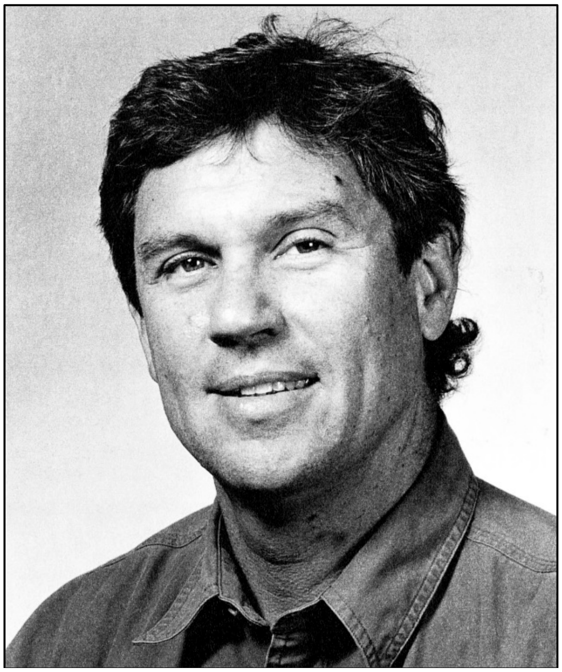

Jerry Nelson, 1994

# JERRY E. NELSON
**(1944 - 2017)**

**INTERVIEWED BY**
**TIMOTHY MOY**

**June 2, 1992**

**ARCHIVES**
**CALIFORNIA INSTITUTE OF TECHNOLOGY**
**Pasadena, California**

## Preface to the Keck Series Interviews

The interview of Jerry Nelson (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 June 1992 with Jerry Nelson, project scientist for the W. M. Keck Observatory from 1985 through 2012 and principal designer of Keck I, the revolutionary 10-meter segmented-mirror telescope on Mauna Kea.

He recalls his undergraduate years as a physics major (BS 1965) at Caltech, especially freshman and sophomore physics with Richard Feynman, and his work on 60-inch telescope at Mt. Wilson with Robert Leighton. Graduate work in physics at Lawrence Berkeley Laboratory; membership on UC's Future of Astronomy Committee. UC's various plans for a big telescope; his segmented design vs. Joseph Wampler's thin-meniscus design. Collaboration with Terry Mast and George Gabor; visits to Kitt Peak. Comments on support (or lack of it) for their design from UC astronomers and administration. Offer of funding from Hoffman Foundation and its collapse. UC/Caltech partnership.

Recollections of his interactions with other colleagues: Harland Epps, George Abell, Rochus E. (Robbie) Vogt, Gerald M. Smith, Harold Ticho, William Frazer, Edward C. Stone. Formation of California Association for Research in Astronomy (CARA). Discusses Itek's problem in manufacturing mirrors; success of the active-control system; his contributions with Steve Medwadoski to the telescope's space frame. Comments on enthusiasm of Caltech astronomers for Keck compared with that of UC astronomers.

## Administrative information

### Access

The interview is unrestricted.

### Copyright



### Preferred citation

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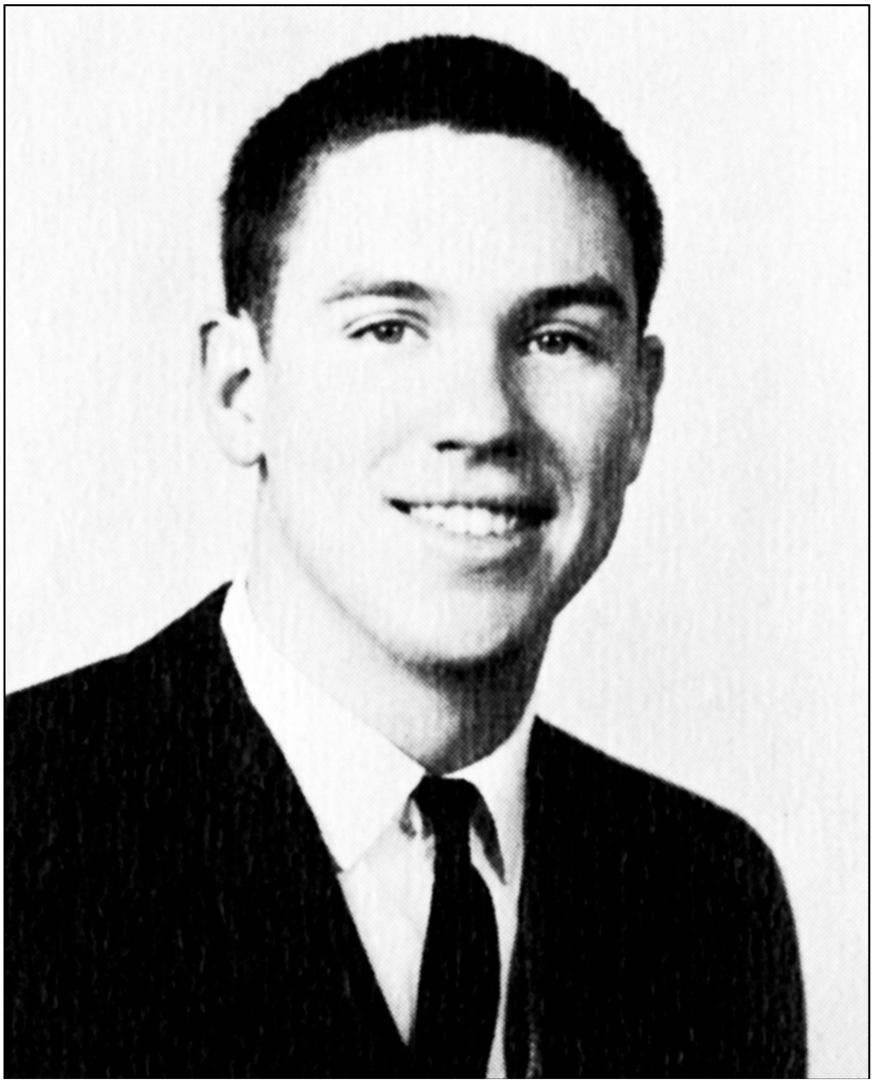

**Senior picture of Jerry Nelson (CIT, BS in physics, 1965)**
**1965 *Big T***

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

# INTERVIEW WITH JERRY E. NELSON

BY TIMOTHY MOY

PASADENA, CALIFORNIA

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# CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
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**Interview with Jerry E. Nelson** **by Timothy Moy**

**Pasadena, California** **June 2, 1992**

**Tape 1, Side 1**

MOY: Where were you born?

NELSON: I was born in Glendale, California, in 1944, and I grew up in Glendale—in the San Fernando Valley—and came to Caltech as an undergraduate in 1961.

MOY: What is your family background?

NELSON: My father was born and raised in Salt Lake City and ran away from home at the age of thirteen or something. He was living with his grandparents, and they were Mormons, and he hated it. [Laughter] He ended up coming to Los Angeles, and I guess by the time I was around, he was working at Lockheed Aircraft Corporation. He spent his whole life working as a tool planner.

My mother was born and raised in North Dakota. She ended up in Los Angeles as a dress designer. I'm not exactly sure how they met; they had some friend in common. They got married and had a daughter, and then two years later had a son—me.

MOY: So your father was around technical matters quite a bit.

NELSON: Yes. He was not an engineer, but he was very interested in technical things. He was a heat-treater for a long time, and then he was into tool planning, so he was always involved with machinery and all those kinds of things on one level or another.

MOY: Did they encourage you to come to Caltech? How did you decide to come to Caltech?

NELSON: From early on, I was interested in math and science, so it was clear to me that that was the direction I wanted to go. I think by the time I was in junior high school I had heard about Caltech and decided that Caltech would be a nice place to go. I guess that was as much as I thought about it, and then as I went into high school it was even more evident that I liked math and science and that I was unusually gifted at those things. And so it would be reasonable to consider pursuing it. So when I was a senior in high school, I applied to Caltech.

MOY: What was your major here?

NELSON: I started off as a math major, actually, and after a year I said, "Oh, that's terrible, man—be a physics major."

MOY: Did you enjoy it?

NELSON: I loved it. I thought Caltech was a great place; I learned a huge amount. The best thing here was that I was extremely fortunate, in that when I came here as a freshman, freshmen physics was taught by [Richard P.] Feynman. And sophomore physics was taught by Feynman. So I got two years of getting to watch Feynman three days a week. I don't know if you ever met him before he died.

MOY: I never actually met him but of course, they live and breathe him here.

NELSON: He was just awesome. He had a personality that you couldn't believe.

MOY: Was he significant in convincing you to switch to physics from math?

NELSON: Oh, sure. I mean, to watch this guy give lectures—he was such a powerful personality. It was like nobody else had ever been. I mean, here was this guy that exuded the excitement of science. And he was a physicist, and he was *ridiculously* brilliant. [Laughter] They can't make people like that—he was so good. And when he gave his lectures in physics, I thought he was extremely clear. I loved his style of teaching. I loved the way he approached problems and the way he wanted us to think. I said, "This is perfect. This is the right one for me." I used to go to his lectures and I'd listen and listen, and I'd say, "I understood every word." And the other guys would say, "God, that was impossible. I didn't understand anything." And I'd say, "No, no. It was easy." And they'd say, "What about such and such?" And I'd go, "Um, well, uh. . . ." [Laughter] And I'd realize that, yeah, maybe I understood it at the time, but twenty minutes later it was gone. [Laughter] But two years of Feynman lectures gave me a taste for physics and a predilection for styles to approaching problems and solving problems which I think has stuck with me.

It was also as a sophomore that I ended up meeting Gerry Neugebauer and Bob [Robert B.] Leighton, who were then teaching assistants for Feynman. Leighton was a full professor and Neugebauer was an assistant professor, and I guess I had met [Matthew] Sands, also, who was another full professor. And they were all out there teaching sections of sophomore physics. Everybody knew Feynman was very, very smart and a first-rate communicator. And when they realized he was going to give three lectures a week to undergraduates for two years, the whole faculty decided to go. [Laughter] It was very funny. As students, we didn't really appreciate the magnitude of such a thing; we took it all for granted when it was happening. But in hindsight— The entire physics department always showed up for all the lectures. He had the whole physics faculty being TAs. It's ridiculous.

MOY: Maybe it's in the introduction to the books—*The Feynman Lectures on Physics.* I think it might be Leighton who says that fewer and fewer students were coming and more and more faculty were coming, so that by the end of the term it was essentially all the department. [Laughter]

Your PhD is from Berkeley, in physics [1972]. And what work did you do for your PhD?

NELSON: Elementary particle physics.

MOY: And with whom?

NELSON: Burt [Burton J.] Moyer was my PhD adviser. I was in the Moyer–[A. Carl] Helmholz group in experimental particle physics at Lawrence Berkeley Laboratory.

MOY: And what was your impression of Berkeley, especially after coming from Caltech?

NELSON: Well, it's a very different place. [Laughter] I enjoyed Berkeley quite a bit.

MOY: Was it as stimulating an environment as Caltech?

NELSON: Yes, I think it was. It was different. But I found graduate school there very interesting and quite challenging.

MOY: And when did you start and finish there?

NELSON: I graduated from Caltech in '65 and immediately went to Berkeley for graduate school in physics. I finished in '72.

MOY: So you were there for a very interesting time.

NELSON: Yes, it was lots and lots of tear gas and marches and riots. I was involved in a lot of that. Yes, it was an interesting period. There was a lot of political turmoil, which was a distraction.

MOY: Was it a distraction?

NELSON: Well, you know, it was part of life's activities. Certainly I put time into those things.

MOY: How did your interests turn toward telescopes and astronomy? Was that something that started happening while you were in grad school or was it later?

NELSON: Of course, growing up I'd been interested in astronomy and math and physics. But as I said, when I came to Caltech I said, "I think I'll be a mathematician." I certainly hadn't decided I was going to build telescopes. Or even that I had an undying love for telescopes when I came here. But I met Neugebauer and Leighton—my TAs as a sophomore—and they invited me to work for them. It turned out that they were involved in infrared astronomy, and in particular, they were involved in building a 60-inch telescope to do an infrared sky survey on Mt. Wilson. And they were building a telescope super cheap. They were going to build everything themselves with a spun epoxy mirror, and they got me involved in practically every aspect of designing and building this thing. I remember the day they hired me. Leighton dragged me into the shop and walked me into one of the rooms and said, "OK, see all these bars of steel? I want them all welded together, and here's how I want them done." And he pulled out a welding torch and he said, "Here's how you weld." He gave me a five-minute lesson and said, "Here." [Laughter]

MOY: And you had never done that before?

NELSON: Oh, no. And Leighton was remarkably good at building things. He was just really gifted in the machine shop. He was a very creative guy. He always had clever, obscure ways of building things that worked very well. So I spent three years working for Neugebauer and Leighton, building the air bearing that they spun this epoxy mirror on. They took an aluminum shell and placed it on an air bearing, so that we could rotate it; it's basically a frictionless bearing. You pour epoxy into it, spin it at the right velocity so that the epoxy flows out and forms the appropriate parabola, and then if you have a slow enough setting, it takes this nice stable configuration and cures and hardens. And then you have a mirror surface; you just have to aluminize it. So I was involved in building all that stuff and machined lots of things in the machine shop and polished some mirrors and did all sorts of stuff with that. And it was just a real kick. It was all kinds of different work, and it was a lot of fun. I did that during the summers fulltime, and part time during the school years.

Then when the project approached the end, we ended up assembling it all on Mt. Wilson. We built a house for it, which was carpentry work—more fun stuff to do. You know, particularly when you're a kid, anything like that is great. And then I spent a summer observing up there, running the telescope all night; and that was a lot of fun. I found that whole experience fascinating and really learned a lot.

Now, I was going at it from physics, not astronomy. When I was an undergraduate, I only took one introductory quarter of astronomy at Caltech. That was it. So I wasn't planning on being an astronomer; I was planning on being a physicist. And I went to Berkeley in physics and got involved in particle physics, and I found that was very stimulating.

By the time I was through with particle physics, though, I decided that— The experimental part of physics involves big groups of people. Of course, big groups then are now puny groups; now it's gargantuan—300 collaborators and that kind of thing. When I was involved, you'd have twenty collaborators, and that was absurdly large. I found it interesting and educational, but after doing it for several years, I thought, while I was doing it, I was a graduate student and I was not sure that this was how I wanted to spend the rest of my life. I didn't like the big groups so much. And I decided that I would see what astronomy was like. So I went down to campus—my office was at LBL [Lawrence Berkeley Laboratory] at the time, because I was in particle physics—and decided to take an astronomy course. I poked around in the department and took an astronomy class and was introduced to Dave Cudaback, who's an astronomer there [d. 2006—ed.]. And I chatted with him, and he thought it was wonderful that somebody in physics was interested in astronomy. We spent a lot of time talking, and he suggested that I should get involved in some kind of project. Because I told him I was a graduate student in physics and wasn't entirely happy with the particular field of research I was in. So he suggested that I take some research topic in astronomy and try it out and see what I liked. And at his suggestion, I ended up getting involved in studying pulsars. This was about 1970, I guess. I ended up setting up an experiment to measure the pulsations from the Crab Pulsar and the spin-down parameters of the Crab Pulsar. I designed and built a bunch of electronics, and it was one of these wonderful things where all of my knowledge of digital electronics and everything from high-energy physics was just perfect. Easy stuff. And yet from the astronomers' view, it was fairly esoteric stuff; they just didn't do things that way. Also, I had the very nice situation that the world of particle physics was *very* wealthy. So I could go to my advisor and say, "I need to buy an atomic clock. It costs $20,000." And he'd say, "Fine." Whereas in the world of astronomy you'd say, "I need to make a long-distance call," and they'd say, "Well, are you sure you can't write a letter?" So it afforded me opportunities that people in astronomy just didn't have. That was a very quirky and very good thing for me.

So I built this equipment and did an experiment, and it turned out to be very successful, and it was a lot of fun. I ended up continuing to study pulsars and cataclysmic variables in astronomy—spinning compact astronomical objects—in parallel with finishing up my thesis. [This was] a crazy thing to do, because—well, you're busy writing up your thesis; you know, you hardly have time to do anything [else], or you know you shouldn't have time to do anything [else]. Finally, I got my degree and got involved with another very large project in high-energy physics and was still doing astronomy. I spent a couple more years involved in high-energy physics and then this big project. We spent a couple years writing a proposal, basically. Particle physics experiments were huge. This was something to do at SLAC [Stanford Linear Accelerator Center].

