard getting support.

Anyway, these two findings—the supergranulation and the five-minute oscillations—were qualitatively unexpected. That started a lot of different lines of research. I gathered a team of graduate students who wound up to be an interesting and compatible group. There were about five or six people who did thesis work on some aspect or another of these things. And, interestingly enough, almost all of those people stayed in the field and are doing follow-on and related work today. I fell off the wagon and went other ways. [Laughter]

I also found that Mount Wilson is a hell of a place to try to take solar pictures throughout the day. First thing in the morning, it's often absolutely superb because of the layering of the air—it's just fine. You know the solar "blue flash"—aka the "green flash"—the optical effect seen with the sun setting or rising on a distant, very sharp horizon? As the sun would come up, I would get the best blue and green flashes on Mount Wilson but the quality of the seeing would begin to deteriorate very soon after that. That is what led us to carry out a site survey for a new solar observatory—that, plus the results we were getting at the 60-foot tower and the realization that we wanted to study more about sunspots and solar flares. The site search ultimately led to the site that wound up being the Big Bear Solar Observatory.

That's an interesting story all in itself, because we made a very careful, objective—a blind selection of what places to look at. It was all carefully set up, so that there was a minimum of human intervention involved, to avoid things like, "Well, let's go today, because I have a hunch that that's a good condition for Mount Piños." We looked at the sites in pairs with two observers, so that on the same day, you could compare Mount Wilson against some other site, or any one site against another site. The site surveillance was carried through by two observers according to a schedule made up ahead of time using a random number process to select which site would be compared to which other site on a given day. The comparison part was random, and it was thus possible to actually compare site A against B, under enough different weather conditions. The observers would always be there early in the morning, ready to take data, even if it was getting ready to storm and even if both observers were assigned the same site for the day. In the latter case, the data were used to compare the observers!

ASPATURIAN: Was this a Caltech-sponsored initiative administratively, or was it something a group of scientists, including yourself, initiated?

LEIGHTON: No, Mount Wilson and Caltech did it themselves to try to find a better all-day site than was then available. The Office of Naval Research also supplied some money.

ASPATURIAN: Who besides you was involved?

LEIGHTON: Well, I guess Hal Zirin was not here quite yet; maybe he was here. At least he was certainly interested. Ernie Lorenz was the fellow who actually operated the site-testing itself—he was one of the observers. We learned a lot of things about seeing. Some people thought that an offshore island, where the incoming air is a gentle onshore flow that had been undisturbed for many miles, would be a good place to put an observatory, because you'd have very good seeing. We tried the beach up at Refugio State Park, north of Santa Barbara; we tried the cliff side of Catalina; we tried Mount Piños and Reyes Peak—I think that's the name of it—in the condor country. We also looked at Lake Elsinore, because we felt that if the sunlight is coming over water or vegetation that's a better condition than if it's coming over rocks and sand. At the beach, you have to cope with the thermals—the "boiling" of the air when you look parallel to the sand. We found that out at Refugio Beach, too. It turns out the beach is a very poor place for an observatory, because the sand warms up and messes things up right away. And Catalina was bad because of the fact that the air had to be lifted orographically to get up over the top of the island, and that started counterflow and turbulence going. It's true that there was very steady air coming in, but it was thermally stratified by a temperature inversion that extends out to the ocean. So when the air was lifted and stirred, you got the beach-sand effect, so it was very poor.

It finally came down to two places: Lake Elsinore versus Big Bear. Lake Elsinore had the "best seeing"; but it was more apt to be fogged in than was Big Bear. So the upshot of it was that even though Big Bear's best conditions were not as good as Elsinore's best conditions, we chose the place where the average conditions were going to be best, and yet a place that was far better than the one we had at Mount Wilson.

There's one other element that I'm just now recollecting: there is a place called the Lockheed Solar Observatory in the Hollywood Hills. One or two of my former students may still be associated with that. Another two or three of them are at Lockheed-Sunnyvale, very bright young people. At Lockheed Solar Observatory they had a site that they used in much the same way that we now use Big Bear. They took monochromatic pictures in the H-alpha hydrogen line, summer and winter. And they got flare pictures, which were time-lapse, so that they could see effects. They were marvelous pictures, and it created a lot of stir. It wasn't just our results that caused people to be optimistic. It was partly the Lockheed results that helped <u>us</u> to become optimistic about not only the things we were trying to do at Mount Wilson, but also the things we thought would be even better at Big Bear.

ASPATURIAN: So you realized you could do a lot more if only you had a better observatory site.

LEIGHTON: That's right. And we realized that between Caltech and the Mount Wilson people, we wanted to be in that business. That, as a matter of fact, was part of the reason why Downs Lab has some rather large holes on the roof. The building was designed that way, for my solar things. We had developed computer-driven data-processing schemes to take the place of our slow photographic cancellation process. A lot of things were also going on at Kitt Peak. They had built the big McMath Telescope, and it was doing its things, too. So, our solar work was probably one of the reasons Caltech was able to get the money from NSF to build Downs Laboratory.

Up to that time, we had been working in lower Bridge. One recurring dream I used to have had to do with suddenly discovering there's an unoccupied laboratory somewhere on this campus that I have some title to, and I can do anything I want to in it. The notion of clean, unsullied space, unlike what it finally becomes, especially in Bridge Lab. [Laughter] I don't know if it was triggered by a particular experience I had, seeing a laboratory with nothing in it, or whether it was just a little frustration of having no place to work. Anyway, the lab is there in my dream, and I bring in a few odds and ends and set them on the shelves. And then I wake up.

In 1963, we got Zirin to come from Colorado; he established himself both at Bridge Lab and at Big Bear. He's very much of a go-getter, so he got money to build the observatory, and also money to make it work. I played no role in that. The work in the lab across the way here [i.e., Downs] succeeded in extending the procedure and improving it to the point where you could take time-lapse pictures of sun spots with magnetic fields moving around and erupting and such things. It is a very active and important observatory now.

ASPATURIAN: Am I correct in thinking that once things had gotten beyond the stage of raw innovation, you wanted to go on to something else?

LEIGHTON: Well, I wasn't afraid to.

ASPATURIAN: But that seems to be kind of your characteristic.

LEIGHTON: Yes. I think of it as incarnation myself.

ASPATURIAN: Present-at-the-creation-type stuff. There does seem to be a flavor of this.

LEIGHTON: [Laughter] Yes, there is. I don't like to be characterized as being a person who isn't interested in the things that his instruments will show, but only interested in the instruments themselves. But I'm afraid the fact of it is that I probably get my biggest kicks and make my best contributions on the instruments—up to a point. If I had really thought deeply about the solar things, I might have made some further significant contributions along the time-lapse lines. Perhaps I got off the bus too soon, perhaps very much too soon, from a certain point of view. But I wouldn't have had a lot of other experiences that I had, and I can't complain. But this was just the time when the linear arrays of photo-sensitive diodes was coming along. And the obvious thing to do was to get rid of the photographic plates up there and put a computer on the line and read out the spectral lines along the photo diodes right along the spectrograph slit. I was very late in getting into computers. As a matter of fact, George Simon, one of my graduate students, rubbed my nose in it so much that I just simply had to learn how to do FORTRAN. I went over and sat in on a course that Bob Nathan, who's at JPL now, was giving on a Burroughs machine. That was a machine where, if you wanted to say "Add," you had to say "fifty-five, forty-four;" if you wanted to subtract, you'd say "fifty-five, fifty-five"—all numerical things for assembly language programming. Terrible. In fact, it wasn't even assembler line, it was machine-language programming. So I just dropped out of that, and I thought, well, computers have some way to go before I'm going to spend my time doing that! [Laughter]

But then I came to the point where I was calculating the parabolic curve that we wanted for a spin-cast infrared dish over here. I had worked it out with my slide rule, but Simon said, as he handed me a book on FORTRAN, "Take this home and study it. Then work up your program to get that parabola." Simon was very gentle, a great person, but he did it in a way that there was no getting around it. And so, lo and behold, my first program spoke to me! [Laughter] And I've been hooked ever since. I still am not all that good at computer hardware, but that's just as well; otherwise, I think I'd spend all my time doing that.

