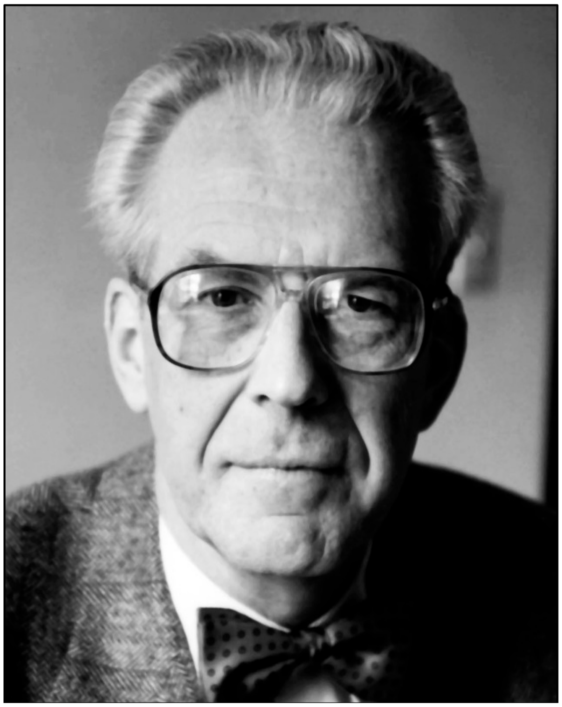

Maarten, Schmidt, 1991

MAARTEN SCHMIDT
(b. 1929)

INTERVIEWED BY
TIMOTHY D. MOY

January 21 & February 5, 1992

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the Keck Series Interviews

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

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

Subject area

Physics, astronomy, Keck Observatory

Abstract

An interview in two sessions, January and February 1992, with Maarten Schmidt, Francis L. Moseley Professor of Astronomy in the Division of Physics, Mathematics, and Astronomy (PMA).

He recalls being brought into plans for a 10-meter telescope in 1978-1979 as director of the Hale Observatories, by Robert Sinsheimer, chancellor of UC Santa Cruz. Appoints Allan Sandage and Keith Matthews to UC committees on the project. His membership on UC Graybeards Committee that chose Jerry Nelson's segmented-mirror design.

He discusses alternative designs, and Caltech's consideration of other big-telescope projects, eventual choice of partnership with the University of California. UC's difficulties with Hoffman Foundation funding; eventual funding for Caltech from Keck Foundation. Howard Keck's early interest in having two telescopes on Mauna Kea. Caltech's efforts to assure UC that they would remain equal partners on Keck project.

Comments on disagreement over siting of telescope headquarters. UCLA's disaffection; contribution of Harland Epps. Discussion of figuring the mirror segments; troubles with Itek. Jerry Nelson's tests at Lawrence Berkeley Laboratory; disadvantages of meniscus design. Drive toward larger telescopes after "back-end" improvements, such as CCDs, were made. Advantages of Keck Telescope over Palomar. Expectations and plans for his own viewing of quasars on Keck I, and its importance to gathering information on galaxies further back in time. Origins and plans for Keck II.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH MAARTEN SCHMIDT

BY TIMOTHY MOY

PASADENA, CALIFORNIA

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

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Maarten Schmidt **by Timothy D. Moy**

Pasadena, California

Session 1 January 21, 1992

Session 2 February 5, 1992

Begin Tape 1, Side 1

MOY: First of all, some brief personal background: When did you first come to Caltech?

SCHMIDT: I came to Caltech in 1959. I had gotten my PhD [1956] in Leiden, in Holland—in fact, I am Dutch. I had come on a postdoctoral fellowship to Pasadena—to Mt. Wilson Observatory, essentially—from 1956 to 1958. I went back to Holland in 1958 and came to Caltech in 1959 as an associate professor. So that's how I started at Caltech.

MOY: How did you first become involved in what became the Keck Telescope project?

SCHMIDT: I was director of the Hale Observatories from 1978 to 1980. The Hale Observatories was the combination of the Mt. Wilson and Las Campanas Observatories of Carnegie Institution and Caltech's Palomar Observatory and the Big Bear Solar Observatory. This was finally dissolved in 1980, at the end of my directorship. But while I was director of the combined observatories—I think it must have been either in late 1978 or early 1979—Chancellor [Robert] Sinsheimer called me from UC Santa Cruz. He had been chairman of the Biology Division here at Caltech, and I had met him here. And he brought up that the University of California was [planning] the TMT project—the Ten-Meter Telescope—and he wondered whether the Hale Observatories would be prepared to cooperate to the degree that it would contribute one member to each of three committees. The committees were for science, and I think I appointed Allan Sandage to that one; he was from Carnegie. [There was the committee for] technical whatever things, and I know I asked [chief instrument scientist] Keith Matthews, who works with [Gerry] Neugebauer [Millikan Professor of Physics, emeritus; d. 2014], for that position. And then there must have been a third committee, and I just don't remember what it was about or who I asked to be on it. I think especially Keith Matthews was for many years active on the relevant committee. And then again, perhaps half or three-quarters of a year later, probably sometime in 1979, I was asked to be a member of the Graybeards Committee.

MOY: It was called that? People referred to it as that?

SCHMIDT: Yes, it was called the Graybeards Committee—well, unofficially of course—and that was the shoot-out between Joe [E. Joseph] Wampler [UC Santa Cruz] with the monolithic single-dish design and Jerry Nelson [Lawrence Berkeley Laboratory] with the segmented mirror. And, needless to say, Jerry Nelson won. The Graybeards Committee consisted of a number of people from UC [University of California]. I was then the only person from outside UC, but just to be sure, they stipulated beforehand that I would not have voting power. This was obviously a very important meeting. There were people like Margaret Burbidge—so it was not just graybeards—Charlie [Charles H.] Townes, Bob [Robert P.] Kraft, and a number of other people. It should be easy to find the composition of that committee, of course. So that was interesting. But that was the earliest experience that I had with the TMT—as director of the Hale Observatories, essentially.

MOY: Were you familiar with the "shoot-out," as you referred to it? From what I understand, that became quite emotional on the UC side of things.

SCHMIDT: Yes. I guess the shoot-out officially happened at that Graybeards meeting; it was contained. As to whether before, or particularly afterwards, it was really contained and things did not become very emotional—that I don't quite know and have very little or no direct knowledge about.

MOY: Joe Wampler did eventually leave UC.

SCHMIDT: He eventually left. It was clearly a major blow, and it changed his whole life, as it were.

MOY: Starting around the middle 1970s, were there ideas other than the segmented mirror and the thin mirror for making telescopes larger than the Mount Palomar Observatory?

SCHMIDT: In general or at UC?

MOY: In general.

SCHMIDT: No, but— [Pause] Well, that depends on whether you consider the [J. Roger P.] Angel mirror, which is the borosilicate mirror. It is, after all, a monolithic mirror. It is not terribly thin, and it has to be made of borosilicate. It is still an important alternative. But I'm not aware that there were other concepts besides that possibility, and I don't quite remember when Angel started.

MOY: When did you very first hear about the Nelson plan—the segmented-mirror plan?

SCHMIDT: That's very hard [to say]. I don't know. I think it must have happened not terribly long after it was proposed by Jerry Nelson, because, after all, the astronomical community, although it's spread out, is not very big, and things like that quickly travel. So I'm sure we knew fairly soon about it.

MOY: Do you have any direct experience with how the idea developed at UC with Nelson?

SCHMIDT: No, no direct information. No, not at all.

MOY: And UC first approached you in your capacity as director of the Hale Observatories. Is that correct?

SCHMIDT: That's right.

MOY: And not really as a representative of the Caltech astronomical community?

