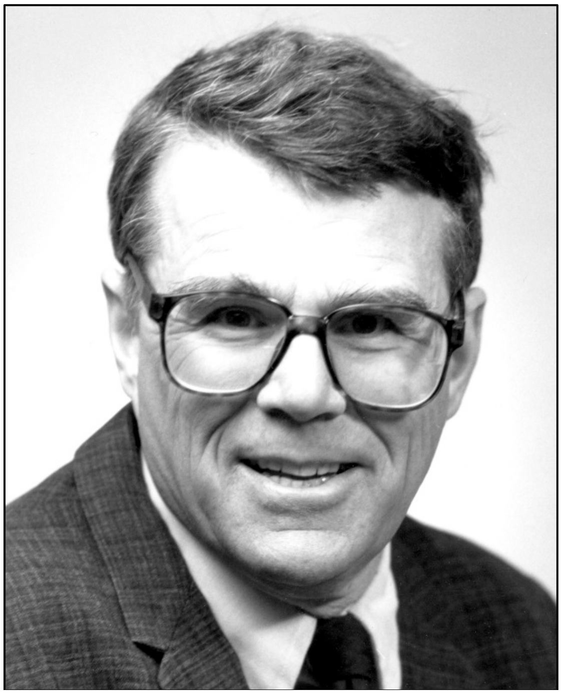

Gerry Neugebauer, 1986

GERRY NEUGEBAUER
(1932 – 2014)

INTERVIEWED BY
TIMOTHY D. MOY

July 15 and 16, 1991

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Preface to the Keck Series Interviews

The interview of Gerry Neugebauer (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, July 1991, with Gerry Neugebauer, Robert Andrews Millikan Professor of Physics in the Division of Physics, Mathematics, and Astronomy (PMA). Received his PhD in physics from Caltech (1960) and joined the faculty in 1962 as assistant professor. Lead scientist on IRAS (Infrared Astronomical Satellite), launched in 1983; director of Palomar Observatory 1980-1984; PMA division chair 1988-1993. Discusses his role in construction of Keck I, the first of W. M. Keck Observatory's two 10-meter telescopes on Mauna Kea.

Recalls his early interest in astronomy; switch to physics at Cornell; Army career working on Mariner program at Jet Propulsion Laboratory. Discusses his stint as Palomar director and its light problems; involvement, beginning in 1983, in planning for a 10-meter telescope with his former student Jerry Nelson and PMA division chair Edward C. Stone. Competing designs. Caltech's decision to go with University of California, then expecting $36 million from Hoffman Foundation, as "junior partner." Involvement of Keck Foundation; early notions of building a Keck telescope and a Hoffman telescope. Caltech becomes equal partner. Establishment of CARA (California Association for Research in Astronomy) to run the observatory.

Comments on Mauna Kea site, conflict with UC over siting the headquarters: Waimea or Hilo. Selection of Waimea; Smart Trust. Discusses design and manufacture of the 36 mirror segments. Comments on difficulty working at telescope's altitude; on partnership between UC, Caltech, and University of Hawaii. Continual drive in astronomy community for larger telescopes; adaptive optics. Itek's problems with polishing the mirrors. Status of work on Keck II.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH GERRY NEUGEBAUER

BY TIMOTHY MOY

PASADENA, CALIFORNIA

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

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Gerry Neugebauer **by Timothy D. Moy**
Pasadena, California

Session 1 **July 15, 1991**
Session 2 **July 16, 1991**

MOY: First, some brief personal background. Where were you born?

NEUGEBAUER: In Germany—Göttingen.

MOY: How long did you stay there?

NEUGEBAUER: Two years.

MOY: So you grew up in the United States?

NEUGEBAUER: We moved to Denmark, and then came here when I was seven.

MOY: Your family background?

NEUGEBAUER: Well, my father [Otto Neugebauer] was a historian of science; a mathematician first; generally, the Renaissance man. He knew about a lot of things in science, and he got me interested in science. I guess I've been interested in astronomy since I was in high school. I went to Cornell and got uninterested in astronomy because of the teacher there, so I went into high-energy physics. After I graduated from Cornell, I came back and worked in high-energy physics and the synchrotron. I was going to go into high-energy work, but I had to go into the Army, and I was stationed at JPL [Jet Propulsion Laboratory].

MOY: Did you volunteer in the Army?

NEUGEBAUER: It was ROTC; it was essentially conscription. Because Cornell is a land-grant college, everybody has to go into ROTC, so I had no choice in the matter. At JPL, Richard Davies decided I should do work on one of the planetary probes, so I started working on an early planetary probe—one of the early Mariners. I did infrared work on that. I sort of shifted over at that point and came back to astronomy, which is what I had liked before, and I've been doing infrared astronomy at Caltech ever since.

