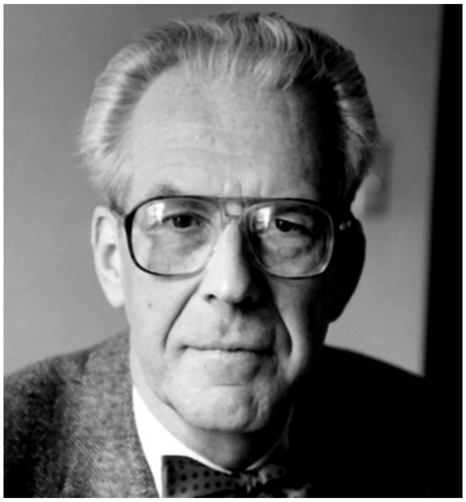

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MAARTEN SCHMIDT
(1929 –)

INTERVIEWED BY
SHIRLEY K. COHEN

April 11 and May 2 & 15, 1996

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Astronomy

Abstract

An interview in three sessions in April and May of 1996 with Maarten Schmidt, Francis L. Moseley Professor of Astronomy, emeritus, in the Division of Physics, Mathematics, and Astronomy. He recalls growing up in Groningen, Holland, during German occupation in World War II; his early education and friendship with Jan Borgman, with whom he built a telescope; photographing the solar eclipse of July 9, 1945. Matriculation at Groningen University in 1946. At an astronomy conference in 1949, Jan Oort asks him to become an assistant at Leiden Observatory. Graduate study at Leiden, where he works with Oort on the brightness of comets. Recalls his time in Kenya, August 1950 to December 1951, making measurements of declination on the equator with G. van Herk. Comments on 1951 discovery of 21-centimeter line and his radio observations of galactic structure with Oort and Henk van de Hulst. PhD from Leiden in 1956; thesis on the distribution of mass in Milky Way galaxy. Comes to Mount Wilson Observatory on a two-year Carnegie Fellowship. Returns to Leiden in 1958; back to Pasadena a year later, as an associate professor at Caltech, where he works in early 1960s on exchange between stars and galactic gas, and on size, mass distribution and rotation of Milky Way galaxy. At Palomar in early 1960s—working with radio astronomer Tom Matthews, who was at Owens Valley—he

takes spectra of optical objects identified with radio sources, which leads to the discovery of quasars. Recalls quasar work and contributions of Jesse Greenstein, John Bolton, J. Beverly Oke, Allan Sandage, Cyril Hazard, and later Richard Green, James Gunn, and Donald Schneider. Recalls early arguments by Halton Arp, Fred Hoyle, Geoffrey Burbidge that quasars were not cosmological objects. Recalls use of CCDs in 1980s-1990s and the discovery in 1993 of a quasar with a redshift of 4.9, largest redshift on record. Comments on his work in X-ray astronomy and gamma-ray astronomy, with ROSAT [Röntgen X-ray Satellite] and the Compton Gamma-Ray Observatory [GRO]. Recalls his graduate students, among them Nobel laureate Robert W. Wilson (co-discoverer of cosmic microwave background). Discusses his administrative career at Caltech, 1972-1980: three years as executive officer for astronomy, three years as PMA division chairman, two years as director of the Hale Observatories. Comments on the concurrent deterioration of relations between Caltech and the Carnegie Institution. Recalls his presidency of the American Astronomical Society, 1984-1986, and his work on behalf of VLBA [Very Large Baseline Array] of radio telescopes and National Science Foundation's astronomy budget. Concludes with a discussion of his chairmanship of AURA [Association of Universities for Research in Astronomy] board, 1992-1995.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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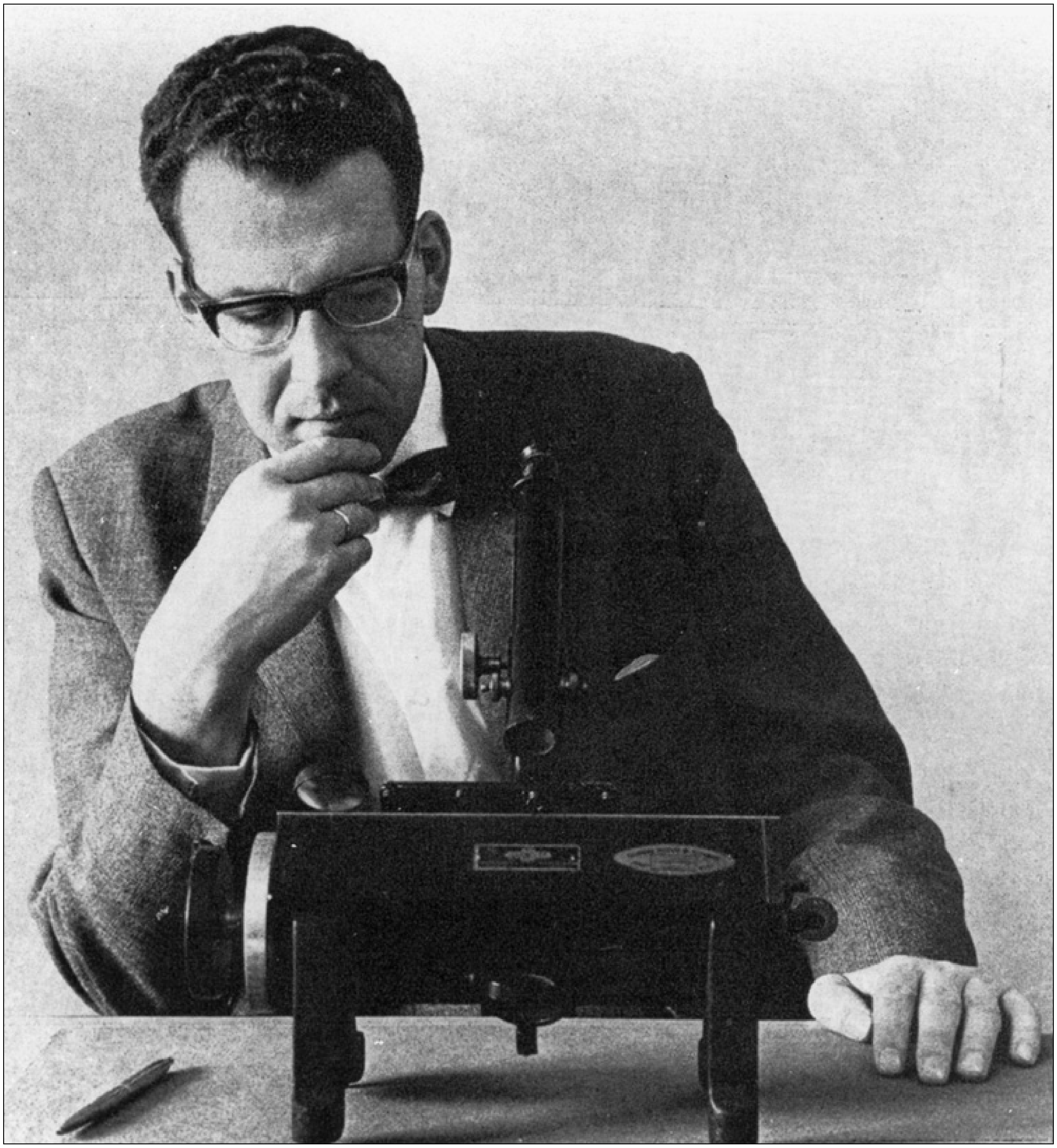

Maarten Schmidt viewing photographic spectra in 1965. He had just published his breakthrough discovery of five quasars at the farthest reaches of the known universe.
Photo by James McClanahan in *Engineering and Science*, Vol. XXVIII, No. 8, May 1965.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH MAARTEN SCHMIDT

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

TABLE OF CONTENTS

INTERVIEW WITH MAARTEN SCHMIDT

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Maarten Schmidt **by Shirley K. Cohen**

Pasadena, California

Session 1	April 11, 1996
Session 2	May 2, 1996
Session 3	May 15, 1996

Begin Tape 1, Side 1

COHEN: I'd like you to start out with your childhood—something about your parents, whatever you'd like to tell us.

SCHMIDT: Yes. Well, I was born in Groningen, Holland, in 1929. My father was a government accountant. I had one brother, four years older, who is still alive. He's in Holland—he lives in Groningen, it happens. I had a very small family: one aunt, who was my mother's sister, and one uncle, my father's brother. His name was Dik Schmidt, and he was a pharmacist by occupation, but he was an amateur astronomer. And he had quite an influence on my life, as is obvious from my career. My grandparents all lived in the northern part of Holland, in Berkhout. On my mother's side they were farmers; on my father's side, house painters. My grandfather on my father's side was, besides a house painter, also quite a philosopher. So in the family there were always going on discussions of some depth—philosophical discussions—between my grandfather and my uncle and my father, and then later on, when I grew up, probably myself. So the atmosphere was quite positive. I went early to elementary school. I was four and three-quarters years old. Six years later, I went to the secondary school, the HBS—Hogere Burger School. My brother had gone to the *Gymnasium*, which of course had Latin and Greek. And since my parents felt that the two of us had to go in different directions, I was sent to the HBS, which was more the sciences.

COHEN: What I would ask you now, since we're getting to 1940, 1941, how did the war affect all this and your family?

SCHMIDT: A lot, of course. During the war, I was between the ages of ten and fifteen, which at least meant that I was not subject to all sorts of measures the Germans took regarding men of the age of eighteen to forty-five or so. At some stage in the war, around '43 or so, the Germans—who, of course, occupied Holland—came out with a decree that every male between eighteen and forty-five should work for the war effort in Germany.

COHEN: In Germany, and not in Holland?

SCHMIDT: First in Holland, with the OT—Organization Todt—and later on in Germany itself. And this led to an extraordinary situation for most people. I was free of that, because the war ended before I became eighteen. My brother and my father were, of course, subject to that. My brother, in particular, was the wrong age. And what you could do, unless you did go and work for the Germans, was to try and get a medical certificate from a doctor that said you were unfit to work. And these certificates, of course, abounded in Holland. My brother had one. But he was caught.

COHEN: Was this something bought and fabricated?

SCHMIDT: Oh, yes, yes. The Dutch Underground supplied these. Actually, I think that my brother had been to a doctor, but the doctor had surely overstated the case. So my brother was caught, and he was sent to a local camp. I think this was around Christmas 1944. We feared that at any time he would be sent to Germany. And somehow—by a combination of two things, as far as I know—he got out. One is that he managed to feign heart trouble. He was somewhat an emotional character, and he made good use of it under the circumstances. In the second place, I still believe that my parents, through a neighbor who was in the Underground, managed somehow to bribe somebody. Anyhow, it was a combination of things.

So after Christmas he suddenly came out, and everything was OK. But, of course, the situation all around us at that time had become a desperate one, where you essentially saw no men walking on the streets. They would be picked up, so they were either underground—that is, either, as they call it in Dutch, which after all has a big history with the sea, "under water," and most of them did that—or they had these false papers. And that was only one aspect of what happened in Holland. The repression of the regime became stronger and stronger as the war went on, and the Underground response in Holland became stronger and stronger. There were attacks on Germans, which were answered in major ways. In one case, a whole village—all the men were taken to Germany in one night. And I'm not so sure they weren't shot.

Although it all sounds very bad—and of course it was—on the other hand, as a young person you tended not to take it too tragically. It was sort of exciting. And I remember that in the middle of all this going on, when we heard rumors of Germans being shot at or people who had been taken out at night—I thought to myself for one fleeting moment, I wonder how it would be if there was no war. Wouldn't it be sort of dull? [Laughter]

COHEN: Now, you continued in school; you went every day to school?

SCHMIDT: I went every day to school. All this went on, and one went to school. Life in Holland was—well, there was this repression, and the attempts to get males to work in Germany—but it was still perhaps tolerable to some degree.

COHEN: You had enough to eat?

SCHMIDT: Yes. And then came September, 1944, which of course in this country is best characterized by *A Bridge Too Far*. The Allies tried to advance in the middle of September of '44 from northern France, through Belgium and into southern Holland, near Arnhem. And there were three bridges to go. And they got two, and finally at the third one they failed. This was the Battle of Arnhem.

Now, while all this was happening, the Dutch population tried to help the Allied effort as much as possible. And in particular, all the railroad workers went on strike a few days before it started. The Germans took this very badly and declared that all the railroad workers who could be found would be shot, so the railroad workers went underground. Now, this had a countrywide, major consequence that I'll take first, and then there was a personal, minor consequence that changed things for me a little bit.

The countrywide consequence was that once the battle was over, the Germans said, "If you are not interested in transportation"—because the railroad workers had to stay underground, since they were persona non grata—the Germans said, "If you feel that way, then we will not take care of transportation anymore." And they took up all the rails and used them in the war effort in Germany. And it's amazing; Holland is about 300 kilometers north-south and half as much east-west—so small that you would think the grain from the farmers could easily get into the big cities. But this turns out not to be the case under the circumstances, and a famine started in the winter of '44-'45. I don't know the total casualties—it was somewhere between 50,000 and 100,000. People went out on bicycles and took their precious things along—like gold, and sheets—and bartered with the farmers to get wheat. So that was a period, especially in the central part of Holland—Amsterdam, Rotterdam, The Hague—that was exceedingly critical. So critical that when the war came to an end, around the early days of May 1945, the first attention of the Allies was to get food in there. I was living in Groningen, in the north, where we were slightly better off. But the Swedes came in with white-painted aircraft and dropped food on soccer fields to try and ease things as soon as possible.

