Department of Astronomy
Radio Astronomy Laboratory

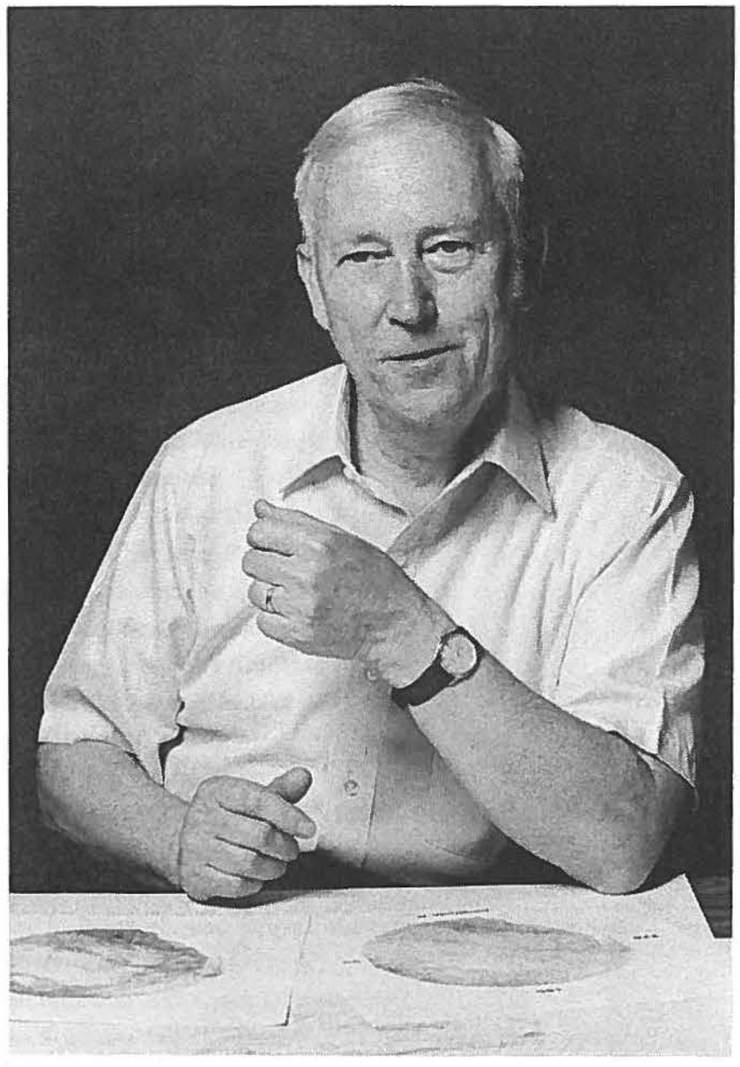
University of California
Berkeley, California

Harold F. Weaver: California Astronomer

An Oral History

With Introductions by
Carl Heiles and Hyron Spinrad

Interviews conducted in 1991 by
Joseph C. Shields



Harold F. Weaver

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Preface

Harold Francis Weaver has had a long and influential involvement in astronomy within California and elsewhere. During his career he has been productively associated with several leading institutions in scientific research, including the Astronomy Department and Radiation Laboratory of the University of California at Berkeley, and the Lick, Mount Wilson, and Yerkes Observatories. He was the founding director of another important astronomical research organization, the Radio Astronomy Laboratory at Berkeley. Prof. Weaver has additionally contributed generously of his time to foster and improve professional organizations that advance scientific interests. Harold F. Weaver has consequently interacted with many of the major figures in twentieth-century astronomy, while producing scientific results that justify his inclusion among this group.

I first became acquainted with Prof. Weaver in 1986 when I took his graduate course at Berkeley on Galactic Structure and Stellar Dynamics. As a participant over many years in this field, Prof. Weaver brought a historical perspective to the topic that I found both unusual and illuminating. In subsequent conversations with Prof. Weaver, I also had the pleasure of learning a variety of revealing information concerning the character of astronomy and astronomers at the University of California in the past, information of which many of the younger faculty and most of my colleagues among the graduate students were unaware. After I expressed concern that much of this institutional history remained ephemeral and undocumented in concrete form, Christine Ann Fidler (my wife) of the Department of History at Berkeley suggested that I explore the possibility of conducting an oral history with Prof. Weaver. After I contacted and received encouragement from the Regional Oral History Office of the Bancroft Library, the current project was undertaken, with full and friendly cooperation from Prof. Weaver.

In preparation for interviewing Prof. Weaver, I conducted background research with the aid of several sources. Prior to the late 1960s, membership in the Astronomical Society of the Pacific was concentrated primarily to California and the San Francisco Bay area in particular. The membership and its regional orientation are reflected in the content of the *Publications of the Astronomical Society of the Pacific* dating from that time. I consequently searched issues of the *PASP* from approximately 1940 – 1970 for information relevant to these interviews, as found in the "General Notes" column, obituaries, observatory reports, and related items. Additional information and suggestions for interview topics were obtained from conversations with Andrew Fraknoi of the ASP; Donald Osterbrock of Lick Observatory; Jack Welch, Carl Heiles, Ivan King, and other members of the Berkeley Astronomy Department; and Tap Lum of the Radio Astronomy Laboratory. Prof. Weaver also provided me directly with reprints of many of his scientific articles.

The interviews were conducted in nine one-hour sessions on nine days between September 3 and 13, 1991, in Prof. Weaver's office on the sixth floor of Campbell Hall at the Berkeley campus. The interviews were structured in such a way that the information, while topically grouped, has a generally chronological organization of content. The interviews were taped with

equipment generously provided by Dan Plonsey of the Department of Astronomy. Copies of the tapes were subsequently deposited for public use with the Regional Oral History Office of the Bancroft Library in Berkeley. I completed transcription of the interviews and initial editing of the result within the ensuing year, following guidelines for this process as outlined in Willa Baum's *Transcribing and Editing Oral History*[1]. Prof. Weaver is an articulate speaker, and only very light editing of the transcript was required. Prof. Weaver himself subsequently completed a careful review of the transcript and noted a modest number of changes to be incorporated. Most of these changes amount to minor wording alterations that improve the clarity of presentation while having little effect on content. In a few instances, additional sentences were added for the sake of clarity or completeness. Prof. Weaver also provided most of the photographs that have been incorporated in this volume.

Prof. Weaver deserves great credit for generously giving of his time and efforts, thus ensuring the successful completion of this oral history. Several other individuals deserve thanks for contributing in various ways to this endeavor, most of whom have been acknowledged by name in the preceding paragraphs. I would also like to thank Malca Chall and Willa Baum of the Regional Oral History Office and Christine Ann Fidler for technical advice. G. Paul Bishop kindly donated a copy of the photograph of Prof. Weaver taken at the time of the latter's formal retirement in 1987. Partial financial support for the material costs of this undertaking was provided by the Department of Astronomy and the Radio Astronomy Laboratory of the University of California at Berkeley, and by the Department of Astronomy at Ohio State University.

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Columbus, Ohio
May 1993

[1] Second Edition, American Association for State and Local History (1981).

Introduction

My first recollections of Harold Weaver are set in the mold of an undergraduate student-teacher relationship. In 1953 Harold was a Professor of Astronomy at the University of California, Berkeley, and I a rather uninspired Junior. My main recollections then were of being challenged by the depth, detail, and practicality of Professor Weaver's approach to scholarship, and the pleasure of extracting an answer from astronomical data. Harold insisted on what we would now term a professional response to problems. It was certainly a good lesson learned (but not always sweet medicine).

Harold Weaver again played a friendly and positive role during my graduate career in the late 1950s and early 1960s, as he did for the increasing number of Ph.D. students entering astrophysics in the "post-Sputnik" years. I remember Harold again from the systematics of course work, but more vividly from his emphasis on new research ideas (radio astronomy in California was just beginning then) and his respect for the literature, the scientific documentation of the immediate past. Harold was readily available for the guidance of uncertain students on projects large and small. His collegial attitude toward graduate students (for example, he organized a collection of graduate student vignettes and some photos) as well as his higher-level academic peers was much appreciated by the students, even if they did not always articulate it well.

In the 1960s Professor Weaver's role in "new-Astronomy" on the Berkeley campus became even more significant, as the Radio Astronomy Laboratory which he directed zoomed to the forefront in galactic structure research and in the discovery of new interstellar molecules such as NH_3 and H_2O.

But Harold's impact on his colleagues in the department went much deeper than just the professional excitement of our science. He and Cecile generated a stimulating social presence to the growing group, with holiday parties and a homey, warm family atmosphere. This was an environment visitors noted, in the department itself, or the Faculty Club, but even more so in their Kensington home.

Over the years Harold has demonstrated a strong desire to be a "good-citizen" in science and an advocate of novel educational experiences in the broader San Francisco Bay area community. He was for years the treasurer of both the American Astronomical Society and the Astronomical Society of the Pacific. The strong and improved fiscal situation of these two professional societies is a testament to Harold's interest and dedication to business matters where others might not have taken the time to succeed to the degree he did. In public education, Harold Weaver has tried to help local school districts with innovation. Recently he has played an important role in monitoring and the advocation of the "rebirth" of the Chabot Observatory in Oakland. It will become a modern and novel educational tool (for young students and adults) in a city not reputed to illustrate educational success. This public service Harold characteristically undertook with no personal gain in sight.

In his post-retirement activities on the Berkeley campus Harold Weaver is still a source of encouragement to young astronomers. He remains a model scientist far different from the common media image.

Hyron Spinrad
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Berkeley, California
April 1993

Introduction

It was hot, that day in 1966 we drove from Berkeley to Hat Creek – my introductory trip as a new assistant professor. Harold talked of how they used to travel by train, but as train service declined the driving became almost mandatory. And in those days there was no 75-mph Route 5. No; along with a long line of heavy trucks, cars, and the occasional farm vehicle we rode north at a snail's pace through the central valley's heat, slowing even more at every little town. No sense in passing just to trade the long line behind one slow truck for another.

