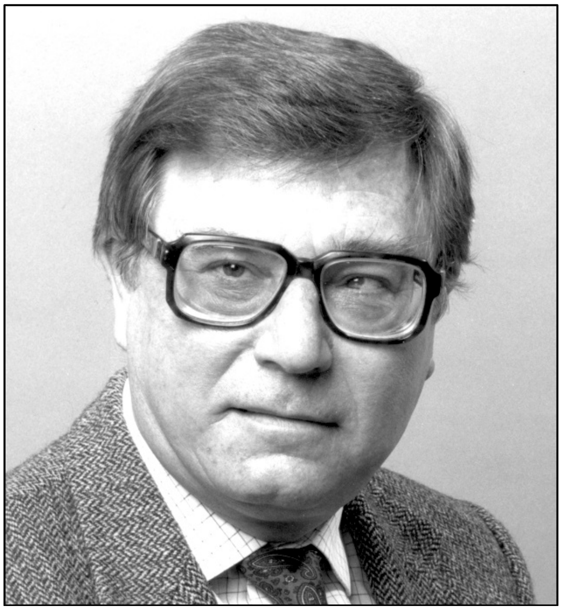

Wallace Sargent, 1991

WALLACE SARGENT
(1935 – 2012)

INTERVIEWED BY
TIMOTHY D. MOY

July 9 and 17, 1991

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the Keck Series Interviews

The interview of Wallace Sargent (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, Palomar Observatory, Keck Observatory

Abstract

An interview in two sessions in July 1991 with Dr. Wallace L. W. Sargent, Ira S. Bowen Professor of Astronomy in the Division of Physics, Mathematics, and Astronomy (PMA). Dr. Sargent received his BS (1956), MS (1957) and PhD in physics (1959) from the University of Manchester, U.K. He became a research fellow at Caltech, working on quasar absorption lines, from 1959 to 1962. In 1966, he joined the Caltech faculty as an assistant professor of astronomy, becoming associate professor two years later, full professor in 1971, and Bowen Professor in 1981. He served as executive officer for astronomy 1975-1981 and 1996-1997 and director of Palomar 1997-2000. In this interview, he discusses his involvement with the origins, in the early 1980s, of the first of the Keck Foundation telescopes on Mauna Kea, known as Keck I.

He recalls the initial plans for a big telescope in partnership either with the Carnegie Institution, the University of Arizona, or the University of California, and the circumstances leading to eventually joining with UC on Keck I; the problematic relationship with Carnegie; and his involvement, along with Rochus Vogt, Gerry Neugebauer, and Edward Stone, in raising money from the Keck Foundation.

He discusses the design competition for the proposed 10-meter telescope; the contributions of Jerry Nelson, then at Lawrence Berkeley Laboratory; and the difficulties encountered in designing and building the telescope. He concludes with an account of the decision to build Keck II.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Sargent, Wallace. Interview by Timothy D. Moy. Pasadena, California, July 9 and 17, 1991. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Sargent_W

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH WALLACE L. W. SARGENT

BY TIMOTHY MOY

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

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Pasadena, California

Session 1 **July 9, 1991**

Session 2 **July 17, 1991**

Begin Tape 1, Side 1

MOY: I'd like to talk very briefly about some personal background. You were born in England, correct?

SARGENT: Yes, in a village in Lincolnshire in 1935.

MOY: Is that the Lincolnshire of Isaac Newton?

SARGENT: Yes. Although Newton was born at the bottom end of the county and I was born at the top end. I've never actually been to the village where he was born—Woolsthorpe, he came from.

MOY: And your family background—your parents?

SARGENT: None of my ancestors went to high school. My father was a gardener when I was born, in a large country house in Lincolnshire. My mother had worked as a shop assistant in the village. She had the ambition that her children should go to a university. So we moved to another village when I was around three, and I did very well in school. Eventually, I took the exam at eleven—which you had to take in England at that time to decide whether you were among the twenty percent who were to go on to more academic things or the eighty percent who were to be the hewers of wood and drawers of water. I was in the twenty percent. Then I went to Manchester University. I was the first person from my high school to go to a university.

MOY: How large was your high school?

SARGENT: About six hundred kids.

MOY: You began in the university majoring in astronomy?

SARGENT: Physics.

MOY: When did you change?

SARGENT: When I was a graduate student. I did well in my bachelor's degree in physics. And I decided, even before I went, that I would like to do astrophysics. I was inspired by Fred Hoyle, the astronomer who broadcast on the radio when I was a teenager. I heard about the expanding universe, or the steady state, which he preferred. So I was familiar with these questions when I was around sixteen. I favored the steady state at the time—no longer.

I got the equivalent of an NSF [National Science Foundation] fellowship to go to graduate school, which meant that I could study anything in physics. I studied theoretical astrophysics, and I got a master's degree in a year, and a PhD in another two years [at Manchester]. So when I was twenty-four, I was ready for a postdoc. I came here because, during the whole of the last year that I was doing my PhD, my advisor was away. He was at Princeton, but he spent three months in the summer in Pasadena at Caltech, which was then, and probably still is, the astronomical center of the world, with Mt. Wilson and Palomar. He recommended me to [Jesse L.] Greenstein [DuBridge Professor of Astrophysics, emeritus; d. 2002] to come here as a postdoc. And I did, with the intention of working on theoretical nuclear astrophysics, which I hadn't studied before, but in those days one thought that one could study anything, because the state of knowledge in any given subject was not very advanced.

MOY: Who was your advisor?

SARGENT: Franz Kahn, he was called. He works on the interstellar medium. He's still a professor at Manchester University [d. 1998—ed.].

After I had been in this department for a few weeks, I went up to Mt. Wilson to see people observing. They allowed me to guide the telescope for a few minutes, and I was immediately captivated by this. I decided that I would become an observer. So I switched immediately, and I worked on the spectra of bright stars to start with, because that was what Greenstein's group was doing. So I stayed for three years at Caltech, beginning with 1959.

MOY: If I could back up for just a minute, you mentioned listening to Fred Hoyle's discussions on the radio. He was the person to coin the term "Big Bang," isn't that correct?

SARGENT: Yes, as a derogatory term.

MOY: Were there any other people, or experiences, that led to your interest in physics and then astrophysics—family friends, teachers?

SARGENT: No. Only books. When I was quite small, my mother was working cleaning somebody else's house for a day a week. And these people were slightly richer than we were and had some encyclopedias that their children had had. They gave me the encyclopedias, through my mother. The whole series was called *The Children's Encyclopedia*. It had astronomical pictures in it, and I remember seeing a picture of M31, the great nebula in Andromeda, and another one of the center of our galaxy, with all the enormous numbers of stars. So when I was around eight or nine, I realized that these things were there and was turned on to astronomy.

MOY: When did you first begin looking at the Lyman-alpha Forest?

SARGENT: In a serious sort of way, around 1973.

MOY: How did the idea of using quasars as a way of exploring the intervening material come about?

SARGENT: Well, that came up when I came back to Caltech in 1966. I was in England for two years, from ’62 to ’64; and then I came back to UCSD [University of California at San Diego], where I was an assistant professor of physics for two years. And then I was invited back to “the center of the universe” in 1966. Quasars had just been discovered three years before, and a postdoc in physics in [Willy] Fowler’s group in Kellogg [Radiation Laboratory] was exploring theoretically the ways in which the light from quasars could be modified by intervening galaxies. [Robert V.] Wagoner, he’s called; he’s now a professor at Stanford. He actually had the correct idea. He realized, as other people did, that you could detect interstellar lines due to the gas. But he also realized that you could detect Faraday rotation in polarized radio waves as they came through the magnetic field of a galaxy way out there that you couldn’t see. So he explored all of these properties.

At about that time, the first absorption lines due to intervening material were discovered. I was part of the very early work. I guess John Bahcall, Greenstein, and I made the first discovery that there was an absorption of more than one redshift in the spectrum of a quasar. So it was either material that had been ejected from the quasar at three completely distinct velocities separated from one another by enormous amounts in velocity, or it was due to intervening objects. And of course we thought it was much more likely that they were due to intervening objects. The problem was that, as Wagoner’s calculations have shown, if you thought that distant galaxies had interstellar gas which was spread over about the same area as our galaxy, the probability of going through one of them out to a quasar or to a redshift of two was only about a tenth. So finding three interceptions was about thirty times too many. The solution that we all came to was that the gas in galaxies earlier in the universe must have been more spread out than now, which I think is correct. But it is an interesting case, where the theoretical predictions were very, very conservative as compared with what was actually observed.

Well, that’s the main line of research that I’ve pursued since 1967, I guess. I came back in ’66, and got on to that sort of stuff in ’67.

MOY: How did you first become involved in the project for what is now the Keck Telescope?

SARGENT: We became involved in it gradually. Until 1978, Caltech was associated with the Carnegie Institution in running Mt. Wilson and Palomar and the Las Campanas Observatory in Chile as one unified operation. In ’78, Maarten Schmidt [Moseley Professor of Astronomy, emeritus] became the director of the Hale Observatories, as this unified operation was called. And in 1979, after one year in office, he decided that it wasn’t working, and he recommended that the joint operation be terminated and that we continue to share the time on the telescopes but each party go its own separate way as far as administration went. And in 1980, I guess, that in fact happened. And one of the strains in the Hale Observatories, between us and the Carnegie Institution, was different feelings about what we would do in the future. There were two particular things I can remember. I wanted to do another sky survey with the 48-inch telescope [at Palomar], something that actually happened; it started in 1985. And also, there was a feeling that we needed to get into bigger telescopes, because we’d heard that the University of California was pushing a 10-meter telescope, using a very novel design. First of all, they pushed any 10-meter telescope, and then there were two competing designs, over which they had a shootout. One design was to have a single, very thin piece of glass that was flexible, the so-called rubber mirror, and adjust it with active controls from the back. The other was [UC Berkeley Professor Jerry] Nelson’s idea, which was to have a segmented mirror in which there would be thirty-six hexagonal solid pieces which would be adjusted, but you wouldn’t adjust the whole. It wouldn’t be a complete surface; there would be cracks in it. We knew that this was going on.