[Some material in this section was originally recorded during interview sessions two and six.]

ROBERT B. LEIGHTON

SESSION 5

January 20, 1987

LEIGHTON: I think it was the early 1960s, like 1961 or '62, that Gerry Neugebauer and I got interested in building an infrared telescope.

ASPATURIAN: Was Neugebauer a student here at that time?

LEIGHTON: Gerry started his doctoral work with Carl Anderson, and then I think he moved over to the synchrotron to do a thesis. I knew him, but at that time not very well. Then, after he got his PhD, he went to JPL for his army service, and I lost sight of him again. We in the physics department were fishing to get him back down on campus. Those were the days when there was a draft. He was a commissioned officer, I believe; he did some Army ROTC, and apparently he was assigned to JPL to be cognizant of various aspects of research up there. Anyway, right along in that period, he and I started to talk about making an infrared telescope. And when he came down as an assistant professor, we got serious about that.

ASPATURIAN: This was the very early period of infrared astronomy. Was it contact with Neugebauer that brought you into it?

LEIGHTON: It's hard to tell. I have somewhere a notebook in which I was fiddling around with how to make superconducting bolometers of some kind. But it never got off the ground; that is to say, I had some ideas, but I was not in a position, or was not inclined or able, to push that. And I'm not sure I would have gotten anywhere on it if I tried. But Gerry, possibly through the JPL connection, was plugged into Santa Barbara Research [Corporation], which was leading the world at that time in sensitive near-infrared detectors. So he had a semi-official source because he knew the people there. He became involved with spacecraft up at JPL, not in a line role, but rather in a scientific capacity—that is to say, pushing for the scientific aspects of space experiments. Gerry was interested in the astronomical applications of all this, as I was. I don't know why I didn't participate more in the preparation of detectors, but that seemed to be in good hands between Gerry and Santa Barbara Research. Gerry had a technician at the time, Dowell Martz, from JPL. I guess Martz had been doing things at JPL also. It boiled down to how we could make an instrument that would be sufficiently sensitive to be interesting and sufficiently precise to be able to locate objects in the sky—and how to make the whole thing sufficiently rapid in measuring the source to be able to cover the entire sky visible from here. Practically right away we started to think in terms of short-focus, large-diameter, optical mirrors, as the way to do it. We went to Hortie-Van Company in Pasadena, who were big on searchlights at the time. You could even find searchlights in the surplus catalogues. We looked very carefully at some of the searchlight mirrors, and they were fine for searchlights, but they were lousy for us: we could see the distortion with the naked eye. There were also a couple of groups that had been making spin-cast epoxy parabolic reflectors. One of them was Kennedy Electronics in Cambridge, Massachusetts, and one was Gerard Kuiper's group in Arizona. They had made one or two pretty good spin-cast mirrors, which were stated to resolve to five arc seconds or so. And I think I may have laid eyes on one of those; Kuiper had literally gold-plated the reflecting surfaces. But he didn't go much farther than that. He did not think about sky surveys, although he was the type who could well have done that.

So it boiled down to trying to figure out how to design and build a reasonable telescope.

In that connection, you may be interested in some experiments I did—I didn't know I was experimenting, I was just having fun—when I was about seven or eight years old. I noticed in my mother's mop bucket, that when it was filled with clean water and had some sand grains or partially buoyant fragments of leaves, and you stirred the bucket to make the water swirl rapidly but smoothly, there's a odd thing—the sand or the leaves go round and round at the bottom of the bucket and finally get deposited as a pile of matter at the center of the bucket's bottom when the swirling dies out. It's a very striking effect. Considering that I went on into physics, I passed up an opportunity at some point in my life to explain what was then a big mystery. I believe it's called Eckmann pumping.

We built the reflecting dish along the same principles. You have a vessel with fluid in it and rotate it very smoothly in an equilibrium condition. This is where the vessel as well as the liquid is rotating so it doesn't slow down, but gradually builds up to a certain constant speed. Pretty soon the liquid is going at the same rotational speed as the vessel it's in. If the speed is just right, the upper surface of the liquid will then have precisely the shape of a parabola. We made our first dish using epoxy as the fluid that makes the parabola. But it sets, so pretty soon you can stop the vessel rotating and aluminize it—we didn't gold-plate ours—and you have a reflector. We made it in the back of my office when we were in Bridge Lab, in a space partitioned off in the back of the office. That was the best place to work because it was on the ground floor, not upstairs where the building would vibrate. And it was in a place where nobody would tramp around or have heavy loads. Initially, we just tried a lot of different kinds of epoxy, most of which didn't work worth a darn, and finally wound up with one, courtesy of somebody whose name I've now forgotten over at a place on San Fernando Road.

I think it's fair to say that a good fraction of the surface of that reflector was good to a few arc seconds. I was also working up drawings of a mounting for this thing. I had the mounting built in the central shop and assembled the whole thing in the cosmic-ray lab. In a matter of a few months we had a device with a photoelectric, infrared-sensitive cell at the focus. Just outside, between the Bridge Library and the cosmic-ray lab, was about a 10 to 15-foot-wide space. We pulled the telescope base on a dolly out of the lab and lined it up as best we could. I'd made gear drives and other such things for it. It was kind of a nice telescope, as a matter of fact.

ASPATURIAN: Was your interest in this mainly the new technology? How much did it actually have to do with observations in the infrared?

LEIGHTON: We were inventing the instrument in a form suitable to make a sky survey. We had automated the gear drives and the declination drives. Whether we did that before looking at something in the sky, I'm not quite sure. But by the time we took it to Mount Wilson, it had been an operable instrument down here on the campus, where we wheeled it out at night to test it and brought it back in during the day.

ASPATURIAN: What were you looking at?

LEIGHTON: Beta Pegasi was the first infrared, very bright red, cool star that we found. The fact that we had found one meant that the survey was worthwhile, because we could only improve from that point.

ASPATURIAN: Did you look at the planets at all, on campus?

LEIGHTON: No. But in conjunction with this infrared survey, Eric Becklin, who had joined the group as a graduate student, and others were interested in using these same detectors on the big Mount Wilson telescopes to look at the planets. This little survey instrument of ours was good enough to resolve maybe two or three elements across Jupiter, but not much more. And we were sweeping it across the sky so rapidly that it wasn't feasible to conduct a planetary study. It was more important to keep a tight schedule on running the survey because of weather coming into the picture. We had to do it and get it over with.