Harold had made this trip many times. Not only this trip to Hat Creek, but many others like it to search for the a good site for his radio observatory. Not only on good blacktop roads, but also on back country roads with two ruts in the hard and sometimes not-so-hard dirt. The criteria for a site included not just the standard radio silence, but also space – space for interferometry. This took foresight, because interferometry in those days was not the popular industry it is today. He settled on Hat Creek: plenty of space, accessibility to bedrock for good, solid telescope foundations, deathly quiet in the radio, and low enough in elevation to have reasonably good weather and not too much wind (!).

We know the rest. Hat Creek, the Radio Lab at Berkeley, has been a major force – and not *only* in world-class research. Its research preeminence made it a major force on the Berkeley campus as well. During the early 1960s, when the new 85-foot telescope was discovering OH maser emission – mysterium – and beginning the 21-cm line surveys, the astronomy department was undergoing tumultuous change. During the fifties, the department had been slowly built up from one preoccupied almost exclusively with orbit calculations to one with a solid basis in stellar astrophysics. During the sixties, many of these stellar people left for what seemed to be greener pastures. They were replaced, in part, with people having broader interests in areas that were, at that time, somewhat offbeat: stellar dynamics, radio astronomy, interstellar astrophysics, x-ray astronomy. I suspect that this emphasis was sparked in large part by Harold's new laboratory: at that time, radio astronomy was, distinctly, new territory and uncharted waters. Only the adventurous – or the deeply, insatiably curious – would go there, and certainly it took a special person to invest the time and effort in the combination of political, administrative, financial, and scientific worries that were required to build up a radio observatory from scratch.

But the Radio Lab did even more in Berkeley, for it defined the campus as a place where new wavelengths were probed. It established this tradition and generated a spirit which said: "let's do new stuff". This spirit focussed interest within the campus and brought new people such as Charles Townes to the campus. So the spirit spread to the physics department, where people developed active, forefront programs that pursued the far infrared and submillimeter wavelengths. And the spirit brought x-ray astronomy to Berkeley in the person of Stu Bowyer, and continues to this day with the explosive interest and investment in EUV astronomy. It even affected the chemistry department, which continues theoretical and laboratory research in interstellar astrochemistry.

Within the Lab the spirit became embodied, first, as the extension to millimeter wavelengths, and then to millimeter-wavelength interferometry; this has kept Hat Creek and the Radio Lab in the real forefront of radioastronomical technology. All this bursting forth of productive activity is the best kind of positive feedback at work.

Harold's interests have always transcended the narrow realm of his own scientific research. Rather, he has spent enormous time and energy behind the scenes to build real, lasting organizations and to deal with real, knotty difficulties in existing ones. These efforts multiply themselves by enormous factors because the organizations provide resources that would be otherwise unavailable and allow many people to perform at their limits of ability – in fact, better, they stimulate people to define for themselves new, far greater limits. This quiet man, with his soft smile and subdued, wry humor, has influenced many of us in this not-so-indirect way and is responsible for many of us being at Berkeley. I, for one, am forever grateful.

Carl Heiles
Professor of Astronomy
University of California at Berkeley

Berkeley, California
March 1993

I. Youth and Education

[Interview 1: September 3, 1991]##[1]

Early Years

JS: You're a native Californian, is that right?

HW: Yes.

JS: Do you recall particular instances from your upbringing that got you interested in science or astronomy in particular?

HW: I think that is a multiple question. First, interested in science; second, interested in astronomy. Yes, I think that I have been interested in mechanical things from earliest childhood. Reading this document that was produced by DeVorkin [2], this interview of some time ago, I was impressed how many times I touched upon having done things with my hands, that is, built things. As a small child I remember a period of playing with steam engines and then later building model airplanes. So I think that interest in mechanical things, machinery of all kinds, was something that was with me from early childhood. And that led to a lot of other scientific things related one way or another to mechanical models and machinery. So that was an interest from very early on.

The interest in astronomy came much later, and it had to share my attention with a variety of other interests. And finally it became stronger and stronger and took over everything. But it was not something that was all by itself from the beginning. The earliest astronomical recollection that I have, as astronomy as such, that is, looking at something, was when I was probably – I was trying to think of that a little earlier today with DeVorkin's interview – I must have been maybe seven or eight, and some of the other fellows around there had a telescope. It must have been about a two-inch refractor, I'll guess; I remember it as a telescope so long, you know [gesturing to indicate length]. And it was mounted quite nicely on a wooden platform, and we all got turns looking at the moon and planets and other things through that telescope. And though that was a long time ago, it still remains in my mind very clearly.

Another thing that I recall from astronomy, in very early times, was my step-grandfather, who had settled in San Jose during the time when all of the strange stories

[1] This symbol (##) indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 129.

[2] Transcript of an oral history interview with Harold Weaver by David DeVorkin, National Air and Space Museum, on the subject of the development of radio astronomy in the U.S.

of James Lick were around; and so I heard as a very small child, stories about James Lick, a great variety of them. But that was incidental to astronomy, not as astronomy itself. So the Lick Observatory was something that had been impressed upon me very early, and there was this very early observing experience, but the real interest in astronomy didn't develop until much later, I think.

JS: You were born in San Jose, is that right?

HW: Yes, I was.

JS: And where did you spend most of your growing-up years?

HW: In San Jose. I never got very far from home, you see, only to Berkeley.

JS: What line of work or matters did your family pursue, your parents?

HW: My father had a store. He was in a food store, in fact he always had the meat market part of the food store. That is, until the Depression. It was lost at that time, and regained later, but was lost for a while; but he had been in the food business. So I did not grow up with a family that was directly involved with science, though my father was interested in science, and in fact I think that he was a disappointed electrical engineer.

JS: You said a little while ago that you had many other interests while you were younger that competed with any interest that you had in astronomy. What were some of those interests?

HW: Early on there were these various interests that involved building things, and I built all kinds of airplanes, model airplanes. I was involved with those for several years, and involved with – in contests, in fact, even as a very young person I taught model airplane building. That was how I made some spare change. In fact, the only change at that time. It seemed like quite a bit of money in those early days. So there were all of these mechanical and building things. Then I had a lot of other interests that developed in a variety of ways. I was very much interested in artistic things, and mechanical drawing, and drawing; and I suppose mechanical drawing was related to the interest in machinery. And out of those interests came some others. Some that were directly related, others that I think were related only through the fact that all of these, it seems to me now, thinking about it, were somehow related to an academic interest; that is, I was always interested in how did it work or what did it mean, or whatever.

And so other interests that developed were geology and chemistry, you know, physics and all the other things. But also another one that I guess came from the – maybe the art, in the mechanical drawing, art part of it, was painting, and I have quite a few paintings that I made as a high school and junior high school student. And that has remained with me. I haven't done anything in recent years, but certainly it remained with me, I recall,

when we lived at Mount Hamilton, at Lick; I still did a few watercolors to try to capture impressions. And I was very much interested in literature. In fact, there were two things that really competed for my interest in a career, and one was classical literature – Greek and Latin – more Latin. I had both of them but I never had much Greek; it was mostly Latin, I had lots of that. So classical literature, and then finally a scientific field, and that was clearly astronomy. After I graduated from high school I stayed out of school a year to decide which I wanted to do. That was a wonderful year.

JS: What did you do during that year?

HW: Well, at that time my father had a store in Carmel, and so all during the time I was in college my home address was Carmel. So between high school and college I spent a year in Carmel, and walked on the beach, and read lots of books, and met some of the interesting people around Carmel. I remember I went to the high school in Monterey, and there I went and visited the mathematics classes. I didn't do any physics there, but I visited the mathematics classes and participated, and sort of worked on all kinds of unusual problems that the other students didn't have time to – and maybe they weren't interested anyway. I was trying to find new proofs of various theorems.

In that time I did a lot of reading and finally decided that as interesting as classical literature was, I would do astronomy. I had already read a good many books in astronomy at that time. I had that whole year free, so I read everything they had in the Carmel library, and got a good many other books on my own, and read them; tried to make sense out of all the mathematics in them. But the math was much simpler then than it is now. And decided that I would become an astronomer.

Study at Berkeley

JS: So it was after that year that you came to Berkeley?

HW: Yes. I came to Berkeley in 1936, so that must have been 1935; that was certainly the year I graduated from high school, so that year. And then I went right into astronomy.

JS: Did you take a fairly varied course load as an undergraduate, or did you focus from the beginning?

HW: Oh, pretty much I focused from the beginning. Though there were a few other things one had to take – a wide variety of courses to satisfy the breadth requirements, as they were called – so I got into a variety of things. Anthropology, psychology; I never did do anything in economics, I guess. I didn't even do any history; I'd had all of that, all the standard history, before. So it was pretty much a concentrated specialization, with a few additions.

JS: As an undergraduate were you counted as part of the astronomical community on campus, or was that reserved for graduate students?

HW: I was very fortunate, I was a part of the astronomical community early on. It was a small group of people over on the hill. Oh, I should bring you a picture I found at home of the early observatory in 1888, or something like that. Before even my time! But over on the hill the graduate students had office space. There were a few offices that they shared, but mostly they were in the library at that time, in desks there. And, from the very beginning I had quite a few good friends there. One, a person I knew, I think from the time I was a freshman, was Lawrence Aller. And Lawrence is still a very close friend. I haven't seen him in the last few years because he hasn't been very well, but I always consider Lawrence Aller as one of my special friends.

JS: And was he a graduate student at that time?

HW: He was then a graduate student. He left not long after that to go to Harvard, where he finally took his Ph.D., and then remained there for quite a while, of course.

JS: What led him to transfer from Berkeley to Harvard?

