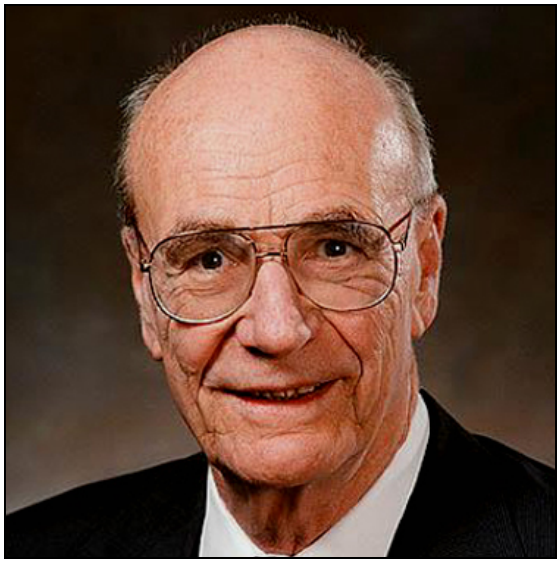

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DONALD E. OSTERBROCK
(1924 – 2007)

INTERVIEWED BY
SHIRLEY K. COHEN

February 10, 2003

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Pasadena, California

Subject area

Astronomy; astrophysics

Abstract

In this brief interview, the American astronomer Donald Osterbrock, who died on January 11, 2007, offers recollections of Enrico Fermi and Subrahmanyan Chandrasekhar in the early 1950s at the University of Chicago. He recalls the atmosphere at Yerkes Observatory, where he got his PhD (1952); his postdoctoral fellowship at Princeton (1952-1953); and the five years he spent at Caltech, first as an instructor (1953-1955) and then as an assistant professor (1955-1958). At Caltech, he worked on gaseous nebulae. He recalls that work and comments on Jesse Greenstein's style as head of the astronomy department. He discusses his various colleagues affiliated with the Carnegie Observatories on Santa Barbara Street in Pasadena, including Walter Baade, Rudolph Minkowski, Armin Deutsch and Maarten Schmidt; his Caltech colleagues Guido Münch, Art Code, and Fritz Zwicky; his two graduate students, George Abell and John Mathis; his decision to leave Caltech in 1958 and help Art Code develop a graduate program in astronomy at the University of Wisconsin.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH DONALD E. OSTERBROCK

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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Begin Tape 1, Side 1

COHEN: What I'd like to ask you about are some of your professors, people like Enrico Fermi and Subrahmanyan Chandrasekhar.

OSTERBROCK: Yes. I was very lucky to be at the University of Chicago when I was. I thought at the time it was the outstanding astronomy and astrophysics department in the United States, and certainly it was the outstanding physics department. I spent three years on the campus. I had been in the Army Air Force as a weather observer during World War II, and in the first year I took a pre-meteorology course—as it was called—which was supposed to teach us all the math and physics it took to get a bachelor's degree in physics, which was what it took to get to meteorology school. The course was taught at the University of Chicago, not by those big names but by TAs [teaching assistants], largely. It counted for being entered in the University of Chicago, so when I came back I could get in there without any problems—and I also could get credit for some of the vector analysis and differential equations and things like that.

And it turned out that all these great physicists were there. So I learned nuclear physics from Enrico Fermi. I learned quantum mechanics from Gregor Wentzel. They were both very interesting people. Fermi was, I think, the best teacher I ever had—very physical, very insightful, and also very demanding. He would give us a homework problem at the end of every lecture—it never failed. We had three lectures a week, so we got three homework problems. I really learned a lot from him. Wentzel was much more formal, organized. Fermi wasn't disorganized, but Wentzel more or less followed the book method throughout, though he explained it very clearly.

I grew up in Cincinnati, and there I had heard [a lecture by] Otto Struve, the young director at Yerkes Observatory. In my third year at Chicago—at that time, I should say, all the teaching, all the [astronomy] professors were at Yerkes Observatory, in Williams Bay, Wisconsin, seventy miles away from the campus. So I had classes in elementary astronomy from people who *weren't* part of Yerkes Observatory—Albert Schatzel and Thornton Page. Page was an extremely good teacher. He had been a Rhodes Scholar himself. He had the concept of one-on-one—talking with students, getting them to say things themselves and do things and try to get the answer rather than telling them. I really profited a lot by him. Then, by my third year at Chicago, Struve was worried that they weren't getting enough graduate students from the campus to come up to Yerkes in astronomy. Struve started, in my third year, a course taught by three outstanding research people—himself, Chandrasekhar, and Gerard Kuiper, each teaching one quarter on the [University of Chicago] campus. They would commute down one day a week. Somebody else who worked with them at Yerkes would come down for a second day a week, and the students would take the course [in Chicago]. So I heard Struve, Kuiper, and Chandra.

