



J. Beverley Oke, 1982

J. BEVERLEY OKE
(1928 – 2004)

INTERVIEWED BY
TIMOTHY D. MOY

September 10 and 11, 1991

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California



Preface to the Keck Series Interviews

The interview of J. Beverley Oke (1991) was done as part of a series of 7 oral histories conducted by the Caltech Archives between 1991 and 1992 to document the early history and development of the W. M. Keck Observatory at Mauna Kea, Hawaii. They capture the observations and perspectives of administrators, astronomers, designers, and managers representing both Caltech and the University of California, who would jointly manage the project.

Thanks to the support of Howard B. Keck, in 1985 the W. M. Keck Foundation donated \$70 million for what would become known as Keck I. Construction began in September 1985 to build a telescope equipped with a 10-meter mirror consisting of 36 hexagonal segments that would work together to form one single reflective surface. Using only 9 of the segments, first light occurred in November 1990. By 1991, a further Keck Foundation donation made it possible to begin construction of Keck II—also with a 10-meter segmented mirror—with first light occurring in October 1996.

Subject area

Physics, astronomy, Keck Observatory, Palomar Observatory

Abstract

An interview in two sessions, September 1991, with J. Beverley Oke, professor of astronomy, emeritus, in the Division of Physics, Mathematics, and Astronomy (PMA). BS and MS from University of Toronto. Graduate work at Princeton with Martin Schwarzschild on modeling giant stars. He describes continual development of improved detectors as staving off interest in building bigger telescopes until the late 1970s. Discusses his instrument building; mentions work of Arthur Code and James A. Westphal.

He discusses need for more light gathering and the various designs for a 10-meter telescope, including his own; rejection of E. Joseph Wampler's meniscus design. Recalls Caltech's plans to partner with other institutions and his support for collaborating with University of California; Jerry Nelson's design. Discusses UC's funding tribulations, Keck Foundation offer, Caltech's eventual equal partnership with UC, and UC's uneasiness.

Comments on debate over siting Keck headquarters. Nelson's work on segmented-mirror setup; detailed discussion of optical system; difficulties with Itek. Altitude problems at Keck site, including oxygen depletion and its effects. Dome design and his part in design of Nasmyth deck and elevator. Discussion of advantages of Keck Telescope over Palomar 200-inch for various astronomy projects. He concludes by describing his work with Jeremy Mould on Virgo Cluster and plans for building Keck II.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH J. BEVERLEY OKE

BY TIMOTHY D. MOY

PASADENA, CALIFORNIA

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courses, so that you give people a beginning. Occasionally, you get students from other fields, like mathematics and so forth, but not very often—they're usually physics students or astronomy students.

Anyway, I stayed at the University of Toronto and got my master's degree in just one year [1950]; by then, I was in the astronomy department. At that point, I went as a graduate student to Princeton. The choices were either Princeton or Caltech. And the reason I went to Princeton was that Martin Schwarzschild, who was a very dynamic astronomer, came and gave a series of lectures while I was a master's degree student at Toronto, and that sort of snared me. So I went to Princeton, where he was. [Laughter] At that time, I didn't really know anybody out here.

MOY: Did you work with Schwarzschild?

OKE: Yes. It was a very interesting time, because people were starting to get interested in how stars evolve and things like that. People really had never done this before. And one of the interesting problems which faced us at that time, and which I worked on under Schwarzschild, was to try and explain how giant stars worked. Everybody used models that were basically homogeneous—that is, with the same chemical composition throughout the star. And with just fifteen minutes of arithmetic, you can tell that if you try to apply such a model to a giant star that has a radius of, say, twenty or thirty or forty or fifty solar radii instead of one, then the temperatures are so low that you don't get any nuclear burning. So there was a real problem.

Nobody understood how to make giant stars at this point. There was one person who did. He was an astronomer named [E. J.] Opik, who was at Tartu University, in Russia. And Schwarzschild knew about the work he had done, but because of the war and so forth it took a long time for this stuff to get out and get to the point where anyone could see it. This person [Opik] had, in fact, argued that you must have a chemical-abundance change somewhere in the interior. So my job was to, first of all, dig out those papers and then start to make models of that sort. We in fact found that if you put in a chemical discontinuity, making the inside of the star out of helium [which we knew must happen, because you burn hydrogen into helium in the center] and the outside

predominantly hydrogen, we could make giant stars that were in proper equilibrium and were burning hydrogen by the proper processes. And in fact, what we were able to show was that as you increase the fraction of helium in the inside, the star models migrated gradually into the giant branch. These were almost the first evolutionary models of stars that had ever been made. That was just great exciting stuff to do, because it was the first glimmer of what stars must actually be doing. This [work] was followed up about a year later by Allan Sandage, who came as a postdoctoral fellow to Princeton. They [he and Schwarzschild] did the first real evolutionary models, where they allowed for the change in the size of the stars, which we hadn't done. And in fact, this led almost immediately to the first tracks, which indicated how a star got from the main sequence and turned off and then went up into the giant branch. Of course, everybody knows now what we were just discovering in 1952 and 1953. It was also exciting, because observationally this was a time when good photometry was beginning, with the use of the photomultiplier tubes. These were ultra-high-gain vacuum tubes, which were used during the war for jamming radar. But it turned out that they were also very good light detectors. People latched on to these right after the war and started building photometers, so that you had several-orders-of-magnitude higher sensitivity than had ever been achieved before. So you could make accurate measurements on faint stars. People then started looking at clusters of stars—the Hyades, the Pleiades, and so on. It was very interesting, because they would publish these so-called color-magnitude diagrams. You'd see a main sequence, and then you'd see stars turning off, and then you'd see a few giant stars. And the people who did this I don't think knew anything about what was going on. They didn't understand it, but at Princeton we did, because we had been making models that told us that this was, in fact, the way stars had to be evolving. And we could very easily calculate how old the Hyades and the Pleiades were just by using a slide rule and simple calculations. So it was fun being in a place where you had the jump on everybody else because something had been discovered there which was still not very widely known.

The other place, actually, where the same sort of information existed was here. There used to be a very close collaboration between Princeton and Caltech. As a matter of fact, as a graduate student at Princeton, I actually came out here and did my thesis, observing at Mt. Wilson on the 100-inch in the fall of 1952. And the people out here,

people like [Jesse L.] Greenstein, they were into all of this because of the close collaboration with Princeton. So there were two places where people really knew what was going on and the rest of the world certainly hadn't yet caught on. So it was just a very exciting time to be doing these kinds of things.

MOY: You got your PhD at Princeton?

OKE: Yes, that was in 1953.

MOY: When did you come to Caltech as a faculty member?

OKE: In 1958. In 1953, after I got my PhD, I went back to the University of Toronto, which was where I'd come from initially. I was there first as a lecturer and then as an assistant professor and worked mostly on Cepheid variables, measuring temperatures of stars and things like that. I was there until 1958, and then came out here and have been here ever since.

MOY: Just to back up a bit—your family background?

OKE: My father was a clergyman. In Canada, it's the United Church of Canada, which was actually a joining of the Presbyterian Church and the Methodist Church. So that's why we moved around quite a lot when I was growing up.

MOY: How did your interest in astronomy develop?

OKE: Mostly, I think, initially from my father. He was always interested in astronomy, so he had books on astronomy around. He had star charts, and he had a little telescope, and so forth. I can still remember him taking me out when I was seven or eight years old. It was a time when five planets were lined up in the sky. You could see all five of them in the western sky, which is pretty spectacular, and I still remember his taking me out and showing me that. He wasn't a scientist at all, but he was interested in things like that. He did a lot of studying, so he was an intellectual kind of person, and astronomy and science

was something he enjoyed reading about, and it sort of caught on in this way. I didn't decide that I would become an astronomer until I had finished my bachelor's degree in Toronto, and then I switched for the master's degree to astronomy. At that time, I in fact thought that I probably *shouldn't* go into astronomy, because it was all pretty well known. And how wrong can you be? [Laughter]

MOY: Since the middle 1970s, what were some of the other ideas for making telescopes larger than the Palomar Observatory?

OKE: Let me tell you what happened before. There wasn't much interest in building very big telescopes, and the reason, basically, was because detectors were improving. Up until 1950 or so—and for many purposes, the 1960s and even up to 1970—most observational astronomy was done using photographic plates. And a photographic plate has an efficiency which is a fraction of one percent. In other words, you lose most of the light. So what was happening in those years was to start using these new photomultiplier tubes, for example; these had what they call photocathodes, which are photoelectric devices. You suddenly had a quantum efficiency of ten or fifteen percent. So you had something like a factor of 10, 15, 30 or so improvement in light sensitivity over a photographic plate. And the other advantage that these particular kinds of detectors had was that they were linear devices. You didn't have to calibrate them as a function of brightness and things of this sort; you could just make measurements. It was relatively straightforward. I got very much involved in that. We built the first spectrographs, which were built with this kind of device. I built one of them while I was still at Toronto, around 1957; they were called spectrum scanners. Then when I moved out here in 1958, Arthur Code had built a similar kind of instrument. And they both had about the same kind of properties and performance. He actually left then; he went to the University of Wisconsin. So I inherited this instrument, which I then used quite a bit.

MOY: Did you mention that the basic technology for these detectors had been developed during the war?

OKE: This was developed largely during the war. Before the war, you had to use

phototubes, and the problem was that the currents were very, very small and very hard to measure. You had to use very high resistances to detect anything, and there was leakage. It was a difficult technique. A lot of it was pioneered here, some in Wisconsin. But it was very hard to do. And the photomultiplier tubes introduced a gain in the signal by about a factor of a million. Suddenly you had signals, which were relatively easy to cope with. It meant that these devices suddenly were practical things. And so they started working into astronomy about 1950 or so. And I guess Arthur Code here, and myself in Toronto, built the first spectrum-scanning instruments using this kind of technology. And we were able to do things that had been very difficult or impossible before.