HW: Oh, his interest was in astrophysics, and Berkeley at that time just wasn't an astrophysically oriented department at all. It was a department that specialized in orbit calculation. So this place didn't fit in with Lawrence's interests at all. Lawrence had really gotten into astronomy through Donald Menzel, and Donald Menzel was then at Harvard. And Donald Menzel was always Lawrence's mentor, and it was quite natural that Lawrence should go there.

JS: Who were some of the faculty in the Berkeley Astronomy Department at that time?

HW: Oh, the faculty then. There was the Chief – that's Armin Otto Leuschner. He was the head of the department. The next person was Russell Tracy Crawford, who had been one of Leuschner's students, and remained then afterwards as a faculty member. There was Sturla Einarsson, who also had been one of Leuschner's students. Einarsson taught navigation, least-squares, things of interest for the engineering group. Taught navigation for the navy. Then there was William F. Meyer, who also had been a student here. It was a very in-grown department. He was not really a specialist in any field; he was a teacher primarily. He did some radial velocity work, and he had done a few other things, but he was primarily a teacher. Then there was C. D. Shane, who was the person most involved with the physics of the situation, astrophysics. He taught the courses in astrophysics, the one graduate course in astrophysics when I was there. And occasionally the undergraduate course, but they traded that one around. That was then called 117 and it's now the 127 series. And that was the sum total of the faculty members. Robert Trumpler was there as a visitor, but he was not

a member. He was at that time still on the Lick Observatory staff. But he was there. And soon thereafter – I don't really remember the year now, come to think about it exactly, when he transferred to Berkeley – but it must have been some time just about then, because I guess I had him in a course before I graduated. So he must have transferred just about that time.

It was a very different department than any of you here now can imagine. As I just mentioned, it was primarily an orbital mill. And astrophysics was just not very powerful. Shane had taught the course, and it was a good course in that it covered the material, but it wasn't taught by someone who was involved with it himself. Shane was a good teacher, but in this case he simply was not discussing a topic that was his own. So it was just taught out of books; at that time it was the Unsöld *Physik der Sternatmosphären*, so you had to read it in German.

JS: Since your career has really been more in astrophysics, did you pick up most of your physics by taking physics classes at that time?

HW: Yes, that's right. And the physics courses were, I think, quite good. They were certainly primitive compared to anything that you have now, but the physics came out of the courses in physics.

JS: There were some now rather famous names among the physicists in that era, such as Raymond Birge.

HW: Yes.

JS: Or Robert Oppenheimer and Ernest Lawrence. Were you acquainted with any of those people?

HW: Yes, yes. I knew all of them. I had classes from Birge, two graduate courses, one in the reduction of observations. That was really his greatest interest, I think. Very precise methods of discussing data and coming up with the best answers. And his course in optics, physical optics, that was always very good. I had quantum mechanics from Oppenheimer, though it was somewhat on an informal basis, in the following sense. I was never a registered student in his class. I simply went and went to the lectures, and took all the notes and did all the work, but I never was a registered student there. It was a graduate course and I did it in my senior year. Lawrence, I knew, not through any classes, but through war work. That was after I had my Ph.D., and I was on the staff of the Radiation Laboratory during the war. And there I did come to know Lawrence because of that work. Yes, it was quite a remarkable group of people.

JS: How did you like Oppenheimer as a teacher?

HW: He was fantastic as a teacher. If you've ever heard him speak, perhaps on some, oh, television programs or something, that's just the way he taught. His vocabulary was a very special one. His sentences were very special ones, the way in which he said things. He would offer very, I felt, very clear insights into the meanings of things, because I think he understood things so well himself. His classes were quite remarkable. I knew a few of his graduate students, and they were certainly devoted to him. Oppenheimer held positions both at Berkeley and Caltech. He would be in Berkeley, as I recall, all during the normal school year, and then go to Caltech for the summer. And his students would always follow him to Caltech. So, later on, when I also went to Pasadena to go, not to Caltech, but to Mount Wilson, I would see some of the students that I had seen here.

JS: Are there other faculty members of that time that you recall as being particularly influential on your college experience?

HW: Well, I'm trying to think of which ones would I think about. I think one who influenced me in a variety of ways was Griffith Evans, for whom the math building over there is named. I had him for a number of courses in mathematics, and I was always impressed by his quiet and gentle and thorough way of doing theorems. He certainly had a very wonderful way of presenting material in classes. Another one that I came to know, and that certainly did influence me also, was Jerzy Neyman, the statistician, from whom I had classes, I guess both graduate and undergraduate classes. I had his undergraduate class in probability and statistics the first year that he was in Berkeley. He had come, I learned later, to establish statistics on the Berkeley campus, and so he lost no time in doing exactly that. When I was a graduate student I was a reader for him, that is, I corrected the papers from other ones of his classes. He was very anxious to have me go on in statistics, and it was a temptation in some ways. I still retain an interest in statistics, but I'm glad I did not succomb to his wiles and become a statistician.

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HW: Among some of the other physicists, ones that I remember very well are both Harvey White and Pan Jenkins, F. A. Jenkins. I remember particularly Harvey White through his work in optics. I had his course in optics, physical optics. In fact, I did it in a somewhat unusual way again. I did it credit-by-examination. I went to the lectures and so on. I hadn't signed up because I had already enough units, and I simply took the exams, and took it that way. I got to know him quite well after I returned to Berkeley on the faculty. And Pan Jenkins I knew because we worked together on a number of problems at the Radiation Lab during the war. I think really so many of them influenced me in a variety of ways. And I haven't even mentioned Trumpler, of course, from whom I did have courses, and who certainly influenced me very much, and I worked with him.

JS: Did you become involved in research at all as an undergraduate?

HW: No, I did not, and one was not encouraged to become involved in research. In fact, one was not encouraged really to become involved as a graduate student until a thesis was started. That is an aspect of the department then and now that is just tremendously different. Now the students are introduced as the earliest possible moment into research. And then it was not at all considered to be that important. The thing was to get all these classes out of the way, and so on. There were a few things one could do, but they tended to be rather uninteresting. There was a physics course you could take in research, but you might be given the job of developing the photographic plates for the professor, or something like that; scarcely real research. Research was looked upon in a very different way then, I think both for the students and for the faculty.

JS: In what sense for the faculty?

HW: Well, they didn't do much research. Leuschner, who retired a few, I guess maybe two years after I came – I had one course from him. I took an upper division course in my sophomore year so that I could have a course from him; I took his course in practical astronomy. I learned to do time sets, and determine the latitude by Talcott's method with the transit instrument, and proved every obscure trigonometric formula that you can find in the books, and – you know, that was the sort of thing one did then – and took a few astronomical photographs. But I think that his research years were over by that time. Einarsson did not do any research. Meyer had one problem that he worked on, and that was a variable star, β Canis Majoris, but it was primarily measuring plates for radial velocity. It was a very unimportant thing, I think, in his life. Crawford didn't do any research. They might determine an orbit now and then, but mostly it would be the graduate students who did that.

Shane was involved in research, and he was, in that sense, the unusual one in the department. He had a high dispersion spectrograph, and he worked on the contours of the sodium D lines in the sun. That spectrograph was a very good one, and Shane used interferometric methods of determining the contour in really very interesting ways. But it was a very specialized kind of work, and once he determined the contour, which took quite a long time, not much came of it after that. There's a publication on the profile of the sodium D lines in the *Lick Observatory Bulletins*. But you see, the faculty didn't do much research themselves. And the students therefore never saw research being done. Trumpler was on the campus at that time, but not directly on the teaching faculty. He did a little teaching. I think he gave, maybe every other year or so, a course for students even though he wasn't on the faculty. He gave a lot more classes when he came here as a faculty member, but he always did research. He was still doing his work on clusters. He was looked upon as the unusual person who did research.

Undergraduate Summers

JS: How did you spend your summers as an undergraduate? Was it doing academic work, or not?

HW: Yes, the first summers, I remember – I guess I still made some telescopes after that, but I do recall trying to to reproduce some of the experiments at home, trying to reproduce some of the experiments that I had seen in physics carried out, and perhaps extend them a little. I remember some things in sensitive measurement of radiation, and aside from that it was just a period of reading. And I recall working out a new method, a different method of doing binary star orbits, for example; that sort of thing. Then, let's see, I guess it was maybe between my sophomore and junior year, I spent a few weeks at Lick Observatory in the summer. But that was as a guest of one of the astronomers there, not as a student. I participated in the observing and so on, but not in a regular way. The graduate students did go to Lick during the summer, and of course, it was renewing acquaintances because I did know them here on the campus, and so I saw them there. But that was as a guest of F. J. Neubauer, who was an astronomer at the Lick Observatory. Between my junior and senior years I spent the summer at Mount Hamilton. I lived with the Trumplers and worked as Trumpler's assistant. I measured radial velocity plates and observed with him at the thirty-six inch refractor. It was a summer of real astronomical work.

Then, let's see, what was the next year? Yes, when I graduated, I went to Mount Wilson for the summer. That was as an experiment in cooperation between the Berkeley campus and Mount Wilson. That had been organized by Trumpler. Trumpler and Walter Baade were students together at Göttingen. And Trumpler made an arrangement with Baade for students from Berkeley to go to Mount Wilson for the summer, and work there with the astronomers as assistants, as the students did at Lick; that was the whole thing, you were apprenticed to an astronomer, actually. Well, I was the guinea pig to see whether or not the system would work, and so I spent that summer at Mount Wilson. I had graduated then, and so it was between my senior and first graduate years. I spent the summer, and came to know the astronomers at Mount Wilson. I worked part-time for Edwin Hubble; not terribly much, but some. But mostly for Baade, and worked, and went observing with Baade, and was like a real apprentice to a master. And measured plates for him, and reduced data for him, and did all kinds of things. Worked in photometry. That's how I really got started in photometry; that is a legacy of Walter Baade. That was his field, and I had been apprenticed to him really, and so I did photometry. I came to know the other astronomers there. Adriaan van Maanen, whom I had met actually before I came to college, I had met very briefly. And, oh, all the others, you know, all the ones who were there at that time. It was like a list from *Who's Who.*

JS: How was it that you got acquainted with van Maanen before then?