COHEN: You must have thought that all astronomers have accents.

OSTERBROCK: Well, that's true. [Laughter] Struve believed in getting the best astronomers, no matter where they were, and his introduction of people like Kuiper, Chandra, Bengt Strömgren, and others created sort of a stir among the old-time American astronomers, who thought you always got your astronomers in America. I think it was a good thing, and I think that's one of the things that Jesse Greenstein got at Yerkes and brought here to Caltech [in 1948]—people from all over the world.

[When I got to] Yerkes [1949], I took courses with all of the people there. Chandra was a very good teacher, very mathematical, but not physical enough for me; he didn't teach us in physics terms. I thought it was a bit formal, but I got a lot out of it. William W. Morgan, who was there, was doing spectral classification—sort of the complete antithesis of Chandra. Nothing mathematical, all what you could see on the spectrum, classification, and very little about theory. But he taught me to observe, and I think I got an appreciation of that kind of astronomy, too.

COHEN: It's interesting that all these people taught, too.

OSTERBROCK: Yes. Well, I wouldn't say they took the teaching real seriously. [Laughter] Up at Yerkes, there were three courses taught every quarter by three different professors. One was on Tuesday, one was on Wednesday, one was on Thursday. Monday was the day we always had a colloquium and Friday was when, if one of the professors hadn't said everything he wanted in his lecture, we got to come back and hear the rest of it.

COHEN: I see. All very organized.

OSTERBROCK: They were all in the afternoon, because the idea was that you might be observing the night before, so they didn't have any [morning] courses.

I took a course from Struve called Stellar Spectroscopy. He had just completed a book at Princeton University Press based on lectures he had given at Princeton [Otto Struve, *Stellar Evolution* (Princeton: Princeton University Press, 1950)]. Struve was a very important man in American astronomy, giving lots of lectures, on lots of committees, doing everything. He had very little time to prepare. We had a lecture one day a week. When he lectured, he would walk into the lecture room—it was a small room, with just a couple rows of chairs and a podium in the front—and he would have the manuscript of his book, typed and separated into chapters. It looked to us as if he'd just dump the pile on the table, pick up one of them—it seemed to us at random—and then lecture from it. They weren't always completely coherent, but there was a lot of good material in them. But at least we learned what we ought to be studying. Chandra, on the other hand, gave very polished, complete, beautiful lectures. Morgan's weren't polished, but sort of the stream of consciousness of his mind and what he was thinking. Kuiper gave very good lectures. And there were several other teachers, so we were very fortunate indeed to be there.

COHEN: I would say so!

OSTERBROCK: The three sets of lectures were all very good, and they inspired not only me but a couple of other students that also started off [with me]. One finished with me, Stewart Sharpless, who was at Mount Wilson Observatory afterwards for a while and at Rochester for many years.

COHEN: So then you did finish up your degree, your PhD.

OSTERBROCK: Yes, I got my PhD at Yerkes [1952] in three years there. I did my thesis with Professor Chandrasekhar. I think another thing that was very good for me was that Struve left Yerkes after my first year there. He was a powerful director; his idea of being a director was being a dictator. He had imported these young men—Chandra, Kuiper, Strömgren, and Morgan—and he told them what to do and they did it. But they became famous, and eventually they wanted to run something too, and he didn't really completely approve of that. Under those circumstances, he decided to leave Yerkes and went to Berkeley [1950], which was good for Berkeley.

COHEN: I see. So he had no choice in this. He had to allow these people to take some kind of leadership.

OSTERBROCK: Well, yes. Among other things, the federal grants in astronomy were just beginning, and so these people could command resources. The University of Chicago is based on departmental faculty running things. Yerkes, because it was isolated, had been much less democratic and much more dictatorial for a long time. But Chandra, in particular, was a favorite of President [Robert Maynard] Hutchins of the University of Chicago. So Hutchins was listening to Chandra. That meant Struve had to listen to Chandra, too. [Laughter]

Anyhow, so Struve left, and Bengt Strömgren then came as director from Denmark. He had been one of the young men whom Struve had imported, long before my time, at the same time that Chandra and Kuiper came, but Strömgren had come saying that he was going to go back to Denmark after three years. Struve, as I just told you, believed that he made the rules, not other people, and he thought he would be able to persuade Strömgren to stay, but he didn't. Strömgren did go back. But then when Struve left, Strömgren was a natural choice to become the next director, and he was willing to come and stay at that time. So he was one of my teachers, too. He was, I think, probably the best astronomy teacher I ever had. He was more physical than Chandra and more theoretical than Morgan. It was a happy medium—at least, for me. I was very lucky. I was at the right place at the right time, through complete accidents of fate.

