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.

Other Octaves

Oral History — Robert B. Leighton

Bob Leighton spent more than half a century at Caltech before his death in 1997. His own story of his life was captured in a 1986–87 series of interviews by Heidi Aspaturian (now editor of Caltech News *and* On Campus*) for the Caltech Archives Oral History Project.*

Born in Detroit in 1919, Leighton came to Southern California as a young boy and later attended the John H. Francis Polytechnic High School in downtown Los Angeles, of which the late Caltech Nobel laureate Carl Anderson was also a graduate. He attended Los Angeles City College for two years, and when he transferred to Caltech as a junior in 1939, he realized that he "was 'home' intellectually." He never left, although he recounts in his oral history that after earning his PhD he was briefly tempted by a job at Rice University; he checked out a book on Texas from the public library (another profound influence on his education) and decided that it was too humid in Houston and that he would prefer to stay in Southern California.

If his geographical life was not varied, his scientific life decidedly was; he describes it in his oral history as having been "divided into a number of reincarnations." The first was as a theoretical physicist: he wrote his 1947 PhD thesis under Paul Epstein, professor of theoretical physics, on the vibration of atoms in a cubic crystal, a tough mathematical problem that Einstein and Bohr had attacked. Leighton ended up building a model of it in the machine shop. His paper was published in Reviews of Modern Physics*, but, he says, "What I learned from that experience was that I was not a theoretical physicist." (He also passed the shop course.)*

In his early years at Caltech, Leighton designed this cosmic ray detector to be flown on a balloon.

In his second incarnation, as an experimental physicist, he worked with Carl Anderson (with whom he had built rocket launchers for the Navy during the war) on cosmic rays, plotting the decay of muons and tracking what are now called strange particles (then they were called hooks and forks). When the competition of bigger and more powerful accelerators appeared to make the necessity of pulling the particles out of the atmosphere obsolete, Leighton found something else to excite his interest.

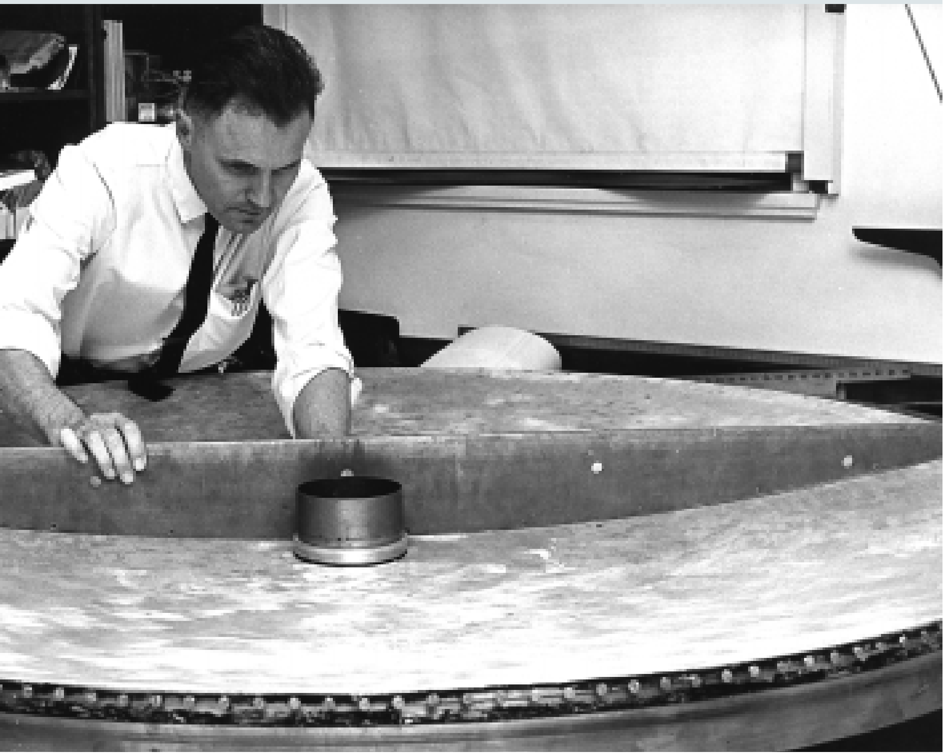
Although he doesn't say much about it in his oral history, Leighton was renowned as a teacher. He wrote an influential and best-selling textbook in 1959, Principles of Modern Physics*, and edited* The Feynman Lectures on Physics*, the famous three red books, into printed form. He was chairman of the Division of Physics, Mathematics and Astronomy from 1970 to 1975 and was named the Valentine Professor of Physics in 1984.*