In my own case, I used to walk to school—it was about a twenty-five-minute walk—with a young man who was the son of a railroad worker. Therefore, in September, 1944, when he went underground with his family, I had nobody to walk with. I was in the fourth grade of the HBS, and under these emergency circumstances I finally conceded and walked to school with a second-grade young man, who happened to be Jan Borgman, who later on became an astronomer in Holland and a major academic administrator. He was an amateur astronomer at the time, and I already had started to become an amateur astronomer under the influence of my uncle—perhaps I should go back to that later. Anyway, that got me in contact with Jan Borgman. And believe it or not, in these circumstances, where you couldn't find tires for your bicycle, where there was no butter, no sugar, essentially no meat—there was a very small amount of bread, but all rationed—we managed to build telescopes under those circumstances. [Laughter] It was almost perhaps an outlet. Anyhow, it was very nice. He was very good.

COHEN: So the two of you worked together.

SCHMIDT: We worked together, yes. Mostly, he assisted me, because he was very good with his hands and could do things in a real metal shop, and I couldn't. And we both were grinding mirrors. Amazing!

COHEN: First things first!

SCHMIDT: First things first! There was an interest there that was deep. And we had lots of time for that, because just around the time of the Battle of Arnhem, the Germans had fortified troops in Holland and had occupied the school where I was studying. We went part-time to another school, and then they needed that one, too. And finally, all we had to do was go there once a week and get our homework and turn in our old homework, and we did schoolwork just by assignments until the end of the war. So from early November of '44 until probably May, 1945, I didn't go to school.

COHEN: But still, you lived in your own home with your own parents.

SCHMIDT: Yes, so our family was very lucky under the circumstances.

COHEN: You say you have so many people going underground, or "under water." Where did they all go?

SCHMIDT: Farms mostly, outside the cities. Although, of course, you can also go underground in a city—like Anne Frank, in the middle of Amsterdam. Anyhow, people were inventive.

COHEN: You wanted to talk about your uncle?

SCHMIDT: Yes, because I had astronomy as a hobby, and that essentially started in 1942, while the situation with the war wasn't so bad yet. In the summer, my brother and I used to go for extended periods to my grandparents—the farmer, and then the house painter—in Berkhout, north of Amsterdam. And during that particular summer, I rummaged around my paternal grandfather's stuff, and I found a big lens about four inches across, in the middle almost an inch thick. And you know, of course, that when you have a lens you can make an image. And I did that. And I asked him whether I could have the lens, and he said I could.

So when I came home from this vacation, I made my first telescope. I had the lens, and then I had the cardboard cylinder from a roll of toilet paper, and at the end I mounted a piece that I could move, and by Jove, it worked!

COHEN: So at thirteen years old, you built a telescope?

SCHMIDT: Yes—twelve years old. And I had during that summer also been with my uncle, and we had looked out the roof with his telescope. He was a fairly active amateur astronomer.

COHEN: You had very dark skies, of course.

SCHMIDT: Yes, that was an advantage. It became better and better. So what happened was I wrote my uncle and said, "Well, I have this small telescope now. I built it myself, and it works. What can I do with it?" He said, "Why don't you look up at that star to see whether you can see its double, because that's the test of the quality of the telescope." So I had to go to the library in order to find a book, in order to find that star he had mentioned, and the constellation, et cetera, and that started things. I got several books and started to read. And at that time—certainly not during the war—you didn't copy, you didn't Xerox, you did nothing of the kind. These things didn't exist. And I copied by hand considerable parts of one of these books. [Laughter] So that started my interest in astronomy. And then it really took off when Jan Borgman and I, near the end of the war, started to build telescopes and then had somewhat larger instruments.

COHEN: Now, you didn't study this in school. This was strictly outside of school?

SCHMIDT: Well, there was at school one subject that was given in the last grade—the fifth grade—of the HBS. It was given by the director of the school, and he and I got once in a while into a conflict with each other, because I knew so much about astronomy. The field was called cosmography, and it was absolutely terrible. It had to do with the circles in the sky, the setting and rising of objects. It was the dullest that you could think of. But even then, I once in a while managed to correct him, and he didn't take that very well. [Laughter] And I got my comeuppance, because, just after the war, on the 9th of July, 1945, there was a solar eclipse. The sun was eclipsed about seventy percent, I think, in Holland. So Jan Borgman and I had extensively prepared ourselves for that. We had cameras mounted on our telescopes so that we could take pictures. At that time, just after the war, we were in class only in the mornings, because the schools had to be shared, because of buildings that were destroyed. And I asked the director whether I could have that morning off, because the eclipse was at something like two o'clock in the afternoon. He said no. [Laughter]

COHEN: You didn't take the morning off?

SCHMIDT: I didn't take the morning off.

COHEN: But did you get the eclipse, then?

SCHMIDT: We got the eclipse. And we got beautiful pictures. And we measured them for a long time. And then together with other amateurs in Holland, we had discussions and meetings and correspondence. It was great fun.

COHEN: So you then finished up the school.

SCHMIDT: That was about that. Then in '46 I went as an undergraduate to Groningen University. I still lived at home. In some fashion that by now has escaped me, I did it in three years. Because I know from my history that in '49 I moved to Leiden. I always thought my time at Groningen was four years until I looked it up about twenty years ago. And I thought, Hey, there's a year missing! So that took until 1949, and there is little to report about that period.

COHEN: Your family took up what they had done before the war?

SCHMIDT: Yes. My father, who was a government accountant, began looking into the business deals that businessmen in Holland had been making during the German occupation, and in particular going after those who had profited from the situation. This was a major effort by the government, and he headed the investigation in the northern part of Holland. For a while, he even had a set of detectives to go and confiscate books and so on from businesses. And many times he had to testify in court about these cases. After about three or four years, that work was done, and he went back to his normal accountancy work.

I had attended, every summer since 1946, a conference of astronomers in Holland, which was held countrywide—all astronomers, all their students, everybody involved in astronomy. Since the country isn't very large, it wasn't an excessively large conference. It was at a nice place somewhere north of Arnhem. And it was at the '49 conference that Jan Oort, who was at Leiden, asked me to become an assistant at Leiden Observatory. I had known him from earlier conferences, and at that time he took me on a walk and asked if I would do that.

So on July 1, 1949, I was appointed at Leiden Observatory as an assistant.

COHEN: That's very interesting, the summer camp. Maybe that explains why there are so many Dutch astronomers.

SCHMIDT: Well, yes.

COHEN: It's a national institution?

SCHMIDT: Yes.

COHEN: Was it sponsored by the government?

SCHMIDT: No, the astronomers did it themselves. They had what they called the Nederlandse Astronomenclub, and it organized the conference every year.

COHEN: And that still goes on?

SCHMIDT: Yes. Yes, it's very healthy and very good. Of course, at the moment it's a huge group, because space science has come in and the numbers have increased.

So Jan Oort asked me to be his assistant. I became his assistant on July 1, but it was a beginning with major interruptions. Since I had already as a student signed up for a big student vacation tour in southern France, I asked his permission to go ahead with that. So actually I appeared only on the 27th of July at Leiden. And then I worked there for only on the order of six weeks, because in early September 1949 I was called into the military service. I had had legal delays, but once I got my BSc degree, I had to serve.

So I went into the army, and I was stationed south of Zwolle. And in some chancey way that had nothing to do with me, I got out after ten weeks.

COHEN: You mean your whole army service was ten weeks?

SCHMIDT: Yes. [Laughter] Amazing, wasn't it? At first, it looked as if we would have to go to the Dutch East Indies, because there was a conflict brewing there, as you know. But that was called off, fortunately. And after the six weeks of basic training, when the time came to assign all of us to particular details, they were left with a lot of people they didn't know what to do with. So they kept us around. I would write tickets for weekend passes for soldiers to use on the railroad. And after a month, they sent eleven of us home. Pure luck!

So I came back to Leiden in November, and my job was gone. But I became an assistant again—*buiten bezwaar van 's Rijks schatkist*, which is an incredible term that means I wasn't paid. [Laughter] It means literally that I wasn't a burden on the government's treasury. [Laughter] So I worked there for nothing, for a whole year, until the next year, and then I got paid.

COHEN: Your parents supported you, then?

SCHMIDT: Yes. And I should tell you that the pay for an assistant there was insufficient to live on. There were two reasons for that. One was that the pay was simply absolutely very low, but there was another reason that was curious: The assistantships at Leiden Observatory were so-called two-third assistantships. What had happened was that under the previous director, [Ejnar] Hertzsprung, of the Hertzsprung-Russell diagram—he was a very famous Danish astronomer, and he was director of the observatory through the war—there had been only two assistants, but he always needed more help. So he went to the university administration and asked for a third, and they said they couldn't do it. So he decided to take the pay of the two assistants and appoint three for that, which somehow happened. Another thing that also was remarkably unfair, but nobody seemed to mind, was that the three weeks' vacation that each of the two got was changed into a two-week vacation for each of the three. [Laughter] So I had two weeks' vacation. But the pay was so low; it was 110 guilders per month, probably about $30 per month. And I had to pay for living at the observatory. So my father had to support me, yes. I remember that he supported me while I was a paid assistant by half the amount I got as an assistant—55 guilders per month. So I had 165.

COHEN: Were you a graduate student?

SCHMIDT: I was a graduate student, yes, and I was starting to take courses. I was a little late, of course, having come in November, and I was trying to catch up. I started to work on research also; it's customary there to do that very early. I did it for Jan Oort. And in fact he put me to work on studying the brightness of comets, because he had just launched what later on became known as the concept of the Oort cloud of comets around the solar system, which he hypothesized consisted of on the order of a billion comets that all belong to the solar system, travel with it through space. And the cloud of these comets is so large that it reaches about halfway to the next star.

Comets come in, and when they come close enough to the sun in the interior solar system, like the one we're seeing at the moment [Ed. Note: comet C/Hyakutake], you can see them. But most of them you don't see. And in order to explain things about the orbits and so on, Oort put me to work on the brightness: How the brightness changes when the comet comes in.

I did a study, worked hard, and within a couple of months I found—and we finally published together—that the comets that come for the first time out of this cloud into the inner part of the solar system have a different behavior of the brightness. Their brightness increases slower than that of comets that have been around many times, like Halley and Encke, and so on.

Just to put in an aside, if I may: This later on explained to Jan Oort and to me—separately, because I lived by that time here and he lived in Holland—why comet Kohoutek put on such a poor performance. This was a comet that had never been here before, and therefore its brightness increased very slowly, and NASA overestimated the brightness at maximum by a factor of, I think, 100 or so. At the time of Kohoutek, I was totally out of comets; this was around 1974. I thought, What do I do? I'm not going to go public and say that all these NASA predictions are just wrong. And I also thought, Well, if I do that, of course, the comet will be very bright, and I will kick myself. [Laughter] But I thought I would at least talk to the astronauts—because an Apollo mission was postponed in order to see comet Kohoutek. So I talked to the astronauts' office and I said, "If you're counting on that comet to be so bright, I wouldn't, because it probably won't be." I thought I'd do a national duty, sort of. The funny thing is that Oort and I had no contact about that. And later on I found, or somebody sent me, a newspaper article in Dutch, in which Oort was interviewed. And he had more courage. It was before the comet really came close to the sun, and he said in the interview, "I think that they are overdoing the predictions about its brightness. A long time ago, twenty-five years ago, together with Maarten Schmidt, who is now at Caltech, we worked out that the brightness," et cetera. And he told the whole story. [Laughter] So he and I separately had our reactions to this.

COHEN: But you did not go on with your interest in comets after this initial work?

SCHMIDT: No, no. I did that for several more months, but there was yet another interruption to come. Between Christmas 1949 and New Year's, Oort called me into his office and asked me if I would go to Kenya to work for the observatory.

COHEN: Did the Dutch people have an observatory in Kenya?

SCHMIDT: Yes, and in August 1950 I went to Kenya, and I worked there until December 1951. So for fifteen months or so, I was out of the country, and I was obviously not active as a graduate student at all. So it was a major interruption.

COHEN: Why did he ask you to do this?

SCHMIDT: Let me explain. The interest had to do with measuring positions of stars, and in particular the position, the coordinate, of stars in the direction of latitude, which in astronomy is called declination. This is rather difficult to measure, and in astronomy there is a need to know these positions—in particular, their changes, how things move. They move very slowly, all these stars, but their motions are of major interest in understanding the structure of the galaxy, for understanding our solar system, for everything. And it turned out that if you looked at the measurements of declination made by different observatories for stars high up in the sky—where they could do it best—there were funny jumps from one zone, done by one observatory, to the next zone. And things didn't fit well together.

So in the middle thirties Hertzsprung got the idea that you could do these observations differently. For that, you needed to be on the equator of the Earth. If you're on the equator, it turns out that when stars are rising and setting, their place along the horizon, which is called azimuth, is just a reflection of their declination. And now it becomes a horizontal angle—unlike the normal vertical angle—and there were major advantages to measuring that way. So for about a year or so, somebody went to Africa and did some observations, and things looked good.

COHEN: Now, the observatory was already there?

SCHMIDT: There was no permanent observatory, but I really only know the story of what happened after the war. It was decided at Leiden that in 1947 the astronomer G. van Herk would go out and do this again, but do it properly. And this all had to be subsidized by the government. As part of the subsidy from the government, a navy lieutenant was detached to the observatory and worked with Van Herk for three years in Africa.

Now, in 1950 that man had to go back to Den Helder, where he was stationed; the government said, "This is it, three years is it." But everything was by now set up, and therefore there wasn't a need for somebody from the navy. So I think Oort just wanted to have an assistant for Van Herk. And he asked me to do it.