Then, a little bit later, things developed. There were image intensifiers, which used the same basic photocathode materials that allowed you to do imaging. This meant you could actually take pictures with these devices. And, again, you were getting a gain in speed of a factor of 30 over what could be done before. I built a thing called a multichannel spectrometer, where we had thirty-two photomultiplier tubes, so we had thirty-two times the gain we had before. And in, I guess, late 1970, we in fact were able to demonstrate that we could measure redshifts for clusters much more distant than had been possible before. So we were able to really push back the frontiers.

MOY: When you say “tubes,” what were these?

OKE: These were actual glass tubes. Some of them looked like old radio tubes basically. Some of them were fancier than that. Some of them were cheap, some of them were expensive. But by using photocathode devices, image-intensifier tubes, or arrays of photomultipliers—you could also use some TV cameras—what you did basically was increase the efficiency with which you used light by about a factor of 50 to 100, total. Effectively, your 200-inch telescope was suddenly a hundred times bigger. So while all this process was developing, there was really no need to push for bigger telescopes, because you could get these big gains in the efficiency of the existing telescopes for an infinitesimal amount of money in comparison. So for a long time, then, the emphasis was on building instruments that were more and more efficient by using the existing light much more efficiently. And this carried right through into the late 1970s, actually. So at

that point, you didn't need bigger telescopes; you could just do all these new things because you had suddenly so much more power.

So people weren't really interested in bigger telescopes. It was in this era, for instance, that the 4-meter telescopes got built at Kitt Peak. They were copies of the 200-inch, basically, so they weren't trying to push back the frontiers in terms of telescope building. The only people who tried to do that at all were the Russians; they built a 6-meter telescope, which is a little bit bigger than the 200-inch, and it was not very successful. It's a bad site; it's a poor telescope, I gather. They really didn't have the technology in Russia to do the mirror properly.

Then there was one more revolution, and that was the introduction of CCDs—charge-coupled devices—which was about 1978. We actually did a joint CCD project with JPL [Jet Propulsion Laboratory]. And I think we were probably the first people who actually had a CCD on a spectrograph, or even to take any kind of pictures in astronomy. We did this in 1978. And then people like [James A.] Westphal [professor of planetary science; d. 2004] got into this with much better CCDs. So for the next seven or eight years, things could improve even more, because with CCDs you could go from fifteen- to twenty-percent quantum efficiency up to sixty percent; so there was still a factor of 3 that you could get pretty cheaply. People really worked hard on that, and that got us into the mid-1980s.

The problem at that point was that the quantum efficiencies were up to sixty percent or so, and there wasn't much more to be had. In principle, maybe you can get from sixty to seventy or eighty or ninety percent, but you're not going to get much more, because your efficiency's already up where you want it. So it was at that stage that people then started thinking, "We've got everything we can get the cheap way. Now the only way we can get more power is basically to go to bigger telescopes."

MOY: At that point, there didn't seem to be another leap on the short-term horizon for some new technology for detectors?

OKE: No, the CCDs were it. There wasn't really any sign of anything new, except in the infrared, and nothing more has actually appeared. The push that's going on today is to

cover more spectrum simultaneously and observe more than one object at a time. We built the double spectrograph, for example. In fact, the multichannel spectrometer was actually a double spectrograph, where we separated the light with the dichroic filter into a blue side and a red side, so we could actually observe a very large amount of spectrum simultaneously. And this gave us a factor of 2 or 3 gain that you otherwise wouldn't get. In 1980, roughly, we built the double spectrograph. It, again, split the light basically into two halves, and that used CCDs. And again, it allowed us to double what you could otherwise do. You didn't have to take a blue spectrum and then a red spectrum; you just did it at the same time, and this gave you another factor of 2.

Then the next stage, which we started to pursue a little bit, and other people as well, was to try and use multiple slits, so that you could observe more than one object at a time. And with the double spectrograph, we got up to the point where we could do five or six objects, if they were nicely spaced. So this gave us another improvement in speed by a factor of a few.

And then the last step, which we have taken at Palomar, is going to the Norris spectrograph. What we've done there is go to a fiber technology, still using CCDs and all those sorts of things we already developed. But with fibers, you could in fact move the probes around, so that with that spectrograph we could do a hundred or more objects at a time. And this gives you another big factor. So the last stage, then, was to try to get more objects at the same time, which effectively gives you more speed and more power.

MOY: So the central parameter that much of this technology addresses is actually the time factor, right?

OKE: Oh, yes. A big telescope is basically defined by how much light you collect and how much time you can spend doing it. And there are limits. There are projects, which we've tried at Palomar, where we have spent, basically, a whole night observing one particular set of objects, for example, and still have not been able to get anywhere. Once you get up to that point with the telescope you have—if you put a whole night onto something and haven't gotten anything—it's obvious that you're going to put a whole week on it to really get something. And that gets prohibitive, because there just isn't that

much observing time. And we've basically been pushing up to that limit with the 200-inch for quite a few years now. Again, the drive now is that the only way you can hope to go further is by increasing the telescope size and getting more light and then using instruments that are basically comparable to instruments we're using now.

MOY: So in the late seventies and into the early eighties, astronomers started feeling the need for more light-gathering power.

OKE: Yes. Everything else, all the other tricks, had already been eaten up and used. Clearly, we were solving problems all the time—I mean, you were getting farther out into the universe, you were looking at fainter stars, you were understanding stars better. So a lot of progress was being made. But it was clear that you needed to strive further still, because there were many problems that in fact were not answered.

MOY: What are some of the problems of trying to make telescopes larger than Palomar?

OKE: One of the problems is how to make a mirror that's bigger and/or at least the equivalent of that. There are lots of technical things involved in that. The other is simply the problem of cost. A telescope like the 5-meter telescope at Palomar, for instance, if you took it and tried to build a duplicate now, would cost probably at least \$100 million. And that kind of money basically just isn't there. So you have to face the cost. The way, for instance, that Keck and most other places have solved part of that is, in fact, to change the design of the telescope. And if you look at the Keck 10-meter telescope, it, in fact, is a much lighter-weight telescope. The Keck 10-meter telescope has four times the collecting area that the 5-meter Hale Telescope at Palomar has, and weighs about the same amount. And it's in the same-size dome and everything. So what you can do is simplify and do a cost-cutting design; make the telescopes shorter but bigger. Then you can make the dome smaller, so that the Keck 10-meter telescope basically has the same-size dome as the 200-inch. And this allows you now to build a 10-meter telescope for basically the same price that the original 200-inch telescope was built for. You can get four times as much telescope for the same amount of money.

Seven or eight years ago, I had concluded that there would be no way that a

telescope bigger than the 200-inch would ever be built with private money. It looked as if the costs were just too big for private foundations. It's just incredible, for instance, that the Keck Telescope has happened. And in fact, most of the other telescopes are government-supported ones. So it's been very impressive—to me, at least—that it is still possible to do what they did with the 200-inch. You can do it one more time, with donations from private individuals, and it's just great that that can happen.

MOY: When did you first hear about the segmented-mirror design?

OKE: I'm not sure I even know the answer to that. [Laughter] We got interested at one point, and I don't even remember the year. We knew that people were talking about making bigger telescopes, and we were interested in bigger telescopes. I had done—and have in my files still—some sketches of what a big telescope would look like. I had some pretty wild ideas at one point. [Laughter]

MOY: Like what?

OKE: One of the ones I thought about doing was building, basically, a Schmidt telescope that was 10 meters in diameter.

MOY: What is unique about the Schmidt design?

OKE: The neat thing about the Schmidt design is that the mirror is spherical in shape. And it also has a big corrector plate, so you can close the telescope, because you have a window at the top. And that seemed like a good idea. You could get a big field of view with relatively simple optics. The problem was that you couldn't make a piece of glass that big which you could support up there. I had worked on some things like that, plus some more conventional things.

Bruce Rule, who was for many years the chief engineer for Palomar, had also been looking at big telescope designs. And he had come up with some rather interesting designs. One, in particular, that was quite impressive to me was where the telescope was not only the telescope but its own housing. When you finished the night, it sort of folded

down and disappeared into itself, so that got around the problem of having a great big dome.

So we had been looking at possible big telescope designs, and then, at one point, and I don't even remember why, we started to become very serious about it. I presume somebody knows why we did, but I've forgotten now. Basically, we started discussions with various people to look at the possibility of a collaboration. Because of the cost, we figured it was going to have to be a collaboration.

MOY: With whom did you talk?

OKE: We had a long series of discussions with the University of Arizona, for example.

MOY: When you say "we," you mean the Caltech astronomers essentially?

OKE: Yes.

MOY: And not the administration.

OKE: No. This was the astronomers, but I guess with the blessings of the administration—I mean, they knew what we were doing. So we were talking with the University of Arizona. We had quite a number of meetings with them. We were discussing mirror design and things of this sort.

MOY: Their design was the spin-casting design?

OKE: I guess the spin-casting was just beginning, so it was conceptually a possible way of doing it.

MOY: Do you remember what year this was, roughly?

OKE: Not really. We were also talking with people at Santa Barbara Street—the Mt. Wilson Observatory. And again, we had quite a number of discussions with them. That

didn't seem to be going anywhere.

MOY: That's essentially the Carnegie [Observatories] people, right?

OKE: Carnegie people, yes. And roughly at the same time we were beginning to talk to the University of California people. At that time, in these early talks before we had any agreement with them, they were still talking in terms of two different kinds of telescope mirrors. One was the segmented mirror, which is what finally happened. They were also talking in terms of what nowadays is called a meniscus mirror—a rather thin, very large mirror. Most of that discussion seems to have happened basically within the University of California system. As far as I know, we didn't participate in that.