COHEN: Well, people make their luck a little bit, too.

OSTERBROCK: OK, maybe I had something to do with it. [Laughter]

COHEN: Right. So then Struve went out to Berkeley.

OSTERBROCK: Yes, and he built up a very good astronomy department there, which they didn't really have before that. They were out of date, because they were locked back in the orbital work of the previous administration.

COHEN: And so then you finished up at Yerkes and—

OSTERBROCK: Then I was a postdoc at Princeton for one year [1952-1953].

COHEN: Oh, that's right.

OSTERBROCK: Astronomy then was much more the old-boy network than it is now. Much more than the old-boy network in radio astronomy, when Marshall [Marshall Cohen, emeritus professor of astronomy and husband of Shirley Cohen] started out. [Laughter]

COHEN: Sure!

OSTERBROCK: Strömgren and Chandra worked very closely with Lyman Spitzer and Martin Schwarzschild at Princeton, and I'm sure they told those people that they should take me as a postdoc. I went there, and I especially worked with Martin Schwarzschild on stellar interiors. I enjoyed it very much. Then I'm also sure that—nobody told me—they must have told Jesse [Greenstein], "This guy is a good guy to take for Caltech." Strömgren and Chandra undoubtedly said that to Jesse.

COHEN: Now, you make an interesting remark in this autobiography of yours [Osterbrock, D. E., "A Fortunate Life in Astronomy." *Annual Review of Astronomy and Astrophysics* 38 (2000) pp.

1-33] that Schwarzschild and Spitzer were two of the nicest people you ever met, which wasn't saying much about some of the others. [Laughter]

OSTERBROCK: Well, no, it wasn't Schwarzschild and Spitzer; it was Schwarzschild and Strömgren. They were two gentlemen who were very nice, that's right. I don't mean that any of these other people were bad, but most great scientists are very single-minded—that is, they want to do their thing. That's why they're good at it. And both of those men were very good scientists, too, but they were more thoughtful and considerate, I would say, of others.

COHEN: Oh, OK. [Laughter] You were at Princeton for only a year?

OSTERBROCK: For one year.

COHEN: And it was understood that it was a year? You knew that?

OSTERBROCK: Well, no, I think I could have stayed. I was hired for a year, but in those days the general presumption was that you could stay for a second year if you didn't fail completely, and I certainly hadn't failed completely. I think I could have stayed, but Jesse offered me a job [at Caltech] in the spring, and I took it.

I was very naïve in those days about jobs. You know, when I was at Yerkes I didn't really think about what I was going to do for a job, and then when I was at Princeton I didn't really think about what I was going to do next. It just sort of came out of the blue. Jesse came and talked to me. I didn't realize it, but he was undoubtedly sizing me up. I probably wouldn't have hired myself on the basis of what I told him, but he did. [Laughter]

COHEN: Well, he probably knew all about you before he ever talked to you.

OSTERBROCK: That's right.

COHEN: So you then just picked up and went to California. Had you been to California before?

OSTERBROCK: In the air force, I was at Victorville for about nine months or something like that.

COHEN: So you certainly knew California.

OSTERBROCK: And at that time we went on passes to Los Angeles.

COHEN: And you were already married when you came out to Caltech?

OSTERBROCK: I married after I got my PhD. Irene was an assistant at Yerkes Observatory. She worked for Morgan, and she had trained in business school; she was a very good secretary. She also had learned on the job to work with photography, especially. We were married before we went to Princeton. Our first child was born after we had been at Caltech for a year.

COHEN: So it was just the two of you in 1953, picking up and driving across the country.

OSTERBROCK: That's right.

COHEN: What did you find when you got here?