MOY: After getting your degree, you stayed on at LBL?

NELSON: Yes. They invited me to stay and I did. And they basically said, "Do what you want to do." So I did that. We were designing a so-called 4/radian—a ball, if you will—detector for an electron-positron colliding-beam project called Mini-Mag—with mini-magnet, superconducting, cylindrical magnet detectors all over the thing. But after a couple of years, when the proposal that a number of us had worked on was finally rejected, I said, "God, that was a lot of work and it really wasn't all that much fun." And it didn't get us anything; all they did was turn us down. And the astronomy I'd been doing was a lot of fun and very productive, so I said, "I think I'll just do astronomy." And all of my associates and bosses in the physics division at LBL said, "That's fine." They were perfectly happy to have diversity in the division. So they supported me, and I did astronomy at LBL. I used the telescopes at Lick Observatory; I used the telescopes at Kitt Peak. Wherever I needed to do it, I wrote applications and had some successes.

MOY: Were there other people who were essentially astronomers at LBL?

NELSON: Not really. Although, I would say since then, there has been a large group of people who have gotten involved in astrophysics there. Rich Muller runs a group of people who do all kinds of physics. But they certainly did astrophysics. And he and George Smoot worked for a number of years on the 3-degree microwave background measurements, which you surely have read all about recently. George Smoot claims that he now sees lumpiness in the microwave background, and all of that began at LBL. And the supernova search, which has been going on for a decade now, is at LBL.

LBL is a loose organization, so they have been generous in allowing groups of people to gather who may not be LBL employees; they might be Space Sciences Lab employees or campus graduate students, and so on. And [LBL provides] office space for them and some financial support. So they've been able to tackle esoteric and time-consuming projects—the supernova search has been just taking forever. And George Smoot has lots of laboratory space at LBL; he's formally at the Space Sciences Lab, but he's usually at LBL doing his work, because there's more space, more technical support there. They've been very generous in supporting at reasonable financial levels. You know, they're not about to take half of their high-energy physics budget and give it to these frill things, but I think there was a keen appreciation for the value of science and that diversity was healthy in their environment. And they could afford it, or they thought they could.

I don't know whether it paid off for them. It's a peculiar thing, because LBL's physics division gets its money from the Department of Energy. And they have a mandate to do particle physics. They used to get their money from the Atomic Energy Commission when I first went there. And it was clear they were supposed to be doing particle physics with it; they weren't supposed to be doing physics in general. And so, although the physicists in charge always thought astrophysics was great stuff, they were always walking a narrow line in justifying it. They were never sure whether it was good or bad to tell the people back in Washington the great stuff we were doing. If you bragged too much about it, people might think they weren't taking their particle physics seriously. [Laughter] So it was an interesting sociological thing. It depends on who's the contact and who's judging LBL's physics reports. Does he like astrophysics? If you think he does, great, you can peddle it. But if he thinks that's terrible and you should be doing something else, then you'd better kind of soft-pedal it. So it was all very interesting stuff. But they've supported it for years. I guess I was probably the first person to slip it [astronomy] in. And they said, "Oh, that worked out really well. Nelson's done interesting things." So when a couple of other people decided to play a similar game, they said, "Oh, good."

MOY: You had mentioned that you had originally thought about being a mathematician. To go from thinking about being a mathematician to welding rods [laughing], was that an easy transition for you to make? Because for some people it certainly isn't.

NELSON: I thought it was fun, actually. By the time I was a sophomore—I was halfway through my sophomore year—I decided to change from math to physics. And I think two things caused me to change. One was Feynman; I just loved it. And the other one was, I remember taking an abstract math course—modern algebra or something like that—and I thought it was utterly opaque and boring, and I said, "If this is what mathematicians do, I don't want to do that. I like doing integration and solving math problems, not doing this weird, esoteric stuff." [Laughter] Whereas physics—it seemed like everything I saw was fun. There were parts of math that I wasn't interested in. So I said, "Ah, I'll become a physicist."

MOY: Had you always enjoyed doing physical things?

NELSON: Yes. I did OK in physics in school, and I liked working on my Dad's car and doing all those kinds of things. So I was reasonably proficient with my hands.

MOY: You mentioned working on this project with Neugebauer and Leighton. Was that the first time you had done anything with telescopes? Had you used telescopes before?

NELSON: Oh, I must have done something. I think I got the astronomy merit badge.

MOY: [Laughter] As a Boy Scout?

NELSON: Yeah. I was an Eagle Scout. Yeah, that was another place where you learned different things. You've got to get all these merit badges in different subjects, so of course you did learn a smattering about a bunch of stuff. But I wasn't an avid amateur astronomer. I didn't have my own 4-inch telescope. I didn't go out and peer at the heavens at night. I didn't do those traditional things that lots of people do. For some reason, that didn't happen to me. Although I often enjoyed going out at night and learning the constellations and wondering at the magnitude of the heavens, of course.

MOY: You've essentially been at LBL since grad school?

NELSON: Yes.

MOY: Well, let's move on to talking about the telescope itself. Since the middle 1970s—it seems to me that's when people really seemed to start talking about making telescopes larger than Palomar. At that time, what seemed to be the main difficulties with trying to do that?

NELSON: Rather than answer that directly, let me just tell you how I got involved in the business. Because then I can explain that, and it might go a little bit easier. In 1977, I was busy minding my own business, doing astronomy, studying cataclysmic variables, having a good time. I knew a number of astronomers both in UC [University of California] and other places from my work. And John Gaustad, who happened to be the chairman of the Berkeley Astronomy Department, called me up one day and asked me if I would be willing to serve on a UC committee to look into the future of astronomy at the University of California. And I said, "Well, what's it about?" And he said, "Well, you know, the Lick 3-meter"—which is the main telescope that UC astronomers use—"is in a very bright place. Light pollution and so on; there are other big telescopes. So they're thinking about building another telescope at a darker site. Or looking at other things they might do to improve the future of UC astronomy." I said, "Oh, that'd be fun." And I said it in all ignorance. I had really paid no attention to telescopes, in the sense that engineering does. I'd used them; I used them all the time. But I just used them and I was happy with what they did for me. And although I periodically had problems, where I needed the biggest telescopes—I've used the Kitt Peak 4-meter and I've used the Cerro Tololo [Inter-American Observatory] 4-meter telescopes—but often small telescopes sufficed for my work. But not always. So I could appreciate that big telescopes were a good idea. Because I certainly used the biggest ones I could get my hands on sometimes, depending on the objects I was studying.

But I wasn't driven. The science I wanted to do wasn't being limited by the size of the telescope. So it wasn't a private compulsion that had developed in me to build a bigger telescope for my research—which you might have thought it would be, because often people are motivated because of that. But in my case, it didn't turn out to be so.

So I went to this committee meeting, in all innocence, not really knowing what it was about. I thought I'd keep my ears open and see what was going on. And we talked about the obvious problem that we all knew about—the Lick Observatory and what's the future. There was a group of UC Santa Cruz Lick Observatory people who were actively trying to get the plans through to build a 3-meter-class telescope on—what was the name of the mountaintop? In the Coast Range, south of Santa Cruz. [Junipero Serra Peak—ed.]

MOY: Hamilton? Is that it?

NELSON: No. That's where Lick is. Anyway, several astronomers there had been working a long time on that, as it turned out. I didn't know anything about it; I wasn't paying attention to what they were doing. But it was a very dark site, would promise to stay dark for a long time, and supposedly had great seeing, as the atmospheric turbulence was minimal.

A couple of other people on the committee thought that we ought to look into building big telescopes somehow—build a 5-meter, build a 6-meter—do something really flashy. And I said, "Oh, I'll sign up to look into big telescopes," because that just seemed like a good idea. I mean, just going to a darker site—we already had dark sites. If we really wanted to be number one, it struck me that just putting a 3-meter telescope on a dark site was not going to get us very far. It's hard to sell, too. And there's a sale thing here which I think matters, and we were all sensitive to it. If you just ask for something—it may be very useful to you—but in the climate of research, if you just ask for something that a bunch of other people already have, they say, "Why should I give you $30 or $40 million to do that?" It doesn't make us special. So it was clear that things like that, useful as they might be, didn't have any sex appeal, and they didn't appeal to me particularly, either. So I said I wanted to go think about how to build a big telescope.

When I went away from that meeting, I asked myself, "Gee, how do you build big telescopes? How do telescopes work?" I knew how they worked, but I hadn't really paid a lot of attention to the details—about what was hard and what wasn't hard. I knew the principles on how you support mirrors and, generally speaking, why you had to pay attention to that, but I hadn't really thought about it very hard, because it didn't matter to me—because the telescopes worked. We used them. It's like you don't necessarily study the details of how a car works in excruciating detail; you just drive it to work.

Well, now it *wasn't* that way. Now I said, "How do telescopes work?" So I got all the books and articles I could from the libraries and started reading. I got blueprints, and talked to people, and read and thought and calculated. And over the course of a month or two, I developed opinions about what made telescopes hard to build: where the money went, where the effort went, what things were perhaps not so hard, what things were really crucial, and how things scaled up, and when things got really tough. And I looked at the different designs, because every telescope's different.

This generation is no different. [Laughter] It's very funny, because now we've got all these big telescopes being built, and none of them are the same—it's really interesting.

But in the course of doing all that, I came to a couple of conclusions. One was that it looked to me very hard, and it looked like a dead end, to try to take the technology that was used to build the existing telescopes and scale it up. I said, "You can't go on forever with that. That's got to stop." So it didn't seem very interesting to me to pursue that.

MOY: And at this time, by big telescopes, you're thinking 6 meters or so?

NELSON: Yes, bigger than 5-meter. So it occurred to me to make the telescope out of segments, so that you didn't need to have a mirror getting bigger and bigger and bigger, where the problems get difficult rapidly with size. Deflections from gravity scale like the $4^{th}$ power of the diameter, so you go from 5 meters to 10 meters and the gravitational deflections go up by a factor of 16. It's terrible. So monoliths [as we call them]—single pieces of glass—they didn't look like a good avenue for the future to me. I said, "Segments are much better. Build a mosaic."

Then the other thing I thought about, in looking into large telescopes, was that it seemed to me that to build a telescope just a little bit larger—like the Soviets built a 6-meter telescope in the mid-seventies, which has never worked well, unfortunately—that didn't appeal to me. That seemed kind of chicken.

MOY: "Chicken" in what way?

NELSON: It didn't seem adventurous enough to me.

MOY: Because it was only 6 meters?

NELSON: Yes. It was so obviously done just to be a little bit bigger, but not because you were going to do anything new and different with it. So I said, "No, we should make it a lot bigger, so it's clearly going to do new science." Instead, it would be a quantum jump in astronomical power—that's what you want. "So, let's build a 1,000-meter one. Right! That would be great!" So you say, "Well, OK, that's good philosophy. But how big a step can you take and still afford it?" And doing all this sort of in parallel—thinking about segments and how you build segmented-mirror telescopes, and looking at other ideas people had for telescopes, and looking at where money went in observatories—I decided that 10 meters was a good size scientifically. You get a factor of 4 in area. So that's a big number. I thought it was affordable. I'd looked at radio telescopes and how the structures worked and how much they cost. And I looked at different kinds of optical telescopes. And the MMT [Multiple Mirror Telescope] was just being built then; that's six telescopes on a common mount. And I knew what their costs were, and I said, "Yeah, I think we can build a 10-meter. It's not stupid to think about that size. It'll be segmented. It'll be interesting enough that we can get money for it. And it'll be cheap." Famous last words.

So I went back to the committee and said—actually I wrote a report, walking through a lot of the logic for different things you might do, what the options were, and so on. And drew my conclusions that 10 meters was a good size and that a segmented-mirror telescope was an appropriate technology to look into. I presented that report to the committee at our next meeting; and it was met with a lot of skepticism. [Laughter]

Perhaps in all institutions, there are cliques and people who run things—people on the inside and people on the outside. And although I was an active astronomer, I was not at the time a professor of astronomy. I was a—what was I at the time?—I was a divisional fellow at LBL, I think. And so to the established astronomers, I didn't really have any serious credentials.

MOY: Were you the only person on this committee looking at the—

NELSON: Joe [E. Joseph] Wampler was interested in large telescopes—he was a professor of astronomy at UC Santa Cruz, and he wanted to build a 7-meter, it turned out. And people took him very seriously. He had an excellent reputation for building instrumentation—had done really good things at Lick. He had gone down to Australia and had been the director when they assembled the 3.9-meter telescope down there. And at Santa Cruz, which more or less runs the hardware end of astronomy at the University of California, he was their darling. And he wanted to build a 7-meter monolith. I didn't really know that until we all started getting together. Then I realized what he wanted to do, and realized that what I wanted to do was different from what he wanted to do. But they were courteous to me, and they formed a subcommittee to specifically look into it. "OK, you guys want to build a big telescope, let's look into it more seriously." So that was good. Joe and I were on the subcommittee, and Dave Rank was on the subcommittee [UC Santa Cruz] and Harland Epps was on the subcommittee [UCLA].

And so the four of us set ourselves the task of studying more deeply what the possibilities were for building large telescopes, as opposed to some of the other people who went off and looked at dark sites and collaborations with other organizations—rather superfluous things. And very rapidly we found ourselves divided, in that Wampler wanted to build a monolith and I wanted to build a segmented telescope. And Rank and Epps, fortunately or unfortunately, also decided a monolith was the way to go. So I found myself isolated, because I didn't have, at least on this subcommittee, any supporters. But I thought what I wanted to do was perfectly sensible. Not that I had all the answers or anything, but I thought it was the right approach. It felt good to me, that it was a good thing to do, and if you solve these problems, you can build telescopes as big as you please. And it seemed like a much more interesting technology. So I was not horribly daunted by their lack of support. And they were kind and courteous.

Then I went back and took advantage of the resources I have alluded to at LBL. One of the first things that happened was that after playing for a few months with this idea, I started getting Terry Mast involved, in little bits of his time. He and I have known each other forever. We were undergraduates at Caltech together, and so we knew each other then. We were both graduate students at Berkeley—both in high-energy physics—although our paths didn't cross much; but after we got our degrees, we ended up working on the same projects. So we were pretty good friends. Then I was doing astronomy, and he was off doing cosmic-ray physics. And then he got involved a little bit, thinking about some of the problems for a segmented-mirror telescope. Then I talked to people in the astronomy department—Dave Cudaback and [William] Jack Welch—who were very supportive. And Len Kuhi, who is another Berkeley astronomer, was very excited about all of it when he saw my ideas. I don't know exactly how this happened—I guess he was head of the department, then he was the dean of physical sciences. He ended up becoming the provost of Berkeley. Then he left; he became a vice president at the University of Minnesota. He had good political connections. So Len's role in this, I think, was really very important. He went to—now, who was it?

MOY: What year was this?

NELSON: '77. It was the chancellor—Albert H. Bowker. He went to the chancellor and to David Saxon, who was the UC president, and told them there was this great idea happening—that we were working on. And so they got excited about it—totally independent of Lick Observatory.

**Tape 1, Side 2**

MOY: So Saxon and others were getting interested in this already.

NELSON: Yes. They were hearing rumors about it, and I was hearing back from Len [Kuhi] and Dave Cudaback that the administration knew about what I was doing and they were interested, which was great. So somebody was encouraging me. Then I decided that to understand better the engineering problems with some of the ideas I was thinking about, I needed some money. I needed people. So I went to the director of LBL and asked him for money.

MOY: And who was that?

NELSON: Andy [Andrew M.] Sessler was the director of LBL at that time. And they have a slush fund called the Director's Fund, where once a year you can go and get seed money for projects. A very nice institution to have something like that, so you don't need to go through all the formality of big-project proposals. And I told them what I was working on. I figured I'd ask him for $50,000 or $100,000, and he basically said, "Sure. Here." [Laughter] So again, it was one of those things that in the astronomy community there really weren't resources like that readily available; it would have been completely impractical to have extracted that kind of money. With LBL, just because that was the club I was in, they said, "Fine, great idea. Here's the money." So I hired George Gabor, who's an engineer there. And I don't know—I think Terry ended up working with me. Rich Muller paid him or something, I don't know. I think Terry probably worked for me for a year before I ever actually paid him. He's a very independent sort. He really wanted to do it. He thought this was neat, so he started working on it. And Terry is really good.