SCHMIDT: That's correct. Essentially, Chancellor Sinsheimer addressed both Caltech and Carnegie through the Hale Observatories.

MOY: This wasn't unusual, was it, for UC to go to either the Carnegie or to Caltech?

SCHMIDT: Yes, I thought it was fairly unusual. I thought it was fairly unusual that, while UC is a fairly large institution, to develop something major, there is one other place from which you invite a member to each of your main committees. I had a very positive reaction to that, really; I thought it was very good. And I think that the two other committees were not all that busy, but the technical one, of course, which Jerry Nelson no doubt was a member of, was busy. And Keith Matthews put in quite a lot of work, I think.

MOY: So would you say that the first Caltech involvement with the project began with those committees in around 1978?

SCHMIDT: Yes, 1978 or 1979.

MOY: Do you recall anything about the funding situation at Berkeley at the time? Do you know how they expected to pay for the project?

SCHMIDT: No, that was not clear at first at all. No.

MOY: Did it become clear later?

SCHMIDT: It became clear later, of course, with the bequest from the lady [Marion O. Hoffman], the widow of [Max Hoffman of] the BMW agency. But until that time, we had no idea how they would fund it.

MOY: So Caltech had been participating in an advisory status, essentially, starting in 1978-1979.

SCHMIDT: Yes.

MOY: At some point, the University of California invited Caltech to participate financially, too, isn't that correct?

SCHMIDT: Yes.

MOY: Did you recall how that happened, who did that?

SCHMIDT: No. But there were things that happened in between that may be of some interest. Because in the early 1980s—and again, I don't know precisely the year, perhaps around 1983—we became concerned that not only UC but also other groups started to talk about large telescopes, and among the prominent observatories, we seemed to be essentially the only one who was just looking on and doing nothing about it. So we felt that we should start to explore what we should do.

And there were initially, I think, discussions with Carnegie about the very special purpose telescope that Steve [Stephen A.] Shectman was designing there, made up, it so happened, of segments. But it was practically a stationary telescope—all the segments would be moved in concert—that may well have developed later into an Arecibo type, which is a radio-telescope-type design. But the cost for that— [Pause] Steve Shectman changed his mind once or twice, and the cost estimates went up. So we finally decided that we would talk to the University of Arizona, which was by that time busy developing plans for the mirror lab and Angel's mirror development. And it went so far that we went there as a whole once to talk with them. And they came over here with their staff and had very substantial discussions.

MOY: Now, when you say "we," you mean Caltech astronomers.

SCHMIDT: Caltech astronomy. Yes.

MOY: Who were some of the principal people in on that?

SCHMIDT: Oh, Neugebauer, probably. [B. Thomas] Soifer, [John Beverley] Oke, [Wallace] Sargent. I don't think Jeremy Mould had joined us yet. Essentially, everybody who was on the [astronomy faculty] was involved in that. Then after a while, I don't know whether the original plan was to talk to both places; that is, University of Arizona—they were into the Angel mirrors by that time—and the University of California. So it probably was our initiative, besides talking to University of Arizona, that we should also talk to the University of California. I am virtually certain that those talks took place before the bequest from the widow came in, so that at that time it was still much more theoretical. I know that at that time we found that our style and philosophy seemed to agree much more with that of the University of California colleagues than with those from Arizona. So after an initial approach to Arizona, that sort of went down, and the University of California contact increased.

MOY: Could you elaborate just a little bit on what you mean when you say that the philosophy with UC was more in keeping with Caltech than was the philosophy of Arizona?

SCHMIDT: That's a hard thing to specify. It sounds odd that somehow the— [Pause] I'm not sure that I can even really say what it comes from. Well, there was one thing—to be slightly more specific—that we did not like very much, and that was that the University of Arizona insisted on the site being Mt. Graham, in Arizona. The fact that it is no more than seventy-five miles perhaps from Phoenix, in connection with the light-pollution issue, and also that it wasn't so clear whether the seeing was good there, made us dubious. There was a seeing expedition—that is, a seeing-monitoring experiment going on on Mt. Graham, together between the University of Arizona and the U.S. National Observatories, which was also interested in large telescopes. And in the discussions, Peter Strittmatter, the director [of Steward Observatory] at Arizona, essentially made it clear that if the astronomers of the National Observatories didn't find that the seeing was good at Mt. Graham, that he was sure that the data could be looked at in a way where it would show that the seeing was good.

At that time, the University of California people, I think, had already settled on Mauna Kea, and we liked that very much—almost no light pollution, very high, and so on.

MOY: I have heard other people say precisely what you just said—that the philosophy was similar—and it sounded to me like it was an issue of style, of methods of approaching problems and deciding what sort of questions are interesting, and so on.

SCHMIDT: Well, the University of Arizona is very much younger, a much younger astronomy group, and they had been up-and-coming for about ten or fifteen years at that time, whereas Lick is a much older observatory, since the turn of the century or before that [1888—ed.]. It's hard to put one's finger on, but we have found in the subsequent years, now that we cooperate with them, that our styles are very similar. I can understand that you are interested, and it's hard to quantify or to express.

MOY: How did the Caltech astronomers come to some sort of consensus as to which way to go? Did this just evolve on its own, or did the department at some point have to make a decision as to what it thought would be the best thing to do?

SCHMIDT: Well, if you don't mind me looking at some paperwork, I do have a file here. [Referring to file, now part of the Keck Telescope Oral History Collection].

MOY: Would it be possible for us to have copies of any documents that you refer to?

SCHMIDT: Yes. Here is a meeting on July 20, 1983, where it says, "I would like to have a follow-up meeting on Caltech's involvement in the Next Generation Telescope."

MOY: And who is saying this?

SCHMIDT: That is a memo from Ed [Edward C.] Stone [then chairman of the Division of Physics, Mathematics, and Astronomy]. I can give you this file so that you can take out of it whatever you want.

But I see here, for instance, that this starts with a piece by Steve Shectman from Mt. Wilson, and it isn't even impossible that this was the only thing that was discussed at the time. This file is mostly around 1983, and I'm really hesitating to talk about it, because you may prefer to have this file, unless you want it on record. But I cannot quite remember how, from our discussions, which had to do with Arizona, UC—and always Carnegie in the background, because we had been so closely associated with them—how that tied in to the announcement that the lady had given the bequest to the University of California. And then perhaps a half year later or so, Caltech was asked [by UC] whether we wanted to be a one-quarter partner, which started things on a very much more definitive course. I cannot quite link [those things] together, and I'm not even sure that from this paperwork it can be done. But this paperwork is about the things that happened just before the discussion took place about how to react to the UC invitation to participate.

MOY: Is it fair to say that even before the UC invitation, the Caltech astronomy community was trying to decide which program, either UC's or Arizona's, it wanted to try to piggyback onto? Was it a belief, even at that point, before there was any kind of a formal invitation, that Caltech would become involved in one of these two if it wanted to?

SCHMIDT: I think so. You can include Carnegie with that, actually. We felt that we were behind and we'd better join up with somebody.

MOY: Was there any concern that they would not be interested in having Caltech as a partner?

SCHMIDT: No, because none of the other groups had money. Of course, we didn't have money either, but I think it was clear to everybody that the probability of raising the required amount of funds [would be] better when we joined up. So, no.

MOY: And as far as the invitation goes, I had heard that there was a meeting between a few Caltech astronomers and a few UC astronomers from Berkeley and Santa Cruz and so on. I don't remember exactly when. It must have been shortly after 1983.