MOY: You got your PhD here at Caltech, is that correct?

NEUGEBAUER: Yes, in high-energy physics—the synchrotron.

MOY: Your interest in astronomy, you say, stems from high school?

NEUGEBAUER: Oh, yes.

MOY: Do you remember any teachers or books that inspired you?

NEUGEBAUER: My father.

MOY: I've noticed your statements concerning light pollution around Palomar. What is the current state of that? Has it gotten better?

NEUGEBAUER: Well, you have to go through that in sequence. I got involved in running Palomar in 1980. One of the big things that was really hurting Palomar was light pollution. We'd gotten into that because there was a girl in the development office of Riverside County, in the land-use office, who said, "Hey, all these developments are springing up. Isn't Palomar going to be worried?" She called me up. So we started working very hard on trying to make the ordinances such that people would be conscious of Palomar. This came at a time when all streetlights had to change anyway, from incandescent lights to sodium-vapor lamps, because of energy conservation. But there are two kinds of sodium-vapor lamps. There are the high-pressure ones, which are white and almost pinkish. Those are very bad for us; they're as bad as an incandescent lamp. The low-pressure, the bug lights, are a really deep orange, and they make everybody look mono-chromed; they're the ones we want. They emit only one spectral line. That's why they remove all color; all you see is that one color. Luckily, they save a little bit more energy than the other ones do.

After that, I worked on the issue alone for two years, and then Bob Brucato joined the observatory. He's taken over the light-pollution thing; it involves dealing with, I think, some thirty different agencies—Caltrans, cities, counties, all the different people have planning groups.

MOY: Are you winning that battle? Or is it a stalemate at this point?

NEUGEBAUER: Well, we're winning in the sense that most of the counties have adopted ordinances that go in our favor. We're losing in the sense that streetlights are only about a third of the light in the sky, and the population is increasing by a factor of 2 every five years. A third is better than nothing, but in the long run, we're losing. It's going to change the nature of the work that's being done at Palomar.

MOY: On to the Keck Telescope. How did you first become personally involved in the project?

NEUGEBAUER: Well, I was fairly deeply involved in the beginning. I was the director of Palomar, and I basically took two years off to run IRAS [InfraRed Astronomical Satellite]. And when I came back from that, there was a question: "What should we do?"

I was worried about the future of Palomar. At that time, there were, and there still are, two competing ideas for how you should make a big telescope. On the one side, there was Jerry Nelson. He was a student of mine; he was my first student. Jerry was working at the Lawrence Berkeley Laboratory, and he wanted to build a big mirror, a 10-meter telescope, out of segmented mirrors. On the other side, there was [J.] Roger [P.] Angel at the University of Arizona, who wanted to build a big single dish. They were both basically asking for money from the NSF [National Science Foundation]. Just at this time, I was put on an NSF committee that spent three days going to Tucson and to Berkeley to evaluate the two different approaches.

MOY: What year was this?

NEUGEBAUER: It was the summer and fall of 1983. So I got a fairly thorough background briefing on both of the competitive systems. When I got through that review, I decided that we at Caltech had to make up our minds on something. So I got together with Ed [Edward C.] Stone, who was division chairman [of Physics, Mathematics, and Astronomy] at that time—I was director of Palomar, and he was division chairman—and I said I wanted to see where we'd go. I had a series of meetings with all the astronomers here at Caltech. We debated the whole issue of what was the right way for our future. And it was not clear. There were some people who said the right way was to go from a 5-meter telescope, which is Palomar, to a 10-meter; that's one way to go. There were some people, and they had very sound arguments, who said that the right way to go in the future was to stick with a 5-meter telescope but to put all of our money and investment into instruments. It was a real debate. But when we looked at all the pros and cons, we decided we had to go to a big telescope.

At that point, the debate became: Which way do we go? Do we go with UC [University of California] and the segmented mirror? Or do we go with the single mirror? Several of us went to Tucson and to Berkeley, and sort of talked to the people and tried to look at the two competing things. UC was already pretty well along, and we decided that it would be the right thing for us to get involved with the UC telescope.

At that time, UC had [been offered] $36 million from a woman who was the widow of the [automobile magnate]—[Max] Hoffman, I think, was the name. They had agreed that they would make the name of the telescope—the dome, and the observatory—the Hoffman Telescope. Well, all they had was $36 million, and $36 million simply was inadequate. We went up to them and, first of all, said that we'd decided to go with the 10-meter and that we wanted to go with them. The proposal was that we'd be junior partners with UC.

MOY: Were these primarily astronomers talking to astronomers, or administration talking to administration?

NEUGEBAUER: Well, that was a long procedure. After we had talked—astronomers talking to Ed Stone and me—Ed and I went to our administration and convinced the administration that the right thing to do was for us to go into it. The most that UC seemed agreeable to was that we go in as junior partners—"junior" meaning that we were somehow a fifth of the project.