I can tell you about one of our most interesting discoveries. As Neugebauer and I were both watching the moving chart paper on which an electronic signal was being recorded, we both noticed a very strong infrared signal which had no visual counterpart. Now you can appreciate that if you go back and forth and back and forth, you get pretty tired of seeing these signals coming along. When you're doing a lot of other things, like reading the right ascension when the signal changes over and writing it on the chart record, you don't pay too much attention to watching the signal. Nevertheless, we both must have been more or less watching the chart as a huge triple "bump" came through one of the infrared channels. We didn't remark about it at the time, but it was pretty big. We did both notice that the red signal data coming through on an adjacent channel and delayed a few seconds in time was not very big; in fact, we didn't even notice it! So we both sensed that something was missing. Either we hadn't seen a big "bump" before the IR [infrared] one came, or as I believe, we were going in the direction where the red signal would come after the infrared signal.

ASPATURIAN: So you had something that indicated high infrared intensity, but very little visible intensity.

LEIGHTON: Oh, yes! We knew that was a prize source. We were at that time trying to find some of these objects on the Schmidt survey—up in Cygnus somewhere. We noticed another one. And that one became known as NML Cygnus—Neugebauer, Martz, and Leighton Cygnus. And it gave rise to the term "dark brown" stars. They were so cool that they were not even red; they were brown. Altogether, we found some tens of thousands of sources. This was a lot more sources than anybody thought we would ever come across, and several of these were of the type I have just described.

[Reading from IRC survey]: "Approximately 20,000 sources were detected in the survey. Of these, 5,562 were brighter than K=3." That's third magnitude in that infrared band. "In addition, 50 sources were included as possible candidates to be variable stars of some kind." We went over the sky three times actually, and checked on the magnitudes of the objects because there is a lot of tendency for these infrared objects to be variable stars. "The data represented in this first survey were obtained from January 30, 1965 through April 7, 1968."

Infrared astronomy was growing by leaps and bounds all through this period. We just happened to be there first. There were other surveys. I think what wasn't appreciated at the time was how many sources there were in the sky that were intrinsically quite bright, but were embedded in nebulosity, possibly of their own making, which made them not part of what the astronomers were originally calling a star. It was a star under special conditions, you might say. They weren't expecting to find so many of these. The numbers, more or less, fit together these days, with extrapolations—like allowing for absorption of the light in the galactic center, for example. But it was just one of those things, one of those little serendipitous goodies that came along. There were a lot of them—as many as several hundred, I would say. People scrambled to categorize the various kinds of things that were showing up, and then to study those. Depending upon what criteria you used for selection, you'd have some set of objects to look at more closely.

ASPATURIAN: Who else, in addition to Neugebauer and you, was involved in this project?

LEIGHTON: Neugebauer ran the group. He's the type of person who always has students around him. I'm not good at things like that; the students have to sort of come to me. On many of the things that I've done, I hated to take up the valuable time of a graduate student, doing the engineering that needed to be done to get whatever finished I was trying to finish, like redesigning the spectroheliograph or something like that.

ASPATURIAN: So did you end up doing it yourself?

LEIGHTON: I wound up doing the design and a lot of the actual construction work myself, because again, money's always tight, and I knew what I wanted, and I could do it much faster than any shop person could. That was not true on the infrared telescope; that was built over in the central shop. But I have to emphasize that it was built out of sheet iron, not out of heavy steel parts and things like that. So as a 60-inch telescope, it cost only a few percent as much as an honest-to-God astronomical 60-inch telescope would have. By the way, my son Ralph saw it listed among the world's largest telescopes in some almanac or other in the 1960s. But on the other hand, you couldn't use an astronomical 60-inch telescope for the survey, because the focal length is too great. And the scale is so great, that the detector you use to cover the sky has to be rather small. That is to say, you want to do several minutes of arc of declination per sweep. So you don't want physically a very big device in the focal plane. Now it's true, we could have used a big telescope and then de-magnified it down to a small field. We might have had much better position data, but we would have had no better sensitivity. As it was, we just about had it nicely matched.

ASPATURIAN: Do you recall, offhand, what were half a dozen of the major discoveries to come out of this survey, in addition to the very cool stars and the dust nebulosities? I guess a lot of it was follow-up work, really, analyzing what some of these phenomena meant.

LEIGHTON: Yes, and it was also making use of the fact that there are objects in the sky that have been known for a long time, like planetary nebulae; the Orion nebula and gaseous nebulae, and some of these have been suspected sites of star formation for quite a while. So one of the ways—I wouldn't call it a great new discovery or anything—the IR catalogue has been found to be useful is looking for clustering of the sources in the sky. That worked rather well except near the galactic center, where the sky is so riddled with possible sources that you lose track in counting them. But as a kind of an enriched source of possible objects, when you state your criteria and what part of the sky you're going to look at, you can pick out likely objects for various infrared questions from the catalogue. I would say that the envelope stars were a major discovery, although the trouble is that what came out of our survey gets mixed up with what the bigger telescopes' projects were, because obviously Neugebauer and his group were also using the 200-inch, the 100-inch, and the other 60-inch [on Mount Wilson], and whatever else could be used. I missed out on some of those things myself, like the galactic center. I certainly wanted to participate in looking at that, but there were other things at that time. Or at least I ran out of steam before I got to the galactic center. [Laughter]

ASPATURIAN: You also worked with Neugebauer on the first Mariner project, in 1964. Did that initially start as a result of your collaboration on the infrared sky survey?

LEIGHTON: Partly, yes. Because of my work at Mount Wilson, which I discussed earlier, I was known at Caltech, and maybe in certain circles around the country, as something of an expert in planetary photography. I can't say I had put years into it the way some people had, but I got some pretty good results with what I had done. Now, in the early sixties or late fifties, while Gerry was up at JPL, he was put in charge of, or was assigned to help with, evaluating proposals for possible scientific payloads for some of the Mariner shots. One of these was Mariner IV, which was slated to go to Mars. Among the various proposals, there was notably missing any proposal just to take photographs of the planet. There had been studies done on what possible approaches could be used to take pictures. These were farmed out to various possible participants.

ASPATURIAN: How is it they didn't approach you on this?

LEIGHTON: I wasn't a planetary scientist. I'm sure if I'd stepped forward and said, "Hey, I think I know how to take pictures of Mars," I would have been listened to.

ASPATURIAN: Which, I guess, is essentially what you did.

LEIGHTON: Well, yes; except that somebody had to grab me from backstage and push me on the stage. [Laughter]

ASPATURIAN: Who brought you?

LEIGHTON: Neugebauer. Actually Neugebauer, and Bruce Murray—who was fresh on the staff. Bruce was interested in planets as physical objects. He borrowed my 16-inch telescope, as a matter of fact, to take to White Mountain to look at the moon and measure in the ten-micron infrared window in the Earth's atmosphere how far solar heating penetrates into the moon. At ten microns, the effective surface from which the moon radiates to the Earth is several centimeters down. The upper material acts like a blanket, so that when the sun goes down on a certain part of the moon that's been baking in the sunshine for two weeks, the moon cools off under the surface only very slowly. So, by making measurements from Earth of the temperature of different spots on the moon during its various phases, you can get some idea of how spongy or dense the moon soil is.

So, Bruce is a real planetary scientist. And Gerry was interested in the infrared. He and Bruce arranged very quickly to write and get accepted a proposal for planetary photography—the Mars imaging experiment. I became a principal investigator [PI] on that experiment. I went to a lot of engineering meetings. JPL did all the hands-on craftsmanship.

ASPATURIAN: That must have been a change for you.

LEIGHTON: That's right. I didn't get near a bench.

ASPATURIAN: Was that by your choice?