MOY: It was quite divisive, wasn't it?

OKE: Oh, yes, very much so. In fact, one of the chaps, who is a good friend of mine, left the University of California at Santa Cruz because of that; this was Joe [E. Joseph] Wampler. He had been pushing the solid [meniscus] design. People like Jerry Nelson [then a divisional fellow at Lawrence Berkeley Laboratory] were pushing the other. Wampler is now at the European Southern Observatory, which is building the meniscus mirror. Yes, that apparently was quite divisive. But I don't think we really played much of a role in that. We knew there were these arguments going on.

We didn't see anything happening as far as Carnegie was concerned, so that got dropped fairly quickly. The discussions with the University of Arizona really weren't going anywhere very fast, and their approach and our approach to astronomy and science were rather different.

MOY: In what way?

OKE: They're more the entrepreneur type. They wanted wild designs. Every time we would go into a meeting with them, there would be a whole new set of telescope designs, and whole new ways of doing things—interesting but not ever trying to really come down and pinpoint something particularly. So those discussions weren't going very well, and

we didn't seem to be getting anywhere with that. The arrangements with UC were getting rather more comfortable.

MOY: Who were the Caltech astronomers who would often be part of these meetings, besides yourself?

OKE: Most of the younger people were involved: Maarten Schmidt, Wallace Sargent. Greenstein, I think, was not really very much involved at this stage; basically, he had already retired. There were five or six of us. [Gerry] Neugebauer was interested, obviously, and the other infrared people to some extent. All the optical astronomers, by and large, were really very much into this.

MOY: Were Caltech people involved with providing technical advice on various aspects of the project before there were real talks on an official collaboration of some sort?

OKE: I don't remember that being the case. My feeling was to go the UC route, and part of the feeling was because UC astronomers do astronomy the way we do astronomy. I mean, they look at the scientific problems the same way, they look at accomplishing them the same way. And that has, in fact, continued. We think in a similar kind of way, so things work. At least I felt much more comfortable going the UC route, no matter how we built the mirror. It was more the personalities involved than it was the technology.

MOY: Who were the people you would usually talk to on the UC side about this?

OKE: Jerry Nelson, of course, was one of them in particular; Joe Miller, Bob [Robert P.] Kraft, and Sandy [Sandra] Faber were involved. They were the main people. They were UC Santa Cruz people, except for Jerry Nelson, who was at Berkeley. Marc Davis was involved in many of these discussions; he was at Berkeley. There were a couple of other people from the other UC campuses as well. There were a couple of people from San Diego who were also involved in this fairly heavily. It was mostly Santa Cruz, but other people represented the other campuses as well.

Begin Tape 1, Side 2

MOY: A lot of the interpersonal negotiations are not really well documented.

OKE: No, they don't get written down. And I find I forget a lot of the details. In fact, just talking to you now vaguely stirs in my memory a meeting we had here, where we basically decided which way we would go—basically between the UC people and the University of Arizona people. Gerry Neugebauer, I think, ran the meeting, but I don't really remember the meeting very much. I know I felt fairly strongly towards the UC thing, and I was fairly impressed that the half a dozen or so people here basically all felt pretty much the same way. I think it was essentially a unanimous decision, because we had talked with the Tucson people for quite a long time. And we'd talked with the UC people also for quite a long time. It got to the point where it became fairly clear that we knew which way we wanted to go. In fact, in this particular meeting, as I remember, everybody agreed that this is what we should do.

MOY: What was it that stimulated the Caltech astronomy community to get into this? Was it simply the knowledge that other institutions were looking at it?

OKE: [Laughter] I'm not sure I even know the answer to that. No, I think it was more than that. We already, of course, had the biggest telescope in the world, and we thought we were doing about as good a thing with it as anybody could. But I think we also realized from the kinds of problems we were working on at Palomar that we needed more "oomph," we needed more collecting area if we were going to really keep up—what people conceived, at least—our leadership role in astronomy.

There was this tradition that Caltech was always—at least, since the 1940s—ahead of things. When we were building instrumentation, we basically had the best spectrographs and the best technology available. By and large, we were on a par with anybody else, and in fact, very often we were way ahead of anybody else. And we maintained that all through the sixties and the seventies. We had instruments that were doing things that other people just couldn't compete with, partly because we had a bigger

telescope and partly because the instrumentation was somewhat better than what they had. With the CCD business, for instance—we had good CCDs running long before other people had them, partly because of our close association with JPL. But everybody realized that with the CCDs, we were finally coming to the end of that direction. So the feeling was that we really had to start thinking about what happens next.

MOY: At the time of this meeting, and the discussions within the Caltech community about wanting to get into this, was the University of California interested in a partner?

OKE: Yes, I think that's true.

MOY: Would it be fair to characterize this as Caltech getting in on essentially a University of California project?

OKE: I guess I would sort of class it that way; I'm not sure everybody would. As a project, they had done a lot of the spade work before we had come in, particularly people like Jerry Nelson, who had worked on the segmented-mirror design. So we really did join a project that was already under way. And I presume the UC system put a substantial amount of money into the thing, one way or another. They had already committed many millions of dollars, I think, at this stage, before we were involved at all.

MOY: After you decided to go the UC route rather than the University of Arizona route, what happened next? Was there some sort of more formal meeting with the UC people?

OKE: Again, I don't really remember. There were two levels of things going on. There was the level where the UC astronomers, which were mostly Santa Cruz people, and we were having discussions. Then there was a slightly different level, where Neugebauer, who was head of the observatories here, was talking with Kraft at that time, who was head of the UC astronomy effort. Then there was the upper level, which was the presidents talking to each other. And I don't really remember what all of those interactions were.

MOY: I've heard that one of the first meetings—and this is before the Keck money entered the picture—was a meeting between Caltech and UC astronomers in San Jose, at the airport.

OKE: Yes, there was such a meeting, I remember that.

MOY: Was that used to come to some kind of commitment, at least as far as the astronomers were concerned?

OKE: It's possible. I just don't remember. [Laughter] I do remember the meeting. It was a rather pleasant place to meet, actually. It was in the terminal. I remember the meeting, but I don't remember what happened. I presume that this is one of these meetings where the astronomers had basically agreed to go ahead. I think the astronomers took the lead in the sense of saying, "Yes, we want to do this; let's get together," and then asking our higher-ups to work out some of the details.

Roughly at the same time, we were also having some meetings with people like Sandy Faber, for instance, on the light-pollution problems. She was very much interested in the problems around San Jose, and we were interested in San Diego, and there were meetings going on, trying to present a united front on this. These were vaguely about the same time, so we had other connections with Santa Cruz as well.

MOY: Do you recall anything about the funding situation on the UC side of things? Did you know what proposals they had for raising the money to do this?

OKE: You mean, after this agreement?

MOY: Yes.

OKE: What was the agreement? Were we to raise fifty percent and they fifty percent?

MOY: I think, early on, there was a twenty-five/seventy-five arrangement—that Caltech would raise twenty-five percent.

OKE: Yes, I think you're right. We were going in for only twenty-five percent of the [observing] time. I think it was still open as to whether there might not be another partner. I mean, they still had seventy-five percent, which is a lot of money to raise. I think there was still the possibility that there might be a second partner who might be another twenty-five percent, and I don't remember anything more about it than that.

MOY: Do you know how UC went about trying to raise their part of the money?

OKE: Not really, no. There is, of course, the Hoffman thing. I presume there were upper-level people at UC making contacts with a variety of people, as far as seeing if they could find a sponsor for the whole thing. I think the plan was not to take it out of the UC budget but rather to raise money to build the telescope, and I suppose one of these contacts led to the Hoffman money.

MOY: And the same thing was happening on the Caltech side?

OKE: I presume we were talking to a variety of people, too.

MOY: Were you familiar or involved with any of that?

OKE: Not really, no.

MOY: [Caltech president] Murph [Marvin L.] Goldberger and [provost] Robbie [Rochus E.] Vogt were primarily doing that?

OKE: Presumably, yes. That was pretty high-level. We were worried about the real telescope, not how to get the money for it. [Laughter]

MOY: How did you find out, first of all, about the Keck money, or at least the amount of money?

OKE: I don't really remember many of the details. As I remember, it was sort of vague.

We didn't really know how much money it was.

MOY: Did you know *who* it was?

OKE: I don't even remember that. I think we did not know initially, until it was announced. I don't really remember that. These are important things to forget!

[Laughter]

I guess we knew early on that there was a problem, in that the amount of money coming from the donor was not enough to do the whole thing. So that was clearly going to be a problem, because we knew the telescope would cost more than the \$70 million that Keck was putting into it. I guess there was a while that we knew something, but it had not been announced.

MOY: And you say that at the beginning, you think that most of the astronomers did not know who the donor was, is that right?

OKE: I don't remember.

MOY: Do you recall what that did to the UC/Caltech funding situation initially?

OKE: There was a real worry, because it sort of undercut the Hoffman gift. The Hoffman gift, which I think was \$40 million, was vaguely sitting there, wasn't really solid; but we knew that that was on. And then suddenly there was this big amount of money [from the Keck Foundation] that we would have. I was certainly worried as to what would happen, because you now obviously had two gifts that were somehow mutually exclusive of each other. It wasn't clear to me, for instance, what UC would do about it. They could have said no, if they had wanted to.

MOY: What do you think would have happened if they had done that—if for whatever reason, they felt they could not return the Hoffman gift, and the Hoffmans and the Kecks just couldn't get together on having a Keck telescope?