Jerry [Gerald M.] Smith I had known because he worked on IRAS with me, and I'd known him before that because he worked on IRTF [InfraRed Telescope Facility] with me, which is another telescope [on Mauna Kea]. So we went up there, and that's when we understood that, first of all, $36 million was not enough money to build the telescope, much less operate it.

I guess before we really understood the total money that we needed, we went up to the Caltech Board of Trustees and gave a presentation. Actually, I wasn't in town that day, so Wal [Wallace L. W.] Sargent and Ed [Stone] went up, and they sort of summarized the views of the astronomers. They gave a presentation to our board and convinced our board that it was OK for us to go out looking for the money to become junior partners.

MOY: And how much money was that considered to be, the entrance fee?

NEUGEBAUER: The entrance fee, that's the exact word. I think the entrance fee was not clearly thought out at the time, but it was some fraction of $36 million, or a little bit more; I think it was on the order of $25 million. It was just a silly number, in retrospect.

Then, Murph [Marvin L.] Goldberger, who was the [Caltech] president [1978-1987], was invited to visit the Keck Foundation. The reason was that at a dinner party earlier, Robbie [Rochus E.] Vogt, who was the provost at that time, was trying to enthuse one of the potential donors and had talked to her about astronomy. And Howard Keck had overheard this conversation. So Murph went to the Keck Foundation, not understanding their enthusiasm but having been briefed by us on the real value of doing this. Howard proposed to him, at that time, that he would fund the whole cost of the telescope, but given that Caltech was a dominant partner. It was at this time—and I'm a little bit vague on when we really understood what the total amount of money was—that we understood, by going over budgets with Jerry Smith and such, that the cost was closer to $100 million. And the real number is $85 million, the number that it will really cost to build the telescope.

Then there was still the question of operational costs. So we made up a preliminary budget and presented it to Keck, and the Keck Foundation agreed to donate that money. UC agreed that it would pay matching money in order to operate it. So the total cost of the thing is closer to $200 million. That's why I say $36 million was simply a crazy number to ever really think about.

MOY: If I can back up for just a minute, at first you said that it was primarily astronomers talking to astronomers. Who were the people in the UC system you would talk to?

NEUGEBAUER: It was Bob Kraft, as I remember, who was the main person. He was, and is, the director of Lick [Observatory]. Sandy [Sandra] Faber, as I remember, wasn't able to come, so the only one I really remember who came was Bob Kraft.

MOY: Hadn't there been some debate on different plans within the UC system, too?

NEUGEBAUER: Yes. Before that, the UC people had had an internal debate about how to make the big telescope. The protagonists were basically Jerry Nelson, who wanted to make the segmented kind, and Joe Wampler, who was pushing for a big monolithic mirror also.

MOY: And he was at Santa Cruz?

NEUGEBAUER: Yes, he was at Santa Cruz. The debate had gone on prior to all of this I'm telling you. And they [UC] had a committee of graybeards, which decided which way to go. They had decided to go the route of Jerry Nelson.

MOY: Could you elaborate a bit on how it was that the people here at Caltech were able to join, or sign onto, the project? Did the people at Berkeley approach you?

NEUGEBAUER: Well, they understood that they needed more than $36 million dollars, so they were seeking a partner to try to increase their finances. I think the administration at Berkeley was receptive to going in with the Kecks, but the astronomers felt they'd been sold down the river, because until then Caltech was going to be a minor partner. We eventually argued with the Kecks that we had to be equal partners with UC, and that's what we are now. I mean, Keck wanted us to be the dominant partner.

MOY: Do you think he wanted to write UC out of it completely?

NEUGEBAUER: No, no. Robbie Vogt insisted from the beginning that it was Jerry Nelson's idea. There was never any serious talk that we wanted to write them out completely; I think that was clear. But I think that the UC astronomers felt they'd been aced out of being the kingpins, so there was resentment there. The administration, which was more practical, always felt from the beginning that this was a fine way to go.

MOY: And the administration there being David Gardner, the president, and Bill Frazer, the [academic] vice president?

NEUGEBAUER: Yes. I think Frazer was the active guy in doing it, but he had to get Gardner's approval. He laid the groundwork, and then he got Gardner's approval.

MOY: When Keck came through with this offer, wasn't there some talk of having two telescopes, a Hoffman telescope and a Keck?

