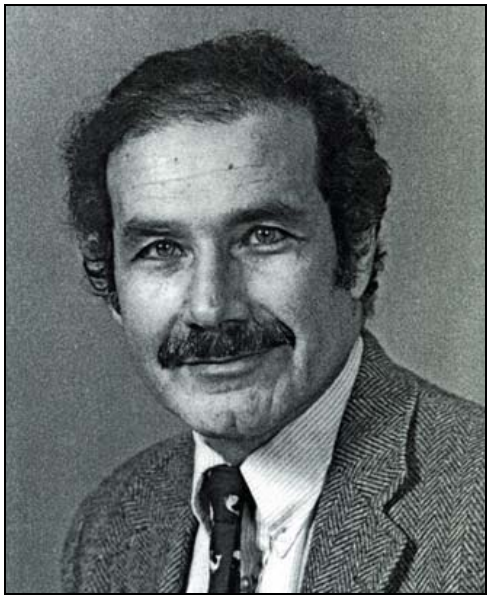

Photo taken 1977

HAROLD ZIRIN
(1930-2012)

INTERVIEWED BY
SHIRLEY K. COHEN

February 3, 10 and 17, 1998

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Pasadena, California

Subject area

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Abstract

An interview in three sessions, in February 1998, by Shirley K. Cohen with Harold Zirin, Professor of Astrophysics, emeritus, in the Division of Physics, Math and Astronomy at Caltech. Dr. Zirin received his undergraduate and graduate education at Harvard (AB, 1950; AM, 1951; PhD, 1953). He joined the Caltech faculty in 1964, became Chief Astronomer at the Big Bear Solar Observatory in 1970 and Director in 1980.

The interview briefly covers Zirin's youth and early education in New York City and Bridgeport, Connecticut, and notes his youthful interest in astronomy and success in school. Recalls Harvard astronomers Bart Bok, Harlow Shapley, Armin Deutsch, Donald Menzel. PhD work on stellar opacities under Philip Morse at MIT with Harvard's approval; leads to first job at RAND Corporation and first move to California, 1952-1953. Denial of security clearance based on father's membership in Communist party sends him back to Harvard for postdoc position. Move to Colorado to High Altitude Observatory and beginning of solar

observing; reminiscences of S. Chandrasekhar, G. Munch. Recruitment to Caltech by J. Greenstein, R. Leighton, 1964. Discusses history of solar observing at Mt. Wilson Observatory. Site survey for new Caltech solar observatory; role of astronomer Sue Kiefer; selection of Big Bear Lake site in San Bernardino Mountains (1967). Story of construction and operation of Big Bear Solar Observatory, concluding with its transfer to New Jersey Institute of Technology (1997). Relates episode of grievances by Sara Martin and other women against him for discrimination (mid-1990s). Discusses decline of solar astronomy at Caltech and elsewhere in favor of other directions in astronomy and cosmology.

The interview concludes with observations concerning Caltech's presidents and other administrators; Zirin's activities on committees and involvement with institute governance and finance. Wife Mary quits as Russian teacher at Caltech. His international connections; 6-month period in Russia's Crimea.

Administrative information

Access

The interview is unrestricted.

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INTERVIEW WITH HAROLD ZIRIN

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Harold Zirin By Shirley K. Cohen

Pasadena, California

Session 1	February 3, 1998
Session 2	February 10, 1998
Session 3	February 17, 1998

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COHEN: Good afternoon, Professor Zirin.

ZIRIN: Good afternoon.

COHEN: I'm delighted to have you here to talk to us. Let's start by having you tell us about your childhood and your parents. Where did they come from? Where were you born?

ZIRIN: Well, my parents came from Europe—my father from Russia and my mother from Galicia. They met and married in this country. I was born in Boston, and very shortly afterwards they moved to New York. I grew up on Charlotte Street in the Bronx, which is that street where former President Jimmy Carter used to stand amid the bombed out buildings. They now have a little housing project there which everyone shows as an example of what you can do with affordable housing. I started school there.

COHEN: What did your father do to feed the family?

ZIRIN: He was mostly unemployed, but at that time—I guess I was about four or five—he sold vegetables on the street. I remember going with him on his horse and wagon down to the rail-yards around 125th Street [in Manhattan] to buy potatoes and other vegetables. Then he'd sell

them on the street. He was not well suited to business at all. He was a revolutionary—or at least he was an enthusiastic communist. That was where his heart and interests lay.

COHEN: Did he have any formal education?

ZIRIN: I think he went to some classes at CCNY [City College of New York].

COHEN: But not in Russia?

ZIRIN: I think he had gone to some kind of high school there. I don't think it was a gymnasium though. He was always vague about it, so I never knew exactly what it was. As I said, he did take a few courses at CCNY, but he was more interested in revolution and the communist party.

COHEN: So he was a party member?

ZIRIN: Oh, yes. Some of my earliest memories are of being taken to party meetings. Little kids were often taken along to meetings. He was pretty serious about it. Then in 1935, when I was five, my elder sister, who was the apple of their eye, died. As a result of this shock, they decided to move to Bridgeport, Connecticut, where my mother's first cousin had a furniture factory. My father was to work for this cousin, Phil, and he did so for most of my early years, very much against his will. It was a macho thing—that he felt bad about having to work for a relative whom he didn't respect. But my father always encouraged me in my studies and things like that. He was somewhat educated—I mean, we talked about some academic topics and he was knowledgeable about them, but he was very much affected by the difficulties he had had in this country. I've been reading the latest collection of stories by Bernard Malamud, in which the men of that time are all similar to my father. They came over here, and instead of being the boss of the family, they suffered a loss in status. For me it was great. I started in school a year too young—Jewish mothers used to push their kids to do that. At first, I got into all kinds of trouble in school. They would send me home all the time for misbehaving. When we moved up to Connecticut I missed a year. I guess my parents just didn't know what to do, so they didn't register me. So I ended up being in the normal grade for my age, in regular school, and I did very well. Then I skipped a grade, so I was once again a year younger than most of the students.

When I was about eleven or twelve I got real interested in astronomy. I remember one night when I was in eighth grade. I was twelve or thirteen years old. One of the kids in the class had a telescope, so we went to his house and looked through his telescope at the moon. When I saw that, I remember saying, "I want to be an astronomer."

Last year I went to my fiftieth high school reunion and this kid was there. Noel Westerman, I think his name was. And I said to him, "I remember the telescope, because you had it nailed onto the side of the barn." And he said, "Oh yeah, I didn't have a very good mount for it." It was sort of astonishing to see him again.

COHEN: Did he ever do anything with astronomy?

ZIRIN: No. I don't remember what he does now.

COHEN: But he put you on the track.

ZIRIN: That's right. In high school I was already excited about it. We had a pretty good science teacher. All of our teachers were Irish maiden ladies, and this one was Miss Cassidy. She assigned a few of us to teach separate sections—to lead sections of the class. I remember doing something on galaxies because I had gotten all these books on them. I think I totally bored the kids. In high school I also built a telescope. I ground the mirror and everything.

COHEN: You must have had encouragement, because that was expensive.

ZIRIN: My father did the mechanical part, which I didn't know how to do. I remember we went down to the Bronx to buy a kit from some guy who was selling them. Then we went back up to Bridgeport, and I worked on the telescope for quite a while. That was my project for the Westinghouse science talent search.

COHEN: So you were in high school at this time?

ZIRIN: Yes, I guess I finished the telescope my junior year in high school. It was a fairly sophisticated project—particularly the testing of the mirror. Then I started observing variable

stars. I was really enthusiastic about it. As it happened, that telescope was what led me to go to Harvard.

COHEN: Did you win the Westinghouse?

ZIRIN: I was one of the top ten.

COHEN: Good enough.

ZIRIN: So was Gordon Newkirk, who was a solar astronomer with me at HAO [High Altitude Observatory]. I remember that my high school teacher, Miss Cassidy, was very upset that I was not first and felt that it was due to anti-Semitic prejudice. She did not say this to me, because I didn't see her afterwards—somebody told me later. This was Miss Cassidy, the Irish maiden lady. [Laughter] Anyway, the Westinghouse award led to my going to Harvard, because the Harvard people used to come down each year and recruit winning high school students from that contest—a clever thing that we at Caltech could do, too. Usually it was Harlow Shapley who represented Harvard, but Shapley was away doing something else and Bart Bok came. I was overwhelmed by this well-known astronomer coming to me. It was well past the time to submit applications for admission to Harvard, but he said, "Don't worry," so I submitted my application. I think it was in May, because it was already getting hot.

COHEN: Had it even occurred to you to apply to a place like Harvard? Your family would not have had the resources.

ZIRIN: We didn't have the resources for me to go away, but Yale was kind of down the street—it was only twenty miles away—so I thought I had a chance to go there. I had also read the Caltech catalog backwards and forwards, but that just seemed unattainable. The other thing that happened around that time was that Pepsi-Cola started a scholarship program, with two scholarships in each state.

COHEN: What year are we talking about?

ZIRIN: 1946. The Pepsi-Cola scholarships were a really good thing. You see, the Westinghouse awards didn't amount to much. They are worth more now.

COHEN: It was the honor, I think.

ZIRIN: Yes. I never would have been approached by the Pepsi-Cola people, if it hadn't been for the Westinghouse. So I won one of the Pepsi-Cola scholarships for Connecticut.

COHEN: This was all because of your telescope?

ZIRIN: Well, the Pepsi-Cola scholarship was based on the academic record, and there may have been an exam. I don't remember now, but I suspect there was. You know, I was sort of a hot-shot at exams. So the Pepsi-Cola scholarship gave me the money to go to Harvard, because it paid your tuition and gave you a monthly allowance. It still was tight, because my parents had almost no resources—my father was still working in the furniture factory. Harvard also gave me a very small scholarship. Yale also admitted me with a small scholarship. Incidentally, I was turned down by MIT, and to this day I don't know whether it was a matter of a *numerus clausus* [i.e., a quota], which I believe they still had at MIT in '46, or my interview with the local MIT alumnus, who was the president of the Warner Brother's Corset Company. Maybe he was unimpressed by me—I don't know. But MIT rejected me.

COHEN: Did it come into play that your father was a member of the communist party?

ZIRIN: Not until later.

COHEN: Nobody cared about that?

ZIRIN: Nobody cared about that in '46.

COHEN: I gather he was still a member of the party at this time.

ZIRIN: Yes, I think he was a member until they threw out Earl Browder in 1948. I don't remember when he quit formally. In any event, I didn't get into MIT, but that was just as well, because Harvard had a big astronomy department. Yale had a very small one. In fact, we had gone up and driven around the Yale campus in New Haven, and I saw their telescope, which was a pretty ancient thing. Although I would have gotten a reasonable education at Yale, I probably wouldn't have gone so far in professional astronomy had I gone there.

COHEN: So you had money from the Pepsi-Cola people?

ZIRIN: Yes. I also had $100 per year from Westinghouse—and $100 went far, because the whole tuition was $400 per year—and I had $100 from Harvard. Then I won a prize at Harvard. The great thing that I remember was that I lent money to Giovanni Agnelli, the guy who has been the owner of Fiat for quite some years now. He was a student there at the time, and a friend of my roommate. I remember lending him $10 at one time.

COHEN: Was that before he came into family money? Or did you just have loose cash?

ZIRIN: The family kept him on a very tight string. He lived in a barren room with a picture of his father with Henry Ford on the wall. But that was the only occasion that I had money to lend. [Laughter]

COHEN: So you went to Harvard as an undergraduate.

ZIRIN: Yes, and I had a wonderful time there—I really enjoyed it. There were so many different things you could do, so much variety. I remember the great feeling when I first got there of having come to a place where it really counted to be studying something. Of course, that hadn't been true in high school. Also I was happy that I'd have some more academic competition, because I loved that.

COHEN: There was none in high school?

ZIRIN: Either there was none to begin with in high school, or I had outstripped the competition. Actually, when I went back to visit my high school class it came out that they had voted me "most likely to succeed," and according to their likes I had [laughter]—they had heard me on NPR [National Public Radio] and seen me on TV. Anyway, I went to Harvard and I got into doing astronomy right away, and taking classes, and roommates, and drinking, and women [laughter]—that sort of thing. I played some football and participated in various other activities, so that was very good. And I think the education was quite good, despite the fact that most of the professors were famous for not being interested in teaching. In astronomy they were quite interested in the students. By the time I graduated I had written two or three papers already.

COHEN: Who were the people that you remember?

ZIRIN: I guess the one I worked most closely with was Bart Bok. I wrote one or two papers that sort of were in his line, which was interstellar matter. Shapley was a little distant and occupied with politics. Armin Deutsch, whom you know, was teaching then, and I did some observing for Armin on peculiar A stars. I think I wrote my bachelor's thesis on observations of peculiar A stars I made up there. So it was mainly Bok and Armin Deutsch. Donald Menzel was a little distant. Fred Whipple was quite nice, but he was working in a different sphere. They were all quite good astronomers, but a little bit too occupied with the politics of getting grants—you know, getting funding from Washington, and all that sort of stuff. But, really, these people were accessible, and they were pretty smart. I took a lot of physics courses—I think just about everybody I had for a teacher in physics got the Nobel Prize eventually. There was [Edward] Purcell and [Julian] Schwinger and [Norman F.] Ramsey. One who didn't was Herbert Goldstein, the guy who wrote the famous book on mechanics. The next year, after I graduated, I met Noel Corngold. A lot of famous people were in that class of '50. [Henry] Kissinger and Zbigniew Brzezinski were in that class. [Note: Brzezinski received a BA from McGill in 1949 and a PhD from Harvard, where he was a graduate student resident in Leverett House, in 1953—*ed*.] I knew Jim [James] Schlesinger quite well—we used to play a lot of bridge together. And Bill Green, who later became a congressman. It was a huge class.

COHEN: This was all men at this time?

ZIRIN: It was all men. There were a few women in the classes. Women from Radcliffe could come to Harvard classes; but there were very few women in science classes at that time—nor were they in the other courses I took. Most of the male students were in the humanities, so I learned a lot about the humanities from roommates and friends. These ties have lasted very long—I still go and visit these guys. There were some who didn't even live in my house. It's true that life at Harvard was centered around your house—just like here, but not to the same extent. I knew quite a few people who were in the other houses, mostly because I had met them as a freshman or doing sports.

COHEN: Did you have a sense of Harvard being a gentleman's school? Because you certainly did not come from the same background that most of the students did.

ZIRIN: No, but there was a dichotomy. About twenty-five percent of the students were called the "white shoe boys," or sometimes the "club members." They belonged to these fine old clubs. There were even a few in the same entry of Leverett House as we were, but I just never got to know them. One student I never quite placed was Gavin Miller. Although he lived out here, just next door in San Marino, I never saw him. He died just recently. So there was a difference—you just didn't know those people, basically.

COHEN: So the gentlemen were always somewhere else.

ZIRIN: Yes—but we all tried to behave like gentlemen. You had to wear a tie to meals. There was a lot of sitting around over meals—not like here, where they get out in ten minutes, which is really unfortunate. I don't know if they still do that or not, but there was just a tradition of people sitting down and taking time at dinner.

COHEN: You were expected to sit down to dinner and behave?

ZIRIN: Well, not only expected to—you did. And you mostly discussed different topics: girls, sports, politics, academics, and so forth. So it was very good.

COHEN: It was civilized.

ZIRIN: Very civilized, that's right. And just quite nice. It actually continued into graduate school. Mealtimes were maybe not quite so formal in graduate school, but the routine continued there, too.

COHEN: Did it ever occur to you to go somewhere else for graduate school?

ZIRIN: Well, Jesse Greenstein tried to talk me into coming out to Caltech. He came out to Harvard. He's forgotten it, but he took me to lunch—or maybe it was supper—at the Wursthaus.

COHEN: Now, this was when you were a senior?

ZIRIN: That's right. I don't remember how he knew about me. He may have taken a couple other fellows out at the same time. Anyway, he tried to recruit me, but he frightened me off by telling me about these qualifying exams I would have to take if I came out here, and that seemed like something I didn't particularly want to do. But then the astronomy department at Harvard invited me to continue and promised me support—which wasn't so easy to come by in those days—if I would stay there.

COHEN: And you were very happy there.

ZIRIN: I was very happy, and the offer was sort of an honor, so I simply stayed, which had some other advantages. As an undergraduate, I had taken some extra classes, so I finished my formal requirements for graduation in three and one-half years, and in my last semester was able to take graduate courses, for which I got graduate school credit. Because I had already had half of the courses I needed, I was able to get my PhD in two years. So, although I was a teaching fellow the first year, and that took a lot of time, I had a head start.

COHEN: Well, but you had to have a thesis. You had to do some work.