HW: Quite by accident. A good friend, who is still here in Berkeley – he's not on the faculty, he lives in Berkeley – Howard Stackpole is his name. We had become quite close friends in high school in San Jose. He and a third member of the triumvirate – the three of us – were interested in astronomy. It was largely because of Howard Stackpole, whom I just mentioned, and another friend, George Swain, that I really got interested in astronomy again. They were interested in astronomy, and knowing them, I became more and more deeply involved. And that resulted really in my going into astronomy. Well, Howard and I had – I think it was before I went to college; I'd have to reconstruct that a little bit. It may have been my first year in college. We had one of his family cars, and we went on a grand tour, just the two of us. Really, it was fantastic in a sense. In a sense it was nutty, in a sense it was wonderful, and it certainly was an enjoyable and memorable occasion. We spent several weeks, I think it must have been two to three, and we drove to southern California, and we simply went up Mount Wilson and knocked on the door, and there was van Maanen! And he was observing with the sixty-inch, and these two brash kids – he let us observe with him, it was very nice, and he did actually go and show us the hundred-inch telescope, and things like that. It was really quite remarkable, and we stayed at the old Mount Wilson Hotel at that time. It would be worth talking about van Maanen at greater length sometime; he's a very remarkable person.

Well, we went on, we went to Lowell Observatory, knocked on the door, were treated very nicely, very royally, and I met all of the astronomers there at the time. The ones that Howard and I spent most of the time with – was E. C. Slipher, though we met V. M. as well. And C. O. Lampland, and Henry Giclas, who was there as an assistant at the time. Looked at all the telescopes, and observed with E. C. Slipher, with the twenty-four-inch refractor, looking at planets. It was really quite remarkable, you know, that these two brash kids, just kids, were treated so well by the astronomical community. Howard and I also visited all the cliff dwellings and went around in that area. Meteor Crater – that's why I still have a meteor [indicating a fragment in his office], and still an interest in Indian things [indicating a petroglyph on his desk], etcetera. We did digging. Things were not forbidden then; my Lord, you can't do anything now when you go, but, you know, there wasn't anything there. There were no rules, anything. So we found lots of old things in the cliff dwellings, and so on. Well, it was quite a remarkable trip.

JS: How was E. C. Slipher related to V. M.?

HW: They were brothers. A brother act.

Work at Mount Wilson Observatory

JS: So, going back to when you actually spent the summer working at Mount Wilson, you said you worked a part of the time with Hubble?

HW: Yes.

JS: And what sort of work was that?

HW: Well, it was kind of make-work. He wanted me to reblink some of his plates, some of his nebular plates. And I did that. It was really just checking some things that he had done. It was nothing original. He was an interesting fellow. I think he didn't relate to, especially, young students quite as easily as Baade did. Though that's odd too, because I remember since reading about Hubble, and the fact that he was a teacher before he had done his work in astronomy. He'd been a teacher, he taught Spanish, of all things, as I remember – in a high school, I believe it was. And there was something else; maybe he taught physics too. I'm not sure of those things. But I recall reading that the students had given him the award of the year, or something like that, so he must have related well. But somehow it just didn't seem like quite the same way. With Baade it was a most remarkable relationship, and we just worked together, and he would give me a job to do and I'd do it, and that'd be it.

JS: Was Rudolph Minkowski there at that time?

HW: Yes, yes, Minkowski was there, just across the hall, a little bit down from Baade. Yes, coming in through the side door, was a big office here [gesturing], then van Maanen was here, and Minkowski was here, and Baade was here, the library was here. Then you go down farther and at the end was – good heavens. In photometry. I think that he had just about retired at that point. I'm trying to remember his name, isn't that disgusting. Ah, it will suddenly come. I can tell you a lot about him, but I can't tell you his name right now. He had been a student here. In fact, do you know the picture that frequently appears of fellows with top hats, and bowler hats, and derby hats, and all that? And they're taking sightings on the –

JS: Yes.

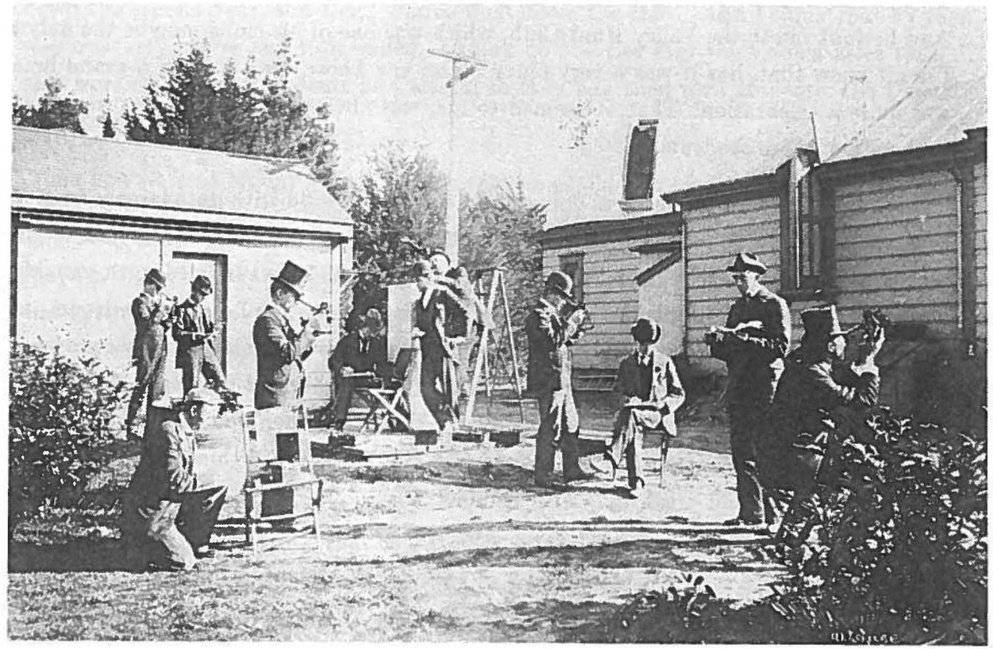
HW: That picture?

JS: The picture of the students at Berkeley?

HW: Students at Berkeley. And he's sitting down making notes. [Frederick Seares.]

JS: Earlier you said that van Maanen was an interesting person?

HW: Yes, he's an interesting person, because he was so interested in students. You might gather that from the fine way in which he took in these two brash students. I think all, maybe all, or a large part of his money, went for fellowships at Caltech. And there are some van Maanen fellowships or scholarships or both. He certainly seems to have been very interested in helping young people. And in fact, he did help me in several ways. I have a lot of very fond rememberances of van Maanen. One is, I guess it was either the first or second summer that I spent – I spent three summers finally at Mount Wilson; I guess it was the first summer



Berkeley Student's Observatory, circa 1900. Tradition has it that the person seated taking notes, located third from the right, is F. H. Seares. The dome opening for the six-inch refractor is visible in the background directly above Seares.

Photo courtesy of the Bancroft Library; reproduced by permission.

– that, living alone, you know, like away from home and everything, and actually, I was married at that time, Cecile and I were married. And I guess I looked rather down in the dumps at one time, and van Maanen asked me what was the matter, and I said, "Well, it's my birthday..." Well, the Dutch are quite sold on birthdays; they make a great deal of them. And van Maanen said, "Your birthday! Then we must celebrate!" And so he took me out to a very fancy lunch. He hailed a taxicab, got a taxicab, phoned for it. He never drove a car. And he took me to the Valley Hunt Club, which was one of his clubs, maybe the only one – I don't know that, but it was a very fancy place, you know. And we had a grand lunch. It was quite a celebration! That, it seemed to me, was always a very nice gesture. Again, he took care of these students.

And then, later when I was doing my thesis, which I did with data largely from Mount Wilson, in fact I guess almost entirely Mount Wilson and Palomar, that I had to go back and measure plates on several times and do things, and I always lived with van Maanen when I did. He was a bachelor, with a very nice little house, and a spare bedroom, and he would invite me to stay with him. And we shared breakfast and dinner; we were each on his own for lunch. And the cleaning lady would always come in, and take care of all the things during the day. But I remember him as a very kind, and when you got to know him, very interesting person, who had a very hard time at Mount Wilson, I think, because of his early experiences there, and great fight with Hubble.

JS: Over the proper motions ...

HW: Over the proper motions.

JS: ...of galaxies.

HW: And that caused other ones of his measurements and so on to be called into question too. And even when he was working on his parallaxes, he was sometimes criticized and damned, that his methods were not entirely the best. One of the stories was that you keep observing a star until you get a positive parallax. But you see, there were nasty remarks. Hubble was such a gentleman, I never heard – I didn't know Hubble that well. I knew him, and you know, we had some talks together, but I never knew him that well. But Baade and Minkowski I came to know quite well, and they were always critical of van Maanen. And he didn't get along with them, because that, of course, was during the war, and they would speak German, and he would slam the door closed. Well, anyway, there are all kinds of stories about those, that's a separate book.

JS: You said that under the arrangement that you were able to spend time down there, you were sort of the first person to go through this.

HW: The guinea pig? Yes.

JS: Did this program continue, with other students?

HW: Yes, it did. Betty Scott went down as the second student. I was invited back as Baade's assistant, years two and three. That worked out quite well. I was actually paid! It seemed a princely sum of money at the time. It was five hundred dollars for the summer, and that was a lot of money in those days, so it helped a good deal in school here. Betty Scott went down the second year, and Keith Pierce went down the third. And I think that by then the war was very active and so it ended. I think no one else ever went down after that. They also were starting up students at Caltech, so they had their own students; the Department of Astronomy at Caltech.

JS: When you were at Mount Wilson, which telescopes did you work with?