MOY: The “we” you’re talking about are the Caltech astronomers?

SARGENT: The whole Caltech-Carnegie—and indeed world—astronomy community. And several of us at Caltech thought that we ought to get in on this sort of thing.

In 1980, Robbie [Rochus] Vogt was then [Physics, Mathematics, and Astronomy] division chairman. Jack [John D.] Roberts [Institute Professor of Chemistry, d. 2016] was provost. We were evidently asked by Vogt to provide Roberts with some information about what we should do next in astronomy. I wrote a memo to Roberts on the 18th of March, 1980, in which I said, “It’s now thought to be technically possible to construct ground-based optical telescopes in the ten- to fifteen-meter range, using thin, segmented primary mirrors and active optics. There are plans to construct such telescopes by the late 1980s. One of these schemes, a ten-meter telescope, is being pushed by the University of California, with some input, not financial, from Caltech." We actually had a few advisors on their scheme. "We should seriously consider becoming a major partner in the UC scheme if we are not to fall behind in the late 1980s." I was then executive officer for astronomy, and I think it was in that capacity that I was writing.

When Schmidt resigned [1980], [Gerry] Neugebauer [Millikan Professor of Physics, emeritus; d. 2014] became director of the Hale Observatories. At that time, UC had three committees going—one on the design of the telescope, one on what kind of science to do, and one on where it should be put. And the Pasadena people had a representative on each of those committees.

MOY: So this was sort of an outside committee?

SARGENT: No, it was inside, but they thought, well, maybe they would one day need a partner. In any case, we knew a fair amount about all of these questions. So they had, as a courtesy almost, these outside advisors. I have a memo which was written to [Caltech president Marvin L. (Murph)] Goldberger and Vogt—by that time, Vogt had become provost—on the 16th of April, 1981, from Neugebauer, [B. Thomas] Soifer [Brown Professor of Physics, emeritus], and [Keith] Matthews [chief instrument scientist], who were advising UC on the infrared capabilities of such a telescope, in particular. In this memo, they recommend that Caltech should try and become part of the UC scheme. So I'd written the thing in 1980, and they wrote something in 1981, all recommending that Caltech try and get in on the UC project. So in the early 1980s, we were associated with it and we were trying to persuade our own administration to get involved.

MOY: Had there been any intimations at this point from the people at Berkeley, or elsewhere in UC, that they were interested?

SARGENT: Yes, they realized that it might be difficult to raise the money. If they'd realized how much it was going to cost, they would have been even more doubtful about how difficult it was going to be to raise the money. But it was all informal. It wasn't done through the UC and Caltech administrations; it was done at the astronomer level. And now, here we were in 1981, trying to get our own administration interested. The administration, particularly Vogt, encouraged Neugebauer to really be serious about a large telescope project. Neugebauer set some meetings going in late 1982 and early 1983, in which the astronomy group here at Caltech—by then we'd been divorced from Mt. Wilson—discussed what sort of thing they wanted to do. I've got a pile of documents here, which are records of meetings that were organized by Neugebauer. And by about February 1984, things had crystallized into very specific projects that we might consider going into. I can list them briefly.

The first possibility was to go with UC for $25 million, plus operating costs of $1.5 million a year, which would require $25 million up front, plus an endowment of $24 million. So we would need, for that scheme, around $50 million to buy a quarter of a 10-meter telescope. We thought the buy-in cost was roughly a quarter of $100 million. That was much higher than the University of California's estimate of what a quarter of a telescope would cost at the time; nevertheless, that's what we thought. That was one possibility.

The second possibility is that we could join with the Carnegie Institution—we were still sharing the telescopes with them—and jointly negotiate going into the University of California scheme with Carnegie. As Neugebauer said in his memo, "This requires that Carnegie abandon their wish not to have this facility at a fourth site," because already there was Mt. Wilson, Palomar, and Chile. And if you spread your activities over too many places, with operating costs at each place, then you start getting into difficulties. It also would have divided the observing time that we would have got by a factor of 2. So we would, on that scheme, be getting an eighth of a telescope rather than a quarter.

A third possibility was that, at that time, Carnegie was getting interested in a big telescope themselves, particularly through Stephen Shectman, who's on their staff, who decided that you could build a very large, fixed bowl, like the Arecibo Radio Telescope, and move the detector around in order to point at a restricted part of the sky, just as the

Arecibo Radio Telescope could. That sort of scheme is actually, in some respects, promising but is extremely hard to realize. If you look into the requirements for the thermal stability of the components that you're moving around, and the mechanical stability, it's pretty bad. Carnegie, for a short time, was interested in that. So we thought, well, maybe we could go into such a scheme with them.

MOY: Did you have any idea how large a reflector you could get that way? Had they done any feasibility studies?

SARGENT: No. It never got very far; but they were thinking in terms of something really big, like 15 meters. And it would have been only for spectroscopy and only for a restricted part of the sky. And it would have had to have been at Las Campanas in Chile.

The fourth possibility was that we could join with Carnegie in building a 200-inch at Las Campanas. Las Campanas is an excellent site. We had a 200-inch in the Northern Hemisphere [Palomar]. If we had another 200-inch in the Southern Hemisphere, we would have been in a reasonably powerful position. And there were some estimates as to how much this would cost—$10 million in construction, plus $10 [million] endowment, which I think would have been too small an amount, actually. But that's what we were considering.

The fifth possibility would have been to go with Carnegie in a 7.5-meter at Las Campanas. At that time, the University of Arizona was promoting the construction of large mirrors—at that time around 7.5 meters, now 8—using a novel scheme that [J.] Roger [P.] Angel devised, where you make a lightweight mirror by spinning the mold as the glass is cooling so that it conforms to a parabolic shape. Then all you have to do is to make it into a real optical surface, which appears to be a real problem. So, one possibility would have been to have got the University of Arizona to join with us and Carnegie and build such a telescope at Las Campanas.

The sixth possibility would have been to go with the University of Arizona to build such a telescope at Mt. Graham, which is their current intention. Mt. Graham is in southeastern Arizona; it's well over 10,000 feet high. There have been enormous environmental problems. The project, which eventually University of Arizona did start with Ohio State and a group in Italy, was to use Mt. Graham. But then it was discovered there was a certain species of squirrel living there that is very rare. And so it's been held up, partly by that and partly by a lack of money.

And then the seventh possibility, which was suggested by Keith Matthews, who works for Neugebauer, was a really radical one, which was to put two 7.5-meter telescopes into the 200-inch dome at Palomar. The 200-inch was built on very generous principles. The actual slit that the 5-meter telescope looks out of is much wider than 5 meters, so you could, Keith calculated, put two 7.5-meters in there. You wouldn't have to build a dome, but you'd have to take the 200-inch out of commission for several years. The site was, by then, beginning to deteriorate because of the lights of the cities. But anyway, that was a seventh possibility.

There were eleven astronomers, including a couple of radio astronomers, who voted on all these things. And there was a fairly narrow agreement that the Possibility A was the favored one—the one to buy twenty-five percent of the University of California's telescope; and Possibility E, which was to build a large telescope with Carnegie at Las Campanas. It was almost a tie. It depended on how you added up the votes.

MOY: This was essentially an internal Caltech thing.

SARGENT: An internal Caltech thing. There were several meetings in which different possibilities were discussed. We had some input from the University of California; we invited somebody to come and talk to us. We had some input from the University of Arizona. Some of us went over to the University of Arizona to see the facilities that they were starting to build to make mirrors. And all of this was in around late '82 to early '83, and it stretched into late '83, and then into '84.

The big political question was whether to stick with Carnegie in whatever we did. During the course of our discussions, we received a letter from the director of the Carnegie Institution—or the Carnegie Observatories—George Preston, who said that they would only wish to continue to have discussions with us if the telescope not be bigger than 5 meters; that it not have any parties other than us and Carnegie involved in it, and that it be at Las Campanas. This was so restricting that it was basically saying, "Well, we're stopping talking." So we then abandoned the idea of going ahead with Carnegie and concentrated on whether we should join with the University of Arizona by ourselves, or go with the University of California, or do something just by ourselves. The Carnegie Institution connections were dropped at that time.

MOY: Had there been some difficulties in the relationship with Carnegie?

SARGENT: Yes. It's very hard to summarize what they were. It's also hard to summarize what was personal and what was produced by the system. But there was a long history of friction arising from the fact that [George Ellery] Hale of the Carnegie Institution was the man who raised the money for the 200-inch. But the Rockefeller Foundation wouldn't give the money to another foundation; they would only give it to an educational institution. Caltech was conveniently close to Mt. Wilson, so we got the telescope without actually doing anything. In fact, there were no astronomers on the faculty at the time when the money for the 200-inch was given to Caltech. So that didn't get things off to a very good start. The system was that Mt. Wilson was completely paid for, and later Las Campanas was completely paid for, by the Carnegie Institution; and Palomar was completely paid for by Caltech. As far as the money went, there were two completely separate sources. And there was the feeling among the Caltech people, developing even by the time I came here in '66, that the director of the joint operation was spending too much time on Carnegie things and not enough time on Palomar, and that Palomar was slipping relative to other large telescopes. This became acute when they started to build a telescope in Chile, because a lot of the resources that had been going to Mt. Wilson, for example, were now put down in Chile.

MOY: When was that begun?