ZIRIN: I did my thesis. Actually, that was an accidental thing, too. After I had gotten the BA, I went back to Harvard in September of 1950 to see Menzel. You see, I was interested in theoretical astrophysics then—everyone wanted to be Einstein. So Menzel said, "I'm really very

busy and can't supervise you," which I thought was a pretty dirty thing to do after recruiting me. Then he said, "But there's a professor down at MIT I'd like you to meet who has a project that I think you might find interesting." And he took me down to meet Philip Morse—a very well-known physicist, who also taught Feynman, by the way. I talked with Morse about the project, and then he said to me, "Well, let's make an appointment for every Monday to meet on this." He said exactly the same thing to Feynman when Feynman was his student. That was the way Phil did business.

COHEN: That was his style.

ZIRIN: Yes. He was a wonderful man. And I could usually—almost every time—see him.

COHEN: What was the project about?

ZIRIN: Stellar opacities. You see, the energy is produced in the center of the star and the photons which are produced there have to get out of the star. The rate at which the photons leave the star is critically determined by how rapidly they get absorbed, and that's called the opacity. So it was an important aspect of the structure of stars. That is how I got my first job, in fact. First of all, Phil Morse was on the board of the RAND Corporation. He had been the founder of RAND's Operations Analysis department in the US. Secondly, opacity was very important in building bombs, because the energy leaves the center of the bomb due to radiation, controlled by the opacity.

COHEN: Was this a theoretical thesis?

ZIRIN: Yes, it was theoretical.

COHEN: So you didn't go off and do observations anywhere?

ZIRIN: I did observations as an undergraduate, but I really was aiming to be a theoretician. It was just that Bok taught me observing, and I enjoyed it as I always had; but I thought that the main thing was to be a hero theoretician, and the work with Morse was completely theoretical.

So I would see him every Monday, which meant that Sunday night I would always work real hard to have something to show him. [Laughter] He was a very good advisor.

COHEN: Was that cooperation between the two schools common?

ZIRIN: I have a feeling that Harvard and MIT had agreed that MIT wouldn't try to compete in astronomy—which of course they're doing now—and that they would somehow work together. It was just very straightforward and simple. MIT welcomed me. In fact, that's another reason I finished so fast. I just don't remember whether I was in my first or second year of graduate school, but when Morse said, "Well, I guess you'd like to write a thesis on this?," I said, "Sure." All I wanted to do was get out of graduate school.

COHEN: I see. So you started writing your graduate thesis almost immediately.

ZIRIN: I passed all the courses, and then I passed the qualifying exam in my first semester of graduate school. So I got all that out of the way.

COHEN: That's kind of phenomenal, isn't it?

ZIRIN: Well, I was probably one of the younger PhDs to get out of there. I was just simply a good student. Whether I'm a good astronomer or not is another story. [Laughter] But as a student I think I was fairly phenomenal.

My first year in graduate school at Harvard I was a teaching assistant to I. Bernard Cohen together with Noel Corngold. So Noel and I go way back together.

COHEN: Who was the other person you mentioned?

ZIRIN: I. Bernard Cohen. He's a well-known historian of science. He's still alive, and he's rather elderly now. He gave a course called NatSci 110, or something like that. It was sort of physics for poets. Noel and I would sit there listening and shudder, because he got a lot of things wrong. [Laughter] Well, you know, a historian of science doesn't have to be so good as the

scientist, and I guess for poets accuracy doesn't matter so much, although he had some very good students in that class.

COHEN: Were you a teaching assistant in that class?

ZIRIN: I was a teaching assistant in that class. That experience actually led me to never contribute to the Harvard graduate school. I was teaching three-fifths time—three classes a week—and I was taking three courses. You weren't allowed to do that. If you taught half time you could take three courses, but not if you taught three-fifths time. As the year wound on, somebody got wise to this, and the solution was simply that I would continue to teach three-fifths time, because there was nobody else to take the sections, but they would only pay me for half time. That was the way they treated students in those days. And worse still, the students were dumb enough—myself included—that they put up with it. I just didn't think that was nice, and I think I appealed it and got turned down. I've always resented that. But I wasn't going to desert them, although perhaps I should have. Anyway, I think my theory was keep your head down, don't make trouble, and they won't notice you. And that's what happened. I don't think they had any idea that I was that close to finishing. At that time also the Korean War was going on, and I was very concerned with staying out of it. My thesis was not quite done when I left, but I had finished everything else. Above all, I wanted to stay out of the war.

COHEN: So you had to go to work someplace.

ZIRIN: That's right. I had to go to work. [The] RAND [Corporation] seemed ideal, because they had a military connection.

COHEN: And it was through Morse that you got involved with RAND?

ZIRIN: Yes. Morse was on the board of trustees. I had tried for a couple other jobs—in fact, I worked at Air Force Cambridge for a while, which would have kept me out of the war. But RAND was so glamorous that it seemed a lot better than Air Force Cambridge.

COHEN: Had you been to California before?

ZIRIN: No. They flew me out here for an interview. A uniformed officer greeted me at the bottom of the stairway out of the plane. It was a totally different world, you know. [Laughter] I had been on planes before, but to come to California and have some guy take you to a hotel! I don't know if the Georgian Hotel, where I stayed, still exists. It was right on the ocean.

COHEN: In Santa Monica?

ZIRIN: Yes. I think it became an old folks' home later. But I remember, coming there from Cambridge, it was just a different world. And the people at RAND were very interesting people, particularly Herman Kahn, who was there then, and the Latter brothers [Richard and Albert], and various other people. The boss was Ernie Plesset, who was Milton's brother. You may remember Milton Plesset.

COHEN: Sure.

ZIRIN: Milton used to come by RAND once in a while. But I never got a security clearance because my father had been a communist. Let's see, should I say anything else about Harvard? I had some great friends there. I was very friendly with Tom Lehrer. There was a group of about eight or ten of Tom's friends in grad school who would eat together all the time, and Tom used to get theater tickets for us. You know, he kept an eye on all the plays that opened in Boston.

COHEN: He was a student at MIT at this time?

ZIRIN: No. He was a student at Harvard. He was trying to be the oldest graduate student in the history of Harvard. They used to have a way of marking your time as a student there. You would be G1, G2, G3, etc. And he was up to G9.

COHEN: He had been there nine years?

ZIRIN: Something like that, yes. He didn't want to go away. I think he eventually finished the PhD. There was a bunch of us who knew him when he did the "Physical Review," the first revue

with all his original songs. He performed them with Dave [David] Robinson and Lew Branscomb. Dave is retired now, but he was vice-president of Carnegie Corporation and provost of NYU. Lew Branscomb built JILA. People like that were Tom's close associates. In fact, Guido Münch was part of the group one year, when I was a postdoc.

COHEN: So it certainly wasn't all work and no play.

ZIRIN: No, except when I had to finish my thesis, when I pretty well worked from eight to five [o'clock]. I felt I'd be stronger that way. At that time, when I was doing the calculations, I didn't even have a desk calculator, but a lady working for Harlow Shapley had one. So I had to go in in the evening and do computation—that was the only time I really worked late, except, of course, when I was doing some astronomy observing. But I don't think I did too much in graduate school. I spent a couple summers up at the observatory. It was a lot of fun, and nice, but I guess I was still driven by the competitiveness. At any rate, I wanted out. I think I should say that, although I learned a lot from my thesis, I don't think I got the level of education at Harvard that I would have gotten if I had come here and studied with Jesse. But I don't know. It's hard to say.

COHEN: Do you mean that the Harvard faculty were just not as involved with the students as the Caltech faculty was?

ZIRIN: That may have been. Although Morse was certainly involved, the others were not very. Also, the other graduate students weren't all that good. Now, a number of them later on became observatory directors and things like that, simply because Harvard was turning out astronomers back then, but I can't say that there was a whole lot that I learned from the other students. As a kid I had heard of the Harvard observatory and had seen the Harvard books on astronomy. I thought it was a great institution for the field. But in fact the really good people were elsewhere—mainly Princeton and here. On the other hand, Bok was very taken with papers I had written as a senior, and he introduced me to [Jan] Oort and [Lyman] Spitzer, whose work I had criticized in those papers. When Oort and Spitzer came to Cambridge as visitors, Bok sent them to meet with me briefly to find out what I had done and so forth. That was quite a good

thing. The papers all got published in the *Harvard Bulletin*, which was the wrong place to publish them, because nobody reads the *Harvard Bulletin*. [Laughter]

COHEN: How would you know that then?

ZIRIN: That's right, I didn't know. And I think Bok could have had me put them in the *Ap J* [*The Astrophysical Journal*]. But the faculty—especially Bok—were impressed enough by my work to drag these famous guys in to talk with me about the subject. Bok was a good guy. Anyway, I got my degree and came out to California and RAND. I was very impressed by it. I loved California. I lived in a motel on Santa Monica Boulevard. On the weekends we sat by the pool and watched the girls go by—you know, that sort of thing.

COHEN: Did you make friends at RAND, or did you already know people there?

ZIRIN: I just made new friends. I was in what was called the Nuclear Division. In fact, I just got a call the other day from Sam Cohen. I guess I mentioned it to you.

COHEN: Yes, but go ahead.

ZIRIN: Later on Sam became the inventor of the neutron bomb. I had house sat for him up on Sunset Boulevard. I was also friendly with Herman Kahn. There was a small group of us bachelors who would go out and eat dinner together.

COHEN: What year was this now?

ZIRIN: This was 1953.

COHEN: And they still didn't care that your father was a communist?

ZIRIN: Well—that's when the trouble started. There were two clearances you needed to work there—an air force clearance and a "Q" clearance, which was from the Atomic Energy Commission. An air force clearance did not concern your family; it just concerned you. The

Atomic Energy Commission demanded that your family be spotless. And so, about the middle of my first and only year at RAND, I got the reply to my application for a security clearance. There were three accusations in the response, and they asked me to appear for a hearing on them. The first accusation was that I had been seen at a communist party meeting in 1940, when I was eleven years old.

COHEN: Which was true.

ZIRIN: It may well have been. I don't know. It probably was. But you don't exactly remember—I was an eleven-year-old kid. The second accusation was that my father had worked for Henry Wallace, who was a communist. (He wasn't, but he was a foolish person.) The third accusation I don't remember now. Anyway, I went up and had a hearing in San Francisco. There was one part of it that I remember, where they asked me a lot of questions. A Harvard friend who was returning from Korea—Chase Troxell, a social friend—testified for me. They asked him, "When you were undergraduates, what was Zirin's politics?" And Chase said—I remember this like it was today—"Oh, the usual liberal line." So they said, "What do you mean by that?" And Chase said, "Somewhat to the right of Harry Truman." [Laughter]

You know, in those days they really thought they could pin people down by where they were from, so they asked Chase, "Well, what does your father do?" And he said, "Well, he's an investment banker." "With whom?" "Dillon Read Company." Dillon Read was a famous banking house. So Chase was an impeccable witness for them, and here I was to the right of Harry Truman. But ultimately I got a letter saying, "We can't reach a decision. This will be passed on to the next higher board." And Plesset called me in and said, "Well, there are still plenty of young postdocs we can get hold of, so you'll have to leave. We're not going to bother fighting it through for you."

COHEN: So that's how it was.

ZIRIN: I didn't fight things—I didn't feel my own inner strength. I was funny that way. And so I wrote to Menzel and told him about what had happened. Historically, Harvard had offered refuge to all kinds of people, because Shapley was somewhat of a leftist, and they were all anti-

McCarthy. This was the hey-day of McCarthy. In fact, I have a striking recollection of coming back to Cambridge to somebody's house—I don't remember who; not everybody had television then—and there was a television tuned to the Army-McCarthy hearings. So I was very sensitive to that. But in any event, that was probably the best thing that ever happened to me, because otherwise I'd still be working there. Actually, most physicists left RAND when RAND went social scientist. And they became—I think they were called—Nuclear Energy Associates Incorporated, or something. They just sold themselves to the AEC rather than selling it cheaply [i.e. working for less money, with RAND as the middlemen]. Herman Kahn went and started the Hudson Institute. And the Latter brothers started this nuclear development company. In fact, Sam Cohen went to work for the Latters, too, because the Government told RAND to stop working on tactical nuclear weapons and Sam couldn't stand that. Sam was a very aggressive guy.

COHEN: So when you wrote to Harvard did they offer you a job?

ZIRIN: Menzel offered me a job right away. Menzel had just gotten contracts for operating Sac [Sacramento] Peak Observatory, and he needed someone like me anyway, but I felt very good that they offered me a job right away. You know, in those days getting a job wasn't easy.

COHEN: What year are we talking about?

ZIRIN: 1953. I had left Harvard in the summer of '52 and worked for RAND from September of '52 to the summer, or the September, of '53. Then I went back to Harvard. At the time, Menzel was starting a group doing solar physics. It was really laughable, because we didn't know a thing about it. In those days, if you studied astrophysics, you learned everything—solar physics, interstellar matter, everything. Now they only teach galaxies—at least here at Caltech. But though I had had the training in solar physics, I knew very little about it, as did the other people there. I mean, they were good theoreticians, but they didn't have a clue about observation. Not many people did, because there weren't many good observatories. So I got to work on some atomic physics problems, mostly collaborating with David Layzer, who is still there, I guess.

Dick Thomas was also active, and I learned some things from him. Did you guys know Dick Thomas? Crazy person.

COHEN: David Layzer also.

ZIRIN: Oh, that's right. Small world. Well, Dave sat next to me. He had gotten his PhD, I remember, the year I got my bachelor's degree, because I remember running into him when he had his PhD robes and I had my bachelor's robes.

COHEN: What was your position at this point?

ZIRIN: I was a postdoc, a research assistant or something. Actually it was more than that. I was also a lecturer, because I was giving the introductory astronomy course.

COHEN: At Harvard?

ZIRIN: At Harvard, right. [Laughter] To them I was a find. I mean, I'd teach the courses they didn't want to teach. There was a course called marine navigation. It was something that started during the war—teaching navigators. And Bok and a lady named Frances Wright taught it. It continued after the war, because there were a lot of amateur yachtsmen at Harvard. It was quite a large class, with about fifty students in it. Well, I didn't have a clue about marine navigation. Actually, I had never been on a boat in my life. But there was a guy I had known from my undergraduate days named Alan Trustman. Alan had made his fame as the author of several movies, "The Thomas Crown Affair" and "Bullet." He worked a lot with Steve McQueen. Alan also became a successful lawyer, but at that time he was just a student in law school. And he and some friends felt it was outrageous that I should teach marine navigation without ever having been on a boat. So they took me up to Marblehead for several weekends and they taught me how to sail. I picked up enough points that, as I was teaching the course, I took on the air of an old salt.

COHEN: You were from Bridgeport.

ZIRIN: Yes, it's true, Bridgeport was right on the sound [i.e., Long Island Sound]. One day a student came up to me with a question—how did he put it? "I know this is way less than your vast experience, but could you explain to me this and that?" I kept one step ahead of the class. I taught it the two years I was a postdoc at Harvard. After the second year I asked to be excused from it, but they just put me down to teach it again. Now, at that point I had had enough. Well, it happened that Walter Roberts [from the University of Colorado] was visiting at Harvard that year. So I interacted with Walt somewhat and let him know I was interested. Walt had a vacancy, but he wanted to hire Gene Parker. But then Gene Parker took the job at Chicago instead, so Walt hired me.

COHEN: And this was at Colorado?

ZIRIN: This was in '55 at Colorado. Actually, I had been out there in '54. Since Menzel was running Sac Peak, I went out to spend a summer there. This was my introduction to solar observing. It had been quite an interesting trip out, because we had a car crash in Michigan. I was in a convertible with Arnold Schlüter and Roger Tayler and a gal named Edith Flather, who was Guido Münch's computer. The other two people got rather badly injured, but Schlüter and I went on in my car westward. We visited Chandrasekhar. That was my first meeting with Chandra.

COHEN: In Chicago?

ZIRIN: In Chicago. Well, in Williams Bay, actually. I remember one incident in particular. I used to smoke then, and when we went into Chandra's office at Williams Bay I was smoking. When I looked around and saw there were no ashtrays, Chandra saw me looking, and he very disdainfully pushed a wastebasket toward me. Afterwards I got to be quite friendly with Guido Münch from one of those two years at Harvard when I went back there.

COHEN: What was Guido doing at Harvard?

ZIRIN: He spent a year at Harvard.

COHEN: Because he was from Chicago, as I recall.

ZIRIN: That's right. Guido had studied with Chandra, and he explained to me that you were never supposed to smoke in front of Chandra, unless you were Rupert Wildt. Rupert Wildt discovered the absorption of the negative hydrogen ion, and he used to smoke these awful German Virginier cigars that had a little straw sticking out of one end. One day Guido had gone into Chandra's office and found the place filled with smoke while Chandra talked with Wildt. [Laughter] So Guido asked Chandra about it, and Chandra said, "Well, he smokes good cigars." They were the worst things you could get. [Laughter]

So I first went to Boulder on that trip, because Schlüter and I visited Boulder on the way down to Sac Peak. Schlüter stayed in Boulder, I guess, and I went on down to Sac Peak and spent the summer there, which I found very enjoyable. At the time I was working at Harvard—Harvard ran Sac Peak. This was the summer following my first year. Then, during the following year, Roberts offered me the position at HAO [High Altitude Observatory], and I jumped at it.