NEUGEBAUER: Yes. The question was, in the very beginning, how could we reconcile the fact that Keck wanted to have a Keck telescope, and UC wanted to have the Hoffman. So how could we reconcile that? How could we keep the $36 million dollars of the Hoffman money? There were a number of proposals. One was—which is actually the most sensible—that we build one in the Northern Hemisphere and one in the Southern

Hemisphere—in Chile, for instance. Scientifically, that's by far the most sensible. That wouldn't have worked out, because UC couldn't build a telescope in Chile. It's bad enough building one in Hawaii instead of in California, but completely out of the country was out of the question. And Keck, because he'd had dealings in the copper mines and such, would not stand for having a telescope in Chile. He had had bad experiences. So two telescopes that way didn't do it.

There was actually a thought at the very beginning of putting two telescopes right next to each other—and I still have a drawing that we made up, in the very beginning, showing two telescopes right next to each other, very much like Keck I and Keck II now. That has great advantages, because we recognized at the beginning that two telescopes were qualitatively and quantitatively better than one. That is, two telescopes allow you to do interferometry, and it effectively makes a 10-meter telescope go to an 80-meter telescope; it's a big jump. So, for a while, we thought the right thing to do was to have a Keck telescope and a Hoffman telescope sitting right next to each other, separated by 80 meters. Well, when you looked at the total money, that simply wasn't in the cards. So we went back to what is happening, namely a Keck telescope that is equally shared between UC and Caltech. But that dream of two telescopes has never died, either in our minds or in Keck's mind, it turned out. He heard about two telescopes, and it fired his imagination.

MOY: Let's talk a bit about CARA [California Association for Research in Astronomy]. CARA was formed out of the agreement, right?

NEUGEBAUER: It *was* the agreement. CARA was the vehicle that we had to run the telescope jointly.

MOY: How has CARA worked?

NEUGEBAUER: Well, CARA has, until now, been an engineering outfit, and as such, I think it has worked extremely well, because that's what you'd expect. Jerry Nelson joined CARA. It was a way that we could get together to run the whole telescope.

I think the only resentment at all was that UC astronomers felt that they had been pushing this thing for a long time, and now they were made, not second-class citizens, but only equal to other people; so they felt that all these guys are bringing in is money.

MOY: Had Caltech people been involved technically prior to the Keck arrangement?

NEUGEBAUER: Only as individuals. Keith Matthews [chief instrument scientist], who works over in the infrared group, for instance, was involved as a private consultant to the UC 10-meter telescope. He's the only one I really know who was involved before that. But that was something completely outside of this. They asked him to come in because he was an expert in infrared, and they needed somebody.

MOY: The UC people eventually had to give the Hoffman money back. Are you familiar with how that worked, or why that happened?

NEUGEBAUER: Well, I'm familiar with why it happened. It happened because, first of all, Hoffman had specified that it had to be called the Hoffman telescope, and it clearly wasn't going to be. Then other people in the Hoffman family, who weren't as intrigued by astronomy, wanted to have the money; they were more intrigued by the $36 million.

MOY: Has CARA functioned primarily through its board meetings?

NEUGEBAUER: No. CARA has functioned primarily as an engineering office, and it's functioned very well. There are three levels that CARA has functioned at: One is as an engineering office, and that's been directly responsible for building the telescope; that's run by Jerry Smith. There's been a committee of scientists—the SSC, Science Steering Committee—which is made up of three people from Caltech, three people from UC, and one person from the University of Hawaii—they're in there because they own the land. That committee has been responsible, first of all, for defining the specifications of the telescope: How well should it point, how good should the figure be, and all that sort of thing. It's a long document, which was generated after much discussion of what the real specifications of the telescope should be. After that, it was responsible for designing, or contracting out to individual campuses, the instruments that are going to be used.

Then the third level is the CARA board. If you have an organization, it needs to have a board of directors. The board of directors, again, has three people from Caltech and three people from UC. At the very beginning, it was Robbie Vogt as our provost, Ed Stone, chairman of the PMA division, and myself, as director of Palomar, on our side. On the other side it was Bill Frazer, who's the [academic] vice president of UC, Ron Brady, who's the vice president [for administration and] finance of UC, and Bob Kraft, as the director of Lick.

MOY: How was the day-to-day management run?

NEUGEBAUER: That was done through what I call engineering. That was done by Jerry Smith, who's [the project] manager, from JPL.

MOY: Would people get together every couple of weeks?

NEUGEBAUER: Well, as for the engineering part, they got a house here on campus and moved in. That was a really close-knit working project until they moved to Hawaii. There's now in Hawaii on the order of twenty-five people. So that's completely continuous. Now the SSC meets only once a month, and the CARA board meets once every six weeks or so.

MOY: Are there any items that come to mind as particular problems in administration?

NEUGEBAUER: Actually, the administration has worked very smoothly. The only serious problem was a problem about whether we should go to Hilo or Waimea for the headquarters.

Begin Tape 1, Side 2

MOY: How was the site on Mauna Kea selected?