LEIGHTON: The proposal was that certain people from JPL would be co-investigators. It was [Robert] Sharp, Murray, and me. I guess Neugebauer wasn't even a co-investigator on that thing. Anyway, that got us started. I guess the only important comments are that I and perhaps Bruce—Bruce was familiar with this—intervened in the matter of deciding how the pictures of Mars were to be encoded in pixels. It was not necessarily a problem of how many pixels there were, but of how many bits of information would there be per pixel, in order to have a wide enough range to distinguish the shades of gray that there are on the Martian surface. There was a problem, because these were the days when you didn't have very much weight available for the payload. The bit rate with which you could modulate the signals coming back was so low because of the data-handling problems on the spacecraft—you had to try and send back all of the picture data and the infrared data and everything like that—that there were real limitations on how many bits you could use. JPL was going to use about three bits. But we absolutely insisted upon there being, I think it was, eight bits. And it was agreed. The point is that Mars is a particularly blank planet in the sense that it has dust storms. It has tremendous features, but it's so far away. The dust storms leave it even more featureless, because dust lands all over everything. The photograph TV part of the mission would have been a real failure if they'd only used the eight shades of gray that are possible with three bits—this was black and white.

ASPATURIAN: Here you were participating in what must have been the first effort to get pictures of another world in the solar system. What struck you and your colleagues at the time as more important—the actual instrumentation planning or the implications of what it was you were doing?

LEIGHTON: Well, it was to find something out about Mars, the surface of Mars, in sufficient detail that we could get to another, higher level of understanding. But you have to appreciate that it was done with twenty pictures. That was it.

ASPATURIAN: Why only twenty pictures?

LEIGHTON: Tape recorder storage capacity. Things had to be taken in a rapid mode as you went by the planet and stored on a tape recorder on a TV; and then it had to be played back at a few bits per second, picture by picture. I guess I was actually on TV when the pictures were coming back; it was real-time when they were broadcasting some of the things that were being found out. I figured out that one picture's worth of bits was like pearls strung some miles apart on a string from Earth to Mars: the length of time it took to transmit one picture from Mars to Earth was about the time it took light to get to Earth from Mars. So there was your picture, all strung out and coming in. And I thought that was kind of a nice way to look at that.

The thing that Mariner IV discovered on Mars was what a lot of people had for years expected and talked about, and that's craters. Now it wasn't clear that Mars should have craters; it wasn't clear that it shouldn't. So the decisive result was important, because then it stops a certain body of science that was pushing no craters. So now the arguments go on on a different plane.

ASPATURIAN: Any other discoveries?

LEIGHTON: The density of Mars' atmosphere from this experiment came out to be only about 10 percent as much as Earth-based observations had indicated.

That really was a landmark experiment. And by today's standards the equipment we used was so rudimentary. The limits of bit rates and things like that were very stringent in those days, and to get back any pictorial data at all was very difficult. The use of the spacecraft for this other purpose, of acting as a probe to check the density of the atmosphere, was a happy idea.

There were two more Mariners that I was closely associated with. Then I had sort of a peripheral role on the Viking lander and the photos what were taken. I got a lot a data; I got to see the pictures. But I was too busy being division chairman then to actually enjoy myself. But there was another interesting feature of those three Mariners that I was involved with. It turned out that each one of them, in flying over the limited part of the planet that we chose, could only make one track of pictures for a fly-by. How you guide a spacecraft to go in the right place to have the right terrain in the field of view was a matter for the navigation people at JPL. But we had to select the things to look at on the basis of what we thought might be the intrinsic interest in various areas. There's an area called Hellas that shows up very light-colored, whitish, on various occasions. Being a manifestation of something that seems to change on Mars, it was a good idea to take a look at that. And then there were the polar caps. There were famous features called Syrtis Major and Margaritifer Sinus, and all sorts of anthropomorphic names as well as mythical ones. But the interesting thing is that each of those three spacecraft—going over terrain which all was selected ahead of time and was not selected on the basis of really very deep knowledge of anything—managed to uncover a particular type of terrain that had not been seen by any of the previous spacecraft, or any other previous experiment. Mariner IV had essentially revealed craters on Mars, but very little else as far as the pictures were concerned. There was a little information about the atmosphere, but much more information came from that fly-by experiment.

Mariner VI, I think, was the one that flew over Hellas. And Hellas turned out to be essentially a featureless area. That is, there were no craters and no mountains; there were no grooves, nothing. And it became the prototype of what is called "featureless terrain." Bob Sharp, being the geologist, supplied the name.

Mariner VII revealed a type of terrain seen near some of the famous markings. That area had some features that came to be called "chaotic terrain;" that is, the kind of thing that you get if you have a random distribution of permafrost, dust, and sand in a granular state. And then the permafrost disappears, leaving the other debris slumping and making surface shapes but not having any lineation, any connection with anything else.

And unfortunately, I fell down on the job with those three experiments. I didn't have the wit to realize that if you could send three spacecraft past Mars in an essentially random manner, being certain only not to look at the same main area twice, and come back with something new each time, that must mean that the chance of seeing something new again was very great. It should have been a tip-off that there were many more things on Mars that would turn out to be examples of something that will have been seen for the first time. And that indeed proved to be the case. Eventually, many more distinctive things, like the big volcanoes, the canals—I mean, the big deep gullies, in which, evidently, fluid has flowed—were found. So that was a bit of an oversight on my part. Anyway, those were great times.

[Some material in this section was originally recorded during Session 6]

ROBERT B. LEIGHTON

SESSION 6

January 27, 1987

ASPATURIAN: At the time you were involved in the infrared and Mariner projects, what essentially was the material you were teaching? I imagine it was somewhat different from what you were doing.

LEIGHTON: Let me think. My teaching started out before the V-12 was off the campus [i.e., during World War II]. I taught a section or two of Navy freshmen, including [former JPL senior scientist and "Voice of Voyager"] Al Hibbs. A lot of them grew up, you might say, and became very prominent engineers and scientists. But the first serious teaching I was assigned to was an introduction to mathematical physics. That was a course that I liked especially when I was an undergraduate, and I did very well in it. And it wasn't all that many years since I'd been an undergraduate. It was a very comfortable teaching assignment, because I knew there was a good textbook available. I never had taught a course in what you might call my research. In the particle studies in cosmic rays, for instance, the theoretical aspects involve either the theory of acceleration of cosmic ray particles or their interaction with the atmosphere, both of which I considered dull at that time.

ASPATURIAN: Did the teaching become kind of tedious after a while?

LEIGHTON: Not in the mathematical physics course. I guess I taught it for two or three years. Then I taught for a couple of years—at least one year and maybe two—a section of the electricity and magnetism course called Smythe's course. I had done modestly well in that course as an undergraduate. [Laughter] I took it as a senior. And with a good deal of preparation during the summer, I was able to work the problems that I felt I should know how to work. The reason that I taught the section was that Smythe's son had registered at Caltech for his graduate work, and since Smythe's course was a required course for all graduate students, it put father and son in a conflict of interest situation. So Smythe saw to it that his son, Rodney, was in my section. That was a great experience, to teach a course that was a serious-level course in electricity and magnetism.

And then, after that, I was presented the opportunity to teach the modern physics course. And that led to my textbook. That was a tour de force.

The modern physics course was probably the last course that I taught with any kind of originality. The Feynman course was important, and I played a role there in the editing, and translating "Feynmanese" into English. That was an interesting and an exciting time.