OSTERBROCK: Well, Caltech was a very good school. This was only five years after the 200-inch [telescope at Palomar] had gone into operation. It was the largest and by far the best telescope in the world. The staff was made up of the best research astronomers in America. It was a very large group, so it was outstanding, and we had many visitors. The Caltech astronomers were the staff of Palomar, and that included Jesse. Palomar operated as a unit with the Mount Wilson people. The second person who came here was Guido Münch, who came in 1950; the third person was me in '53, and the fourth person was Art Code, who came in '56. We all, by being Caltech astrophysics faculty, automatically became members of what they called Mount Wilson and Palomar Observatories. The Carnegie Institution of Washington operated Mount Wilson. They had their own staff who were all the other people you've heard of. [Laughter]

COHEN: Right. That must have been a much bigger group than at Caltech.

OSTERBROCK: There were about fourteen or fifteen people there. Oh, and Fritz Zwicky was also

here, so it was five people here.

Zwicky was very isolated from the rest of the department. His office was right next to mine. I talked to him frequently, but he played no part, as far as I could see, in the discussions of what we were doing. Of course I was very young. Guido was somewhat older than I, but pretty young, too. So Jesse mostly ran the department. He was much more democratic than Struve—I mean, he always asked us things. But I don't remember that we had formal meetings of the astronomy faculty. Zwicky went his own way. Somehow or other, before I came, it had been established that Jesse ran astrophysics, Zwicky could do whatever he wanted to do, and the rest of the people would do what Jesse wanted. [Laughter]

So that's the way it worked. For instance, the colloquia were at Caltech. They didn't have colloquia at Santa Barbara Street. [The reference is to the Carnegie Institution; the headquarters of the Carnegie Observatories is on Santa Barbara Street in Pasadena.—ed.] And all of those people would come down to the campus for the colloquia. We had an astrophysics lunch one day a week at the Athenaeum. All of the faculty here would go, except Zwicky. Some of the physicists who were interested in astronomy would eat with us. All the Mount Wilson people were invited, but generally speaking there was a small group that came that included Walter Baade, Rudolph Minkowski, and Armin Deutsch, who had been a Harvard undergraduate and a Yerkes PhD. Olin Wilson usually came. That was about the size of it. And partly because our faculty was so small, we tried to get one or two Mount Wilson staff members to give a lecture course on their specialty here at Caltech as a graduate course; the deal was that they'd automatically have faculty status at Caltech. Baade taught a course one quarter, Deutsch taught a course one quarter, Olin Wilson taught a course one quarter.

COHEN: And everybody got along in those days?

OSTERBROCK: Yes, quite well.

COHEN: OK, because it turned out at the end that they didn't.

OSTERBROCK: Well, I was here from '53 to '58. I never saw any friction between anybody, except that Zwicky was on bad terms with a lot of the people. But he was in a tough position. He had started out as a physicist, came here, was a very good teacher, didn't really do much

frontier research in physics, then got into astronomy through supernovae and did very well at that. And he did a lot of important things, but the astronomers at Mount Wilson didn't think of him as one of them. He was a very sensitive guy to start with and needed adulation, so it was tough. He wasn't as bad as I'll say, but he wasn't as good as he'd say. [Laughter]

COHEN: [Laughter] That's a good way to put it. How much interaction did you have then with the rest of Caltech? It was a very small place when you were here.

OSTERBROCK: Yes. Not a lot of interaction, but some. Several people in physics were quite interested in astronomy. Leverett Davis was a theorist; he was one. The guy that had the most to do with astronomy was Willy [William A.] Fowler. When I came, his nuclear physics group was already concentrating on astrophysics. That's what he was really interested in. He liked me because he was from Lima, Ohio, and I'm from Cincinnati. That's pronounced "Lima," like the bean, in Ohio. [Laughter]

COHEN: I should know that. I grew up in Cleveland, so I'm from Ohio, too. [Laughter]

OSTERBROCK: OK. He always called me "my old Ohio buddy." So he came to our talks, we talked to him. He got me to give talks on—see, what I had done on red dwarfs had to do with nuclear energy production, which had to do with what they were doing at Kellogg [W. K. Kellogg Radiation Laboratory]. I gave talks on that to his informal group, at his home—so we had a lot to do [with each other].

[Richard P.] Feynman was not really deeply interested, I think, in astrophysics, but he was sort of interested in everything that was going on, so he would often eat with us and ask questions and provide thumbnail theories, which usually had a lot to do with reality I found. [Laughter] He was one smart guy. Bob [Robert B.] Leighton was another guy who was very interested in astronomy. He always joined in our lunches.

COHEN: It sounds like intellectually there was a lot of ferment going on here.