SCHMIDT: You mention here [looking at discussion outline] something that happened at San Jose airport. I'm sure that I know that meeting. I was indeed there. In fact, I found to my surprise that my travel was not even paid for by Caltech. It came from discretionary funds, research funds of mine. I'm rather proud of that actually, you know. I contributed to this effort by paying my own travel. Well, almost. [Laughter] Yes, this was on April 28, 1984, at San Jose airport. You have to watch it, because in my paperwork I found that there was another meeting at San Jose airport before that. And again, I don't even remember whether at that time Mrs. Hoffman had indicated that she would contribute the money. So I cannot remember in detail what we discussed. On the other hand, it stands out in my mind that it was a very important meeting, that last one. I was there and Bev Oke, Wal Sargent, and, I think, Neugebauer. And then three or four astronomers from UC. And it was positive and it was important. But I don't know what it was. [Laughter]

MOY: So it stands out in your mind as sort of a meeting of minds, some sort of a common agreement that the two groups would work together.

SCHMIDT: Yes. But what we decided, I don't know. And on what basis, I don't know exactly either. I suspect that the lady had, in fact, indicated that she would give the money. I suspect that that was the case.

MOY: By that point?

SCHMIDT: I think so.

MOY: At the time, if I remember right, it was about $36 million?

SCHMIDT: Yes. It was between $33 and $36 [million] or so. That's the amount I remember.

MOY: And at the time that was considered sufficient for the project.

SCHMIDT: No. That was clear immediately. I seem to remember that in this paperwork [referring to file] there are some estimates from around that time as to how much the telescope would cost. I would have to look that up.

Well, there is a memo here, for instance, of March 19, 1984, from Gerry Neugebauer, in which he says somewhere, "The biggest problem with joining UC concerns the timescale, since it is a project that is well under way, and the fraction of project which is ours." I don't understand that last part, but that's the way it's put. "UC asks us to join them by putting in about one-quarter the cost"—about $25 million inflated—"and getting one-quarter of the observing time." I'm sure that that only came after they had had the money from the lady—or at least a promise for quite a while; they found that they couldn't raise enough.

MOY: So if one-quarter is $25 million, then they have a projection that actually turns out to be quite accurate—that it's going to cost about $100 million.

SCHMIDT: Yes. On the other hand, I don't know exactly where that came from. There is here another table [referring to file], in fact, in this same document of Gerry Neugebauer's, in which it says that the total cost of the telescope in 1983 dollars will be $72 million.

MOY: But in any case, it was quite clear that the $36 million would not be sufficient.

SCHMIDT: That's correct. That's correct.

MOY: Do you recall what happened to the Hoffman money, on the UC side?

SCHMIDT: As far as I know, at some stage the estate either threatened to sue or sued University of California for breach of contract. I think that when the money was awarded, the University of California signed a contract—well, of course, I've never seen it, and I'm sure the facts are always different from what I think they are—in which they promised or contracted to build a 10-meter telescope with the Hoffman name and so on. And once Caltech became involved—and through the Keck gift became not a minor but a major partner—this was not going to happen. The University of California, I think, only came out of that by an amiable agreement in which they returned the money.

MOY: But before the Keck money came in, hadn't there been some difficulty with the Hoffman money because Mrs. Hoffman died [December 16, 1983]?

SCHMIDT: Yes, as far as I remember, there was some trouble, in that there were two persons who somehow had influence, and one was a sister of Mrs. Hoffman and somebody else—I don't know whether it was a nurse or a secretary. I think at one particular moment one of the two parties decided to withdraw the money.

MOY: And those difficulties were going on even before the Keck money came in?

SCHMIDT: I think that particular difficulty happened before the Keck money, yes.

MOY: How did you first hear about the Keck donation, which was $70 million, I believe.

SCHMIDT: Yes. I think it was sometime in August 1984, but I'm not sure of that and I don't have paperwork. But it was a meeting that was called by Robbie [Rochus E.] Vogt, who was then [Caltech provost]. He called in the astronomers and told us what had happened. He also made clear—and we soon realized it ourselves—that we were all in a very difficult position, because it looked very much as if we were taking over. So there was a lot of discussion about how we could approach and negotiate with the University of California in a way where we would maintain a partnership and not seem to take over.

MOY: Was the entire astronomy department at Caltech in on this discussion?

SCHMIDT: I think so. I'm not sure about it. The astronomy department, of course, is very small, so that doesn't mean all that much. And as far as I remember, around that time or soon thereafter—there may have been more than one meeting—we essentially settled on three scenarios of how things could be handled. And one would be that while we would build the telescope on Mauna Kea, the University of California would find enough money to build one in the Southern Hemisphere, and there would be cooperation, and the design would be the same. The second option was that, near the [Keck] telescope on the same mountain, another one might be built by the University of California; and [we] would do interferometry between the two and further cooperate. And a third option essentially came down to this: that Caltech would provide the funds to build the telescope, and the University of California would bring in enough funds to run the installation for a considerable time, which turned out to be twenty-five years. Well, as we know, the final option was the one that became operative.

On the other hand, I believe that the fact that we at the moment are working on Keck II is essentially due to the discussions that took place at that time. Because from what I understand, and that's only indirect, Mr. [Howard] Keck himself became intrigued by this possibility of having these two telescopes on one mountaintop, and even though the final solution was not to do it [at the time], the fact that it registered with him must have had an effect that we all now understand and appreciate.

MOY: So you would say that some of the groundwork for that may have been laid even in these early discussions.

SCHMIDT: In those discussions, I think so, yes.

MOY: Do you recall some intrigue surrounding who the donor was at first? I have heard some people say that they were told that an enormous amount of money was coming, but at first they weren't told who was giving it. Does that ring any bells for you?

SCHMIDT: Yes, but not that it was an intrigue. It's entirely possible that the name was not mentioned to us immediately, although it was not long before we knew. But perhaps during the first meeting it was not mentioned; that is possible. Although if you had not introduced the question this way, I would have said that we knew right away that it was Mr. Keck.

MOY: And could you elaborate a little more on the discussion and the concern about how to approach the UC people with this blessing and problem?

SCHMIDT: Yes. It was clear, when we had brief contacts with our University of California colleagues, that they initially were very concerned and did not necessarily have charitable thoughts about our motives. But it was really appreciated by us that we had to make absolutely sure that this was carefully thought about and prepared and that the proposals we would make in the negotiations with UC would make clear that we wanted them as a full partner. And that has worked, I think. But initially there were instances where individuals at the University of California were quite upset about things. That slowly went away.

MOY: There was an article some time ago in the *Los Angeles Times* that implied something that's quite astonishing:[1] At some time, [Caltech president Marvin L.] Murph Goldberger went to see [UC president] David Gardner, either him or [William] Frazer, the vice president, and supposedly had as one possibility the prospect of Caltech going it alone. If the University of California was not amenable to taking on a half-time partner, Caltech might conceivably do it on its own. Is that possible, in your recollection?

SCHMIDT: That I've never heard. In fact, I would have expected from the nature of the discussion that we had with Robbie Vogt, and the sensitivity that he himself exhibited to how UC would react to the news that we were now in this position, that that reflected sensitivity also on the part of Murph. I cannot believe that this was not the case. So you could think that a fourth possibility among the three I mentioned would indeed be, "If you have absolutely no money and/or no interest, then we will have to go it alone." That that would have been said, either up front or emphasized at all, to me seems very

[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.

unlikely. The atmosphere at that time was, "How are we going to stay together?" And we all have tremendously benefited from that. But I cannot be sure, of course.

MOY: Do you have any speculation as to what the Caltech and the Keck people would have done in that case? If the University of California had said, "Absolutely not. We've agreed to take this money from Hoffman. It must be the Hoffman telescope."

SCHMIDT: My speculation would be that it [would have been] untenable—not necessarily to Caltech but certainly to the Keck Foundation. In the case of these large gifts, the name associated with the gift often is very important. And from what I've seen in fund-raising, you would expect that the donor of a dominant gift is the one that decides about a name. On the other hand, as I mentioned, the estate of the Hoffman bequest was threatening to sue the University of California.