About 1963, which you'll recognize as the time when Neugebauer and I were talking about infrared, and when I was getting interested in Mariner, along came the Feynman lectures. That resulted from a project—in which I played some direct role—to redo the freshman physics course. I had some ideas about how to do that, and some of the other people on the freshman physics committee had some ideas as well. But partway through the discussions, Matt Sands said, "Well, really, we should have Dick Feynman present the lectures and have them tape-recorded." Sands was then a professor of physics at Caltech. He was a very forward type of fellow. He had been on the Los Alamos project as a young person, so he knew Feynman well enough to go and talk to him. But Feynman resisted.

ASPATURIAN: What was it about Feynman's lectures that made him the obvious choice for this kind of thing?

LEIGHTON: Feynman has a peculiar property, which is that at the time he's explaining something, it appears very clear and transparent; you can see how everything fits. And you go away feeling very good about it, as if, "Well, there's a lot of loose ends there I want to follow up on; but boy, wasn't that great!" And about two days later, like what they say about Chinese food, it's all gone and you're hungry again. And you don't remember quite what happened.

I witnessed it myself. In the late fifties, Feynman gave a talk to a lay audience on the special theory of relativity. In his characteristic way he reduced the subject to its lowest terms, about the $1-\frac{v^2}{c^2}$ 5; all you have to learn about is the square root of $1-\frac{v^2}{c^2}$ 6. And so he proceeded in an hour to lay before this lay audience the basic ideas of Einstein's theory. 201 East Bridge was the room, and it was tremendously crowded, of course. On the way out, I heard a girl saying to her escort, "I didn't understand much of what he said, but it sure was interesting." He had a way of doing that.

ASPATURIAN: It sounds like he gave virtual lectures in the sense of virtual particles.

LEIGHTON: [Laughter] Well, that's right. Yes, bringing the thing out into reality only for a limited time, and then watching it sink back into the sea.

ASPATURIAN: The idea was to get him out of the vacuum permanently.

LEIGHTON: Yes. So Matt Sands went to Feynman, and Feynman balked, but eventually he agreed to do it. And that was where the Feynman lectures came from.

Begin Tape 5, Side 2

LEIGHTON: In his teaching, Feynman tried to organize undergraduate physics into a two-year sequence, which turned out to be three years, because in the first two years he didn't really get to quantum mechanics, although he did deal with isolated pieces here and there. He started right out with atoms; he didn't hold back on atoms, leaving them to the chemists, and teaching pulleys and strings to the freshmen! He pushed the freshmen's nose into the fact that what physics *is* is the properties of atoms. In this categorizing way, he tried to make each lecture an independent, self-standing thing. Now you can only do that to a certain extent, because you've got to base your knowledge on some level of mathematics and on some sophistication in the application of mathematics to physics and things like that.

Anyway, at first it seemed like a great idea to get Feynman to do this. As a matter of fact, it turned out to be a better deal for mature physicists than for the freshmen. Feynman's course was a little too rich for most of our freshmen. For about 20 percent, it was the ideal thing, absolutely great. For about 60 percent, it was not. Their reaction was more like, "Exactly what do they expect us to learn about all this?"

I was in charge of the laboratory and the coordination of the course for that first year. I was also in charge of the transcription of the lectures into written form. I explain in the forward to the book how we expected that the editing was going to be a job for a graduate student to dot some I's and cross some T's, and change a word here and there that the transcriber might have misunderstood or something.

ASPATURIAN: How did you happen to get the assignment of overseeing the editing?

LEIGHTON: I'd been chairman of the course modification group. You don't want to hand it to Feynman to run the whole course himself; he's going to give the lectures, and that takes all of his time to do that. There also had to be laboratory experiments to go with them, and the new material was sufficiently different to call for quite different experiments in the freshman lab. Dr. [H. Victor] Neher, who is now retired, really was in charge of the laboratory part. But I was the coordinator.

The lectures were taped; Feynman used one of those cordless lapel microphones, and we hired a young lady to transcribe them. She was just as happy as could be, listening to that material and typing it. She did a fine job. But about six or eight lectures went by and nothing usable came out the other end. The transcript was verbatim, and in this case verbatim is bad—because Feynman never says anything once, he says it at least two-and-a-half, if not three-and-a-half or four times. He puts it a different way each time. Then he'll go on to the next topic for another couple of minutes, and he'll still be thinking about whether he could have explained his earlier topic better, and then he comes back. The results were loosely organized, modestly disorganized. I wound up, myself, personally, doing the editing for the first volume. It was a fulltime job; you couldn't present the material successfully without paying very careful attention to it.

There's one particular passage, which I'm sure I could find if I could look in the Feynman book. I'd like you to see what form it was in when it first came out of Feynman. [Laughter] It had to do with physics before Newton and physics after Newton. Feynman's point was that, before, the world was just a tremendous confusion of darkness and superstition. And afterwards, it was all light and structured and understandable. It was absolutely true. But he was trying to say this in a way that never did quite gel. He had a sentence in there that never had a verb in it. [Laughter]

ASPATURIAN: How well did you know Feynman when you started?

LEIGHTON: Oh, about as well as I know him today. I guess he and I share a certain aspect of social ineptness. I can't remember people's names unless I study them very carefully and for quite a long time. If I want to catalogue somebody's name in my head so I can get it again, I have to do it right then. But the trouble is, I'm introduced to somebody just in the middle of a conversation, and the conversation goes on; and who he or she is just drops out of my mind. It's one of those handicaps; Feynman has it, too. He roomed for at least a term at MIT, I believe, with somebody who was later at Caltech, and he couldn't remember his name. [Laughter]

ASPATURIAN: What was it like working with him on this?

LEIGHTON: What initially came out in the transcript was absolutely raw "Feynmanese" that had to be rough-edited right on the original sheets. After I got his material from each lecture into a form that I thought was ready for typing on the master sheet, it was sent back to the young lady and rendered into a form where it could be shown to Feynman. He would look at the thing now and then, but usually had no comment. That is to say, he was sufficiently satisfied with it. Another thing is that the lecture came at eleven o'clock, followed by lunch. We would walk to lunch together, and when he was dissatisfied about the way something or other was worked out, there would be questions or comments about—"What could we do to do it better?" There would be ideas and we'd talk. There'd be other people at the lecture—professors and TA's—and there would be sort of a floating lunchtime, which was partly devoted to just talking about the lecture. It was not structured consciously that way, but it just was an opportunity to get some ideas.

ASPATURIAN: Was this originally designed mostly for the benefit of Caltech students?

LEIGHTON: Oh, yes.

ASPATURIAN: But then it sort of spread out, didn't it?

LEIGHTON: Well, no physics instructor who was teaching freshman physics could resist having a copy of the Feynman lectures, whether or not he was using it in his class. This project was financed by a Ford grant, and I don't know what the royalty figure has come up to. It was an arrangement where the Institute agreed to put any royalties that the texts might accrue into support for similar kinds of activities at Caltech. None of the royalties went to any of the people involved with the lectures themselves. These were academic assignments, so the project was not treated as a copyrighted manuscript. It was just as well. At the time, Feynman said, "We will know whether it sells very well by seeing how big our salaries are in the next four or five years." [Laughter] And he was right. Our salaries went way up, his, for obvious reasons; and a lot of the rest of us because of being nearby, I guess.

ASPATURIAN: And your son got involved in doing something similar.[1] How did that happen? Has this become a family privilege?