MOY: What year was this—roughly—that you started?

NELSON: Probably the end of '77 I asked for money, about six months after I started this whole thing. I realized I was spending a lot of my time on it, and I had more things that I wanted to do than I could do all by myself. So I needed money.

MOY: Can I ask you to back up for just a minute? One thing historians, especially historians of science, often try to get a handle on is where ideas come from. Do you have any sense of where the idea of the segmented mirror came from? Did it come in one amazing vision? [Laughter]

NELSON: No, not really. It would be wonderful to say you're struck by lightning and something happened. But the idea of a segmented-mirror telescope—I didn't invent that. Radio telescopes are all made out of panels. If I did anything, it was just that I kept my eyes open and realized that telescopes weren't just optical telescopes. So I said, "Oh, obviously you can build a mirror out of pieces." And people have been building, you know, with tiles—people tessellate surfaces all the time—for millennia. So it's just common sense.

MOY: Were you thinking in terms of radio telescopes? Because people have made the comparison to radio telescopes.

NELSON: Oh, very much so, yes. The image I had in those naïve days was that one would build something that looked like a radio telescope, but the optical quality of the surface would be that of an optical telescope. Because radio telescopes were very inexpensive, compared to optical telescopes of the same size—vastly different, and you have to ask why. In fact, I read a number of papers on the design and construction of radio telescopes to understand the rules by which they were built, which in principle are, of course, the same rules any telescope is built by. But in fact they'd been approached, I thought, a lot more rationally than optical telescopes. Radio astronomers have played a lot of clever games with homologous deformations. In radio astronomy, you take a telescope that's supposed to be a parabola, and as it moves from zenith to horizon, gravitational forces will deform it. They design the stiffness such that as it deforms, it stays parabolic; it just changes focal length. And so that way you don't absolutely fight gravity; you tailor gravity as you design your structure to yield appropriately to gravity. Which is very clever. And I thought that was really nice. And that, in fact, was a well developed field in radio astronomy; articles had been written on it, and several telescopes had been designed and built that way. So I thought it was a nice scientific idea the radio astronomers were using, and I said, "We'll do things along those lines." Now, as it turned out, as I looked more closely into it—that particular idea of homologous deformations—I couldn't figure out how to apply that to an optical telescope. It turned out that I didn't want to deform the individual segments, and that screwed it up. It turns out that our problem was thousands of times harder than that of the radio telescope. So that didn't turn out; but nevertheless I was looking at radio telescopes for guidance for ideas, as well as optical telescopes. But it's silly to think that I invented segments.

The thing I did recognize, very early on, was that it was clear there were two problems that had to be confronted with the segmented telescope. One is, How do you control the positions? When I did the calculations, I realized that with segments, you put them on some kind of structure. It's going to deform by such a large amount that there's no way you can just position them carefully and they'll stay where they belong. Myology won't work. I realized we were going to have to have some sort of active-control system. We were going to have to sense where the mirrors were and move them around with pistons, which nobody had done. Nobody was doing that. I said, "Oh, this is a new problem." But I said, "Wait a minute. That's just electronics. I know all about electronics; I do that stuff for a living." So the thought that you had to do sensitive detection in small motions just struck me: Well, if that's what you have to do, that's what you do. That doesn't mean it's trivial, but you shouldn't be daunted by it. And the other problem we recognized early on was that the mirror segments themselves had to be polished and they were going to be off-axis pieces of a parabola.

Another thing that happened in this same period was that I had a few conversations with Luis Alvarez, who was a particle physicist, of course, up at LBL. I knew him moderately well. And I showed him some of the things I was doing for a segmented-mirror telescope. He was very enthusiastic and very supportive and said, "Keep up the good work." And he did everything he could to encourage me to go in there and think about it, and design it right, and not be daunted by the conservative, old, stodgy guys at Santa Cruz—or his perception of them as being that way. Partially true, but— So anyway, I dove into this thing, into the design details. And at this time Terry was getting pretty committed to it and George Gabor was involved. The three of us were working pretty much fulltime on various aspects of this project.

MOY: You mentioned that when you first started looking into this, you didn't know that Wampler and some of the others already had in mind building a larger monolith mirror.

NELSON: No.

MOY: Do you think that if you had known that, you might have been less likely to have—

NELSON: Yes. It's possible, because I think it was actually at our second meeting that we flew down to LA or something, and I sat with Len Kuhi, who was also on this committee. This is after I had written the report, and I was taking a pounding. And Len was telling me about Wampler's ideas to do a 7-meter. And then he did the most amazing thing. He pulled out a report that he had on the NGT—the Next Generation Telescope project. This was something that Kitt Peak National Observatory was working on, which I had never heard anything about at all. They wanted to build a 25-meter telescope. And he pulled out a report of some enormously complex design, interesting and innovative. And I realized there was a whole field out there, and there were people—not just me—thinking about this stuff, which was very nice to know. Because one of the things I started doing was, when I discovered that Kitt Peak National Observatory had a whole engineering group that was thinking about how to build very large telescopes, I started going out there. I was going out there once a month. I would spend three or four days talking to their engineers, picking their brains, arguing with them, telling them how they ought to do it, or how I was going to do it, to try to get them to criticize me. Because, of course, you absolutely need criticism for this stuff. Maybe theorists don't; but I'll tell you, if you want to build something, criticism is really good. And these guys at Kitt Peak—Larry Barr in particular, their chief engineer—had a very broad knowledge of telescopes and a fair amount of experience with building things. Big projects. Larry had opinions about what things cost and the like, and he was easy to talk to. So I spent a lot of time talking to him and several engineers who worked with him. And I used to go out and talk to their optical designers and their structural engineers, and it was generally very fruitful. I also talked to Mike Reed, who was the chief engineer of the MMT. I remember talking to him. I was interested in finding a way to solve our active-control problem: How do we sense where the mirrors are? He had an active-control system for the MMT [six mirrors], and they had to control the tip tilts. They did it with lasers that they bounced off the mirrors and ran through the optical train, and sadly the system that he designed was never successful. I don't really think it was his fault. At the end of the project, as it was all getting built, there was some kind of a falling out, and he quit and left before it was all working. And the guys who picked it up didn't really understand it. So they put it together, and they had a lot of problems with it. They finally scrapped it. They didn't use the adjustment system that he designed. That always disappointed me, because I looked at their criticisms of it and I said, "Those are stupid. Come on, you guys. You should have just—you could have finished it there." There wasn't anything wrong with the idea. But it wasn't my project, so I couldn't do anything about that. And I thought Mike Reed was an excellent source of ideas. So I talked to him several times.

George and I, of course, were constantly stirring the pot, and buying pieces of hardware, and testing them in the lab to see what kind of sensors might be stable—looking at actuators. We knew we had to sense where mirrors were to nanometer levels, and we had to move them at those levels. So we pursued both of those problems. And sometime maybe toward the middle to end of '78, Terry and I finally decided mathematically how we wanted to do the active-control system. And that was a wonderful time—that was really fun. I'd been playing with patterns for the mirrors—hexagons and squares—and thinking about edge sensing. There are lots of ways you can sense where mirrors are; close-proximity edge sensing is just one of a number of ways you might do it. But I'd been thinking about edge sensing and decided it had a lot of attractive features. And I was thinking about where you might put sensors, and how you might process the information. I was telling Terry about it, and I think it was Terry who realized that, really, it's an elementary mathematical problem to process all this information. You just do a big least-squares fit to all the data, if you will. And that was a linear problem. I intuitively knew it was simple, but I hadn't tackled the specifics yet. Then we wrote all the equations out and wrote a big computer program and started modeling the active-control system. It's sufficiently complex; just writing the equation down doesn't tell you everything you need to know—there are just too many variables. But we just started doing Monte Carlo calculations to study the behavior, and we discovered it was a very stable and robust control system, which was good news. Because particularly at that stage—this is an attitude that evolved over the course of time—none of the astronomers believed that the active-control system would work. They thought that doing things at the level of nanometers was not possible electronically, somehow—that was their instinct. It wasn't that they intellectually knew that we couldn't do it, but emotionally their response was, "No, that sounds impossible. It'll shake itself all off." It'll do something—they didn't know what—but it wouldn't work.

MOY: It's interesting. I haven't been that close to electronic matters for a while. And that was my reaction too, actually, [laughing] when I first heard about the plan. It was pretty well along by this point, so I knew it was going to work. But at first I didn't see how it could work. And I realized very quickly that it was a very intuitive thing. There was something that seemed as though the synchronization just wouldn't be possible. And I'm not sure why I felt that. In some ways, I'm curious that other people felt this way, too. But you didn't have that intuition?

NELSON: No. No.

MOY: I'm not sure why I did.

NELSON: We certainly have an awful lot of common experience with things that vibrate. You know, someone turns on a motor, and you can grab the desk and you can feel it grinding. And you know there's something bad there. And if you try to do something on the nanometer level— But no, I wasn't concerned about it particularly. But I knew it was an issue, one that had to be solved. We had to be sensitive to it. But I didn't at all have the perception that that was going to be a big problem. Smoothness was the issue that I was after. That is, if I want to make a 10-nanometer move, I need something that will move that; I need to be able to sense that. It's only if I'm only sensing 1,000 nanometers that I'm going to find something bouncing around and getting vibration. So my intuition was that if I could measure adequately and I had a motion device that could make small enough moves, then I'd be home free. That's proved to be, in fact, the simplistic but correct way to look at the problem. As I said, Terry and I finally got to working on the mathematical control law for this, and worked that out. And it proved to be terribly robust. It was just wonderful. We were really happy with that, wrote several papers on that, and were really quite happy with how that worked out.

Then George and Terry and I were looking into lots of actuators and sensors, where George was really doing most of the mechanical work. He was a real hardware-oriented guy. And after talking about it, looking into it, and doing lots of lab tests on a variety of things, we finally settled on a capacitor sensor and a roller screw as the basic motion mechanism for our actuators. And started doing tests. You know, you have tons of ideas—we just started evaluating them. We'd say, "No, that doesn't look good. That's too expensive. It's too fragile. It's too coarse. It's not available." Whatever. Narrowing down our options and using our good or bad sense to narrow them down further, until we finally ended up with— Well, we'd do tests. We didn't just do it all theoretically. We'd say, "This looks good," and we'd go out and buy a piece of hardware and evaluate it and say, "You know, it didn't actually work out the way it was supposed to. It has terrible thermal properties."

MOY: This was really the three of you. You, Terry Mast, and George Gabor, up at LBL.

NELSON: Yes. We finally ended up narrowing it down to an actuator design that we liked, and George built a prototype in the lab. [There was] a sensor that we liked, and George built a prototype of that. All these things were happening simultaneously, and it's terribly important that they were, because everything fed on everything else to filter out the bad ideas and to make the whole complex of ideas grow up as a nice whole.

MOY: A question comes to mind here. As you were doing this—because this is long before the committee had come to any kind of consensus—you were doing this obviously to demonstrate the ideas to yourselves, but also was it in your mind that you were in some way getting ready to present this to the committee as the way to go?

NELSON: Yes. At the same time we were doing this work, I was on this committee. In fact, through a quirk, I was the chairman.

MOY: This is the telescope subcommittee?

NELSON: Yes. And the telescope subcommittee was meeting regularly, and Wampler and company were puttering around with monolith ideas. And curiously it happened there, actually, on the main committee—on this Future of Astronomy Committee—

MOY: Also called the Graybeards Committee, isn't that right?

NELSON: No. That's another committee. We'll get to that.

When my ideas gelled on doing a 10-meter, Wampler found himself in the very awkward position of appearing short-sighted. He had walked in there, unbeknownst to me, early on—here was their fair-haired boy wanting to do a 7-meter—I didn't even know about it.

MOY: How old is he?

NELSON: He's ten years older than I am. And then I came in, started a lot of ideas, and wrote things down, actually; did the calculations and then thought about it and said, "We can do a 10-meter." And it wasn't all hot air; I'd actually thought about it. And he found himself in the position where I think he felt like he couldn't defend a 7-meter. So he said, "OK, we'll build a 10-meter monolith." Because that, as the saying goes, leveled the playing field. Then what happened was, we effectively set up a competition. We said, "All right. You guys look into how to build a 10-meter monolith, and I'll look into how to build a 10-meter segmented-mirror telescope." And really, you get put at a disadvantage by that, because, of course, a 10-meter monolith is much harder than a 7-meter monolith. Whereas, for a segmented-mirror telescope, the whole beauty of the concept is that size is not a terribly important parameter. They cost more money, but the technology doesn't particularly change with size, and so it was all the same to me. But I was adamant about 10 meters. I thought it was stupid to do 7 meters because 10 was so easy and we could afford it. And Wampler bought into that, or felt that he had to. And he was reluctant. Of course the astronomers, on the one hand, were quite skeptical about what I was doing at the time. But on the other hand, they were totally entranced with it. Who wouldn't want a 10-meter telescope? Here some joker comes along and says, "I'll build you a telescope ten times bigger than the one you're using." And you may laugh, but what if he's right? My God, it'd be great! So they really wanted it to be true, and everybody was quite happy that Joe would say, "Yes, I think I can do a 10-meter monolith." If we'd said 20 meters, they'd have bought that. So he found himself tackling a hard technological problem—not insoluble, but a hard one.

So we had two groups, if you will. One group was really me on this committee and my colleagues at LBL—with LBL money behind it, too—to push innovative ideas. And then we had Wampler and company, with some money from Lick but not so much originally. And Joe's style was very different from mine. He was very much of the style of "Leave it to me. I'll take care of it. It'll be OK, trust me." He liked to do stuff his own way; he didn't like to have to prove it to anybody. He's not a physicist, he's an astronomer; he didn't want to have to do the detail stuff. Well, it turned out that when you're pushing technology that far, you can't be sloppy. And his whole approach to convince them—when people said, "Let's work out these two designs"—his approach to working up a design was very sloppy and casual. And easy to criticize because of that. And we knew we couldn't be sloppy or casual, because we knew we were doing new stuff. There was no way we could say, "Well so-and-so did it twenty years ago, so it'll probably be OK." We said, "We've got to build hardware. Words won't cut it."

And we designed hardware, we built hardware, we built computer models, and we wrote, if I may say so, wonderful reports. We had rigorous, logical, analytical reports on what we were doing—reports that were coherent and packed with ideas and mathematics and equations. If you read them, you said, "Yes, this is right. These guys have thought it through, and this is OK." An engineering thing, you know, is a piece of hardware, and you've got to build hardware for that. But our mathematical ideas were sound and thought out. I spent ages writing papers on sensitivity analysis and differentiating this and that, checking every conceivable systematic whatever and writing it up. Both Terry and I spent a lot of time doing that, because we wanted to make sure that there was no flaw, because there wasn't anybody who paved that road before us. We couldn't just say, "Oh, other people have made it work, so it'll be OK."

The result of that was, we generated paper. And our paper had photographs of equipment that we built, and graphs of test results of measurements we made. You know, hard stuff—science. So we'd go to meetings, and we'd go *plunk* [motioning tossing report onto table] and pass out reports half an inch thick. And Wampler would pass out a little three-page memo. And I think people saw, "Wow, these guys have very different styles." And they realized the problems were somewhat different. Our two groups were fairly hostile.

MOY: That was my next question. I was wondering how the competition went. Was it friendly, or was it kind of—

NELSON: Sometimes it was downright hostile. There were a couple instances where tempers flared very seriously.

MOY: In meetings, or privately, or both?

NELSON: Well, we'd control ourselves a little bit in meetings—but a little bit. I think everybody knew there was a lot of tension there. I remember there was one thing: We had a public meeting in Berkeley—the UC astronomy meeting or something like that—and I gave a talk on the progress for the segmented-mirror telescope and showed some results on the sensors and actuators and what we were doing. And I was very encouraged. Of course, I knew the meeting was coming, and we pushed particularly hard to have a set of coherent results for it, because we knew we had a community to convince and we weren't naïve enough to think that they didn't matter. And I remember after I gave my talk, Wampler accosted me and accused me of being a liar and a cheat. [Laughter] He was just absolutely furious; he thought I had said something that was absolutely not true. I never figured out what it was, because I was only quoting the data that I collected. But there was something in there—I never knew what it was—that he thought was lying to people, and he was just absolutely furious. Of course, I got pretty mad at him, too, for having said that thing. I took offense at it. So our relationship was pretty strained.