Begin Tape 1, Side 2

MOY: Another possibility that comes to mind is the prospect of a Keck telescope in the Southern Hemisphere—say, in Chile—and a Hoffman telescope on Mauna Kea. Was that discussed?

SCHMIDT: I can't remember that thing that way around. No. For some reason that I now cannot remember, in that initial discussion in Robbie Vogt's office, I do not think that that possibility came up—although logically it would be one, obviously. I think we felt that the segmented design of the mirror was sufficiently experimental that we wanted—[Pause] I suspect that if it had come up, we would have said, "Well, we'd better do that in the Northern Hemisphere, where the technical support in Hawaii is very good." Whereas we all know that when you are in Chile—and there are quite a number of observatories, as you know, in Chile—you have to provide all your own technical support, and certainly for something as sophisticated as Jerry Nelson's design. So if it came up, we probably rejected it on that basis. But I don't remember it coming up.

MOY: Were you involved at all with the working out of the final financial details with the UC people, the Keck Foundation, and CARA [California Association for Research in Astronomy]?

SCHMIDT: No.

MOY: The Keck gift was for a very large portion, but still a portion, of the expected costs. It was not for the entire amount. I recall that there were some in the Caltech administration who wanted to try to get the rest of the [necessary] money as well. Are you familiar with who wanted that and who didn't want that, and how that played out?

SCHMIDT: No. That all came to us essentially by rumors, and I think it's quite likely that Robbie Vogt and Murph Goldberger didn't see eye to eye about it in the final instance. But I'm not even entirely sure about that.

MOY: My recollection is that Vogt was eager to try to get the rest of the money from the Keck Foundation, but that Goldberger, for reasons I'm not completely clear on, was not keen on it. Are you familiar at all with why he might not have wanted to try to have that done?

SCHMIDT: There must have been a number of discussions between Murph and Howard Keck privately—that somehow Murph appreciated that that was exactly what Howard Keck wanted to give and that he, in fact, purposely did not want to give the full amount. It certainly keeps an organization very aware of not wanting to go over budget in cases where the money is already somewhat tight. So I suspect there was a purpose to it.

MOY: Are you familiar with how the Mauna Kea site was selected originally?

SCHMIDT: No, I don't know that.

MOY: I had heard that there was some debate on the site of the headquarters—Waimea or Hilo.

SCHMIDT: Yes.

MOY: Are you familiar with that debate?

SCHMIDT: Yes. We only knew about it, we were never involved in it. From what I remember, the Caltech administration wanted to go to Waimea, and I think UC wanted to go to Hilo [It was the other way round—ed.]. Somehow, at some stage, there was a small committee made up of one or two members of each administration—perhaps only one of each—and somehow they decided it was exactly even. I've never quite understood what happened there. So I've heard about it, but I don't really understand what happened.

MOY: Do you know from whom these various sites were acquired, or how they were acquired?

SCHMIDT: As far as I know, the site in Waimea came from [Richard] Smart, who is the main cattle farmer over there. I think it's one of the largest cattle farms in the United States. He's a very influential person over there. And he gave it to us, I think.

MOY: There had also been a rumor, now that you mention Smart, that some of the UC people did want to go to Waimea, and there had been the prospect of the Smarts' contributing money for another telescope if the headquarters were in Waimea. Does that ring any bells for you?

SCHMIDT: I don't know that UC people were involved in that. It's entirely possible. I do know that there was some talk for a while about a possibility that he would give a substantial amount of money, which never came about. So it sounds familiar, but I don't know details.

MOY: It's interesting that you talk about the immediate concern about going back to the UC people after the Keck gift. It seems to me it would have been an incredibly difficult situation.

SCHMIDT: Astonishing, in fact.

MOY: Was it depressing in some ways?

SCHMIDT: Of course it was not depressing, because suddenly we saw the prospect of essentially all the money being there, and that the telescope was going to be built. But now how did we hold on to this partnership? Yes, there was genuine concern. As far as I know, there have been essentially no instances where Caltech or Caltech people essentially emphasized that they take it over. But I'm sure that many UC astronomers thought for quite a while that we had. [Laughter]

MOY: The relationship has evolved very nicely; it is very amiable now and very cooperative and is working very well. But do you have any sense at all that some of the people on the UC side still grate a little bit?

SCHMIDT: No. I would say that within the UC system, which is very large and diverse, there are feelings relative to the telescope that vary much more than most of the UC astronomers' and our point of view. In particular, for some reason, at a very early time, UCLA thought that they were not sufficiently involved, and for many years—and perhaps it's still going on—served as critics within the UC system of the whole thing.

MOY: Do you have any idea of why that's so?

SCHMIDT: No, not particularly. But I know it from experience.

MOY: Really?

SCHMIDT: Well, they talk that way. This was about two years ago; I don't know whether they still do. I gave a lecture there once, and it was clear from the way they talked about the telescope. They don't talk [about] "our" telescope but essentially about, you know, "the thing that you and Lick are doing"—that is, Caltech and Lick Observatory, essentially.

MOY: I find this very mysterious. Is there some sense that the design isn't going to work sufficiently well? Or is it the notion that you don't need big telescopes?

SCHMIDT: No. I don't know. I think that it's intercampus really, within UC.

MOY: So the primary campuses have been Berkeley and Santa Cruz?

SCHMIDT: Yes.

MOY: And UCLA has been sort of out of it.

SCHMIDT: Sort of very distant from it. And UCSD [University of California at San Diego] is also involved.

MAARTEN SCHMIDT

SESSION 2

February 5, 1992

Begin Tape 2, Side 1

MOY: In the deliberations in your file [transferred to Archives], there is a fair amount of attention given to something called the Straw Man Proposal. My impression was that the Straw Man Proposal was primarily Gerry Neugebauer's idea, and as an alternative to going in with UC, was to do some sort of joint project with Carnegie and Arizona for two 7.5-meter telescopes, one in Las Campanas and one probably on Mt. Graham. Does that ring any bells?

SCHMIDT: Yes it does. And I would not be surprised if that Straw Man Proposal was something that Gerry Neugebauer did not necessarily want but just wanted to have it carefully discussed, so that we would get the arguments for and against it, and therefore also for and against UC.

MOY: It seems to have been called the Straw Man Proposal right from the beginning.

SCHMIDT: Yes, but that's Gerry Neugebauer's way of setting up things.

MOY: Would he often call things a straw-man proposal?

SCHMIDT: No, that I'm not aware of. But I remember that was one that he called that way.

MOY: Because it strikes me, reading it, that it was deliberately proposed as something to be knocked down.

SCHMIDT: I think it probably was.

MOY: Are there people, aside from Jerry Nelson, who stand out as having made extraordinary contributions?

SCHMIDT: Yes, I think that Harland Epps at UCLA, who is now at Lick Observatory, made a very important contribution in terms of optics. But it's a curious situation, because the relationship between Harland Epps and the project was not terribly good for a long time. It's no doubt better now because he's now at Lick. But I mentioned to you earlier that the UCLA people, for a long time, felt left out, and he was at UCLA.

And of course the four groups that are building the four instruments at the moment have made important contributions. Those will be up front and visible, I'm sure. But as I say, I think that Harland Epps made in optics, certainly, one if not more very important contributions.

MOY: Could you just characterize or summarize the new features of the design of this telescope [Keck I]. What makes this particular design special.

SCHMIDT: The segmented mirror, of course.

Something that is less special, because most new observatories now will attempt to do that, is control of heat sources so that the air inside the dome is well adjusted to the outside temperature, so that the image is not affected by turbulence and things like that. That's very important.