COHEN: You liked it there in Colorado?

ZIRIN: Yes, I was very impressed. It was beautiful in Boulder the two days that we were there. The people observing the Sun there had some very good ideas, and really seemed very enthusiastic. [Tape ends]

HAROLD ZIRIN

SESSION 2

February 10, 1998

Begin Tape 2, Side 1

COHEN: Good afternoon, Professor Zirin. I'm glad to have you back. I think we've gotten you safely into Boulder. So tell us a little bit about that.

ZIRIN: You know, I was thinking about it awhile after we talked. I had a real creative period my last couple years at Harvard. I really proved some theses. Then in graduate school, I got through fast and I learned a lot, but I didn't do anything real creative. Then I went into solar work in Boulder. I don't think I did really good work for some years. Then around 1958, I got some new ideas. The creativity depends so much on whether you can get good observational data, because we know so little about the world—and to just create it from your mind just doesn't work. In 1958 I began doing better work, and then around '61 I began to get some really exciting ideas. Mostly I was proving everyone else wrong, but I also did some original work. I think I started on my book before I left Boulder. I felt a little isolated there, because everyone else was heavily into theory. It started out very nice. The group was a bunch of young people doing solar work who were not that possessive of their positions. And, of course, I met Mary and we got married. We met through George Gamow, who was spending a lot of time in Boulder then and who eventually became a professor there. Now they have a tower named after him.

My time there was quite successful—I learned a lot. We were in a broken down old building, without much money. They had a lousy place for doing astronomy, on the ridge of the Rockies.

Then Caltech beckoned. I had known Jesse Greenstein from various meetings, and also from when he tried to recruit me in 1950 as a graduate student. And I had been quite close to Guido Münch—from the days when I was in Munich, and also the days Guido had been at Harvard—we wrote a paper together. So anyway, I got a call from Jesse right after Otto Struve died. Apparently, Struve used to come and give a guest lecture course every year, and now Jesse needed somebody to do it. So he invited me out. As it happened, we had just adopted our first

child, so it was a tough time to do it. I arranged with John Firor to share the course. He would come out for a few weeks and I would come out for a few weeks. Then, while I was out here, I saw what Bob [Robert] Leighton was doing. I also got to learn more about how much better the West Coast was than anything we could ever have in Colorado for what I wanted to do.

COHEN: Better in the sense of the people around you? Because there wasn't a solar telescope here.

ZIRIN: Oh, there was—Mount Wilson.

COHEN: Oh, on Mount Wilson, of course.

ZIRIN: It was basically a matter of the weather. You had not only clear skies but this very stable air which is the result of the cold California current. So you could do things that you couldn't do anywhere else. Even Mount Wilson, which later on turned out to be ninety-ninth out of one hundred sites, was then considered a great site by comparison. It tells you how far they had to come in the observatory field. I saw the stuff that Leighton was doing, and it was absolutely brilliant. Instead of being deep in theory and equations, he was trying to learn what was out there and doing it in very ingenious ways. He just had the right attitude toward things. He always wanted to learn—he didn't feel that he knew it *ab initio*. Actually, later on Bob got much deeper into solar cycle theory, and I think there he finally got to where he thought he knew it. I think that prejudiced what he did—I don't think the later stuff he did was all that great. But he was doing beautiful work earlier.

COHEN: This was in the early sixties?

ZIRIN: 1963. So when I went back to Boulder I kept wondering how I could get a job at Caltech. In those days they didn't advertise jobs—you just had to have somebody come to you. And that happened. There was some kind of a meeting sponsored by JPL and I was sitting in Kerckhoff, right over here, listening to a lecture. Guido came down the aisle, where I was sitting. He said, "Do you think you would be willing to take a position here?" Just like that. And I said, "Sure." [Laughter]

COHEN: Just like that.

ZIRIN: Just like that, right. Then I started talking with people here. I had a pretty good position in Boulder, so I told them I would only come if they gave me a full professorship. In those days it was easier to do. I guess Carl Anderson just wrote it out and that was it. Later on, I always felt so lucky that they picked me. Two things that Jesse said amazed me. One was when I asked him why he hired me and he said, "Well, I felt we needed somebody in solar." They would never say that today—nobody thinks of studying the Sun, even though it's the most important thing in our lives. And the other thing Jesse said was, "Bob Leighton said he was responsible for hiring you, but I was." I felt so good that people would fight over who was responsible for hiring me. [Laughter]

The other thing was that Bob had gotten money for a site survey. Now, I was very much influenced by the concept of the *Zauberberg*, the Magic Mountain. Mann's novel really has nothing to do with a magic mountain, in a sense. But for me it epitomized what I wanted to do—in my case, to find the place where you could really do solar astronomy. And Leighton had the wisdom to do that. You see, most people don't realize that when you look at the Sun—and probably if we could look at other stars too—what you see is a very dynamic thing. But when you see a phenomenon at one point in time, you don't understand how it's going to evolve. So what's important is the whole four-dimensional picture of the three-dimensional motions, and the time vector, to see how the phenomenon develops and what happens to it. In the work at Mount Wilson, they were restricted to just observing in the first half hour or so of the day, which was a fun way to do it. They would leave to go up before dawn. You might get up to the top of the mountain and find it was cloudy, because you never could see what the visibility was up there because of the low clouds. Now, when the weather's clear you can see the upper clouds, but about half the year you can't see what's going on at the mountain top. But if it was clear they'd go up and prepare everything, and when the Sun would come up they'd have about a half hour to do observation.

COHEN: Why couldn't they proceed during the day?

ZIRIN: Because the rocks would heat up. To backtrack a little: Most people don't realize—and I wasn't so aware of it either then—that what really counts in astronomy is seeing—what they call seeing—and whether it's cloudy or not may not matter, because if you have—and this is true in solar astronomy too—a star that's a point of light, it's really bright. If you have a turbulent atmosphere, and you spread that out over an area ten or twenty times as big, you are actually losing ninety percent of the light. In solar astronomy it's not so much the amount of light you see as being able to figure out what's going on, because you are looking at a two-dimensional structure. A star is a star; you only see the totality. But on the Sun we look at all the different things that are happening in different places on the surface. In fact, it was really kind of a new kind of solar astronomy, because before that people were not that aware of the difference from point to point. I think it was really Leighton who started that and I who came in and finished it—the idea that there was this thing called a chromospheric network. You had a network of magnetic fields that had a very peculiar velocity pattern, and peculiar magnetic patterns, and you couldn't just take a spectrograph and look at one place without knowing what that place was. It's like the streets and the boulevards and the houses—they're all different if you happen to look at them differently. Anyway, Bob had gotten money to do this project of looking for a new site, not so much a site that would be the greatest in the world, but a site where, instead of observing for just half an hour a day, they might observe for four or five hours a day.

COHEN: Now when you say that the rocks heated up, do you mean that the mountain itself got too hot?

ZIRIN: The whole mountain heats up, right. Of course they had a tower, which was there for various purposes, one of which was to get above the heat. But the mountain is 5,000 feet high and the tower is 150 feet high. It's the wrong scale. It's a lot of heat, and you could just see it happen every day. So Bob solved the problem. That's the difference between a real scientist and what I would call not a faker but—

COHEN: An ordinary scientist?

ZIRIN: Right. The first thing the Europeans did in making their recent site survey was to hire a meteorologist who would explain where the best place to house an observatory was. They didn't bother to test much. We were somewhat influenced by K. O. Kiepenheuer, who had a number of theories about this. Bob and the other Bob, Bob Howard—who was really a participant in this too—and I together had the better idea of being empirical, going to see the places [laughter], which turned out to be exactly the way to do it, because nobody understands it well enough. I think we do now, but I still wouldn't bet on it. I used to have a German professor at Harvard who would say, "The experimental facts," and I remember his strong German accent on the word "facts." And you do need the experimental facts. We went around for a couple of years looking at sites, and it was sort of fun, and very exciting.

COHEN: Do you mean going around and looking at sites?

ZIRIN: I mean, observing the Sun from the different sites. Bob had hired one guy to help, and then we hired a second guy. Actually, it was very empirical, because in the beginning we were going to build towers, because we knew it was better at the top of the tower than at the bottom. But then, it turned out that how many towers can you build? In fact, there is still a tower on Mount Wilson, which was the first one built. We found out two things from that tower. First of all, what you study with a tower is the shakiness of the tower, because it shakes. [Laughter] You're looking for something that's very, very small—[equivalent to] a small boy standing on the Mark Hopkins Hotel—that's the kind of thing you're looking for—as seen from Pasadena. And also, we could only afford to build four or five towers, so you had to know the four or five best places to start with, which we didn't have a clue about. And in fact, the last places we studied, that turned out to be the best—Big Bear, Lake Arrowhead, and also Tournament Park—were not even on the list at the beginning, which was interesting.

COHEN: When did the idea of water come in?

ZIRIN: The idea of water was already in the literature for a long time. There was a royal astronomer in India—Evershed—who had observed an eclipse and noticed that if he looked across the lagoon—I think it was in Saipan or Truk or one of those Pacific islands—the seeing

was much better than it was over the land. So this was in the culture. But nobody had had a deep enough interest in it to pursue that. People also thought that it was easy to study the Sun. You could have an observatory right in the middle of the city, they thought, without worrying about lights or anything, and you can do your observing. Actually, for the first hundred years of modern studies, it didn't make a big difference, because we couldn't really analyze all these things that we can now. So we knew that water was important, but we thought so for the wrong reasons, because K. O. Kiepenheuer had said that the place to be was where the fresh air comes in off of the ocean. So in fact our early list included places like Malibu Peak and Santiago Peak. Mount Wilson, of course, also gets the fresh air from the ocean. Big Bear does to a certain extent also, but the air there is interrupted by some of the other nearby mountains. We also went right down to the beach, where Bob had at first wanted to build the towers. And then Sue Kiefer—she was Sue Werner at that time—who had been a student of mine at Colorado, got a summer assistantship to come out here and work with me. I didn't have that much for her to do, so I suggested to Bob that Sue could work on the site survey. So Sue got a truck. They gave her a forty-five caliber revolver to stick in the glove compartment. She had a telescope. And she'd drive out every day to look at places. She'd set down the tower by jacking up the truck, actually, so it would be stiff, and make her observations and visually estimate the quality. At that time, Bob Sharp, the head of geology, was against women graduate students—he felt they couldn't do field work. But he saw Sue jacking up the truck every day, and he said, "Ah, this gal can do field work." I made the mistake of thinking that Sue probably was not good enough to come into astronomy as a graduate student, so she went into planetary sciences. Later on she married a guy named Kiefer, whom she eventually separated from. She's now a member of the National Academy of Sciences.

COHEN: [Laughter] You goofed.

ZIRIN: Yes. She's been in Canada for some years. So she helped get it going. Then I remember the first day we went to Refugio Beach, which is at the base of the pass that goes up to the other side of Santa Barbara. At first it appeared that Refugio was quite good, but every other time we went to Refugio it turned out not to be very good. But that convinced Bob that with a small

telescope you could see the difference. Now Bob was very bright, but he was also conservative. But, boy, he learned fast—he was a very sound guy.

So anyway, we started this survey and it turned out that most of the places we picked weren't very good. Interestingly enough, Mount Wilson, which everyone thought was the greatest place in the country was, out of the hundred sites, ninety-ninth. It just wasn't very good—after all, it hadn't been picked with a survey. I mean, Hale came out here and said, "Look, there's a likely mountain," and that was it. Actually, Big Bear was not on the list, because the list said, "100 miles from Pasadena," and I think Big Bear was 101 miles. But I had been there when I first came out and worked for RAND. I had been there on a weekend, back in my woman-chasing days, with a bunch from the Y in Santa Monica. I remembered it being a beautiful spot, so I talked them into making measurements there, too. Those measurements really showed that, while Big Bear wasn't the very best place, if you took the average of all day and all year it was the best. We did this survey for a couple of years, and collected all this data.

COHEN: Who fronted this search? Was it Caltech?

ZIRIN: The Office of Naval Research. I did get money from Caltech when we needed to finish the observatory. Harold Glazer was running the Office of Naval Research, and he was a very nice and intelligent bureaucrat. He was quite sympathetic to what we did and he funded Bob. Later on he got involved with that lady at USC [University of Southern California] who does space research—she's got a funny, complicated name which I can't remember. It wasn't hard to get money for solar research in those days, although it was really hard if you were starting out. I think every year I have been in this business I've heard the same story from somebody else who's trying, that "money is really hard this year." Now we look back and say, "Geez, we could raise money in those days." Anyway, we found several sites that were very good, and I chose Big Bear for a number of reasons. First of all, I wanted to do coronal work, and you needed a high site. Lake Elsinore was very good, but it had a lot of haze and stuff, so you couldn't do that. It turned out Lake Elsinore also had a problem—nobody knew what the maximum level of Lake Elsinore was. In fact, we were offered free land there, but it didn't suit me. At Big Bear we were able to lease the land. You see, if I had had to buy land, I would have had to talk

Caltech into paying for it. And they wouldn't have done it, I don't think. I don't know. Anyway, we got a lease on the land.

COHEN: This was at Fawnskin?

ZIRIN: Yes, Fawnskin, where we still are.

COHEN: What year are we talking about?

ZIRIN: We are now talking about '68, because we finished the survey in '67. It took maybe a half year or a year to analyze the data, and then I had to start looking for money. Word came from the NSF [National Science Foundation] saying that they were going to give facility money. It's usually hard to get facility money, and there are laws against using such money to buy land, etc. I asked for $50,000 and I got $50,000, practically by return mail.

COHEN: From the NSF?

ZIRIN: From the NSF. That was kind of a seed grant, because then I went to Walter Roberts, who had been my boss in Boulder. He was on the Fleischmann Foundation. I don't remember how much I asked them for, but I got that practically by return mail, too. And then Henry Smith, who had been a classmate of mine at Harvard, had gone into the new solar office in NASA. He had a lot of money, and he wanted to support me. He was a funny guy. He was a very meticulous student, so if I ever missed a lecture I could always use his notes, yet he wasn't that good academically. He's dead now. Anyway, Henry was supporting me. And of course it made a big impression on Caltech that I had gotten money from Fleischmann, because they had been trying to get support from them for years. Eventually, I think, Murray [Gell-Mann] did get some money from them. But I got it, frankly, by return mail.

COHEN: Were these the Fleischmann margarine people?

ZIRIN: Yes, right. Mr. Fleischmann left his money. It wasn't a big foundation, but Walter Roberts had gotten money for the observatory building in Boulder from them, and they liked him

so much—he was a very outgoing and smart guy—they put him on the board. So Walt helped me get the money. Then Jesse Greenstein had introduced me to an architect friend of his, Douglas Honold, in Los Angeles. Mary and I had gotten friendly with him, and I suggested that he design the observatory, which was a big mistake, because you should never go to one of those commercial architects—they charge you a fortune. And he really didn't know the conditions at Big Bear. So, while the observatory building was beautifully designed and has held up very well, the buildings on shore had shortcomings. They were built just like wooden buildings in LA would be, so they weren't properly insulated, among other shortcomings. But the observatory is a nice looking building, so it came out all right. Then I got enmeshed in the politics of Physical Plant. I had to go through them, and it was a nightmare. They had a guy who was overseeing the construction, except he really wasn't doing anything but conveying messages between us and the architect. Physical Plant knew nothing at all about it, but I had to pay them, even though they weren't doing me any good.

COHEN: Were you completely in charge of this? Was Bob Leighton out of it by this time?

ZIRIN: That was interesting, too, and a real surprise. When it came time to build the observatory, I found out that Bob really wasn't ready to build one—he really was just interested in finding the best site.

COHEN: I see. He liked the search, not the goal.

ZIRIN: That's right. Well, he had gotten interested in other stuff too, such as infrared telescopes. He had already started building those. I think that eventually, toward the end, he felt solar astronomy was a dumb thing to be doing—at least he wasn't excited about it. On the other hand, in 1960, a couple years after the observatory started, he built the magnetograph, which is the most powerful instrument we have there. Without Bob I never would have had that. It was a very clever execution of that idea.

COHEN: That was after the observatory was built?

ZIRIN: That's right.

COHEN: Let's go back to the building.

ZIRIN: Okay. Anyway, we milled around and I could see I was running out of money, because I was spending so much on the architect and the bids were getting higher. I had enough money to meet the bid—we got, really, a pretty low bid for the building—but the contingency kept me from having enough money.

COHEN: This was for the building that the telescope was going to be in?

ZIRIN: No, the whole thing—the building for the telescope and the front wooden building—the lodge.

COHEN: So it was the whole package.