COHEN: Now, Kenya at that time was an English colony?

SCHMIDT: It was a protectorate, yes. So after only one year of classes, barely, in August, 1950, I traveled by ship to Mombasa and took the train to Nairobi. And there I met the Van Herks, who collected me. And the first thing Mrs. Van Herk said when she saw me get out of the train was "Oh, he looks so clean!" We were there under fairly primitive circumstances, and I never looked that clean again.

There I worked with Van Herk on this job. Every clear night, we would open the observatory, which meant riding away a small house on rails, put the little house to one side. Then on a pedestal with a horizontal circle, there was a telescope that looked at an altitude of only seven degrees. So you were looking only at stars near rising and setting—seven degrees above the horizon. The idea was that when a star was rising—a star of interest—you'd turn the telescope that way and do a number of measurements, and this was all photographed with cameras. You got film that had to be developed, and the measurements could be done later of all these positions. And that's how you worked hard.

COHEN: For fifteen months you did this?

SCHMIDT: Yes.

COHEN: That must have gotten a little tedious?

SCHMIDT: No, it was sort of interesting. No, it was not tedious. Van Herk and I would share the night. Sometimes I would take the first half, and sometimes I would take the second half. And when the weather was not clear, which of course happened quite a bit in Kenya, we played bridge with a fourth person there—an Amsterdam cosmic-ray physicist who was only a little older than I was. I've never played so much bridge in my life.

The site was really remarkable. It was more than 9,000 feet high, in the highlands of Kenya, way inland. It was halfway between Nairobi and Lake Victoria, along the road from Nakuru to Eldoret. It was very beautiful. At sunset we could often see Lake Victoria in reflection down there. At sunrise, I could see Mt. Kenya, which is way above Nairobi, hundreds of miles north. Very beautiful!

I think we had a total staff there of fifteen or so native people. They were all Kikuyus. [Tape ends]

Begin Tape 1, Side 2

COHEN: I believe you were speaking about the cook?

SCHMIDT: Yes, we were speaking of the cook. He was the only one who was not a Kikuyu. We left around November, 1951, and the situation got indeed very bad soon thereafter, in February, 1952. The Mau Mau rose up, which was a sort of activist, if not terrorist, part of the Kikuyu tribe. So if we had stayed a half a year later, I don't know what would have happened to us. We would have taken some extraordinary measures, but as it was there was no trouble at all.

Very interesting life! I learned Swahili—upland Swahili you call it, because it's broken and it's not grammatically entirely correct. I managed to speak it reasonably well.

In February, 1951, after I'd been there for a half year, I overturned a truck. It was on a Sunday morning, and we were going to our neighbors for breakfast. We had neighbors at typical distances of one or two miles, and they were either farmers or, in this case, it was a wood business.

COHEN: Were these English people mostly?

SCHMIDT: Mostly English people, yes. We were on the access road, and there must have been a log that I missed. The sun was still fairly low, and there were long shadows over the road. And Jan Strackee, who was sitting next to me while I was driving this truck, said, "Hey!" and at that moment it had already happened. The whole thing turned over. It was a huge truck, a Canadian Army telephone truck, a big square thing. It fell over. I was sitting on the righthand side—because this, after all, was under British rule—and in the commotion as the whole thing fell over I must have tried to support myself, and therefore apparently stuck my leg out a window. And when everything came down, my leg was under it. My ankle was broken. It took about half an hour to find people from the wood business, natives who could lift the car up.

COHEN: So you were pinned under this truck?

SCHMIDT: Yes, with my leg. Pretty unpleasant. I'm not even sure it was broken at first, but it surely was broken by the time I arrived at the hospital. What happened was that these natives are trained to carry heavy things, so there was a foreman type who said, "OK, let's try it out." And they all lifted the truck up, and then dropped it. [Laughter] At which I pleaded with him to not do it that way. And then I was taken to hospital in Nakuru, stayed for twenty-three days in the hospital, was operated on by a good man, fortunately—three times. And I can walk, as you know. It's amazing—and remarkable for Central Africa.

This destroyed my plan to climb Kilimanjaro, which would have been fantastic. It would have cost me a little over 100 shilling, for five days. And you would have gotten four helpers along, and an animal—amazing, for 100 shilling, even at that time!

So as soon as possible I started observing again, although I hobbled badly. And just to add a couple of things: Once, during my observing, I had the scare of my life, probably. The place was not entirely undangerous. You know, we were in the middle of Africa. What happened was that the telescope was surrounded by a small fence. You were outside, all by yourself, in the dark. So I'm working hard, and I'm looking through the telescope, and I hear something galloping towards me through the high grass. And of course, I freeze. So, after I recover, I take my flashlight and start to walk around, looking outside the fence. Couldn't find anything. I still don't know what it was. There were leopards in the neighborhood, by the way, that we always watched for carefully when we walked under trees everywhere, because they liked to sleep in them. I was sure I was being attacked, and I'm sure the observations afterwards were fairly poor, you know, and there were a few missing.

But there were also nice moments. Like at Christmas, when I was working: It was clear, and here come twenty natives. I'd already heard them singing. Apparently they walked by farms, and they would sing Christmas carols, and then they would get money. And believe it or not, here they come. And they come through the door in my fence, and they stand there and they start to carol me while I'm doing all these observations. [Laughter] So I managed to find a brief moment and I talked with them. And I happened to have some money with me, thank God! But that was not the issue: that you were there, under that dark African sky—that was very nice!

The other major activity that you had to do was to maintain the road. We were about two miles from the main road from Nairobi to Kampala, which in our area was very poor—a dirt road. And then the road up our hill, which was called Timboroa Hill, was exceedingly poor. So in preparation for departure, in August or September, 1951, I worked a whole month on the road, with natives, going to a stone quarry, getting stone, pieces of rock, putting them on the road. So finally, in late November 1951, we abandoned camp. The Van Herks traveled one way, and I traveled via Lake Victoria, Tanzania, Rwanda, Burundi, into the Congo, as it was called at that time. I traveled for five days upstream on a paddle boat on the Congo. And then it was a hundred hours by train to Capetown. And I made several stops, among them Victoria Falls and

Johannesburg. The trip took a long time, more than a month. And then I took the boat home.

That takes us back to Leiden. Around the first of January, 1952, I finally started classes again. And it took me, after that, four and a half years to get my degree.

COHEN: In some sense, you were really just starting, then.

SCHMIDT: I was essentially starting, yes. I'd done a little research on the corona of the sun earlier, and then on some variables—and then with Oort on the comets.

COHEN: What happened to the observatory in Kenya? Did that continue?

SCHMIDT: No, it was abandoned. It was, as it were, portable, practically. Van Herk has been back to see it, and I don't know whether there's anything more than some pillar or pedestal left.

Let me perhaps say a little bit about the research I did while I was a student at Leiden.

COHEN: Were you still working with Oort, or had you gone with someone else?

SCHMIDT: No, it was all essentially with Oort. What had happened while I was away was something very important for astronomy as a whole—namely, in popular terms, the twenty-one-centimeter line had been discovered. It was discovered in 1951, in rapid succession, at Harvard and by the Dutch and the Australians, and they all published together in *Nature*. And a whole industry started, of observing these lines in various directions—because now you've got a lot of information about the structure of our galaxy which until that time had not been accessible in any way. Because twenty-one-centimeter radio waves are not absorbed by dust, you could see through the whole galaxy, in a transparent fashion that was totally new.

So I soon started to partake in the observations there. And that was essentially guided by both Oort and Henk van de Hulst. It was a very active program, in which many people worked, and the part I was given to do was the interior part of the galaxy, closer to the center than the sun is, and of course on the northern side, because the southern side was being done by the Australians; we couldn't see that. And Westerhout got us to do the exterior part of the galaxy. That led to publication in 1957 of major articles in which we came out with the first spiral structure.

COHEN: What telescopes did you use to do this?

SCHMIDT: We used a telescope that had been left by the Germans and used by them for radar defense. It was a so-called 7.5-meter Würzburg antenna—the dish was 7.5 meters. It was equipped, and of course taken from the coast, where it probably had been found. That was at Kootwijk Radio, that's where the observatory was.

So that led to major publications, and it could have been my thesis work. But in the meantime Oort had given me a different thesis project, which was the distribution of mass in the galaxy, and that tied in very closely to the radio work but was a major extension theoretically.

COHEN: That saved you from being a radio astronomer.

SCHMIDT: Yes. Although when I graduated I was pretty close to being a radio astronomer, really, because the twenty-one-centimeter work was extensive. All that work went on more or less in parallel. And that led finally to my PhD defense in 1956.

COHEN: Were you married by then?

SCHMIDT: Corrie and I got married in 1955, September. So by that time we were married, and we stayed in the foreign astronomers' apartment, which was next to Oort's house, on the observatory grounds, which was very nice.

So I finished up and got my degree in the spring of '56. And then I got a Carnegie Fellowship, which brought me for the first time to this country.

COHEN: Now, was that ordinary for somebody from Holland, to come to the Carnegie Institution here?

SCHMIDT: No, that was not so ordinary. That was, of course, the time when everything was done by the old boys' network. I had met [Carnegie astronomer Walter] Baade from Mt. Wilson at Leiden during his visit over there in 1953, and I had had quite a few contacts with him around that time, both socially and scientifically. So he knew me well. And essentially what Oort did, by the time my thesis got near completion, is that he wrote Baade, and that was it. [Laughter] So I came to Pasadena as a Carnegie Fellow. I was a Carnegie Fellow for two years, and then went back to Holland in ’58.

COHEN: Now, Corrie, of course, came here with you.

SCHMIDT: She came with me. And that’s where our first child was born—Elizabeth.

COHEN: Where did you live here in Pasadena?

SCHMIDT: 1404 North Los Robles.

COHEN: Close to the observatory?

SCHMIDT: Fairly close, yes. Upon arrival, we bought a second-hand car in Washington, D.C., stayed at DTM [Department of Terrestrial Magnetism]—that’s the Carnegie division there—with Merle Tuve. Traveled all across the country, stayed with the Boks in Cambridge—Bart and Priscilla Bok. Visited Leo Goldberg in Michigan. Went to Yerkes [Ed. note: Yerkes Observatory, at Williams Bay, Wisconsin] and stayed there for a while. Adriaan Blaauw, a Dutch astronomer, was there for an extended period. He may even have worked there at that time. We stayed there for about a week, and met everybody there—[William W.] Morgan and [Bengt] Stromgren, and many others, of course.

COHEN: So astronomy was very democratic in those days.

SCHMIDT: Oh, yes, yes. It was very nice. I was young, but people everywhere were very friendly. I gave colloquia at most places, and we finally arrived here in July, I think, in ’56.

MAARTEN SCHMIDT

SESSION 2

May 2, 1996

Begin Tape 2, Side 1

COHEN: We have you driving across the country to take up your Carnegie Fellowship. You might tell us about your understanding of this position.

SCHMIDT: Yes. As I mentioned before, it simply came about because Jan Oort had written to Walter Baade at Mt. Wilson. And that was all that was necessary at the time to get you a fellowship. [Laughter] Times have changed.

As described, we came here in ’56. And we stayed for approximately two years. And I did observing at Mt. Wilson.

COHEN: And you were working with just the people at Carnegie. Did you have anything to do with the people at Caltech at all?

SCHMIDT: Yes, of course. There were the lunches, and the colloquia. There was a lot of contact. I did observing at Mt. Wilson with the 60- and 100-inch, and also at Palomar with the 48-inch Schmidt. But the 200-inch at Palomar was not accessible to fellows of any kind at that time. It was just for professorial staff and for Santa Barbara Street [Ed. note: Carnegie Institution offices] permanent staff.

My fellowship lasted until the spring of ’58, when we went back to Holland. That was the understanding—here for two years, and then I would go back.

Jesse Greenstein made me an offer. He called me in, and I even saw [Robert] Bacher with him, the [Physics, Mathematics, and Astronomy] division chairman. And they offered me a position at Caltech, but at that time I said I couldn’t do it. I was committed to go back to Holland. So I did.

And conditions actually were not all that good in Holland somehow. It was a culture shock, going back. My father found us an apartment to live in. Housing was still very short in

Holland, and beyond my means.

COHEN: You were in Leiden?

SCHMIDT: In Leiden, yes, and I had a position as some type of scientific officer—or perhaps even an adjunct scientific officer. But Jan Oort somehow was not very aggressive in promoting me, so we were losing money per year. We were going backward. We had a second child, and we were going backward financially.

And then there came this—I asked Jesse about it last night—there came this strange letter from [Rudolph] Minkowski, who was at Carnegie, at Santa Barbara Street. And he said that he'd heard from [Ed. note: Swedish astronomer Bertil] Lindblad that I was considering going back to Caltech after all. I think it was a made-up story, actually. So after a while, I wrote back to Minkowski, saying that I couldn't remember talking to Lindblad about coming back to Caltech, but that actually I had given it some thought. At which, Jesse started again; and it eventually came to an agreement.

So in October 1959 we came back permanently.

COHEN: Now, when all these things were going on, Oort did not try to encourage you to stay?

SCHMIDT: Yes, yes, he did try to encourage me to stay. And he, in fact, came up with one or two positions. But by that time I was bound by my word to Caltech, so that didn't work. And he took it in good grace, but he did his best.

So I became associate professor here in 1959, and we lived in a house in Altadena. And that started my career at Caltech, which, in terms of active participation, lasted until the first of January, 1996. The day after I arrived, I began teaching. So things immediately became very active.