An integral and necessary part, of course, of the segmented nature of the mirror is that the parts have to be monitored relative to each other and adjusted—that is the active support. It is, of course, an alt-azimuth design rather than a parallactic design; you use the direction to the pole as one of the axes of support. But that is something that is also uncontroversial in new telescopes—large telescopes will have that.

MOY: Around 1984, did you harbor any serious doubts that this design simply wouldn't work?

SCHMIDT: No, I had no fears that it wouldn't work. My feeling was that it had to work, but it was not necessarily an easy task. The whole business of aligning the mirrors so that the surfaces essentially act as one surface, is something that— [Pause] Well, I see it like a physics lab, where you tune up things, you improve things, and you say, "Now I want to make it yet three times better, because that's what I really need." And after a week you get a factor of 2 and then something happens and you're back to a factor of 3 again. But at least you work on it and you improve things; you study what goes wrong and you come up with clues, and so on. So my feeling was that it seemed quite likely that right in the beginning you would not get the images that you'd want out of a telescope like that, but after one or two or three years, things would gradually improve to where you would indeed achieve that. I would say at this moment—while the telescope isn't quite finished yet; there certainly is not a routine operation yet—even that may have been slightly too pessimistic. I have the impression that it is quite likely that we will get very good images immediately, and that then the slow process will be to go from very good images to almost perfect ones, considering the size of the telescope. I'm rather optimistic at the moment.

MOY: I'd like to talk a little bit more about that in a moment. Have you been personally involved in dealing with CARA and helping to monitor the construction process?

SCHMIDT: I was for two years—that is, from 1988 to 1990, July to July—on the Science Steering Committee. And the second of those two years, I was co-chairman.

MOY: From that experience, are there elements of the design and construction that have been surprisingly difficult, more difficult than you had anticipated originally?

SCHMIDT: Yes. The figuring of the individual segments—the polishing and getting them in the shape required—turned out to be much more difficult than I had thought. Offhand, you would think that getting something that is only of the order of 6 feet across, which in optics is not huge—after all, the 200-inch mirror is 5 meters—should be relatively easy to get into good shape. On the other hand, of course, it has to be admitted that, including the spare segments, that has to be done for one Keck telescope forty-two times. Time is money, and therefore you cannot spend a huge time on each of them. So the difference between whether you do it eleven weeks per segment, or six or seven weeks per segment, becomes very important and can make millions of dollars' difference.

To me, the surprise was the ineptness of Itek, the company that was hired to do this. I think it took on the order of two years—although initially I wasn't that close to it, of course—for them to perform anywhere near what one would expect of a reputable optical firm. This was a big surprise to me, and it suggests to me that in general there must be parts of the aerospace and defense industry that are very bad. We made sizable overruns on the mirror ourselves, in the Keck and CARA. I bet you these quarter-billion-dollar overruns for nuclear submarines are created just the same way.

But actually, CARA was very aggressive about it. It started to, in effect, place people permanently at Itek from the organization. We had several meetings at Itek. I remember once there was also a long-term meeting at Itek. So, this is not a defense contract, and although the funds for the Keck are considerable, it is still a university-type project, where it has to be lean and well done, and you can hardly stand any substantial overruns. And there was a danger that this would not succeed. That to me turned out to be surprisingly difficult. And I was quite disappointed when, later on, CARA went to a second firm to help with the polishing, because things went too slowly. This was Tinsley in [Richmond, California]; their startup troubles were rather similar to those of Itek, although the [troubles] didn't last as long. It was surprising to me that it was so difficult, and that established firms do not seem to be able to just take it and almost immediately start to produce a quality product.

MOY: So, do you feel as though it was ineptitude on the part of these companies or simply the magnitude of the technical problem?

SCHMIDT: No, I don't think so.

MOY: Do you think it's perhaps because they were not well experienced in doing this sort of optical work?

SCHMIDT: I doubt it. One gets very strongly the impression that in cases like this, what happens is the following: The company comes up with its best team of people and writes the proposal for the bid. Once the bid is in hand, second- or third-level people come in to execute it. And the first thing is to appoint a project manager who knows nothing technically about what is to be achieved and acts as a public-relations person in order to keep the customer happy—and these were all things that didn't lie well with CARA, of course. And when there are problems finally, the attitude in general seems to be to just pour as much money and people into it as you can to try and resolve them. All these are at variance with university practices. [Laughter] But I'm afraid that was probably indicative of the defense industry.

MOY: I'm not as familiar with Tinsley. Do they also do mostly defense work?

SCHMIDT: I'm not so sure it was mostly defense work, but they certainly did defense work, as far as I remember. But perhaps much less so.

MOY: Could you just be a little more specific? Could you elaborate just a little bit on what sorts of problems Itek seemed to come up against that they seem surprisingly unable to overcome in an efficient manner? Was it due to the fact that the segments would warp again after you cut them?

SCHMIDT: I cannot go into enough detail, although I heard some of the complaints at the time. I don't know enough about it to comment on that. I know that there were all sorts of difficulties in terms of the support of the blank, in terms of the testing—that different tests gave different results. But I wouldn't like to go into detail, because I'm not well versed or an expert on that.

MOY: Were there elements of the design and construction that surprised you with their ease—that turned out to be a lot easier than you had anticipated?

SCHMIDT: Well, it's easy for me to say, of course, what was difficult. But it is all, of course, relative to what my own internal expectations were, as a non-technical person. The thing that struck me as something that went particularly well was the alignment system of the segments. That was, I think, only first tested just a little under a year ago, last March or April 1990 or so, close to first light. And when that system was turned on to align the first nine segments, the noise in the alignment was on the order of 15 nanometers, whereas when the dome was rotating, it became on the order of 40, and that was the first result, which was well within the requirements. And that's remarkable, because people in the community who had been following all this had been most worried about, or critical of, the claims made by Jerry Nelson: that you can align these segments and then it's as good as if it were one surface. And add two aspects to that: the narrow groove in between the mirrors, of course, but also deviations from a perfect match. Now that doesn't mean that it's easy. [Laughter] Jerry Nelson somehow did it particularly well.

MOY: I remember seeing some newspaper articles, somewhere, where he and some other people originally tested it up at LBL [Lawrence Berkeley Laboratory].

SCHMIDT: That's right. I saw those tests. He used one of the ears, as it is called. When you have a segment, then you have to cut it into a hexagon, of course; there are ears that are taken off. And he had taken one of the ears and aligned it, relative to another ear, or whatever it was.

MOY: Someone said that you could go up and kick the thing on the side and it would still maintain the alignment. Was it really that precise or was that an exaggeration?

SCHMIDT: Well, I don't know about kicking, but you could certainly press it and then it would immediately go off and come back again. When you released it, it would go off again and come back again. That to me was surprisingly successful.

MOY: The tolerances are so tight.

SCHMIDT: They are stunning. And that seemed to go particularly well.

MOY: Who actually produced all of the supports? Of course I know Nelson and people on his team at LBL designed them, but do you know who actually produced them?

SCHMIDT: No, but I think they were made at LBL in the shops.

MOY: So that wasn't contracted out.

SCHMIDT: I think it was not. I'm not sure.

MOY: Let me just jump back for a moment to something you mentioned before. When I asked you whether, early on, you had any doubts whether this Nelson design would eventually work, you said, "For the most part, no." Did you feel the same way about the Angel and the meniscus-mirror designs—that eventually they, too, would work but that it might be just very expensive?