SARGENT: Around 1973. And there was a feeling, which I think was correct, that Horace Babcock, the [Palomar] director, was spending all of his time on building up Chile and none of his time on making sure that Palomar was a viable operation. And so the instrumentation at Palomar was not up to the standards of other places. For example, money-raising was not done with the sort of enthusiasm that it really required.

Then there were personal problems. Allan Sandage, over at the Carnegie Institution, has proved to be, over the years, a difficult person to get on with. Some of us are difficult people to get on with. So Schmidt found that he had a staff that was uncontrollable. Caltech tried to cure the situation by insisting that there be an associate director who was appointed for Palomar from Caltech. And that was [J. Beverly "Bev"] Oke, from 1970 until Schmidt became director of the Hale Observatories in 1978. Since Schmidt was a Caltech man, he didn't think that he needed another person. But in fact, when he was director of the joint operation, he spent all of his time over at Santa Barbara Street [at the Carnegie offices] and practically none of his time here. He finally realized he couldn't do both jobs and decided they should be two separate jobs. It doesn't sound very important, actually, and I think, under a very strong director, the system could have survived. But at the same time, I don't think that we would have got into the big telescope league under the prevailing system, with the Carnegie. They were more conservative than us.

MOY: So that left, as the most favored option, buying into—

SARGENT: UC. That decision was made around early '84. Vogt was provost. [Edward C.] Stone was [PMA] division chairman, and Neugebauer was director of Palomar. And in May 1984, it was decided to ask the Caltech trustees for the $25 million.

MOY: I know at some point there was some sort of formal invitation from the University of California.

SARGENT: Ah, yes, we should go back a bit. The University of California began by encouraging our interest. Then they went cold on us. It's hard for me to remember when, but it was around 1982. At that time, they had started to raise money, and they thought they could raise all the money themselves.

MOY: Who were the principal people that you talked to from the UC side?

SARGENT: The UC side was [Robert P.] Kraft, who is the present director of the Lick

Observatory [d. 2015]; Ivan King at Berkeley; Hyron Spinrad at Berkeley; a guy called Gene Smith [Harding Eugene Smith, Jr.] at UC San Diego. Those were the main ones. The different campuses at UC had different attitudes to the whole project. In fact, there are four campuses with a big interest in astronomy—Berkeley; Santa Cruz, which runs the Lick Observatory; UCLA; and San Diego. They have their own frictions. Also, they couldn't agree on what was technically best. The scheme that won came out of Berkeley; the scheme that lost came out of Santa Cruz.

MOY: What was the Santa Cruz idea?

SARGENT: To have this big, thin meniscus. In fact, the man who was pushing that was so peeved that he went off to Europe—Joe [E. Joseph] Wampler—and joined the European Southern Observatory and never came back again. UCLA always supported that scheme, and they have consistently taken little interest in the Keck project for that reason.

Around April 1984, Maarten Schmidt received a call from Kraft, who was up at Santa Cruz, asking if we wanted to talk to them about getting into their scheme seriously. There was a meeting at San Jose airport which was attended by Neugebauer, Schmidt, Oke, and myself from Caltech, and by four astronomers from the University of California—Kraft, Spinrad, Gene Smith, and Ivan King. So there were four on each side. We met in a private meeting room up in the airport in San Jose. We basically agreed that we wanted to join with one another, that they would invite us back in again. So this was the sort of official go-ahead, done through the astronomers.

MOY: Did you have the mandates of your respective administrations at that point?

SARGENT: Well, not to make any agreement, but to have discussions, yes. They didn't send us from Caltech with a certain amount of money. After that, it was agreed by the Caltech administration that we would then ask the trustees for the $25 million. There was a trustees' meeting in May 1984, at which I was chosen to give a talk to the trustees. I think that they thought the British accent would help. Ed Stone and I went to the trustees' meeting. I gave them a talk lasting about twenty minutes—about astronomy, about what sorts of things we would do with a big telescope, about why Palomar was no longer going to be up there at the front unless we did something, and about the scheme that the University of California had developed. And then it finished with the request for $25 million. Then they asked questions, and Stone and I both answered the questions. Then we left, but I heard at the end of the meeting that the trustees had approved that Caltech try and raise $25 million. So at that point, in May 1984, the money-raising began, and by processes which I don't really understand, by August, Mr. [Howard B.] Keck had come on the scene. I went off to observe in South Africa in July 1984, and when I came back, on August 5, 1984, a few of us were invited into the provost's office, where Vogt told us that it wasn't possible to get $25 million but somebody had come up with an offer of between $60 and $70 million if the telescope could be named after him. Now, this created a difficulty, because at that time the University of California was trying to get $36 million out of the Hoffman Foundation, which you've no doubt heard. So they thought they had $36 million. We had $65 million. They thought that the telescope was only going to cost about $47 million. It had started off as $5 million—Jerry Nelson's first estimate. This created an embarrassment.

What Vogt had done was to suggest to the donor, who turned out to be Mr. Keck, and to the University of California, that we build two telescopes.

MOY: So that was Vogt's idea.

SARGENT: That was Vogt's idea. The initial proposal was to build telescopes in both hemispheres—one in Chile, one on Mauna Kea. By that time, the University of California had selected Mauna Kea as being the best site, so if we went in with them, we would have to agree. We looked into this, as well, in all our discussions: Where was the best place to put the telescope? One of the reasons we rejected the idea of going in with the University of Arizona was that we thought Mt. Graham in Arizona was not going to be such a good site. We didn't know about the squirrels in those days—that was just luck—but we knew that the site at Mauna Kea had been well studied; we knew that it was a certainty to be good. Mt. Graham was much more speculative.

MOY: Were there other observatories on Mt. Graham already?

SARGENT: No. If there had been, it would have been easier to get around the squirrel problem. But they were trying to start something from scratch and put a large number of telescopes there, in addition to the big one.

So Vogt evidently suggested first that we build a telescope in each hemisphere. There would be a Hoffman telescope on Mauna Kea and a Keck Telescope in Chile. That idea was rejected by the Keck Foundation on the grounds that Mr. Keck had spent a lot of time in South America. He knew that it was a very unstable place; he had caused some of the instability himself. If this was to happen, then his telescope would have to be on U.S. territory—Mauna Kea. So I think Vogt was beginning to try and persuade the University of California to put *their* telescope in Chile. [Laughter] Or to have two side by side. When the Hoffman thing collapsed—you've no doubt read about the Hoffman circumstances.

MOY: I have seen some descriptions of this, but they are all rather murky. Are you familiar with what happened there?

SARGENT: Not very, but basically it came down to $36 million. Mr. [Maximilian] Hoffman had died. Mrs. Hoffman read in the newspaper that UC wanted to build a telescope, wrote the University of California, offered the $36 million. Then there was a long period of delays while lawyers were playing around. According to the high University of California officials that I've talked to, the delays were instigated by the lawyers in order to make more money for themselves.

Mrs. Hoffman died the day before she was to have signed the papers with the University of California officials. Then there were two trustees, each looking after half the money: Mrs. Hoffman's sister and her long-term assistant. The assistant was willing to carry out Mrs. Hoffman's wishes, but the sister wanted to continue juggling with the money for a much longer time. Eventually, it all disappeared, as far as I can tell from the University of California.

Begin Tape 1, Side 2

MOY: Some of the articles that I have seen suggest that, at some point while the University of California was trying to work out this legal morass of getting all the money or part of it, [President] Goldberger went up to UC to try to straighten things out. One article in the *Los Angeles Times*, for example, even suggested that he essentially said either straighten it out—we'll go in on the one Keck Telescope—or Caltech might go it alone.[1]

SARGENT: Yes, I think that was in the air. Because when the Hoffman money disappeared, we, of course, wanted to know what to do. Taking a purely moral position, it was quite wicked to just walk off and take the University of California's idea and then go and execute it oneself. [Laughter] But I think that was considered as a possibility. We were thinking seriously: What shall we do? And as a last resort, we would have had to do something like that.

MOY: Was that feasible for Caltech, financially and otherwise?

SARGENT: The operating costs would have been a problem. I don't think it was feasible, but when you're thrashing around, thinking what to do, and you've got 65 million bucks, you try and figure out what you're going to do with it. I guess it came to $70 million in the end.

In any case, I told you how in August there was a milestone when Vogt was asking us what we should do. And we said, "Great, build either two telescopes in one place or a telescope in Chile and a telescope in Hawaii." From an astronomical point of view, either would be a great idea.

Then, the next thing that I had anything to do with was the Keck Foundation itself. I guess in late November, early December, 1984, there was to be a meeting of the Keck Foundation board, where we had to formally ask for the money. A proposal was written by Neugebauer and Stone and me—it came to about ten pages—with help from the Development Office; their grammar isn't as good as ours, but nevertheless they try. All along, between August and late November, nobody had told us who the donor was.

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[1]Paul Ciotti, "Mr. Keck's Bequest: Caltech Vs. UC Berkeley in a Story of Academic Intrigue, Technological Breakthroughs and Astronomical Ambition," *Los Angeles Times,* May 24, 1987.

And the arrangement now was that the donor was to come along with some of his henchmen and listen to a rehearsal of the speech, because we were told that the money was in a foundation and that different members of the family didn't get along with one another. [Laughter] Again, I was to give the speech. I was to talk about this particular project, how it would benefit science; and Stone was to talk about astronomy at Caltech generally, including the Voyager missions, all of the space things that JPL [Jet Propulsion Laboratory] had done, all the radio astronomy that we did—give the impression that we were really into astronomy. And then I was to concentrate specifically on this particular thing. I think this was to happen on a Wednesday. In any case, on the Monday, I went to Vogt and said, "I cannot give a talk to somebody without knowing their educational background—what they know about the subject, how to pitch it." Even when I'm at a professional meeting, I have to go and sit among the audience before I can give a talk, because I really need to smell the crowd. So at this point, he relented. He told me for the first time—I think Stone already knew—that it was Mr. Keck and the Keck Foundation.