MOY: And you hadn't really known each other that well before this, had you?

NELSON: We had worked together on pulsars a little bit, so I knew him. And we'd collaborated on a couple of papers. But we didn't have a long tradition, other than several years before, when we had—back in 1970 or something—worked on a couple of papers together. But after that, we hadn't worked together.

And then Harland Epps, a professor at UCLA, who is . . . oh, a terribly irascible person, just a horrible guy to be with—has absolutely iron-clad opinions about almost everything. He always knows the truth.

MOY: And he was older.

NELSON: Yes. He's ten years older than I am, maybe more. A very good optical designer, but incredibly opinionated, and he'll talk for hours and hours and hours. He just drove me crazy. And he didn't know anything about physics at all. He couldn't tell you how to take a ball and [get it to] fall off a table. But he thought he knew everything. And he was on the committee, so I just wasted thousands of hours listening to him on the telephone and arguing with him at meetings.

And he had ideas. And I sort of put up with him and with Joe, too, partly because we were on the same subcommittee together and we had an obligation. And partly because I really wanted their criticism. I mean, even if a lot of it's half-baked, that doesn't give you an excuse to stop listening. Because they wanted to find things wrong with my design, and that was good. I was paying attention. I wasn't proud at all. "Boy, if you can find something wrong, the sooner you tell me, the better."

But I, of course, would defend my ideas endlessly. Because if someone said, "It won't work," my thought was, "Well, I'll convince you that it will. And if I can't convince you, we've got a problem." Not an emotional, human problem but a technical problem. "Somehow I haven't thought through my ideas enough, because you didn't agree with me—or you haven't explained the fatal flaw enough to me." So I was willing to talk for a long time to people if they didn't think it would work. Unless they were total idiots. But scientists aren't total idiots. Even if I sometimes think they are, I know they're not.

So I really always wanted to get criticism, hoping that I'd find a flaw so that I could fix it. The sooner you fix it, the better. You hope it's not fatal; you don't want your ideas to go down in flames.

But I never really worried about that—I don't know why. Somehow, as this whole thing evolved over a relatively short period of time—six months or a year—I had the feeling that this was a great idea; this was the way you're supposed to do telescopes, there was no way this could fail. That's just the way I thought about it. Which is nice, because it took a long time to get here. [Laughter]

MOY: So was Wampler already—before, for example, he had shifted from the 7- to the 10-meter at your suggestion—was he already thinking in terms of the thin meniscus-type mirror at that time?

NELSON: Yes. Well, we all knew that you didn't just make supermassive blocks. And he was open about various ideas, but he ended up thinking that for a 10-meter, a thin meniscus was the way to do it.

MOY: Had he been thinking about that already? Do you know?

NELSON: Probably he wanted to do that for the 7-meter. I'm not sure, but I would guess that's what he had in mind.

MOY: And was Epps also thinking about that? Do you know?

NELSON: I don't know about Epps's thinking—Epps really wasn't thinking about telescopes until he got involved in this committee. Then, of course, he thought he was the instant expert on everything.

MOY: I picked up somewhere that he's a pretty significant figure at UCLA.

NELSON: He's not there anymore. He's in Santa Cruz now.

MOY: OK. But at the time—and still—my impression is that the UCLA people haven't been active.

NELSON: That's correct.

MOY: Do you think it's partly because of Epps?

NELSON: Oh, yes, he had a lot to do with it. The whole psychology, or the whole sociology, of support for this project within UC has been really interesting. UCLA was adamantly opposed to it. Anyway, we'll get back to this in just a second, because it is interesting.

We had these two groups working on designs—generating reports to our parent committee. And we talked to Saxon, got some more money from Saxon, too. So now Wampler had money, too, to fund some studies.

MOY: Who was chairing the committee itself?

NELSON: I think Bob [Robert P.] Kraft [UC Santa Cruz] was. It might have been—I'm trying to think of who the director of Lick was before Bob Kraft. Anyway, it became clear to this Future of Astronomy committee that we weren't likely to agree on how to build a big telescope. They knew where I was going, they knew where Wampler was going, and they saw that neither one of us was going to say, "Yes, he's right, I'm wrong." So in their wisdom, they said, "Well, look, here's what we'll do. We'll set up a formal"—"contest" isn't the way it was phrased—but they said, "We'll set up a graybeard committee of people who aren't intimately involved in this process but who are wise folks about technology and astronomy. And we'll ask them to evaluate these designs, when the designs are matured sufficiently." And we said, "Fine." And then in 1980—probably the end of '79, when this committee was going to meet—there was a group of six or seven people on this committee, and we presented reports to them, both written and oral.

MOY: Was this an all–UC committee?

NELSON: Yes. Charlie [Charles H.] Townes was on it and Margaret Burbidge from UC San Diego, Holland Ford from UCLA, Bob Kraft, and Don Osterbrock was the other person I'm thinking of. And who else? There must have been someone else from Santa Cruz on there. I don't remember.

We presented our work to this committee, and after they reviewed it and talked to us at length, they ended up saying the segmented-mirror telescope was the way to go. So that decided that particular issue.

MOY: Was the competition still pretty intense?

NELSON: Well, up until that decision, yes. Absolutely.

MOY: Was there heavy lobbying going on?

NELSON: Oh, you bet! Your talking about UCLA reminded me of this particular thing that happened. Holland Ford was a professor of astronomy at UCLA, and he had not been involved with this stuff at all. But he really had his feet on the ground. Very quiet, mild guy, thoughtful person. He was on this committee and he knew Epps, because Epps was his colleague right down the hall. And he ended up voting for segments. And UCLA disowned him. Harland Epps was so incensed at it that he just berated him endlessly. And Roger Ulrich, who was the chairman of the department at the time, somehow believed Harland, so Roger Ulrich got on his case, too. And about a year later, Holland Ford left UCLA. He went to the Space Telescope Science Institute. He said he just couldn't take the atmosphere. It was just too hostile.

And yet Harland and I talk to each other. We always have. But he's always very skeptical. And Roger Ulrich and I really don't talk to each other if we can help it. He's had nothing but negative things to say and nothing to contribute. He never even had any criticisms. He's just a dour guy, in my view.

We never got much support out of UCLA. It's not true now. Eric Becklin is there. He was very supportive. But he's only been there for the last couple of years. It was the traditional UCLA group that was definitely not interested in this project.

There's another interesting thing for a history, which is perhaps worth noting. When I was in high school, I went to something called the Summer Science Program.

MOY: Where is that?

NELSON: That's in Ojai, California. It's a special program where they pick the twenty most gifted kids or whatever.

MOY: Yes. I think some kids from my high school went.

NELSON: They still have it. You go to Ojai and live there for the summer, and you do astronomy.

MOY: And it's not just California? Is it nationwide?

NELSON: Yes. It's anywhere, but mainly it's California. But it's a nationwide thing. And I went there. And when I was there, one of the two professors who ran the place was George Abell, who was a professor of astronomy at UCLA. A wonderful guy and, of course, a very eminent, genial astronomer. He died a few years ago [1983]. But he was alive at UCLA through all of this stuff. And so that was kind of an interesting counterpoint to all this, because George knew me as a high school kid. [Laughter] And so here he sees this kid that he taught astronomy and physics to one summer, come back a decade or two later and do this project. So George was, of course, not at all able to be against me. But he never claimed to have any technical knowledge whatsoever, so he never inserted himself into the fray. But I know he was always personally sympathetic; he was that kind of guy. I always thought that was interesting and kind of nice to know that George, at least, could think this was all interesting stuff and not have to dislike me because of what I was doing. [Laughter]

But some of the people at UCLA did. It was very odd. And it may have been done by Harland, who is a very vociferous person—a very emotional person. People got sucked in by it. It must happen in all big projects; when money starts flying around, things get important somehow. Potentially, there's money flowing around, and people's careers get involved.

MOY: That and the fact we're talking about a major jump. I mean, whoever built this telescope—however—it was going to be major.

NELSON: That's right. And after the monolith was rejected, Wampler withdrew and within a year or two, he left. He went to ESO [European Southern Observatory]. I think he first went there on sabbatical, then he came back for a little bit, then he went permanently. It was very much a case where there was a contest and he was defeated. It isn't that we meant it to be that way, exactly. It just evolved to be literally a confrontation, where somebody won and somebody lost. It wasn't that we tried to make it that way, it just turned out that we didn't know how to avoid it.

And losing is bad. So I always felt really bad for Joe, because he had ideas, and he had talents, and he tried hard. You'd like to create a situation where everybody wins, not where someone has to lose in an emotional sense. But we weren't able to do that. And Joe lost, and as a result I think it took the heart out of him for staying at UC, and he left.

MOY: But the idea lives on, right? I mean his idea of the thin mirror.

NELSON: That's right. And ESO is now going to build four 8-meter telescopes that are thin monoliths. He's not very directly involved in it. I mean, if he were directly involved, I'd say, "Oh, yeah, I know what's happening over there." But he's not, in fact. He's been very peripherally involved in telescope work over there.

MOY: Where are they based, by the way?

NELSON: Oh, they're in Garching, near Munich—in Germany.

**Tape 2, Side 1**

MOY: Other than these two ideas, the segmented mirror and the thin mirror, were there other designs discussed?

NELSON: We talked, as a group, about a scale-up of the MMT, the Multiple Mirror Telescope, and rejected it. The field of view was small, and we decided that that was a poor design because of that. It was a limited optical configuration, not as versatile as a traditional telescope configuration. But we looked into it fairly seriously, I'd say, before we rejected it.

Kitt Peak, which I mentioned before, had a whole group of people working on this so-called NGT project to build a 25-meter telescope. And they made wonderful progress. They were very inventive. One of their designs was a shoe—a so-called shoe—it was a segmented strip of a spherical mirror that was 10 meters wide and 40 meters long, or something. Big! It curved like this [motioning in an arc] and the whole assembly could rotate about a horizontal axis. And then up at the center of curvature of this thing were some ancillary optics that pointed to an appropriate subsection of this gigantic strip. So it's a spherical mirror that doesn't point anywhere. It's an Arecibo-like telescope; the Arecibo is a spherical bowl in Puerto Rico for radio astronomy, and this was an optical version of that. So that was one of their ideas, and they wrote a big report on it. Interesting, full of all kinds of ideas—technical details for bearings and all sorts of stuff. I always read their reports; I memorized them. I mean, I was into this stuff, so anybody who had anything to say about telescopes, I read it, I understood every sentence. And if there was something wrong, I'd call the guys up and talk to them until they said, "Yeah, we made a mistake" or they convinced me they were right. I was relentless. And, as I said, I'd go to Tucson all the time and talk to these guys, too. Because they were generating ideas, which was wonderful. So I thought that was really fun. And they had good stuff in their reports. They had good technical appendices, where they talked about engineering aspects of telescopes, which I hadn't seen written down other places. They'd write down the mathematics and engineering details, which I thought was very nice. I thought it was really a good source of information. Even though some of their ideas, I thought, were a little bit bad.

They had another design, the steerable dish, which was literally the idea I originally had: to build a radio telescope. And it looked like a radio telescope. It had this big dish, and it was 25 meters in diameter, and it had about 2 million segments on it. It was great. You know, as you looked into the details, there were things where I said, "Well, OK, I'm not going to do it that way," but I thought generally speaking it was OK.

They also had an MMT design. They were going to build six 10-meter telescopes on a common mount. And each 10-meter telescope had, I think, a 7-meter central mirror with a single ring of petals around it. There was a perception that big mirrors were somehow easier to make, and the segments were really tricky, so you wanted to minimize the number of segments.

And then they had another design, which was an array of telescopes. The array concept was bifurcated. They had one that was, I think, an array of a hundred 6-meter telescopes, or something like that. And they had this picture of a flat mountaintop; it was truly a mushroom farm. [Laughter]

MOY: So at this point, it really was like a Very Large Array of optical telescopes.

NELSON: Right. And then they looked at having six 10-meter telescopes—which is peculiar. I was working on the 10-meter telescope at the time, not a 25-meter. But it was nice that these guys were pushing the thing further and reminding you of where the limits are. So, as I said, I thought it was always stimulating talking to them. And I always liked their 6-times-10 meters. Maybe for obvious reasons. [Laughter]

MOY: And in going to Arizona so often, did you come in contact much with the [J.] Roger [P.] Angel [University of Arizona] kind of idea? Spin-casting?

NELSON: Well, it was while I was doing that, that Roger got involved in big telescopes. You know, you can just smell telescopes in the air. I was out there for some meeting, and we were talking about telescopes, and Roger had gone off and built some furnace in the basement in the laboratory and melted some Pyrex and said, "Hey, this is really easy to do." It's really easy to melt Pyrex glass over forms and make any shape you want to. It's dirt cheap, and the temperatures are low. The furnace materials are very inexpensive. He got really enthused about it. You know, it was trivial; he just kind of built something and plugged it into the wall, and the glass melted, and he says, "Hey, this is neat!" And I said, "Yeah, I can relate to that." You just putter around and something good happens. That was—I don't know—1979. Then Angel started getting involved in Pyrex mirrors and thinking about how they worked.

There was another thing that was central to our success which happened back in '79. And that had to do with the polishing of mirrors—stressed-mirror polishing. Early on, not knowing much about how you fabricate optics, I just sort of thought you take these off-axis mirrors and you get them polished somehow. And as I looked into that, I realized that, "No, wait a minute, polishing is not like machining metal. It's an art, not a science or even a craft." [Laughter] And I'd talked to people who'd polished optics, and they said, "Oh, no. We don't know how to do that. That's hard." Or they'd say they knew how to do it, and I'd listen to them explain to me how they'd do it, and *I'd* say, "Oh, yeah. It's hard." And I realized that this was a real issue, this wasn't a minor matter that we could solve someday in the future. It was a fundamental stumbling block, just as important as the active-control system. It would be idiotic to say, "Well, someday we'll develop an active-control system. Trust me, it will be OK." Obviously that wouldn't work—you're not going to convince anybody. And I realized that the optical polishing was the same kind of problem. You couldn't just say, "We'll work it out next year." You'd better have an answer to that one.

I started thinking about this problem and went through all the mathematical calculations of what shape I wanted, and how different it was from spheres, the things that are easy to make. I just started chewing on the problem. One of the ways I tend to work on things is, if I have a problem, I don't necessarily pose the problem and solve it. Unless it's easy. With hard problems, I'll pose the problem to myself and then just start chewing on it. And sleep on it, and dream about it, and talk to people about it, and ask dumb questions, and build little models. You know—waste my time on it. And sometimes, out of all that waste of time, ideas start percolating out, until I say, "Oh, yeah, I know how to solve that problem." And I'm always synthesizing. Somebody says something, and I'll say, "Yeah, that's a good idea. I'll toss that into the pot." So I'm always working on synthesizing up something for these sticky problems when I really don't know how to solve them.

So this was one of those, where I didn't know what to do. And I just started learning about how you polish mirrors, and what you can do, and I read about various tricks and things that people had done. And I discovered that [Bernhard] Schmidt had made Schmidt plates with this clever thing of using a vacuum. The Schmidt camera has a lens in front which has an aspheric surface on it. And Schmidt discovered that if you took a plate and pulled a vacuum on it to deform it, and then polished a sphere into the deformed plate, and then let go of it, when it popped back up, he got a little bit of the particular axis-symmetric sphere that he wanted—just because of the way the glass deforms elastically when under a vacuum.

It turns out that an amateur astronomer invented this thing called the Yolo telescope. I don't remember the guy's name, but he was actually a retired mechanical engineer from UC Davis, who was in Sacramento [Arthur S. Leonard—ed.]. The Yolo telescope was an off-axis telescope, so you didn't have any central obstruction. The hard thing about an off-axis telescope is that the optics aren't symmetric anymore. But what this guy did was, he built a secondary mirror, which he deformed. You just take the mirror and you basically push on two points on the front and push on two points on the back—like this [motioning]—to make the thing kind of potato-chip-shaped. And that's astigmatism, and that's the aberration correction you want from this particular telescope.

MOY: He had taken a regular parabolic mirror and just pushed on it?

NELSON: Probably a spherical mirror and pushed on it. So he was deforming mirrors. In fact, people have been deforming mirrors for a long time, too. In various places you see people play that game in a variety of simple ways. And this group at Kitt Peak looked into deforming mirrors, and they did something—they had a very elaborate scheme. They were grappling with the same problem I was: How on earth do you make off-axis mirrors? How do you polish them? And they said, "Well, we'll take a mirror and we'll put two hundred actuators behind the mirror. We'll just push and pull all over the back of the mirror to change its shape."