HW: The sixty-inch and hundred-inch. And I also used the little photographic thing for some spectra. And, I used the eighteen-inch Schmidt at Palomar. Again, with an objective prism, and getting spectra. And there, again, it was Baade who took me there, and really for my own work, rather than his, and spent the nights with me. And then Fritz Zwicky, whom I came to know quite well, because of that telescope, and I spent quite a bit of time with him on it. Again, lots of interesting stories about that pair.

JS: I guess Zwicky has a reputation that persists for being a rather strong personality.

HW: He was a strong personality, that he was. Yes, he had very positive opinions and he expressed them. He had remarkable insight into many, many problems. You know, I think he has not received perhaps as much credit as he should. He had a lot of firsts to his credit. Including, of all strange things, probably being the first man who ever launched something into space.

JS: And what was that?

HW: Well, he was doing some rocket experiments. So, they launched a rocket, and then as a second stage he used a very high powerful rifle, and shot a bullet out. Launched it. A kind of sideshow.

[Interview 2: September 4, 1991]##

Graduate Study at Berkeley

JS: Yesterday we were talking some about your experiences as you started graduate school and you worked some at Mount Wilson. During that time, during the school year, did you also take graduate classes at Berkeley?

HW: Oh, during the regular term? Oh yes, yes. Those were in the years 1941 and 1942, and I had graduated in 1940. I was, that summer, in Mount Wilson, and then in '41 and '42 I took graduate classes here in Berkeley, and got my Doctors degree in '42.

JS: Compared to the typical time that graduate students take now, that's a very rapid timescale. Were there special circumstances that enabled you to finish so rapidly?

HW: Yes, I guess there were. I knew exactly what I wanted to do; that was a great help. And I had, in fact, taken graduate courses in my senior year. During the summers at Mount Wilson I had been permitted to do some work for myself, and so I had early on decided what I wanted to do for a thesis, and I obtained observations during those summers, actually. So it was a somewhat different situation than the graduate students now have. I think also things were also a great deal simpler in that time. For example, we spoke together yesterday about the fact that graduate students now do a great deal of research. It's expected that you will do research, and that that will be an important part of your career. That lengthens the time it takes you to do everything. That was not the case when I was a graduate student. Research was not emphasized, and in a sense that made it very much easier; to do research at the end and just produce a thesis. Otherwise, it was fairly much the same. There were fewer graduate courses, I guess. Well, no, I guess during the years that I was taking courses, it was about the same as it is now – same number of units and so on. There was a far smaller choice of courses, but there were about as many units that one took per year. And the exams were about the same. There was a prelim, and then something you don't have now, that is, there was actually a defense of the thesis. Now you have only to get your three signatures and that's the end of it, but in those days you had to take an oral examination before the thesis committee of five members.

JS: Do you know when that requirement was dispensed with, and why?

HW: Yes, I'd have to reconstruct the date. I can't tell you the year in which we did that. But it was under the reign of George Field. A number of things were changed at that time. There was a reconsideration of classes. You remember that was in the sixties in the time of troubles here on the campus. There were many things that were changed. There had been student strikes, and classes weren't held. George Field was a rather strong supporter of the students. He didn't hold his classes, for example. Or I guess he held them off campus, or something like that. And there was a kind of ferment that led to a number of changes. One was that the students were no longer required to take language examinations. Previously, a student had to take an exam in two languages, of which he could choose from French, German, Russian, and another language if it were necessary for the thesis work. Well, the language examinations were abandoned. And in their place, two more physics courses were substituted. So there are now more graduate physics course requirements and no language requirements. My recollection is that at that same time the thesis examination was abandoned also. I think that the examination never led to anything. If the student didn't have it by that time, it was really pretty late. So I think that was no loss, but I do think that in a sense, though the students might consider it just one more hurdle to jump over, I'm somewhat saddened by the fact that language requirements have been abandoned. I think they did add something to a

student's career, though they certainly are far less important now than they would have been earlier. Now you have almost everything you need in translation. There's almost nothing written in these other languages now. It's all in English, so you certainly can obtain a view of what the rest of the world is doing by reading English alone. Did you have languages too?

JS: I studied German as an undergraduate, but haven't studied language here. When you were a graduate student, I guess your principal advisor was Trumpler?

HW: Yes, he was. Though I must say I considered Baade about equivalent. I learned a great deal from Baade; his sort of an approach to life, among other things. So I really felt that the two of them had been my thesis advisors.

JS: Your relationship with Trumpler is a bit unusual compared with most people dealing with their advisor, in that he was also your father-in-law. Were you married by the time you were working with him?

HW: Oh yes, oh yes indeed, yes.

JS: In what year did you marry?

HW: I married in 1939, so I was still an undergraduate when we married. And so, Trumpler was my father-in-law all through my relations with him as my professor. It was a very friendly and amicable relationship, and one that I certainly appreciated and treasured very much, for all that he was and represented as a scientist. He was a very thorough and careful person, who worked always down to the most meticulous detail. And it was a privilege to work with him and to know him very well as a person as well as someone that I would know normally as a faculty member.

JS: Was it through your astronomical associations that you met your wife?

HW: Oh yes. We met as students in Astronomy, I think it was called, 104, or something like that at that time. It was in Leuschner's practical astronomy course. So that was how we met. We always say that really we had never been introduced, so it was just an accident.

JS: Did your wife study astronomy in an advanced way?

HW: Yes, oh yes. In fact, she has a somewhat unique honor. I think she may be the only graduate, at least in that era, the only graduate from the Department of Astronomy with a degree, Bachelor of Astrophysics. I have a Bachelor of Astronomy, and that's what everybody else around had at that time, except for Cecile. Cecile did not care for orbits. She was interested in physics, and so she did not take any of the courses or any of the things that led to what would have been a Bachelor of Astronomy, which involved not only physics but these darned things going around the sun. And so she had a Bachelor of Astro*physics*. She went east for a while. She was a graduate student in astronomy at Vassar. Then, when she returned, she

did one year of graduate work here. But then we had children and that made it very difficult, and so she did not continue. I'm not sure that she was vitally interested in astronomy; her interests were changing, with children. Witness the fact that when our children grew up and were away, she returned to Berkeley. She came back as a student – what are they called now, mature students? Return after the children are grown up – and she took a degree in social work. And she has followed that profession ever since.

JS: As part of your graduate experience, you were a Lick Fellow at Lick Observatory.

HW: Yes.

JS: Was that for a full year, or nine months that you were at Lick?

HW: Oh. You mean the difference between nine months and a year?

JS: I'm just wondering, a Lick Fellowship was held for a year, is that correct?

HW: Yes. You know, that's curious, I may have had it two years; it could be for two years, and I think I may have had it during both of my graduate years. I'm pretty sure I did. There were two Lick Fellows, if I recall, at that time. It paid the magnificent sum of six hundred dollars per year. It was very hard to live on. Of course, the dollar was worth a lot more then than it is now, but it was a different kind of thing. You can see, I mentioned yesterday that in the second and third summers at Mount Wilson, Baade had arranged that I received five hundred dollars. So you see, that was a very substantial addition to the Lick Fellowship of sixty dollars a month. There's a curious thing that remains in my mind about that Lick Fellowship. During one of the years, through the department, I was offered the opportunity to teach one of the extension courses. That meant just handing out assignments, and correcting papers, and things like that. And that would have been a welcome addition to the sum provided by the Lick Fellowships. But the Scholarship, or Fellowship Committee decided that was not permissable, that the great honor of a Lick Fellowship meant that I had to devote all of my time to my studies. And so I wasn't permitted to do it, and I always thought it was a rather small sort of thing to do.

JS: When you were a Lick Fellow, did you reside in Berkeley or at Mount Hamilton?

HW: In Berkeley, yes.

JS: Did most students, when they had that, reside in Berkeley, or stay at Mount Hamilton?

HW: It could be worked in either way. Many who had it were in the last stages of their thesis preparation. If that involved observations at the Lick Observatory, they might be in residence at Lick, though it was not necessary if their work kept them on the campus. And all of my observations that resulted in the thesis were made at either Mount Wilson or Palomar. And in fact, most of the measurements on the plates were made at Mount Wilson. So there was

no need at all for me to be at Mount Hamilton. I did participate in observations at Mount Hamilton. I assisted Trumpler in his observations of star clusters.

JS: You said that at that time, there were typically two Lick Fellows at any given time?

HW: Yes, I think that is right. There may sometimes have been three, but I think that two was the normal number.

JS: So who was the other Lick Fellow when you held that position?

HW: Well, let's see, there were several different ones at different times. Now let's see...

JS: Was John Irwin named at the same time?

HW: John Irwin may have been, but John Russell was. John Russell, as I recall, was a Lick Fellow at that time, and John Irwin was working on binaries, eclipsing binaries. Do you have the years of the Lick Fellows?

JS: Well, I looked in the *Publications of the ASP*, and what they listed at the time you were appointed was John Irwin. The preceding year was Blanche White and Hans Panofsky?

HW: Yes.

JS: And the following year, after you, was Elizabeth Scott and Keith Pierce, with Julie Vinter Hansen as a Kellogg Fellow. So you must have been acquainted with all those people as graduate students?

HW: Oh yes, yes. Now, not Julie Vinter Hansen, because she was a full professor in Copenhagen. And she came here, and was really caught by the war, and they provided her with some sort of fellowship at Lick.

JS: During the time that you were a graduate student, for those two years, were there students you counted as particular friends or colleagues you worked with at that time? Or with whom you talked a lot about your science?