MOY: Had you heard of the name? Did that mean anything to you?

SARGENT: No. The name "Keck" was familiar, as that of a Caltech trustee, and there was a Keck building [W. M. Keck Engineering Laboratories, Keck Graduate House—ed.]. But no, it didn't mean very much.

So the performance was held in 114 East Bridge, and Mr. Keck brought along some of the other members of the board. And then, the next day, Vogt went along to the Keck Foundation, down in Los Angeles, and gave the talks that I and Stone had given, but did them himself this time. Robbie is a brilliant man, and he really likes to study what he's going to say. So the idea that you get other people to do it for you, and then you do it properly, is an excellent idea; actually, it works fine.

MOY: Did he use your scripts?

SARGENT: Yes. We don't have scripts usually; we have pictures, and then we talk about the pictures. So he showed the same pictures and talked about them, and elaborated on what I'd said. And they approved, giving the $70 million. It was announced in early

January 1985, when the project actually started.

MOY: And the idea of having a Hoffman telescope and a Keck telescope essentially went away when the Hoffman money fell through?

SARGENT: Yes.

MOY: From there, how did the agreement come about to go fifty-fifty [with the University of California] and establish CARA [California Association for Research in Astronomy] and so on? Were you involved in much of that?

SARGENT: No. That was purely of Vogt's doing, I think. Murph [Goldberger] may have been the front man for some of the discussions, but I think the actual legalistic side was looked after by Vogt, and maybe by Stone. Robbie is very good at things like that. We feared there would be resentment from the University of California, but in practice, at the astronomer level, there's been no problem.

MOY: To a certain extent, only time showed that the relationship would work as well as it has. But was there much fear here? You knew many of these people quite personally.

SARGENT: The vital thing for me was the meeting in the airport at San Jose, when the four senior astronomers from Caltech and four senior astronomers from UC got together and then decided that they would each try with their administration to go in one-quarter, three-quarters. At that time, we realized that they were, or we thought that they were, more realistic than the people at the University of Arizona, and much more farsighted than the people of the Carnegie Institution of Washington. They were like us. [Laughter]

MOY: At that point, had UC already committed to the segmented mirror?

SARGENT: Yes. When we went in, we knew that we were signing on with a segmented telescope. The decision to do that had already been taken. So we were not part of the wars that went on within the University of California, fortunately. If we had been, I think things would have been worse.

MOY: You mentioned the UCLA faction and the Berkeley faction. Was it bipolar in that sense?

SARGENT: No. There was one outstanding instrumental person at Santa Cruz and essentially all of the UCLA astronomers versus the rest on this question.

MOY: But it was primarily those two schemes.

SARGENT: They were the two schemes. I think subsequent events have shown that either would have worked. The Europeans are pushing the scheme that was rejected. I think there are some logistical difficulties, like, how do you carry around a piece of glass that is 10 meters in diameter and only a few inches thick? And how do you get it to the top of a mountain that is 2,000 miles away or wherever? And how do you get it safely into an aluminizing tank on top of a mountain? How do you build such a tank which is more than 30 feet across?

There are various reasons for rejecting such a scheme, because it's more cumbersome. But on the other hand, lining up thirty-six segments is not too easy, either. [Laughter] I think Nelson now would say his scheme's the best. The rubber mirror is the second best, and the Angel scheme of the University of Arizona, the spinning thing, is the third best. But all would work, if you put enough money into it.

MOY: And the Mauna Kea site—you said that that had already been selected.

SARGENT: Yes, and we'd already studied it as part of our discussions. There'd been telescopes there since, I guess, the late sixties. The U.S. National Observatory was beginning to make plans to make a large telescope, and they did a systematic study of sites both in the Northern Hemisphere and in Chile for their projects. They came down to comparing Mt. Graham and Mauna Kea, and their comparison showed clearly that Mauna Kea was better. So we had all this material. The one thing that we didn't know when we got into it was that the particular place on the mountain selected for the Keck site had not been tested. The top of the mountain is quite big; it's a very rounded top, with cinder cones dotted around, so that there are several tops, of which one is higher than all the rest. And that one is not allowed to be built on for religious reasons; there's a Hawaiian religion that involves the goddess of fire, Pele, who spent some of her time up there. You're not allowed to build on that particular place.

So the sites where existing telescopes were have been well characterized. Towers had been put up in places where telescopes hadn't been put up. The University of California had a guy from Berkeley install a tower with some measuring equipment on it. The thing that you measure is micro-thermals. You put very accurate thermometers at different places at the tower and measure the fluctuations in temperature that occur during the night. In a really good place, the temperature stays exactly constant. But if there are slight convection currents, then you get slight fluctuations. You can figure out fairly well, from such measurements, what the astronomical quality of the site is in terms of the size of the seeing disc.

Anyway, an incompetent guy from Berkeley had put up a tower, and the equipment on it didn't work. And early in the Keck project, we had to decide—the site having been chosen—how high to make the telescope. The received wisdom, until about five years ago, was that these fluctuations in temperature fall off as you go higher above the ground, because it's the ground that produces convection currents. And so you'll see some telescopes, like the Kitt Peak 4-meter on very, very high buildings. The towers can be used to give you an idea as to how high the building should be.

The equipment didn't work, and we didn't know whether to build the Keck telescope high or low. It was going to cost another $5 million to build it very much higher. In late '85, there was one last attempt made to get this equipment working. Something wasn't working; the recorder that recorded the signals wasn't working. They had to get a technician over from the mainland to fix it. They flew this guy out; they drove him to the top of the mountain. When he got out of the truck, the wind was blowing 100 miles an hour, and he was immediately blown flat on his face. He scrambled back into the truck and said that he would not get out again. [Laughter] And so there was a standoff, in which the driver said he wasn't going to drive down the mountain and the technician said he wasn't going to get out. Eventually, there was a bit of a lull in the winds. He got out. He fixed it. The next day, the wind was even stronger, and it blew the tower over. So the measurements were never made on that particular site. [Laughter] But it has turned out to be very good, actually. The first tests that were done on the real telescope show that it's excellent.

MOY: Do you know whom these sites were acquired from? Did the University of California handle most of that, or was that CARA?

SARGENT: CARA. I think the site had already been selected by the University of California. I think, to be more precise, it had been allocated by the University of Hawaii. The University of Hawaii had the whole of the top of the mountain made into an astronomical reserve, with permission to build so many telescopes. They went through all the environmental protection stuff, I think, before it became as fierce as it is now. And then they were in a position to dole out sites for consideration. Also, the University of Hawaii owns facilities at 9,000 feet, where the astronomers sleep and eat.

MOY: Is that where the headquarters are?

SARGENT: No, the headquarters are at 2,000 feet. The headquarters belong to us, in Waimea. But this joint accommodation, which is operated by the University of Hawaii, is halfway up the mountain, basically. And we had to buy into all of that.

MOY: But that was essentially pre-existing.

SARGENT: Well, it was easy to plug into. The facilities were extended; dormitories were extended, using our money, for example.

MOY: I'd heard that there was, for a time, some debate as to precisely where the headquarters should be, Waimea or Hilo. Are you familiar with that?

SARGENT: Very!

MOY: What was the debate there?

SARGENT: Well, when the project started in early 1985, an existing committee, on which we had some representation, called the something Coordinating Committee, became the Science Steering Committee [SSC]. I've been on that since it started in January 1985, and I've been the Caltech chairman for six out of the seven years it's existed. I have the job now until 1993. The SSC is supposed to give advice to the project on anything of scientific interest. So, for example, when it approved the technical specifications for the telescope, and when the engineers found that they couldn't meet a specification, then they would negotiate with us about relaxing it or changing it. So we had to deal directly with the project engineers, always. We've attended all of the engineering reviews of all the parts of the telescope.

Anyway, one of the questions that came up was where to put the headquarters. The Science Steering Committee was split down the middle, with all the Caltech representatives on the side of Hilo and the UC ones on the side of Waimea. We looked into all of the criteria—like, for example, ease of communications. It turns out that with a small microwave antenna on the side of the dome, on the side of the building, you could beam straight down to Waimea. So that was an argument in favor of Waimea; whereas you have to go around the corner to get to Hilo. The climate in Waimea is much more temperate, because it's at 2,000 feet. Hilo is a tropical, rainy, humid sort of place, which is a little uncomfortable. The road to get up to the top of the mountain from either place is not very good; it's probably slightly easier from Hilo. The public schools in Hilo are better, but the private schools are worse. There's an excellent private school in Waimea, called the Hawaii Preparatory Academy, which, of course, you had to pay to go to. And there are stories about non-native children being beaten up in the public schools. All sorts of things. [Laughter]

Then there's the question of housing. The housing's much cheaper in Hilo. So, if you hire a technician, first, there are more people to choose from, and secondly, they can afford to live there. [Laughter] So when we discussed it here, we were quite convinced that although Hilo was not such a nice place to live if you were well off, nevertheless, you could run a better operation from Hilo.

The University of California thought quite the reverse. Actually, I think they agreed with what we said, and we could never understand why they wanted Waimea. And finally the issue was settled when Goldberger agreed with [David P.] Gardner, the president of the University of California, that Caltech would agree to go to Waimea. At this point, for Vogt, who was the provost, it was the last straw of many disagreements with Goldberger, so he said he would have nothing more to do with the Keck project, even though he was provost. And he never has. A few weeks or months later, he was dismissed from being provost [1987]. Goldberger and he just couldn't get along anymore. I think that Murph's agreeing to go to Waimea was one of the big disagreements they had.