SCHMIDT: I never had much doubt about the Angel dishes. The meniscus is one that, for whatever reason, worried me most, and perhaps it still worries me a lot. Because that's going to be just a very thin dish of the order of perhaps 7 inches, [and] something like 8 meters across or so. That is something whose support will have to be very solid, exquisite, and very good, obviously—if something went wrong with the support, barely, it could even break. Handling a thin thing like that during fabrication, too, seems to me— [Pause] It's awkward when you see one of our 2-meter segments hanging upside down in an optical shop; you hold your breath. But when it's your one 8-meter mirror that is hanging upside down, I don't think I would even want to see that. But I think it can be done, probably.

MOY: It seems that it would be incredibly fragile. Do you think that that project will benefit from the active-support knowledge that was developed for the Keck?

SCHMIDT: I don't know about that. I think what they are doing is fairly independent in terms of active support. The problem is somewhat different, in that we have unattached segments that have to be aligned, and there you have one solid mirror, and you push and pull everywhere. So there the response, of course, is quite a lot more complicated. If you push at a particular place, you get a hill, and stuff nearby moves, too, whereas if you do it with one segment, the other segment next to it of course doesn't move at all. So it is different. But I think people who want to use the other designs have come very far in studying that and modeling it.

MOY: Is it possible to summarize the various contributions that came for the most part from Caltech and for the most part from the University of California? And, for that matter, from the University of Hawaii? Is it possible to summarize who did what? Or was it really much more of a group endeavor?

SCHMIDT: I would say that once we got together, it was really a group endeavor. And I would hardly know. Because among the instruments there is one optical spectrograph being built at Lick and there is one being built here. The infrared instruments—one here and one over there. So it is very much a group effort, once we got all together. And before that time, it has to be said that the University of California did the major job in design: Jerry Nelson, in first thinking up the idea and then testing it, as we discussed. So that's a fair summary.

MOY: When did astronomers begin thinking it was necessary to have a telescope larger than Palomar? Or do astronomers always want larger telescopes?

SCHMIDT: Yes, they always wanted larger telescopes. I remember that at Santa Barbara Street—the Carnegie Institution—there was a picture of a design by somebody by the name of Johnson, and I don't remember his first name. And it was for a 300-inch telescope. And I have no idea when the design was made, but I think it was in the 1930s.

I remember that in the 1950s, Carnegie wasn't called Hale Observatories yet, it was called Mt. Wilson and Palomar Observatories. The predecessor of [Horace W.] Babcock, Ira [S.] Bowen, was the director, and he made it the Hale Observatories. And I remember that when I came to Carnegie as a postdoctoral fellow in the late 1950s, I may have asked Dr. Bowen once about larger telescopes. And he explained that, in a telescope there are two things that are important. First is how much light you catch, and therefore you have to work with that end of the tube. And the second thing is, What do you do with it? And he explained that there is enormous inefficiency in what you do with it. Photographic plates, in general, don't record much more than one percent of the photons that are caught by the telescope. At that time, image tubes came into use, and they improved the efficiency probably by a factor of 10 or so for spectrograph design and other things. So he said that everybody was working hard on the back end of the telescope to make that more efficient, and it was much cheaper to do it there than up front. Whereas if you wanted to double the thing, it would be roughly four to eight times as expensive. The 200-inch already cost $6 million in the late 1930s, so you could imagine what kind of cost that would entail in the 1950s. And what changed things, I think, in the late 1960s or so, is that it was realized that if you went for a mirror that was lighter in weight and also had a shorter focal length, that somehow you could do everything cheaper. The MMT, the Multi-Mirror Telescope of the University of Arizona—a set of six 72-inch telescopes together on one mount—showed that, indeed, for a large effective aperture, you can do it in different ways where costs are reduced quite a bit.

So, two things happened more or less at the same time. The detectors at the back end, with better image tubes and, later on, CCDs [charge coupled devices]—which now of course is the detector of choice in astronomy—made things so good and effective at the back end that you had improved things at the back to [the point] where you could not do much more. The improvement from seventy percent to eighty or ninety percent is not all that exciting. The one percent to ten or twelve percent is hugely exciting—that's a factor of 10 or 12. So there were no large factors to be gained anymore. So if you wanted to make progress, you should do it at the front end of the telescope. And then, the development of lightweight mirrors, with Angel and the meniscus, and then Jerry Nelson's idea, opened the possibility of having telescopes that were very considerably cheaper per unit area than they used to be, by a factor of probably 5 to 10 or so. So it's a huge amount. I'm sure that otherwise the Keck Telescope would have cost between a half billion and a billion dollars or so.

MOY: If it were one large objective mirror.

SCHMIDT: If it had been the Palomar design, as it were. So those two things happened. The improvements at the back end came to a natural end. And as for the front end, it turned out that because of the lightweight approaches, things could be made that were bigger. So that's what I think led to it.

MOY: There are some people who try to draw an analogy between large telescopes and particle accelerators. This is done mostly in the media, because they just see big machines and a large amount of money. But one important difference is that with particle energies there are thresholds—certain experiments that you need certain energies for. Whereas that's simply not the case in astronomy. Is that right?

SCHMIDT: That's true, because you can always do something fainter in astronomy if you are willing to spend more time on it. And that certainly goes for the CCD detectors, which are photomatically very good. Which means that if you go four times as long with a CCD, you are able to go twice deeper—in other words, to have a limiting flux that is half the one you had before. Therefore, you can exchange time for aperture. What you can do with the big Keck Telescope, you can do at Palomar, if you spend enough time on it.

On the other hand, the thing that is, in the case of the Keck, very remarkable is that the factor of improvement in that case is enormous over the instrument most people would use as a comparison, and that's Palomar. There is a combination of three factors that make Keck faster than Palomar. The first consideration is, of course, the aperture. It's twice as large, and therefore you gain four times the number of photons you get. So that's a gain by a factor of 4. The second is the image that is formed, which is partly a function of how well the telescope does but also the upper atmosphere. Now, the telescope in this case does better than Palomar, and the atmosphere certainly is better, because Mauna Kea is chosen for its good seeing. So my conservative guess is that the image in seconds of arc on the sky from the Palomar telescope to the Keck telescope is a decrease by about a factor of 2 in size, which means a four-times-smaller area. And this factor comes in with as much weight as the whole photo-catching aperture area of the telescope as a whole. So there's another factor of 4, so that's a factor of 16. Then there is the question of how bright the background is, against which you try to detect a faint object—that is, the brightness of the night sky. And I think, there again, a rather conservative estimate is that it's probably half as bright there [at Mauna Kea] as it is at Palomar, partly because of the higher altitude and partly, of course, because of the absence of Los Angeles and San Diego. And that adds another factor of 2. Now that's a factor of 32 in gain. And I cannot think of any occasion in this century when astronomers had the opportunity to enjoy a thirty-fold gain. Because if you think of what happened from the 60-inch to the 100-inch [on Mt. Wilson] between 1908 and 1916 or so, it was in the same location, the night sky was equally bright, the seeing for the two telescopes was about the same. It was a factor of 2. Because it went from 100 squares to 60 squares, it was a factor of 2.6. From the 100-inch to the 200-inch, in night-sky brightness and so on, things might have improved a little bit, but in 1948 Los Angeles wasn't so bad yet in terms of light. So it was a slight improvement, perhaps, there. But it's likely that the factor was of the order of the ratio squared of the apertures. That's a factor of 4, let's say a factor of 5 or 6—that's good. But now a factor of 30! I mean, this is just an incredible factor. So while it's very true that there are no thresholds to get over, then you are in a new land, where you can look around and do things you could never do before. In this case, with a factor of 30 [improvement], there are things that become possible that never were possible at all. On the 200-inch telescope, you wouldn't want to spend, say, on the order of thirty nights to do a particular project if you can do it in a single night.