ZIRIN: Yes, although we built a couple of small buildings afterwards. The bid came in at $280,000. I had that much money, but I didn't have a five percent contingency—or ten percent—I don't remember what. And I was stuck, because every day I had less money, because the architects were eating it up. At this point, Henry Smith became involved again. I had told you NASA was helping me. NASA wasn't supposed to pay for building construction, but they didn't look at these things very closely. I had a grant then of $160,000 a year from them, and I remember Henry saying to me one year, "Harold, let it go to $200,000." Can you imagine anyone ever offering you more money than you asked for? And that was 200,000 real dollars—1968 dollars. So it was the equivalent of having a million-dollar grant today. I could siphon money from that, because I had all that money and I didn't have a big group at all. So that went into the building, but I still didn't have enough money. Finally I decided—I was at my wit's end—to go see [President Lee A.] DuBridge. I told him, "Look, I've got the money for this thing, but I just don't have the contingency. And everything else is wrapped up. Could Caltech help me with this?" And he said, "Sure, go ahead." Now, later on somebody said—and I don't understand this—that DuBridge didn't actually know where he was going to get the money. But it wasn't a very large amount. Eventually, I think, Caltech paid about $30,000, but it was somewhat open-ended. I always felt kind of proud that I did it with my own money, except for

that. So we went ahead. First of all, we designed the building. That was quite an adventure, too.

COHEN: We?

ZIRIN: Well, I and the architects did. Leighton didn't play much of a role, and [Robert F.] Howard was busy with his empire on Mount Wilson. Actually, at the beginning I used to do some work up on Mount Wilson, but it was clear that two women in one kitchen doesn't work out. I always got along very well with Bob, but he had his things to do, and I had mine, so it was really better that I go to a different place. Besides, the equipment at Mount Wilson was old, and Bob—I guess you probably knew him—was a fairly passive person. A very intelligent person—you can't tell that by how quiet he is—but very passive, so he wasn't going to do anything to change things. And of course [Horace W.] Babcock [the Mount Wilson director] probably wouldn't let him, but we never knew what Babcock's attitude was. You may remember, we used to be under the thumb of the Mount Wilson and Palomar director.

COHEN: Who was it at that time?

ZIRIN: I'm pretty sure that Babcock had become director by then. I think, if he had really been aware of our work at Big Bear, he would have stopped the project, just out of general bastardy [laughter], or conservatism. I don't know. Also, when I first came here they told me I couldn't get government grants, because Carnegie [the Carnegie Institution, which was owner of the Mt. Wilson observatory] wouldn't take government grants. But we weren't Carnegie, so it was crazy. Anyway, Babcock didn't seem to notice what we were doing. But after it was built and successful, he dug out the part of the agreement that said they would be in charge of any optical facilities, so Big Bear came under his purview, too. He appointed me chief astronomer. You see, I wasn't even director at the time. [Laughter] But he didn't bother me and I didn't bother him.

COHEN: Let's go back to building the thing. Did it all progress as it was supposed to?

ZIRIN: One thing had happened in the interim before we started construction. We had some very rainy years—they didn't call it El Niño then, but it probably was—and the lake was filling up. As a matter of fact, the last site I had seen before we started construction was the lake. There was an island out there, you see. I had seen that island when I first came up—the lake was lower then, before the rain. And there had been a couple pickup trucks out there on the island. You could drive out and fish. I had said, "Gee, do you think the owners would let me build out there?" [We had leased the land.] Well, they did. And I did sort of a dumb thing. I was walking the tract with the man—his name was Horace Hinckley—who ran the water company that owned the land. It was a private water company. And he said, "Well, do you want all this shoreline?" There were 1,300 feet of shoreline available. I didn't want to be too pushy about it and said, "No, about 500 feet would be right." Then I changed my mind, but he changed his mind, too, at the same time. So we stuck at 500. You know, we played this little game. He was one of the shrewdest people I ever met. I think he just passed away a little while ago. He was the business agent for the water company that owned the lake.

COHEN: What water company is this?

ZIRIN: The Bear Valley Mutual Water Company. It was put together by a bunch of orange growers down in Redlands to irrigate their groves. I had gone and talked to them. There was a Caltech graduate on their board, I think, or maybe he was the chairman. I don't remember. They were very friendly. But I had to sell them the idea that they should lease the land to an observatory. They probably had never thought of it. I remember I went to the Elks [Club], and I showed them movies of the Sun, and that sort of thing.

COHEN: So it was their water? It was their land?

ZIRIN: It was their land and their water, and they owned all the land around the lake—not only the lake itself, but a large plot all around.

COHEN: That's right. It's not a natural lake.

ZIRIN: Right. They owned about a mile from the shore in all directions, except where the city is—quite a bit, anyway. The rest was national forest. They would not sell their land; they would only lease it. That was in my favor, because it meant I didn't have to go and buy land, which would have been very hard to get money for. I suppose I could have talked Caltech into it. Curiously enough, about three years later I saw Ivan Betts, who was the Caltech treasurer, and Ivan said to me, "You know, Hal, we picked up your piece of land up there. The water company decided they would get out of the land holdings, except the land under the lake, and they sold off at very low prices." So Caltech got seven or eight acres of land for about $65,000. It's worth a couple million [dollars] now.

COHEN: Very informal.

ZIRIN: Very informal. It can't go that way anymore, maybe, but it was like that when I came here. For example, at first they gave me a certain amount of money for moving to Pasadena, and I found my expenses were twice as great. When I went to [Robert F.] Bacher's office about the difference, they said, "Oh, well, before, you weren't really here yet, but now you're a member of the family and we can pay your whole expenses." [Laughter] It was really strange. Anyway, it worked that way then. DuBridge said, "Okay, we'll do it." He didn't worry about it—he just wanted to create. I should say, he was like that with everything. I remember when he came back from being Nixon's advisor in Washington, there was some kind of reception. I shook hands with him and he said, "Well, how's Big Bear going?" I didn't know that he remembered that I even had the observatory. Of course, that's a political thing, too—you learn to remember everybody's name, I suppose.

COHEN: But it works.

ZIRIN: It works. DuBridge was always, you know, "*immer forwärts*"—we'll just go and build and do things. I wish we had more of that. Robbie [Rochus] Vogt was like that too, really—but nobody else, I don't think.

COHEN: Did the building come in on time?

ZIRIN: They never do. It took me so long to get the money and the drawings organized that we didn't start until, I think, about Labor Day in 1968. The dedication was in May of 1970, a year and a half later. Actually, I had gone up earlier and hired a well driller. I could do that on my own. I was worried about water, which probably was foolish, but I hired a well driller to drill a well. Then one weekend we rode up to Big Bear and saw this well-drilling rig coming down the mountain. It turned out to be our crew, and they had struck water. Actually, there is plenty of water up there, but we didn't know that. I had also gone up and given talks to the Rotary Club and people like that up there to get public support, because I felt we would need it in the long run. We agreed that we would have open houses and that sort of thing, just to get local support, which you always need. The public couldn't come in, but I remember somebody seeing the picture and saying, "Oh, he's going to build a Taj Mahal out in the middle of the lake." So they really liked that. Libbrecht's addition kind of spoiled the profile—until then it really was a very handsome building.

Anyway, after we found the water, the contractor came. The construction crew started working, and they worked very hard. After we started building, the water began creeping up because of all the rain. In order to build they had to construct an artificial island, because the existing island was already under water. Now, I had to have an island, and it had to be far enough out so that there always was water around it. When I was up there in '53 and I first saw the place, it was in the middle of the drought and the lake was twenty feet lower. Now this place which had been an island was only the edge of the lake.

COHEN: It was a causeway, in a sense?

ZIRIN: Well, it wasn't even a causeway. It was just the shore—just a little point on the shore. I wanted to make sure that we would always have water around us, which was very important. The farther out you got, the more you got into the clean air that's moving across the lake. So we determined to build the causeway, and they scooped up land, dirt, on either side. They went and they built it and they poured the foundations and all of that. And then it began to snow and rain just terribly, and everything just became mud. So they stopped the construction at a time when all the concrete pouring had been done, fortunately. For the buildings on shore the shells had been built, so they were protected. The only thing is that there wasn't a dome or a roof on the

observatory, because the dome was not part of the project. So I got a local roofer to put a roof on the observatory, and it was the stupidest-looking thing. There are pictures, in albums up at Big Bear. Maybe we have them here, too—I think there are copies. So there was this nice-looking observatory with this thing that looked like a subdivision's flat roof on top of it. But that got us through the winter. I don't know how much it protected us, because it was all concrete anyway and it probably would have been all right.

COHEN: You must have spent a lot of time up there.

ZIRIN: Oh yes, but, actually, I had a very good agent. His name was Ernie Lorenz. He was an amateur astronomer who used to make sunspot drawings every day, all his life. Leighton had hired him. He was a tremendously energetic guy, and he moved up there and handled it locally.

COHEN: So you had somebody really good up there.

ZIRIN: Yes. So they stopped the work, because they couldn't do anything in the mud, because they were still going out and back on this muddy causeway they had built. Then I remember going to Washington for a meeting of some sort and I called Mary back home. It was raining, sort of like now, but even worse. And she said, "Big Bear is reporting 26.5 inches of rain this weekend." And I said, "That must be a mistake." It turned out it wasn't. They had twenty-six inches of rain. The lake filled up for the first time in sixty years. Our observatory wasn't under water, because we had built it well. Now again, people just don't realize that you can plan for contingencies. We had prepared so that the place would be accessible no matter what the water level was. It goes up and down like a yo-yo. So we had an entrance at the top and we had an entrance down below. We had it set up so that if there was a lot of water or an intermediate amount of water, you could go out on a boat. Then we had big doors through the building, which are still there, so that, with a hoist on top, you could hoist things up through the building. You'd bring the boat in and hoist it up. We had so many dumb people say, "Oh, you've got a wet cellar," but it was all designed. We knew where the water would be—we just didn't expect it to happen that fast. I had already bought two boats. We stopped work for the winter, because

there was this enormous snow. I mean, most of the time you were driving with snow above car level.

COHEN: It must be like that now.

ZIRIN: No, it's been warm enough that the snow has melted. One other time we had nearly as much, but this was really incredible. So the work was all stopped. Then when winter was over we were confronted by water between us and the observatory. So Ernie, who was quite resourceful, got a couple of docks put together with plastic foam as the floater part and boards on it. And with a couple motor boats we had, he would bring things out and in. The contractor finished his work by doing that.

COHEN: Boating his material out?

ZIRIN: Right. We were lucky that we had the concrete all poured. Otherwise, it never would have worked. I was just so god-damned lucky with so many things there. Meanwhile, they finished the buildings on shore. Everything was done except the dome and the painting, because I didn't have money to pay for them. I figured I'd take care of them later—only get the place done. So, what did we do? Ernie, who was just capable of anything, really, got the dome built. He got hold of a welder from Physical Plant who was assigned up there. Besides the building money, I was getting my grant each year, so I could pay these guys. And then, once we really moved into it, we had a couple guys—we had our guy for the machine shop and our janitor-sort-of guy—and they would paint every day. Eventually they painted everything. Painting isn't so hard, after all. Building the dome was very difficult, because they were out in the middle of the lake. They put these girders up, you see, and then sat on the girders and brought up the plates and welded them into place. We have pictures of this stuff being taken out on the docks—it was really quite extraordinary. I guess when I retire I ought to bring the picture albums over here too, because they tell more than anything, I think. But anyway, Ernie did it. In the meantime, we had a small telescope on shore that I had gotten earlier for use in the site survey, and we started observing the Sun then. Ernie had a dome of his own that he brought out and we set it up on the shore. Then I was worried about where Ernie would live, because he was going to be in

charge of the place. I started trying to think of ways to fund that. I kept on coming to Ivan Betts with new plans for how I would do it, because I didn't have the capital. So I said, "Okay, let Caltech build it and I'll lease it from you." I had been there for so long that he felt he had to do something for me. [Laughter] That's a good lesson, too. He was a decent guy. People thought of him as a bureaucrat, but he was a very decent, intelligent man. Very conservative, of course. He called me up one day and said, "Are you sitting down?" And I said, "Yes." And he said, "I've got money for you. Would you be willing to let it be named the "Somebody Mudd Cottage?" There was a residual from the estate of the Mudd family, and Ivan knew I was hunting for money. They gave us only $25,000. I built a 1,500 square foot house—a nice little house—for $25,000. But around that time Ernie got married, and his wife stuck her nose into everything. It got very uncomfortable. We were discussing the rent he would pay on the cottage. I named a rent which was pretty nominal—about $150 a month or something—and she didn't like it. She also installed herself as cook in the beginning, but we really didn't need a cook.

COHEN: Do you mean for all the people there?

ZIRIN: Yes, well, there were a fair number of people there—I had a number of students, some of whom are professors of astronomy today. So we got in a fight and I really was bothered by the fact that, every time I talked with him, she would pick up the phone too and get in an argument with me. I finally had to fire him, although he had put in so much work and was so good. Eventually Ernie went to work for Jim Westphal [James A. Westphal] and did important work with him. Ernie and I eventually got back on good terms. He worked for many years for Westphal.

COHEN: So he continued to work for Caltech.

ZIRIN: There was an intermediate period when he got a job at Mount Wilson, so it was not Caltech. He was there for a couple years as Assistant Mountain Superintendent. His wife got him into trouble again, by sticking her nose into the business, and he got fired. Then he went to

work for Westphal. He never worked for Leighton. I replaced Ernie with Gene Longbrake, who was working then for [Edwin W.] Dennison. He was a very sensible technician.

COHEN: And that person had to live at the site?

ZIRIN: Yes, in this house that we had built—the Mudd hut—which was a rather nice house. They had built a similar structure at Climax [Colorado], and I was very impressed by it. Climax has a lot of snow, so the living quarters are on the upper floor of a two-story house. The bedrooms are downstairs, where you don't want too much heat anyway, and the living quarters are upstairs where it's nice and warm and where you can see beyond the snow to the world outside. Ernie didn't like this. A conventional house has bedrooms upstairs. It's interesting that he had thought, all the time, that I was building the house for myself. Eventually Phil Goode completely converted the quarters in the front lodge to his own private dwelling, which I wasn't terribly happy with. That's the present situation. As far as Mary and I were concerned, Mary, with her usual good sense, said, "We don't want to live anywhere near this place. We want to live back in the woods, somewhere where you don't have the problems facing you at night." A wise decision. We bought a cabin back in the woods, and it's very nice.

But I had to have somebody run the place, and I got Gene Longbrake to run it. Oh, I should say the telescope had been designed by Bruce Rule, who was the chief engineer here and who was a pretty knowledgeable guy. But the interesting thing was that Rule was convinced that I would never get an observatory built at Big Bear, so he designed the telescope for the latitude of Palomar, without ever asking me. And Palomar wasn't a good site, period. We owned Palomar, but Rule just didn't understand that if you get one of these hot mountain sites it kills you. He had a good understanding, however, of how to build a telescope.

COHEN: But the altitude must be quite similar?

ZIRIN: It's not the altitude, it's the latitude. The axis has to point to the North Star to compensate for the rotation of the Earth, and if you don't point it exactly to the North Star—the true pole of the sky—you'd get this wobbling all the time. But Ernie was equal to anything. We

carried the telescope out on the boat and lifted it up and put it in there with the hoist and propped it up. We had some students. We got our first observations in October of ’69. [Tape ends]

Begin Tape 2, Side 2

ZIRIN: I had three periods in my life where I was really creative. The first was when I was a senior at Harvard. The second was when I caught on after some years at HAO. And the third was when I came here and everything was open to me; I could do whatever I wanted.

COHEN: And you really enjoyed building this telescope?

ZIRIN: Well, it was a big job, but I liked the challenge. I liked to show that "I can do that, and that, and that." But, also, I had no choice. What I wanted to do was be able to observe these phenomena, and I would learn an awful lot by being able to do it. So I had to build it, because the national observatory was no good, because they have clouds in the summer and they're on these mountain tops where the air is shimmering all the time. We had to build our own place. There was no choice. It was sort of fun to do it. I learned a lot, because I had never built a telescope before, except in high school, and that was a different story. When you have to do it, you learn. I don't want to pat myself on the back, but we made very few mistakes. Except, interestingly enough, if you go up there now—and you've probably seen it—the old telescope is sitting out in front of the building, because there were a lot of things wrong with the old telescope. In 1972, we got money to build a new telescope—that was a separate story, really. We built the new telescope well enough that we were able to take these beautiful images. People had never seen stuff like that before. From there I got the support to get more money to do more things.

COHEN: Let's just go back a minute, because now you would flash it on email and everybody would know it instantly. How did it get known then, in those days?