MOY: So, very active, essentially.

NELSON: Yes. "And then we'll polish it." And it just looked— I didn't like it. But the whole idea of "Maybe we can deform the mirror and polish it" seemed nice to me. I went and talked to Luis Alvarez, and he said, "Oh, yes, that should be real easy to do." Luis knows everything. He really did; he was amazing. He said, "Oh, yes, you have the equations of elasticity that will tell you how to do all that stuff." I didn't know anything about elasticity—not a lot. And he said, "What you have to do is go talk to so-and-so," and he gave me the name of a guy in the structural engineering department. And I went down and talked to him about it, and he said, "Oh, I know who you need to talk to. You should go talk to our resident genius, Jacob Lubliner." So I went and talked to Coby Lubliner. And, indeed, he's their resident genius. [Laughter] He was an engineer, educated at Caltech, and very smart. And he was a theoretician. I mean, he did theory of elasticity for a living. I don't think Coby knew a nut from a bolt.

MOY: But he was in engineering.

NELSON: He was in the College of Engineering, but he was all theory, nothing pragmatic. We talked about this problem, and he just loved it. He said, "This is great," and he started working on the problem. We started meeting very frequently as we were making progress. He was just whipping through all kinds of mathematics, arguing about this and that, and what worked and what didn't work. And almost entirely due to his efforts, we came up with a very specific procedure, taking advantage of the laws of elasticity for deforming mirrors and polishing them, which we call stressed-mirror polishing. I always thought it was very elegant and really liked it. All the math you could do on the back of an envelope—a big envelope, maybe. [Laughter]

MOY: In addition to being boggled by the idea of the active support system, I was boggled by the idea of being able to calculate the deformities.

NELSON: You were probably a physics major as an undergraduate.

MOY: Well—almost.

NELSON: Do you remember Laplace's equation?

MOY: Yes.

NELSON: It turns out that the equations that govern elastic deformations, instead of being *delta* squared of the deflection equals something, it's *delta* to the $4^{th}$. So it's just the Laplacian. So it's just $4^{th}$-degree differential equation. So you just write out the appropriate coordinate system, and you look at it and you say, "Oh, it's just a boundary value problem. What are the boundary conditions? And how do I control those?" And it turned out that when you look at the mathematics of what we want to make, the difference between the sphere—the thing that's easy to do—and the off-axis pieces of the parabola is all stuff that's lower than $4^{th}$-order. It's all quadratics and cubics. And so you realize that you're basically solving a homogenous $4^{th}$ order equation. And it's just the boundary conditions that dictate that. So I realized that, "Oh, we don't do what Kitt Peak was thinking about, where we push all over the back." We just wanted to find the edge of the circular disk—just find the low conditions on the edge, and we can get what we want. So the math gets real simple. You just write down the differential equation and you say, "Oh, OK, here's the constraints I want," and *boom, boom, boom,* [motioning—a list of calculations], and all of a sudden you realize, "OK, I'll put in shear forces around the edge that vary sinusoidally and bending moments around the edge that vary sinusoidally, and I can make anything I want to." It's just really nice.

So, Coby worked it out and even went one better and worked it out for thick plates. The elasticity really gets very complex. The *del* [to the] $4^{th}$ of the deflection equals the force—his thin-plate theory—is very powerful, but our plates were maybe not thin, and so Coby worked it out for thick plates, where basically you get the same answer. It's a little hairier, and there's some small correction terms, and he worked it out so we had a much greater generality—more than we needed, actually.

Having done that, I said, "We know how to do this problem." But I also realized we *should* do it. So we got more money from LBL and built a stressing jig and polished a mirror. We actually polished an off-axis parabola.

MOY: And this is still in '79, roughly?

NELSON: Yes. We did this before the Graybeards [Committee]. We polished an off-axis parabola, and it was successful technically, and it was also very successful psychologically—because we told people what we were doing. We never kept anything secret, of course. But, again, people were incredulous— "That will never work. That's crazy."

And we developed nice techniques for analyzing the data for the optical test data. So we could take a mirror that looked like junk during the optical test and say, "This is an excellent parabola, but it happens to have its axis over here." It's not so much that what we did was remarkable, in that sense; it's just that optical testing technology, at the time, was so oriented toward intuitive results that to have somebody take an interferogram that looked crazy and analyze it mathematically and say, "Oh no, it's actually a beautiful mirror if you had only tested it this way"—people just didn't do that. The opticians typically would look at the interferogram and say, "I'd better go rub over there."

So anyway, we made a good mirror, and even partway through, before it looked good to anybody, we said, "This is a very good mirror, and we made it even better"—wrote it all up and published a paper on it.[1] And again people were, I think, really impressed that we actually said, "This is what we're going to do. Here's all the mathematics for it." And we did it.

It was amazing it worked the first time. It really is. We were lucky. My experience is that even when you know what you're doing, even when you've done your fair share of homework on it, it doesn't work the first time. You discover little things you overlooked that bite you. And we just lucked out on that one. It went very smoothly.

[1] Jacob Lubliner & Jerry E. Nelson, "Stressed mirror polishing. 1: A technique for producing nonaxisymmetric mirrors," *Applied Optics*, 19:14, 2332-40 (1980).

There was another thing that happened in ’79, which I think was really important, about the monolith. They were busy criticizing us, and we were busy criticizing them, of course. And after some time they said, “We know how to make a blank. Corning Glass Works can make a blank for us that’s ten meters in diameter.” Wampler was talking to Corning, and Corning was thinking about it. Their engineers were pursuing the subject of how to make a meniscus. And they finally came back and gave Wampler a report and said, “Yes, we can do it,” and they told him how. And he told me how they were going to do it, and particularly how they were going to slump it, because it’s a meniscus—it’s got to be curved. They were going to slump it by putting it on a little pedestal over a mold, then heating it up and let it sag down. And when he told me how they were going to support it. I went off and did a calculation, and I concluded that the mirror would break. Which would be disastrous! It’s just a blank—it’s not polished yet—but still, it’s an enormously expensive piece of glass. And I decided it was going to break.

So I went back to Wampler and said, “How do you know it’s not going to break? It looks dangerous to me.” And he said, “It’s OK.” And I said, “Well, how do you know it’s OK?” And he said, “Because Corning calculated it.” And I said, “Well, can I have a copy of their report?” I nagged him for about two months, and I finally got a copy of the engineer’s report from Corning who had gone through all the stress calculations. And, of course, Wampler was just laughing at me. Because he had this expert who had gone through all this and I was skeptical. And I was really worried. *I* calculated it and I said, “This is really dangerous. How could this guy say it was OK?”

I got his report. I went over it, and there’s pages of stuff. I went through it, and it turned out his stress calculations were exactly the same as mine, but he got a different answer than I did. So I said, “What’s going on here?” The guy had made the dumbest mistake: He had a formula that involved logarithms, and he thought they were log base 10. When he typed them in on his calculator, instead of natural logs—and of course it’s just, you know, you’re doing physics, and so natural logs come out, right? Not base 10. And so he just punched the wrong buttons on his calculator and got entirely the wrong stresses, so he concluded something was safe when it was broken. So we had a big meeting, and I said, “Hey, guys, I read this report and there’s a mistake in it. It’ll break.” And everyone was kind of *“Uuhhhhh”* [motioning, mouth hanging open]. [Laughter]

And even at that, none of them knew how to do these calculation, so none of them quite believed me. Wampler promptly called up Corning, and they said, “Oops.”

But here was another confrontation. You know, people make mistakes, but here’s a place where these guys go *blam* [motioning tossing a report on a table] and Wampler says, “Hey, I’m right. This is OK,” and I said, “You’re wrong.” And it turned out he *was* wrong. So they said, “Oh, Nelson’s not a total idiot.” So I got some Brownie points for that one. Which was just a little fluky thing—important for the monolith, but presumably somebody would have re-checked the calculations. But it looked bad for him. He had been so cocky to think it was OK.

Anyway, by 1980 we had decided on the segmented-mirror design. Then we got a substantial amount of money from UC to pursue the design. And we proposed building a technical demonstration—a full-size segment, a proper support for it, actuators, a piece of the neighboring segment, sensors, and we were going to actively control two mirrors.

MOY: And up until that point, all told, the money had been coming from LBL and a little bit from UC via Saxon?

NELSON: Right.

MOY: And not totally more than a couple hundred thousand?

NELSON: Maybe $300,000—$300,000 or $400,000, but no more than that, I don’t think.

MOY: Then it was the Graybeards decision that essentially gave the go ahead?

NELSON: Yes. And then we got several million.

MOY: One thing I’m wondering about: Was the Future of Astronomy Committee determined to decide on a telescope design, or start fleshing out telescope designs?

NELSON: They got sufficiently excited about what we were doing that they said, “Yes, let’s go for a big telescope. Let’s not go for a 3-meter on some dark mountaintop.” And also, the mountaintop—I still can't remember what it was—they had all kinds of environmental problems. And I think all of us were of the opinion it would probably be easier to raise money for a 10-meter telescope than for a 3-meter telescope. Even though it was obviously going to cost a lot more.

And the other thing we had going for us was that Saxon, who was a physicist, said, "Hey, this is a new idea. I like it." He liked the thought of a big telescope and the new technology, more than the astronomy that would come out of it. And he was UC's president. If you tickle the imagination of the leadership, you're in business. So it was, again, a very fortunate circumstance. David Gardner, who came in in 1983—he had a PhD in education or something. None of this development could have happened, I think, if he'd been president. It just wouldn't have gotten off the ground. As it was, it was a very curious period when Saxon retired and Gardner came in.

Well, we did this so-called technical demonstration, where we built a full-scale model, if you will—full-scale segment—to demonstrate a lot of stuff: that we knew how to polish mirrors, we knew how to support them, we knew how to build an active-control system and really check the control *system*, not just the components. And we built a telescope that we could move so that we could change gravity. A bunch of stuff. We wanted it as complete a test as possible, so it would leave as little to the imagination [as possible]. It's not so much that we said, "Oh, we're worried about this—we'd better do a test," as that we wanted to make sure that we tested enough stuff so that no skeptic outside would say, "Well, I don't know why, but I bet you it's not going to work when you change gravity." We couldn't build the whole telescope, so we did the best we could. And toward the end of that, as we were finishing up the technical demonstration—and of course there was media attention for all this stuff all along—there was an article written in the *San Jose Mercury* [*News*], and a fellow read it who lived in San Jose. And his sister, Mrs. Hoffman, was rich. Her [late] husband, Max Hoffman, was the guy who imported BMWs and ran the Hoffman Motor Company in New York City. Now this story you've probably already heard.

MOY: I've heard it, but I hear different versions.

NELSON: [Laughing] That's the funny thing about oral histories. You get all these different definitions of the truth, and they're totally incompatible.

Anyway, this guy read in the newspaper about what we were doing, and he called us up. He says, "What's going on? My sister's interested in this kind of stuff." We sent him to Bob Kraft. And he talked to Bob Kraft, and Mrs. Hoffman got very interested and said she wanted to donate a bunch of money. Basically, she wanted to donate her estate of, like, $50 million to the University of California to build the Hoffman Telescope. It was wonderful.

Eugene Trefethen was doing fund-raising for us. He was president of Kaiser Industries. He's still alive [d. 1996—ed.]. He's been retired for years and years and years. I think he raised the money for the Bechtel Engineering Center.

MOY: So this was a UC fund-raiser, essentially?

NELSON: Yes. And Gene just did it for fun. He was a millionaire. He was retired, but a UC alum.

MOY: And do you know, had he been making any headway other than this?

NELSON: No, he hadn't. I dealt with him and talked to Gordon Getty. That was kind of interesting.

MOY: What was that like?

NELSON: That was fun. We went over to his house.

MOY: Nice house? [Laughter]

NELSON: [Laughter] Oh, yes. I went over there with [Ira] Heyman, who was chancellor of Berkeley at the time, and Gene Trefethen, and talked about astronomy. Getty's thing is the origins of man. He's funded all this stuff in Africa. But he gave us a few $100,000. Nothing significant on the scale that we were looking for.

MOY: So had you worked up sort of a little slide show by this point?

NELSON: Oh, I guess, some stuff. I was always happy to talk about this stuff. It seemed like a reasonable thing to do.

MOY: And the money had been coming—that you had to do the demonstrations—that was just UC money?

NELSON: Yes. Trefethen was going around the world, though. A president of Kaiser Industries, he knew every billionaire on the planet. So he was off in Hong Kong, he was off in New Delhi, he was talking to all these super-rich guys, the guys who could afford $50 million, which was what we said the price was at the time. And he hadn't come up with anything. And then out of the blue, this total stranger, who's worth $50 million, not worth $500 million or a billion, says, "I want to give you all my money." And we thought it was great. And just as she's doing this, Saxon retires.

MOY: This is '83, right?

NELSON: Yes. Saxon retires, Gardner comes in, and it wasn't clear whether she had $40 million or $50 million—and that wasn't going to be enough money. We needed more. And so Gardner walked into a situation where here's somebody who's offering $50 million to get a telescope named after her—or after her husband—but it was not all the money that was needed. Therefore, for Gardner to accept the money, he had to promise to put up the remaining money. And he walks in and says, "What is this project? I don't know anything about it." I'm sure he was scared shitless about saying, "Oh, great, give me the money." And as a matter of fact, he just sat on the deal. And it took him a year of very occasional communications before they finally shook on the deal and said, "Yes, we're going to do it."

MOY: And do you think he was trying to make sure that he'd be able to get the rest—but just not that hard?

NELSON: Yes. I mean, he had just walked into a new job—a great big job. A job much bigger than this telescope. UC is a big place. But the thought of casually committing yourself to getting $20 or $30 million of UC's funds—I'm sure he said, "Whoa, I don't need that kind of problem." I'm sure he was trying to put it off as much as he could. You don't want to say no; that would be nuts. But I'm sure he was handling it carefully. Because he knew the whole situation. She [Mrs. Hoffman] was dying of cancer, so it was tricky. And then there were lawyers playing games about setting up a trust fund and all this kind of stuff. It must have been at the end of '82 that we first were approached. Because at the end of '83—I think it was in December of '83—they finally agreed. They shook hands on it, they met face-to-face for the first time, after a year, and agreed what they were going to do. And she died the next day. A total fluke; she died the next day. She had cancer of the esophagus and had some blockage, and she went into surgery and died in surgery. Relatively minor surgical procedure. They had agreed, but they hadn't signed anything.

Then the relatives show up. It was very interesting. She had set up a board of directors consisting of herself, her secretary, and her sister, just to administer giving us all of her estate. She [Mrs. Hoffman] died, and her secretary handed us the estate, because that's what Mrs. Hoffman had wanted. But the sister said, "Wait a minute. Wait a minute. This is a great thing and we really want to do it. But wait a minute. Let's look at it more carefully." And she brought in tons of lawyers. Boy, I remember at LBL—buses of lawyers came up from UC and from New York City and came in to look at our project. And they just wanted to put strings in every place: "Well, we want reports on this. Then we'll give you another dollar." They clearly did not want to do it. And we talked and talked and talked, and it became more and more obvious that they didn't want to do it. And UC said, "Well, hey, we're not supposed to be taking gifts that people don't want to give us. It looks bad." And so after a lot of hassle, we ended up giving the money back.

MOY: I had heard that the deal that was worked out right before she died wasn't all money. There were paintings involved.

NELSON: Right. Renoirs.

MOY: Renoirs. BMW royalties.

NELSON: Right. We proceeded to sell off the paintings. I've got somewhere in my files one of these catalogs from—what's that big auction house in New York City—Sotheby's?

MOY: Yes.

NELSON: And they've got pictures of Renoirs in there—$5.3 million asking price. UC started selling off her estate. Because it was just given to them—a bunch of cash and all these BMW royalties, and art.

MOY: It was something like $36 million. Is that right?

NELSON: Yes. Maybe that was the cash part of it or something. It was never sharply defined what the estate was worth, because some of it was these non-cash assets that had to be liquidated. And then royalties are future assets.

MOY: You must have thought that the Hoffman money, before they met face to face, was actually going to happen. Was there some point where you had some sense of relief that this was really going to happen now because there was this source of money?