Yet, it is as an astronomer and inventor of telescopes that Leighton is perhaps best known. He was "present at the creation" of all the major directions in astronomy that took off in the latter half of the 20th century—solar, infrared, and millimeter- and submillimeter-wave astronomy, not to mention the exploration of our own planetary system. So it is this segment of Leighton's Oral History, this final reincarnation, that we publish here.

Solar Astronomy

Heidi Aspaturian: I'd like to ask you about your research in solar astronomy, which seems to have started in the mid-'50s while you were still involved in the cosmic-ray research.

Robert 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



In the early '60s, Leighton sculpts the surface of the first infrared telescope, which is now in the Smithsonian Museum. At 62 inches, it was once the second largest telescope on Mount Wilson.

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 10 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 20 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.

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.

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

RL: Not at all. As a matter of fact, Bob Bacher, who was then the physics, mathematics and astronomy 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. 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.

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 gray. 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. "Well, I told those guys," was one of his favorite phrases.

And the funny thing is, almost all the procedures that we used to do the job were absolutely available to George Ellery Hale perhaps 20 or 30 years earlier!

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 gray 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 are also emitted. This was evidence that some high-energy particles were being generated somehow—that nuclear reactions are 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, 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.

[*This cancellation approach, which Leighton worked on between 1957 and 1959, led to a "much better photographic resolution of the sun in terms of kilometers." The project "really took off," leading to Leighton's discovery of a five-minute oscillation in the solar atmosphere and of "supergranulation," caused by convection currents in cells of material on the sun's surface.*]

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

RL: 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 10 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

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.

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 20 or 30 years earlier!

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

RL: 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.

[*Leighton's work led to Caltech's entry into the field of solar astronomy, the arrival in 1963 of Hal Zirin, now professor of astrophysics, emeritus, and the establishment of Big Bear Solar Observatory.*]

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

RL: Well, I wasn't afraid to. 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 photosensitive diodes were 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 photodiodes 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. . . . 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.

Infrared Astronomy

RL: I think it was in 1961 or '62 that Gerry Neugebauer [*now the Millikan Professor of Physics*] and I got interested in building an infrared telescope.

HA: Was Neugebauer a student here at that time?

RL: 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. We in the physics department were fishing to get him back down on campus. 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 it. . . .

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 looked very carefully at some 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-case epoxy parabolic reflectors. Gerard Kuiper's group in Arizona had made one or two pretty good spin-case mirrors, which were stated to resolve to five arc seconds or so. Kuiper had literally gold-plated the reflecting surfaces. But he didn't go much further than that.

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 an 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. 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 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.

Infrared astronomy was growing by leaps and bounds all through this period.

We just happened to be there first.

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.

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

RL: 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.

HA: What were you looking at?