OSTERBROCK: Yes, there was a lot, and in personal relations. We were in very close relations. What I was doing wasn't very directly related to what Jesse was doing on abundances, but it was

on nebulae, and Guido Münch worked on interstellar matter, so he and I had a lot to do with each other. But at Mount Wilson, Baade and Minkowski were really the people I talked to the most about what I was doing, and I would usually go up there and see them once a week and talk.

COHEN: I get the impression that you had to be careful that you weren't choosing somebody else's piece of sky. [Laughter] Is there anything to that?

OSTERBROCK: Not a piece of sky, but a subject, sort of. At that time, one of the most exciting things that was going on was about stellar evolution. It played back and forth between theory, which was what I was doing at Princeton—but not about evolving stars; I was doing it about stars that were stuck on the main sequence—and observations, which were globular-cluster color-magnitude diagrams. Allan Sandage and Chip [Halton C.] Arp had pioneered that here, under Baade's aegis, and Bill [William A.] Baum was brought in to work on it also. I had arrived here after stopping off at the summer school at the University of Michigan, where Baade had lectured, and I was fired with enthusiasm for that kind of stuff, and I thought I'd work on it, but that was a mistake, because these other three people were already working on it. They already had done all the kind of stuff I wanted to do, but it took me a long time to realize that.

COHEN: I see. So nobody, in the beginning, said, "You can't do that."

OSTERBROCK: No, no, there was nothing like that, but I gradually learned that I wasn't getting anywhere with it. [Laughter]

COHEN: I see. Well, how about just joining their group? Was that a possibility or did you have to be invited?

OSTERBROCK: No, I could have gone and worked with them, but I wanted to work for myself. It was much less a world of groups than it is today. It was much more one person doing his own thing. Sandage and Arp were both working, but they weren't working in collaboration with each other, they were working on different things within this same frame, and there was room for two of them. Baum was sort of brought in to do the standard stuff; he wasn't really participating at the same level as them. I never considered working with somebody else. [Laughter]

COHEN: That's right. It's a very singular operation. I don't know if that's true anymore, actually.

OSTERBROCK: No I don't think it is, because there are only a few large telescopes. There are a lot more astronomers. You have to work with other people to be able to get enough data on whatever you want to know about.

COHEN: OK. I was going to ask you about that statement, "Oh, they let me do that without telling me it wasn't a good idea." [Laughter]

OSTERBROCK: Well, Jesse said, "You ought to do something else," and at the time I didn't take it seriously. And he was never a demanding, pushy boss. But I came to realize, as time went on, that Jesse knew a lot that I didn't know. [Laughter] Maybe next time I'd listen a little bit more closely, right from the start.

COHEN: And you taught here also.

OSTERBROCK: Oh, yes, I taught here. I did a lot of teaching.

COHEN: What was your life like in general, in Pasadena?

OSTERBROCK: Well, we were poor to start off with; we lived on Los Robles, first in an apartment above a garage and then in a house that we rented when we had children. But we weren't poverty stricken or anything like that. There was a retired chemical engineering professor from the University of Cincinnati, where my father taught, named Saul Arenson, who lived in Hollywood. He was a bachelor. He lived in Laurel Canyon, when it was back in the wilds, you know. And when we first came, we stayed with him for several days until we found a place to live. We'd go and see him once in a while. We didn't know too many other people outside astronomy. They had plenty of astronomy parties and things like that.

On the other hand, at that time neither of us really liked the area—it seemed dry and hot and smoggy. Backyard burning was still going on at that time. We seemed to be an awfully long way from woods, rivers, and things like that, that we were both used to. We didn't really

like living here, although we liked everything about the institute.

COHEN: How about the ocean?

OSTERBROCK: Well, we went there, yes, but it was a long way away. We would go to Huntington Beach frequently with our kids. You know, at Yerkes Observatory you walk down a hill to the lake. You walk two minutes away from the observatory and you're in the woods.

COHEN: Just to come back to Caltech for a little bit, [Lee A.] DuBridge would have been the president then.

OSTERBROCK: DuBridge was president, yes. He was very supportive of astronomy, a very good guy. I admired him a lot. I didn't have a close relationship with him, but we heard him give talks, saw him, and things like that.

COHEN: Was the department growing at this time? Were more people coming in?

OSTERBROCK: Yes, but not tremendously. We had more students each year. As I said, Jesse was here for two years, and then Guido came. Guido was here for three years, and then I came. I was here for three years, and then Art came. That was the rate of growth. And we were talking about the next professor. In fact, Bev [John Beverley] Oke was the person we all three hoped to get here. And then, when Art Code and I left—

COHEN: You both left at the same time?