OKE: Goodness knows! I guess it wasn't quite clear how definite the Hoffman money was, because the woman [Marion O. Hoffman] who was giving the gift in fact died before it was officially written in the will. So it depended on her heirs honoring what they knew she wanted, but not legally having to do so. There was a real question, for instance, as to whether that was real money or not, and I presume that the UC people saw it the same way. So they may not have been too unhappy to give up something which was maybe thirty-percent possible, or fifty-percent possible, for something that seemed to be quite definite. And I guess at some point, our part of it became fifty percent instead of twenty-five percent. I don't remember. I presume it was when this gift came through that that number suddenly changed. So there must have been a lot of discussions in the UC system. Suddenly, they were losing a quarter of the telescope they thought they had. On the other hand, it was now at the point where it would get built. So there must have been a lot of very interesting discussions going on as to how you should weigh these different things. And we probably didn't really know what would happen, how they would respond to all of this.

MOY: Presumably Goldberger and/or Vogt must have been in rather deep discussions with them about various possible ways of resolving it.

OKE: I suppose, yes. I was not really in on that.

MOY: Do you recall, at the time when UC and Caltech were trying to resolve this, that there was some talk of building two telescopes even then?

OKE: Yes, I think this was always something in mind.

MOY: Like having a Keck Telescope and a Hoffman Telescope?

OKE: I don't know if it ever was a combination like that. But now that you mention it, I suspect that was talked about at one point.

MOY: But you are not familiar with how that idea fell apart?

OKE: I suppose that all fell through because the Hoffman thing suddenly disappeared, so it was no longer a problem. And I suspect the Hoffman thing failed not because of us and Keck, or anything like that, but simply because it just fell on its face; although I don't know the details of that.

MOY: Were you involved in working out the final financial deal with UC?

OKE: No, not at all.

MOY: What about the formation of CARA [California Association for Research in Astronomy]?

OKE: Not really. I guess I was on committees and groups that were discussing all of this. CARA, I think, was largely done by the top-level people.

MOY: Do you know who?

OKE: Vogt, I presume. I presume Neugebauer was involved. I presume that Goldberger was involved to some extent. Then there were the president and vice-president of UC. I think Bob Kraft was involved as the counterpart of Gerry Neugebauer at that time. I think it was those people who formed this whole system.

MOY: Do you know how the site on Mauna Kea was selected?

OKE: I should. [Laughter] What were the alternatives? [Reflecting]

We had been talking to various people. For instance, we were talking to the University of Arizona people, and there were questions there as to where we would put the telescope if we built one. They were very strongly pushing a site in Arizona that to our minds was out of the question. They were pushing Mt. Graham at that time, which we thought was absolutely stupid; it's a lousy place to put a telescope. And at that time, I believe we were pushing Mauna Kea as a possible site. I certainly thought a bit about

where on the United States mainland you could build a telescope. Palomar was clearly out of the question, because it's too close to the cities. I actually looked a little bit into the possibility of a telescope up in the Big Bear area, south of that, because it was somewhat higher than Palomar, and it's another thirty miles or so farther away from the cities. I thought a bit in terms of Mexico. At one time, we actually had discussions about a telescope in Baja somewhere. I'm not sure whether we ever talked to the Mexicans officially about this at all, but it seemed to be, from the national standpoint, just out of the question.

Another site I thought about was one of the islands off the coast, south of Catalina. But it was owned by Mexico and not by the United States, and it wasn't high enough. But it was out where you would like to have a telescope, because there would be no city there, ever. I think we thought about other sites, out in the desert, for example. We even thought a bit about White Mountain, east of the Sierras. But that really wasn't a candidate, because it was pretty grim.

Then, the discussion with Carnegie was largely centered on putting the telescope on Las Campanas, and in fact there was a very good site. If we'd gone in with Carnegie, I think there'd be no question that the site would have been on the highest peak, which was reserved for a big telescope, so that was the obvious place to put that.

When we were talking with UC, the other place they were thinking about was a site considerably south of Santa Cruz, in the Coast Ranges; it was south of Salinas—sort of between Salinas and the coast [Junipero Serra Peak].

MOY: Are there other observatories there now?

OKE: No. But this is a site they had looked at. They realized that Mt. Hamilton [site of the Lick Observatory] is a poor site, because it's not quite high enough and it's a wee bit too far north. So what they wanted was a site farther south, to get somewhat better weather, and also a site that was higher and far away from the lights. So they had this site that was in a national forest, which they were pushing very hard. There were some problems with that—because of environmental things and so forth—which were going to cause difficulties: whether, for visual reasons, you could paint the dome white, for

example. Lick was pushing that site, I guess, and we were pushing Hawaii. And I'm not sure whether we were considering the Southern Hemisphere or not; I don't think so.

MOY: Did you say Lick was pushing their site?

OKE: The UC system, and primarily Lick. Some of the Lick people had spent a lot of time looking at the site and measuring the seeing, and so forth.

MOY: Who eventually chose the Mauna Kea site?

OKE: I don't really remember. I think those were the two sites that were in contention. And I think the Lick people realized that their site was probably not viable.

MOY: There must have been pressure, I presume, for the University of California to support an observatory in California rather than in Hawaii.

OKE: Yes, there certainly was that. There was the whole question, for instance, of whether the UC system would go in on a telescope that wasn't in California. Hawaii was an obvious problem, in that case. At some stage, it was determined that the UC system could in fact live with a telescope which was not necessarily in California.

MOY: Do you think it was ever a viable alternative for Caltech and UC to part ways on this, and for Caltech to build this telescope alone?

OKE: I would suppose, for instance, when the Hoffman money was there, if UC said no, we would in fact be sitting with basically Keck money. And Keck was really on the Caltech side of things. I think Keck would not have given the money to UC if Caltech had not been involved. So if UC at that point had said, "No, we are going to stick with the Hoffman money," then I presume the next thing would have been what you mentioned before—namely, "Let's talk about two telescopes." That would be the way around that problem. And I think, in fact, that must have happened, because there *was* talk about two telescopes. And I guess there was some discussion as to how you would

actually do that, and how you would fund the other part of the UC thing. And maybe at that point, UC could think about another institution involved in it.

If it had all fallen through completely— [Pause] I don't think this ever really came close to happening, primarily because UC at least realized that this was a way to go that was going to be successful, so it would have been crazy not to. But it was obviously hard for them. I would guess that if UC had pulled out completely, Caltech would have gone on alone. I could imagine, for instance, that we might have gone back to Carnegie and tried to make an arrangement for Las Campanas. So we were really pretty overjoyed when it all came together at UC. Meetings after that were just a little bit uncomfortable, because we realized we had sort of elbowed our way in, in a not-very-polite way. We elbowed our way into the quarter [share], which was fine, and then suddenly we were steering the show. So we felt—at least I certainly felt—a little uncomfortable that we had obviously not played unfairly but that circumstances had made it not so very nice for the other side.

MOY: Were you concerned at the time, socially, that this might be a serious disruption in a cooperative effort?

OKE: Yes, I was a little worried as to how our colleagues would look upon this. And there was obviously some friction. They were very polite about it. They really didn't say very much; they kept going and participating in a real way. But under the surface, there was— [Pause] I certainly felt uncomfortable about what was happening, and they were certainly uncomfortable.

J. BEVERLEY OKE**SESSION 2****September 11, 1991****Begin Tape 2, Side 1**

MOY: The last thing you said yesterday was a comment about some seeming discomfort when you first got together again with UC astronomers after the Keck deal had come through. Did you have any sense, at the time, about whom the UC astronomers were resentful of? Was it Caltech astronomers, or Caltech administration, or perhaps the Keck Foundation, or even UC's own administration?

OKE: I'm not sure that there was any real sort of *feeling* to speak of. It was just a bit of uneasiness. I know the UC people were somewhat unhappy with their administration because they would learn things that their administration was doing by talking to *us*. There were some funny things like that, where the top people would talk to our people, and we would learn what these discussions were, but they didn't often go down the UC pipeline in the proper way; so they came around through us. There used to be general comments that they learned more talking to us about what their administration was doing than they were learning [from the administration]. There was a little bit of friction, or lack of communication, within the UC system. And that's partly, I suppose, because it's so big. I think the UC people were a little unhappy to be upstaged, but they realized in fact that it was a good deal and something was really going to happen. Whereas if it had gone their way, things might have been much more uncertain. And I think it gradually disappeared. They were very polite about it. They never really said anything to anybody; they never accused us of doing things that were improper. So they were very good about it that way.

MOY: Do you have any recollection of what Howard Keck personally felt about the UC-Caltech partnership?

OKE: No. I've only met Keck maybe once or twice, and I've never really talked to him, other than to say "Good day." I really don't even know him and I have never heard him say much other than what he said in the press conferences. So I really do not know anything about his thoughts.

MOY: Do you recall some debate on the site of the headquarters, whether it should be in Waimea or Hilo?

OKE: Oh, yes, there was a lot of debate.

MOY: Were you familiar with that?

OKE: I was familiar with our side of it. [Laughter] There were two possible sites. One of them was Hilo, and the other one was Waimea. And they each had things to offer. Hilo had the advantage that the [Caltech sub] millimeter dish people were already installed, but only in a temporary way, in Hilo. But we would, in fact, have been given land there, or have it leased to us for a hundred years for a dollar, or something like that. As far as I know, it would have been next door to the UKIRT—the United Kingdom Infrared Telescope. They had their headquarters in Hilo. I've been at that headquarters, and it's a very nice place. And there are good people there; they're good astronomers to associate with. So there was that side of it.

For employees with children going to school, there is a part of the University of Hawaii, which is a little more than a junior college, in Hilo. And it does have a school associated with it. And that school apparently was fairly good. I'd met on one occasion the people who ran the university, and we discussed these things. So it looked as if the school system was perhaps fairly reasonable there. So it [Hilo] had a lot of attractions.