HW: Well, certainly John Irwin, because John Irwin and I shared the same office. We were in Room 8, if I remember the number. John finally went off in war work and he didn't finish his degree until rather later. I don't remember the year – have to look it up. So certainly with John. And I remember assisting John Russell in some of the photometry that he had been doing, so we often talked about that. Hans Panofsky was a very close friend. And Maury Osborne, whom you have not mentioned. Maury, I guess, was really my closest friend. He left for war work before he finished his thesis, and he was really very much delayed in completing. And he never did complete it in astronomy. He completed it in, I guess I'd have to say, biology or icthyology. It was about how fish swim. He was an extraordinary fellow; he has lots of interests. He left to do war work at the Naval Research Laboratory,

and he remained there then, for all of his career. He finally retired from there. He was away occasionally on – I don't know, we might call them sabbaticals. He came here to teach one semester. And of all things, he taught in the business administration school.

Maury had a great many interests. At the Naval Research Lab he became a specialist. He had worked in underwater explosives, and submarine warfare, and things of that sort. That's how he became interested in fish swimming. He would work out all kinds of – I'll call them puzzles – how this worked, how that worked. He has several papers on how bumblebees fly, as well as this thesis I mentioned. He was their specialist in relativity theory. He did very extensive work in relativity. He also, for one of his examples, I remember, needed some random numbers. And so he said, "Well I'll get 'em from the" – this was while he was at the Naval Research Lab – "I'll get them from the stock market quotations." And so he became interested in that, and became a discoverer of the random walk theory of stock market prices. Now it turns out that he had been scooped by some Frenchman a few years before, in a sort of hidden paper, one of these things – a double discovery, and so on. But in this country he was really the founder of the random walk theory of stock market prices. And he's done a great deal of research in the stock market. And it was when he was here that he was teaching a course, a graduate course in theory of the stock market to the business administration students. Well, he was really my closest – I think he and Hans were my closest friends as graduate students. I still see Maury occasionally. Hans, unfortunately, retired. He left astronomy and went into meteorology. He retired, and he died a few years ago.

JS: Was Hans Panofsky in an academic position?

HW: Yes, he was the head of the meteorology department at Pennsylvania State.

JS: Some of these other people that you worked with at that time, did they stay in astronomy? John Irwin and John Russell?

HW: Oh yes. John Russell was at the University of Southern California. I'm pretty sure that was the only position that he ever had. He was in the Department of Astronomy, has retired since. Gibson Reeves, who was a graduate student later, that was rather considerably later, also went there, another member of that department. John Irwin was at Indiana for a long period of time. He also was involved in establishing observatories, and was very much involved in establishing the National Optical Astronomy Observatory. Betty Scott – you probably knew her here – went into statistics. Roxie White did not continue in astronomy. Poor Roxie had a rather bad time. Her story shows the difference between then and now. Roxie was a Lick Fellow, but she got married. And Leuschner wanted to throw her out. I think he canceled her fellowship, as I remember it. We were all scandalized by this procedure. But she really was sort of ousted because she had the audacity to get married as a graduate student. This was frowned upon. The story at Lick was that you can't bring them with you

– you can't bring either wives or cars. You were not permitted to have those two items on Mount Hamilton.

JS: Since you spent a large part of your time at Berkeley, did you have much association with the astronomers at Lick at that time?

HW: Well, a modest amount, yes. I had been there for a portion of, I guess I was there one summer. I'm sure I was there one summer, yes. Let's see, I'll try to put in, and think. I was there that first visit I mentioned yesterday, where I stayed with Neubauers. That was in '38. I spent the summer of '39 at Mount Hamilton, the summers of '40, '41, and '42 at Mount Wilson; and from Mount Wilson at the end of the summer, I then had a postdoc, a National Research Fellowship, and I went to Yerkes Observatory. So that is the sequence. So I had been at Lick one summer, so I did come to know the Lick astronomers then very well.

JS: Were there astronomers at Lick that you interacted with particularly? You mentioned Neubauer that you visited there one summer.

HW: Yes, he was a very close friend. And I interacted with him more as a friend, though our ages were obviously very different, than as someone from whom I learned something. Well, I'll put it bluntly; Neubauer was not the world's best astronomer. So I think that made it difficult to learn much. I had helped Trumpler that summer with his observing, and later on trips to Mount Hamilton, because he lived in Berkeley during the academic year. The one that I really wish I had known better was Art Wyse. Art Wyse was the astrophysicist at the place. The others were – well, some of them would have called themself astrophysicists, but their work had to do more frequently with sort of careful observations, identification of lines in spectra, and things of that sort. Binary stars, yes, things of that sort, but not with deep astrophysical significance. On the other hand, Art Wyse was an astrophysicist. He was interested in the theory of atoms and how they worked, and what it meant to observe certain combinations of lines. He was interested in interpretations.

JS: Did he work primarily on nebular spectroscopy?

HW: He worked a good deal on that, but it wasn't the only thing. But he certainly worked on the spectra of nebulae. He was a very friendly, inspiring young man who had great, great promise. It is a great shame that he was killed in an accident during war work. He and Nick Mayall both left Lick to go into war work at the time. And I might very well have done a thesis with Art Wyse if he had been available and had survived. I don't know, maybe I wouldn't have; I might have done the same thing I did. But I can see that I would have enjoyed very much working with him, and working more in the field of astrophysics than with the sort of statistics and things that I did.

##

Doctoral Research

JS: Could you say a few words in more detail about the nature of your dissertation research?

HW: Yes. It was a study of a very small section of the Milky Way in the constellation of Aquila in the dark rift, which is a concentration of material that obscures the stars in that region. It involved the determination of the distribution in space of the stars themselves. That's done by counting the stars in brightness intervals, and solving an integral equation. The one other thing that you must know in order to solve that directly is the distribution in a given volume of space, the luminosity function, so called. From the counts of stars and a knowledge of the luminosity function you can tell the, I'll call it, the apparent distribution of stars in space of the stars along a certain cone of view, so looking at a certain direction. However, that apparent distribution is only that. That is, the number of stars per cubic parsec as you move out. It is only that, because the distance scale is severely distorted by the presence of obscuring matter in space. You don't see as far as you think, because the stars are fainter not only by distance, but also by the dust that obscures them in the intervening space.

So you have to determine the distribution of obscuring material in space as a function of distance, and that you do by looking at the colors of stars as a function of their apparent distance. You get the apparent distance by knowing the absolute magnitude, which you determine from spectral type and luminosity class. And you get a measure of the amount of material in the space between you and the star as the difference between the observed color and true color of the star. The true color is inferred from the spectral type and luminosity class. So you can determine the amount of extinction as a function of distance, real or apparent, and you can determine from star counts the apparent distribution of stars, stars per cubic parsec as a function of distance. And you can then correct this apparent distribution into the true one. So that all involves a lot of determination of magnitudes and spectral types, and you had to do all kinds of observational work to get it done. Then, counting all these darn stars through a microscope. Now you could do it, oh so much better!

Well, that was part of it, and then I worked out a different scheme of solving the problem by looking at the magnitude-color diagram, the quantitative distribution of apparent magnitudes and apparent colors. That's called a Hess diagram – it's a kind of quantitative H-R diagram. The Hess diagram can give you information on the distribution of stars in space and the distribution of dust in space if you numerically solve a double integral. I think my solution was the first that had been made. It wasn't done as well I would have liked, because there wasn't really sufficient information on the bivariate luminosity function of magnitudes and colors that had to go into the integral. I had to approximate the bivariate function and hope that it was sufficiently accurate. The solution of the double integral did agree quite well with the first solution of the problem using magnitudes and colors to determine the distribution of dust, and star counts to determine the distribution of stars.

The solution was a lot of work. The thesis wasn't world-shaking, but it did result in a few papers, some on photometry which were interesting. Again, people didn't consider it so important to publish things in those days; it was a major difference in the view of research then and now.

JS: To digress for a minute, do you think that the change has been largely the consequence of funding mechanisms and the necessity of publishing to receive funding? Or are there other issues that are more important?

HW: Yes, I think that funding is certainly one. Though the competition is so much sharper and harder now. That also means that if you don't get your work done and published quickly, you may not be the one who publishes. I think that the competitive spirit is much stronger now than it was all those years ago. The group of people working in any field then was much smaller than it is now, and you didn't have to be afraid that somebody else was going to come out with a solution or an idea if you didn't publish fast. It would come out eventually, it would be published, but there really wasn't the pressure that there is now. Now, even among the students, I think, the pressure to publish is very great. It provides them with a means of showing their ability and getting fellowships at a later time. Again, it's this kind of competitive spirit that seems to me to be so strong now.

JS: Did you define your thesis topic largely on your own, or did Trumpler suggest it, or how did that come about?

HW: I did it. I had been reading a lot of papers relating to photometry at that time, and it seemed to me that this was an interesting application of photometry. I was really thinking about photometry as much as anything else. So I had proposed it. That particular area of the Milky Way had already been studied before. There was a fair amount of information on it; in fact, one of Trumpler's earlier students had worked in that area of the sky[3]. And it just seemed to me that I could do it very much better, and it was a good illustration of these methods. So I did it. So I proposed it and he approved. And I guess it was a good thesis. At any rate, it was enough to get me a fellowship at Harvard that I never accepted.

Robert Trumpler and His Family

JS: Yesterday it came up that it was while you were an undergraduate here that Trumpler transferred to Berkeley.

HW: To Berkeley, yes.

JS: Do you know much about his motivation, or circumstances why he made that change?

[3] Clifford E. Smith, *Lick Observatory Bulletion*, no. 484, vol. 18, pp. 39-51

HW: Well, I think there were several motivating factors. One was that he was at Berkeley. You see, the Trumplers had been living in Berkeley for several years. The reason for that was that the children – they had five children – their children were all of an age that they couldn't go to school on Mount Hamilton, so they had to go away. In fact, Cecile had been away in Switzerland in school. She's the oldest in the family. And she had been in Switzerland going to high school there. Then, other ones of the children came along to go into high school. So the Trumplers did, in fact, move to Berkeley, even while he was on the staff at Mount Hamilton, and they had bought a house on Piedmont Avenue. So at Berkeley Trumpler continued his research, which involved mostly measurements of radial velocities, reduction of data, and so on. He did occasionally teach. He taught a graduate course in statistical astronomy, maybe every other year or so – I don't know the exact schedule of that; I'd have to look it up in the catalog. So he was in Berkeley. He did enjoy teaching. That's why there's a Trumpler Prize. That was in his honor because of his real interest in students and in teaching. He really liked to teach. So it was a kind of natural move for him. Leuschner retired; there was a position available, and Trumpler was invited to take it. And he did.