NEUGEBAUER: I'd like to say that it was a very carefully thought-out thing, but it wasn't. The pressure of the time meant that we had to say something—you can spend a lot of time really doing a thorough site test. I knew enough about Mauna Kea, because I'd gone to Mauna Kea a number of times. We looked at it for the site of what's now called IRTF, and I was convinced it was a good site. Other people were also convinced. So we just made the administrative, or executive, decision that Mauna Kea was going to be it. We did not do an exhaustive search of competing sites all over. We froze on Mauna Kea from the beginning.

MOY: And when you say "we," is this CARA?

NEUGEBAUER: The people from UC, I think, felt pretty much the same way. There was a little bit of conversation, not even an argument; all of us thought the same way. Bob Kraft certainly represented that that was the right way to go.

MOY: What are some of the features of the site, besides its altitude?

NEUGEBAUER: The sites that are good for astronomy turn out to be sites that are high, or near water, so you have very smooth air flowing over it. That's why the good sites are here in California, in Chile—Chile is geographically identical to California, except it's in the Southern Hemisphere. The mountains are within a few miles of the water. Or you can go to an island, and you have the wind blowing over the island in a nice smooth way. Mauna Kea is higher than most mountains around here.

MOY: Were there any other sites in the running?

NEUGEBAUER: No. We did not consider other sites.

MOY: What about the debate on the site of the headquarters—Waimea or Hilo?

NEUGEBAUER: That was the one place where, I think, relations between UC and Caltech got somewhat strained. It reflects the different personalities. The difference in sites is that Hilo is a non-tourist town. It was the center of the sugar plantations in Hawaii. It was a big agricultural center—heavy machinery. It rains 100 to 200 inches a year; it's always overcast, even when it isn't actually raining. It's not what you'd call a tropical paradise. On the other hand, Waimea is inland a bit. It's right at the demarcation between the wet side of the island and the dry side. It's an idyllic site. It's not very heavily developed, and the development that's going on now is clearly going in the leisure class. It's more like what you have a picture of Hawaii being. It's fifteen minutes from the best beach on the Big Island.

The Caltech people, in general, who were more inclined towards engineering and heavy construction, wanted very badly for it [the headquarters] to be in Hilo. And the UC people, who were in general more inclined toward the niceties of life, they wanted Waimea. So it was basically an impasse. I think the thing that finally made it get decided was Keck. In both places, we were offered land essentially free. In Hilo, the land was state land that was given for a dollar a year for a hundred years. And in Waimea, it was given outright. I think Keck didn't like the association with the State [of Hawaii]. I think he sort of gave the swing vote.

MOY: Had you known Howard Keck before?

NEUGEBAUER: No, not before this.

MOY: I sometimes get the impression that he has been very determined that it be a very private, as opposed to a public, organization.

NEUGEBAUER: I would say that Howard has not been deeply involved with the real running of it; he trusts Caltech and UC to take care of the running of it. On the other hand, what you say is perfectly right. He makes it clear that he doesn't really trust the State. But on the other hand, he's careful to not micromanage; he's very good that way.

GERRY NEUGEBAUER

SESSION 2

July 16, 1991

Begin Tape 2, Side 1

MOY: Just one point of clarification from yesterday regarding the site for the headquarters: Hilo, you mentioned, would have been from the State of Hawaii. Do you know from whom Waimea was acquired?

NEUGEBAUER: Yes. It was Richard Smart, who is the head of the Smart Trust; it's part of the Parker Ranch.

MOY: Had there been discussions between the UC people and the Smart people?

NEUGEBAUER: Yes. I think there had been discussions between them. So I think that's one of the reasons they favored it.

MOY: Do you know what was involved for them? Was there some incentive for some other funding?

NEUGEBAUER: I don't really know. But I know that they definitely favored Waimea, and they had connections with Smart.

MOY: On to a few general questions about the design and construction. Who, would you say, have been the principle designers of the telescope, aside from Jerry Nelson? Is there anyone else who stands out?

NEUGEBAUER: Well, Jerry Nelson had the ideas behind it. But if you want to talk about *building* the telescope, then it's Jerry Smith, because the person who really pushed that something get done and that compromises be made, and made in an orderly fashion and on time, that was Jerry Smith. And I don't think that Jerry Smith, as far as I know, has ever really hurt the telescope, in the sense of making it perform less well. But I think he's had a very realistic view. There have been some technical compromises, largely on the quality of the mirrors, for instance.

MOY: Let's talk about that a little bit. Would you say that the fundamental new feature of this design is the mirror?

NEUGEBAUER: Right. It's a way of getting a 10-meter mirror for a reasonable price. The Japanese are building an 8-meter telescope, and they have $370 million, or some large number like that, in comparison with under $100 million for this one. It's a big change.

MOY: What process are the Japanese using?

NEUGEBAUER: They're using a big single mirror. They're not using spin-casting, but they are using a monolithic mirror.