Perhaps I should describe the type of scientific work I've done at Caltech. I will be fairly brief about the various categories, but I will be somewhat lengthy about quasars, because that started something that was of prime interest.

When I came back, the work I continued was something I had started already as a Carnegie Fellow here. And it was theoretical—it was to look at the effects of star formation on galaxies. When a galaxy forms, it's one big ball of gas, and stars form out of it. And when these stars age and die, they throw much of the gas out into the galaxy again, and then new stars are formed from that. So there is an exchange between the mass going into stars and then eventually back into the gas in the galaxy, from which new stars are formed.

I had decided as a Carnegie Fellow to do some of the bookkeeping on that, and it led to some interesting considerations. I continued that work when I came here, and even some of the observational work I did was aimed at that.

COHEN: Was this an interest of any specific person here at that time?

SCHMIDT: No, this work actually began in Holland, where it had been shown that many galaxies, including our own, had a substantial amount of their mass in gas—on the order of ten or twenty percent or so. And it was Sidney van den Bergh who, in fact, called me once—or at some meeting we had dinner together, at which I told him about that. He started this business of looking at the exchange between stars and gas in galaxies, and I took it up also, later on.

So that was interesting, but it had fairly little to do with observational work.

Something else I worked on early—and this is all up to about 1963, '59 to '63—was the size and the mass of our own galaxy. I had written a thesis in Holland about the mass distribution in our galaxy. [Ed. note: "A Model of the Distribution of Mass in the Galactic System." Leiden, 1956] I had worked on the spiral structure, from twenty-one-centimeter radio astronomy. And in fact around that time I was writing an article for the so-called Kuiper series, in which I wrote about the scale size of the galaxy and about the rotation constants A and B of Oort's that characterized the rotation of the galaxy. And that book, in fact, was published in 1965. [Ed. note: *Galactic Structure*, eds. Adriaan Blaauw and Maarten Schmidt, Univ. of Chicago Press, 1965]

So I was still in the middle of that earlier work, as it were. And through a contact with Bob Kraft, who was first at Carnegie and then went to Lick, we wrote an article about the rotation properties of the galaxy that you can derive from Cepheid variables. [Ed. note: "Galactic Structure and Rotation from Cepheids," in *The Galaxy and the Magellanic Clouds* IAU-URSI Symposium no. 20, Australian Acad. Of Sci. 1964] So that kept me busy.

When I finally started to do more observing at Palomar, an opportunity arose that at first looked as if it was just a little extra work. And that was that Rudolph Minkowski at Carnegie had retired in the middle of 1960—just after, by the way, he had discovered the redshift of the radio galaxy 3C 295. And that was the largest redshift at that time—forty-six percent. [Ed. note: 3C for third Cambridge catalog of radio sources.]

Rudolph had been working with Tom Matthews here at Caltech, a research fellow who was doing radio astronomy under John Bolton, who was director of Owens Valley at the time. In their work, Bolton and Matthews derived positions of radio sources in the sky, and Minkowski had been following up by taking optical objects at the positions of the radio sources in the sky, taking spectra of these optical objects, and finding out what they were, essentially. This was identification work of radio sources in the optical.

COHEN: He did this work at Palomar?

SCHMIDT: He did this work at Palomar. And then when he retired, I remembered that Dr. [Ira S.] Bowen, who was director of the Mt. Wilson and Palomar observatories at the time, called a little meeting in which I think Jesse [Greenstein] and Guido [Münch] and Bev [J. Beverly Oke] and I took part, in which he said that something had to be done about this work, because it was interesting work, and how was it going to be arranged? We agreed that we all would do a little share of taking these positions and objects that Tom Matthews produced and that we then would, in turn, take spectra at Palomar of them.

And without any formal arrangement being made, it soon came down to it that I did it all, and most of the others did not. Which was fine—it didn't matter.

COHEN: Now, that was only going on here? Nobody else was doing that anywhere else?

SCHMIDT: No, hardly. There was, in parallel, work going on at Cambridge University in England, under [Martin] Ryle. But the follow-up work of the optical objects identified with these radio sources, I think in quite a few cases Tom Matthews got their positions, and then it would go to me. Part of what I got probably came from other sources, but Tom Matthews was really my conduit of information.

So in the middle of '61, I started doing that. In fact, three or four years later I wrote an article in which I discussed the spectra of all these objects. [Ed. note: "Optical Spectra and Redshifts of 31 Radio Galaxies." *Astrophysical Journal* 141:1 (1965)] Those were all radio galaxies, about thirty or so.

Now Minkowski—except for his famous 3C 295, which was very far away—Minkowski's objects that he had gotten from Matthews had all been rather close. But Tom became more and more interested in distant objects, so he started to concentrate on radio sources that looked small in the sky, in the hope that this was caused by their being exceedingly distant from us. So the objects I got from him were in fact all quite distant. But there came a time when he gave me an object that looked very stellar—like a star rather than a galaxy. Now, in order to supply the background for this, I have to take a step back and tell you about another activity that had happened here in '60, which I was never involved in, and that was the discovery of 3C 48. 3C 48 was identified and optical work done in the middle of 1960 by a group of people, and I happened to be not involved. It was just before I started to get into this radio source business.

COHEN: Where were these other people?

SCHMIDT: They were all at Caltech. They were Tom Matthews, John Bolton, Allan Sandage at Carnegie, Guido Münch, and Jesse Greenstein. The position of 3C 48 was determined very accurately. They found an optical object. It turned out that there was a sixteenth-magnitude blue star at that position. And all these people started to do work on it. Sandage did photometry. And when he repeated it a few times, he found that the object was variable. Guido took spectra. Jesse took spectra. And they all, obviously, worked together and came up with a late paper at the AAS [American Astronomical Society] meeting of December, 1960, in which they announced that the radio source 3C 48 was to be identified with this sixteenth-magnitude star, that it was blue, that it was variable, that it had in its spectrum a number of emission lines that could not be identified. They said that although you couldn't exclude the possibility that it was a distant galaxy, they believed that it was a nearby radio star of mysterious properties.

OK, that's the aside. When Tom Matthews started to give me these positions in '61 or '62, occasionally there would be a radio object that was identified with a star. There were three of them. And one of them in particular, 3C 286, was a star in the sky optically. I took the spectrum, and it had one emission line near 5100 angstrom. And in fact in September of '62 I published a letter in the *Astrophysical Journal*, a one-page letter, saying that this object was mysterious, had only one line, and I couldn't identify the line. [Ed. note: "Spectrum of a Stellar Object Identified in the Radio Source 3C 286," 136:5 (1962)] The letter was almost like a complaint—that's all it said. There was no outcome, no conclusion. And the two others, which I didn't publish anything about, gave equally mysterious results. One of them I'm not sure I found any emission lines in, but I found them in the other one, all at different places in the spectrum. The wavelengths of these emission lines were all different among themselves, and also different from 3C 48.

So that's where it stood. In the meantime, Sandage and Matthews had a manuscript written about 3C 48, in which they gave all the data and essentially took the attitude that 3C 48 was a star. This was published, with an addendum you could understand later.

So there we were. And we are now advancing to the middle of '62, when the fifth of these mysterious radio stars comes up. And that was 3C 273. It's a very bright radio source. It's the brightest of them all—maybe number five or number ten in the northern sky. Actually, it is barely in the northern sky, and that's relevant, because what happened was that in Australia Cyril Hazard *et al.* took the opportunity to work with the Parkes 210-foot telescope to observe occultations of 3C 273 by the moon. In those days, radio positions were quite inaccurate, and an accuracy of a radio position on the sky of the order of one or two minutes of arc—that is, around 100 seconds of arc—was fairly normal. Nowadays, of course, you can do it to a second of arc. So in general, identifications were still uncertain, and observations were quite difficult.

Now, using the rim of the moon as a cutoff leads to positions that are much more accurate than a minute of arc. Hazard *et al.* did at least three occultation observations of the moon going by 3C 273 at different times in 1962, and came up with quite an accurate position for the radio source. And the radio source turned out to be double—there were two radio sources, at a separation of about twenty seconds of arc. And they communicated these results to Tom Matthews, probably late in '62, and Tom gave them to me. Of the two radio sources, one was very close to a bright star—thirteenth magnitude. And the other source seemed to be at the end of a linear streak, or jet, that came out from the star.

When I went to Palomar in late December 1962, to do my regular observing of radio galaxies given to me by Tom Matthews, I took that one along too. And I was sure that the jet was the radio source and that the star was a coincidence, because a thirteenth-magnitude star is not supposed to be a strong radio source. But the jet was very faint. Also, in December I could barely reach 3C 273, and only at the end of the night, because it's a spring object.

So at the end of the night of the 27th of December, 1962, I took a spectrum, for about half an hour, of the star. It was so bright that the plate was totally overexposed. My habit at that time was to work on objects of the order of eighteenth or nineteenth magnitude—sometimes seventeenth—much fainter objects. And typically I would spend my night on two objects, doing two hours of exposure on the brighter object, and the other six, seven, eight hours on the fainter of the objects—just two objects per night.

COHEN: Well, it was leisurely. You had a lot of nights.

SCHMIDT: Yes, and you sat in the cage—the prime-focus cage of the 200-inch—and you looked out, and it was very nice. But slow progress.

With this thirteenth-magnitude object, I was too conservative; I exposed too long. And the whole exposure was practically black on the plate. But I looked at it carefully upon development, and there was a funny feature at the end, in the far blue, that looked suspicious. But I cannot have been too intrigued by the object, strangely enough, because the next night I did nothing about it. But then on the third night, I did. On the 29th of December, I took it again at the end of the night—I think it was my last night. And I did a proper exposure time. So when I developed it and looked at it, there was a set of four or five lines that could be seen, all broad, in the spectrum, and it was a mystery what they were.

So I came back to Pasadena, and I looked at the spectrum, and I couldn't understand it. It was just like the others. And I remember talking to Dr. Bowen—because, of course, one of the most famous things he had done, in the late twenties, was to identify Mysterium. At that time, observers had found mysterious lines in the spectra of planetary nebula that couldn't be understood and that for a little while were characterized as, I think, "Mysterium." And Dr. Bowen was instrumental in understanding that these were the so-called forbidden lines of doubly ionized oxygen, and that forbidden lines—which under Earth circumstances will not show up in the lab, because they are terribly faint—can compete in cosmic conditions. So Dr. Bowen was the right authority to go to, and he looked at the spectrum, and we tried a number of things, but nothing came out that was promising. And it was left that way until the 5th of February, which was about six weeks after the run. This is 1963 by now.

Of course, I told John Bolton, who by then was back in Australia, as director of the Parkes Observatory. Bolton and Hazard proposed that they were going to write an article about 3C 273's radio observations and the occultations in *Nature*, and I was to write a companion article about the optical work. I said I would do that. So on the 5th of February '63, early in the afternoon, I was sitting in my office writing the article. And I decided, in order to check on something that I was writing, to look once more with an eyepiece at the spectrum. And it suddenly dawned upon me that if, out of the five lines, I ignored two, then the other three—together with a line that Bev Oke had found in the meantime in the far red, which was not on my plates—seemed to be regular in spacing and decreasing in intensity. There was regular, decreasing spacing from red to blue, and from red to blue they also got weaker and weaker.

Then I did something that I'm not an expert in. I decided that I would make an energy-level diagram—something I'd never done before—which is a spectroscopic device to bring order out of chaos, you might say. In doing that, I clearly made an error, because things didn't come out regular in what I was doing. And I got irritated at myself and said, "Now, hold it! These lines do have a regularly decreasing spacing." And I thought I would check that—that it was really true that the spacing was regularly decreasing.

So I decided I would take the ratio of each of the lines to the lines in the Balmer series of the hydrogen spectrum, which are known to have a regularly decreasing spacing. From H alpha to H beta is a lot, H gamma is fairly close, et cetera. It's a decreasing spacing between successive line pairs. So I did that. I took the first line, the brightest line I had, to H beta, which was just below it, and I found that the ratio of the two wavelengths was 1.16. Then I took my next line to H gamma in the Balmer spectrum, and I found a ratio of 1.16. Now I got suddenly excited. I take the third line pair, my last line and H delta; it's 1.16. I take Bev's line in the far red, at 7600, to H alpha: 1.16. And suddenly I realized that the spectrum was nothing but the Balmer spectrum shifted up in wavelength by sixteen percent. That was what it all came down to.

Now a sixteen-percent redshift by itself is not all that much. Minkowski had found, with 3C 295, a forty-six-percent redshift, which is much larger. But Minkowski's object was very faint—twentieth or twenty-first magnitude. This was a thirteenth-magnitude star, much brighter.

So I got all excited, I must say. I was pacing up and down in the office, and my door was open, and I emerged into the hallway. And I was walking back and forth, thinking. And here I see Jesse. I say, “Jesse, come and see what happened.” So I showed him. He said, “Oh, my God! Let’s look at the spectrum with 3C 48.” So we dug out his article about 3C 48, which had not been published yet but which had all the lines in it. And we worked on it. And in about five or seven minutes, we found a redshift of thirty-seven percent in the lines for 3C 48. It was not only Balmer lines but also other lines—mostly other lines.

So here we are; 273 has a redshift of sixteen percent; 3C 48 of thirty-seven percent. They mutually confirmed each other. And we were just aghast.