OSTERBROCK: Yes, we both went to the University of Wisconsin together [1958].

COHEN: Oh, that's right.

OSTERBROCK: And Bev and Maarten Schmidt came. Maarten was here as a Carnegie Fellow, and I knew him very well. Of course he did the same kind of observing I did, and we wanted to get him. I wrote a book on Walter Baade [*Walter Baade: A Life in Astrophysics* (Princeton:

Princeton University Press, 2001)], and in the book on Walter Baade I write about Maarten that he was the fair-haired boy of Jan Oort in Holland. Before him Lo [Lodewijk] Woltjer had been the star student. Before that, Henk van de Hulst. All great astronomers, and Maarten was certainly a great astronomer. Oort recommended him for a Carnegie Fellowship, and Baade recommended him to [Ira Sprague] Bowen. At least Baade claimed that they had this fellowship "reserved" for Maarten—what happened, I don't know. But there was no doubt about it, Maarten came. Oort also told Baade and Bowen that Schmidt had to come back to Holland because they needed him there. In those days, the directors saw young astronomers as blocks of wood that you push around on the table. [Laughter] Well, Maarten did go back, but he didn't stay; he came back here. I don't know what Baade told Maarten, but Baade, in the two letters I have from him where he gave advice, always gave young astronomers the advice to go where they wanted to go, not be locked up by where they used to be. Anyhow, so Maarten and Bev came after Code and I left.

COHEN: So what really took you away is that you wanted the open country of Wisconsin.

OSTERBROCK: Yes, that's right.

COHEN: You didn't care about the winters there?

OSTERBROCK: At that time I didn't. [Laughter] Over the years in Madison, each winter got harder and harder for me. Also I would say that in those days, when we first came, Caltech was much more rundown-looking than it is now. The campus was all brown. There wasn't much in the way of plantings at all. It seemed sort of like an extension of the Los Angeles desert. I didn't feel anything negative about it, but it wasn't a beautiful place—

COHEN: Like it is now.

OSTERBROCK: Right. Now when I walk around Caltech I say, "Why did I leave this place anyhow?" [Laughter]

COHEN: I see. You know, people talk about leaving Chicago in February and getting off the

train [in Pasadena] in February and the flowers are blooming. But that wasn't true for you?

OSTERBROCK: No.

COHEN: That's interesting that you say that.

OSTERBROCK: We drove out each time in the summer, or early fall, when it was parched. [Laughter]

COHEN: And it was awful. I see. They should start the semester here in January.

OSTERBROCK: In the first few years we were here—in fact, all the years we were here—we would go back in the summer. The first couple of times we flew, and you know, it's a long flight. We flew overnight, because that was less expensive in those days. Some friend would drive us to the airport. It was dry, hot, awful. We'd get off the plane the next morning, and Irene's dad would meet us—not at O'Hare, but at Midway Airport—and in ten minutes we were out in this green, luxurious, jungle-like Wisconsin summer. [Laughter] That was our picture of Wisconsin. I forgot all about the winters. [Laughter]

COHEN: So it really was the weather.

OSTERBROCK: Oh, yes.

COHEN: Because you certainly must have been stimulated in your work here.

OSTERBROCK: Yes, I was. But also, you see, I liked it here. I would much rather have stayed here, astronomically speaking. But Wisconsin had a long tradition in astronomy. They had only a very small department, and they had, at that time, no regular graduate program. The one astronomer was the director, Albert Whitford. I saw him frequently at Yerkes when I was a student, because the two institutions are close to each other, and I had been to Madison a few times. When you went to Madison from Williams Bay, it looked like this beautiful city with all kinds of amenities. So I had told Whitford that if they ever had a job there, I wanted to be

considered for it, because I thought I would like to live in Madison and do astronomy there. Well, it turned out they were thinking of expanding their department. They got this thirty-six-inch telescope, which Whitford built. It was nothing compared with here, but still it was a very good telescope for that time. And they wanted to start a department with more than one person in it, and so they got Art Code as director [to succeed Whitford, who left to become director of Lick Observatory—ed.] and me to come. So it was sort of a challenge to start a new thing.

COHEN: Did you have any sense that at Caltech, here you were with all these superstars, and you were anxious to be your own superstar—which you then of course became? Do you think that had any influence on you?