JS: In the history of Lick Observatory that's been written by Osterbrock and his collaborators[4], there is perhaps a suggestion that one reason Trumpler may have wanted to make that transition was some tension between Trumpler and W. H. Wright, who became the director about that time. Do you think that was an important consideration?

HW: That book, by the way, is not a popular one among the Trumpler family. Even to my knowledge, it is full of stories that are untrue. I think that the Trumplers had their problems on Mount Hamilton. There's no doubt of that, it's quite clear. Because they were different. They were Europeans, and they had problems because of that. They didn't understand American customs. And the astronomers at Lick Observatory were not entirely free of prejudices. They didn't understand Europeans. And certainly Trumpler did have his differences with Wright. Interesting that that should come up. There were various pet names, for example. Things of that sort. That I will not repeat. [laughter] It's conceivable that it was a factor. I don't think it was an important factor, because you see, Trumpler was already here. He had the use of the Lick instruments, and he went observing when he needed to observe. He had the thirty-six-inch [refractor] telescope to use for his radial velocity work. For a long time, the Trumplers lived at Mount Hamilton during the summers. They retained their house at Mount Hamilton, the house that had been built for them, which is the one nearest the Crossley [telescope] – just down from the Crossley. And so he had everything that he wanted. He just didn't interact very much with Wright. But for some reason, and I do not know the details of that, there certainly were tensions between those two people.

[4] *Eye on the Sky, Lick Observatory's First Century*, by Donald E. Osterbrock, John R. Gustafson, and W. J. Shiloh Unruh, University of California Press, 1988

In fact, there are many well-known stories showing that tensions always tend to develop at Mount Hamilton. Astronomers didn't speak to each other for years and years because of problems with the dogs and cats and things of that sort. It's a universal problem with a group of people living in a small area, a very small area. And having difficulties that build up, imagined or perhaps not imagined, but starting because of imagination. It's cabin fever, and it showed up in many different situations at Mount Hamilton. Especially true because the individuals there had rather different interests, you see. There was a small group of astronomers, and Lord knows, they had enough tensions between themselves. But then, there were all the workmen who hadn't really the slightest interest in astronomy and had quite different interests in life, and so on. And you have these two different populations with vastly different interests and drives, interacting on a daily or nearly daily basis. And so, problems, and difficulties, and slights imagined and real, and jealousies imagined and real, etcetera, etcetera, build up. Mount Hamilton was always a difficult place because of that. It was a wonderful sociological study, and would have been really great to have worked on that as a sociology thesis.

JS: You commented earlier that this Lick history book, you feel, or the Trumpler family has felt, misrepresented some circumstances. Are there other points you think that were brought up in that book that you'd like to comment on?

HW: Well, the interesting part is, whenever I have discussed the book with astronomers who have been associated with Lick, the same sorts of remarks have been made. A characteristic remark: "Well, it seems good, but in the years that I was there it wasn't like that." You see, it always sounds fine when that individual wasn't present, but when the individual was present, it sounds a little distorted.

Well, just the one thing that I think really bugged the dickens out of the Trumplers was the fact that they are quoted as being these – I'll paraphrase it and perhaps ham it up – these Swiss peasants who had all sorts of goats and dogs and cats and animals. And as a matter of fact, they never had any of those things! For one thing, they were all forbidden on the mountain, because of an early incident in which there was a fight over – I guess it was a dog. Two astronomers didn't speak to each other for years and years and years, and dogs were absolutely forbidden on the mountain. It was not until Shane came as director, and the Baustians had a dog, because Nancy, and I guess Bill maybe, wanted a dog. But that was why there was finally a dog on the mountain. And when we went to live there – we lived on the mountain, Cecile and the children and I lived on the mountain for several years – Cecile was always just astounded by the fact that there was a dog on the mountain. Cats on the mountain, even, were forbidden. Hamilton Jeffers had a cat, Kitty Mules. In the early days, there were hours when one family could leave leave its dog or cat out, and other hours when the family had to have it in because another dog or cat would be out. These were the sorts of things, you see, that lead to these difficulties on a small mountain

top, or in any small community. If it had been a town in the valley, not on a mountain, but in the valley, a town of fifty people, there would be all these same tensions and difficulties that arise. I think they always do.

JS: Concerning the two astronomers who were unwilling to speak to each other because of a matter of a dog, are you willing to name those people, or are those names best left unsaid?

HW: Well, that was before my time. I think one of them was Heber Curtis, but I'm not sure at the moment. Cecile would know, because that was, you know, a story of her childhood, so she would know. But that's the kind of difficulty that arose.

JS: Today on Mount Hamilton there's still a locale that's identified as Trumpler's garden. Was that an active pursuit when he lived on the mountain?

HW: Very much so. Yes. In fact, the road that goes over there was built by the Trumpler family with shovels and picks. The children, the Trumpler children, the four of them who are still alive, still talk about the enormous family project of building that road. They carried the lumber – have you ever been there?

JS: No, I haven't been there.

HW: There's a little house there. It's badly run down now. Cecile and I walked down to it a few years ago. It was over the saddle and down. It's quite a few feet down, you know, like five hundred, maybe, or so. That's a guess. There's a spring there. That's the reason for its existence at that point. It had been used as a garden before Trumpler. But Trumpler was the one who really developed it. He was always interested in a garden. He was a strong gardener. He had lots of flowers and lawns and things like that, in Berkeley and later in Rio del Mar. He spent all of his spare time doing that. And on the mountain, where things are so dry, they didn't have much of anything around the house in the way of a garden. But there was this one opportunity. So they built the little house where they could actually stay overnight. And developed a garden, which was largely, as I remember it, it was mostly a vegetable garden, and things of that sort. With some trees. The place was called – the family called it always – Cherry Shade, because there was a large cherry tree that was over this little house that they built, just a one-room cabin. And there were all kinds of vegetables, and some flowers, but it was mostly a garden where I guess he could do something different and get away from the grind of measuring plates, which he did an enormous amount of. The place was – there's many interesting stories about it. It was originally associated with the name of Joaquin Murrieta, who was the Robin Hood of California. Have you ever read or heard about him?

JS: No.

HW: Well, he was the Mexican bandit who escaped the law, as Robin Hood had done. And he took money from the rich and gave it to the poor, and he's one of California's legends. Well, he hid out with his band of bandits, and one of the places they're reported to have hidden away is at that spring, back of Mount Hamilton. And so it's called – it's Murrieta Springs. There's an interesting personal story about it, that has many connections. When I was a child in San Jose, I worked at various small jobs when I was in junior high school, I guess it was junior high. I was pretty good at art work, and I could letter signs and do all those things. And I like to do that. And I made signs for a bookstore. I made all of their signs. And it was during Depression times, and I didn't get paid in money; I got paid in books. Now that was fine for me because I loved books. And I had picked out one when I had built up enough money from making my signs, on Joaquin Murrieta. Well, I came to get the book, and it had been sold. I was told that it had been sold to someone who wanted it as a birthday present for a child. And it turns out that it was the Trumplers who had bought it for Cecile. And so, it's still around home, because I have the book now in the library. But it came through a very long and tortuous path. [laughter] And I once told her – you know, I didn't tell her that story right away. We had been married quite a few years when I told her, and I told her I really married her only to get that book which I had been wanting from the time I was a child. [laughter]

II. Postdoctoral Research, War Work, and Appointment to Lick Observatory

Postdoctoral Appointment

JS: You mentioned that one result from your dissertation was an offer of a fellowship at Harvard.

HW: Yes.

JS: Which you turned down.

HW: Yes. I had been awarded a National Research Fellowship when I finished my thesis, and that was when I went to Yerkes. Postdocs were very rare in those days. It was quite different from now. I think there were essentially only two. There may have been a few others around, but there were essentially two national ones, the National Research Fellows of the Research Council. And that year Al Hiltner and I were the National Research Fellows, both at Yerkes. I published one paper[1], and I had data for a lot of others. I published on a nova, a paper on a nova, and identified the lines, and was trying to get some of the physics out of it. It was the first discovery of forbidden Fe II in a fast nova. They were in slow novae, but this was a fast nova. And I had a lot of material for the study of the evolution of galactic clusters, which is what I had chosen to work on under that fellowship.

Well, it was getting pretty hard to stay in astronomy. The war was underway at that time. Jesse Greenstein and Louie Henyey were still at Yer – they were at Yerkes. They had given up their research and were doing optics, optical design. That's when the Henyey-Greenstein camera was developed. It was quite clear that I would not be able to stay in astronomy very long. Otto Struve [Yerkes Observatory director] was trying very hard to get a few people to stay. He once said, "I, Otto Struve, am running the whole observatory, there's no one here to observe and take care of it" – and there was the [McDonald] observatory in Texas, and the observatory at Yerkes. But it was really impossible to continue doing pure science and research when the war was in progress, and more and more people were being drawn into the war.