Then I remembered that the commotion we made attracted Bev Oke. And I remember that the last thing we did was try, on my blackboard, to prove or disprove that these were redshifts—to see whether you could, with highly ionized states of heavier stuff, perhaps in any way simulate what we had. And we never finished the proof on my blackboard. It was about six o’clock, I think. We all—which we never did—we all trooped with Jesse to his house. And Naomi was immensely surprised, because we all wanted a drink. [Laughter]

I came home late that night, and I think I said to my wife, “Something terrible happened at the office.” It’s not necessarily the right expression, but that’s what I said.

In the case of 3C 295, where there was a redshift of forty-six percent, nobody doubted that this was due to the expansion of the universe. [Edwin] Hubble discovered in 1929 that the universe expands, and objects that are more distant have bigger redshifts—they move faster away from us. So the redshift, therefore, is a distance indicator. Well, if you took the redshift of 3C 273 and put it at a distance according to Hubble’s law, you would find that the object is about two billion light-years away. But in that case, 3C 273’s luminosity is forty times larger than that of the biggest galaxies. And for 3C 48, something fairly similar came out.

And that was strange for two reasons: First, it was strange to find objects that are forty times brighter than the biggest galaxies. Second, they both looked like stars, not galaxies. Also, 3C 48 was variable. Galaxies don’t vary; they cannot vary. They’re too big to show variability.

So the other possibility was that what we were seeing was a gravitational redshift. If you compress the mass in a body sufficiently, then from the surface of the body you can get redshifts that are very large. Light from the sun has a redshift that is of the order of several parts in a million—exceedingly small. But if you were to compress the sun sufficiently to a body on the order of, say, a few kilometers, you could get a redshift of sixteen percent from it, or thirty-seven percent. So that was the other possibility that came up immediately—that we were looking at compressed stars in our galaxy. Fairly unusual, but interesting. So those were the two possibilities—a cosmological redshift or a gravitational redshift.

So I communicated the results to Australia, and they, of course, were all very excited. As a consequence of this, in the middle of March, 1963—things moved fast at that time—*Nature* published four articles: one by Cyril Hazard about the radio properties of 3C 273, one by myself about the redshift of 3C 273, one by Bev Oke about his infrared observations, and one by Jesse Greenstein and Tom Matthews about the redshift of 3C 48. In my article, I stated that although there were two possibilities to explain the large redshift, that unless an irrefutable parallax was found for the object, I thought it had to be extragalactic—part of the expanding universe and therefore very distant.

An enormous amount of publicity resulted, which hit me somewhat suddenly. I was on my way to Australia for a symposium in March, 1963, when these articles came out. I did travel—that I don't want to go further into, because it took me six days to get from Glendale, where I boarded the train, to Sydney, because I was traveling MATS—Military Air Transport Service—and I got stuck in the Philippine Islands. I found myself in Vietnam at one point. Amazing travel! And then there was further air delay in Singapore, because of a motor that didn't work. But that's not the issue.

Finally, I arrive at the airport in Sydney, at six in the morning on the Monday. And I walk down the stairs. And I couldn't see how anybody in the world knew where I was by that time, because I'd made these strange gyrations all over the Pacific. And I walked down the stairs, and somebody says, "Are you Maarten Schmidt?" And I say, "Yes." He says, "I'm from the *Sydney Morning Herald*." And that started the publicity. [Laughter]

COHEN: Was this your first experience with the press?

SCHMIDT: Yes, and that started a lot of publicity, of course, which lasted for a long time.

COHEN: You had not given a name or anything to these objects?

SCHMIDT: No, no. In the articles I don't know what we called them. We initially, at a very early stage, called them quasi-stellar radio sources. And on the occasion of some conference in Dallas in December ’63, where these things were discussed, the person who wrote about it at the conference for, I think, *Science* or *Scientific American* decided to abbreviate the name to “quasar”—which still denotes a radio source—and even though many of them are not radio sources, that’s the name that finally stuck.

COHEN: So it was really the press that gave the name to these things.

SCHMIDT: Well, no, no. This was a scientist, actually. His name was Little Chiu—Hong-yee Chiu—at the Goddard Institute for Space Studies, in New York.

From that time on, Tom Matthews and I concentrated strongly on these radio sources that were identified with starlike objects. And within a year we managed to get to a redshift of fifty-five percent for one of these objects that was fainter. So in ’64 we published two objects, one of which had the redshift of fifty-five percent. And then in 1965 I published five quasars with redshifts that went all the way up to 2.01—that was for 3C 9, meaning that 3C 9 was way out in the universe. And that started the business of these exceedingly distant quasars—of looking far backward into the history of the universe. This development was really explosive. Things really took off.

Around that time, there were a number of other discoveries that made astronomy very exciting—the discovery of the microwave background, the discovery of pulsars, the discovery of radio emission lines in local gas. Astronomy really took off. And so a lot of attention was given to all this, and it became all very exciting.

Now we are in ’65, and we now know that quasars go all the way out to a redshift of 2.

COHEN: There was no argument about this? The community really accepted this?

SCHMIDT: Up to that time there wasn’t, because the community had been very silent about it. It was only in ’66 that other observers started to play a role—publishing spectra on redshifts, and so on. People at Lick—Margaret Burbidge, among others—and at Kitt Peak, like Roger Lynds, were also successful in doing this work. But I had the field to myself until the end of ’65, essentially.

Just for the record, let me say that the doubts about whether the redshift really was indicative of these very large distances came to the fore essentially in 1966. And it was at that time that [Halton] Arp came out with his first statements that the redshifts were not cosmological. It was also at that time that [Fred] Hoyle, on a visit to Caltech, came up with an alternative explanation. This was the beginning of a controversy that lasted for a very long time and that at times became very public. The number of people who believed that the redshifts were not cosmological was not very large, but they are all well-known astronomers. And I should add Geoffrey Burbidge to this list of Chip Arp and Fred Hoyle. At the moment, Burbidge is perhaps the most vocal opponent of the redshifts being cosmological.

Begin Tape 2, Side 2

COHEN: You and Jesse continued to work on this in ’66.

SCHMIDT: Well, this is slightly out of chronological order. In ’64, we had published a paper which was the first exploration of the properties of quasars. [Ed. note: “The Quasi-Stellar Radio Sources 3C 48 and 3C 273,” *Astrophysical Journal* 140:1 (1964)]. Although practically nothing of this paper stands now, because we know so much more about the objects. We discussed the possibility of other types of redshifts in that article, and we came up with such a decisive rejection of the gravitational redshift that that hardly ever came up anymore. So the opposition from Arp and Hoyle and Burbidge and some others—most of it—does not have a convincing explanation of the redshift, and sometimes no explanation at all. So that seems to indicate that if they’re right, there’s some fundamental physics involved that we don’t know about.

Let me go back. It’s ’65 now, and from then on my observational work with Tom Matthews was, of course, much concentrated on these quasi-stellar radio sources.

COHEN: You’re looking at them optically?

SCHMIDT: Yes, yes. Tom gives me the radio objects and their positions.

COHEN: Where did he do his work?

SCHMIDT: In Owens Valley, with the twin interferometer. And often he also got positions from other radio astronomers.

So I would do that. And spectra would be obtained, redshifts obtained. And after a while I realized that it was perhaps a little aimless just to take random objects in the sky. Perhaps it would be useful to do a complete job in a particular part of the sky; i.e., take all the radio sources in a square of the sky, or a strip, and look at all of them and try to make complete identifications. If you do that, then you can make statements—it's like a census. You can make statements about how many of these objects there are per unit volume in the universe, while if you do it at random, as we did at first, you cannot say anything.

So Tom and I decided that we would work a little more systematically. And that led in '68 to a paper I wrote about the quasars in the 3CR catalog—the Third Cambridge Catalog of radio sources—in which I discussed the forty or so quasars we knew about at that time and found what their distribution in space was. [Ed. note: "Space Distribution and Luminosity Functions of Quasi-Stellar Radio Sources," *Astrophysical Journal* 151:2 (1968)] The results in that paper were quite startling, because the number of these objects—their density in space—increased enormously when you went to larger and larger distances. In fact, it increased about a hundredfold from here to the distance corresponding to a redshift of 2.

Now, radio astronomers had known that this increase in density is true of radio sources in general. But up to that time it had not been possible to say specifically for an object that you could work on in the optical, that these enormously increased in density with increasing distance. So it all fitted together, but it was a nice finding that quasars did this. And in fact, for quite a while the radio astronomers thought it was the quasars that were solely responsible for this increase in density of radio sources. But I later found that the same increase happens with radio galaxies. So it's true of all the radio sources.

Now in working on all this, I found that it was pretty complex to have to account for both the selection effects—both the radio and the optical—simultaneously. And I realized that if you could do it in only one wavelength region, things would be simpler.

Now in the meantime, [Allan] Sandage had made a discovery that quasars need not be radio sources. In fact, that came about very early—I think in '64 or '65, when he discovered what he first called "interlopers." That is, he would look at the position of a radio source on the plate, and search for the stellar object—usually blue in color—that could be the quasar.

Sometimes he would find nothing on the position of the radio source, but then elsewhere in the field he would see a blue object. And when he started to take spectra of these blue objects, he found that they were sometimes quasars—so apparently quasars did not have to be radio sources. That started the whole field of optical selection of quasars—not using radio, but just doing it optically. And eventually I decided to switch over to quasars that were only optically selected, and that's essentially what I've been doing in the quasar field until the present day.

What happened is that Richard Green, who was one of my students in 1972, decided to do a survey of the northern sky, looking for blue objects. The idea was that you would find quasars that way, and you would also find other objects of interest. And this resulted eventually in the so-called PG catalog—the Palomar Green Catalog. He did the survey with the 18-inch Schmidt at Palomar, then he did all sorts of observations with the 100-inch on Mt. Wilson. He and I worked for years, taking of the order of 3,000 spectra with the 200-inch and then the 60-inch. And out of that work came the Palomar Green Catalog, which has about 1,500 objects, many of which are very interesting types of galactic stars. What interested me most were the 100 or so quasars we found, and that became the Palomar Bright Quasar Survey—the largest survey of quasars over the sky, up until now. It soon, hopefully, will be superseded by others. And it is fairly bright, of course, because it covers such a large area. It reached, on the average, down to objects of about sixteenth magnitude. And Richard and I wrote an article in 1983 in which we discussed the change in space density of quasars determined solely, entirely, by their optical properties [M. Schmidt and R. F. Green, "Quasar Evolution Derived from the Palomar Bright Quasar Survey and Other Complete Quasar Surveys," *Astrophysical Journal* 269:352 (1983)]. We showed that the density of quasars increased very much with distance, except that we found that this effect was more pronounced for the big quasars, the strong ones, than for the weak ones.

Then I went into a period of administration.

COHEN: Now, you took over as executive officer for astronomy from Jesse in 1972?

SCHMIDT: Yes. But let me finish the scientific. Because the administrative thing is a rather long story.

COHEN: Why don't we separate them? Why don't you continue the research, and then we'll go back.

SCHMIDT: Yes. It's probably better if I complete the science. I emerged from administration in 1980.

COHEN: So 1972 to 1980 was a quiet time, scientifically, for you, when you were doing all this administration?

SCHMIDT: That actually started in 1975, when I became division chairman. And to pick up at the time when I resumed active observing—that took off in 1981 or '82, when I teamed up with Jim Gunn, who had been here but now was—and still is—at Princeton University, and Don Schneider, who became my postdoctoral fellow in 1981 when I returned from administration and a leave of absence.

The three of us decided that we would search for quasars—this time not for bright quasars, as Richard Green had done, but for quasars that were as distant as possible. Because in the meantime there had been a number of hints in the work from other people that perhaps this density increase didn't persist forever. And, of course, it wouldn't. Because you are looking back in time, and these objects must have started to form at some time. So we wanted to see that time—the time of formation of the quasars.

We decided that instead of doing it with photographic plates, we would use CCDs [Charge Coupled Devices], which are these small solid-state detectors that allow you to get data immediately in digital shape, so that it can be handled with computers. But to do this type of work with CCDs was unusual, because CCDs are very small, and therefore the field that we covered on the sky would be very small. Now, at first, in the middle eighties, we took care of that by aiming at many different parts of the sky and adding up areas. But we certainly didn't find very distant quasars, and also it became frustrating to have to constantly reposition the telescope. And I also had to find positions in the sky where there were no bright stars, because the CCD didn't like that. And so on.

Then we got the idea that perhaps you could do the following: Rather than aim at different parts of the sky, perhaps what you should do is park the telescope and let the sky go by.

Now, normally, of course, if you did that with a photographic plate, you would get streaks. Every object would be a streak as it came by. But CCDs are read out electronically, so you can compensate for that. And therefore, it was just as if you had a roll of film in the focus and you moved the film at such a rate that everything was projected properly. There was no blurring. And essentially, what we got was a very long strip of the sky observed. We called that a "transit method," which is an old astronomy term. So we did our transit surveys, and those became very successful.

COHEN: You built a lot of equipment for that, didn't you?

SCHMIDT: Yes. Well, the equipment was already there, but Jim Gunn had to do the electronics, which he was fantastically good at. Whereas other people who had similar ideas took one or two years to get their instrument in shape, Jim did it in two months. And we immediately started working. This survey finally produced results, all of which were published between 1994 and '95. Our work showed that from a redshift of 2.7 out to 4.7, the density of quasars doesn't increase but decreases. And when you combine this result at the higher redshift with the results we and many other people in the meantime had gotten for redshifts up to 2, it was such that up to 2, the density increases, and beyond 3 it declines. So somewhere between 2 and 3, there must be a plateau, and then the density declines.

COHEN: So you think you're getting close to the edge up there?