OSTERBROCK: I think the fact that a lot of these people were quite famous people, and I wasn't, had *some* influence. I had to wonder what my own future would be. Jesse always said it would be fine, that I should stay. The minute I told him I wanted to leave, he said he'd promote me to associate professor if I'd stay. But I didn't want to stay. Also, on a more personal level, in 1956 my mother came out from Cincinnati, after our second son was born, to take care of me and my wife and the baby, possibly in that order. And when she went home, she was in a terrible airplane accident—she and all the people on the plane and all the people on another plane were killed. There was an airplane collision over the Grand Canyon, and there were about 130 people killed.

COHEN: Oh, how terrible!

OSTERBROCK: That happened when I was here. If I had been in Wisconsin, nothing like that would have happened. I didn't think of it in those terms, but probably subconsciously that maybe had something to do with it.

COHEN: Well, I'm sorry.

OSTERBROCK: Well, it was a long time ago.

COHEN: I know, but those things do matter.

OSTERBROCK: It stayed with me for a long time, but I gradually got over it.

COHEN: Yes. I see. So you were here, and it was good, but you didn't feel permanent.

OSTERBROCK: Yes, that's right. I didn't feel part of the community. I did scientifically, but personally I didn't. We had formed no strong attachments with people outside astronomy, though we were friendly with our neighbors. It still seems, to me, not only here but in Santa Cruz, and everywhere I've been in California, that there is less solidarity, you might say—group spirit, or whatever—in California communities than there is in Wisconsin communities, or at least than there is in Madison and Williams Bay.

COHEN: You're comparing those two, but what if you throw Princeton into the pot? [Laughter]

Of course you weren't there very long.

OSTERBROCK: That's right.

COHEN: But we have spent some time in the Cambridge area, and I find it very friendly here compared to the East Coast.

OSTERBROCK: Is that so?

COHEN: Yes. [Tape ends—mid-side]

Begin Tape 1, Side 2 [mid-side]

OSTERBROCK: We were very young in Princeton. I liked Princeton, and I had a chance to go back there from Wisconsin in 1960, '61, but I didn't really want to. I think it had more to do with the fact that I felt I was part of something growing in Madison.

COHEN: It was something you could develop yourself, which was very attractive.

OSTERBROCK: Yes.

COHEN: Well, is there anything more you would like to say about those years that you were here?

OSTERBROCK: Yes. I thought that probably the biggest influence on me astronomically was Walter Baade. He was at the height of his fame, and he had done all these wonderful things. He was basically the person who discovered that stellar evolution works. That got all the theorists working on it. They explained it, and there was playback between theory and observation. He knew everything about all kinds of observing. He encouraged me a lot, talked to me.

COHEN: Now, he was of course at Carnegie.

OSTERBROCK: He was at Carnegie, right.

COHEN: But there didn't seem to be such a division then?

OSTERBROCK: Well, I didn't see him every day. I had to go up there and see him. But I did that. And they had lunch once a week. And Minkowski was even closer to me, because he was in nebulae—not only in nighttime observing, like Baade was, but nighttime observing of nebulae. And he taught me a lot. I was inspired, or guided, a lot by him. He was the son of a medical school professor [Oskar Minkowski], who was called the grandfather of insulin or something like that. Early in Strasbourg, Germany, he was a researcher who discovered the connection in dogs between diabetes and the pancreas. Minkowski's family was Jewish; they had fled from Russia to Germany. They had been assimilated, they called it—he was a baptized Christian. But when Hitler came to power, he had to leave. He couldn't have an appointment anywhere. His wife—I don't know if you ever met Luise.

COHEN: Yes, sure. She was from a Jewish family.

OSTERBROCK: That's right! And her father was something like the chief justice of the German Supreme Court—they didn't really have a Supreme Court, but the highest court in Germany. And he was kicked off the bench, lost everything, and he came with them here. So it was kind of sad. But Rudolph had become completely Americanized. He fitted in very well here.

COHEN: Oh, and so did Luise. She liked it here. She worked for the airplane—

OSTERBROCK: That's right. She was Rosy the Riveter. [Laughter]

COHEN: Right.

OSTERBROCK: Their children had been born in Germany but came here very young and grew up speaking English. Baade, on the other hand, had no children, and he and his wife always spoke German at home. He could speak English very well—tremendous use of slang and his own funny words that he threw in every now and then. But they never became part of the American scene. We knew Guido. Guido's first wife was from Williams Bay, Wisconsin.