ZIRIN: Well, I would go to meetings and I would show movies, basically. That was it. You see, there's a funny thing in astronomy, particularly in solar astronomy. People don't have a lot of confidence in themselves, so they have to show the world they are doing big theoretical calculations. So other people feel that, if you are not doing big theoretical calculations, you're not Einstein and your work is not so difficult. Whereas the fact is that it is very hard to get good

observations—there are a lot of mistakes you make. It's even harder to interpret them, because after you see it, then you have to figure out how it got that way and why it's doing what it's doing. I always felt that I had to learn what was going on before I tried to explain it theoretically. I remember that I was very lucky; a couple of good postdocs came to work for me. Almost all these guys are professors now—one or two are even retired. The first postdoc I had was Bob Stein, who was a theoretician. Bob is now a professor at Michigan State. And then I had David Lambert, who is a professor at Texas. Actually, neither of them was very deep into the sort of culture I was getting into, studying flares and phenomena—they were more into the astrophysical thing. Then there was Mitsugu Makita from Japan, who was fairly good. He's just retired as director of an observatory there in Kyoto. Then I had a guy named Katsuo Tanaka, who was really the man I worked with very closely. During the summer of '72 there was a big eruption on the Sun—a big series of flares—and we got beautiful observations of all of that and published it. That was really the thing that made our reputation. That paper gets referred to all the time. It was the very first time anyone had followed a huge active sunspot region through its lifetime. We saw things that nobody knew would be there. So it was very good. It kind of made Tanaka's reputation; except, as you know, he eventually got leukemia and died at a young age. But anyway, that did a lot for us—people would go to meetings and see what you were doing. And the movies were particularly important. Also, I loved to publish. The advantage I had over these other people was that I was really, in the end, interested in the science. The observatory was a path to the science, so I published a lot. I had the sense that if you didn't publish you weren't doing anything.

COHEN: That's right, particularly at Caltech.

ZIRIN: Particularly at Caltech. But I've seen so many of these guys who say: "Oh yeah, I don't have time to publish," but it turns out they have never thought the thing through and they are too lazy to write it down. What good does it do if nobody learns what you did? And there was a fair expansion in the field at that time.

COHEN: This brings us to the seventies. Is that correct?

ZIRIN: We're in the early seventies, right. I started a project, together with Bob Howard actually, to build a solar telescope to go in the Skylab, but we didn't get in on that, because they didn't choose that by an open competition—it was chosen by deals. But we had a chance to go on the second one, which was also chosen by deals, except eventually NASA canceled the project because they abandoned Skylab to build the Space Shuttle. To backtrack—I had had a group up at JPL. I had to convince Bill Pickering to build this telescope, but in fact I was already getting the money for it, so it was no sweat for him. In fact, as soon as we got any real money, somebody at JPL always got a hold of it and spent it all before you could get to it. Incredible. Anyway, one of the things we had done before the Skylab project got canceled was to build a telescope—not a mock-up, but what they called a functional verification unit, which basically would do all the things that the real telescope would do, except it wasn't space-hardened. We designed that, and the design was completed when they canceled the project. I had a certain amount of money left that the people at JPL had tried to grab and spend, but I jumped in and stopped that. With the money that was left I asked Ball Brothers, the people who had done the design, how much they would charge me to put this thing together. Because they wanted the project to go forward, they made me a low-ball estimate, so I told them to go ahead and build it. The interesting thing is that, if you have a building, you can raise money a lot easier than if you don't, and if you have a telescope, that's even better. That was when the first Skylab was flying. So NASA gave us the money to co-observe with the Skylab telescope. That was in the days when they had a fair amount of money. So I got money to build a new telescope which would go on top of the mount at Big Bear. We pulled the one we had, which was okay but couldn't do everything that I had designed it for, because of my stupidity about some of the design. This time we designed a fancy telescope. We had found out, in the meantime, that you had to have a vacuum for the telescope, so that meant we had to have a window. We built the telescope and put it in up there. It had some failings, but not many. All of a sudden we had a telescope for solar work which was as big as any in the world—there are very few with really big apertures.

COHEN: And this was your original design for the Skylab that didn't go, I take it.

ZIRIN: That's right. The one thing I am very proud of also—there are a lot of things I am proud of [laughter]—was that I was great at copying other people's ideas. This ability goes back to the days when we first came out, when the costs here for Mary and me were high enough that I looked around for a consulting job and I hired on at Aerospace Corporation as a consultant, going there one day a week. They eventually built this observatory in San Fernando. But meanwhile, I learned from them of this design that had been used in Göttingen by [Paul] ten Bruggencate for what they call a "Gregorian" solar telescope. Nowadays this is the most commonly used design for a new, short telescope. In fact, their design was going to be in an orbiting solar laboratory. That was another project that got canceled—basically because it was very hard to sell solar research to NASA. The reason Skylab had done it was that it was fairly easy to do. You didn't need a big Hubble telescope, which is so expensive. The Sun is bright and a smaller telescope will do. But then we couldn't get them to go further on it. So I had the telescope and I proposed that, if NASA gave me the money, I would put the telescope in a new tube and replace what we had. And we did that in '73—a little bit late, actually, for Skylab, but we used it. That was another step forward, although it took us a long time to learn how to use it. Now we use it all the time. It's interesting that we had been going along with this ten-inch telescope—the original design had a couple of ten-inch telescopes—and then we jumped up to twenty-six inches and we didn't know how to use it. It took a couple of years, or maybe even more, before I really learned how to use that thing. Now that's the basic instrument and the ten-inch [telescope] is kind of on the side. So you live and learn. We made some optical changes, and we kept on finding things out. Now, in order to have an observatory, it takes a hell of a lot work and support. Because we wanted to have continuing time coverage, we took movies. When you take movies, you have to develop the film. Then you have to put the film in a form that you could show. That meant that we took the movies with thirty-five-millimeter film and then we copied it to sixteen-millimeter film. Every step of this takes an expensive machine. First of all, while it's not so hard to buy a camera that just takes pictures at sixteen frames per second—

COHEN: This attaches to the telescope?

ZIRIN: Right. We had to build a camera that took a picture every ten or fifteen seconds—whenever we told it to—because if you took sixteen pictures a second, you'd use up all your film right away. And anyway, there's not that much happening that you can see moving, because it's ninety-three million miles away.

COHEN: I understand.

ZIRIN: You take a picture every ten seconds. It's called time-lapse photography. They do it also to watch plants grow and eggs grow and whatnot. But, in order to do that, we had to have the cameras, which cost $10,000 each, and you had to have a film processor. For a while we used to send the film to Hollywood to get processed, but it cost a lot and it didn't come out the way we wanted. In those days there was a lot of stuff available in war surplus, so we got a surplus film processor, and then we got a surplus reduction printer. I had this process built up, with a guy working full time in the photo lab.

COHEN: And this was up at Big Bear?

ZIRIN: The first processor was up at Big Bear. The sixteen-millimeter films were made down here at Caltech. So I had a processor in each place. They would shoot the film in thirty-five millimeter, and then they'd send it down here and with a special printing machine that we had also found—I don't remember now if that was in surplus or if we bought it used—we'd reduction-print it to sixteen. Then we had the projectors and so forth. It took a tremendous amount of equipment to do it. Today you can buy a CCD [charge-coupled device] for $10,000, but then you don't need to buy film for it—all you need is money to buy magnetic tapes. And you can get a computer for $3,000 to process it. In fact, if you don't want the CCD, you can buy a television camera for $1,000 and a VCR for $500, and you can work with that, too.

COHEN: But you didn't have that then.

ZIRIN: That's right. We had to do it with film. Film has many virtues and many failings. The virtue is that it is a terrific storage medium, and it has a lot of resolution. The defects are, first of all, you have to process it, which is tiresome. Also, it's not linear, so it's hard to make

quantitative measurements. Fortunately, the Sun is kind of a standard candle, so you can refer to it.

There is another thing I have been lucky with. To do this work it takes a special kind of filter, which was invented by Bernard Lyot, called a Lyot filter. It enables you to look at the Sun in a very narrow wavelength band. Now, when I started out I desperately wanted such a filter. Actually, Hale had invented the spectroheliograph that did the same thing, except that the spectroheliograph is a big, massive thing that's a nuisance and very slow and has a lot of failings. So we needed the filters, which cost $10,000. That was quite a bit of money in 1969, but piece by piece I bought them. Then, when they closed out the Skylab project at JPL, I got in there and grabbed the money—because your money would just disappear—and had to do something with it in a hurry. So I bought four filters, three from Halle in Berlin and one from Zeiss. So actually, the largest collection of Lyot filters in the world is at Big Bear today, because I just kept on buying them when they were cheap. Now they cost about $200,000 each. They are only made in China.

COHEN: And are they still being used?

ZIRIN: A few of them are being used, but since they don't do much at Big Bear, they don't use them. It's unfortunate and sad, but that's another story that we'll come to at the end.

So anyway, we got these filters, and some for different wavelengths so we could start observing and making these movies. The hard part was getting other people interested in it, because everybody gets trained to be a slide-rule or computer manipulator, and they don't get trained to be an observer. That's because most places don't have clear skies to begin with, whereas you can always sit down and compute some model. I had the pleasure on Friday of having a graduate student in geology who has been doing some stuff with me, come in and say, "Since I've been in planetary sciences they have given me this problem of the interior of Jupiter to do. It's boring and I just can't get anywhere with it. It's all just calculating unreal stuff."

COHEN: This was a modeling kind of problem?

ZIRIN: Right. He went on to say, "Here I can work with you, and observe this stuff. So I want to come over and be a graduate student with you." But usually it was very hard to convince people to do this kind of work. Of course, in astronomy it was always difficult, because a student would come in to study with me and he would see that all the glamour was in quasars, and he'd be gone in a few months. Bob Kirshner came here to be a student of mine. Doug Rabin, who runs the National Solar [Laboratory] now, came to do solar. Then he saw that the glory was in quasars. He went and worked and worked and worked in quasars, but couldn't make it, so he came back to solar. There are endless variations of the story. It's been very hard to get graduate students since I've been here.

COHEN: Marshall [H. Cohen] tells the same story in long baseline. He says it's too hard until you get some data out.

ZIRIN: I'm surprised by that. I would have thought that long baseline was sufficiently glamorous. But what I could do, because I had enough money, was hire postdocs. So I've had a long string of postdocs, typically three or four at a time. It was hard to get ones that were really interested in observing, but we could do it. And I'm personally very greedy. When, for example, the radio telescopes at OVRO [Owens Valley Radio Observatory] became available, I grabbed them, because I wanted to do radio observations. Radio observations give you a chance to study a lot of things on the Sun that you could never study optically. So I got into that business, and got somebody to do it. I found out that, if you go forward, you don't make mistakes. The worst is being too conservative. Now, with some things it's better to be conservative, because you just sit and do your job and you don't get excited chasing butterflies, but where I was you had to build.

COHEN: But tell me something now. After a few years you really had been there working in a very organized way, so it did not take 100 percent of your time, did it?

ZIRIN: It took a lot of my time. You see, I called up every morning to direct them. There was only one guy I had who was competent to really decide what to observe every day. On top of that, the observers were not scientists. They didn't care a whole lot what it was they were

getting. So you had to have quality control—you had to look at all the data. It took quite a bit of time. I have a lot more time now to do research than I did then. Then there were the other problems of raising money and all that stuff.

COHEN: But you did spend your summers there?

ZIRIN: Yes, I spent my summers there with the family right from the beginning. It was a nice place to spend the summer, I should say. I bought a sailboat and I enjoyed it. On the other hand, I also knew that it was going to be a hell of a lot easier to get students to spend a summer there than it would if we were in, say, Lake Elsinore or some unpleasant place. So that worked out well. During the summer I worked very hard. Also, it turns out the summer is the best time for observing. Basically, this time of the winter you can do almost nothing, because there are a lot of storms and the air is unstable—not so much because of the snow, but because of the clouds and everything. The seeing is not very good either in the winter, because the air is cold from the night and then, when the Sun shines on it and heats it up, it becomes turbulent. So it isn't very good. So, one way or another, the summer was the ideal time to be up there. Then I could show the students how to observe and get them interested. And that worked out pretty well, although I only had a small number of students who stayed in the field. I felt it was very important, because there were only a few departments left in the country where they teach solar, and somebody had to produce new scientists. That was the worst part of the national observatories: all the money was going there, and they couldn't care less about teaching. They made a show—bringing in students each summer and so forth, but it wasn't a continuing thing. But we had a lot of fun. We got some good things done. What's-his-name—the guy who writes science fiction—was up there one summer.

COHEN: Brin?

ZIRIN: Yeah, David Brin worked one summer.

COHEN: Poor David Brin.

ZIRIN: Why poor David?

COHEN: Oh, he's not poor, but his movie *The Postman* isn't great.

ZIRIN: His movie isn't very great, yes. He's the only person we know who took classes from both me and Mary. He took Mary's Russian classes. He wasn't too good at astronomy.

COHEN: During all this period when Big Bear was coming on line and sort of moving and you were raising all the money, what about Caltech down here in the lowlands? Were you teaching?

ZIRIN: I felt, in the beginning, that Caltech was wonderful. That was in the days when Jesse [Greenstein] was running the department. We used to go to lunch every day together—Maarten [Schmidt] and Bev [J. Beverly Oke] and Guido [Münch]. Of course, Fritz [Zwicky] was out of it. Who else was there then? There weren't too many people in the department. Jesse sort of presided over it, and I found that very good. There were a lot of ideas.

COHEN: The Round Table at the Athenaeum?

ZIRIN: Yes, that sort of thing. We generally talked science and it was a pleasure. Later on people got older, they got to be big-shots, they had their own groups to talk with, and this or that. It really broke up when Jesse retired. At that time contact was lost, and that was very unfortunate. I had learned a lot from the other people, and it was really nice to be at a place where almost everybody was really a top scientist—that was a huge thing. I remember when I first got to Harvard it felt so good to come to a school where everybody was good. Then at Colorado there were some good people and there were some mediocre people—I felt it was a place that would not really recognize quality. So it was a tremendous thing to come to a place where you were challenged all the time and there were very few low-grade folks. I enjoyed Caltech very much.

COHEN: We're talking about the seventies now, and the beginning of the eighties?

ZIRIN: Yes. I seemed to have a fair number of students for the classes I taught, and everything was great. That was when the *Captain Corona* comics got written up. I had a lot of interactions with the students, and that was fine. I was just getting a lot of research done, because nobody

had ever been able to look at the Sun the way we did. We were seeing things that had never been seen before. In a way it was easy, because it was stuff that hadn't been seen, and in a way it was difficult, because when you looked at something you had to make some kind of sense out of it, you couldn't just describe it. And then in the beginning before I had my own data coming down from Big Bear, I finished the first edition of my textbook, so that was a good thing. I found Caltech just wonderful. It was unfortunate that Bob Leighton went off in a different direction, but for some years, when we were doing the site survey and even afterwards, we used to have solar lunch once a week. We'd eat at the Athenaeum, we would talk, and then we would talk with the guys working on the site survey.

COHEN: Now, you were still having good relations with Carnegie at this time, right?

ZIRIN: That's right. That was basically Bob Howard, but there were other aspects. For example, much of the optical stuff was done in the Santa Barbara Street optical shop. You would just bring it in. Nobody would think of whatever they were charging you for it, they would just do it. And I was quite friendly with Olin [C.] Wilson and other people like that. [Allan R.] Sandage's wife was Mary's cousin by a complicated connection.

COHEN: Oh, I didn't know that.

ZIRIN: Well, they had the same uncle. So in a way, Sandage and I are cousins. I had actually known Allan from way back. He had been at Harvard for a while—that was when I first knew him. Then, when I first came out here in '53, he showed me through Palomar. So I knew all the people over there. The groups were a lot closer. They were doing different things, but somehow, as time went by, people seemed to look down on solar work, and I became an outsider in this group here. But, you see, it wasn't their fault. It was as much mine, because I got a big group and I was spending all the time with this group. And if I went to lunch, I went with my postdocs.

COHEN: So you were doing the thing that you are saying broke things apart in a sense.

ZIRIN: That's right. Each person got his own group. Not all the people had a group up there, but it tended to be that way. I think that hurt the department. I don't remember when Marshall [H. Cohen] came and whether we were still that way or not.

COHEN: That was in 1968. Well, he was involved with the radio astronomers.

ZIRIN: That's true, too. That was beginning to become a different group also. Maybe that was so.

COHEN: I don't think they had adjusted to radio astronomy being respectable.

ZIRIN: Well, gee, Marshall can tell you the story of radio astronomy here, but it was a pretty crummy thing. Afterwards you find all the people who were supporting radio astronomy, but you don't find those who kept it down. [Laughter] Anyway, I think Jesse did a wonderful job here. Jesse was close to Leo Goldberg, in a way. They were deadly enemies, and yet they were the same generation and very close. Of course, when I looked at it I remember what my mother used to say, "It's a *shande* for the *goyim* that two famous Jewish scientists like that should be at one another's throats." [Laughter] And I saw a few other cases like that. Anyway, Leo became a big-shot, and Jesse was still a scientist. Jesse had far more influence, in a way, than Leo did. Leo was a pretty sharp guy too, but he got carried away by this power shtick. And that's the way it went. Of course, Jesse became the board chairman, so he had more power in a sense. His influence in the field was also very wide. Leo had a lot of influence, too.

COHEN: So you think there was a real change when Jesse retired. You think that was the end of an era?