SCHMIDT: That's right. And in the process we found, three times, the most distant quasar on record. First, one that had a redshift of the order of 4.1, and then one that had a redshift of 4.7, and then finally in '93 we found a redshift of 4.9.

COHEN: This work was done at Palomar?

SCHMIDT: This was all done at Palomar. The quasar with a redshift of 4.9 is still the largest redshift on record. It's the farthest away. And in an expanding universe that has an age of about thirteen billion years, the light from this quasar was emitted one billion years after the beginning—and we caught it now, twelve billion years later. One night that light came into the dome and entered the telescope, and we recorded it.

So you look plenty far back in the universe.

COHEN: You can see why this has caught the imagination of the press.

SCHMIDT: It's quite a development, yes. I remember that sometime before 1963, when I had done an exposure on a radio galaxy, Dr. Bowen visited. And I had just done a particularly difficult radio galaxy that I could barely see. I'd exposed it practically all night, and there was hardly anything on the plate. And unfortunately Dr. Bowen at lunch had asked if he could see what I'd got. So I said this was not a good result, of course. And he looked at it, and he said he liked it. He said, "This was work at the edge." I was trying to get out of the instrument what was possible, or perhaps slightly beyond. He said that the 200-inch was often used for things where people took an exposure every ten minutes and did a lot of them, but he felt that the telescope should be exploited to its limits.

COHEN: So he really encouraged you.

SCHMIDT: He was quite happy. [Laughter]

And I remember, in my dreams about radio galaxies at that time, that I thought in my career I wanted to get a redshift of two-thirds—we wanted to get beyond the redshift of forty-six percent, but I hoped I could reach two-thirds in my career.

COHEN: So have you made the move now to Keck on these? Have you found anything farther out than this at Keck?

SCHMIDT: No, no. No, we're not looking at Keck this way, because the instrument capabilities have to be there, and we don't have them at Keck in precisely the way that we can use for this type of work.

Let's see. It's probably better if I finish talking about my science. There are two other areas in which I'm active, which I'd like to tie in: X-ray astronomy and gamma-ray astronomy.

Many of the X-ray sources in the sky—and, of course, you have to work with satellites, because X rays don't penetrate the Earth's atmosphere—many of the X-ray sources are quasars or Seyfert galaxies. Seyfert galaxies are galaxies we've known for a long time, but they seem to have a quasar of modest activity in their center. And we really believe that quasars and Seyfert galaxies are all the same thing. In the case of quasars, the activity is enormous; in the case of Seyferts, it's somewhat more moderate. And there are, in fact, Seyferts in which you can hardly see the activity. So there's a whole range.

It turns out that most of the objects you find in X-ray surveys of the sky that go very deep—perhaps eighty or eighty-five percent of them—are Seyferts and quasars. So X-ray astronomy allows you to find out how things are distributed in space in a way that is totally independent of what we've done optically. Almost nothing is known about the distribution of Seyfert galaxies from optical work.

So I became involved in a deep survey with a large team of people on ROSAT [Röntgen X-ray Satellite], the German X-ray satellite launched in 1990.

COHEN: So that's what takes you to Munich so much?

SCHMIDT: That's right. I've been working with Riccardo Giacconi, who is now the director-general of ESO—European Southern Observatory; with the Germans, Joachim Trümper, who is at the Max Planck Institute for Extraterrestrial Physics in Munich; Günther Hasinger, who was there but now is at Potsdam; and several others, to prepare this and eventually do the observations and the optical follow-up. We have ROSAT observations in an area in the northern sky with little foreground hydrogen. We go very deep, deeper than any other investigators have. We identify the X-ray sources with the optical objects we have gotten from optical work at Palomar and elsewhere. And now we are in the process of taking spectra of these things. But these optical objects are so faint that we have been doing it at Keck. And that has been very successful; we really need the extra light power of Keck to do this work.

COHEN: So you've merely detected them on the satellite, and now you're looking at them at Keck?

SCHMIDT: That's right. The satellite gives you a map on the sky, and then you put it over the map of the optical sky—just like the radio work earlier—and you identify them. And then you do the optical work.

COHEN: This ROSAT satellite is your Tom Matthews?

SCHMIDT: Exactly, my radio telescope, as it were, except this is X rays. So we find very interesting results. We already are finding that Seyferts show very strong changes in space distribution, which we really didn't know before. So this work is going on, and is still going on right now, and is succeeding very well.

Then let me mention the last thing that I've become interested in, in the past three or four years, and that is the so-called gamma-ray bursts. If you observe the sky in gamma rays or high-energy X rays, you find that every once in a while—typically once or perhaps more times per day—a source flares up for perhaps a few seconds, or perhaps thirty seconds or so. It's brighter than anything else in the gamma-ray sky, and then it goes out. And it's typically not seen again—at least, not for thirty years.

A satellite called the Compton Gamma-Ray Observatory [GRO], which is flying at the moment, has an instrument on board particularly suited for detecting these sudden events. And it has now recorded more than a thousand of them. These bursts are isotropic in the sky—you see an equal number in every direction. And they cannot have a uniform distribution in space—but unlike the quasars, where the density increases as you go out, these objects apparently have a decreasing density as you go out. The isotropy and the density decrease make people believe that gamma-ray bursts are all at larger redshifts. I'm not going to go into details here.

COHEN: This is work in progress?

SCHMIDT: Yes. And the gamma-ray community thinks that they are all at cosmological distances. For some curious reason, I find myself drawn to disbelieve that. It's curious, because you'd say from my history that anything that turns out to be distant and cosmological I should like. But somehow I still don't quite believe it. The trouble with these objects is that you cannot see them optically. You only see them at gamma rays.

I am essentially redoing the entire derivation of the positions and the properties of the objects from data supplied by the GRO satellite. In other words, what their team has been doing for years, I'm locally trying to repeat, using the raw data as it comes from the satellite. And this involves major computing, major memory problems. It's an unbelievable job. And I'm in the middle of it. I have no idea what will come out. We'll see. [Laughter]

MAARTEN SCHMIDT

SESSION 3

May 15, 1996

Begin Tape 3, Side 1

COHEN: We will continue this history. I think you wanted to say something about former students.

SCHMIDT: Yes. I've had six PhD students. I'll be very brief about them. The first one was Bob [Robert W.] Wilson. That was in the early sixties.

COHEN: Of Nobel Prize fame?

SCHMIDT: That's right. I took him over from John Bolton, who in the early sixties was the director of Owens Valley Radio Observatory. Bolton went back to Australia in the early sixties, and I took Bob's supervision over from him. Bob Wilson did a radio survey for his thesis work. I thought it was actually only of moderate interest. I had seen Westerhout at Leiden do the same thesis, which was at least as good.

Anyhow, Wilson got his PhD, subsequently went to Bell Labs, then of course discovered, with [Arno] Penzias, the cosmic microwave background radiation and got the Nobel Prize for it. So it's not bad to have your first student—or half student—get a Nobel Prize. So we start at the peak. [Laughter]

Then Bruce Peterson, who did brightest cluster galaxies, photometry, in order to derive the galactic absorption in our own galaxy, the observed clusters of galaxies at different galactic latitudes. And he was able to show, essentially for the first time, that the absorption of light by galactic dust was much less than the traditional value, by a factor of three or so. There had been suspicions that everybody had been overestimating the galactic absorption—that is, the absorption in our own galaxy. His results were based on material mostly gotten at Mt. Wilson—not easy; it was a very good piece of work.

Then there was Donna Weistrop. She did a thesis on the luminosity function of local stars, which was an old interest of mine. It was a good thesis, but in the course of it she—and also I—made an overinterpretation on the red side of the luminosity function, causing us to think that there were large numbers of very faint red stars in the galaxy. And since there is always the problem of the unseen mass, it looked as if this was important. It got us, and particularly her, into a major argument with Willem Luyten, at Minnesota, who disagreed for the right reasons—but in his typical fashion was exceedingly public and vocal about it. In fact, one paper he published from Minnesota was called "Weistropgate," which is not awfully nice.

Then there was Richard Green, who, as I mentioned, did a survey of blue stars in the northern skies. He started it in 1972, with the 18-inch Schmidt, and then he did photometric calibration at the 100-inch on Mt. Wilson. He and I did an enormous amount of work in taking spectra eventually of about 3,000 stars from that catalog at the 200-inch at Palomar. Out of that came the Palomar Green Catalog of about 1,500 or 1,800 objects, which is very useful—much used at present in astronomy. And in that group were 100 bright quasars, the so-called Palomar Bright Quasar Survey, which remains unique in that it covers a quarter of the sky and lists the brightest known quasars in the sky.

Then there was Rick [Richard] Edelson, who did multiwavelength spectra of active galaxies. And finally, there was Irwin Horowitz, who did luminosity functions of emission-line galaxies in the surveys that Jim Gunn and Don Schneider had done in the late eighties at Palomar. We had not only discovered distant quasars but also many emission-line galaxies, and Horowitz did a thesis about the luminosity function of these objects.

All six, except for Bob Wilson, were present a little over a year ago, when there was a symposium at Caltech on luminosity functions, which was essentially in my honor.

COHEN: It was a good party.

SCHMIDT: That should take care of students. I don't think I have anything to say about colleagues.

COHEN: And these students are mostly still working in astronomy, aren't they?

SCHMIDT: Yes. Bob Wilson is at Bell Labs. Bruce Peterson is in Australia at the ATT—or the national university. Donna Weistrop is at Nevada. Richard Green is at the National Optical Astronomy Observatories—NOAO. And in fact he may well still be deputy director, I think, under Sidney Wolff. Rick Edelson is at Colorado. And Irwin Horowitz has moved to a smaller college, whose name I can't quite think of.

COHEN: So you're really proud of your students.

SCHMIDT: Yes, yes. Not a huge number.

COHEN: Well, quality is what counts. All right. We were then going to go on to your other life. Would you say that that started probably in '72, when you took over [the astronomy department] for Jesse?

SCHMIDT: Yes, yes. That was the beginning of my administrative career at Caltech, which ended in 1980. I was executive officer for three years. Jesse—I don't even know why—but Jesse finally decided that he had had enough. He had been executive officer for astronomy—if that position existed when he came here—since 1948; I think it did. So he had been leading everybody for twenty-four years, and probably wanted to have some respite.

Anyhow, I became executive officer and did it for three years. At that time, the group was fairly quiet. Fritz Zwicky was always sort of interesting and rebellious, and so on, but we were used to that. So things went along fairly well. And there is little that I can remember that I should report about.

COHEN: Who was the provost at that time?

SCHMIDT: Provost at that time was Bob Christy. And the division chairman at that time was Bob Leighton. Everything went quite well and easy. There is not much I remember about it. Then in '75 I became division chairman [of Physics, Mathematics, and Astronomy], which I was for three years. And that was a job that was very much heavier. It turned out to take practically all your time. And the reason is that being division chairman at the institute essentially means that, as Truman said, "The buck stops here" for the entire faculty of physics and math and astronomy, which was on the order of sixty-five professors. Any problem that cannot be solved at lower levels drifts up to the chairman. You never get a problem that is easily solved, because everybody has agreed that essentially it cannot be solved, or they would have solved it.

COHEN: Do you have any handle on the budget in that position?

SCHMIDT: No. Essentially, I was given a budget. At that time, by the way, in ’75, Harold Brown was president and Bob Christy was provost. In terms of budget, at least at that time, the position of division chairman was totally unexciting. You were assigned a budget that mostly covered the salaries of the faculty and hardly anything else. Caltech’s attitude is that research should be funded from grants and contracts with outside agencies. There are some discretionary funds the chairman has which have accumulated over the years, but that’s in total a fairly small leverage that the chairman has.

So it’s mostly that you had to try to resolve all the problems that came up, and in particular, of course, in terms of new appointments and tenure cases, do a job that by that time had become much more regulated and connected with civil rights and equal opportunity. The appointments were, of course, always positive and interesting, because you try to add to the faculty. The tenure cases in a number of instances were very difficult, and in fact there were a number of negative tenure decisions during my chairmanship, particularly in mathematics. But it went all right, in the sense that there were no major consequences, no lawsuits or newspaper articles about it. This was also the time when the Jenijoy La Belle case happened in the Humanities Division, and it was a good warning to me how not to do things.

There were not many matters that were highly controversial at the time, although I may have conveniently forgotten them. The thing that was most positive, and that I’m quite happy about, is that the initiative to go into gravitational-wave physics came up at that time, started by Kip Thorne. And as division chairman I could have slowed that down if I’d wished to, but I supported it. Kip came up with a formal proposal, and the division discussed it, and things were very positive. And that led to, under my administration, an attempt to get [Ronald W. P.] Drever here, from Glasgow. By the time I was at the end of my term as division chairman, that had not quite worked out. So Robbie [Rochus] Vogt took over as chairman and was the one who finished the discussions with Drever. And Drever was then appointed part-time, and eventually became full-time.

COHEN: So you were division chairman for only three years. Isn't the traditional term five years?

SCHMIDT: Yes, but in the meantime Horace Babcock was going to end his directorship of the Hale Observatories in 1978. So in '77 the institutions and the staff decided to ask me to succeed him as director. That's why I held the division chairmanship for only three years: I moved from one position to the other.

The pressure of the division chairman job was enormous. It essentially takes all your time, and the only reason I stayed somewhat in science was that I had told Richard Green, who was then my student, that he could interrupt me anytime for important science. And he would do that once in a while. And now I remember that every time he did, I was slightly irritated. An interruption, and I'm working, and there's pressure! But of course it kept me scientifically alive—so that was good. He did it very faithfully. [Laughter] So that worked all right.