MOY: I find this interesting, because a future historian looking back on this might describe it as largely a social advance, because it permits more astronomers to use more telescope per unit time. It's a technical advance certainly, but it's a technical advance devoted largely toward serving the community better in a social sense, not as much in a pure technical sense of being able to allow them to get knowledge or data that they couldn't have gotten otherwise. Is that fair to say?

SCHMIDT: Yes and no. It would be fair to say that, if we talked about a national telescope where you have a very large clientele—like for Kitt Peak and Cerro Tololo National Observatories, where only a small fraction of all those who could use it actually get time, because there is no more time. There are always plenty of proposals and only a very small fraction of people get chosen. Now, if they switched and went to 8-meter telescopes, they would be able to serve more people, each of them doing the same or bigger projects, but it would improve the overall probability and set of people in the community who work with the telescope. I imagine that here at Caltech, those who are going to use the Keck Telescope are probably not a very different group from those who already have access to Palomar. So, I would think, in this somewhat more close community, that it is the quality of the work that is going to increase immensely as well as the quantity. That is, people now can go five times fainter and still do six times as many objects, as it were—since five times six is thirty. Or they can go to an object thirty times fainter, which I think you just couldn't afford to do in the present system.

MOY: So is it the case also that you couldn't afford to do things thirty times fainter partly because of time, both in the sense that you couldn't spend the time and also because there is a problem in that the detector can't be exposed forever, right?

SCHMIDT: No. No. But in many cases that has not been explored carefully. We know that if you go from one hour to four hours, you get, roughly, twice as deep. But if you go from a total of, say, 100 hours to 400 hours, I am not absolutely sure that you would still get twice as deep. It's always better to have lots of photons immediately available than to have to wait for them so long that circumstances may change. But you must understand about this business of certain things you simply don't do if it will take too much time. A typical staff member here in optical astronomy may get on the order of ten or fifteen nights with the 200-inch. In ten or fifteen nights with the 200-inch, for objects around 18th to 21st magnitude, you can do a very extensive program. Now you could, of course, decide you want to spend all fifteen nights on the spectrum of a single 26.5-magnitude object. And you've not the least idea what will come out, of course. Now that's tremendously risky. You'd probably find that if you did that three or four years in a row, the time-assignment committee we set up would say, "Well, so-and-so always goes for a year-long-no-exposure in terms of his time and never gets anything, so what's the use of giving him time?" Of course, you may make major discoveries, but if you do one object per year, essentially, that's not much good. And then comes the improvement—that you could do it in half a night with the Keck Telescope. So you say, "Well, I think that's so exciting, I'm going to do that." And then you're disappointed at the end of the half night if nothing comes out, so you go on with the regular program, where you get 130 objects from your regular program in the second half of the night. [Laughter]

MOY: Do you recall what the anticipated resolution of the [Keck] telescope was, back in 1985?

SCHMIDT: Yes. All the parameters had been set up by the UC people—by the Lick people. There was never much discussion about that. I regularly came to feel that I wasn't quite sure how critically those had originally been discussed, and I was never quite clear why we on the Caltech side did not initiate with the UC people a discussion on whether that was the right image size to go for. Well, that especially came to my mind when the Itek performance was such that we found, at best, we could expect images two-and-a-half times as big as the design value.

MOY: Did you remember what the design value was?

SCHMIDT: Yes. I think the design value was 0.24 seconds of arc for the diameter of a circle that would enclose eighty percent of the energy.

MOY: And what do you expect to get now, when everything is up and running?

SCHMIDT: We are still going to get it. That's because things have changed a lot technically, in the meantime, because of developments that are totally outside the normal range of polishing and which have to do with what is called ion polishing. It is a new procedure that I think was originally explored at the University of Virginia. It was not classified or patented, as far as I know. And Eastman Kodak became interested and has for quite a number of years been experimenting with the procedure. Essentially, what you have is a particle gun that shoots at the mirror from not too large a distance and essentially digs a Gaussian hole in it—how much depends on the momentum, et cetera. One of the advantages is that it's very quantitative—you know precisely how much you dig out of the mirror; whereas with rubbing—which is polishing or grinding—you never quite know precisely and quantitatively what you're doing. Now, we started to get interested in this possibility about three years ago, and first we had a number of tests done. Some of these tests were remarkably unsuccessful, but eventually all the bugs were gotten out, and we are now regularly sending segments from Keck I to Kodak, wherever the factory is. It takes on the order of a few days, and since usually time is money, but money is time also, it is therefore a total expenditure that is considerably less than one would do in the polishing procedure at Itek. So, as far as I know, the idea is to have essentially all Keck I and Keck II segments finished with ion polishing. It means that the polishing at Itek and at Tinsley can be done to a lesser degree.

Begin Tape 2, Side 2

I would not be surprised if Keck I will begin with a mixture of segments that went through the ion polishing and ones that did not. But the ideal course for each of the Keck telescopes is that on the order of every two or three weeks, a segment will be lifted out for cleaning and/or re-aluminizing and replaced by a segment of the right shape immediately, so that one can go on with the observing. And I imagine that when segments are taken out for cleaning or re-aluminizing, that they will be sent to Kodak to get their final figure. And this has meant—and somewhat to my surprise, because ion polishing was not in anybody's mind in the early 1980s when these design criteria were set up—that several of our segments now have precisely this shape to where eighty percent of the light is within a quarter of second of arc.

MOY: So, what will happen now is that the stress polishing will be done and the ion polishing will be the touchup that had been tried earlier with the computer-control polishing. So do you still expect to use the warping harnesses?

SCHMIDT: Yes. I think nothing will be done to that. I expect that that will be the same. I just expect that the procedure at the end at Tinsley and Itek will be curtailed a bit. They will try for lesser goals but will still be well within the range of what Kodak can take out.

This is very exciting, I think. In the best seeing conditions, it's the telescope that will more or less also decide how large the image is. When the atmosphere is particularly quiet, we can now have an image that is two-and-a-half times smaller in diameter, which means that the area of the image is six times less. That's a potential gain of a factor of 6, again. Now, I'm not saying that it comes in addition to the 30, because I already took into account the technical ratio of seeing diameters, but this really suggests to me that on nights of exceedingly good seeing that we will probably, to my mind, be working with gains of 100 or 150.

MOY: That actually leads me to a question just for you personally: If we can stipulate that you're going to have several good nights of very good seeing, what sorts of things do you think you would start to work on with the Keck Telescope that you might not have been as eager to try with Palomar?

SCHMIDT: Well, it's likely that I would actually go on with the things I am doing at the moment, which is search for distant quasars. That is a rather slow process. The successful part of the program has been going on for on the order of four or five years, and we now have a bunch of ninety quasars that have a redshift larger than 3 or so—between 3 and 4.9. That's still pretty slow going. It would be very agreeable to be able to do that much faster. One has to go through enormous amounts of numbers of objects and tests on the basis of an initial survey. Based on an initial survey, all candidates for this quasar have to be tested, separate spectra taken, and that really is time-consuming. If that can now be done very much faster, we can also go considerably fainter, to where we will get new information for quasars of lower luminosity and perhaps even larger redshift. It doesn't sound terribly imaginative that I want to go on with a program I've already been doing. [Laughter] I actually think that a major advantage of this telescope will be in the field of galaxies rather than point-like sources. And the reason is that with quasars, we've already gotten, with these large redshifts, to where we look back into the first billion years of the 15-billion-year-old universe. So you can push back a little more, but you're already plenty far back. With galaxies, it's hard to say where we really are in that arena. But it's more like if you want to look halfway back in time, with galaxies you already have quite a time. And the information gotten is probably very slight. And people should be able to study these objects much better. Then, of course, there are some galaxies that have been seen farther back, but those are probably a handful or so—of the order of ten or twelve—and those have been very poorly studied. I can hardly believe you can see anything from the spectra, unlike with the quasars. And the reason why for galaxies it's so important to look back in time and see the first stages in their life and perhaps things that have to do with the end of their formation, is that galaxies are building blocks that we understand quite well. We live in a galaxy. And the galaxy has a disk of stars, and it has a halo, and we know there are differences in the chemical composition of the two, and we understand roughly why that is. We have globular clusters, Cepheids; we have spiral arms; we have gas in the plane. We understand these things fairly well and would like to know how things happened. And it will be very informative.