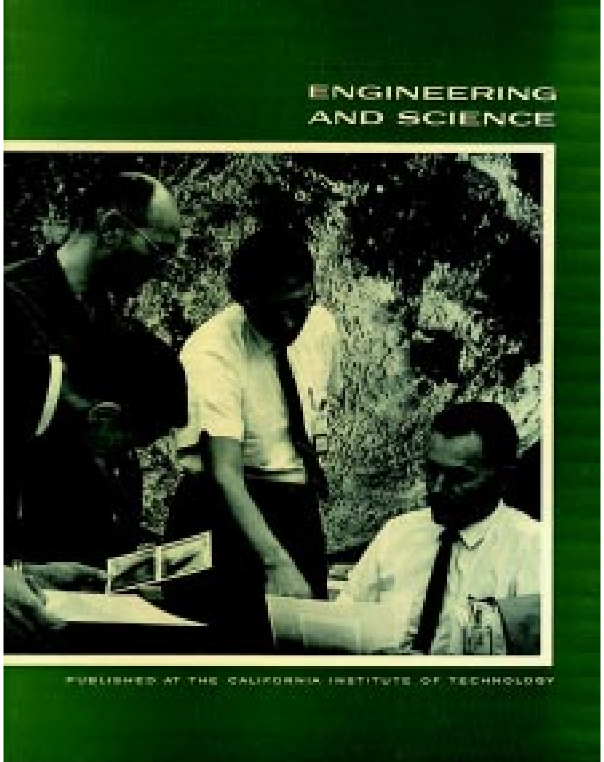
RL: Beta Pegasi was the first infrared, very bright red, cool star that we found. The fact that we had found one meant that the survey was worthwhile, because we could only improve from that point. . . .

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 that 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 most 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 infrared one came, or, as I believe, we were going in the direction where the red signal would come after the infrared signal.

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

RL: 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.

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.



Above: Picture number 11, snapped by Mariner IV from a distance of 7,800 above Mars, was definitive evidence of craters on the planet.
Top: Leighton (lower right corner), principal investigator for the Mariner IV television experiment, studies the first pictures from Mars with other Mariner scientists. (The heavily cratered body in the background is the moon, not Mars.)

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

RL: 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.

HA: So did you end up doing it yourself?

RL: 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. This was not true on the infrared telescope; that was built over in the central shop. . . .

Mariner Missions to Mars

HA: 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?

RL: 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 '60s or late '50s, 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. . . .

Neugebauer and Bruce Murray, who was fresh on the staff [*later to become director of JPL, and who is now professor of planetary science and geology*], brought me into it. Bruce was interested in planets as physical objects; he's 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 on that experiment. I went to a lot of engineering meetings. JPL did all the hands-on craftsmanship.

HA: That must have been a change for you.

RL: That's right. I didn't get near a bench. 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 relatively blank Martian surface. JPL was going to use about three bits, but we absolutely insisted on there being, I think it was, eight bits. 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.

HA: 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?

RL: 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 20 pictures. That was it.

HA: Why only 20 pictures?

RL: 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 the 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 a different plane.

There were two more Mariners [*VI and VII*] that I was closely associated with. Then I had sort of a peripheral role on the Viking Lander and the photos that were taken. I got a lot of data; I got to see the pictures. But I was too busy being division chairman then to actually enjoy myself.

[*Besides craters, Mariner IV discovered that the density of the Martian atmosphere is only about 10 percent of what Earth-based observations had suggested. The swaths of Mars photographed by the three spacecraft Leighton was involved in all revealed astonishingly varied terrains.*]

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 was being seen for the first time. And indeed that proved to be the case. Eventually, many more distinctive things, like the big volcanoes and 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.