ZIRIN: Yes, that was the end of an era, and it was unfortunate. You see, Jesse was not a cosmologist. He was a classically-trained astronomer, just like I was. As a matter of fact, all the other guys were too, but then everybody began running off for the end of the universe, which is today the most popular thing.

COHEN: Well, that's progress.

ZIRIN: Maybe that's progress. You just can't tell. I was running observing programs at Palomar all this time—as much as fifteen or twenty nights a year. There were some bad things, too—the whole Santa Barbara Street business, and Babcock. I can tell you something that happened to me—you know, everyone has their horror stories. I decided that I'd put in an NSF proposal for support of my Palomar work. I had never had support for it; I just went and did it and subsidized it out of my other grants.

COHEN: You were looking at the Sun at Palomar also?

ZIRIN: No. I was working on solar activity in stars. I was using the 200-inch telescope and was working on a rather original project that nobody else had done before. It was quite successful, actually, over the years, but I thought I'd get some money so I could get better equipment for it. Well, I put in a proposal. It got kicked back by the observatory committee. You had to have proposals approved by the observatory committee. Normally they approved everything. I think, in this case, they felt I was poaching on their preserve, as if the astronomy money were theirs. At that time I had put in my request for time that year, and Babcock called me in. He said, "Well, I think you are stretched pretty far, Hal. You're so busy, I really don't think you should do any observing at Palomar anymore." It was just because I had had the gall to try to get some money to support my work. He didn't give me any time, so I went back to the observatory committee and appealed, and they gave me a modest amount of time. But it was infuriating—the dead hand of Babcock. Babcock was such a funny guy. He was, in some ways, a rather bright person. But he was incredibly insecure and incredibly conservative.

COHEN: He's still around, you know.

ZIRIN: Yeah, I think I've seen him. In fact, I arranged one time for him to give a talk down here—I think to the library friends—about the old days. So I became fairly friendly with him afterwards. What the hell, you know?

COHEN: Of course.

ZIRIN: But he was a real pain to deal with. But anyway, that isn't important really. I felt the people at Santa Barbara Street were generally friendly—we were astronomers. I think people today are a little bit more uptight and competitive than they used to be.

COHEN: It certainly isn't like that anymore, but maybe that's how it goes. Maybe this is a good time for us to stop. [Tape ends]

HAROLD ZIRIN

SESSION 3

February 17, 1998

Begin Tape 3, Side 1

COHEN: Good afternoon, Dr. Zirin. It's good to have you back.

ZIRIN: Good afternoon. I'd like to say a little bit more about the observatory. I don't remember whether I told you this. We were using film, and film was a rather expensive and difficult medium. You needed rather expensive 35-millimeter cameras. They had to be pin-registered, which meant that they take the same position very accurately, and they had to fire off every few seconds, instead of just continuing. We bought one or two of them for about $10,000 each. Then we started making them by ourselves. I had a very good engineer named John Cowley, who has now passed on. There was another good engineer who is still here on campus—Rich Goeden. Between those two we started getting cameras from the military in surplus and modifying them until we eventually had about eight. We probably had more cameras than anyone else in the world. We had to have a processing machine to process the film. We had to have a printer which would convert 35-millimeter film to 16-millimeter film. We had to have special projectors so we could look at things. It was a very complicated thing which took some years to really put together. It enabled you to look at events as movies. Basically, this had been done very little elsewhere, and in fact wasn't very popular, because the science is very heavily influenced by academics, and the academic idea is not to learn about something but to write down numbers about it and give a journal article and things like that.

COHEN: Prejudices are showing.

ZIRIN: Yes. The other advantage we had was that because I had bought all these filters, we were able to, when a flare occurred on the Sun—which we couldn't predict at the time—look at it in many different wavelengths and scales, so we had a lot of information. That really proved to be very valuable over the years. Not everybody liked it, but at least I did. [Laughter] Many people did, because we kept on getting supported. That was also because nobody else was doing it.

Then the digital cameras began to appear. My first digital camera was built by Lockheed. Later on I realized that we had reached the point where, although the digital cameras were very expensive—about $10,000 a pop—the cost of film, the cost of the film laboratory, and the value of our cameras would rapidly be paid for if I switched to digital. In fact there were some severe budget cuts at that time. We managed to weather all of them by simply stopping our spending on film—which was nearly $50,000 a year—and our processing. We had two photo labs going at the time. So we converted to digital. That of course brought us into a new age of digital images, which we're still learning about. I brought people in who taught us how to do it. It turned out that the technology enabled us to do all kinds of image filtering. For many years we never processed our images beyond just making prints. Now we could filter out the image jitter and really get good stuff. That truly revolutionized things, and it's still going on. That's one reason I want to do some more observing, because there are things I never got to do until digital cameras appeared. Then there were two real traumas. The first didn't come to anything, but took up a couple years of my life. Sara Martin had come to work for me around 1985, I think. She was a lady who just had a bachelor's degree, and she had worked at Lockheed observatory. They were interested in taking images, and she was interested in studying them. So she had done some good work. I wouldn't have hired her, because normally I just hired PhDs, because I felt we were doing more science which required technical education, but she had her own funds. She came with a dowry, so to speak.

COHEN: Where did she get this money from?

ZIRIN: She was very popular in the field, particularly with the Air Force people. In part, they liked having a female principal investigator [PI]. She was just very good at raising money. We had an agreement that she would bring her own money, and as long as she had that money she could work for me, more or less autonomously. I think that probably was against the rules here, but, at any rate, we did it.

COHEN: So was she a Caltech employee?

ZIRIN: She was a Caltech employee for all those years. She came over with a grant that first was written to me, and then we got her a position which enabled her to be PI on the grant, I think as a member of the professional staff, or something like that. She didn't have to have a PhD for that. Many people are MPS [Members of the Professional Staff] who don't have that. She didn't work with me—that was an interesting thing—but generally with visitors. We never wrote a paper together. She wrote only one paper with someone in my group, and even that was just with the person who helped her with the computer. She tended to prefer working with outside people. After about ten years—I think it was about 1995—her funding ended. In retrospect, I thought that was peculiar. I don't know what set it off, as she was quite good at raising money. In fact, right now, since all this happened, she is still raising money. She formed a little private company and she is getting grants, but, for some reason, at that time she didn't get any. I told her that I wouldn't be able to support her, because she was the highest-paid person in my group. She was getting paid about $60,000 a year, which means about $120,000 with overhead and everything, so there was no way I could keep her. I don't think she paid much attention to it. Finally I sent her a termination notice. At that point she filed a complaint to Caltech against me for sexual discrimination. Not harassment—none of these people ever accused me of harassment—they thought I was too old, or something. So she brought this complaint. It was really not very pleasant at all. We had a number of hearings here—there is a regular format for it. Among the things she suggested was that I should fire a couple of postdocs so I could pay her. I felt I owed her nothing, because we had an agreement that as long as she could support herself she could be here and when she couldn't then I wouldn't keep her anymore. The thing went up and down, and the group found in my favor. I could cover all the arguments. But basically I guess I never quite understood why she just didn't raise her own money. President Everhart—I guess I was abroad or something when this came up—had to review the findings. I never found out the findings. He reviewed the findings and sent it back to Human Resources. I've never seen what his review was, except that there was nothing that Caltech could do, because Everhart had to say, "Yes, I agree with you and we'll do this," or "I don't," and all he said was, "Gee, that's too bad."

COHEN: You mean that he didn't give a "yes" or a "no" on it?

ZIRIN: Right. They had to send it back to him. I think I was in Japan at the time. The committee had decided completely in my favor, because there was really nothing to support her claim except these continual accusations of sexual discrimination, which I don't think were ever proved. Also, I had quite a number of women working for me. As I had pointed out, when I started out, one reason I liked to hire women was because they were cheaper than men.

COHEN: [Laughter] Well, that's discrimination.

ZIRIN: It may be discrimination, but it's a true statement. It seemed to me that I should be allowed to hire the employee I could get for the lowest wage, and get the best person for the amount of money I had to spend. At any rate, that was my feeling. I don't understand anything else. Because—especially when I started here—you really could get quite a well-educated graduate of a women's college—an intelligent person—for not a great deal of money.

COHEN: I think there's a precedent in astronomy for this, actually.

ZIRIN: Yes, but the point is, what's wrong with that? Should you not hire them, and hire dumber people? Should you pay them more than they are willing to work for? What is the right thing? Because you've only got limited funds. Everybody works voluntarily. Slavery is gone. Anyway, that's what happened. Anyway, Everhart changed his mind and wrote that he agreed with the finding and that he would provide her with a year's support, because she didn't understand what she was doing. Of course, she understood very well what she was doing, because she had been doing it—she had raised her own grants and everything. She proceeded to use that year to bring a whole series of complaints against me. That, I think, was the worst thing Everhart did to me—to set her up inside Caltech, where she could continue bringing these complaints. She got two other women who were working for me—you see, I was so prejudiced against women that I had this small army of women in my group—about thirty or forty percent of the group, I guess. One was a black lady, who was running our photo lab. She was so black that nobody could mistake her color when I hired her. I had hired her because the fellow working for me in the photo lab thought she was the best qualified, and she was moderately qualified. But that was when I had decided to quit using film. Now, when you quit using film

you don't need a photo lab anymore. So I gave her a layoff notice. Now, I should say that I had warned Human Resources about this the year before and they did nothing. I said, "Look, we're going to get into trouble with this. You should look for something for her at JPL." So she was induced to write a grievance, too. And because she was illiterate, Sara wrote her grievance for her.

Then we had a lady graduate student from the University of Colorado. Her advisor, who had once been a student of mine when I was at Colorado, had left the country, and this student was left high and dry. So she asked permission to come and study with us—in particular with Dale Gary, who was a senior research fellow in my group. She too accused me of—I can give you the details—not being nice to her, of discriminating against her. What bothered me about this was that I had made an appointment—I was trying to see her every month or so—to find out what her progress was. All of a sudden, on the day she was supposed to see me, I was told she wouldn't come to see me because I was no longer her advisor. When I called up, Arden Albee told me that she had complained about me and they had decided to transfer her to another advisor—without ever telling me, which I thought wasn't very nice. I was perfectly happy to get rid of her as an advisee, because I didn't think she was very bright. In fact, my intention that day was to tell her, "I really don't think you're going to make it through to a PhD." She had another complaint about an incident that occurred over a Fourth of July weekend. She had filled up our computer disk so that I was unable to store some data I had received that I was trying to store. I felt that it really wasn't fair for a visitor to take up thirty percent of our disk space when about fifteen different people used it. So I deleted some of her files because otherwise I would have lost my data, and I gave her information on that. So she brought that as a complaint. But these were not internal Caltech complaints—they went to the Department of Education, or whomever it is that handles those things. And a man came down and he looked into these complaints, and of course the thinking was, "Gee, if one woman brings a complaint it's possible it's unfounded. But if three women are complaining against you, then you must have done something terrible." They never considered the fact that I had three women working for me, as well as another three or four. At any rate, the Inspector General came down and visited with all the people here. I never saw him. I don't think Caltech wanted me to speak with him. Finally they reached a decision which was fairly harmless—because there really was no evidence of discrimination—that I should step down as director for three months and Ken Libbrecht should take over. That

was fine, and we were very careful—because we knew that Sara would be watching us—that Ken should really act as director. I continued to supervise some of the work on my own grants, because they can't take your grants away from you.

COHEN: Why would this alleviate anything if you weren't the director anymore?

ZIRIN: I don't know.

COHEN: It doesn't make any sense.

ZIRIN: I know, but you see, they were doing a certain bargaining. The guy comes down, he wants to achieve something. I mean, you'd have to ask Ann Bussone about that, because she was the HR person involved. By the way, I got really good support from Human Resources from Ann and [Associate General Counsel] Kathy Katovich, who was here then. They helped me a lot, as did Nora Knicker—now it's Nora Harrington—my secretary. They were all very supportive and helpful. I think I had as many women on my side as there were on the other side. [Laughter] Anyway, Sara was still mad despite the fact that the decision had been reached. Her proposal was that they fire me as director, and she would help them choose a new director. That was pretty naïve, because I was the principal investigator on all the grants, and I was the only person who knew how everything worked up there anyway. But, you know, just the chutzpah of it… [trails off]. From another perspective, I would say this kind of thing does more harm than good, because an experienced person will realize that if he ever hires a minority and decides he doesn't want him or her, he can't fire them. Therefore, the smartest thing to do is to avoid hiring minorities.

COHEN: Yes, that's what one could conclude.

ZIRIN: A lot of that has happened. I found it difficult to resist that reasoning. Actually, I haven't hired any women. We had one woman researcher who was very good. The good ones are no problem. It's really the second-rate ones that are raising hell. In any event, Sara brought a complaint to Washington, through the San Francisco office of this anti-discrimination commission, to the effect that the commission was not doing its job—that, in fact, I was still

director of the observatory. At that point, the people in Washington apparently got mad at her too, and that sort of ended that series of complaints. After that, there was a separate Caltech hearing on the case of the black lady who complained that I had discriminated against her. That time they went and found her a job at JPL, which I had requested years earlier. I think that had been a failing of Human Resources—that HR was busy and didn't take care of it—because she's not useless. I mean, I have to admit that I was looking for a way to get rid of her, because she wasn't doing her job. As for hiring women because they were cheap, it was something I had to do, because I had to save that money.

COHEN: It was just convenient.

ZIRIN: It was more than convenient—it was necessary. Because of the Sara Martin thing, I had had my budget cut on my NSF grants. She had friends in Washington.

COHEN: She sounds like a powerful lady.

ZIRIN: A very powerful lady. She got a group of a half-dozen scientists who knew nothing about the case to come out here and write a letter to the president of Caltech asking him to look at her case. These were people who never asked for the facts. They just thought, "Of course he's discriminating against her." She actually was not stupid. She was uneducated, but she was bright. But somehow she got into this bitterness thing, which hurt her. She started up a private company for which she gets money in grants.

COHEN: Is she still working on the Sun, as she did before?

ZIRIN: She still works on the Sun. Every year, or every other year, we let her come up and observe at Big Bear. It was strange, because she had really put a lot into the place. She and her husband built a house up there and planned to retire up there and come over and observe. So it was really a sad thing all around. That leads me to what happened finally to the observatory.

COHEN: But how did this all end then?

ZIRIN: It ended with nothing happening. The women all went away, and I went back to running the observatory. The only change was that our budget had been severely cut through Sara's influence in Washington, but that's come back up since. We went back to doing science.

COHEN: What year are we talking about?

ZIRIN: Well, I'd say that episode ended probably about '96. I think it probably was happening from '94 to '96, but I don't remember the exact years. So we went back to doing science, but it became a little more difficult to find good people. You know, this is a field which really has been shrinking. I really can't understand why, but I have some ideas. When I started, a lot of people did solar research, and there were a lot of departments across the country training students. One problem was that much of the research was done in government labs, where it's not done at a very high level. Another problem was that the national observatories were in such bad sites that people couldn't really see the Sun. We were doing a lot of the observational stuff here. Then they began doing it in Europe. Then the CCDs came into the astronomy field, and the quasars, and everyone ran off to look for God. So the decline was the result of people wanting to go do something else and of people being discouraged from studying the Sun because they were very much limited. The CCDs and the Big Bear observatory created a new pathway where you could learn new things, but it was very difficult for us to sell that idea, although I've always had money, because we were the only place doing it, and they had to support us. But other people weren't getting into it. Now I have sent out a bunch of former students—postdocs that gave me a sixty-fifth birthday party recently—and those people are contributing, but not enough. So I don't know what's going to happen in the field. Hopefully it will hang on, because everyone always has to know about it, from a practical point of view.

COHEN: But they are doing this in other countries, aren't they? I mean, is there good research going on in other places?

ZIRIN: There is, but they have troubles, too, although not to the extent we have here. If you want to hire a new student in Germany, you can get one. In Holland, I know a guy who says he's the last solar physicist in Holland—and that used to be the main thing there. By and large, people

have mostly gone off to study the quasars. Eventually the cosmology field will be saturated. I remember when I went into astronomy there were about three people studying galaxies in the country. Everyone else was studying stars. These trends go back and forth. I don't know what will happen to the study of the Sun. Until they relocate the national observatories it won't go far.