The main thing people didn't like particularly about Hilo is that it's very wet and humid; it's a tropical atmosphere, basically, and would not be that pleasant. But it had more facilities; there was a half-decent hardware store, for example. There's a big town of about 30,000 or 40,000 people. There were a lot of good eating places and an airport there.

Now, Waimea, on the other hand— [pause]—one of its attractions was clearly

that the climate is rather pleasant. It's drier; it's higher. It's rather like the climate here, in fact—perhaps even nicer than the climate here. The altitude is somewhat higher, so it's not tropical at all. It's a very small town, only a few thousand people, I think. It has a shopping center or two, but it's a real little town.

Access to the mountain was essentially similar—a fairly narrow, windy road until you got to the 11,000-foot level, and then it's the same on up. So that didn't make very much difference.

Another advantage of Waimea, which we haven't actually been able to make use of, would have been that we had hoped we could have a microwave link. We have a line of sight to the dome from Waimea that you don't have at Hilo.

From the standpoint of associations with astronomers—the Canada-France-Hawaii Telescope, the CFHT, has its headquarters in Waimea. In fact, at one point, there also was a large lot next to the CFHT organization which we could have had and probably even for free. That would have been rather nice, because it led to the possibility, for instance, of sharing a joint library. It had some nice advantages and also would connect with people we respected and wanted to work with. So there were many things, which were pretty equally balanced. Most of the astronomers liked the idea of Waimea much more than Hilo—mainly because of the climate. Of the astronomers who were involved in this project, I think ninety percent of them would have voted for Waimea.

MOY: You mean both Caltech and UC astronomers?

OKE: I'm thinking mostly of Caltech astronomers. I don't actually know what the UC people thought. I bet you they would have thought the same way, because they would have the same sort of feelings about climate that we had. Our administration—or Neugebauer in particular, and I guess Vogt—were rather strongly leaning towards Hilo.

MOY: Do you know why?

OKE: I don't really know why. We interpreted it as meaning that they thought we ought to be as uncomfortable as possible; that if you get too comfortable, you don't do science

as well, so you should be put in a bad climate. And I think, in fact, the UC system, as a whole, wanted to go to Waimea. Most of us wanted to go to Waimea, but our administration people were still pushing for Hilo. So it was still evenly balanced in this way. And we were sort of unhappy, because we weren't really getting any input; we weren't being consulted as to where we wanted to have it [the headquarters]. This stewed around for several months without a decision being made. And [Howard] Keck, for whatever reason, went over to Hawaii and talked to some of his friends there, and I guess got the commitment of the land in Waimea, and he basically said to put it in Waimea. That ended it. As far as I know, he was the one who made the decision. And he, in fact, arranged for the five acres or so of land that we have in Waimea. So we were all very happy.

MOY: Do you know from whom the land was acquired?

OKE: No, I don't.

MOY: What about the Mauna Kea site?

OKE: I don't know how that was chosen.

MOY: Or acquired?

OKE: Anybody can arrange to go to Hawaii. They get their pound of flesh out of it all. But if you do the right things by them, and if you want to go to Hawaii, you can arrange to do that. There were site tests. The feeling was that the CFH telescope and the Hawaiian telescope—they were up on the top ridge—were the best sites on the mountain. Where we were was a little bit lower, but it was a site big enough for a telescope of the sort we were thinking about. And we were also thinking, of course, of a second one. The second telescope never really disappeared from consideration, even though there were no plans to build it. So you needed a site where you could put two telescopes. And then there was the question of the building, and there were talks about that at some stage. But this site looked OK as far as we could see. The prevailing winds, the best we could tell,

were in fact favorable to it. It was high up and far enough from other domes and other potential domes so that wouldn't be a problem. We were pretty happy with it.

MOY: Let's talk about the design and construction of the telescope. Who would you say have been the principal designers of the telescope, besides Jerry Nelson?

OKE: The details I don't really know, early on. All the UC stuff was done before the money was ready to go. My understanding was that most of it had to do with how to do the mirror. First of all, there was the argument about what kind of mirror it would be. Once that decision had been made, then it had to be demonstrated that you could in fact have a multiple-mirror telescope and make it work. That was a pretty heretical idea; nobody knew how to do that. The MMT [Multiple Mirror Telescope] in Arizona was the only other telescope that had done anything remotely like that. And they had sort of succeeded, but rather painfully and not terribly well.

MOY: How does that work?

OKE: It's actually six mirrors. It differs from the Keck Telescope in that each of the six mirrors is, in fact, an actual telescope. It's an on-axis telescope with a symmetrical mirror, so there's nothing funny about it. They're basically just six telescopes stacked in a circle, and each of the telescopes is of a rather conventional design. Then, when they went up to the secondary and the tertiary mirrors, they steered those so that you took the images from these six telescopes—they were deflected into the middle, and the light was combined at that point. So it's a different concept entirely. But it has some of the same problems, in that you have to keep the mirrors adjusted to keep the image proper. And they had, at that time, fairly sophisticated ways of doing this. They had a very elaborate system, which never worked. They used a fallback system, which was rather makeshift initially but works reasonably well at the present time.

But they were not trying to do the same thing that Keck was. We had the whole mirrors within a tenth of a wave, basically. And that was something nobody had ever done. Jerry Nelson, with his group up in Berkeley, spent a lot of effort designing the hardware to make a multi-mirror telescope work. That was the main thing; nobody

worried about the rest of it at all, at that point. It was just a matter of building it. The rest of the telescope is a pretty conventional space-frame structure. You can design it with a computer; you can simulate what it will do under gravity, and so forth. All of that was considered pretty cut-and-dried. The real new thing was how to do the multi-mirror control system and actuators and all that. And Jerry Nelson's people were spending a lot of time on that. They made sensors, for instance, which were like the ones that were finally used; they tested those on benches to see whether they had the right sensitivity. They designed and built the actuators, and those were all pretty good. I think the final sensor arrangement is basically what they designed. They had to change the actuators slightly, because there was a bit of stiction; they had to improve the way they worked a bit. There was a lot of thought as to how you would do this, how you adjust these things, and how you get the mirror the right shape to begin with. They made a test mirror— basically a segment, a round one, in the right thickness. Then they built a support structure for the mirror, which had also to be done; they're very thin mirrors. It takes an elaborate support system to make even a single mirror work. So they, in fact, designed all of that and made all that work. They made a single segment and then sliced off a part of it and then mounted it to simulate having two mirrors side by side. And they demonstrated that you could, under gravity loads, hold those two mirrors in phase with each other. I was up at one of the meetings where they demonstrated this.

MOY: Is that at LBL [Lawrence Berkeley Laboratory]?

OKE: This is at LBL. So there was a lot of that kind of effort, and most of the time was being spent doing that. The telescope was clearly going to be an alt-azimuth telescope rather than a polar-axis-type telescope, because all telescopes that are very big are done that way. So people didn't really worry very much about what the telescope design was going to be. At some point, a decision had to be made, for instance, whether it would be a conventional telescope or a Ritchey-Chrétien telescope. This doesn't change the telescope design, but it does very slightly change what kind of mirrors you need.

Then there was a lot of discussion as to what the focal ratio should be. They wanted a short primary focal length, just to keep the telescope short so that the dome

could be kept reasonably small. Then you had to decide what the other foci—the Cassegrain—[would be]; we decided on $f/15$. There's a lot of debate, since this is where there's a lot of different feelings as to how you should do things. Other people, for instance, take $f/8$ or $f/9$ Cassegrain systems, for example. So there were decisions like that that had to be made. These were relatively easy to make. And as I remember, there wasn't that much discussion about all that.

MOY: Who made those decisions?

OKE: I don't remember actually. It may have mostly been done before CARA. Harland Epps [UCLA professor of astronomy] did the optical design. But I don't really remember when those decisions were made. They didn't seem to be very important decisions that took much effort.

MOY: Does your position on what optical system you want to have depend on what kind of astronomy you do?

OKE: It does a little. One of the nice things you aim at—and the Keck Telescope has done that—is that you know what the detectors are going to be, and you know that the pixel size is going to be 15 to 25 microns—all CCDs are basically built that way. And that's an easy tolerance to do with optics. You also know what sampling you want. You know that the images are going to be, say, one-half or two-thirds of a second of arc. You want to have more than one pixel per image; you want approximately three pixels per image. So one of the nice things you can do, for instance, is to make the prime focus in such a way that the image matches the CCD size. And that, in fact, is done at the Keck prime focus; when it gets activated, it will have an image scale that is very close to what you want. You can't be absolutely sure, because you never know how good the telescope is going to be; you have to worry about dome seeing and things like that. So there are a lot of things you don't know, until after you're finished, whether you did it all right. And some of these things affect that decision slightly, but you do the best you can. There were other telescopes, of course, at Mauna Kea, and it was known that the seeing was very good, so the prime focal length was picked that way initially, I presume. It was the

obvious thing to do, and that's what did happen. It's actually just about the same scale as the 200-inch prime focus, which is quite good for the same reason.

The Cassegrain—I guess there are some different things. You have to worry about the magnification a little bit; how well you have to make and position the secondary, for instance, becomes more critical under certain circumstances than others. On the other hand, with Cassegrain instruments, it's nice to start with a fairly slow beam. I don't remember where the $f/15$ decision got made, other than that it was, again, the same as the 200-inch; we were sort of following tradition, I think, in that sense. When that decision was made—and the instruments had not been designed at this point—it wasn't clear, for instance, whether you could design instruments that would work the way you wanted. And the $f/15$ looked like you could do this, because we'd already done it with the 200-inch, for example. If we'd gone to $f/8$, one would have had to really look at how you would design the instruments of an $f/8$ Cassegrain, to see whether in fact it was even possible. But going to $f/15$ meant that you thought things were all going to be good.