MOY: Could you summarize the procedure for making the mirror segments?

NEUGEBAUER: There are two features of this telescope that are innovative. One is, instead of making one big mirror, making the thirty-six smaller mirrors. And there were technical things which were necessary to overcome in order to make the telescope this way.

The first technical challenge was, how do you make 1.8-meter mirrors inexpensively? Basically, the mirrors have to be in the form of a parabola after they're assembled. But it's much easier to polish them as spheres. So what you do is you take the piece of glass and "stress polish" it. You bend the glass with weights along the edges, in just the right shape, you polish that into a sphere. Then, when you release the weights, it springs back to the right shape. It's a very clever idea. It was Jerry Nelson's idea. That has turned out to work. It took more effort than was initially envisioned, because it turns out that if you do this, each one of the thirty-six mirrors has to have exactly the right prescription. That is, it has to be a parabola—actually it's modified somewhat from a parabola—with the right focal length and to very exact tolerances. Ordinarily, when you make a mirror, you don't care what the focal length is, because that's something that you adjust in the telescope. The people who had to build this had to be really trained. The opticians, who mainly are craftsmen, not scientists, had to be trained in doing things in a very different way and thinking about things very differently. And that took a lot longer. It was, I think, to a large extent, a personnel problem. Once that got cured, then another technical problem came up—in order to put all the mirrors together, they have to be cut from circular mirrors to hexagonals. And that cutting relieved strain, so they bent a little bit. Jerry Nelson came up with a solution to that: In back of the support mechanisms, we put what he calls "warping harnesses." It's just a simple little spring with a screw put into it that bends just a little bit to put a little bit of pressure onto the glass. So by the combination of these two things, it turns out, the individual mirrors can be made.

The third big technical problem, which turned out to work relatively easily, was to make all the mirrors play together. That was demonstrated on nine mirrors; they could make nine mirrors all point together and act as if they were one big mirror.

MOY: And to within what tolerances?

NEUGEBAUER: It's down to a millionth of an inch.

MOY: How difficult is the calculation for the stress polishing? How much is trial and error?

NEUGEBAUER: It is actually iterative. But you can calculate it quite well, because the geometry is relatively straightforward. So you just take a disc and bend it. But the real answer is that for the first one that you do, you put on a series of weights, then you polish it. Then you understand, after you release it, what happened to it, and you can do a second iteration. But the first-order calculation is quite straightforward, and you can do it.

MOY: How much weight would you use? Roughly, what would the masses be?

NEUGEBAUER: They're measured in grams. It's reasonable weight.

MOY: You described what some of the technical problems were. Were there any that turned out to be easier than you had anticipated?

NEUGEBAUER: Well, I think the problem of making all of them work together turned out to be slightly easier.

MOY: And that is done with microprocessors?

NEUGEBAUER: Right. Each of the thirty-six mirrors is supported in thirty-six places. It's actually driven in three places; it has three actuators. So each of the thirty-six mirrors has a computer sitting right with it which tells it how to move.

MOY: You've been to the site. What has it been like working up there?

NEUGEBAUER: It's rough. It's a desolate place.

MOY: How would you adjust to the lack of oxygen?

NEUGEBAUER: There's no way you can adjust to it, except going up a few times. But I think, even at that, you're not as good working up there as you are down below. The main thing is that you don't care very much, and you don't really try to get to the bottom of problems while you're up there. Then as soon as you go down, you say, "Gee, I should have thought about this and this."

MOY: I've heard that some people would make lists for themselves down at the bottom.

NEUGEBAUER: Yes, well, you do that all the time. You try to figure out what you're going to do, and then you go. But again, the problem is that if you do something and it looks like it works, you say, "Well, yeah, it works. On to the next thing."

MOY: The impression I have is that you don't realize that you're in this condition while you're there.

NEUGEBAUER: Yes. That's part of the not caring. You don't realize that you're stupider. And that's why we are going to have remote operations; that is, people are going to work from down in Waimea to control the telescope.

MOY: They'll control it mechanically from headquarters?

NEUGEBAUER: Yes, you can control everything either from headquarters or from the mountaintop. Everything's being built so that you can do that. It's a complicated combination of optic fiber and microwaves and telephone wires.

MOY: So astronomers will practically never have to go to the top.

NEUGEBAUER: Well, there's some problems with that, too. There are going to be problems with maintaining the quality of the equipment and the people who go up. If the astronomers never go up, then everybody who has to go up will think they're second-class citizens. And that's very bad.

MOY: Could you just summarize the various contributions from Caltech, from UC, and then from the University of Hawaii?