At any rate, back to the transition with Big Bear. Gerry Neugebauer was the division chairman, and he was quite concerned about what would happen to the observatory. Gerry's a really good guy—not very pleasant sometimes, but a really good guy. He's just like Robbie Vogt, with that kind of character. Gerry hit on the idea of bringing a number of prominent solar astronomers to give talks here at Caltech to the astronomy professors, in order to convince the astronomers to keep the observatory. So we had a number—I think three or four—of well-known people, like Gene Parker and this fellow, Rütter, from the Netherlands. They came and talked about what was going on in the field. It made a very good impression. We had a division meeting, and the faculty agreed to look for a successor to me to keep the laboratory going. A search committee was set up, and we came down to three candidates. The three candidates were invited and there was a certain amount of discussion back-and-forth. We hit on a fellow named Christoph Keller. He was from Switzerland, from the ETH [Eidgenössische Technische Hochschule (Federal Institute of Technology)] in Zurich. We offered him a position, and he said that he thought he'd take it. So then it was brought to the divisional faculty, and the divisional faculty was split over it. Basically, the nighttime astronomers came out with the statement that since we had the Keck telescopes to operate and we didn't have enough slots, we shouldn't be appointing a solar astronomer. The vote was sort of fifty-fifty, with a slight majority in favor of appointing him. He was a very talented young man. That was that. He wasn't hired. There wasn't anything I could do about it. If we had had a strong administration then or were doing it today with a new president, I would have fought harder, but it was fairly clear that nobody was going to do anything to help me, and that meant we had to have another owner. It also meant that Caltech was going to lose a great deal of money, but I don't think the administration understood, or cared, much about money. So now they're out the money—about $400,000 a year, not a small amount. And the best solar observatory in the world is gone.

COHEN: This was from overhead and grants?

ZIRIN: From overhead, yes. I'm sorry—that's an exaggeration. We were bringing in about $500,000, but then Caltech was giving us a budget of a couple of hundred thousand dollars to run the place, because, you see, they are supposed to support it. Then they collect that back from the overhead. So they lost maybe $300,000 in cash per year. Actually, they still have some of it, because I have some other grants running, but there was probably a clear loss of about $200,000 a year, plus not having the observatory anymore. But that was an indication of how much the study of the Sun has declined. Maybe it's my fault. I don't know.

COHEN: Did better sites appear?

ZIRIN: No. I don't think so. We truly had the best observatory in the world. Somewhat later in this period, a new man took over the National Solar Observatory. He had a new way of testing sites, and he did a new test and showed that all the sites where there were observatories in the world were inferior to Big Bear—that Big Bear's just qualitatively different from any other site. So that again started up the possibility of moving the national observatories to Big Bear, or to someplace like it. I've taken the position that it doesn't have to be Big Bear, but it has to be the best place, which is a new and original plan, believe it or not. You'd think you'd naturally put it in the best place, but no.

Well, to make a long story short, we were stuck. An announcement was made that we were looking for people to come in and take over the observatory. We received one proposal. That shows how far down the field is. There are several national laboratories which should be running sites like this—and are running bad sites—that were not interested. The only place that was interested was the New Jersey Institute of Technology, which is kind of an odd place. But a fellow named Phil Goode had taken over the physics department there. He managed to persuade his institution that this would be a valuable thing for them to do.

COHEN: Did he have any previous connection with solar astronomy?

ZIRIN: He had done some work in helioseismology. I don't think he had ever been up to Big Bear. However, a year earlier he had had an assistant professorship vacancy in solar physics, and he hired my student at the time, Haimin Wang, who had taken his PhD here and then stayed

on as a postdoc and was really doing outstanding work. Haimin went back there as an assistant professor. He's now an associate professor, and he'll probably get tenure shortly. Then, because we were operating this solar radioastronomy lab up at Owens Valley, and because that site had several substantial grants too, they offered a position to Dale Gary, who was a senior research fellow with me. When you ask for a grant, you have to give a list of all the grants you have. In order to run a big operation like that, I had a lot of grants and a lot of money. I didn't want to write down "these are Hal Zirin's grants," so I turned a bunch of them over to the other people. Sara Martin was the PI of her grant. Dale Gary was the PI of several grants. There were other people in my lab who I put on the grants. I just decided that was the easiest way to do it. Then, in principle, they would write the proposals and everything. It wasn't completely legal, but it was somewhat legal.

COHEN: I thought you had to be a professor to be a PI on a grant. That's not true?

ZIRIN: No. That was changed a number of years ago. You do have to have a professor say, "Yes, I approve of this." For example, in the case of Sara, because she was a woman she had a path to getting grants. You know, you hear all these stories of discrimination, but in fact there is equal opportunity—or affirmative action even, because in science there are so few women that these guys are looking for a woman as a PI. I was asked several times to make Sara PI, so they could show her.

Anyway, both Dale and Haimin were hired. They got faculty jobs, which are not so easy to come by today, even if it was at NJIT. It's not a bad school, by the way, but not a real strong school. I think they rank sixth or something among the technical schools in the country. So last June they took over.

COHEN: Are they affiliated with Rutgers?

ZIRIN: Yes. For example, they gave me an honorific position of Distinguished Research Professor, and now I'm also a research professor at Rutgers. Haimin also teaches at Rutgers. Rutgers and NJIT share a campus in Newark. The main Rutgers campus is in New Brunswick,

but a big chunk of Rutgers is next to NJIT in Newark. I don't know the details so well, but the two are similar. Rutgers is old and NJIT is relatively new, so not as well known.

COHEN: Rutgers has a very good reputation.

ZIRIN: Certainly in physics they do—they have a lot of fancy people out there. So NJIT's running Big Bear now, and I fight with Phil Goode.

COHEN: [Laughter]

ZIRIN: That will be all right, though, I guess. I hope. I want to go out and build a new national observatory. So that's the end of the story of the Big Bear observatory, I would say.

COHEN: Are they still running every day?

ZIRIN: They're not running so much—they run on a somewhat different philosophy—but they do take synoptic data every day. Otherwise, there is some debate. What they are doing is something that I must say I am impressed by. There's a big problem with heating in the dome; you get air escaping from the dome. And Jacques Beckers, the guy who is doing these site surveys, put two heat detectors at Big Bear, one on a tower outside and one inside the dome, and he found that the conditions inside the dome were significantly worse. So Goode has been trying to get rid of all the heat sources in the dome, and that has cut down the amount of observing. The other problem was that he took over all the people who had been working for me. Now, they were okay as long as I was bossing them around, but Phil doesn't know enough about the details of observing to really boss them around, so they're not doing as good a job as they did before. So I often say, "Well, it's my fault; I hired all these guys." But I was able to keep them going by hollering at them, and now they can get back at me for all that hollering. In fact, I've had a great deal of difficulty with them, but it's okay. Something good will happen.

COHEN: Are you still observing up there?

ZIRIN: When they let me. The problem is that, when I go to observe, I don't get any support. They refuse to help. I just spent about an hour with Phil yesterday working out a way to get around that. I think what I'm going to do is bring up my own people. I have some money and I have some people working for me. Then I have the guys who used to work for me who are supportive. They stayed down on campus, because they didn't want to go up there to work with those people. So I'll be bringing up my own people until things get straightened out. I don't know when that will happen.

COHEN: But at least the observatory is working.

ZIRIN: The observatory's working—not as well as I would like to see it—but it exists and it is working and probably will continue. The main thing now is that, because of the direction they've taken, they are not doing much science. Eventually they've got to do science to get further support.

COHEN: Now, Caltech eventually bought all that land, didn't they?

ZIRIN: Yes, that's right.

COHEN: Do they still own the land?

ZIRIN: Caltech owns the land. That was a great bargain. We had gotten the lease, and then Ivan Betts told me one day that they had bought up the land for $60,000. The land is worth about a million dollars today. I think it was leased to NJIT for five percent of the payroll—a rather small amount. I guess I helped them fight for a low amount, but in fact I shouldn't have, because I am supposed to get the rent. I haven't gotten it yet, but I think we were supposed to get it and use it for solar physics. In any event, we're managing. I have a small group—now it's just a couple of guys—and that enables me to focus more on research. So it may not be so bad after all. I spend all day on research. It's really neat.

COHEN: Well, there are advantages to not being a boss anymore.

ZIRIN: Right. So let me tell you the other stories. Let's go back and start with Russia.

COHEN: Now these are concurrent activities to the solar observatory. Is that how we should categorize them?

ZIRIN: Well, we went to Russia in 1960-61. That was before the solar observatory got started.

COHEN: You were not a professor here yet?

ZIRIN: No, I was still at the High Altitude Observatory. We had been there in 1958 to the IAU [International Astronomical Union]. You were there. I had an advantage with the Russians. I read Russian; I had studied it at Harvard, and my wife was taking a PhD in Russian. So I knew the language reasonably well. I had been reading some of their papers, and I had referred to them. I had no idea how much that would affect them, but they were coming out from behind the Iron Curtain, and the fact that other people could see their work and recognize it meant a lot to them. They were doing a lot of solar physics at that time. They still have a couple of pretty good programs, but not very good people. A couple of the programs are good. So I had the great joy in '58 of meeting all these people whose names and work I knew, and who knew my work. We spent six weeks there and drove down to the Crimean observatory. The amusing thing is that our trip was financed by the CIA. The CIA was very anxious for me to go down there and see the observatory. It probably wasn't real espionage, but it was glamorous to do it that way. We just looked around and kept an eye on things, and then after we came out, we were debriefed by them.

COHEN: Actually, everybody who went to that meeting in '58 was debriefed in some way.

ZIRIN: I think that was probably pretty true. For us it was good, because we went to Munich for three months after that, and we got taken to an awful lot of good dinners in Munich by the CIA guy. Anyway, I was invited to go again in 1960, because they liked the idea—you know, I spoke Russian and I was interested in observing. So they invited me to the Crimean observatory. Mary decided we should go across in the winter, because she hated the flies and dirt that you had in the summer, so it was agreed that we would go in 1960. It was very difficult to ratify it.

Finally, just a couple months before we left, we received what the Russians call a "*pamycetnaya zapiska*," an *aide-memoire*—a "remembering note." And it said that they agreed to the exchange of Harold Zirin for Alexander Boyarchuk. We arranged to buy a Volkswagen—to pick it up in Wolfsburg and drive it into Russia. Three days before we embarked for Europe we got a telegram from Severny, the observatory director, saying it's okay to bring a car as a tourist but not to bring a car if you come as a scientist. We didn't pay any attention to it, as I had already bought the car. We went and we picked up the car, and we drove up through Sweden and Finland and into Leningrad. I could spend all day telling you about that.

COHEN: Well, let's move on.

ZIRIN: At any rate, we lived there for six months at the observatory in Crimea. It was very good scientifically for me. They had observed a lot of things that I had not been close to observing before that, and I don't think I would have done what I did here had I not had the experience there. I just learned a hell of a lot about instruments and magnetographs and things like that. We also learned a lot about Russia—about how lousy it was. As we've told people before, as bad as you think it might be, it was much worse. That was pretty much it, except that we made friends and it was nice in some ways. It was quite a fantastic adventure. Maybe we should get back to Caltech.

COHEN: Right. So that was a concurrent activity while you were at the HAO?

ZIRIN: The HAO was just kind of a lab.

COHEN: So you started your life at Caltech as a full professor.

ZIRIN: That's right. I demanded that, and I got it. I liked the idea of faculty committees—I know a lot of people object to it. I was also fairly active on the student houses committee. I was shocked that Caltech was not like Harvard, which was the only other school I really knew. When I retired, Caltech was still not like Harvard. [Laughter] I was chairman of the library committee for some years, along with Noel Corngold. Noel and I have been on several committees together, at Harvard and at Caltech, and I enjoyed that. My life has been made a lot

more pleasant by having people like Noel and Marshall [Cohen] around who are old friends and who I can work with. The library committee was a little frustrating, but, at any rate, we chose a new librarian. I think I really did the most on the financial aid committee. For some years I was just a member of it, and later chairman for three years. That's where the SURF program comes from. I can never remember dates, but it was probably around 1970 that Caltech got a grant from Lew Wasserman. Wasserman's goal was to enable every Caltech student to be on an equal financial footing as a student going to Berkeley, because he knew we were losing students to Berkeley. Wasserman said that he really couldn't afford to do that, but he would give a certain amount of money that we should use to help the better students who couldn't get financial aid. This was in the late sixties or early seventies, and the committee was dominated by people who were socialists. They voted to use all the money to bring in Hispanic and black students. I was infuriated by this, along with others. There were a couple of us on the committee who thought this was outrageous, because it wasn't what Wasserman gave the money for. So we went to see [David] Morrisroe, because Morrisroe had gotten the money, and Morrisroe came back and explained to the committee that Mr. Wasserman would not be very happy to hear that we were going to use his money for poor but dumb Hispanic students.

COHEN: Well, they're not necessarily dumb.

ZIRIN: Not necessarily dumb, but obviously they were not going to be what he was after. If they were good, surely everybody would run after them. So the committee reversed its decision, and then the discussion began about what to do with it. I was the originator of two things in this regard. First, the merit scholarships, which were, I think, completely my idea, and then the SURF program, an idea I shared with Fred Shair.

COHEN: What was the merit scholarship?

ZIRIN: Well, it still goes on today. Actually, it was funded by Carnation, in the form of fifty shares left to Caltech. The shares eventually became worth a million dollars apiece, so they could finance a lot. We developed the idea of giving scholarships to upper-class students, pretty much independent of need, except that—and this was the dirty part of it—if a student already

had a scholarship, he didn't get the additional money; he was only allowed a total of a certain amount. But for students who didn't have scholarships, this was really terrific.

COHEN: And this was strictly on merit? That's why you called them merit scholarships?

ZIRIN: Yes. Students can apply in their upper-class years—freshmen are excluded. So basically it's an opportunity for a third- or fourth-year student to get a really healthy chunk of money. I think about five of them are full tuition—about $15,000. All together there are about fifty awards going down to about $5,000. We used to argue over the years whether it would go one way or another. The students present their applications, and get recommendations from their professors. We look at their grades. Then there's this terrible thing of having to go through about 150 or 200 applications. It's hard to get members for the committee. But the merit scholarships got adopted. In the meantime, I had been shocked at the way Caltech students worked very hard on their classes but really didn't do much research. There were several reasons: People were busy, but also it took money to have a student working for you, and in those days a lot of professors didn't have grants. In fact, in some departments they were discouraged from getting grants—you know the story of Mount Wilson and astronomy. So, one way or another, they weren't getting any of what Caltech had the most of to offer. So I proposed that we start some kind of fellowship where we would pay for the student to work with a professor. Fred Shair and I were sitting in the back of the room talking about what we should call this thing, and I don't remember whether he or I came up with the word SURF—Summer Undergraduate Research Fellowship. It just fit perfectly. The committee accepted the idea, because they had been driven away from this socialist thing. [Laughter] I mean, we have some smart professors, but in some ways they're not very bright. I think I mentioned to you that Fred Shair and I went to see Murph [Marvin L.] Goldberger with this wonderful idea. It really is a great idea.

COHEN: It was a good idea. It's been very successful.

ZIRIN: That's true. There are some weaknesses in it, but it's a good thing. By the way, I was just astonished at the luke-warm reception we got from Murph. I would have thought he'd say,

"Geez, that's great! Let's go for it!" Instead he said, "Oh, yeah, it might be all right." Maybe he wasn't ready for it that day, maybe you have to prepare people. I don't know. At any rate, I was busy running my observatory, but Fred was kind of looking for something to do, so he picked up the ball and ran with it. That was really my main contribution, although Fred and I did discuss things together.

Begin Tape 3, Side 2

ZIRIN: I think Fred knew his way around the campus politics better than I did. But, at any rate, he got it going. He raised the money, and got various people. After some years the Institute began helping him. They were not tremendously supportive at the start—out of inertia, I think, as much as anything—but now it's a regular part of the fabric around here, and a lot of other schools do it. It's a good program. When I was at Harvard, Bok used to hire all the undergraduates to come out and spend the summer at the Harvard observing station.

COHEN: Ah, so you had the idea from that?

ZIRIN: Yes. We all ran the telescopes then. At Big Bear in the summertime the days are very long, so our regular observers couldn't really do everything. I started right away hiring undergraduates to work up there with my grant money, and also with the hope that some of them would stay in the field, because somebody has to encourage good people to go into it. And there have been some who did, although not as many as I would have liked—maybe a half-dozen people who are still active in the field. So SURF eventually worked out very well, as did the merit scholarships, although we don't publicize them. Some years later I became chairman of the financial aid committee. It's the only committee around here that has any money. Basically Noel and Fred and I ran it for those years. Actually, another thing happened earlier which wasn't very nice. I was supposed to be the next chairman of the financial aid committee, and I had discussed this with the nominating committee. Then, at the last minute, they told me that they had promised the chairmanship to be nominated by Lance Davis [an economics professor]. He was kind of a political type. His ex-wife is the division administrator. Apparently, Davis had gotten a commitment that he would get to appoint the next chairman of that committee. Boy, I

really got mad. But they said that was their committee and their commitment to him was stronger than their commitment to me. At any rate, a year or two later I became chairman, and the first thing I thought of was the Millikan fellowships. My thinking was, if you want to get the very best students—and you couldn't give more than a certain amount of money because of rules and regulations—at least you could give them a medal. So we launched the Millikan scholarships, and that turned out to be a disaster. Perhaps not a disaster, but if you look at the records of the students they picked, they did worse than the average student here. I still don't understand why. Somebody told me that the problem was that those students were not chosen by the admissions faculty but by the administrators. Again, I didn't follow through—I was too busy running an observatory. So that was a good idea, but it didn't work out. I'm trying to remember what else we did. We modified the loan program.