But it was an all-consuming job; it's a heavy responsibility. And the fact that I had two physicists looking over my shoulder—namely, Christy and Harold Brown—was both an advantage and a disadvantage. The advantage is that I couldn't stray so easily—truly stray. They knew too much of what was going on, you see. The disadvantage was that, in a sense, at times it was difficult to convince them of something. [Laughter]

COHEN: In retrospect, do you think it's a good system?

SCHMIDT: The division chairmanship?

COHEN: Yes, the institute working this way?

SCHMIDT: I'm not sure. I am worried that probably in each division somebody who is doing well as a researcher—and teacher, perhaps, but certainly as a researcher—is going to do a heavy job in administration. My feeling really is that the administration should give much more support to the division chairmen than they did at least at that time. You are the top person—in the case of our division—of one-quarter of the entire institute, with really minimum help: one administrator and one secretary. And when you look at the upper administration, there is much more solid support for the people there, who however don't have to be the leaders of one-quarter of the faculty. So I'm worried that the best people spend many years in these positions and often will not be able to return to science.

In my own case, I was essentially out of science, mostly, from '75 to '80. And it almost did me in, and yet I could hardly cut back. I just managed. It was very difficult.

So then I took over as the director of the Hale Observatories, and that was only a two-year period.

COHEN: And this was not calm going.

SCHMIDT: No. The relations between Carnegie and Caltech… First, the Hale Observatories consisted of Palomar and the Big Bear Solar Observatory, and on the Carnegie side Mt. Wilson and Las Campanas, in Chile. I moved my office from Caltech to Santa Barbara Street, at the upper level, facing north.

COHEN: Was this a symbolic move on your part?

SCHMIDT: Well, there's so much going on at Caltech besides astronomy, and there is nothing going on at Santa Barbara Street but astronomy, so this seemed the right thing to do. And it certainly, at least as a gesture, must have made the Santa Barbara Street staff believe that I was fully involved—and I was. And in fact, the activities as director, which also were very heavy, were certainly more than fifty percent—probably sixty-five percent or so—on the Santa Barbara Street side, primarily because of Chile.

COHEN: Are there more people there? Are there more astronomers at the Carnegie Institution than at Caltech?

SCHMIDT: Somewhat, yes, it is a somewhat larger group. And they certainly have a larger number of fellows, although at that time we also had quite a few research fellows at Caltech.

But the astronomy group was slightly larger, yes.

COHEN: They didn't have to go out and get their own money the way Caltech astronomers do, did they?

SCHMIDT: Well, that was changing at the time. The Carnegie Institution, until about that time or somewhat earlier, had had a policy of not requesting federal funds, but that was slowly changing and some of the Carnegie astronomers did have federal grants.

The relationship between Caltech and Carnegie concerning the observatories had not been overly good. And curiously enough, that didn't apply so much to the astronomers but more to the administrative levels. Jesse Greenstein certainly had his conflicts with the Carnegie administration.

COHEN: Was this over the running of Palomar?

SCHMIDT: Well, it was a curious system: the two halves were financially, and to quite a degree organizationally, entirely independent, but the facilities were utilized jointly and there was also a common staff appointed from the scientists at the two institutions.

COHEN: You mean the people who actually worked at the observatories?

SCHMIDT: Yes. There was the title of staff member of the Hale Observatories, and it included those people approved by the Observatory Committee and the director of the Hale Observatories for the particular position. And it included all the professors in the astronomy department at Caltech. But through this staff membership—which was an essential feature of life and stature in this community—the one side essentially could affect academic appointments or scientific appointments on the other side. If the Caltech group proposed that a potential faculty appointee become a staff member of the Hale Observatories, that then had to be approved by the Observatory Committee, which consisted half of Carnegie and half of Caltech astronomers. So that meant that the Carnegie side was able to influence, or bias, or perhaps even veto, or make difficult, Caltech's academic appointments. And that was not a good feature and it happened a lot. Then there came a time—in October, 1979, after I'd been director for a year and three or four months—when an appointment proposed by the Carnegie side was essentially nixed by the Caltech astronomers. Until that time, I had mostly seen the process work the one way, but this time it worked the other way. And I really felt that the system wasn't working.

COHEN: So there was bitterness over this?

SCHMIDT: Yes. And there were other things, of course—things going on that I cannot recall in detail. But this was sort of what broke the camel's back.

It was at the end of October, soon after this Observatory Committee meeting, that I decided the bull should be taken by the horns. I wrote a letter to the two presidents—of Carnegie and Caltech—proposing that the agreement between Caltech and Carnegie to operate jointly the observatories, an agreement that that had existed since 1948 and had been amended several times, be terminated.

COHEN: You made this decision yourself? You didn't talk to anybody?

SCHMIDT: Yes. I talked to one person to some degree, but lightly. This was an unusually abrupt decision on my part. That's all I can say. It happened. I indicated in the letter that the mutual interference between one side and the other had led to a system that didn't work well, and that I felt there were more disadvantages to doing it this way than advantages. And therefore I proposed a separation. I indicated that since clearly there would have to be discussions about this in which my performance as director would also become an issue, that I was resigning as of the first of July 1980—which was nine months from then. And my final paragraph was that I proposed that the two presidents start the process by consulting their astronomy staffs.

COHEN: Now, the actual workings of the observatories, the way that functioned, that was all right? People were happy with their access to the telescopes?

SCHMIDT: Yes. That was not at issue. In fact, that was the one aspect that would be maintained for many years—and has finally disappeared.

Of course, this letter of mine was rather stunning. I had telephone conversations with the president of Carnegie, Jim Ebert, who was very surprised. I talked to [Caltech president Marvin] Murph Goldberger, who was very surprised. I talked to Robbie Vogt, my successor as division chairman.

Robbie's reaction was most interesting. I came to him and I said, "Well, I resigned as director of the Hale Observatories." "Ah," he said. I said, "Well, that means that discussions will have to start." He looked at me and said, "What do you mean?" I said, "Well, I will be resigning as of July 1." He says, "You mean it?" I said, "Yes." And then he smiled broadly. Because I knew that as soon as he became division chairman, he had been interested in annexing Palomar to the division—which is, of course, where one could say it belongs. But because of its status as a joint unit with Carnegie, it was sort of outside the division, and Robbie had not liked that. He smiled broadly and couldn't believe it. Because Robbie's system has been different. He would resign any moment, and then of course go on.

Indeed, I was kept out of the discussions that later took place—although I know what happened, of course, and there is also documentation about it. But essentially what it came down to is that the Carnegie side opposed the separation—that is, the astronomers and the president. And the Caltech side supported it.

COHEN: Why do you think this was? Did the Carnegie side feel they were losing their status there, not being part of Caltech?

SCHMIDT: It's hard to say why it was. And that's a question that should be asked of others, of course, because my position was a very difficult one. And I had no part in those discussions, as I say.

COHEN: But you must have laid out the arguments for them.

SCHMIDT: No, no. Now it's true, of course, that I probably was thinking as a Caltech person in this whole conflict. We had at Caltech a situation that was different—where the astronomy effort is closely related to physics. There were many physicists who actually were doing astronomy, like Leighton, for instance. To be part of the institute and then to have a relationship to an outside institution that could influence our appointments somehow just seemed to be not right. Because if anybody influences our appointments, it ought to be the physicists and the mathematicians, with whom we are joined.

COHEN: So this is the tilt, always toward physics.

SCHMIDT: Yes. We were part of another culture, as it were. And I can imagine that Carnegie felt that part of their strength was in a solid union with Caltech in this astronomy effort, and that losing that—and perhaps losing access to the 200-inch Palomar telescope, which they, of course, had as much access to as we did—was very bad.

So that's where it stood for a while. And what happened is that the separation was executed on July 1, when I stepped down, by the presidents. The joint management or operation of the observatories was replaced by joint utilization, and it meant that the time-assignment committee still consisted of Carnegie and Caltech astronomers. And that went on very well, until the late eighties; it ran smoothly and was appreciated by both sides.

COHEN: So the only thing that really happened was that the two parties did not have a say in each other's appointments, which you said was the biggest problem.

SCHMIDT: Yes, that's right. And separate directors for the two institutions. So that is what happened there.

COHEN: Why was there so much bitterness?

SCHMIDT: Great bitterness, yes, yes. Carnegie felt betrayed, essentially.

It is not up to me to make a judgment as to whether this was good or bad, what had been done. I should say that the original agreement between Caltech and Carnegie was negotiated by administrators—people at higher levels. It was not the astronomers who necessarily wanted it. But when Caltech got the 200-inch telescope from the Rockefeller Foundation, it had no astronomers, and it was natural to look to Santa Barbara Street, where there were lots of world-known astronomers—it was natural to link together.

The only thing I would say in retrospect, something I didn't bring up at the time, was that each of the administrations of the two institutions, in dealing with their joint observatories, were part of the time blaming the other side for not wanting this, not being able to move and do that. And they were waiting for the other side to come to reason, as it were.

The advantage of the separation was—and, as I say, I'm not necessarily taking credit for it, but I still think it was a healthy thing—that as soon as the separation took place, each of the two institutions became keenly aware that they were responsible for their own astronomy facilities, which were major. So now Caltech owns two 10-meter telescopes, and Carnegie will have two 6.5-meter telescopes. And I cannot believe that if we'd stayed together we would have had four of the largest telescopes in the world together.

COHEN: So everybody has more, is that what you're saying?

SCHMIDT: Yes. I think it sharpened the administrations' concerns and commitment to their facilities—once they couldn't blame administrative difficulty on anyone else.

There were, of course, lots of things going on in Chile in the meantime. Las Campañas had been operating for a long time. But Chilean inflation was major in the beginning of my directorship, and that made life immensely difficult in terms of budgeting. These were not easy things. I went about three times a year to Chile, and I think things went all right. But it was a hard job.

So that was five years, in total, of essentially a 100-percent administration. And starting July 1, 1980, I took a leave of absence to go to the Institute for Advanced Study in Princeton to recover from all this, and to try to get back into astronomy.

COHEN: Ah, yes. I remember that we [Marshall and Shirley Cohen] were on sabbatical the same year. I remember seeing you there.

SCHMIDT: That's right.

COHEN: We were just visiting in Princeton. I think we watched the first of [Carl] Sagan's television programs [Ed. note: the *Cosmos* series] together.

SCHMIDT: Oh, my God! Yes. [Laughter]

COHEN: I think I recall sitting in the [John and Neta] Bahcalls' bedroom—

SCHMIDT: Yes, that's right!

COHEN: So you were there for a whole year? And you moved your family?

SCHMIDT: That's right. Corrie came along, and the children were already in college.

COHEN: That must have been nice for you.

SCHMIDT: Oh, yes, it was great. I didn't publish a single paper there, except one that John Bahcall wrote. It was a great time. [Laughter]

COHEN: Let's go now into your national and international activities. What year were you president of the AAS?

SCHMIDT: I was president of the AAS from 1984 to '86.

COHEN: So this would have been after you came back, you were then again immersed in your research and teaching.

SCHMIDT: Yes, that's right.

COHEN: Richard Green must have been gone by that time?

SCHMIDT: Yes, and Don Schneider in the meantime had become my postdoc, in '81.

COHEN: When you came back from Princeton?

SCHMIDT: Yes.

COHEN: Had you been active in the AAS before?

SCHMIDT: Oh, I must have been in the council at some stage. And when you are elected president, you first become president-elect for a year, and then you're president for two years. And then you're past president or so for a year. So in total, it's four years. But during those middle two years, of course, you're in charge. And it means conducting council meetings at every AAS meeting—that is, twice per year. But it also means trying to do things in the interest of astronomy on a national scale, because the society can speak independent of the particular institution where you work, and can attempt to make the case to funding agencies or the government.

There are three things I want to briefly mention that happened during that time. The one was internal, and is totally unimportant, but it nonetheless was not easy. I always seemed to have to conduct meetings that are not easy—you'll find that again in the AURA [Association of Universities for Research in Astronomy] board. And that is that the AAS has divisions—that is, special-interest divisions, like the high-energy division, the planetary science division, the solar division, et cetera. And these divisions came about some twenty years earlier, when it appeared that a number of space scientists wanted to abandon the AAS and go to the American Physical Society, because they felt that astronomy and the society were not heeding them. So the high-energy division was started at that time. And everything went fine, when, at the last meeting, my predecessor, Arthur Code, heeded a request from the ultraviolet astronomers for a separate division. He allowed them to form a working group for a year, in order to prepare for it. And I had a very difficult time with that, because it turns out that ultraviolet astronomy, which also has to be done from satellites, is so central to everything that's happening in astronomy that I felt very strongly that if we were going to have a division for it, you could have a division for everything. It ought to be on the periphery that you have divisions. [Tape ends]

Begin Tape 3, Side 2

COHEN: So you did not want all these divisions.

SCHMIDT: Well, I didn't want to have an explosion of divisions. So there came this council meeting, at which we decided that there would not be a UV astronomy division and that the working group would be disbanded. And then the working group asked me to appear before them to argue why they should be disbanded. Now, this is, of course, a group of people who don't like that at all. So it was very interesting. But I survived. Anyhow, I'm very glad that there still, as far as I know, is not a UV astronomy division.