NEUGEBAUER: I think that it's not right to, because I think that after the initial unpleasantness I described yesterday, the two sides have worked together very closely. I'm not going to go through and say this was a Caltech effort and that was a UC effort. I think that individuals from both sides contributed in their own way. Some people clearly contribute more and some people contribute less, but it's an individual thing, not depending on whether you go to Caltech or UC. It's been a total team effort.

MOY: I've heard and read that after the initial unpleasantness, that it really became very collegial.

NEUGEBAUER: Yes, I think so.

MOY: Did that surprise you at all?

NEUGEBAUER: Well, there was never any unpleasantness on our side, because we felt we were getting a fair and good deal. It took some while for the UC feeling that they were being robbed to wear off.

MOY: At the time, say in late ’84, early ’85, when Keck had made this offer to [Caltech president] Goldberger, do you recall how you felt about how the project might unfold? Were you concerned that the financial situation might have soured things in some way?

NEUGEBAUER: I wasn’t worried about it. I had just come off of IRAS, which was a combined United States, Dutch, and English experiment. And a great success. If you could make three nations work together, you could make two universities work together. The cultural differences between the nations were much more severe than between us.

MOY: And what about the role of the University of Hawaii?

NEUGEBAUER: They have ten percent of the time. So far, they’ve attended the meetings of the SSC, and that’s been their only involvement. It’s been pretty minimal.

MOY: When, do you remember, did astronomers begin thinking that it would be necessary to have a telescope larger than Palomar? Do astronomers always want bigger telescopes?

NEUGEBAUER: Yes.

MOY: Was there anything new, in the middle or late seventies, that brought this to a head?

NEUGEBAUER: No, there was nothing new. In fact, that's one of the problems of trying this. In some fields, you can have a threshold. For instance, in particle physics, everybody knew that when the bevatron worked, it was going to produce a Nobel Prize; the first person who ran the bevatron was going to discover an antiproton and he was going to get a Nobel Prize. The threshold was set at that energy; that was a calculable energy.

So one of the things that we're looking for is a threshold experiment in astronomy, where you go from 5 meters to 10 meters and suddenly something is possible. One of the things that struck me in our conversations leading up to this was the vivid feeling I had when I heard [Australian astronomer] Jeremy Mould give a list of things that you would do with 10-meter telescope. I thought, "Hell, if we just took everybody and said that they would do this on the 200-inch, we could do it." The trouble is that you can't get everybody to do it. And remember, if you ask the question, "What does a 10-meter do that a 5-meter doesn't do?" for certain problems, the time you take to do that problem goes not as the square but as the fourth power of the ratio. So it goes as 2^4, which is a really big factor. That means that right now we do problems that take four or five hours' observing time, which is about as much as you can do before things leave your sight. Well, this now suddenly means that you can do something that would take sixteen nights of four hours. That makes a big difference, because you wouldn't dream of doing that at Palomar, as a practical matter. If all the astronomers got together and said, "This is what we're going to do," then you could do it. But you've got to somehow divide up the time so that everybody gets to apply their expertise to it. So a factor of 16 is a really big factor.

So, as I say, there's not a threshold. It's not a watershed, where once we have this telescope, we're going to find something. But it'll make life significantly easier.

MOY: I've read that detectors have been getting more and more efficient.

NEUGEBAUER: Yes. Film, at best, was a fraction of a percent quantum efficiency. Now we're talking about detectors that have seventy-percent quantum efficiency. In general, the efficiency gains as the square in observing time. So if you have 100-gain sensitivity, that's 10^4 less time.

MOY: So it really is simply a matter of bigger mirrors, bigger telescopes. There's really no end to that.

NEUGEBAUER: Right, there's no end.

MOY: And with spin-casting, or the rubber mirror, or the segmented mirror, do there seem to be practical limits to how large you can make a mirror?

NEUGEBAUER: The other limitation on making a big telescope, which is fundamental, is that you have to improve—or else you can't take advantage of the telescope—the spatial resolution. That is, if you had a blur circle which was as bad as the 200-inch when it was built, for instance, it would just destroy the value of the Keck telescope. So the optics have to be better. You have to have a higher resolution. That's why the Hubble Space Telescope, even though it's 2.4 meters, or something like that, is potentially so good, because there's no blurring due to the atmosphere. So the requirements are increasingly severe to have good optics and good seeing conditions. That's the big push that you have to make.

MOY: What sorts of things can you do in that respect for ground-based telescopes? You could just make them higher, obviously.

NEUGEBAUER: Well, high is not enough. The thing you can do is worry about the temperature. You've got to try to make the temperatures of the telescope and structure very constant. You've got to try to make it so that the air flow that goes in front of them is laminar.

You can also improve the seeing by using what they call adaptive optics. That is, you can put a third mirror in there, which, instead of being a big mirror, is made up of what you call a rubber mirror. So one of the things which is happening right now is that people are building up adaptive optics.