NELSON: Yes, sure, I thought, "This is great!"

MOY: And were you convinced at that point that now this telescope is going to exist?

NELSON: I was, but the funny thing is, I didn't ever really go around thinking it *wasn't* going to happen. Obviously it didn't need to happen, but I didn't dwell at all on, "Gee, I hope this happens. I wonder if it's going to happen. I hope nothing goes wrong." I just assumed it was going to go OK. I had an extremely positive attitude about it right from the beginning, just because I thought it was a great idea. And we were doing a good job. There's nothing more powerful than ideas whose time has come. And so my attitude was, "It's only a matter of time. Great, we've got the money now—or a good part of it." And when we got this money and realized it wasn't enough, then we said, "We need a partner." And we said, "Oh, OK. Twenty percent. We need a twenty-percent partner [The offer to Caltech from UC was for a twenty-five-percent share.—ed.]." Then we went to Caltech, they being eminent in astronomy, and we said, "Hey, would you guys like to become twenty-percent partners in this big telescope project?" And they said, "Interesting. Let us think about it. We'll see if we can find somebody with some money."

MOY: Was there anybody else in the running? Did you approach anybody else?

NELSON: We talked to Stanford. And Stanford has toyed on and off over the years with building up a strong astronomy department. They have some astronomers, but you don't hear about them at all in astronomy. They have a couple of radio astronomers and a few theoreticians, but they're not anything like a powerhouse in astronomy. And they seriously entertained doing that. Lots of universities do that: They say, "Oh, it's time for English. We're going to become number one in the country. We're gonna just go hire the best five guys in the world. *Blam*—overnight—we're going to be the best in the world." Stanford, being an extraordinarily rich place, has the wherewithal for doing stuff like that. They were thinking about diving into astronomy, and so they were thinking about buying into the telescope. We talked to them about it, but nothing came of it.

The Caltech thing—you've probably already heard about all this. [Caltech provost] Robbie [Rochus E.] Vogt was the guy who talked to Howard [B. Keck] at dinner one night and was telling him about this exciting prospect, and Howard Keck said, "That's great! Let's buy it. I'll buy it." He didn't want to buy twenty percent of it, he wanted to buy the whole thing—it was all or nothing. And there was a wonderfully complex story, where what he wanted to do was buy it for Caltech. He didn't want to become partners with UC at all. He said, "Hey, let's just buy this thing. What'll it cost?" And there was this terribly deadly thing going on where, perhaps at Keck's insistence, Caltech was trying to figure out whether they could take this idea and build a telescope without UC.

MOY: Now, let me back up just a little bit. I'd like to pin this down. My impression is that Caltech astronomers had been somewhat involved with some of the technical matters that had been going on already. Not officially, perhaps.

NELSON: Yes. Keith Matthews [chief instrument scientist] was really the one. He and I had talked a lot about the infrared parts of the telescope. So Keith was really the person who made contributions to the design of the telescope. Other people at Caltech I had talked to over meetings and stuff, and they knew what was going on, but they didn't play any particular role. But Keith did.

MOY: But the first communication really had been between UC astronomers and Caltech astronomers. As opposed to the administration, say.

NELSON: You mean for this twenty-percent thing?

MOY: I mean, even before the Keck money.

NELSON: Oh, well, certainly, as astronomers we all talked to each other about what we were doing. And they had their own ideas about big telescopes. So there was always lots of chitchat at meetings and the like. And I suspect for this twenty-percent thing, it was—surely there was discussion between Bob Kraft and the director of Palomar, Gerry Neugebauer. But then the administrations talked. [Marvin L. "Murph"] Goldberger was Caltech's president at the time. I don't know if Saxon approached him or whether Saxon had just retired—yeah, Saxon probably had left. So it must have been Gardner who pushed at the end of '83. Then all this stuff started happening. And then Howard Keck just said, "Yes, let's buy it for Caltech" and started this very stressful thing.

MOY: Yes. When I first read about this, it sounded like the Apocalypse—because the Hoffman thing was still pending, right? It wasn't finished.

NELSON: Right.

MOY: Do you remember where you were when you first heard about the Keck offer? Not that this was like the Kennedy assassination or something. [Laughter] But it does sound as though it could have been extremely— **[Tape ends]**

**Tape 2, Side 2**

NELSON: I think I heard it probably from Bill Frazer [then UC vice president for academic affairs], because the administration was talking about it. Well, once the administrations got involved, they kept everything very close to their chests, so it wasn't always easy to find out what was going on. We had hired Jerry [Gerald M.] Smith as our project manager, and he was working for us part-time. He worked at JPL [Jet Propulsion Laboratory], so he had a Caltech connection that was pretty profound. And I remember one incident on the astronomy advisory committee that he was attending when I discovered from him, indirectly— And this really made me mad. Here's a guy we hired; he's working for us. He had been asked by Caltech to figure out how much it would cost Caltech to build this telescope without UC, and he proceeded to do that, and he wouldn't tell us anything about it.

MOY: Do you think that this was primarily the [Caltech] administration? The Keck Foundation, it seems to me, was clearly the impetus for even investigating that, right?

NELSON: Right.

MOY: But then Goldberger, and maybe Vogt, were almost certainly part of coordinating that. But do you think the astronomers, too, were also—

NELSON: I doubt if they were involved at all. I think this kind of stuff happens at, you know—[motions, hand at high level]. The astronomers would have been a distraction. [Laughter] I mean, this is real power politics; this is heavy-duty stuff going on. And I'll bet these guys were just squirming like crazy. You know, I went to Caltech, and I know tons of people at Caltech. I've reported to them very closely now, since '85—'84, really. And I've been at UC forever. Caltech is a ruthless organization. They are really aggressive.

I remember, long before we got involved with Caltech—before we were even thinking about Caltech—Harold Ticho talking to me about how, if we ever got involved with Caltech on this project, he was quitting. Harold was the chairman of our Executive Management Committee at UC. He was a physicist at UCLA and he's now a vice chancellor at UC San Diego or something. He said, "Absolutely no way would I support that." Because, he said, "I've done this before. I've been involved in projects, and we let Caltech in, and they take the whole damn thing over, and they just cut you out." And he had just the strongest views: "Don't ever consider it. It would never be worth the price."

MOY: Had this ever occurred to you before—to you, personally, before?

NELSON: No. My interest was in building the telescope; I didn't really care who built it. If we needed to collaborate, that was fine with me. I wasn't trying to aggrandize UC, I was trying to push an idea. But I was struck by how strongly Ticho felt about how bad it would be for Caltech to be involved. This was out of the blue. It had nothing to do with what was to come. But then it was very interesting, because some of what he said was in fact true. I found that out in this first stage, when I realized that the people at Caltech took very seriously this thought of, "Let's take this UC-developed idea and steal it." And they looked into it.

MOY: And not even by buying the organization? Not even by taking you, for example. Just take the papers and run?

NELSON: Right. They looked into it.

MOY: I find it hard to imagine, first of all, that they would have thought that it could even work.

NELSON: Well, as I said, they looked into it. They had Jerry Smith, *our* project manager [laughing], evaluate that possibility for them: "How much more will it cost if we do it by ourselves than if we do it with UC?" And he came back with cost estimates: "Well, it will cost you $15 million extra," or some such thing. Then they decided it was impractical.

I suspect they were also just squirming at the thought of doing it. Because, I mean, science is competitive at some level, but that's pretty down-and-dirty. So I'm sure some of these guys were saying, "Wait a minute, we can't do this." But we all get greedy. And you've got some guy that says, "Hey, guys, I've got all the money you want. What do you say?" You know, it's easy to be tempted. It would be naïve to think that only bad guys would be tempted. I think we all could be tempted in the right circumstances.

I don't really know to what extent the impracticality deterred them, or whether somebody said [motioning, pounding table], "I don't think this is ethical. We can't do it that way." Probably a little bit of both. Reality is complex.

MOY: To hear Goldberger tell it, he said that Howard Keck—I think he said this in newspaper interviews—that Keck was personally really pushing to try to cut UC out of it, partly because he seemed to have a dislike of public institutions, generally.

NELSON: Yes. That was the perception we had at the time—that he didn't like public institutions.

MOY: Right, and that he—Goldberger—fought this idea rather vigorously, but not wanting to completely tell Howard Keck to go jump in the lake, you know, for obvious reasons. But rather forcefully tried to scuttle the idea.

NELSON: Uh-huh. And they did. [Laughter] And I suppose, depending on how you're telling the story, Goldberger might have said that in order to convince Howard Keck that it was a bad idea, he appealed to pragmatism and convinced him it would cost a lot more money. [Laughter] Who knows what people thought? But it was a very interesting circumstance.

MOY: Yes, and I'm curious to know a little more detail of what your feelings were at the time. First of all, when you first heard about the gift, did it seem as though this would cause just incredible problems, particularly with trying to work with the Caltech people again?

NELSON: I didn't have any bad feelings about working with Caltech. I came from Caltech. I know these guys.

MOY: Right. I mean the idea of the Keck offer, with the Hoffman offer still pending.

NELSON: Well, there was this period where Howard Keck said, "Hey, guys, let's just steal it and run." Of course Caltech was cooking on that one; they weren't telling UC about it. And I only got wind of it because I happened to have this conversation with our project manager—which really annoyed me—that *our* project manager was doing this.

MOY: Did it just slip, that he mentioned this?

NELSON: He made some slip—that he was doing some cost estimate for them. I asked him for details, and he refused to give them to me. I realized what he was up to, and I was mad. I was really mad. I just thought that was incredibly unethical, that he could be working for us and doing something so much against our interests.

MOY: This is why I asked. Because it seems to me that if something like that happened—if I were trying to steer a project, or my idea was being worked on this way, and it turned out that my partner, or at least some important faction of my partner, was doing this sort of thing—it could queer the relationship. And I would have serious concerns as to whether or not we'd be able to continue to work together. Were you concerned about that?

NELSON: Yes, everybody got concerned about it. After this initial thing—where Caltech somehow decided, "No, we're not going to do that. We're going to work with UC"—then the question was, What relationship were we going to have? We still had the

Hoffman money. So we went into a period where we said, "We've got two gifts. We'll build two telescopes." And we worked very hard and developed a design and a justification for two 10-meter telescopes.

MOY: Whose idea was that, by the way? Or was that so obvious that people just started talking about the idea?

NELSON: Yes, I think it was more that it was obvious. Bill Frazer was involved in that. I think Robbie Vogt was involved in that. I talked to Charlie Townes about it. Charlie has been interested in interferometry, and with two telescopes you do interferometry. But Frazer and Vogt—they were the guys doing all the negotiating between the institutions.

MOY: So this was at the administrative level.

NELSON: Oh, yes. They often kept me informed—I think Bill did as a courtesy. But they didn't *always* keep me informed.

MOY: Was that frustrating?

NELSON: Oh, sure. It drove me nuts. It still does. I always feel like this is my personal telescope so I deserve to always know everything that's going on. Whereas the administration, they feel like, "Wait a minute, the administration does all sorts of things in private, and it's nobody's business." And it's the institution's telescope and all I am is the project scientist. Thank you very much, but— [Laughter] There's no reason why they should tell me their secrets. And you know, I accept that, but at an emotional level I always feel very paternalistic about it. I feel like I want to know everything about it, even stuff that's irrelevant. So I'd ask questions and poke around and privately get a little annoyed when I didn't know what I wanted to know, and when I thought I was being kept in the dark.

But anyway, we worked on two telescopes. And that was good, in the sense that Keck had agreed that, yes, UC had a right to live and breathe on this planet—which wasn't his starting position, of course. And we worked assiduously to get money out of the Hoffmans. And, as I said, all their lawyers came out, and I think Bill [Frazer] and his lawyers went back to New York many times. And I went back there and talked to the lawyers and talked to the secretary. But the end of all that was that we ended up giving the money back. It just wasn't going to work out. It looked incredibly difficult to get the Hoffman money.

MOY: It seems to me that the foundations' offers seemed to compete with each other. The fact that there was this Keck offer might spur the Hoffman people to re-think [their position].

NELSON: I think Mrs. Hoffman's sister felt even more strongly that she could get out from under this obligation because of the other offer. Because she didn't want us to have the money. She wanted to spend it herself on whatever her charities were—giving it to her relatives or something. I don't mean to say that she ever said that, but everybody read that. When there was Keck money now, I think she said, "Hey, you guys are going to build your telescope. You don't need us." So she felt it was even easier to play a hard line.

MOY: There was also this story—I think this is from an *LA Times* article—that the final impetus, at least, for giving the Hoffman money back was something of an ultimatum from Murph Goldberger to either Saxon or Frazer: Work it out either one way or another, or Caltech is just going to take the Keck money and run.[2]

NELSON: I don't know that.

MOY: I haven't been able to figure out exactly where this reporter got this idea either.

NELSON: First off, here's UC astronomers who one day think they're going to build a telescope. They're going to own the world's largest telescope. They're going to really corner the market on astronomy. And then the next day they hear that their fiercest

[2] 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.

competitors are going to get half of their telescope. So the UC astronomers were, "Hey, wait a minute. We got shafted here." There's a mixture of competition and respect and perhaps a little contempt between the institutions and astronomers. So there are some real concerns, part of it I think probably along the same lines as Ticho's feelings—that Caltech people are difficult to work with.

MOY: And when you say Caltech people, you're talking about the astronomers, too—not just the administration?

NELSON: Astronomers—yes. The scientists, the faculty, not the administration. The perception is, I think, that Caltech scientists think of themselves as clearly the best, that there is no need for them to ever consider anybody else, you just walk over them. And if you see something, then it must be yours. And it's just, you know, utter egomania. Obviously that's a cartoon, but it's a not-unheard-of view of how Caltech people work, and I think there's some truth to it. An institution does cultivate certain attitudes, which rub off in varying degrees on its people; some people may exhibit enormous amounts of it and others exhibit none at all. My experience in dealing with Caltech people is that some of them really do have a painful amount of arrogance: "If Caltech does it, it's right." If they decide to step on your foot, it must be right. And so they just take what they want. Not at all to suggest that most of the astronomers feel that way, but I see that in the administration and a little bit in some of the astronomers.

Anyway, there was a lot of trepidation. There was a lot of disappointment at UC that "Oh, we just lost half of our telescope." They didn't say, "We just got half a telescope," right? They thought they had the whole thing—that was the weird thing—because of the Hoffman money. They lost half a telescope to their worst competitors. That was really a disappointment.

MOY: And did you share this disappointment?

NELSON: I think not nearly as strongly as my colleagues did. My colleagues used to berate me with it. There was a lot of anger about it. People didn't want to work with Caltech. There was a lot of worrying that it wouldn't work out, that we were just going to fight and fight and fight. And I more or less reserved judgment. I mean, I grew up here [at Caltech]. I knew guys and respected them, and I said, "Well, that will work out."

MOY: And there wasn't any sense that you personally had lost part of the telescope. Because it's still yours.

NELSON: Exactly. So I wasn't concerned in the same way at all. But there was a period, when we were developing this marriage, when there was a lot of anxiety on both sides over whether this was going to work. Everybody was really touchy. Newspaper articles would come out, and people would just go totally nonlinear about some anonymous remark said by some UC astronomer. I remember once, in the middle of the night, Robbie Vogt called me up and started ranting and raving at me. I don't know if you've ever met Robbie. Have you met him?

MOY: I haven't.

NELSON: He's a very powerful and very emotional character. And paranoid. It was very strange. I don't meet very many paranoids in science. He's paranoid.

MOY: An honest-to-god paranoid?

NELSON: Honest to god. He's a paranoid. And he's this very powerful person, and he was very influential. It's kind of weird. Paranoids are usually hiding out, but he wasn't. He was right out there, and he was paranoid. And I found I couldn't reason with him.

So one night he called me up in the middle of the night—2:00 a.m. or something—and he just started screaming at me about some dastardly thing I had said to some newspaper. Which I had never said; I had never even spoken to the guy. And I couldn't convince him. He thought it was a plot. He thought we were scheming against him. We were deliberately doing these things and doing all sorts of subterfuges, and we were against *him*. It was like, you know, we were against Robbie [motioning, pounding table]. It was very odd. And actually it got so bad—he felt so much rancor against these amorphous forces that were against him—that he finally stepped down as the chairman of the Caltech side of it. My sense was that they asked him to step down because they realized that psychologically he couldn't handle that position. I don't know what the facts were, but I saw what was happening, and I said, "Man, this guy is bad news. He's gonna screw things up." He just overwhelmed me.