MOY: Over all, what elements of the design and construction have been more difficult than you had expected?

OKE: Not very much of it, actually. From my own standpoint, at least one of the things I worried about was whether the mirror actuators could be made good enough. And it turned out that it was relatively easy to do that, and not too expensive. And the sensors, for instance—the experiments Nelson had done indicated that that was not a problem. In fact, they were actually able to relax the requirements on those a little, because they worked better than they needed to. So there were things like that to worry about.

As far as the dome was concerned, one of the things that seems to have been quite a problem is the dome shutters. The drive mechanism and things for them have tended to break. I suspect they weren't built quite heavy enough initially; the loads were just a bit too heavy. There's been quite a lot of trouble getting the dome shutters working properly, and I'm not sure they've even got that all cured yet.

There were many other questions, too. When it came to designing the whole thing, there were lots of things to consider. The telescope structure was quite

straightforward; there was some good modeling done of that. The original scheme was to correct the mirrors several times a second. That was relaxed a little, partly because if you tried to correct the mirror too fast, you were getting close to shaking the telescope. So they backed off on that. They may gradually speed that up later on, as computers get faster. But there are natural resonant frequencies in the telescope, around 15 to 20 hertz, and you have to stay away from those or do something very sophisticated to avoid stimulating those oscillations. There are a variety of things like that. But given the way they're doing things at the present time, that doesn't seem to be a real problem.

MOY: They're adjusting now at about twice a second?

OKE: No, I think once every two seconds, which is plenty often. There was some talk whether you would try and compensate for wind loads and buffeting by the wind. That's not being planned at the moment, but, again, something could be done in the future. One of the things that wasn't known was how big the wind loads were going to be. If I remember, the assumption was that the wind loads down at the mirror would be about ten percent of what the wind loads would be if the mirror wasn't protected by a dome. There was actually a model made of the dome and shutters, and it was run in the wind tunnel over here to see what was happening. And that was pretty interesting. A friend of mine did that, and I saw the thing running a couple of times. And that showed that their assumption of ten percent was a little on the low side, as I remember; I think it turned out that the numbers should have been more like fifteen percent. But they weren't off by a big factor, so they were OK. That may be the only dome that's ever been put in a wind tunnel to see what happens when wind blows around a dome with shutters, and a hole in the shutter.

MOY: Aside from the actuators, were there problems that turned out to be a lot easier than you had expected?

OKE: Not particularly. The dome itself seemed to go up with no unforeseen problems. It turned out that the instrument designs were relatively straightforward. We had to do some innovative things, which were fun to do, and they turned out very well, actually.

I guess the mirrors were one real worry. Again, it was not only doing segmented mirrors but you had to invent a way of doing it. And that also had to be new, because nobody had ever done that before. There were a lot of discussions as to how to do the mirror. The way we wanted to do it—and again, I think it was Jerry Nelson largely who pushed this—was stressed-mirror polishing. Basically, you take the mirror and you distort it by loading it with weights and levers. Then you polish it into a sphere of the right radius, and when you let it go it should pop back into the shape of an off-axis hyperboloid. So it's a very asymmetrical kind of shape.

What Itek wanted to do was use what they called computer-controlled polishing, where the computer runs the tool over the surface. We were given sort of a snow job on that, I think, from Itek. They showed us what they had done on military mirrors, where they had used the same kind of technique. But the problem was that the military mirrors were being used in millimeter wavelengths and not optical wavelengths. So we were really very worried. I was at one of the early meetings at Itek where they presented the whole scheme, and we weren't quite wary enough. We felt that they were maybe snow-jobbing us a little bit, but we were impressed by the people—not terribly impressed; they weren't the great company we thought they were. And I think we should have been more wary than we were of the problems.

The stressed-mirror polishing had already been tested by Jerry Nelson and was known to work. So that was OK. It was the computer-controlled polishing afterwards—
[Pause] When you cut the hexagon, you also release strains so that the mirror distorts a little after you've cut it. So you want to go back in and touch things up a little. That's really where the computer-controlled polishing was going to happen. So the plan was to make a reasonably good mirror with the stressed-mirror polishing, then cut it, and then the computer-controlled polishing would finish off—do all the things to make it really nice. What some of us wanted to do was use a scheme that Jerry Nelson came up with, which was to put a warping harness on the whiffletree structure that supported the mirror, so that you could distort the mirror a wee bit by just putting spring pressure on the support structure.

MOY: Just out of curiosity, how much pressure are we talking about?

OKE: Oh, 20 or 30 pounds on a dozen points or so. You were trying to bend the mirror by on the order of a micron, and that doesn't take very much. If you take a mirror and just push on it with 20 or 30 pounds, you'll bend it that much. It wasn't a big deal to do it. The problem with the warping harness is you can change the curvature a little bit, and you could change the astigmatism a little bit. You could only change big terms, but those were the ones that were important. So some of us pushed very hard, along with Jerry Nelson, to build warping harnesses into the design, on the suspicion that maybe Itek wouldn't be able to do the computer-controlled polishing. And in some of the computer-controlled polishing stuff they'd done, you could see scalloping along the edge, which is a typical problem you get when you use a tool that's small and don't do it perfectly; it's simply very hard to do it perfectly, even with a computer. We finally persuaded the Keck project manager, Jerry [Gerald M.] Smith, that this should be done. It cost a wee bit, but it turned out that it was absolutely necessary. It would have cost a fair amount of money if we had not done it when it was being built initially. All the warping-harness fixtures were built into the design of the whiffletree structure, so all that stuff was actually there.

MOY: Everything but the springs, practically?

OKE: Yes. So all you had to do was put them in. And just as we had suspected, but hoped wouldn't happen, it turned out that they couldn't do computer-controlled polishing work worth a hoot. They polished one mirror this way, and it was terrible. So we turned that off right there and then and went to the warping-harness scheme. So pushing that at the right time made that problem partly go away, at least.

The only other problem with the mirrors was that it was costing more than originally thought. It was mostly in getting the thing going, getting the right team together and getting the techniques down, and so forth, at Itek. Getting the whole system going was much more expensive than it was estimated to be. Eventually it was running fairly smoothly. And this took, again, a fair amount of "oomph" from Jerry Nelson. Jerry Nelson's a great troubleshooter! He just understands everything. If Itek had listened to Jerry Nelson more than they were willing to, it would have helped a lot. [Laughter] But they finally got them into shape and things started running relatively

smoothly, and the costs settled down so that the actual cost per mirror probably isn't any more than what was originally budgeted, except that it probably cost \$10 or \$15 million more to get it going in the first place.

MOY: Some of the stressed-mirror polishing was subcontracted back to Tinsley, isn't that right?

OKE: They're doing some of it now, yes. They're doing about a third of the mirrors at Tinsley, using the same techniques.

MOY: And that was done primarily to try to speed things up, right?

OKE: Yes, because things went slowly, the initial part of it. And I guess the first stressed-mirror polishing tests were done at Kitt Peak; they did mirrors of that size to demonstrate that this was possible.

MOY: You were talking about the wind loading. I've heard horror stories about what it's like to work up on Mauna Kea. Have you been up there very much?

OKE: I've only been up there a couple of times, and it was absolutely still. [Laughter]

Begin Tape 2, Side 2

OKE: Both times I've been there, it's been very calm. So I don't have much feeling for it. But it does get really windy, and cold, too.

MOY: What about the oxygen depletion? Has that caused you much trouble?

OKE: It doesn't seem to bother me at all. I worried about it quite a bit, because I had bypass heart surgery about ten years ago, so I was a little apprehensive as to whether I could even go up to that altitude safely. I asked my doctor whether she thought it was OK or not, and she said, "Yes, it's fine, but just be careful. Don't be stupid." The first

time I went up there, which was about four years ago, I had a little apprehension, and I was watching pretty carefully to see how things were going. We went down to see the James Clerk Maxwell Telescope, which is the 15-meter dish. In fact, I climbed up all the way to the top of that thing. So I found no particular problems, as long as you take it slowly; you can't rush around, obviously.

When I was in college, we were hiking up at 13,000 and 14,000 feet on one or two occasions, and I used to get splitting headaches when I'd do that. But I don't anymore. I've never felt any problems at all.

MOY: Any problems in your thought processes while you're up there?

OKE: I don't know. There's a real problem there, in that if there are problems, you don't know about them. Some people, in fact, have taken mental tests, doing arithmetic and things like that, and apparently it's rather surprising how poorly you do when you think everything is great. But I have not noticed it particularly. Apparently, the problem is you think everything is fine.

There's a marvelous story that Malcolm Smith, who was head of UKIRT, told about one of his engineers who was up on top of the mountain. He was cutting a piece of steel to weld in to fill up a hole, and apparently he fiddled and he fiddled and he fiddled, trying to get this piece of steel to fit into the hole properly. Finally, he just gave up and he phoned down, and his comment was, "I've cut the thing off twice, and it's still too small." [Laughter]

There was a lot of debate as to whether the observing room should have oxygen-enriched air. In fact, I think it's designed to be done. It's actually sealed, in the sense of having vapor barriers in the walls, so that you can enrich the oxygen in the computer room where people work, if that's wanted. You have to put in a fair amount of machinery to do it. In other words, you have to provide the oxygen. There are facilities to put that in, but there's no plan at this point to do it.

The UKIRT people had a big study done. They're using Air Force medical officers to do this, because they have experience, of course, with low-pressure situations. So they had a doctor do quite a lot of work, and he produced a forty- or fifty-page report

which is really very interesting reading. I still have a copy of that. But it isn't at all clear to me, reading that, whether there's any particular virtue in enriching the oxygen.