COHEN: Oh, is that right?

OSTERBROCK: He had met her there and married. She was younger than me, younger than Irene, younger than Guido.

COHEN: So his children are with that first wife?

OSTERBROCK: Well, he's got different children with different wives. [Laughter] So we knew them quite well, and other astronomy people. We didn't go to too many Caltech things. I don't remember that there were so many lectures, and certainly not so many musical events aimed at the public as there are now.

COHEN: Oh, I think that has grown considerably over the last years.

OSTERBROCK: The first year I was here, I observed only at Mount Wilson. The time was given out on the basis, supposedly, of the importance of your scientific program. I probably got more observing time on Mount Wilson than on Palomar, so I went up there frequently to observe. I knew the people up there very well, though I knew the people at Palomar too.

COHEN: At that time, the lights of the city were not really spoiling anything?

OSTERBROCK: Oh, they were a problem, but not nearly as much of a problem as they became later. Here it was not as dark a sky as at Palomar, certainly.

I learned a lot about gaseous nebulae when I was here. I had two graduate students do their PhDs with me here. The first was George Abell, who had been a graduate student here for some years. He had taken all the courses before I came and had already started his thesis ["The Distribution of Rich Clusters of Galaxies," 1957], which was based on observing with the forty-eight-inch Schmidt. He was taking the plates for the sky survey and then could use them himself. The second was John Mathis, who came and took courses with me, and then he did his thesis with me ["The Abundance of Helium Relative to Hydrogen in the Orion Nebula," 1956]. They were both very good theses, so I was pleased with them. And I taught a lot of other people who went on into other branches of astronomy than what I was doing.

COHEN: How often did Fred Hoyle come here in those days? Was he visiting back then?

OSTERBROCK: He came for a quarter to us as a visiting professor. He came just about every year to Willy's [William A. Fowler] lab to work. I don't know the arrangement, but sometimes I imagine it was during vacation times and they paid him something for travel and whatnot. Other times he came for a quarter. Geoff [Geoffrey] and Margaret Burbidge also came to work with Willy. They were at Yerkes at the time. They were working on the abundance project. T. G. Cowling came just for a quarter; Ludwig Biermann came just for a quarter. We had one visiting professor from abroad in the spring, usually, and two people from Mount Wilson teaching one quarter each.

COHEN: Now, did you have all of Robinson [Henry M. Robinson Laboratory of Astrophysics] in those days?

OSTERBROCK: No, no, there was a lot of other stuff [going on there]. A lot of the math was in Robinson when I came here. They were along the first floor, where you go out to the little square that's out there. The basements were practically unpopulated; they were more or less storerooms for old stuff. But all of the second floor was astronomy offices. On the first floor, there was a big classroom on one side of the hall, and small classrooms on the other, and a library, and the department office was on that floor. Jesse had one of the end offices.

COHEN: Well, he remained in that office, I think.

OSTERBROCK: Yes, for a long time. Back then he had his own assistant, Mildred Shapley. She was the daughter of Harlow Shapley.

COHEN: Oh, really?

OSTERBROCK: He had been one of Jesse's teachers. She worked as Jesse's assistant, doing reductions and measurements and stuff like that for him. Dorothea Davis was the astronomy secretary. And Robert Bacher was the head of the Division of Physics, Math, and Astronomy during the entire time I was here.

COHEN: I think he was quite enthusiastic about astronomy.

OSTERBROCK: Yes, he was enthusiastic about astronomy. He was an OK guy, but he was rather brusque. DuBridge was much more of a diplomat, and so I admired DuBridge and didn't think much about Bacher.

That reminds me of an amusing experience. At the time I left Caltech, nobody had left astronomy before me—of course, there hadn't been many astronomers here. So I told Jesse, after they offered me the job [at Wisconsin] and I'd said I'd take it. Jesse tried to persuade me to stay. I appreciated that, and I told him I'd like to stay but I did feel I wanted to do this. OK. So it turned out that that summer the American Astronomical Society meeting was going to be in Madison, Wisconsin, right where I was going, and the meeting started on June 30th, when I was on the faculty here, and it ended on July 3rd or something like that, when I was faculty there. And at that time at Caltech, if you went to a scientific meeting and gave a paper, they'd pay your way for one round trip. So I asked for one round trip, but I wouldn't be coming back. [Laughter] And Bacher called me in and really sort of raked me over the coals for daring to leave Caltech. [Laughter] So I paid my own way. [Laughter] [Tape ends]