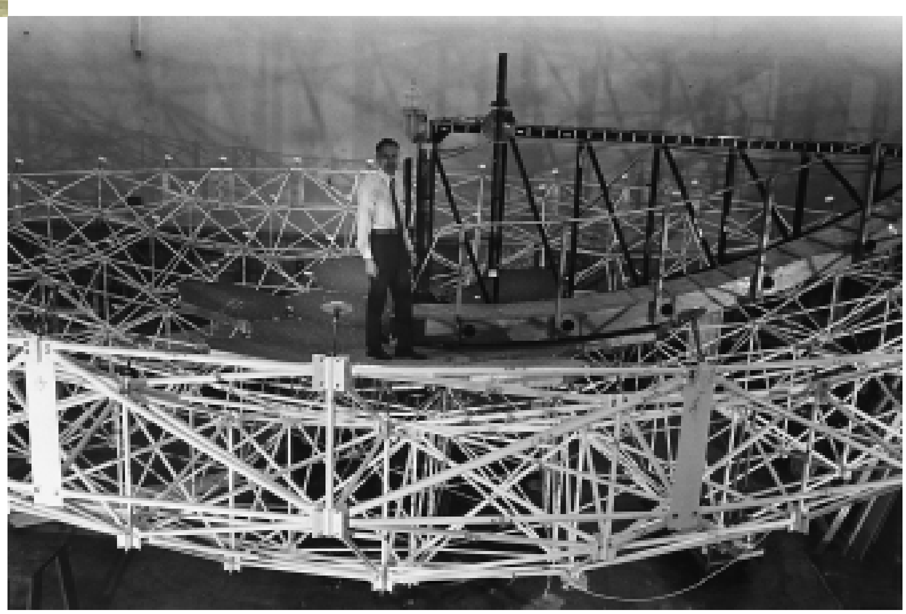
Millimeter and Submillimeter Astronomy

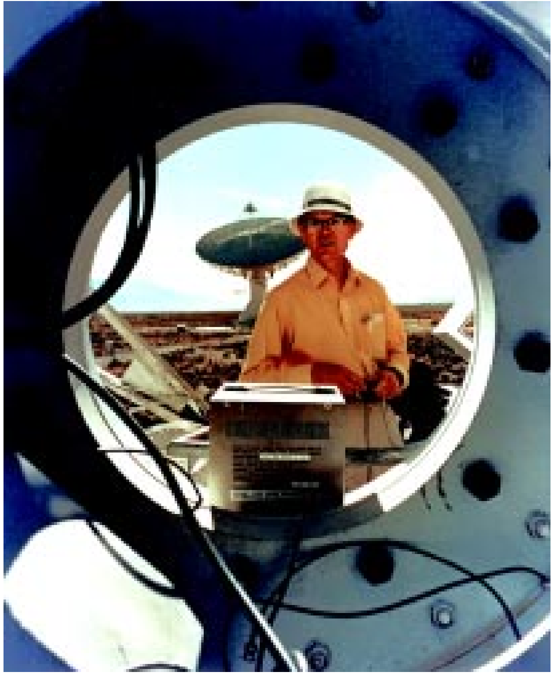
HA: After your work on Mariner, you went on to still another project—instrumentation for millimeter and submillimeter astronomy. How did that come about?

RL: 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 or 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—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 useful thing, but in the meantime, we decided that was not what we wanted to do. For one thing, Neugebauer and his group had access to the 200-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

Bottom: Leighton constructs the first 10-meter dish on campus in the early 70s. In 1998 there are six of these millimeter-wave telescopes linked together as an interferometer at Owens Valley Radio Observatory.





With OVRO's huge 40-meter telescope looming over his shoulder, Leighton looks through the elevation bearing of the first Leighton telescope during its construction in 1978.

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

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.

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 calculated by a very simple computer program. The idea was that if you had a whole lot of struts coming together at the bottom of, say, a post, these 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 10 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 10-meter dish wouldn't exist today if we'd had to go to new sources of funds.

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

RL: 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 '60s, 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: 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.

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

RL: That's right. And of course, the people who

Bottom: Leighton climbs up the struts of the Caltech Submillimeter Observatory (CSO) when it was first erected on the Caltech athletic field in 1983–84. It was disassembled and moved to the 13,300-foot level on Mauna Kea, where it was dedicated in 1986 (below).

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.

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

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

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

RL: 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 10-meter dishes. . . .

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

RL: That's the way it worked out. 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 a kind. I think the right word is eclectic—seeing opportunities and salvaging the best of them. But I had the freedom to do it without being looked down upon as that funny guy who looked at planets, or something.

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

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

HA: 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?

RL: 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 were 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.

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

RL: 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.

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.

HA: Looking back, do you have anything you want to say regarding your past 40 years at Caltech?

RL: 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—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.

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

RL: 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. . . .

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

RL: 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.

HA: If OVRO gets the funding for the other three dishes, are you going to build them? [*There are now six Leighton dishes at Owens Valley.*]

RL: 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 30-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-thousands 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-thousands* 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. □

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