Then, after a few months, Gerry Neugebauer and I talked to Kraft and the other co-chairman on the UC side, Joe Miller. We had dinner sometime after a Science Steering Committee meeting, and they finally told us why they wanted to go to Waimea. It was an amazing business. There's a thing called the Parker Ranch, which you've no doubt heard of. One of the first Europeans to go to the Big Island was [John Palmer] Parker, on a ship that landed in the early 19th century. He married a Hawaiian princess. As a result, he eventually became the owner of a large part of the Big Island—all of the northern slopes of Mauna Kea, the area around Waimea—a huge amount of land. I think it's either the biggest or the second biggest property in the whole of the United States under one control; the King Ranch in Texas is the only competitor. The Parkers became very rich, although they own more hypothetical money than real money.

But eventually, the money came into the hands of a man called Richard Smart, who is a Broadway actor who is now in his seventies [d. 1992], and he lives in Waimea but goes off to Broadway to act in musicals and comedies from time to time. And he had one child, a man called Gil Smart. This Gil and his father own property which I last heard was valued at $300 million. A lot of it has gone into the hands of a trust [Parker Ranch Foundation Trust], probably for tax reasons, I think.

But there was a possibility that this trust would give money for another telescope. And the University of California was negotiating on the sly with the Smart trust, after the Keck decision was made, to build a second telescope. The condition would have been for the headquarters to be in Waimea, because that's where the Smarts own their property.

And in fact, the Smart family gave us nine acres of prime property in Waimea as an inducement to go there. The University of Hawaii at Hilo, in turn, offered us land to go there. So we wouldn't have to pay to go to either place, but the Smart offer was very generous, in terms of actual money, if we were ever to turn it into money.

Apparently, there were three trustees involved in this Smart trust money, and at one point, UC had two of them willing to vote to give the money. But they needed all three to get money for another telescope. So they came damn close, but they didn't tell us. And nothing came of it.

MOY: And this would have been for another 10-meter?

SARGENT: Yes. Which eventually has happened, but through different auspices.

One of the other things that happened unofficially is that Vogt instituted the idea of talking to the project manager once a week. The project office used to be across the street here on Wilson, and then it moved to Hawaii, two years ago. But there was always a difficulty: How does CARA, which only meets once every six weeks, keep control of the project from day to day? The project manager, a former JPL guy [Gerald M. Smith], is very strong-willed and has very definite ideas on what to do. Smith only respects people who are clearly above him on the ladder, and the provost at Caltech clearly was. So Robbie had the idea that once a week, Smith, Neugebauer, Stone, and I and he would all meet, and Smith would describe what had happened in the project that week. This would happen every Friday afternoon at four. And that was the only way in which the project was monitored from day to day. The University of California, of course, had nothing to do with this scheme. They eventually learned that it was going on and just never said anything about it.

So, one day, I think it was in October 1987—I would need to check the date—I received a telephone message: "Dr. Vogt will not be holding the Keck Friday meeting today. There will be no more meetings." That's when he'd had the row with Goldberger about Waimea. That was the end as far as Robbie was concerned. So then Stone, who was the division chairman, continued to have the meetings. And they've gone on without stopping all through the project, but they're not official. Now they're held by telephone, a conference call from Hawaii to JPL and Caltech—Neugebauer, me, Stone, and Smith. We talk every Friday afternoon.

MOY: How has the administrative side of things run in your estimation? Has it been pretty smooth?

SARGENT: I think very well. They chose a very good person in Smith. Smith would be an ideal person to run a concentration camp or something. He is tremendously well organized, chooses people well who will work under him—as long as they work under him, he's happy. And he has very good judgment about people, and he has very good judgment about what to do. Once he gets confidence in somebody, he's a very good colleague. But it sometimes takes a few years for him to achieve this confidence. [Laughter] He doesn't like scientists. In fact, every Wednesday, there's another meeting, which is official, that's called the Project Staff Meeting. And for about two years, the Science Steering Committee tried to get one of us to go to the Project Staff Meeting. And after two years, Smith finally agreed. So, for several years, I would go over to the project office every Wednesday morning and sit with the engineers for about an hour and a half. They would each report to Smith on what they were doing, and I would get to say what the scientists were up to. I got to know all the engineers well through that. But he didn't like it, to start with. Eventually, he liked it. But that was also a main sort of intersection between the science and the engineering.

For these other meetings, the Friday meetings, which officially don't exist, I've kept a record of all of them. And what happens is that Smith would produce a piece of paper, which would summarize what he was going to talk about. Nowadays it's faxed over so you can read it maybe a few minutes before the telephone call starts. I've also kept a record of what other people have said. But sometimes really critical things have been discussed on Fridays that have not been mentioned outside, like when we were in trouble with Itek [Optical Systems] over the mirror—things like that.

MOY: That's fascinating. I'd been curious as to how the day-to-day operation had actually been managed.

SARGENT: Well, a person who's in charge of a thing like that really needs to have a sort of father confessor, particularly when a guy's stuck out in Hawaii. Whom does he talk to if he's got some personnel problem, or if the mirrors are not working properly and he doesn't know what to do? You know, often tossing around ideas with other people is useful, even if they don't contribute anything.

WALLACE SARGENT

SESSION 2

July 17, 1991

Begin Tape 2, Side 1

SARGENT: On the invention of the name CARA: Robbie Vogt called me at the time when the discussions were going on with the University of California and asked me to come up with a name for the joint corporation, which he was in the process of setting up. The people at the University of California were enamored of the name Ten-Meter Telescope—it used to be known as the TMT. They wanted—as Robbie told me—something like the “UC-CIT-TMT Corporation,” which was extremely ugly. Robbie asked me, in my capacity as an expert on the English language, to come up with something better. So I immediately thought of CARA and sent him a note on a scrap of paper. It’s based on AURA, the Association of Universities for Research in Astronomy, which runs Kitt Peak. So I thought, “California Association for Research in Astronomy” was just what we needed. I was partly influenced by the John le Carré spy novels in which the Russian spy is called Karla, which is almost the same. I particularly liked that name. [Laughter] So I sent him a note saying this was it.

A couple of days later I got a call from a secretary, asking me what C-A-R-A stands for, because they were now going to produce the official letterhead. Robbie had just said it was going to be CARA but hadn’t explained what it stood for. So I explained. That’s how it came about. I’ve noticed that there’s an Antarctic consortium which has copied the name. I’ve forgotten now what the acronym stands for, but it’s something like Combined Association for Research in Antarctica, or something like that.

MOY: That brings to mind a previous question regarding CARA. Was there some sort of cooperative entity in the works prior to the Keck money? Since Caltech had been involved, there must have been some thoughts of an arrangement even before Howard Keck came in with the $70 million.

SARGENT: As far as I know, there was not. It was only after the money was got that the constitution was thought of. After all, the original arrangement, or intention, was for us to only get a quarter of the action. In that case, whatever organization would have been set up would have been very unequal. It's equal in its present manifestation because UC provides all of the operating money for the first twenty-five years. But it wouldn't have been equal at all; it might even have been a University of California operation.

MOY: What is supposed to happen in twenty-five years?

SARGENT: Caltech is supposed to then raise half the operating costs. UC will raise half and Caltech will raise half. And I presume that if Caltech fails, they would have to bring in another party, or sell the telescope or something. But it will create problems in twenty-five years' time.

MOY: My next set of questions deals with the design and construction of the telescope. Who would you say have been the principal designers of the telescope, aside from Jerry Nelson?

SARGENT: It depends at what level you're looking. I think that Nelson and his group had sketched out the main idea before it was handed over to the engineers. But after that, there was a lot of design work on the actual telescope structure—the moving part of the telescope—done by a man called Steve Medwadowski [UC Berkeley]. Otherwise, the work was handed over to companies under the supervision of an engineer at CARA. So there was an engineer in charge of the mechanical stuff, like the dome; there was an engineer in charge of the drive and control system—DCS as we call it in the trade. Different companies did the work. The dome and building design was done by a company called MBT from San Francisco. The dome was actually built by TIW—Toronto Iron Works, in Canada. But I don't have in my mind any very prominent individuals. It was more a case of bringing together the skills of a lot of engineers.

The trickiest part of it was the mirror-support system, which was done at LBL [Lawrence Berkeley Laboratory], where Nelson was at the time when the whole project started. I'm not entirely clear how this happened, but there was a group of people—engineers who'd been used to working on other kinds of problems—who were brought together to work on the mirror-support system in particular. How Nelson did that I'm not sure.

The mirror-support system was divided into two parts—the active support system and the passive support system. The passive support system is a pillar that fits onto a diaphragm, which is in the central hole. That just sits there, and the mirror can rock on the diaphragm. The diaphragm is a very thin piece of metal, which you can easily displace by tilting it, but you can't displace it sideways; it's very stiff. And then the active support system consists of the whiffletrees. They're very stiff in the vertical direction but very compliant in the horizontal direction. So these two support systems, one of which moves, and the other one which doesn't, are not supposed to interact with each other; they have these two orthogonal properties for that reason.

Different people were involved in the design of the two parts. You had the impression at design reviews that there was not much communication between the active people and the passive people. But it did work out. It all came together fine.

MOY: It's all controlled by microprocessors, isn't that correct?

SARGENT: Yes.

MOY: And the tolerances are just staggering. The controls for the mirror segments is on the order of millionths of an inch, is that correct?

SARGENT: I can't think in terms of millionths of an inch. The smallest steps that are made are 4 nanometers; a nanometer is 10^{-3} of a micron. The computing involved at the time of the design was quite a severe test of computing power, particularly since the different things have to move simultaneously. Special programs had to be written which would allow many operations to be conducted through one program.

MOY: Each mirror segment has its own microprocessor, right?

SARGENT: Yes.