LEIGHTON: It must be about the same time span. In 1963, Feynman did the freshman lectures [Volume I of the Feynman *Lectures on Physics*]. In 1964, he did the electricity lectures [Volume II]. And in 1965, he did the quantum mechanics lectures [Volume III]. And I can't quite remember the order in which things happened, but my wife and I would have dinner parties, and Feynman must have come to one or more of them. My son, Ralph, was at that time in high school and interested in drumming, and he was friendly with a very musical family in which there were lots of kids and parents who played various instruments. That would bring another group of visitors to our house. On one of these occasions, Feynman heard Ralph and his friends drumming at the other end of the house and, of course, he went in—he was more comfortable with kids anyway. He introduced himself and they invited him in to drum. And that led to rather regular drumming by Feynman, my son, and a couple of other drop-in friends. I myself was curious about Feynman's drumming ability, so I asked Ralph one time, "Well, how good a drummer is Feynman?" He said, "Well, he picks up the rhythms all right, and he's very fast but sometimes he has a hard time getting started. But for an old guy, he's pretty good." [Laughter] I informed Ralph that he had just spoken of the capabilities of possibly the one person in the world who knew more about how everything in the universe worked than anyone else on Earth at that moment. [Laughter]

Anyway, Ralph's other musical friends gradually went off to college here and there, but Feynman and Ralph continued drumming together. The Feynmans would invite Ralph over to their home in Altadena quite often. He was teaching in Pasadena schools at the time. Sometime

[1] Ralph Leighton is the co-author with Feynman of two collections of Feynman's reminiscences: *Surely You're Joking, Mr. Feynman: Adventures of a Curious Character*, (Norton, 1985), and *What Do You Care What Other People Think?: Further Adventures of a Curious Character*, (Norton, 1988).

during this period the Feynmans bought a beach house in Baja, and they would drum there, too. If you were around Feynman enough, you'd hear these amazing stories in some random order. Undoubtedly they gain with the telling, but they're all quite real. There's an infinite cauldron, out of which he would dig one of them up on occasion. That is to say, something in the conversation would recall such-and-such. If you happened to have been near him during some similar conversation, you might have heard the same story—but you didn't mind hearing it again! They're illustrative of various principles of social interaction and various things. [Laughter] Particularly the Los Alamos experience—Feynman as a kid interacting with generals. And Feynman, he can go on forever; one thing reminds him of another. It's amazing. The man is absolutely incredible.

ASPATURIAN: So, there's an inexhaustible store of lore here.

LEIGHTON: Or, as some people would say, inexcusable.

On the drives back and forth from Baja, Ralph made tapes of these stories. Then he transcribed them, first on a typewriter, and then on my computer. Feynman was in favor of this; it was not surreptitious at all. It was simply Ralph saying, "These stories are so great, but they're like gems slipping through my fingers—can I tape them?"

Then at some stage, I said to Ralph, "How about running the transcripts by me? I'd just like to refresh my memory." So I read most of them. Now and then I would see some word that was misunderstood.

ASPATURIAN: You were familiar with most of them?

LEIGHTON: Oh, yes. Only about 20 percent were new to me. I think Ralph and I, without ever discussing it, on our very different projects, realized the same thing about Dick: namely, you should do a minimum of editing on what he says. You should leave it as close to the original as possible, including the mannerisms, although not the repetition. In the physics lectures, I found it absolutely essential to crunch the repetitive material down into what might be a good way to put it, and then let it go at that. But Ralph has a lot of talent along those lines. However, that particular job was the first time he had ever tried to write something for publication, so he got some valuable lessons on editing from Ed Hutchings.

ASPATURIAN: Is there a sequel planned?

LEIGHTON: Well, there are still more stories. And then there's also QED[2] which has come out and has gotten pretty good reviews. And I guess Ralph is still running the tape recorder.

ASPATURIAN: There were a few things in that book [i.e., *Surely You're Joking, Mr. Feynman*] that I found did not reflect terribly favorably on Feynman. Was there any discussion about getting rid of some of those?

LEIGHTON: No. That's the man.

[2]*QED: The Strange Theory of Light and Matter* by Richard Feynman, (Norton, 1986)

ROBERT B. LEIGHTON

SESSION 7

February 12, 1987

ASPATURIAN: After your work on Mariner, you went on to still another project—instrumentation for millimeter and submillimeter astronomy. How did that come about?

LEIGHTON: You have to remember that I participated in the infrared sky survey, but for one reason or another, maybe being involved with Mariner, I didn't participate in established observing programs where you take some nights at the telescope and go and measure this and that star. That didn't interest me. I did make a machine to look for polarized stars or nebulae. But it turned out, after a week or two in the shop, I figured out that my way wasn't the way to do it. Then I saw a very nice device at Mauna Kea—the University of Hawaii telescope there—which showed me how it should be done. But by that time—this was about 1965—I was no longer interested in it. I did become interested, though, in building a new dish for infrared observations that would be twice the size of the original. It was basically a question of how much epoxy had to be mixed up in a short time, and how uniformly it had to be mixed. It's a little bit like pulling taffy, so at those dimensions it just fell down. It was a moderately potentially useful thing, but in the meantime, we decided that that was not what we wanted to do. For one thing, Neugebauer and his group were having access to the 200-hundred inch, which was so much bigger and better that it sort of took the pressure off making our own device.

However, in the process of thinking about this two-times-larger dish, we found the way to make a proper support structure—the tubular or other kinds of members on the back surface of the dish that hold the surface in the proper shape. We figured out a way to build posts and struts very easily in the shop, so that the process of putting the support structure together really became one of assembling pieces. It was a procedure of reducing the whole construction of the support structure to what you could call a one-dimensional problem: make struts and posts to a precisely set, precisely defined dimension, and you're on your way.

It turned out that while I was sitting at my desk in the division chairman's office, I had a little terminal hooked into the PDP-10 over in the computing center. And I was able to use that to design the basic structure for a bigger dish. We decided to see how big a dish we could think of making—not actually doing it, but devising the ways to do it and estimating how accurately we might do things. Once we had come up with a way of making the struts to the right length, taking into account as far as possible the stresses and deformations they would be subjected to, Jim Westphal, who's famous over in planetary science, said, "What you really need is a laser interferometer to measure the length of these things." And indeed, that's the secret beyond a certain point. If you want to go smaller than three thousandths of an inch, you just about have to have something that goes down to the wavelength of light. So we bought a laser interferometer and used it in the shop to build the struts to the right lengths, which were being calculated by a very simple, BASIC computer program. The idea was that if you had a whole lot of struts coming together at the bottom of, say, a post, those struts had to be lined up in such a way that if they were projected into the axis of the post, they would all meet at the same point. If you got right on line, the thing would have the stiffness of the original strut itself. In this way we had come up with a support structure that was very easy to build.

Now we had set ten meters as the right size for the dish, but we still had not solved the problem about how to make the surface. The surface was a factor of three bigger than the double-sized prototype we had made out of epoxy and thrown out. We made some experiments in the shop and found that making the surface out of aluminum honeycomb was clearly the way to do it. By then we had enough NASA money to build a prototype without having to convince the NSF that they should fund it. The ten-meter dish wouldn't exist today if we'd had to go to new sources of funds.

ASPATURIAN: Had millimeter and submillimeter astronomy started coming into vogue?

LEIGHTON: Oh, yes, people were thinking in terms of detectors that would do this.

ASPATURIAN: But people didn't realize at that time how ideally suited it was for studying certain things.

LEIGHTON: That's right. And then the airplane observatory—the Kuiper Observatory—was built for far-infrared observations. There were also people here who were using that, and thereby putting pressure on the 200-inch, which was the most accurate dish that could be used for submillimeter and millimeter observations.

ASPATURIAN: What was the rationale for going into submillimeter and millimeter? Just to look in a different wavelength?