With quasars, we have this crazy business of something that's not much bigger than the solar system emitting, say, 1,000 times as much as a whole galaxy; it is a very exotic object. The nearest is still a half-billion light-years away. It's not something we're familiar with. It's interesting to penetrate that further into the past, and that's going particularly well. I don't want to discount the work on quasars—I'm doing nothing but quasars—but it's in the arena of *galaxies* that we would really wish to know what they were like in the first 1 billion or 2 billion years. With quasars, we find out and we find the difference, and we say "Oh." With galaxies, if we see what they looked like in the first 1 or 2 billion years, that will be tremendously informative, because we already have lots of knowledge of the situation at this time and how it was a few billion years ago, and then it starts to fade out. There's a lot of room for improvement in our insight into the formation of our own galaxy, other galaxies, clusters of galaxies, and so on.

Quasars are still pretty much the "Wow!" business, but they still essentially escape from being well understood. We take it the way it comes. When quasars tell us it was that way at that time, we say, "OK, I accept." If I knew what our galaxy looked like 500 million years after the beginning, I think I could spend the rest of my astronomical life just working on that and fitting it into what we already know, and making conclusions about this or that—it would be enormously informative. Well, when I find out what a distant quasar is like—as I say, I just accept it. With quasars, we're still more in the descriptive stage. And with galaxies we are in the area of understanding. We have lots of ideas of about how it all must have come about, but in many cases we are uncertain. That's where the Keck, I think, will be priceless. It will really allow us to do things with distant galaxies that you now wouldn't think of.

MOY: Is there anything on the technological horizon for astronomers that would contribute to understanding the evolution and structure of quasars the way you're describing?

SCHMIDT: Yes, but that's for the next century. That's LIGO [Laser Interferometer Gravitational-wave Observatory], the gravitational-wave business. Quasars, when they were formed, almost certainly went through a collapse and/or accelerations that were so enormous that those events must be observable over most of the universe.

LIGO is seen as a rather far cry by many of the astronomers, but one of the [exact] reasons why we don't understand quasars too well is that they are so compact and so dense that you don't really see what's happening in the very interior part, where the black hole is. You see things around it—an accretion disk. It's still very small—[the size of] the solar system; it's ridiculous—but you don't see that very small thing [inside], and in order to better understand what is happening, you have to get out of most of the radiated areas, where there are opacity effects. It's like the sun: You just don't see inward, you see the surface. And if you really want to understand what happened and how [quasars] formed, you have to look at the effects of acceleration, and you get into gravitational waves.

MOY: I'd like to talk very briefly about Keck II. You mentioned last time that you felt that the seeds for the second telescope had been sown early. But do you recall how you personally found out that there was going to be a second telescope? Did this come as a bolt from the blue?

SCHMIDT: It happened when Wal Sargent and I were sitting here drinking coffee in the morning and there was a CARA board meeting. And [Julian O.] von Kalinowski, Mr. Keck's lawyer, came by. He was a bit early, so we offered him some coffee. And we sat down, and he just let on about a second Keck telescope. We were sitting there, sort of so [mouth open wide]. It's more difficult to know when that was—quite a while ago. It probably was in the spring of 1988. But that is terribly difficult—it's so easy to be a year off.

Well, we kept it to ourselves. And then at a later time, while Ed Stone was still division chairman, I seem to remember there was a faculty meeting about it, at which it was requested that we would be quiet about it, which is rather difficult for people, in general, it seems to me. You have forty people in the faculty meeting. Anyhow, I told you how I first found out.

MOY: And had you had any intimations before then, that this was coming?

SCHMIDT: No, that was the first time.

MOY: Are there any ways, aside from ion polishing, in which Keck II will differ from Keck I, or will it be pretty much the same?

SCHMIDT: I don't have much direct knowledge about it. From what I know or would suspect, it is going to be quite similar. The major advantage of a second one, in terms of cost, is that you don't have to redesign, and in particular you don't have to go through the whole new setting up of optical polishing procedures. So I think Itek is already working on segments for number two, and they hardly know whether it's number 36 on the first or number 5 on the second. It all is the same thing. So that advantage is tremendous. You can just go on with things. I'm sure that in the auxiliary instruments, at the back end, there will be differences, because it doesn't make sense to duplicate something that is rarely used, and you can take a larger variety of instruments there. But that's still under discussion.

MOY: Will there be any management changes that you know of?

SCHMIDT: As far as I know, there will be a third partner.

MOY: NASA.

SCHMIDT: Yes. But that situation is still unclear, because while it is known that it will be NASA, I don't think there is congressional approval for it yet. So this is something that we don't broadcast too much. But obviously NASA cannot do this without congressional approval.

MOY: Is there any notion at this point how NASA will come in, in terms of money?

SCHMIDT: Yes, I had the impression that they would come in on the order of one-third of one telescope.

MOY: How did that come about?

SCHMIDT: As far as I know, Mr. Keck essentially indicated that there should be a third partner. And that's all I know. We talked around quite a bit to find a third partner. In fact, as far as I remember, we had brief discussions with the Carnegie Institution to see whether they would come in. They are, at the moment, in a consortium that has broken apart, because Johns Hopkins has stepped out on an 8-meter, so they are now going for a 6.5-meter. Things are very complicated with all these consortia and groups. So it finally became NASA, and I hope it will work out well.

MOY: That's interesting to me that this would be the idea of Howard Keck, because my impression of him is that he is generally fond of private institutions but not very fond of public institutions.

SCHMIDT: No, that's clear. But I don't think he specified that it should be NASA, I think he specified that there should be a third partner.

MOY: And do you recall that at some point he even suggested investigating the possibility, early on, of cutting UC out, partly because of his lack of fondness for public institutions like a state university?

SCHMIDT: I see. No, that I'm not aware of. And that was for [Keck II] he was contemplating that for, you say?

MOY: I think that was even for Keck I, actually.

SCHMIDT: Oh really. I see. If that happened, then I'm sure Caltech objected and said that we just couldn't do that.

MOY: OK. What sorts of things will we be able to do with two telescopes that we couldn't do with one.

SCHMIDT: There the interferometry comes in, and you may know that in the early days here at Mt. Wilson, [Albert A.] Michelson mounted a beam on the 100-inch so that he could take the light from two positions well outside the 100-inch mirror, deflected back into the mirror, and then do interferometry on the two images and that way get fringes and measure diameters of stars or double-star separations, et cetera And essentially when you have two telescopes—but in this case at 85 meters' distance—you can do the same thing. It does become very difficult, because the accuracy with which the path links have to be the same for a central fringe are extreme. And in all this business, it turns out that when you go to shorter and shorter wavelengths, it becomes difficult very, very rapidly, which means that there should be no trouble at all doing it at 10 microns, and that is well into the infrared. Then it would be very interesting to go down as far as 2 microns, where I have no idea how difficult it would be. I do have the impression that it would be immensely difficult to do it very well at 5,000 angstroms, the middle of the visual wavelength range. But I'm not an expert in that, and there are quite a few people here who know much more about it. So at least it can be tried, once they are in place. It can be tried quickly at a longer wavelength. The challenge really is to do it also at shorter wavelengths.