MOY: I do know he did step down because of issues revolving around the telescope. That was certainly the case. And he and Goldberger were apparently at odds.

NELSON: He was just consumed by paranoia when I interacted with him and saw him interact with people. Instead of asking, "What are our problems today? What do we need to go forward?" he was worried about getting stabbed in the back by UC or something. It was just terrible.

And people were *not* trying to stab him in the back. UC people— Particularly Bill Frazer, whom you interviewed, is an extremely meek and mild-mannered person. The way the UC organization works, it's got campuses that are autonomous and a system that has to hold them together and raise money for them in Sacramento. But all the students, all the faculty, all the non–State of California fund-raising, is all done by the campuses. The administration's job is to be perpetually pouring oil on troubled waters and trying to keep these campuses together and let them be sort of autonomous but follow reasonable rules about this and that, and have everybody feel good. Bill's perfect for that because he's a very nice guy. He's not trying to break new ground, he's trying to mollify problems.

And some of us got frustrated with that. We wanted UC to be more aggressive in its negotiations with Caltech. And Bill was just persistent in being extremely meek and mild-mannered. Caltech would ask for something, and he'd say, "It's a very interesting offer," just trying to figure out a way that he could in effect say, "Yes, we accept your offer" without losing what he thought was essential for UC. And it frustrated many of us, who thought he should have said, "No, that's a stupid, shitty thing. Fuck you." [Laughter]

So Bill was certainly never out to stab anybody's back, because it's his whole nature—it's much more supportive than that. I think he found it very difficult negotiating with Robbie, because of Robbie's paranoia. And it just didn't go away. It got worse, actually, over the course of a year or so. And then he [Robbie] stepped down. As I said, I think all of it just consumed him.

MOY: So he and Frazer, as far as I can see, were primarily the architects for the final agreement and the formation of CARA [California Association for Research in Astronomy] and so on. Were you involved in that very much?

NELSON: My involvement certainly wasn't central. But I saw the drafts, I edited them and talked to Bill Frazer and suggested changes, and maybe he listened or maybe he didn't. But I didn't go to the negotiating sessions.

MOY: But did the working out of that quell any of your concerns or the concerns of other UC astronomers, as far as you know, about what it would be like working with Caltech?

NELSON: No. We were nervous. One of the things, for example—I don't know if you ever looked at the contract.

MOY: No, I haven't.

NELSON: You look at the contract, and you'll discover that Caltech owns the telescope.

MOY: I hadn't realized that.

NELSON: They own the telescope. UC doesn't own half the telescope. Caltech owns the whole telescope.

MOY: Because they paid for the construction?

NELSON: No. That was the rationalization, of course. I thought it was terrible that we would accept that position—that they would own the telescope. Caltech owns the lease to the land, not UC. And a lot of us who saw these things in the contract were horrified.

We said, “Wait a minute, that’s not equal partners.” Because the deal was “equal partners.” Caltech puts up the capital, UC puts up instrument money and the operating expenses for twenty-five years, and we put in all the technological know-how to build the telescope and all the ideas. And we said OK. Whatever “fair” means, we thought that was an equitable deal. It’s kind of hard to balance intellectual things against money, particularly—I mean, it’s Keck’s money, it’s not an astronomer putting up money. But after a while you say, “There isn’t any ‘fair’ here.” [Laughter] But the concept of equal partners—which I finally decided was fine—I thought it was perfectly reasonable and arbitrary, and let’s go with it. When I discovered that Caltech owned the telescope and that Caltech controlled the lease to the land, I thought, “That’s not fair.” I said, “You’re gonna get screwed.” And Bill said, “No, Caltech is insistent on doing it that way. And my lawyers inform me that as long as the contract says that all decisions about anything and even how the agreement is renewed in the future are all done equally, then we have the control we need to ensure that UC astronomers get half the observing time”—which is what matters. I think the contract does that. And to date there have been no indications that Caltech has any inclinations to, in some clever way, take advantage of the fact that in the contract they are the legal owner of the telescope. But I think it’s an interesting symbolism of the imbalance between the two institutions. And Bill Frazer was perfectly willing to give away those things, as long as he thought he had adequate guarantees that astronomers would get half the use—and have their half of the control of how monies were spent and the like.

MOY: Actually, that does bring to mind a question I was wondering about—the selection of the site. How was the Mauna Kea site selected?

NELSON: A group of people at UC, back in the late seventies, looked into what’s the best site. Bob Kraft was the chair of that committee, in fact. And they looked at the Canary Islands, and looked at this place I can’t remember anymore [Junipero Serra Peak], south of Santa Cruz. We looked at White Mountain. We actually did a seeing study for two years at White Mountain—that’s east of the Sierras. And looked at Mauna Kea and looked at Chile. Did a reasonably comprehensive job. Some measurements and a lot of gathering of other people's statistics. Merle Walker, who was a professor at UC Santa Cruz at the time, fortunately was the world's expert on seeing, and he had placed seeing monitors all over the world. So he had great access to all the data—knew lots about all these sites. So it was relatively straightforward to compile tabulations of the relevant parameters of good sites and have some confidence that you knew what you were talking about, because Merle had been studying the subject for years. He was the kind of guy—he wasn't a very decisive person—but he gathered the information, and if you could get it out of him, you could then say, "Oh, look, this sky's got two hundred clear nights a year, this sky has a one hundred. Two hundred is better than a one hundred, right?" He may not tell you that it's better, but you could figure it out yourself. And he knew all the numbers. In looking over all the pros and cons of all the sites, we decided, back in '79 or '80, or something, that Mauna Kea was where we wanted to go.

MOY: And how was the site acquired? Do you know? Did CARA buy the site?

NELSON: Well, we approached the University of Hawaii, which controls the whole summit area of Mauna Kea; everything above 12,000 feet is a science preserve. And it's administered by the Institute for Astronomy at the University of Hawaii. Other than an endless list of permits you have to get, you need to get the approval of the Institute for Astronomy. And they're a bunch of astronomers, and they drive an extremely hard bargain. They know they have a good site. We signed a fifty-year lease, I think, for the piece of property that we have there.

MOY: And was there some debate about where the headquarters should be? In Waimea or Hilo?

NELSON: Uh huh. Yeah.

MOY: I never really understood exactly why that was.

NELSON: That was funny. That was actually a hot potato.

MOY: Yes, and I never understood why.

NELSON: I think what happened is this. Most of us at UC wanted to go to Waimea. I'd been out to Hawaii a number of times, because, of course, that was a good site. There were telescopes there, and I looked at all these telescopes. I was interested in the site. And I thought—and many other astronomers at UC that have been out there observing—they all thought Waimea was nicer than Hilo. Hilo's hot and rainy, and Waimea's kind of a cool, temperate climate and a really quiet place. And the rest of us city slickers thought it would be nice to get out of the city. So we thought it would be a nice place to go. Caltech already was building a 10-meter submillimeter wave telescope here—a much smaller system, but they nonetheless already had a crew for that in Hilo. And so they already had one foot in Hilo. I think it only gave them a predilection toward Hilo; I don't know that there was anything specific stronger than that. But it was not easy to tell sometimes.

I saw Gerry Neugebauer thrash around, trying very hard to open the issue up for debate as though he didn't care where we went. But it always looked like he absolutely had the strongest opinion in the world about where we should go, that it should be Hilo, but he would never say it. If you ever said, "Well, let's go to Waimea," he'd say, "No, no. I think it's really important that we discuss this properly, and that we get all the opinions." He never said, "No, I kind of prefer Hilo." It was very weird. But it was very contentious for a while. And this was one place where I think Bill [Frazer] got involved and said, "Look, we really want it to be in Waimea." There wasn't a good argument against going to Waimea. You know, you can make lists of schools and jobs and size of population and quality of life until you're blue in the face, and whatever side you want to win will win. This is the one instance I can think of, actually, where UC wanted something and basically Caltech wanted something else and finally said, "OK, we'll go to Waimea." I don't really know how that got sorted out. There was a little committee that was supposed to resolve it, and I was on the committee. And I remember once saying, "Oh, I think we should go to Waimea." And I realized, "God, that was naïve, Nelson. You're not supposed to have an opinion on a committee." It was like there's some secret rules and you're supposed to be so judge-like, so nobody ever knows what you think. I'm just not that kind of person.

MOY: I had a number of questions about the technical matters that you've actually discussed already. The project's not completely finished yet, obviously, but it's clearly on its way and I think we pretty much know what's going to happen. Are there things now that stand out as being surprisingly difficult to you?

NELSON: The mirror fabrication was really hard. And a lot of it was really hard because of Itek, the vendor we picked. They weren't a good choice. They were very bureaucratic and extraordinarily expensive. They weren't flexible at all, and the guys we worked with turned out to be not very smart. They didn't have innovative ideas for solving problems, they just had expensive ways of solving problems.

MOY: They did mostly Star Wars stuff, right?

NELSON: Yes, high-tech military optics. And, as I said, they're very expensive and really a ponderous organization. I mean, you asked for something and you had to fill out miles of paperwork. You'd say, "Can you test such-and-such?" And you think it will maybe take a couple hours for one of their guys to do it in the afternoon, and you've got to spend two months writing letters back and forth and signing contracts and change orders. It's just your worst nightmare.

MOY: They've been working for the government too long.

NELSON: Yeah, exactly. That was bad. And the people weren't good. The combination of the bureaucratic structure, which was inflexible, and the fact that we had mediocre people, cost us a huge amount of money. It wouldn't be fair to say that that was the sole difficulty in making the optics—it wasn't, but it was sure a major contributor.

But there were other problems with the optics, which we still don't understand. I told you we had stressed-mirror polishing with a model—a quarter-scale model. Kitt Peak stressed-mirror-polished a mirror—a full-size mirror. And we had polished a full-size mirror and cut a full-size mirror as part of the technical demonstration. And we thought we understood this business. When we got into doing it with Itek, getting the spheres that we polished in was harder than we had experienced at Tinsley or that I think Kitt Peak had experienced.

MOY: And Kitt Peak had done it themselves? Or did they contract out to somebody to do it?

NELSON: No, they did it themselves in their optical shop.

MOY: But you guys subcontracted out to Tinsley, right? Just to do this demo?

NELSON: Yes. Well, and we worked with them often. Tinsley was really nice because we'd just go down there.

MOY: They're in Richmond, California—right?

NELSON: Yes. They were in Berkeley at the time, so it was really quite convenient. We'd just go there and work all day. They were just so loose and so happy to do this kind of stuff.

MOY: What kind of stuff had they done before?

NELSON: Small optics. They did a few medium-size telescopes. And they liked esoteric objects. Harvey Morton, who's the chairman [d. 2005—ed.], basically was a gadgeteer. So he liked neat stuff. And he didn't care about making money. Because we were coming there with neat ideas, he said, "Great, sure, come on in." And as long as we paid for the labor of the guys we were using, he was happy—the whole place was open to us. It was really nice. They'll probably never get super-rich doing that stuff, but it was really a nice environment for what we were doing. We were doing development, so we wanted the freedom to be able to go in and check something, and do a test another way, and do the optical tests ourselves—mess around. We'd go in there and work for weeks at a time and never use them at all; three or four of us using all their equipment and none of their personnel. And that was fine with them, which was really nice. It was a big help.

But Itek, they were awkward and very expensive. They still are, of course. They had a hard time polishing good spheres. And I really don't know why. It's not fair to just blame it on them. Because I don't know exactly what they were doing that wasn't right. But it proved to be difficult.

MOY: And they had anticipated using a computer-controlled tool to do touch-up, right? And that didn't work out?

NELSON: Right. That didn't work out. We got sold a bill of goods on that one. Terry and I saw it coming. Both Terry and I were on the selection committee, and we were both opposed to Itek. We just— They smelled funny. It's hard to put your finger on exactly what it was. We had all sorts of criteria for judging these companies, and they're very good at playing this game. They bring in engineers and scientists who you swear they said were going to be on your project, and they're really good guys. You talk to them and, "Hey, these are smart guys." But when your project starts, they're nowhere to be seen. We asked. We said, "Look, we want to know who our team is going to be." We weren't totally naïve. In fact, Jerry Smith is very savvy about this kind of stuff—he organized some of this. But they fool you nonetheless. They're very experienced and so they traipse through all their best guys, and after a while you get confused as to who is a consultant in the background and who's going to be on your team half-time, or something. Then you discover you don't have the guys you wanted.

Anyway, the cutting of the mirrors caused them to warp. That was a bigger problem than we had thought. And then the computer-controlled polishing. Terry and I had been very nervous about that, from what we'd seen of it. And then the guy at Itek who was in charge of it was a blithering idiot. He was just terrible. It wasn't so much that the idea was bad, it was that I thought his implementation of it was very bad, and you couldn't talk to him. He couldn't explain what he was doing, and he had zero interest in anybody else's input into improving the method. So if he did something wrong, that was life. You couldn't say, "Oh, if you would just run it clockwise and then counterclockwise half the time, it might be smoother." He absolutely didn't give a damn what you thought. So that was bad, because they had a method that didn't really work. And both Terry and I had all kinds of opinions as to how to make it work better. But it was an impenetrable system. The method worked poorly—converged very slowly, roughened the mirrors up, really didn't look like it was what we wanted. Didn't look like we could afford it.

And so we ended up saying, "All right, we'll use warping harnesses," something Terry and I developed, back in the technical demonstration when we realized that the mirrors weren't going to come out perfect, and you really needed some ability to make adjustments of the mirror surfaces. But even with warping harnesses, we realized that our mirrors really weren't going to be as good as we wanted them to be.

And very fortunately ion-figuring matured at that time, and Kodak developed a facility that could handle mirrors of our size. And now they're ion-figuring our mirrors. That's a great technique, and it's real cheap and simple and does a beautiful job.

MOY: Ion-figuring? I'm not familiar with that.

NELSON: Basically, you take an ion gun—it just blasts argon atoms out at about a kilovolt energy—and you put the whole mess in a vacuum chamber, and you point this ion gun—it has a throat about, you know, this big [motioning, hand width].

MOY: Just a hand across or something?

NELSON: Yeah. If you look at the intensity of the beam coming out of it, it's actually perfectly Gaussian with a sigma of about 2.5 centimeters. And you simply put this thing on a stage—you point it at the mirror and the argon atoms that run into the glass knock off atoms. So it erodes away the surface. Where there's a high spot, you go real slow over it. If there's a low spot, you go real fast over it. So you just do a raster scan, but because the edges are Gaussian, it blends in just perfectly. And it's highly predictable.

MOY: And Kodak is doing this?

NELSON: Yes, Kodak. People in New Mexico developed it. NASA actually developed the high-intensity ion beam some years back for space propulsion, which was the missing ingredient. Aden Meinel invented this—I don't know—some twenty-five years ago.

MOY: Who was that?

NELSON: Aden Meinel is another telescope designer. A great character. But they didn't have any intense ion sources, so there was no practical use. But then NASA developed intense ion beams, and then these guys in New Mexico developed it and did tests on different kinds of glasses and made it look like, "Yeah, this is a real technique." And then Kodak scaled it up to 2-meter size—perfect for our segments. So it really works well.

**Tape 3, Side 1**

MOY: Some of the mirrors were eventually contracted back to Tinsley. Isn't that right?

NELSON: Yes. Tinsley did about a third of the mirrors for Keck I, and they'll do about two-thirds of the mirrors for Keck II.

MOY: And have they been generally better than the Itek people?

NELSON: No, they haven't. They're cheaper and faster, but they're not better. It's very odd, and I wish I understood it. They basically support the mirror the same way Itek does. Not identical, but very similar. But their polishing techniques are different, yet the mirrors don't come out any better. They're OK—they're as good as Itek's. But they still have medium-frequency lumps and bumps in them which we don't expect.

MOY: Well, my last question is, are there things that have been surprisingly easy in this endeavor?