LEIGHTON: Yes. Imagine that piano keys stand for the electromagnetic spectrum. We have one octave if we confine ourselves to the visual. You can imagine how dull Mozart would be if he had to stay in one octave. And there's something new in everything, you know.

So in the late sixties, in discussions with the radio astronomers, particularly with Al Moffet, we talked about making several dishes and making a radio interferometer for high frequency, short radio waves.

We were thinking in terms of one to five millimeters, and then the submillimeter came along as an idea. We thought that it might be worthwhile to go to a mountaintop with one of those telescopes, where you could get thin enough atmosphere to have a submillimeter window. However, we also thought the next time we make a dish, we'll improve it somehow. And so the dishes did improve somewhat as time went on. We learned more about them. They were certainly better built, if not more accurate. So we then pushed very hard on the Mauna Kea Observatory, and now we have the Caltech Submillimeter Observatory there.

It was like typical research: namely, you get a new idea, and you can't stand it until you've exploited the idea. Either it works and you love it and you do things with it, or else it doesn't work and you improve it, or forget you ever had it. Once you start on a thing like that, you don't know how long it's going to take before you're finished. If you did, you'd never start in the first place, in some cases. However, it was very straightforward to do the millimeter and submillimeter stuff.

ASPATURIAN: So basically, this was initiated as an effort to find out new things about interstellar chemistry.

LEIGHTON: That's right. And, of course, the people who build the detectors and the radiation receivers always are pushing their frequency range or whatever to new limits. Or else they run into the atmospheric wall that prevents them from doing that.

We began working on a prototype for the millimeter dishes now at Owens Valley [Radio Observatory] in about 1974 or '75. I was making sketches of possible things to do in about 1971, but we didn't actually start building things until about 1975. The prototype's now in pieces up in

Owens Valley. We used the panels for weather tests and various other things.

ASPATURIAN: And then did that become sort of your chief research project for the next several years?

LEIGHTON: Yes, for a while. I was division chairman at the same time.

Begin Tape 6, Side 2

LEIGHTON: There was an initial grant proposal to the NSF, which fell through, and there were problems initially getting funding. But we had a lot of good ideas, and we also had Colin Masson show up about that time as a postdoc. He was a genius on a certain aspect of what we needed, which is now called an acousto-optic spectrometer. As a matter of fact, Al Moffet had already figured out that we needed to build it. When Colin came we realized he had some mechanical and a lot of electrical and electronic capabilities. Our proposals were being written and being addressed to referees—our peers, of course—and they each objected one by one to something different, each within his own area of expertise in which he thought, "How do these guys think they're possibly going to be doing these things? Look, they're proposing to make an acousto-optical spectrometer. And they're assigning it to a young person on the staff, who's never even seen one in operation!" But Colin showed an interest in doing it. It was one of the problems that was standing out there, and he got some ideas. He made error analyses and performance analyses long before there was hardware. And he knew where all the main sticky points were, and he had sense enough to buy a great big, heavy optical table. The person who had made the only successful one heretofore was in a picture with it on the cover of *Physics Today* once, and I could see right away why it would have problems. He had it mounted on a very flimsy piece of steel. So we made it with much more stiffness, and it worked like a charm.

The referees also didn't like the fact that we proposed some very mundane receivers. They said, "How come they proposed these things that anybody can do? Why fund them? We can fund other people." Well, we knew that receivers were being improved very fast; we knew that we could do receivers when they came along. And so, one by one, the answers came to these objections. And so we finally did it. We got the funding in 1978 or '79, but with a very tight leash.

Neugebauer, Moffet, and I were the principal investigators. Then when we brought in Tom Phillips from Bell Labs to build the receivers, he became one of the PI's. We had tried to get him before because of his expertise on building receivers, but he didn't want to do it. But he did come to use the radio telescopes at OVRO [Owens Valley Radio Observatory], and he eventually became the leader.

ASPATURIAN: Did you build all three of the OVRO millimeter dishes?

LEIGHTON: I seem to remember we built the prototype; and then we proposed building four dishes. The NSF proposal went through for three dishes, and we had NASA funding for the fourth dish, which would go on Mauna Kea. When we got to the point of building these struts to precise lengths, we saw to it that dishes one and two got done together. By building the struts for two dishes at once a lot of setting-up time was saved. We did dishes three and four together also.

ASPATURIAN: Aside from the technology, were you particularly interested in much of the actual research at these wavelengths?

LEIGHTON: I wish I'd done more with the center of the galaxy. I think I became the division chairman just about the time we were doing that stuff, and I just didn't want to get entangled, spread even thinner.

ASPATURIAN: Was there anything you found rewarding about being division chairman?

LEIGHTON: Not that would make it an intrinsically desirable thing to do. The most significant thing I did as division chairman was to use the computer behind my desk to calculate the properties of our ten-meter dishes.

ASPATURIAN: Did you have a chance to start up any new programs, or bring in any really exciting people?

LEIGHTON: It was a time of fairly severe retrenchment. And you knew darn well you were going to have a hard time of it.

ASPATURIAN: The money dried up?

LEIGHTON: It was showing signs of it.

ASPATURIAN: Was it at that point that it became more of an imperative for people to start bringing in their own funding?

LEIGHTON: Physics was always on one end of the spectrum with respect to finding its funding. The Office of Naval Research [ONR], and some of the Department of Defense [DOD] and Atomic Energy Commission [AEC] departments, of course, supported research because there were still things being discovered—like quarks—which might be sources of incredible power. The physics people were born here, getting at least 50 percent of their funding from AEC or NSF. They've always been scrambling for whatever Institute funds there were, and for corresponding faculty slots and so forth. There's the "good" guys and the "bad" guys. I have a whole set of notes here having to do with teaching loads and what degree of janitorial support and secretarial support, and things like that, the various divisions enjoyed. And there were, and probably are, severe variations across the campus. Some people have to have their nirvana right in their laboratories, and other people are willing to travel a little ways. And the physicists have been pretty much scramblers all the time. It has gotten to be pretty tough, even in physics, because there's now so much competition for machine time. It used to be that we had an electron synchrotron here and we could get money to support it and so forth. Then pretty soon, the money was cut back and you couldn't afford to run the machine, but you still had the people, so they had to go to Chicago and Stanford.

ASPATURIAN: You've worked here under a number of divisional chairmen. Is there one you consider the best?

LEIGHTON: Bob Bacher probably broke ground. It was partly the times that called for innovation, with all the marvelous new things there were—radar techniques, electronic techniques, spectroscopy. There were many opportunities to do things. The war had essentially side-tracked people for a considerable time. A lot of people came back from wartime duty, especially people in the Navy and the Air Force, knowing an awful lot about electronics. And so, what an opportunity to apply it to physical research. Bacher came here just at that time. I guess Houston probably would have become the division chairman if he hadn't become president of Rice, and that would have been okay. But Bacher, I think, was, if anything, better timber for division chairman on the basis of having his own research interests in a modern physics field, namely spectroscopy. And also now, nuclear physics, by some osmosis, from the Los Alamos project. Bacher was, of course, at Los Alamos. He knew a lot of people. All told, he had the mandate, he had the wherewithal, he had the scientific sense, and he had the willingness to work hard to make Caltech into a new place. And that was really great. I think everybody appreciated that Bacher was a cracker-jack guy. There are remarks that Bacher had no input button, though. [Laughter]

ASPATURIAN: Which means he doesn't take advice?