MOY: It's the Motorola chip, isn't it, used in the Apple Macintosh?

SARGENT: Right.

MOY: But there must be some central processor.

SARGENT: There's a central processor which tells these things what to do, a MicroVax. But the main cleverness is in the program that does the operation. It uses a system called VX Works, which enables you to carry out many operations simultaneously with a central computer.

MOY: I think I saw somewhere that the term "whiffletree" comes from—

SARGENT: Horses. If you have four horses pulling a single carriage, running at slightly different speeds, you want to equalize the pull. There's a contraption of pivots, basically, that enables you to do that. I don't know who invented that term, actually. In fact, I think I once asked Jerry Nelson. I think it had been used for flexible mirror supports before, and the term was already in existence in that particular kind of work.

MOY: The procedure for actually making the mirror segments seemed surprisingly complex to me. Could you summarize the overall procedure for turning thirty-six flat hexagonal pieces into one large paraboloid?

SARGENT: Well, first of all, you start with circular blanks. And they are made, at the Schott company in Germany, already slightly curved, so that not much additional glass has to be taken off. The glass that is used, ZERODUR, has this very low coefficient of expansion and is very hard to make; it's very expensive. The original blanks cost us $100,000 each. The next lot is going to cost even more, because of the switch in the dollar-to-mark ratio, as well as inflation.

Tests were carried out at the University of California on how you were going to end up with hexagonal segments which form—it's not actually a parabola, it's a hyperboloid of revolution, a so-called Ritchey-Chretien system. A parabolic telescope is simple in that the primary mirror is a parabola and the secondary, if you have one, is flat. So it's easy to make, but the field is small. The Ritchey–Chrétien system enables you to increase the field by a considerable amount, probably multiplying by almost a factor of 10 the useful area that you can get in focus. But it means that the primary is not a parabola; it's a hyperbola, and so is the secondary. So it's much harder to make.

You then want to make all of these thirty-six segments come together at the same focus. That itself is difficult; the tolerance on that is less than a tenth of a millimeter on something that has a focal length, or at least a radius of curvature, of 35 meters. So that was anticipated to be a difficulty.

Now when the system was tried, Nelson had one mirror polished and then used a part of another mirror—one side of what would have been another hexagon—next to it, just as a test of the whole mirror-support system and of the actuators and sensors, which enable you to move one mirror and keep one mirror next to another.

While this first hexagon was made at Tinsley [Laboratories], it was discovered that when you cut the circular blank into a hexagon, it distorts. And the reasons for the distortion is that there are stresses in the glass that are released when the cutting operation occurs. There's no way of doing the cutting that's been figured out that will save you from the distortion.

MOY: And there's no way to predict the stresses either?

SARGENT: Well, the stresses can be measured to some extent by measuring the birefringence in the glass, because stressed glass has the property that right and left circularly polarized light travels through it at different velocities. If you measure the birefringence in many places over the mirror, you can produce a rough idea of what the stresses are. But you can have a situation where, for example, the top side of the glass has the opposite birefringence of the bottom, so the stresses make the birefringences cancel out when the light goes all the way through.

There is a specification on the stresses as measured by the birefringence and the blanks as they're bought. So, it was decided to try and predict the stress from the birefringence and then make the mirror slightly wrong so that it would relax to the correct shape. That, to some extent, works, but there have been gross departures from the normal behavior in the following sense. Of the thirty or so that have been polished, two of them, instead of becoming more concave when they were cut, which is the normal rule, became more convex. So not only was the prediction of the *magnitude* of the effect wrong; the sine was wrong also. [Laughter] It was known that this problem was going to exist. It was known that you could reduce it by buying glass that was as close to strain-free as possible, and that was done, but still the problem existed.

Now, you start off in this game by polishing a sphere onto a blank that has weights hanging around the edge, and then the idea is that it's easy to polish a sphere. Moreover, you can make all the spheres have the same radius of curvature. And tested, they do. Then, when you let the blank go, it will relax to a part of the hyperboloid of revolution. In the thirty-six mirrors, there are six different shapes that result from these configurations—six different arrangements of weights around the edge of the mirrors that have to be used.

MOY: This is known as stressed polishing?

SARGENT: This is stressed polishing. Nelson revived that. It had been used in optics before, but not on such a large scale.

MOY: For telescopes as well?

SARGENT: Not as far as I know, but for other complicated optical surfaces.

So the stressed polishing was tested. That worked fine. The problem that was left when we went into production was this question of how much the mirrors were going to be distorted as a result of the cutting. Various schemes were entertained for getting around this. It was thought at one time that if you cut using a high-powered jet of water, maybe the effect would be different than if you used a saw. There was a test done with the water jet at Itek in which the jet was so powerful—the water was being caught in a bucket—that they boiled the water in the bucket, just due to the impact of the jet. And it turned out that didn't work.

MOY: What was the rationale for thinking that would work?

SARGENT: Well, it's different from sawing at something with a rotating diamond, for example. The hope was that you wouldn't generate as much heat that way, for one thing. But it turned out that it didn't work.

Another idea that was considered was to cut the thing into a hexagon before you started, and then glue the ears that you'd cut off back on again. And then polish the thing; and then somehow get the glue off. But that was the problem, because in order to do the whole operation, the epoxy was going to have to be on so tight that you would introduce strains into the glass getting it off. So that didn't work.

Nelson, all the time, thought that one solution would be to deliberately put unequal forces on the back of the mirror in the telescope through the whiffletrees, rather than equal forces, which the whiffletrees are designed to produce. So holes were put in the support system at the appropriate places so that extra springs could be inserted later if the need arose. These are called warping harnesses—leaf springs. And they were designed in as a possibility from the word go. So it wasn't a scramble, when a problem was discovered, to invent this. It was in Nelson's mind, and in the minds of the people in the project, that they would do this if worst came to worst. And they are being used.

Itek was chosen as the company to carry out all of this, even though the preliminary work had been done at the Tinsley company in Richmond, California, which is closer to us.

MOY: Itek is in Massachusetts?

SARGENT: Lexington, Massachusetts. It is a high-tech company and does Star Wars–type stuff, stuff for the military. Their original proposal was to do the whole job for around $11 or $12 million. They were the lowest bidder, and part of the reason was that they already had some machinery that was going to be used in the operation, which would have had to have been bought to work at Tinsley and at some of the other companies. There are machines for accurately boring the holes in the backs of the mirrors for different components, for example, which were already there at Itek; they were very expensive. So Itek was chosen. They were the ones who got into trouble when the distortion due to the cutting occurred.

First of all, they had difficulties in carrying out the stressed polishing, and those took a long time to be cured. Then there was the problem with the cutting. Their proposed solution was to use computer-controlled polishing with a small tool. Stressed-mirror polishing is done with a tool, which is as large as the blanks. They were going to cut the blank into a hexagon and then use a tool about 4 inches in diameter, controlled by a computer, so that you could remove different amounts of glass in different parts of the mirror and also polish into the corners of the hexagon. That turned out, when it was tried on one segment or two, to introduce small-scale irregularities in the glass that were not there as a result of the first stressed-mirror polishing. The interferogram should have straight lines across it, and the interferograms before this operation showed gross, large-scale distortions in the mirror; the lines weren't straight. But after the computer-controlled polishing, there would be a lot of ripple in the mirror so that an interferogram would now be the right overall shape, but it had a small scale jiggle on it, which is really unacceptable.

At this point, the computer-controlled polishing was abandoned after one try, and it was decided to go for the warping harnesses instead.

MOY: Had the computer-controlled polishing been in Itek's original proposal?

SARGENT: Yes, that was another reason why they were chosen. They claimed to have had expertise in that in their military work. But I think that it was not possible to look very hard at their results because of the military aspect.

MOY: Did you get the impression that they simply hadn't had to work within these tolerances before, for this sort of procedure? Or that they actually hadn't had much experience in it?

SARGENT: It's hard to say, really hard to say.

MOY: Wasn't there a third major competitor after Tinsley and Itek—the people who made the Hubble mirror?

SARGENT: Yes, Perkin-Elmer. Another company called Contraves-Goerz in Pittsburgh also bid. And Kodak, I think. There were at least four, maybe five, competitors for the job.

MOY: Some of the stressed polishing was side-contracted back to Tinsley, is that right?

SARGENT: Yes, well, Itek was not doing it very well, so it was decided that we needed to go faster. Tinsley had had some experience on the trial that Nelson carried out at LBL, so they were used just to do stressed-mirror polishing. They were much cheaper than Itek; their overhead is much less. The overhead at Itek is something like 210 percent. And Tinsley was willing to do some of the things for a fixed fee, whereas everything at Itek is cost plus a fee, which depends on how well they're doing; they quickly brought the fee down to its lowest possible level by their initial delays.

So Tinsley set up two stressed-mirror polishing fixtures. But they used completely different principles from the Itek ones. The Itek ones have the conventional system, whereby the blank is rotated and the polishing tool moves only in a radial direction—it only moves backward and forward. Tinsley decided that it would be better to have a fixed table and have an arm that could move both in the radial and the transverse directions, so as to simulate what you would get if you had a rotating table and a radial motion of the arm. This was worked on by computer. In addition, they set up two tables and one arm, which could work on either table; it could swing around in two different directions. In my view, this was a mistake, because it took them a long time to develop this completely different system. If they'd just copied the original one, it would have been better.

MOY: Part of the reason for going back to Tinsley was the delay, wasn't it?

SARGENT: It was the delay, yes.

MOY: Was it a good idea, do you think, to go back to Tinsley?

SARGENT: Well, it was a good idea in the sense that at about that time, Itek started to get their act together. It may be that they would have got their act together anyway. The mirrors would certainly have taken longer, and having two or three times the through-put has certainly been an advantage. But building another table at Itek may have produced just the same effect—it's hard to know, in retrospect. But it has certainly been cheaper to go to Tinsley.