That was internal. There were many other issues. Let me point out the two that I think were perhaps the most important outside issues. The first was the Very Large Baseline Array [VLBA] of radio antennas that were going to be used for very-large-baseline work. The original budget had already been approved. The VLBA now is working. It has ten antennas all over the country, and it is a dedicated array. Until that time, very-large-baseline work in radio astronomy was done by antennas that were hooked up, but each of them existed for other purposes. The VLBA proposal came from one of the [National Science] Academy decade studies. It had been approved, had in fact gone through the Congress, and then somehow the Congress stopped the funding. And it was stopped by Mr. Boland [Edward P. Boland, D.-Mass.], of the Boland Amendments restricting covert aid to the Nicaraguan Contras. Mr. Boland was unhappy about how people having to do with the Hubble Space Telescope had treated him.

So Mr. Boland's subcommittee refused to authorize the start of funding for the VLBA. And I spent an enormous amount of time conspiring in the background with several other people in the community to get this going. I think it took about a year or a year and a half, and it finally did get going. But it meant getting Senator [Pete] Domenici of New Mexico actively involved. This was politicking—pure lobbying in the interest of the community. And it finally worked.

The other thing that happened was that the astronomy budget in the NSF [National Science Foundation] around that time was doing very poorly. And there was one year in 1986—this started in 1984—when the astronomy budget for 1987, which was being discussed, was to be one percent over that of '86. And all the other fields in NSF got increases of between seven and ten percent. So we felt singled out.

COHEN: Was there any reason for this?

SCHMIDT: Not a good reason. So through the intervention of an astronomer elsewhere in the country who talked to a senator, I was invited to testify on the Hill before the relevant Senate subcommittee. The director of the NSF [Erich Bloch] testified before me, and I came next. And curiously enough, I saw [Caltech professor of chemistry] Harry Gray there, who was also testifying. [Laughter] He was very worried when he saw me. He said, "What are you doing here?" [Laughter]

Now, essentially my plea was that astronomy was a very worthwhile science. And our proposal—which had been prepared by Peter Boyce, who was executive officer of the AAS—was that the NSF budget be increased so as to accommodate a larger increase for astronomy. We were very careful in not asking for other fields to be reduced, but rather for an increase.

I had contact with Mr. Bloch's office, the director of NSF, about this whole thing. He knew that I would testify, and we tried to arrange to meet at the NSF before I went up the Hill. But that was impossible, because I arrived too late in the day. So, in order to be absolutely sure that he knew what I was going to say, I sent the past president of the AAS, as well as the next president of the AAS, to his office. And therefore, Art Code and Bernie [Bernard] Burke went to Mr. Bloch's office and discussed things with him and presented him with a copy of my testimony.

What happened at the hearing itself is that Mr. Bloch testified and he immediately left. And I testified. And somehow things did not sit well with him, and I cannot understand why, because I'd done all the courtesies necessary to keep up a good relationship. In subsequent meetings, before both of the NSF's astronomy advisory committee and the physics advisory committee, he berated me for having done what I did. And in the physics committee, in effect, he was saying that I had tried to get money away from physics, et cetera.

So this was an interesting experience. And even though I took extreme measures to be courteous to him, it didn't work too well.

As a comment, the astronomy budget has since 1984 been going up slower than for most other fields. And therefore by now astronomy is pressed very hard nationally. And there recently appeared on the Internet and the World Wide Web a statement from Sidney Wolff that if this goes on, all the telescopes at Kitt Peak will have to be closed by 1998 or 1999.

So that's the tail end of this business that started in '84.

COHEN: So the NSF has not been sympathetic to astronomy?

SCHMIDT: No, I don't think it has. For some reason, they felt we got too much money in total.

COHEN: Maybe they see all these big telescopes and a small number of people?

SCHMIDT: Well, if you add space science to it—if you look at it globally—let's forget about agencies, let's add Hubble, which is close to $2 billion—then it's easy to say there is enough money. And indeed, the fact that you find so much state money and private money going into astronomy also allows an agency to say, "You have enough."

COHEN: So that was your big job.

SCHMIDT: Being AAS president was a sizable job. And it kept you continuously busy in terms of keeping up with what's going on in the country. For instance, you had to deal with a proposal by a company in Texas to fly human remains in space in a gleaming satellite, so that people could watch Uncle go by once in a while.

COHEN: Things like this did present themselves?

SCHMIDT: Oh, yes. It was even an ex-astronaut who was heading that venture. So there was contact with the Department of Commerce, which was about to give the company a license to do that, and so forth. There were innumerable things that came up.

The next role I had that was time-consuming was as chairman of the AURA board. AURA has twenty-three or so universities and a few foreign affiliates who each provided one director on the board. So the board was a rather large body. It's essentially a management organization, a consortium of universities, with a total number of about thirty-five on the board of directors. And the organization, as a whole, manages, for the NSF, the National Optical Astronomy Observatories [NOAO]—that is, Kitt Peak, Sacramento Peak, and Cerro Tololo in Chile; and for NASA it manages the Space Telescope Science Institute in Baltimore.

COHEN: Who funds them?

SCHMIDT: Well, the National Optical Astronomy Observatories are funded by the NSF, and the

Space Telescope Science Institute is funded by NASA. The universities don't put in any money, except a fee to be a member. They contribute expertise and management oversight and such.

COHEN: Let me understand this. The AURA board is made up of university people, but they supervise these other very large installations.

SCHMIDT: That's right. AURA has contracts with NSF and NASA to manage these facilities. AURA appoints all the people at Kitt Peak, et cetera.

COHEN: Whose idea was it to have the universities manage these things?

SCHMIDT: Well, it's not unusual, I should say, because the National Radio Astronomy Observatory—NRAO—is managed by AUI, Associated Universities, Incorporated, which is very similar. Fermilab [Fermi National Accelerator Laboratory] is managed by URA—Universities Research Association. So this setup is not unusual. The reason NSF wanted things done this way in the first place—many years ago, when the need for national observatories came up—is that the NSF does not manage facilities. So you need a management organization. You can either go for a so-called beltway bandit, of which you have many these days within the beltway who do management; or you can leave it to a university consortium, in which case you get much expertise, essentially for free—because nobody pays us anything for doing all this.

And the budgets for each of these two major parts is on the order of $25 million per year. And the number of employees on each side is around 250 or 300. So everything has to be taken care of—medical insurance, health benefits.

COHEN: But your board must deal just with policy?

SCHMIDT: Exactly. These institutes—NOAO and STSI—have their own staffs and experts in finance and employment.

I became a member of the AURA board as Caltech's representative in 1983, and I remained on the board for twelve years, until the middle of '95, a year ago. I was chairman of the board from '92 to '95. And that was just like being president of the AAS—you had to be cognizant of everything that was happening, which in these systems is an awful lot—that is, the business of the government, OMB [Office of Management and Budget], the agencies, the observer community, which in both cases is very large. These institutions have hundreds and hundreds of clients.

Let me mention in particular three things that were time-consuming, and especially the first two.

The first one was the Gemini project. Mr. Bloch of the NSF, who was perhaps not particularly friendly to astronomy as a whole during one of the G-7 conferences—economic conferences of the top seven developed countries—was heading a subcommittee on international cooperation. And in order to make the point, Mr. Bloch decided that he would not only discuss it in the committee, he would show how it's done. So he chose astronomy. And before we knew it, there was an arrangement that Canada, the United Kingdom, and the United States, would jointly build two eight-meter telescopes for the NOAO—fifty percent of the money to come from the United States, twenty-five percent from the United Kingdom, and twenty-five percent from Canada.

So he set this all up with a special board, called the Gemini board, half the members appointed by the United States and the other half by the two other countries. And he just told Sidney Wolff [the director of NOAO] that this was going to happen. And I think there was relatively little that one could do about it.

So here we found ourselves with the Gemini project, which in itself is nice. On the other hand, it was somewhat disappointing, of course, that the whole United States could not afford more than one eight-meter telescope—that is, we would get half of two eight-meter telescopes—if California can have two Keck telescopes. But anyhow, that was all that the Congress could be talked into.

So we found ourselves, as the AURA board, making an approach to the Gemini board—and that happened in the beginning of my chairmanship—in order to set up a relationship with them, because we would be involved, through Kitt Peak in Tucson, in the construction of these two eight-meter telescopes. And in fact Sidney Wolff soon became the director of the Gemini project, even though she was also the director of NOAO. And this was very complex, because now we had a board—the AURA board—essentially working for another board. And this can be immensely complicated. And there were built-in complications stemming from different international standards and cultures. So it was very interesting, but we did all right and we did get things set up.

But the relationship between the Gemini board and the AURA board was not an easy one. And the major difficulty, really, came when the award for construction of the mirrors of the two telescopes was to be made. This was all very carefully prepared, and bids were collected. The University of Arizona had been engaged in a long-term effort to make borosilicate mirrors, and they had developed this with a lot of NSF support. Most people in the community thought they would get the award.

COHEN: Where was this telescope to be built?

SCHMIDT: Oh, sorry. It's two telescopes: one in Hawaii—and the foundations are already there—and the other one on Cerro Pachon, next to Cerro Tololo, in Chile.

The main thing I remember about this mirror award was that it was very, very surprising. There were three bidders, of which one was the University of Arizona. However, it turned out that the bid of the University of Arizona was very high—higher than the two others, which were commercial ventures. And this was a shock, because everybody had automatically expected that the University of Arizona would be awarded the bid. The review boards that were in place recommended Corning, and Sidney Wolff recommended that to the Gemini board, and the Gemini board approved it. Soon after that, I had my first AURA board meeting as chairman, in April, 1993, in Washington, DC. And the University of Arizona astronomers in the meantime had started an E-mail war against this decision.

As the contractors, AURA could say nothing about the bids. Essentially, everything that the University of Arizona communicated to a large fraction of the astronomy community through E-mail remained unanswered. You cannot respond and make parts of the bidding public. And that led to a very difficult situation. I saw it coming, from what I had heard, and therefore the first meeting I had as chairman of the board was, I think, probably the worst ever. I realized that people were very upset. I don't know why E-mail should get people upset, but apparently they were upset. Human nature—although I found it disappointing that board members should react this way. Anyhow, it happened. So I adjusted the agenda and decided that we should immediately discuss this issue. And for the two-day meeting, I reserved the first full morning for it. An AURA board meeting has fifteen or twenty items on the agenda, you know, and I gave the first three hours to this. It was a very difficult and controversial and unfriendly meeting in the beginning. The first day was really terrible. The second day, we finally came to closure—I'll mention something about that. But by the end, I had spent in the meeting eight and a quarter hours on this one issue. [Laughter] And only overnight did closure start to appear. The gang of people who were so unhappy had somebody approach me late at night and say, "Actually, what do you want, Maarten?" That was a sign of some thoughtfulness. But I never said what I wanted. One of the many committees, or subcommittees, decided that they would come up with an attempt to resolve things, and they organized a breakfast for six o'clock next morning, to precede the meeting. And we finally came to good closure, believe it or not, after this raucous meeting. We came up with two resolutions which were fully supportive of what had happened. And the funny thing was that later our board was severely criticized by the Gemini board for having discussed this at all. Because it was none of our business, they said. [Laughter] Well, if I had considered it none of our business, there would have been an explosion, you know. So this was an exceedingly difficult meeting. As I say, I often seem to have somewhat difficult meetings. But it went well. The people remember it as certainly a very memorable meeting.

One other development started while I was chairman. And that was that gradually, through this experience and through other experiences that I won't go into detail about, there arose a feeling that the AURA board structure might not work awfully well. Too many people. Too large a board. Also, since there are some people on the board—like the director of the Institute for Astronomy at Hawaii, Don Hall, and Peter Strittmatter at the University of Arizona—who very clearly have programs going that often are in conflict with NOAO, the perception existed that there was conflict of interest on the board. And we addressed that while I was president; I gave it great attention, and at times to do so was fairly difficult. Nonetheless, in the last year, the question came up as to whether we should not listen to what the community and also the NSF was telling us—namely, that they had the perception that the conflict of interest was serious. And I finally decided that it doesn't matter anymore whether a conflict of interest actually exists or is just perceived. If your clients—that is, the overall community—as well as the agents think it exists and it's harmful, let's do something about it. So a committee was formed. And all I can say is that it came up with tentative recommendations for reorganization just at the end of my tenure as chairman. And now all these directors who are representatives from the universities are not the board of directors anymore, but instead they get together once a year and elect a board of directors consisting of twelve people. It was a good reorganization, and I'm happy that it's taking place.

COHEN: So now you're finished with that?

SCHMIDT: I'm finished with that, yes.

COHEN: Well, that's a long time you were on AURA.

SCHMIDT: From '83 to '95. But the last three years were all-consuming.

COHEN: But it was not like being division chairman—you were doing your research all these years?

SCHMIDT: Oh, yes, I was doing my research. It was mostly that I was—and just like the AAS job—you have to be aware of everything that's happening, have frequent contacts with many people, and so on. But at least it was a background job, although heavy.

COHEN: You said that you didn't have a great deal to do with Keck.

SCHMIDT: No, that's not true. There is a separate interview about Keck; you have that here. So Keck is taken care of.

COHEN: How do you think things are doing here?

SCHMIDT: I don't think I want to address the whole question of the direction of astronomy at Caltech. It's something that's too current, I would say, to be considered of historical interest.

COHEN: But you may have some feelings about it?

SCHMIDT: I do have feelings about it, but I'm not ready to talk about it, I think. [Laughter]

COHEN: [Looking over Schmidt's curriculum vitae, specifically his honors and awards] It's a lovely list here. Do any of these mean more to you than others?

SCHMIDT: Well, being Oort Visiting Professor at Leiden [in 1991] while Jan Oort was still alive was, of course, a marvelous experience. It is a visiting professorship that Leiden University has set up in the name of Jan Oort.