The problem is, if you're made to go up there and sit in this room and just stay there, then enriching the oxygen is probably OK; if you go down at the end of the night, you quickly go down to 10,000 feet. But if you're in and out of there, and going out on to the observing floor to do something, then you're in and out of this atmosphere, and that defeats the purpose of it. As far as I could see, reading what people had found out in this report, it wasn't at all obvious that there was any point in doing it. And since it costs quite a lot of money to do it, I think the feeling was let's not do it until it looks as if it's really necessary.

MOY: Do people use oxygen while they're up there?

OKE: No, not normally. I talked to some of the welders who are working on the dome. I talked to one of the guys quite a bit, asking him whether he noticed the effect, and it apparently doesn't seem to bother them at all. On the other hand, there have been some problems up there. Occasionally people get into trouble. The UKIRT people are all trained to watch other people they're working with, and they're alert to the symptoms of not having enough oxygen. So that if there's an obvious problem, don't wait for him to tell you; you just get him off the mountain as fast as you can.

MOY: Is it possible to summarize the various contributions from Caltech people and UC people, and perhaps even University of Hawaii people? Have there been characteristic contributions from the different places?

OKE: It's a little hard to pin down a lot of the things. My way of thinking, the real super guy in the whole thing has been Jerry Nelson. The project will succeed, I think, primarily because he had his foot into so many parts of this and made sure that it all happened right. He was just the right guy to do it, and he was very, very good. There have been many other contributions. There were lots of decisions which needed to be made.

For instance, one of the problems was in the design of the dome itself—not the outside part but what went inside. The old-style domes, they put all the living quarters

inside the dome. The 200-inch has done this. But it became increasingly clear when the Keck Telescope was being designed that there are some real gains to be had by being very careful about thermal properties of the dome. As I remember, the original dome design was just terrible. What they had done was to put all the rooms built into the dome, and the whole dome rotated. Then you had all the problems of how to insulate all of these rooms, where you had to work and which had to be warm. It's very hard to insulate, because they have to have steel struts that attach them to the dome to hold the whole thing together. So they had a telescope sitting in the middle of this structure, which had rooms all over it, sort of like the MMT in Arizona, except on a much bigger scale. I was one of the people who pushed very hard not to go this way. What we finally decided to do, basically, was to take all of that stuff out and have the dome completely empty, and then make a subsidiary building out to the side which could be properly controlled and where all of the living things went on.

That evolved, then, into another problem. And that is, you needed platforms around the telescope to provide access to it. What you need is what you call the Nasmyth deck, where you can get up to the level of telescope. Since all of these rooms were now gone, you had to have something so that you could get 80 feet off the ground to where the telescope was, to service instruments, and so forth.

MOY: What is that platform called?

OKE: It's called the Nasmyth deck. If you take the light straight through from the secondary down through the telescope, that's the Cassegrain. And in an alt-azimuth system, if you go through the axis the telescope rotates in, if you take the light out that axis, it comes out onto a sort of platform at the end of the bearing. And that's called the Nasmyth focus. And the Keck Telescope, like most alt-azimuth telescopes, has two—one on each side. As a matter of fact, there are several others that you can put small instruments around as well. But these instruments are 50 feet off the ground, and you have to be able to get to them. The original scheme was to build a Nasmyth deck, which was attached to the dome. It would be light—an open structure so that air went through, so it didn't have heat associated with it, which was the prime concern. But the idea was

basically to have the access area attached to the dome itself. And then what had to happen was that you rotated the telescope and the dome together. You had to keep the two within a few inches of each other all the time.

This also had some problems. It was fairly awkward. One of the problems was access, because this thing was rotating. You had to have an elevator to get up there, because it was too high; at that altitude, you shouldn't be running up and down 40 feet of stairs all the time, so there had to be an elevator. And the problem was that the elevator had to go to this deck, which was moving all the time; you had to have an elevator that didn't have any bottom! They tried to design this, and it was a nightmare to try and figure out how to do it safely. It turned out later that the elevator company wouldn't have done it anyway.

My scheme basically was pushed: to modify the deck and have it fixed. So it became a circular thing. The difficulty is that, for instance, the two Nasmyth platforms are a fixed deck; you just walk from one to the other and that's fine, that's very easy to do. But because the telescope can move freely inside the dome with this deck in place without having to move the dome, there are a few places where, in fact, you don't have access. So I did a whole bunch of calculations to figure out, for instance, what fraction of the time you would have problems accessing things. The advantage of it was that since it was fixed, there was no problem with the elevator.

The other problem with a moving elevator is that if you go out onto the ground floor of the dome, if you're running the telescope, you have no idea where the elevator is. [Laughter] It may be anywhere in this three-dimensional space. This way, you could go out and you know where the elevator will be. So I did a series of calculations that showed that you could have good access but you didn't restrict the motions of the telescope in any serious way at all. It obscures a wee bit of the sky, for instance, around where the elevator sticks up. You can't bring the telescope all the way down. So I did all sorts of calculations: what fraction of the sky this would be a problem for, and whether this was really a problem for astronomy. And I finally sold them on it. There is no doubt this was the way it should be done. So that was satisfying, to see something there that I actually designed—not in detail but conceptually, the size and shape, so that it would actually work. It ended up with basically an empty dome with just a Nasmyth deck—

basically a spindly, open structure that air goes through so there are no heat sources. And I think it will all work very well.

Another thing we found: Early on, a committee was formed to look into the instrumentation. This had a dozen people, I guess, altogether. All these committees are basically fifty-fifty committees, with half the people coming from Caltech and half from UC. So there was an instrument committee, of which I was the chairman. Early on, before the telescope building was going on—in fact, while the design of the telescope was still going on—we started designing theoretical instruments. And we were looking at high-resolution instruments, and moderate-resolution, and lower-resolution instruments. We were looking also at imaging at the prime focus. We were doing preliminary designs mainly to make sure there weren't any problems. In other words, if the $f/15$ was going to be OK, you could design a spectrograph that would work. So we quickly tried to get through this early stage to be sure there weren't any serious problems.

We ran into one problem, actually. And that is, when we started the design on the low-resolution spectrograph, which was going to be a Cassegrain instrument, it rapidly became clear that the space they had allocated to put the instrument in was not going to be big enough. Fortunately, we discovered this soon enough that they were able to change the design of the big mirror support structure, which is the whole bottom of the telescope. They were able to modify it so they could increase the diameter of this area.

Again, because of the detailed design, we had to move the focal length—move the Cassegrain focus—about half a meter. So we were able to get these problems of the instrument design back into the telescope design. We ended up with a volume at the Cassegrain, where you can design and build an instrument that does have room for itself. The original plan would have been just hell to try and do properly.

MOY: You talked a bit yesterday about the development of detectors and how that in some ways staved off the drive for larger telescopes until the 1980s. Were there any intellectual problems in astronomy, research questions of any sort, that were also becoming prominent in the early eighties and made astronomers want more light-gathering power? Or is that sort of a constant, endless quest?

OKE: The real astronomical problems were pushing in the direction of needing a big telescope. We were, for instance, working on clusters of galaxies. It was clear that we were running out of collecting area with the 200-inch. Even pushing as hard as we could on the 200-inch telescope, we were obviously facing a real barrier.

It was also getting fairly clear—about five or six years ago—that in looking at clusters of galaxies, to study galaxy evolution and cluster evolution, going as far as the 200-inch telescope could go, the barrier was not far enough back in time. And that was letting us look back 6 or 7 billion years. You wanted to get significantly beyond that, because it was clear that things were not changing back to that point. We thought they *would* change, if you could get somewhat farther back in time. It was obvious that these particular kinds of problems weren't going to be solved without going to a bigger telescope, so that we could go to fainter objects.

MOY: Was that a long-standing issue, or did that also come to prominence at this time?

OKE: It came to prominence because people were looking at these closer objects and gradually finding that things weren't changing very rapidly up to as far as we were going. In other words, we weren't sensing galaxy evolution back to this point. So that was a problem; those are very faint objects. That's where a big telescope will be very useful.

MOY: How far back in time was the 200-inch seeing?

OKE: Now we're getting back about 8 billion years. In the photographic days, we were going back maybe a billion. We've gone a long way, in terms of the volume in the universe we look at; it's enormously bigger. But there's still a big gap between 8 billion years and the 3-degree blackbody radiation, which is the first 100,000 years or so. So there's a gap in there of about half the history of the universe. There are a few galaxies known in this region, and there are lots of quasars that are known, so there's some information on this gap, but there's a lot that still needs to be found. It's a real gap, where a lot of interesting things must have happened.

There's also the current problem that if you look at the microwave background, it's very smooth as far as one can tell. Whereas, if you look at the present universe, it's

very bumpy. And the problem is how you got from there to here. All the cosmologists were trying to work on models to make this possible, but it's very hard. And eventually one has to go out there and make observations to see how, in fact, this all happened. The Norris spectrograph, for instance, on the 200-inch, is aimed at working on that problem to some extent.

Another very interesting problem, which was known ten, fifteen years or so ago, and again has become very important, is looking at the so-called Lyman-alpha Forest. And Wal Sargent probably talked a lot about this. With the Lyman-alpha Forest, you have to observe with very high spectral resolution, because the lines are very narrow. And, in fact, in many cases, you really want to measure the profiles of the lines. And the problem is that you have to use quasars to do this. Quasars are 19th and 20th and 21st magnitude; there's just not many photons. You need high resolutions with objects that have very few photons, and you simply run out of photons eventually. Wal has done a great job; he's using the 200-inch to push very hard on this. But there's still a range that needs to be done further. And the high-resolution spectrograph on the Keck Telescope will be designed to work on that problem. You need more collecting area so you can spread out the light more and get higher resolution.