LEIGHTON: Well, he doesn't hear you. He nods, and undoubtedly it's getting stored somewhere. He tells you reasons why you can't do it. But then three months later he's coming around and talking about how good an idea it would be to do that. On occasion you hear stories like this; but I have no first-hand examples.

ASPATURIAN: You have spent your entire academic and research life at Caltech, from undergraduate to emeritus.

LEIGHTON: That's the way it worked out. I've been to meetings; I've had research that had some interest to people elsewhere. But I like to combine different things. I did a lot of that. As it is, I don't know how significant the things are. Perhaps it's like a lot of bric-a-brac in a ceramic shop—a lot of pretty pieces but only one of each kind. [Laughter] I think the right word's eclectic—seeing opportunities and salvaging the best of it. But I had the freedom to do it without being looked down upon as that funny guy who looked at planets, or something.

ASPATURIAN: Do you think this would have been possible at another institution, what you did here?

LEIGHTON: I have no way to tell. I do think that publish or perish was more of an imperative elsewhere than it was here.

ASPATURIAN: That's interesting. Has that changed?

LEIGHTON: I think we've become more like the others, unfortunately. I think that to be a young experimentalist just coming on line, you might say, at Caltech or any good institution, is a terribly difficult position to be in. As a result of all my other interests, I've become lazy. I haven't published very much, except now and then a textbook.

ASPATURIAN: Did you do a lot of publishing when you were younger? Or once you got out of the whole cosmic ray area, did you kind of taper off simply because you had the opportunity to do all this other stuff?

LEIGHTON: I've been on a lot of papers with the infrared and the millimeter and submillimeter projects. I've latched on to a couple of things and pursued them, sort of sideways, extracting them from the pile of results that are coming in. And I think my work on the behavior of volatiles on Mars and the atmosphere—this business of the low atmospheric pressure and the fact that most of the atmosphere was lying on the ground in the wintertime—was a totally new idea. This came out of the Mariner experiments. To be there, able to see that and do it, and then to have a guy like Bruce Murray around, who'd done volatiles on the moon—we just naturally gravitated together and did a joint paper. There were only two authors on that. I like that much better than finding my name on a paper where I don't even really basically remember what the objective was.

ASPATURIAN: Do you have any sense of what you consider the most important or significant thing you did here?

LEIGHTON: Well, almost everything could have been done by somebody else. As a matter of fact, one of the nicest and one of the worst things about the solar results is that there was no technique, other than possibly the optical coating of surfaces to eliminate reflections—which was needed to show the magnetic fields and discover the solar oscillations—that went beyond the intrinsic capabilities of what had already been built at Mount Wilson by 1908.

ASPATURIAN: But no one realized that.

LEIGHTON: That's right. Originally, Mount Wilson was ahead of the world in solar astronomy. But the whole field gravitated to counting sunspots and keeping track of how they disperse and things like that. And the Greenwich Observatory, not to mention the Mount Wilson Observatory, essentially got stuck at that level, of studying sunspots. Ike Bowen, when he was the head of the Mount Wilson and Palomar Observatories, said that the one thing that he saw which was just like day and night with respect to astronomers versus physicists, was that physicists used apparatus and did real experiments—in the sense of designing an experiment, taking data, and so forth—whereas astronomers wanted to know what spectroscopes were available, already built by somebody, that they could use to study the spectrum of such-and-such kind of binary stars. Not that those types aren't also needed, but they're just different.

ASPATURIAN: Looking back, do you have anything you want to say regarding your past forty years at Caltech?

LEIGHTON: It can't be literally true, but I have the distinct feeling that when I first came to Caltech as a junior, I didn't change after that. I still have the feeling I'm the same person I was when I came here in 1939.

ASPATURIAN: In what sense?

LEIGHTON: In the sense of what I'm interested in, what I really find exciting in terms of subject matter, what I read. I do try to read Science and Reviews of Modern Physics, not that I can keep up with it, really. The things that are going on in elementary particle physics are things that I really wish I'd done more of, except that the circumstances were such that I just didn't want to lead that kind of life, having to travel for a week or two at a time to some remote place, and then having to do double teaching when I got back. It was just too much of an upset of an orderly life.

ASPATURIAN: Was there a point where you realized you basically were happy to just stay here, that this was your preferred environment?

LEIGHTON: In the abstract, I guess I realized that it was not necessarily so, that I would always be here. And as a matter of fact, I got some job offers from what became aerospace industries. But when it actually came down to leaving, well, I was perfectly happy to do what seemed to be the next thing to do here.

ASPATURIAN: Comparing the way Caltech is now to how it was when you first came here, I imagine it's become more bureaucratic as it's grown.

LEIGHTON: I think that's the way of saying it. Before I became division chairman, I actually started at one time to find out what research was going on at Caltech. My plan was to walk into some building on the campus until I found an open laboratory door, introduce myself, if that was necessary, and ask if it was convenient to ask what was going on in the lab. It wasn't at all that I had the idea that some day I might be provost. I was just genuinely interested. I did that for about four weeks. One day I was going through what's now the Thomas Engineering Building. That was where the seismology—the earthquake engineering—people were, George Housner and Don Hudson, and other, younger, people. And I got a marvelous introduction to what they were doing. Unfortunately, I bogged down, and it never quite picked up again. But the reason I mentioned it was that the very idea that I could even manage to think about doing that in a finite time would be incomprehensible these days. There are just so many more people doing so many more things in so many more labs. It used to be that the way to meet people was to be on the Admissions Committee or on the Upperclass Admissions Committee or on the By-laws Committee or the Honors and Standards Committee, where you all got sort of mixed together. Pretty soon, the people you knew were not only your cohorts that moved right around you, but people across the campus. You could find somebody sitting next to you at the Athenaeum and not wonder, "Well, who are you?" and be reminded that you'd been introduced five times already, which in my case might be literally true! [Laughter]

ASPATURIAN: Is there anything you're working on now?

LEIGHTON: Well, there's one more dish in the works that we haven't yet got the full funding for, but it goes with the struts that are in the lab.

ASPATURIAN: If OVRO gets the funding for the other three dishes, are you going to build those?

LEIGHTON: There is a proposal for that. However, so far unproposed but prepared for, is to make a replacement dish for Mauna Kea. I know that if we were to make another dish like the one we have now on Mauna Kea, but with three support points for each of the 84 hexagonal panels, we would improve the surface precision by a factor of two, maybe three. Even a factor of two would make surface accuracy to five microns; and that might permit much more meaningful measurements in the thirty-micron window of the submillimeter range. So there's another window that would open up for ground-based observing. As a matter of fact, I wanted to build that dish. I've got ideas that go beyond what we're doing now in the submillimeter.

In this connection, I remember a story about my father. Although he and my mother were separated when I was growing up, and he was in the East most of those years, every now and then he would show up unexpectedly and spend part of the day with us. He would spend his time telling me how accurate his die-work was, and how he'd made this four-inch-in-diameter surface smooth to three ten-thousandths of an inch. And then he'd raise an eyebrow as if I was supposed to say, "Oh, boy, that's great!" I didn't know what he was talking about. But the funny thing is that his son has perfected a system for making a radio dish surface, not a mere four inches or so in diameter only, where you have control of everything, but on this big, strange four *hundred*-inch-diameter structure, which floats delicately on a thousandth of an inch air film, and which flops around a little bit. That surface is good to maybe one or two *ten-thousandths* of an inch! So it's rather interesting. Without any instruction from him, I must have had it in my genes. He, no doubt, endowed me with the right DNA to have the interest. It's all part of a pattern. I've always been enamored of mechanical things like that.