Jerry Smith, the project manager, has kept a record of the time it's taken to polish each segment, and you can see in the case of Itek a clear learning curve. It started off at some extremely long time, like sixteen weeks, and it's gone down gradually to something like five. It was a very well-defined curve. And if you plot the Tinsley data, they seem to have followed exactly the same curve at the same speed. So they're now getting to where Itek arrived at, but they are producing more mirrors, because they're polishing on two rather than one. But it doesn't look, from that point of view, that Itek was all that inefficient; it's just that it's a procedure that is very hard to learn.

Begin Tape 2, Side 2

MOY: Were there technical problems that turned out to be more difficult than you had anticipated?

SARGENT: Well, making the mirrors was thought from the word go to be the primary difficulty ahead. Whether or not the mirror support system would work was thought to be the second difficulty; that doesn't look as if it really is of the same magnitude as the mirrors. After that, there haven't been all that many surprises. The biggest one to me is the fact that we couldn't make a successful mirror cover for the primary mirror. Most telescopes have some sort of cover that you can put over the primary mirror so as to keep dust off it and things from dropping on it when people are working high up in the dome. The normal cover is an arrangement of butterfly pieces that come down and mesh together. The 10-meter is too big for a conventional cover, particularly the way the elevation ring is designed.

Fairly late in the game, this outfit called MBT Associates, from San Francisco, thought they'd designed something that would work: sort of a stiff canvas, which, like the sail on a ship, would be on arms that come out and rest on the central tower in some way. That has proved to be a complete failure and has been taken out. So it's the only telescope in the world of any size that doesn't have a way of covering the primary mirror.

MOY: And there's nothing in the works?

SARGENT: No. In fact, for the second telescope, we shan't even try.

MOY: Have there been any technical problems that have been easier to solve than you had anticipated?

SARGENT: I can't think of anything. I think the dome track is better than people expected; the track that the telescope moves on is better than expected. I guess we've yet to find out about the tracking; that might turn out to be very important. The dome motion, I don't think, is really a major thing. But you do notice, if you're in that dome when it goes around, that it doesn't shake and grind like many modern telescopes do. The 200-inch moves so smoothly that you wouldn't know that it was moving, and this one is the same.

MOY: Could you describe what it's like working on Mauna Kea?

SARGENT: Well, almost everybody finds it difficult, and that has led to major difficulties in getting things done. The people assembling the dome and the telescope sometimes wouldn't stay the course.

It's very hard to predict who can work up there and who can't. There are the usual stories: a guy was sent up from Hilo to paint something. He was a marathon runner, yet when he got up there, he couldn't function at all. I find it necessary to do breathing exercises when I go up there. I did Lamaze classes when my daughters were born, in which there are lessons in how to breathe during childbirth. The idea is to get more oxygen into your lungs. You breathe in and hold it; and then you let it out through pursed lips. And if you feel slightly peculiar up there, if you breathe in this way, it makes a big difference.

MOY: Have you tried demonstrating this to others?

SARGENT: Well, we were told that by a doctor. Several of us have tried it, and we all agree that it's an improvement. And people's mental faculties are not as good as they are at sea level—that you notice quite easily. In my case, I find that sentences come out with the words in the wrong order. I'm completely conscious of the fact, but it's too late to stop it.

MOY: Although it was very much a team effort, are there certain contributions that can be identified as coming from UC, or Caltech, or the University of Hawaii?

SARGENT: Well, I don't think that the University of Hawaii has contributed very much at all. In fact, I think they've mostly got in the way, through their bureaucracy, which is very inefficient. As for UC versus Caltech, it's very hard without a lot of thought to give a pocket summary. But I would say that within UC, not many of the astronomers have been terribly interested. I think I mentioned to you the UCLA problem. Basically, nobody at UCLA has taken any interest.

MOY: And that's still fallout from the earlier debate.

SARGENT: Yes. In fact, they thought it wouldn't work, and they have continued to assert that it won't, ever since. [Laughter] There is one astronomer who moved from the University of Hawaii to UCLA about two years ago, Eric Becklin, who is very interested in the project and comes to the meetings. But none of the people who've always been at UCLA will pay much attention. At San Diego, at Berkeley, and at Santa Cruz in particular, there are little pockets of people who really have put a lot of effort into it. But the average UC astronomer hasn't.

MOY: Do some of these people, like those at UCLA, expect to come in out of the cold at any time?

SARGENT: Oh, yes. As soon as it's working, they'll insist that they have rights to use it, and they will be elbowing other people out of the way. How UC will deal with that, I don't know. But at Caltech, a much greater proportion of the astronomers have been forced to pay a lot of attention.

MOY: My impression is that almost everybody has worked on it at one point or another.

SARGENT: Yes.

MOY: Do astronomers always want bigger telescopes? Or was there something new in the middle or late seventies that made people think that now was the time for a bigger telescope?

SARGENT: I think the answer is no. Hale did consider making a 300-inch telescope at the time the 200-inch was built; it was sort of a design sketch of a 300-inch. So I think it's only been technical difficulties that have held people back, not the thought that you didn't need them. And the ability to make lightweight mirrors was the thing that made it all possible. As we discussed earlier, three schemes have been produced. One is to have this very thin, flexible, floppy mirror that you support with many actuators under it. The other is Nelson's scheme. And the other is the University of Arizona's scheme, where you build an egg-crate-like structure, which is very stiff and light, with a rotating mold. Most of us think that any of these schemes will work if you put enough effort into it. So the problem was more technical.

MOY: There weren't any new, intellectual problems in astronomy driving for larger telescopes?

SARGENT: Not to me there weren't. In fact, when we had our discussions in 1983 and 1984 about what we wanted to do, I remember having to start with the quite strong view that what you really needed was a good site—dark, with very good seeing—and a telescope of around 200 inches in aperture, and a lot of access to it. But I didn't think the size was very important, and to some extent, I think that's still true for almost all of the work. What counts is the product of the number of square inches of glass and the number of nights you can get it. If you can have a 200-inch telescope all to yourself, that would be as good as a 10-meter for part of the time.

But I did find at that time, and I've found more since, that there were various observations I wanted to make in my own work on quasars that I've just not been able to do with a 200-inch telescope, even having a fair amount of time.

MOY: Because of time?

SARGENT: Yes. There are one or two where the rate at which the data comes in means that it gets drowned out by noise in the detector; there is a sort of fixed lower limit to the noise of the detector. With CCDs—charge-coupled devices, the silicon devices with very high quantum efficiency that we use nowadays instead of photographic plates—for example, you can make exposures that last roughly an hour. Now, those devices get hit by cosmic rays. And if you expose them for longer than an hour, the number of cosmic-ray events in the picture—or spectrum or whatever—starts to get unacceptable. So you really have to make do with whatever photons you've gathered during that hour. Then you have to compare that with the readout noise of the device itself. Once you get into a regime where the signal coming in is getting to be comparable to the readout, then you're really in trouble. And you can't just expose longer on the same thing. So, in that case, a bigger telescope definitely gets you someplace.

Some of the work I do on quasars is in that regime. I'm particularly interested in looking at the absorption spectra of quasars that are close together in the sky, to study the sizes and physical properties of the intervening objects, so that you get two lines of sight through two sides of a distant galaxy, for example. Quasars are sufficiently rare that to get two of them within a few arc seconds of one another is very unlikely, but it gets more likely if you go fainter. So I'm driven to having to work on, say, 21st- or 22nd-magnitude quasars that are close enough together in the sky for my particular interest. There are several important measurements that I have just not been able to do with a 200-inch telescope that could be done with a 10-meter.

MOY: So there are problems that really need a telescope this large. There are a number of astronomers who have indicated that one of the biggest problems is simply the crunch for time among astronomers who have to share a telescope, not the technical time problem you've described.

SARGENT: Well, I think only the astronomers who have had a lot of time on a really big telescope would have found that there are things they can't do and have bumped into this problem. So it's not a very large fraction of the community. It took me some time to realize that there was a wall I was getting to. And the reason it took a long time was that for maybe ten or fifteen years between around 1970 and the mid-eighties, the detectors kept on getting better and better and better. You could do better and better without having a bigger telescope. But then it was clear that the detectors were getting to a point where they could theoretically never become better. So then, the size of the telescope started to limit you. But for a long time, people were happy to be getting twice as much data with a telescope that they already had.

MOY: In terms of time, how long would data gathering take on the 10-meter as opposed to the 200-inch? It's a factor of 4?

SARGENT: Yes, a factor of 4 for most measurements. That's a hell of a difference. Moreover, certain observations require extremely special conditions in which the images are as small as it ever gets, and for which there's no Moon, and the sky background is long because there have been no solar flares for several days. There are all sorts of things that conspire together to give you extra good time. Now then, that might only last, say, two hours. And if you're there, then you can do it. But if the telescope required eight hours to make the same observation, you would be up shit creek, as it were. So I think the best results that we will get from the 10-meter will be better than four times as good as the best results we get from a 5-meter.

MOY: Do you recall what the anticipated goal was for the image resolution when it was first specified, back in 1985?

SARGENT: Yes. You should understand that the specifications were given two names: one was specifications, and one was goals. Basically, the project manager made an initial cut as to what he thought would be achievable, and these were called specifications. The things the astronomers wanted that might be difficult were called goals. So there was never a specification for the image quality. But the *goal* was that eighty percent of the light would go into 0.32 of an arc second for the primary mirror alone; and the secondary was thought at that time not to introduce any additional problems. That meant that fifty percent of the light would go into something like half that, 0.16 of an arc second. Normally, when astronomers quote image size, they mean full width, half maximum—fifty percent of it, roughly.