Begin Tape 4, Side 1

ZIRIN: There are a couple other things that I thought were interesting in my adventures here. When I first came, it took me a few years until I got my first grant to start working on the observatory, and I was shocked by the amount of money I had to pay in overhead. One year an announcement came around from Bob Gilmore, who was then the vice president for financial, saying, "I am pleased to announce that we have been able to raise the overhead rate from fifty-four to fifty-seven percent." He thought this was a great thing, reflecting the fact that a lot of the administration had never gotten grants for themselves because they had been administrators or they were there before there were grants. They didn't understand that the overhead came out of your working money. A couple of government agencies would add the overhead on, so that was good—I think that's what Bob was thinking—but the rest of us were paying it out of our pockets. So I rounded up some people who were all paying too much overhead, including Gerry Wasserburg.

COHEN: Was it too much, or just what it was supposed to be?

ZIRIN: They were paying what was supposed to be paid, but they felt that was too much. And it was being ratcheted up by a few percent every year. One aspect was unfair: while we paid the

overhead, it was the Institute that negotiated with the Office of Naval Research. And the Institute was interested in making that overhead as high as possible, because they thought they would get it for their unrestricted funds. The people involved were—I'm suddenly terrible on names—Wasserburg, Bob [Robert] Christy, who was provost and acting president, and a couple other guys. And Morrisroe spotted us in the Athenaeum and came over to say, "I wonder what this revolutionary committee is up to." [Laughter] At any rate, we were fighting to lower the overhead, or at least restrict it, and I do think it had a salubrious effect. I think after that Morrisroe tried to keep it down, because he was always trying to please the faculty, and it was kept lower than other schools—higher than I would have liked it, but at least lower than other schools. And that made a huge effect, because the question is, when you get money from the government, who is the better judge of how to spend it? The researcher or the administrators? Now our administrators aren't bad compared to other administrators, but still… [trails off]. Also, if the overhead is high, it means the money is going to non-productive things, which is even worse. So I felt that business with lowering the overhead was a bit of an achievement.

The other un-achievement was when I was on the faculty board. I tried to get them to let us invest our retirement money outside of TIAA. At that time TIAA did offer a common stock option, but if you looked at what they were doing, they were way under-performing the well-known mutual funds. About once a year there was an article in *Barrons* asking why TIAA was doing such a lousy job and how come they were squandering the money. The universities knew nothing about finance, and most of the vice-presidents of finance knew very little about it, too. They just wanted to cover their asses. They felt that, if they gave the professor control over his retirement, he would spend it like a drunken sailor and they'd be left with this guy that they still had to support, even though they'd paid for his retirement. So I proposed that we be allowed to at least invest the 403b money, but also our retirement money, in some of the well-known mutual funds like Vanguard, Fidelity, and so forth. The faculty board, to the man—I think there was only one exception—all started hollering, "1929, 1929, 1929!" They had this idea that, if you invested your money with Fidelity, you might go broke in a crash, whereas, if you invested it with TIAA, it would be safe. They didn't understand that all TIAA was doing was also investing money, except not as well. With Fidelity you had a chance. Now TIAA has changed quite a bit. They are still below the average of the market, which sort of outrages me. All of those guys would be a hell of a lot better off today if they had gone along with it. It was the same

impracticality that came up in the overhead business. Very bright people just didn't understand anything about financial matters.

COHEN: People don't like change either.

ZIRIN: That's right. They don't like change, but feel they're safe the way they are. So I felt those were two things that I did while I was here. Otherwise, I did a lot of research, and had a lot of students.

COHEN: Did you have any other leaves after that when you were here? You went to Japan several times.

ZIRIN: Oh yes. It was my habit to go away for about three months at a time every couple years. I went to Paris twice and to Japan twice. I have been going to Australia in recent years. I didn't go to Russia again. I went to Italy for a month—not very long. That was about it. The trips abroad were wonderful as trips abroad, but from a scientific point of view they were mixed. In some cases I would get some great ideas. You'd talk with new people and get ideas. In some ways the research going on in these places was not all that great, although I did enjoy my trips to Japan, because I had more people to really work with. France was a pleasure as France, but the level of the research there was not strong at all. Again, I think that's solar physics, rather than the rest of astronomy—I don't know about the rest of astronomy. Oh, the other place I spent my visits was in Israel. I had several long trips to Israel. I had an observatory in Israel that I ran: I had an empire around the world. Eventually it folded, for a whole lot of reasons.

COHEN: Coming back to observations, except for the couple of years that were unpleasant, would you say your experience here has been good?

ZIRIN: Oh God, it was great. It's funny, because I think, "Well, I have some money to leave. What shall I do with it?" Mary doesn't want to leave it to Caltech astronomy, because she feels they weren't nice to me. I feel that nowhere else would I have had the chance to do what I did at Caltech. Once you are a professor here, people will listen to you and you can do pretty much what you want. And the school does back you to a certain extent. Usually I prided myself on

not asking for any money, but the two or three times I needed money, I got it right away—maybe because I didn't ask very often. At other schools that doesn't happen. When I see the way these poor guys from NJIT are treated, I realize that this place is really run with the idea of enabling the professors to do the best possible job. Most other schools kind of view them as a nuisance. Well, the thing also feeds on itself. The professors raise a whole lot of the money, so they are entitled to the kind of support they get, because what the administration brings in is nothing compared to what the professors raise.

COHEN: Now you have actually been here through all the presidents except for Millikan. Is that true?

ZIRIN: That's right. It's funny to think of that.

COHEN: Do you have anything to say about any of the presidents?

ZIRIN: I suppose I could. DuBridge seemed like a wonderful man to me. He mostly had no fear—he just wanted to build up science. I remember when we needed money I went to him and it just came right back. He used to come to the Athenaeum and sit at the astronomy table. Other presidents have done that once in a while too, but DuBridge had something to say, and he was very interested in what we were doing. He was an intellectually active man. Everyone says that he was president at a time when there was more money. I don't think that's true. I think there's a lot more money today than there was then. I think he was closer to running the thing. He was a more experienced administrator, among other things, but he also had Bob Bacher with him, too. The pair of them were Mr. Inside and Mr. Outside. And Bob was okay, too. I remember he hauled me over the coals once. I had hired a young man to be a postdoc, David Bohlin. The guy had to go back to take his final PhD oral. I guess I was out of town or something, so he went to whomever was running the finances of the observatory and asked for money for the trip. Then he went back to take the final oral. Well, I didn't know anything about this. I got a call from Bacher to come see him. When I went to see him, he said, "You hired this guy and he hadn't passed his oral." And I said, "Well, he submitted his thesis. The oral is…" "Never mind! You certified that he had his PhD." Bob was just madder than hell about it. And that was the way the

place was run—he had a rule and he stuck to it. I must say, at other places I've been I've never witnessed the details of faculty hiring to the extent we do it here. It works, and I think a lot of that was set up by Bacher, who really did a good job.

The mistake they made was they tolerated the Mount Wilson and Palomar structures, which they shouldn't have done. They didn't back Jesse far enough. You see, at that time Bruce Rule ran the astronomy department. Jesse was the chairman, but Bruce Rule controlled Palomar and the money. Jesse had something of a voice, but not a very loud one. I don't know the whole story. Have you talked with Jesse about that history? That would be very interesting to know, because Jesse came here when the Mount Wilson and Palomar [observatories] were established. How he was treated—because he was "an impractical scientist, a Jew (which means he's not one of us)," and how much his was a very strong voice, I don't know. Eventually he got to be very influential, but he took a lot of crap. You really ought to talk to him—I don't know what he'll tell you.

COHEN: I'd hate to do an oral history. Maybe some other person would.

ZIRIN: I've always been curious about that. I know he had some resentment. Let's see. Then Harold Brown—well, he was just taking a job for when the democrats would get back in again. He wasn't that interested in it. I heard the story of his leaving all the books behind that people had given him while he was president. [Laughter] He was kind of a cold fish. I looked at my history, and it was very parallel to Harold Brown's. That is, if I had stayed at RAND, I probably would have gone in the same direction. These were guys who stayed inside the AEC circle and the military—the secret military circle. Brown was a protégé of Edward Teller. He was obviously a bright man, but he didn't understand what universities were about.

I did cross swords once with him. Mary was very angry when he proposed closing down the language departments here and said that languages should be taught at Occidental, because who needed a foreign language? Everybody in the world speaks English. That was a very stupid thing for a man who was president of a university to say. So Mary quit. Later I went to him—I very rarely go to the president, after all—and said, "Look, we really have to teach languages here." And his answer was, "Well, we don't have any nepotism here." And I said, "For Christ's

sake, my wife has quit already. She's not asking for a job. I think part of the education should be to teach about the rest of the world." So he was very narrow that way.

I had known Murph before, because when I was a postdoc I had rented an apartment in Cambridge from Al and Shirley Wattenberg, and they were good friends of his. I had met Murph at their house in Cambridge, so I knew him slightly. Then one year, at a New Year's Eve party, in came the two Wattenbergs, whom I hadn't seen in thirty years, along with Murph. They were visiting the Goldbergers. I didn't have too much interaction with him.

Of course, I was just tremendously impressed by Robbie [Vogt] and the things that he was doing, and particularly by the fact that he got me a professor. It was his idea to hire Ken Libbrecht. Ken came in and ran this wonderful program. But we had the money and we had the people, so Ken could come in. Robbie got us the money. He virtually dictated that appointment to the faculty. Ken was able to hit the ground running. We gave him the support. A lot of guys who get all that easy support will just sit back. Ken instead worked very hard and got wonderful results, which are just now, many years later, being duplicated by other people, via satellite. Nobody else ever did it on the ground like Ken did. So that was a very good thing that Robbie did, and I was mad at Murph for firing Robbie. On the other hand, I could understand that Murph didn't like being kicked around by Robbie all the time. Robbie told me what a good guy Murph was compared to the bum we have now—to Everhart. I don't think he called Everhart a bum, but he said, "At least when Murph said something, he did it." It's funny to hear Robbie talking that way when Murph fired him. [Laughter] But you never know when you're well off, you see. Robbie was better off under Murph than he was under Everhart.

Everhart was a non-entity. He knew he should have a dream. I remember I met him when he first came. I went to the freshman camp—it's the only time I've ever gone there—and he went to it, too. Most new presidents go to freshman camp. I talked to him a bit. He was a pleasant person, actually. He felt that he should have a dream for Caltech. The only trouble was he couldn't dream of anything. [Laughter] So he was kind of stuck. He knew there had to be a new vision, but he couldn't do it. It was beyond him.

COHEN: So now we have a new president.

ZIRIN: Now we have a new president, and I have no idea what he'll be like. I have an up and I have a down. I talked with some guy in Chicago who didn't like him—a biologist; who knows whether it's a professional thing or whatever. He seems perfectly nice. He seems very interested in the mechanisms of things, which is important. And he's a windsurfer, which is probably in his favor.

COHEN: And he's gone off to see the eclipse.

ZIRIN: Right. As a matter of fact, I felt good when he came into my office. He was being shown around the department, and he said, "Oh, this is the place where they gave away the observatory." DuBridge would never have done it. DuBridge was always moving forward. He would only build up, he wouldn't tear down. I think that's the right way to be. I mean, somehow moving forward always does you good. It's easy to hunker down and make believe you are protecting yourself, but it is a terrible mistake. I think also that, if somebody other than Charlie Peck had been division chairman, it wouldn't have happened. I mean, Robbie certainly wouldn't have gone along, or Neugebauer, who really wanted to build things up, too—those guys were builders, not the sort who just kind of go along with what was established. Many of the latter were quite intelligent people. It's just a matter of nature, I guess.

I should say something more about the end of the Big Bear Solar Observatory and solar research at Caltech. The study of the Sun has a long history in Pasadena. Hale was a solar astronomer and discovered the structure of the solar cycle. In the '50s and '60s Bob Leighton and his students made great discoveries. Much of the early neutrino work was done in Kellogg. And I was hired because Greenstein, an astrophysicist, felt he should have a solar guy in his department. Building the Big Bear Observatory opened new possibilities in the field, and, along with the OVRO [Owens Valley Radio Observatory] solar interferometer, we now had two world-class instruments to study the Sun.

Robbie Vogt, the division chairman, was concerned about who would succeed me in exploiting the facility. He got a job inquiry from Ken Libbrecht, who had been an outstanding undergrad here, and completed a thesis on the solar oblateness with Bob Dicke at Princeton. Robbie told Ken he could get an assistant professor's slot if he worked in solar physics. Because he had been such a top student and Robbie dominated the Division, it was approved.

Ken did an outstanding job, and things looked good. However, he lost interest in solar work. At that time, people were getting excited about cosmology. The wonderful new discoveries of gamma ray sources and evidences of the Big Bang, combined with the public interest in the search for God and the origins of the universe, carried everybody away. While we were doing great in solar, Ken felt there wasn't much glory in the field, and decided to do something else. He gave up a really successful career in the Sun for a less successful search for what is called the Bose-Einstein condensate.

When Gary was division chairman, he started a systematic search for a successor to me, and brought three outstanding solar astronomers to Caltech to educate the astronomy faculty to what was going on in solar. These talks were quite successful, and people were satisfied that the solar program should continue. We were, in fact, permitted to search for someone, and "if we found a suitable person, they would consider an appointment." The implication was that no one who studies the Sun could possibly be good enough. The search committee came up with three suitable candidates, and selected one of them. The proposal went to the divisional faculty, which supported it, 12-10. But, the astronomy faculty voted against it, almost unanimously even though they had earlier agreed to the proposal. They stated that we needed additional faculty to operate Keck. Only the two top astronomy theorists, Blandford and Goldreich, voted in favor. Had we had a stronger division chair, it might have been different. Had the school had a stronger administration, or had Baltimore been here, it might have been different. A year or two later, when I proposed to the provost to endow a professorship in solar physics, he told me that that would be like taking over a buggy-whip factory. He knew nothing about the field, as did most of the astronomers, but was ready to judge it. When I explained that the Sun was the most important astronomical body, he waved it aside. When I pointed out that the Institute would lose $300,000 a year in excess overhead, he told me, "Caltech has plenty of money." Thus Caltech gave up the best facility in the world to study the star upon which our lives depend. I had learned years ago that nighttime astronomers are a nasty, selfish bunch.

Without a professor to succeed me, the observatory had to close. It was offered up to the world for lease. Only one proposal came in, from the New Jersey Institute of Technology, which was trying to start a solar program. It was symbol of the bankruptcy of the field that none of the three national observatories, all of which operate in terrible locations, was interested in this installation, which would have reinvigorated their operation. As long as their checks arrive

monthly, they have no desire to improve. NJIT had hired a student of mine as an assistant professor, and, as part of the deal would hire another student as an associate professor, contingent on receiving his grants. I turned over my grants to them, and expected things to work out OK.

As soon as the transfer had taken place, with a fancy ceremony with the presidents of both schools present, things changed. I was forbidden by the new director to come to the site when observing, and forbidden to speak to anyone else on the site. I was accused of harassing and speaking rudely to the observers. When I started bringing my own staff along as witnesses, this stopped abruptly, and the accusations have only resurfaced if I go there without witnesses. I have never understood their motives, since they might benefit from what I know. Anyway, the place is still functioning—albeit lamely—and I should be grateful for that. We are allocated ten percent of the observing time, and have been doing successful work.

I should state that, although the MOU for operation of the observatory foresees a cooperative arrangement, the Caltech administration has refused to support me in participating in the observatory. Frankly, I don't understand what drives NJIT, although I guess the administration is terrified of anyone studying the Sun. Since these events, independent tests have shown that Big Bear is probably the best site in the world, and there is talk of building a new national facility there. Whether that will occur, I do not know.

COHEN: But you are still working hard, which is the tradition here.

ZIRIN: I am working hard and doing research. I have actually applied for the directorship of the National Solar Observatory. I doubt that they will hire me, and if they do, I don't know whether I will take it. [Laughter] I've done it almost to be a hooligan. But if I got it, I would close down the existing observatories and move them all to the shores of Big Bear Lake. I've got a congressman, who is the chairman of the committee in Congress which gives money for such matters, who represents that district and who will pay for it and do it. In order to do that I would, again, have to have Caltech support. What some people would like to do is to have a science center in Pasadena and run the observatory up there. It's a very distant dream. Things like that are not easy to come by, and the community itself is probably not that much in favor, because it's just easier to do what you've been doing.

COHEN: Well, you always have to try new things.

ZIRIN: Yes. Without leadership nothing happens.

COHEN: Well, that's all very interesting. So you've had a good run.

ZIRIN: I've had a wonderful run. Gee, it's been, what now, thirty or thirty-four years.

COHEN: You've been through all the presidents.

ZIRIN: I remember the first time I came here in '52, when I went to RAND. I remember Allan Sandage showed the 200-inch telescope to me. If I had been able to get on the faculty then, I would have done it [laughter], but nobody invited me. It really is a great place. No place can be perfect. You just try. [Tape ends]