NELSON: The active-control system. Early on, people said, "Oh, my God! You can't do that." It's actually been straightforward from the beginning. Well, "surprisingly easy" is not quite the term—Terry and I always thought it would be easy. Once we worked out the mathematics, we said, "Oh, this is going to work. We understand it; it'll work OK." And I still think that. But the hardware had some rough spots in the early eighties. Once we got into the Keck project, it's going along very well and the hardware's been wonderful. We've really been quite happy with it. We've used the ACS [active-control system] now, and although there are lots of things in it still to debug, generally it's been a trouble-free part of the system. It's not a big hindrance in debugging the rest of the telescope. It's there, it does what it's supposed to do. There are lots of alignment issues that we still haven't solved, but something that many people very early on thought was sort of a fatal flaw in this kind of a telescope has really not proved to be a terrible problem to us. It cost a fair amount of money to build the hardware, and we were quite careful with it, which is why it cost so much money, and maybe why it works so well.

MOY: And computer-wise, it hasn't been too much of a difficulty?

NELSON: The computing is actually easy. The way it's done, it's not really computationally intensive. We in fact have a dozen microprocessors involved with the active-control system. But about ninety-nine percent of the computations they do is irrelevant to the control. It's doing other superfluous stuff; what people put in it I think we don't even need. The control algorithm itself is really quite elementary.

MOY: OK. That pretty much does it for the questions I had. Are there any other things you wanted to mention?

NELSON: I don't know. It has sure been an interesting project. And, of course, it's been a long time now. I've been at it for fifteen years. But it's very diverse. I got very involved also in the structural engineering of a telescope in the late seventies, which I found was a lot of fun. Not the optics—the primary mirror—which we spent all our time talking about, but the space frame. How do you do that? How are you supposed to do it? How do you optimize it to make it as stiff as possible? And I've had wonderful battles with Steve Medwadowski, the structural engineer we hired, who was a professor at Berkeley.

MOY: He designed the actual frame, right?

NELSON: Yes, right. He and I worked closely together on that and just fought and fought and fought.

MOY: Is he still at Berkeley?

NELSON: No, he's a consulting engineer in San Francisco now. He's quit teaching at Berkeley. He was approaching the idea of building a telescope sort of the way you would build an office building—a big, massive kind of thing. And I said, "No, I want it to be massless." [Laughter] I said, "Wait a minute. No, no, we want stiffness here. We don't want mass. You don't need mass for stiffness. It's just a geometry problem." And it took me a year to convince him, to get him to see my perspective on what the design philosophy should be. It took a year of just endless arguing about stuff. It was very stimulating. I mean, I'm a better physicist than he is, but he's a better engineer than I am. He knows all the practical stuff, which I don't know anything about. But I do what I want with it. But I was always listening, and I could go off and do calculations, and he could go off and do calculations, and sometimes I understood the principle better than he did. And every so often I would say, "Steve, that's wrong." And he'd go off and he'd come back and say, "Jeez, you're right. It's wrong." It was gratifying to be able to do that to him—a professional engineer who is very good. It was fun. So I learned a lot. Studying the structure was really fun.

MOY: That's interesting. Because another person I talked to was Bev [John Beverley] Oke [professor of astronomy, emeritus; d. 2004], and he spent most of the conversation talking about the contributions he made to the design of the dome—you know, where the Nasmyth deck should be. And he was really into that. Of course, what I read is all about the optics. And technically speaking, that seems to be the most challenging and the most engaging element of the entire device. And then people start talking about how to make the elevators, and he was really into the elevators. And in thinking about it, I realized that could become a very interesting problem.

NELSON: Indeed. One of the things that happened in my life just as I was getting involved in this project, I ran into a children's toy called Ramagon. And I highly recommend it to you. You can probably still get a Ramagon set if you go to the Lawrence Hall of Science, or the Exploratorium, or something. It's [like] a Tinkertoy, and basically it's a box full of struts—little plastic struts—with snaps on the ends, and balls with holes in them. You just push the struts into the holes in the balls, and they click into place. You can build space frames with these things. And the nodes—the balls with the holes in them—are intelligently designed, so you can build all sorts of regular tetrahedral and square stuff. It's quite a versatile toy. I just bought pounds of this stuff, and I have spent literally thousands of hours sitting on the living room floor at night—I'd go home and spend five hours every night—playing with these damn things, building stuff. It was wonderful relaxation, wonderful fun, and I think it was valuable to this project. Because doing that, I developed an extremely keen awareness for how to optimize structures. And I learned about determinate structures—that is, structures that are stiff with no extra members. And I'm pretty good. You give me a structure and I can say, "Wait a minute, you don't need that one, that one, and that one—those are redundant." Or, "There's not enough there." And of course, as I play with them I'm thinking, and I write down mathematical formulas for stuff. I like to play with my hands and play with math at the same time, so I thought it was just a wonderful toy.

Being able to think about structures was, of course, a big help for the telescope structure itself. I did a lot of work on that design. So did Lubliner, by the way. Even though Medwadowski was our structural engineer, Coby did a lot of the early work on it, and I did a lot of the early work on it. I worked with Steve very, very closely over almost every detail.

There is another issue that we haven't touched on at all, which is the support of the individual mirrors—the whiffletrees and the radial support. That was tricky stuff, which was a lot of fun. Almost every place you look in the telescope, you run into questions of how you hold something stiffly without over-constraining it. If you overconstrain it, you'll bend it. So you have to think about the number of degrees of freedom there are in an object that are needed to define it in space. If it's supposedly a rigid object, you need six constraints on it—x, y, z, and three rotations. And you say, "OK, how do I connect this thing to something else, rigidly, without ever inducing any stresses in either one of them, even when gravity changes and temperature changes?" That general class of problems comes up over and over again, in telescopes in particular, where deformations are deadly. So being able to think through structural, mechanical questions easily and efficiently is really helpful. And it was a funny thing—I found that I actually got pretty good at that, and the engineers I worked with I found *weren't* very good at that. Some of them were just awful. They just had no idea about what I considered to be the principles of engineering. [Laughter] They think it's probably irrelevant, but for at least this kind of a project, they're very important principles, which they had never learned in school and didn't appreciate. So I found—not because I planned on it—but I found that, in fact, engineers as a group don't have those skills. So the fact that I had developed them was really important. I used them all the time in working with our engineers to correct terrible design flaws that would have caused a lot of grief.

Now you go to a place like Kitt Peak and you find people who have been designing telescopes for decades, and those guys know those rules. But it's not easy to find people who know those rules. So that was kind of a fun thing. One of the things I've enjoyed about the project is it's very diverse. I'm trained as a physicist, I'm a professor of astronomy, but somehow I feel in my heart I'm probably an applied physicist, or something, because I like engineering stuff. And these mundane little things I get a big kick out of.

MOY: Well, let me ask you this, then, because I was also struck by the diversity of the problems. And I'm interested that you said that you feel you've gotten a taste or sense for how to deal with problems that turn out to be engineering problems peculiar to telescope design and construction. I'm just wondering, after having gone through all this, would you want to do this again for other people? I'm just curious whether you look forward to building more telescopes.

NELSON: Sure. Yeah. I mean, I'm on the review committee for the Green Bank telescope, which is a 100-meter radio telescope, which has been interesting. Now they're far along, so I really don't have much to contribute. But early on, in their first couple of meetings, when they weren't so far along, I had all kinds of criticisms and comments and did calculations for them and gave them all sorts of ideas—because they were grappling with problems that were sufficiently closely related to my experience. I had all kinds of knowledge and input that was useful to them, I think. It was a lot of fun. I'd say, "Oh, new problem, but close enough to mine that I have something to say about it." It's always a kick to be able to do that.

MOY: But if someone came along, for example, and said that they did want to build a 25-meter optical telescope now, would you be at the door waiting for them?

NELSON: I'd think about it. I'd definitely think about it. I mean, there are things about this project that I haven't liked. Technically it's been diverse, and I've found almost all the engineering interesting to learn about: "Oh, there's a problem. OK, well, get out your books, learn stuff." So I'm sort of the self-taught engineer—master of no skills. But it's been fun and that's what you do in life. You should just go along and have a good time.

But the management process, all the meetings— Meetings are really tedious. I go to meetings all the time, and I'm not good at saying, "No, I'm not going to the meetings." And I don't have the skill to recognize when my input is not needed, when I have nothing to contribute or nothing to learn. So I end up saying, "Oh, it's about the telescope. Yeah, I should go." Then I come out, "God, I've wasted another day." Terrible.

We have a project manager, Jerry Smith. I don't know if you've talked to him yet.

MOY: No, but it's something we were considering.

NELSON: Yes, you might be interested in doing so. He's our project manager and he's in charge. He controls the money, he hires and fires people, and I sort of advise him; I guess that's my position. And we have a terrible relationship. You know, he's the guy who was doing this stuff for Caltech when he was working for us. I find him a cold, heartless bastard. No, really. I've been working with him for years and it's just incredibly difficult. And that's the one part of the project I would have really loved to have not gone through. Because this guy is so indifferent to human needs and to technical needs. He's a manager. He says, "No, we're going to do this! [Motions, pounds table.] Shut up! Let's do that."

MOY: And your relationship was like this before you found out that he was working for Caltech?

NELSON: I didn't work with him very much, but I always knew he was pretty cold. And I've seen, over the years, watching him work with everybody, that's he's very uncaring and often cruel to people. Which bothers me. He comes from the hard-ass school of management, somehow. Some managers, I think, are that way; they think that's the only way to do it. The amazing thing is that it just comes natural to him. He absolutely doesn't care if he runs you over. He doesn't know you exist. I find that incredible, because most people aren't that way. They might be mean, they might get mad and slug you, or do something, but—you know—there are feelings in there. But he's just a bastard.

It's really hard to work with him. And I'm the only voice in the observatory that's independent of him. I'll say, "Smith, you're wrong. Shouldn't do that, it's not the right thing to do." And I'll argue with him about it. Of course, he hates it, because he's an authoritarian guy and he's the god. Nobody else can challenge him or he'll fire them. So I'm always stuck with the job, and it never does any direct good. So perpetually when there are problems I end up having to work around him and go and talk to the engineers and say, "Look, Smith said we should do this, but it's really the wrong thing to do." And I'll explain to the engineer what's going on, and why if you do that, it will break, and if you do this, it will be OK, and they'll go away and design stuff. It's inefficient and it just has driven me crazy.

MOY: But has he been effective, do you think, as a manager?

NELSON: He's OK. He's very good about money and schedule, which are terribly important things. He knows about those things. And he's real sensitive to when they're getting out of control. So it isn't like, "Oh, gosh, gee, I didn't realize that." He's really on top of that stuff and is so decisive that when he sees it happening, he starts making decisions so that you don't get stuck. "Oh, let's study this more." He doesn't study stuff more, he just says, "Don't do that, and get on with it."

MOY: What is his background?

NELSON: I think he was in the Navy—was he in the military? He has a degree in electrical engineering from USC, and he was a project manager at JPL. He was the project manager for the IRTF [Infrared Telescope Facility], a telescope on Mauna Kea. And he came on for the last half of the IRAS, the Infrared Astronomical Satellite. He was the manager for that. So he has excellent credentials, but everybody knows him as a hard-ass guy. Clearly, particularly in projects that have risks in them—technological risks—you need people who can keep you moving. Because if you get stuck studying too many things too long, you spend time on it, and time is money, and pretty soon you're in serious trouble. So he has real skills. But I'm not of the opinion that those skills inevitably are connected to the kind of cruelty and indifference that he also happens to have.

So personally, that's been just a terrible burden to me. And over the duration of my relationship with him, I'd have to go home and talk to my wife endlessly about, "Guess what horrible thing happened today." Everybody in the project spends a lot of time talking about it, because it causes everybody a lot of grief. I've been perpetually a thorn in his side, because he can't fire me. He tried once, actually.

MOY: Really? Who'd he go to?

NELSON: Well, what happened was, when the project got created, it was decided to put the project headquarters at Caltech. It had to go somewhere. You might have thought the natural place would have been Berkeley, where those of us who had created the design were. But Jerry Smith wanted to put it down here in Pasadena. He wasn't interested in putting it where all the work had been done. Being a professor of astronomy in Berkeley, with a family in Berkeley, my intellectual resources were in Berkeley. I said, "I'm going to stay in Berkeley. I'm not going to move to Caltech or move to Pasadena." But he set up the project office in Pasadena. I came down here regularly. Because I was so independent of him, I didn't do what he wanted me to do, I did what I thought was right. I didn't act like his employee. We'd had friction from the start. And at some point after—I don't know—about a year or something, he decided it would be advantageous if there were a project scientist down here full-time. So he set out to hire a project scientist to have in Pasadena. I would still be project scientist in Berkeley, that was fine. But he was going to hire a project scientist in Pasadena.

MOY: Was that the idea—that there would be two project scientists?

NELSON: Who can say? My impression was that the idea was that he would get a project scientist to his liking who could do whatever he thought a project scientist was supposed to do. And not have me around giving opinions about how things ought to be built. I think his hope was that I would go off and do whatever I wanted to do and be irrelevant because he wouldn't need me anymore. He certainly needed somebody called a project scientist, for nothing else but for face. He's got to go to the committees and say, "Oh, that's my project scientist. I'm getting good advice on that astronomical stuff." Because I sort of played two roles. One role was the role of scientist astronomer. The other role was that of designer, and he's never liked that role. He's always wanted to sort of put me in a box: "Why don't you be the astronomer, keep track of the quality control, but stay out of the engineering?" But I've always been completely unwilling to play that game. Anyway, he tried to hire another astronomer to come in and be the local project scientist. We managed to keep that from happening.

MOY: Did the effort go anywhere on his part?

NELSON: Yes. He picked out a candidate, brought him over here and interviewed him, and sent him up to talk to me. And I said, "Hey, Rich, don't do this. This is going to be real bad news. You're getting in the middle of a nightmare here, if you do this." And I don't know exactly how it was avoided, but it didn't come to pass. I think there were a lot of people who said, "Wait a minute. This is not a good thing to do here."

So there have been little bumpy spots in the road. As I said, this conflict with Smith—who's a very tough guy—that's been the real pain to me in this project. Everything else has been great. And I really enjoy working with the UC astronomers. I really like working with the Caltech astronomers. I say these negative things about Caltech, generically, but they have been *super* about contributing to the project once we got started. If you needed advice on something—management, technical, anything—Caltech astronomers, they'd come to the meetings and they'd start giving you opinions. They were always real good. They said, "Oh, this is our duty to get involved and help." And they'd help. Much better than UC astronomers. A lot of the UC astronomers, we never see them. We're going to see them when the telescope is finished, but they're not looking to serve on committees and do grunt work. Which needs people. But they don't want to do any of that. Whereas the Caltech guys have been much better at that. And the Caltech administration has been very good in pushing this telescope. Everybody does things for their own self-interest, and I think Caltech sees more glory in this telescope than UC does. UC is so much bigger, and it's just one nice thing they're doing. But to Caltech, this is really a major thing. They care about it in ways where I see UC being kind of indifferent sometimes. Caltech has always been there to make sure that nobody slacks off and screws things up. Ed [Edward C.] Stone [Morrisroe Professor of Physics] was chairman of CARA for a long time [1987-1990]; he really minded the store carefully. And Caltech made him—what was it?—vice president for astronomical facilities [1988-1990]; his job was monitoring this telescope project, basically. And he's done a real good job. He's a tough guy, too. I find him a little hard to get along with, but I have a lot of respect for him. He definitely wants the project to succeed and puts time into it. So I think Caltech has done an excellent job in those senses. Has shown, I said, more dedication to it than UC. Even Bill Frazer, who did an awful lot of work on it over the years, he's been busy trying to manage Lawrence Livermore National Laboratory. He's in charge of that. You know, at Berkeley you hear all about that: "Let's boot those national labs out of the system." Well, Bill Frazer is always dealing with Los Alamos and Livermore—that's a huge job. And now he's chairman of the Keck board of directors—that's a piddling job.  I mean, he doesn't spend forty hours a week on Keck. [Laughter]

MOY:  Well, it must be very satisfying to be looking through it now.

NELSON:  Yeah.  Oh, we still have millions of problems, but they're getting solved one by one, and I think it's going to be a great success.  I'm heartened by the progress we're making, and I figure another year from now and we'll say, "Hey, this telescope works."

MOY:  Yes.  Let's build the next one.

NELSON:  Well, we've already started the next one.  Since it's just a copy of this one, I don't think I'll have to have very much to do with it.  I'll be involved, but my expectation is I'll be in Hawaii another year and then I'll go back to Berkeley.

MOY:  And then what?

NELSON:  Go back to being a professor of astronomy.

MOY:  Relax for a while?

NELSON:  [Laughter]  I don't know what I'm going to do.  People ask me that.  I feel like I'm supposed to know, but I don't.