If you don't do anything, you'll get on the order of a second of arc image. If you work at it, you can get down to a third of a second of arc. If you do adaptive optics, you can get down to a tenth of a second of arc. And since the gain goes as a square of this number that I'm telling you, there's a change on the order of 10, which you square and it's another factor of 100.

MOY: What was the anticipated resolution of this design back in 1985, before anything had been done?

NEUGEBAUER: Two-tenths or three-tenths of an arc-second.

MOY: And now what is expected?

NEUGEBAUER: That's about what we'll get.

MOY: For the mirrors, the original contractor was Itek, is that right?

NEUGEBAUER: That's right.

MOY: And at some point there had been a decision to have Tinsley do some of the work instead. Was that the personnel problem you referred to before?

NEUGEBAUER: Well, it was a result of that; Itek was taking too long to do the stress mirror polishing. In addition, they have other things that they have to do. They have to do the cutting and the measuring of what the real figure is, which takes a big facility. In order to get it done on time—because time is money, literally—it was decided to set up another line of stress mirror polishing. Itek has actually done quite well, after a rocky start. And Tinsley took some time to come up to speed. But now it looks like we're going to get it done before the end of this year.

MOY: Are there any sorts of data that we can get with the 10-meter that we could not have gotten with the 5-meter?

NEUGEBAUER: Not really. You can get it in a finite time now, whereas before it would take much longer. That's the real difference.

MOY: Are there any kinds of data that we simply can't get with the size of telescopes of the magnitude that we have now? Is there some kind of threshold in the distant future, for 100-meter or space-based telescopes, where we know there's some data out there that we simply can't get now?

NEUGEBAUER: No. Excuse me, there's one thing that maybe you can get from the ground by working at it: The way that we measure the size of the universe fundamentally depends on looking at individual stars in other galaxies of a certain type. So you have to have enough resolution so that you can look at an individual star and not be bothered by neighboring stars. That is what the Hubble telescope was built for, to do that. Now, the next closer galaxies that we're trying to do that with require seeing on the order of 0.2 of a second, 0.1 of a second. So you can do it. I predict that in another few years, the adaptive optics will be such that you can really do that from the ground. You may not be able to do it every night from the ground. But if, on the other hand, you can do it ten nights a year, the cost will still make it worthwhile.

MOY: If, after all thirty-six mirrors are in place and it's up and running, you were given the first full night of great seeing, what do you think you would want to work on?

NEUGEBAUER: I would actually want to look at a program that would take a number of years, but I would try and look at the center of the ultra-luminous galaxies. There are a series of galaxies that have a hundred to a thousand times more energy than the galaxies we know about. I would try to look at those and really study what makes them tick.

MOY: I'd like to talk briefly about Keck II, the second telescope. First of all, what's the status of the second telescope project?

NEUGEBAUER: We've ordered the glass, and the first blank should be at Itek in a few months. People are starting to build it.

MOY: How is it going to differ from the first telescope?

NEUGEBAUER: Not at all. I hope not at all, because that's the only way that we'll save money to make it feasible.

MOY: There should be savings, simply because it's the second time around?

NEUGEBAUER: Yes, because a big fraction of the cost of doing anything is in design. And we don't have to redesign. That's why I'm saying, "Don't change it at all." Itek has learned how to build mirrors, and if they don't forget it in the meantime, we're over that hurdle.

MOY: Will there be any management changes?

NEUGEBAUER: No.

MOY: Were you surprised when Keck came through with the money for the second telescope?

NEUGEBAUER: No. I knew he wanted to. He had the gleam in his eye. More or less from the beginning, he talked about it. I'm particularly interested in two telescopes because I like to do interferometry, and that's what two is really good for. And we'd been talking about it. Once it was known that we could make one work—he wanted that assurance—but once that assurance was given him, then, I think, he really wanted to do it.

MOY: When the first telescope was funded, the Keck Foundation contributed $70 million dollars. But it became pretty clear that that still left a $20-million shortfall.

NEUGEBAUER: They only give eighty percent of the cost by policy.

MOY: Had there been any attempts to try to get the remaining money?

NEUGEBAUER: Sure. But it didn't pan out.

MOY: Is optical interferometry essentially the same as radiowave interferometry?

NEUGEBAUER: It's theoretically identical. The only difference is that with radio interferometry, you can connect them by wires and cables, and you can't do that here. You have to build light pipes, which are variable length, at a fraction of a wavelength of the light. But Mike Shao, who works up at Mount Wilson, has demonstrated that he can do that already. So I think it's completely doable.

MOY: Has this been done before astronomically?

NEUGEBAUER: Yes. I think that Mike is the one who's demonstrated it the first time with the variable-delay lines. As far as I know, he's the first one. People have done crude interferometry by waiting until the seeing is just right and conditions are just right. But Mike is the first one who set up a system that works.