Another rather recent development is [professor of physics] Ken [Kenneth G.] Libbrecht's work. He's been doing solar seismology—basically looking at all of the oscillation modes of the sun. The beauty of this is that since these oscillations are in the interiors as well as on the surface, you can actually probe what the structure of the sun must look like. So far, it's only been possible to look at the sun; it would be very interesting to look at different kinds of stars to see what they do. But it's very, very hard. And you need enormous signal-to-noise ratio to do this, as well. Ken Libbrecht is building an instrument for the 200-inch to try and do this better. He has tried this on the 200-inch with some of the more conventional instruments that are already there, and he basically hasn't succeeded on stars. He's now building another instrument, which will go to maybe an order of magnitude fainter, which may be enough to at least start this project. But again, you need a lot of light for things like that, so a big telescope is going to be a big advantage.

MOY: This is interesting, because I've heard from other astronomers that the main reason you need a telescope larger than the 200-inch is not really for technical reasons but for social reasons, in that most of the problems you could do on the Keck Telescope are things you could do on the 200-inch telescope if you had the whole thing to yourself all the time.

OKE: Yes, there are a lot of problems where time and aperture is a trade-off. For instance, the faint galaxy thing. I think we probably could do it if you had two hundred nights a year on the 200-inch telescope. But that certainly isn't possible.

MOY: But you were mentioning problems that you think require that much light.

OKE: Yes. Some of these—for instance, the stellar seismology problem— [Pause] Well, maybe you could do it. It would take a horrendous amount of computational stuff to do it. But again, you're looking at periods which are like hours to days, and you really want to have signals that tell you what's going on in those intervals of time. You don't want to have to integrate for weeks and weeks and weeks, because then you'd have to put all that stuff together. You'd have to do great, enormous inverse Fourier transforms, and it gets to be just a horrendous job. But it is, in fact, somewhat of a trade-off between telescope size and observing time.

If you don't go the big telescope route, of course, then the only way you're going to make any advances is to make a whole bunch of 200-inch telescopes. This is, in a sense, the way in which ESO is going. They are building four 8-meter telescopes. Now, I suspect they could actually tell this guy, "Instead of giving you ten nights on a great big telescope, we will give you forty nights on one-quarter of one of these telescopes." So then you have to sit there for forty nights and do it. If there was remote observing and somebody else doing it all, maybe then it's feasible to think about doing that. But the instrumentation is such that you're really limited nowadays largely by the night sky brightness; the amount of light collected versus the CCD detector is not really a problem anymore.

There are some problems if you go to very-high-resolution spectroscopy, like Wal Sargent did, for example. Then you do have to start worrying about what the CCD does.

He wants to look at spectra, for instance, where a signal is 30 photons. And if the detector is generating noise at that kind of level, it wipes them out. Whereas if the noise the detector generates is fixed, it doesn't depend on the time. So if you can get more data squeezed into a limited length of time, you can beat that problem. There are some problems where you really do have to have that light and have it all the time.

MOY: Are you pretty happy with how the resolution on the telescope is going to be, as opposed to what was hoped for in 1985?

OKE: It's hard to tell yet. All the evidence suggests that the telescope will be quite good. One advantage of a telescope like this is that if you make a single-mirror telescope, you make one mirror and that's it. In something like the Keck multi-mirror telescope, you can imagine fixing and improving mirrors one by one, and you don't have to take the telescope out of commission to do it. You have six spares. So what you imagine doing, for instance, is to have a list of each of the segments. And to some extent, they even know that now; you can put them in the order of how good they are. I mean, there are some really good ones up here, and there's some not quite so good, and there are one or two which are way, way down here at the bottom; they're sort of lousy. What you can imagine doing, as the money and time come in, is to see that one segment is going to come out in April, so let's arrange to send it to Kodak and they will do the ion polishing on it and improve its figure. So it comes out for aluminizing, and you send it to Kodak, and they work on the surface and send it back, and you aluminize it, and it goes back into the next one. With a telescope like this, you can continuously upgrade its performance. So in that sense, if things aren't perfect—and they won't be perfect initially—there's a potential to gradually and fairly easily build up its quality.

There are other factors, too: All the problems with the domes, with the dome seeing, and so forth; we hope we have cured all of those problems. There were lots of worries, for instance, about what colors to paint things and what the emissivity of the paint would be. What you want is to try and couple the inside of the dome to the outside sky so that you don't get temperature gradients. A lot of work was done on this dome to make all of this right. But until the telescope is in fairly full use and in equilibrium, we

really won't know whether there are some problems or not. We will gradually also find out what the real seeing can be and how good you could really make images. So we'll know how good to make the mirror. It's a flexible situation; it's just great to have something like that.

MOY: What do you think you would work on, the first time you're going to have a set of nights of great seeing?

OKE: Well, the first things we will do when we put on the spectrograph, which we're designing, are pretty simple things. We'll take pictures that are pretty, obviously, spectacular—nice to look at. For instance, there are lots of Seyfert galaxies and things like that, with lots of nice broad emission lines. We need to produce some things for the public. For first light, they did that, too. They'll be just single objects initially, or a galaxy sitting on a single slit. And then as things start to get working better and better, you try long-slit kinds of things, and then you start multi-slit things, and then you try fainter and fainter things. I would guess there will probably be ten nights when we're running the spectrograph in that kind of mode, where if something goes wrong, you sit down and fix it, then hopefully continue. After that, once that is all done with, then you have an instrument that works and does what it's supposed to. One of the problems I'd like to at least start on is looking at some distant clusters of galaxies. In particular, early on, we'll look at some of the most distant clusters we've done a little work on at Palomar, and we'll be able to get much better data, much better spectra, on the Keck telescope. And then the next stage is to look at some of these clusters that we still can't get anywhere with on the 200-inch. These are things we ought to be able to do with the Keck Telescope without any trouble.

MOY: Again, this is with an eye toward looking at evolution?

OKE: Yes. We're getting up into the area where we think galaxies and clusters of galaxies really are beginning to change. We see some evidence of that already, but we need a lot more evidence. We already have a whole bunch of clusters we can work on in which we can look at galaxy evolution. And then we have these other clusters that we

can't do anything with yet, because they're too faint. And that's obviously a challenging problem.

Another problem, which Jeremy Mould [professor of astronomy 1988-1993] and I worked on already, is looking at the system of globular clusters around galaxies in the Virgo Cluster. These globular clusters are way, way out. The galaxy is here [motioning with hand flat], and these clusters are way around like this [motioning above and below hand]. Using these as test points, you can measure the gravitational field, which tells you what mass you have. And we've done about twenty or so clusters in the Virgo Cluster. And the evidence is that there's a lot of mass that we, in effect, don't see. People suspected this, and you find this in other cases, too. But one of the things we plan to do is to make a much bigger sample, so that we can measure the gravitational field accurately as far as the cluster really goes. At that point, we'll be able to see how big a galaxy is, and we'll actually be able to see how the mass is distributed out as far as the galaxy goes. Most evidence that there is this mass is rather indirect; it depends, for instance, on the assumption that the Virgo Cluster and all the galaxies are bound to each other. And that probably is true, but you don't really know that. Whereas with this kind of measurement, you can actually say, Yes, there is this much mass around this galaxy and that galaxy, and so forth. That's another instance where we need to get a lot more data than we get with the 200-inch.

MOY: I'd like to talk briefly about Keck II. Were you surprised when you heard that the Keck Foundation had come through with money for a second telescope?

OKE: Not entirely. The second Keck has always been sitting there as a possibility. I don't know, for instance, whether this second telescope was something [Howard] Keck had talked about. But the rumors were around that he had talked a bit about the possibility of there being two. And in the early UC combination, there was talk about two. So this idea of a second Keck had always been in the deep background. And since we were all busy building the first one, no one worried particularly about the second one. The rumors weren't just rumors, they were things that apparently other people had heard Keck express at one time or another. So it wasn't a real surprise.

MOY: Aside from the relatively minor problems you mentioned with the dome of the first telescope and so on, will there be any significant differences in design between the second and the first?

OKE: I don't think so. What they will do, of course, in these places where there have been problems—like the shutters, for example—they might, for instance, decide to redesign the drives and make them heavier, so that there's much less chance of their breaking. We might know a little bit more about the thermal properties of the dome by then, and it might turn out that we would want to do something differently. But I think most of these things will be superficial.

One of the interesting problems, which hasn't even been discussed yet, is what kind of instruments to build. Because we have a low-resolution spectrograph on the first Keck Telescope that will presumably fit on the second Keck Telescope. But then you have to look at how you do observing—whether you spend a lot more time on one telescope. For instance, do you need a second identical instrument, or should you build an instrument that will do something else very well? So you could leave the low-resolution spectrograph on the one telescope, and do that kind of problem predominantly on one telescope, and then have another spectrograph of a different kind to be put on the second one.

The European Southern Observatory—with four telescopes—that's going to be a *really* interesting problem, figuring out how you do observing. You have an enormous amount of flexibility with four, compared with one. With things that cost this much money, you really have to think about how to get the most out of a telescope the most economical and fastest possible way. If it turns out, for instance, that changing instruments is rather a hassle and takes a long time, it may influence how often you change instruments, or move instruments from one telescope to another.

I guess you know that there is already a tunnel underneath for the second telescope. So a second telescope was certainly not just a pipe dream. That [tunnel] was built when the building was put up; it was part of the foundation, obviously. So again, this second telescope was not something that wasn't expected. It was, in a sense, planned.

MOY: The main problem you can tackle with two that you couldn't do with one is interferometry, isn't that right?

OKE: Yes, at least in the infrared. It's going to be very, very hard to do interferometry in the visible.