We didn't get eighty percent of the light into 0.3 of an arc second. We still don't know exactly what the number is. It certainly is going to be better than 0.16 of an arc second, because already images have been got which are that good. And taking into account the seeing—the fluctuations in the atmosphere—the contribution of the telescope is less than 0.16 arc seconds. So that was the original goal.

MOY: And now you're expecting—?

SARGENT: I think now that we'll get something like 0.5 of the light within about 0.25 of an arc second, and eighty percent of the light within 0.5 of an arc second. I think that's a very conservative estimate of what we'll achieve.

MOY: And just for comparison, what is the resolution at Palomar?

SARGENT: Well, I don't know what the mirror is capable of producing, but there the best images that are ever obtained have something like fifty percent of the light inside 0.8 of an arc second at visible wavelengths. This is a considerable improvement, but the site is much better.

MOY: And for the Hubble Space Telescope, as a point of comparison?

SARGENT: It was to put fifty percent of the light into more like 0.05 of an arc second. The Wide Field Planetary Camera, which is the main picture-taking device, is much worse than that, because you have to strike a compromise between having very, very good images but a small field, or somewhat worse images and a bigger field. It depends on how well you sample the image. I think the Wide Field Planetary Camera will get, when the space telescope works properly, fifty percent of the light inside 0.15 or 0.2 of an arc second. It was about what we were aiming for. But there'd be a big difference, because the space telescope would achieve that every day. It would be a hell of a difference. Also, the image profile would be stable and predictable, so you could de-convolve it with great confidence and get higher resolution, whereas through the atmosphere it's much more difficult. But we were aiming at something which is not all that much worse than the space telescope.

MOY: You may have answered this already, but just to elaborate a bit: When you first get time on the telescope, and presuming you're going to have great seeing, what will you look at?

SARGENT: Well, I shall do two things. One is, with the low-resolution spectrograph, I shall look at certain faint pairs of quasars that I found in conjunction with another astronomer in England five or seven years ago, which would answer key questions about the sizes of the Lyman-alpha cloud, but that are too faint to be studied with the 200-inch. That would be my first thing. And then with the high-resolution spectrograph—which I worked on with Steven Vogt at Lick and will be a very impressive device—I shall work on the compositions of the objects that produce the quasar absorption lines, using unprecedentedly high resolution. I'm not exactly sure which quasars I'll observe for this particular purpose, but I have a program in mind.

May I say one more thing? On this second problem, some work of this kind has been done in Australia, using exposures that last about ten hours on a 3.6-meter telescope. And I figure that we will be better by a factor of 20, and so it'll really make half an hour worth ten hours. So you can really do this sort of work seriously with less time.

MOY: How will Keck II differ from the first telescope, if at all, in terms of design or construction?

SARGENT: I think there will be small changes, cosmetic changes. For example, due to a misalignment of axes, an air conditioner was put in front of the window through which the telescope operator can see the telescope. [Laughter] I guess that will be fixed. But I don't think there'll be any major changes to the optical system.

The main thing we're considering is whether to do the infrared focus the same on the second telescope. And there are discussions that are going to take place in Hawaii on Saturday about this. Right now, the infrared work is put in an f/25 focus, which is on top of that central pillar in the middle of the telescope. That's very hard to get at, and it's hard to get liquid helium up there. The reason for putting it ahead of the primary mirror is that you want to have as little between you and the external universe as possible, because everything—the telescope itself, the supports for the prime focus—they all radiate strongly at infrared wavelengths. And so you want to get this focus in a position where it sees the sky and nothing else, basically—except the primary mirror, of course. There are considerations to change the specifications of the telescope for infrared. But I don't know. Since we have it under discussion, I don't know what the solution will be.

MOY: Will there be any management changes that you anticipate?

SARGENT: Not until the second telescope is finished. So the project manager will stay on as the overall head, and an operations director for Keck I will report to the overall project manager.

MOY: Were you surprised when Keck gave the money for the second telescope?

SARGENT: There had been rumors and hints for a long time before. It started when the Keck Foundation asked us, around 1988 or so, to put in a proposal for something that would raise the possibility of a second telescope with their board. Mr. Keck had asked for this. This information was given to Neugebauer and me, and we felt the only thing that we could come up with that would bring the idea of two telescopes in for a fairly small amount of money was interferometry. We had just hired [Shrinivas R.] Kulkarni, who's an associate professor of astronomy, to work on radio interferometry. He'd gotten interested in optical interferometry. And another Caltech radio astronomer, [Anthony C.

S.] Readhead, had gotten interested in applying techniques that are well developed in radio astronomy to optical wavelengths.

So we wrote a proposal to the Keck Foundation to support the work of Kulkarni and Readhead in applying radio techniques to optical astronomy on the 200-inch. And they gave us $230,000 to pursue these studies, some of them theoretical, some of them practical. But it was at their suggestion.

As part of this work, Readhead used programs that have been used for designing the Very Long Baseline Array in radio wavelengths, and exploring what you could do with a radio telescope on a satellite, working in conjunction with radio telescopes on the Earth: What would be the optimum orbit of the satellite? These programs were adapted to figure out what you could do with two optical telescopes and what arrangement of smaller telescopes you would want to put around these two telescopes. So Readhead was brought into the secret—that there was now a possibility of a second Keck telescope. [Laughter] We wrote a proposal. And then we gave Kulkarni the money, but we didn't tell him where it had come from. A report was written for the Keck Foundation on experiments using the 200-inch for optical interferometry, and on these theoretical studies of what you could do with two telescopes in Hawaii. For some time, we were told not to mention this; the Keck Foundation was very anxious that word of a second telescope should not leak out.

MOY: Do you know why?

SARGENT: No. They're a very funny lot. But a few people were in the know, and gradually it became a larger number. And it began to be discussed as a possibility at CARA board meetings, for example, in a disguised sort of way. There were one or two people within the University of California who knew that this was a possibility. There were a few of us at Caltech, but the ones at Caltech were instructed not to say anything. It was never mentioned in meetings between the astronomers on the team. The whole business was very peculiar.

Then one day in, I've forgotten, 1990 or so, Maarten Schmidt and I were sitting out, drinking our tea—being Europeans, we spend a lot of time idling away our time—we were sitting out in the mall outside, drinking our tea. The Keck Foundation representative to the CARA board came by, a man whom we had known for years, Julian von Kalinowski, or Kali, as we call him. He came and sat with us and said, "You know, it's really great that Mr. Keck wants a second telescope."[2] [Laughter] And we'd been told not to say anything, and he just comes out with this. He then told us a lot about what the plan was. But we didn't tell anyone that we'd spoken to him.

But even with all this gradual introduction of the possibility of a second telescope and the rumors coming back—"Yes, Mr. Keck's interested, but now he's in a lawsuit with his family this week"—when it actually happened, it was a hell of surprise, particularly the amount—eighty percent of the cost. A hell of a big donation!

MOY: I had heard that at higher levels in the Caltech administration there had been a fairly heated debate over whether or not to try to get all of the money for the first telescope out of the Keck Foundation—that is, to try and get the full $90 million or so, as opposed to the $70 million. There were people who were strongly against using any endowment money and said that Caltech could definitely get the remaining money out of Keck if we just squeezed him a little more. And others said, "No, it's not going to happen." And now that there's going to be another telescope, it seems as though it might have happened.

SARGENT: I guess there are several answers to that. One is, the Keck Foundation people that I've talked to have said to me that it's a consistent policy of theirs to make the recipients suffer as well as the donor. [Laughter] I don't think they quite phrased it that way. But the idea is that they never actually totally fund anything. They always want the other party to stretch themselves as well as they.

However, Robbie Vogt, who had the great row with Murph and who is no longer provost, was convinced that he could have gotten all of the money out of the Keck Foundation. And that was one of the points of difference. He wanted to go down to the Keck Foundation early on in the project and point out how noble we were being in spending all this time in building the telescope, and how we were poor. I don't know

[2] Julian von Kalinowski, a partner in the law firm of Gibson, Dunn & Criutcher, was on the Keck Foundation's board.—ed.

exactly what his plan was, but he certainly had some scheme for raising all of the money, and he was not allowed to execute said scheme. He claims that several of the trustees were on his side, maybe even a majority. But it was Murph who didn't want to do it, and he didn't want Robbie to do it, either. So those are two slightly orthogonal remarks.

The third one is that if all goes well on Keck II, Caltech will get its money back, it looks to me.

MOY: Its money from the first one?

SARGENT: Yes. If you add up the contributions that are being made by Keck, and perhaps NASA, they come to more than the cost of the second telescope. It would be unwise to write that down. But I did see an article the other day somewhere in which a writer, who is not clued in, drew that conclusion correctly. NASA's going to pay $35 million for one-third of the telescope, or one-sixth of two telescopes, and Keck is paying eighty percent. Now, one-third and eighty percent don't add up to a hundred percent. [Laughter] They add up to 113 percent! So I think Caltech would end up, if all goes well, in getting back roughly the $17 million. Plus, Caltech made money in interest on the first $70 million. So the last time I talked to [David W.] Morrisroe [Caltech vice president for business and finance and treasurer], we'd actually got $76 million out of that $70 million. And any cost over $87.4 million was to be split equally between us and UC; that was the original agreement. So I think we've put in $87.4 million plus $2 million, and we had $76 million. That means the shortfall was like $13 or $14 million as far as Caltech was concerned. And that they will get out of the combined NASA/Keck contributions.

MOY: Sounds like quite a deal!

SARGENT: Yes. I'm sure if you poke around, you will find that that was the reason that Caltech went in for a second telescope. Had they been coming out equal, or making a loss, they wouldn't do it. And that was the argument presented to the trustees, I'm sure, and to the chemists and biologists, who were against the whole thing.