Well, at just that time – I was trying to stay in astronomy, because I wanted to do it – well, at that time, while I still had the postdoc, the National Research Fellowship, I received a letter from Harvard inviting me to come for an interview for a Junior Fellowship, which would have been, of course, a marvelous opportunity; several years, with nothing to do but what you want to do, and so on. I had known Bart Bok. I had met him in earlier times. But that was when I first came to know Harlow Shapley, and Cecilia Gaposhkin, and Donald Menzel, and so on. And after an interview, which was a very interesting one in many ways – it was almost like a doctor's oral – I was invited to become a Junior Fellow. And though it hurt me terribly, I had to say no. And in fact, at that same time, I had to give

[1] "The Spectrum of Nova Puppis 1942," *Astrophysical Journal*, vol. 99, p. 980 (1944).

up the National Research Fellowship, and join in the war effort. I really would have enjoyed having that fellowship. It would have been a great opportunity. I had to give up all of them, and go into war work. And so I left Yerkes, we – Cecile and two very young children and I – left Yerkes, which we really enjoyed very much, and where I also had an offer to be on the faculty. We left Yerkes and all the other things, and went to Cambridge, where I joined Ted Dunham, whom I had first known at Mount Wilson, in the Optics Division of the National Defense Research Committee.

[Interview 3: September 5, 1991]##

JS: Yesterday we talked about your experiences as a graduate student, and then started talking some about your work after you left Berkeley. And you said that after the summer you spent again at Mount Wilson, you went to Yerkes.

HW: Yes, to Yerkes. One thing I was thinking through on the way home last night was the sequence of years when I was a graduate student. I'm sure now that I was a teaching assistant the first graduate year and a Lick Fellow the second.

JS: Ah.

HW: That was it. Because I remember I was a teaching assistant one year, and that could only have been the first. So this is now after Mount Wilson and going to Yerkes.

JS: You said that your postdoctoral fellowship was from the National Research Council?

HW: National Research, yes, National Research Council Fellowship.

JS: During the interval that you were at Yerkes, did you collaborate with any particular people on the faculty there, or did you work mainly on your own?

HW: Well, it was substantially on my own, though the plan was that I would work with Struve and Pol Swings. The problem that I wanted to investigate was the H-R diagrams of Galactic clusters. There had been quite a few of them looked at in a variety of ways, though it was just beginning to be understood that somehow different clusters had different diagrams. Trumpler had published that some time before.

And Gerard Kuiper, who was on the staff at Yerkes – though he was away on war work assignment at that time – had early on published – I guess as a result of his being at Lick as a postdoc – the diagrams of several clusters that departed from the (now we would say zero-age) main sequence. Now, we know, clusters depart from the main sequence as a function of age; then it was attributed to the hydrogen content of the cluster stars. What I wanted to do was to investigate and compare the chemical compositions of a few clusters that seemed to define an essentially straight line sequence with the chemical compositions of a few clusters that seemed to depart substantially from such a straight line sequence.

And what I started to do was to obtain with the – I used whatever plate material was available at Yerkes. Bill Morgan had been investigating clusters to some extent. He had a number of sets of plates of the Pleiades, for example, I remember, and I used his plates on the Pleiades. And I also obtained some from my own observations at McDonald, plates particularly that went into the ultraviolet, at pretty good dispersion for those days. I observed spectra of a number of stars in a number of clusters, and the hope was to investigate those in a fairly thorough way. That project never got anywhere because I left before I had finished anything. I did continue it after the war, when I went to Lick when I was on the staff there. The first things I did at Lick related to this work that I had started earlier. The one thing that I did publish in the time that I had at Yerkes – I guess I was there something like six months or so; yes, it was just about six, maybe seven months – was a paper on a nova, which I think I mentioned earlier. Nova Puppis.

JS: You said you observed at McDonald. Did you make multiple trips there to work?

HW: I made a couple of trips there to work. And they were fairly long runs. I remember – I *still* remember – one, when I was almost happy to see clouds come in. Horrible thing to say. But I had several weeks, every night, all night, and because of the war, there was no night assistant. And so it was an all night affair, through December. And so I'd really had it at the end of that run. Because at that time, you see, you had to take all of the plates, they were photographic plates, and you had to develop them the next day. And so it was really about a twenty-hour-a-day job. It got very tiresome toward the end, I can assure you. But I got a lot of good plates! There was really a lot of clear weather. Strange to remember that one run so vividly. My stay at Yerkes Observatory was all too short a period of time, and I'm sorry that I had to cut it short. I enjoyed very much working with Struve and Swings. I especially got to know Swings quite well.

JS: Did you live in Williams Bay when you worked at Yerkes?

HW: Yes. I started, I went alone. That was my fate in those days. I would start out alone, and find a house, and then the rest of the family would come later. But I remember staying first in a very nice, homey boarding house that had a few people. It was a kind of hotel-like place, down right on the shore of the lake. And walking up to Yerkes every morning and going to work, and so on. And then finding a house to rent. We had a very nice house again, on Congress Avenue, on down toward the lake. It was a house that was owned by Mr. Charles Ridell, who was the – I guess he was the chief, I think they had more than one – the chief instrument maker at Yerkes. It was quite a large house, with a large garden, and was very pleasant while our stay lasted – mostly in the snow, through winter. We just barely saw the spring and the earliest part of summer. The garden grew magnificently once it started.

JS: When you went observing at McDonald, how did you actually get from Chicago to Fort Davis?

HW: By train. It was train travel in those days. You could also go by bus. Dan Popper, who lived near us in Williams Bay, and whom I had known from Berkeley, always traveled by bus, but I didn't enjoy that very much. I went by train.

JS: And how far could you get by train?

HW: As I remember, I ended up in Marfa. And then someone from the observatory came and got me.

JS: At that time, were the observers using McDonald exclusively from Chicago, or were people actually at the University of Texas starting to work there?

HW: Oh no, it was exclusively Chicago. There wasn't anything at Texas at that time. That came later. So it was really a Chicago outpost. Had been established by Otto Struve.

JS: You said yesterday that when you showed up at Yerkes, many of the staff members there were not actually doing astronomy because they'd been diverted into work related to the war.

HW: Yes.

JS: Who were the people that were still doing astronomy?

HW: The ones who were still doing astronomy were, of course, Struve, who said he would run the observatory by himself as a one-man show if he had to. And Pol Swings. George Van Biesbroeck was there, observing comets. I had a very interesting time with Van Biesbroeck. And W. W. Morgan was still there. S. Chandrasekhar was there. And there were some students who are still around. Some graduate students. The one thing that I did sort of apart from the research that I started there was, I went to Chandra's lectures on stellar structure. I'd had a course here in Berkeley with Shane on stellar structure, that used Chandra's book. And so I enjoyed hearing it again, and from the master himself.

JS: Were all the functions of the observatory and astronomy program then out at Yerkes Observatory, ...

HW: Yes.

JS: ...or did you go in to the University of Chicago?

HW: Oh no. Nothing was at the Uni– well, I'd better be careful how I say that. But all of the astronomical operations were at Williams Bay. The students lived there. They had all their classes there. There was one room at the observatory that was devoted to a classroom. And Chandra was there and did all of his work. It was only later, when there were a variety of difficulties, personality difficulties at Yerkes, that people began to leave and when they started Chicago as a – when Chandra left for Chicago, and so on.

JS: Did you use any of the telescopes at Yerkes itself when you were there?

HW: No. I spent some time with people who were using them, so I just saw what they were like. And I do remember observing one night on the little reflector with van Biesbrock, when he observed comets. It was a very cold night, and he had to break the ice out of his whiskers when he got through.

JS: Was the atmosphere significantly different at Chicago from what you had experienced at Berkeley, with the greater emphasis there on astrophysics?

HW: Oh yes. It was totally different. It was much more like Mount Wilson, which had been a research atmosphere. People were doing research at Mount Wilson. The people here at Berkeley were doing teaching. As I mentioned, the only one on the faculty staff – that does not include Trumpler – the only one who was doing research in a regular way was Shane. I better be careful I don't damn Leuschner with faint praise, but he did have a project going, with WPA workers doing the work. They were somewhat like work-study students, except they tended not to be necessarily students, but people who were out of work and doing things in order to make a living. This was after the Depression period was tailing off, when I was first a student here. And Leuschner did have a large project going. I would hardly call it research in a formal sense. But these people were compiling all the data on the minor planets. That resulted in a very thick Lick publication volume on research surveys of the minor planets, which was Leuschner's, of course. So he was, in that sense, doing research, but one scarcely recognized it as research as we see it here, for example.

So yes, Yerkes was much more a research-oriented organization, and it was a real pleasure in that sense to be a member of the place. It was a kind of combination of Berkeley and Mount Wilson in a way, because Chandra, who did not observe and measure plates – in fact, he used to boast that his contract formally excused him from any observing, he did not have to do that. He once participated, I recall, in a brief foray into – I think it was Canada – with a couple of the people at Yerkes to observe an eclipse; and that was his part of observing. But, with Chandra there as a wonderful lecturer, it was a kind of a combination of a super-Berkeley and a research institution. It was, I think, a splendid place for students at the time. Because they had both aspects of it, as I think all of you have here, which I think is the wonderful strength of Berkeley, that it has both teaching and research. Very vigorous research in a number of fields.

JS: When you initially accepted your fellowship to go to Yerkes, what was the interval of time that would have funded you for?

HW: It was a couple of years. My recollection is it was one year, renewable. And I think it would have amounted to two years. Though I could have stayed; Struve had already invited me to remain at Yerkes as a staff member, so I could have either had a position at Yerkes

or a continuation of the fellowship. Or, actually also as it turned out, a period of time at Harvard. So I would have had a choice of two places and several possibilities.

JS: Did Struve extend this offer after you arrived there to take your fellowship?

HW: It was after he came to know me at Yerkes that he made the offer. Yes, it was not before, it was not before, no.

JS: You left that position before the full interval of time you could have taken it, and to pursue war work. And as we mentioned, there were quite a few other people who were not working in astronomy at that time. Did people move into war work out of a sense of obligation to help the government in the war, or under pressure from the government to assist with some of this research? What was the motivation?

HW: There must have been different motivations for different people. Certainly I think many went because they felt that it was a – that everyone had to pitch in at that time. It was a serious affair, though I think the fears of invasion were overdone. There was fear that there would be an invasion on the West Coast. I remember very well the great fears here that the Japanese would continue right in after Pearl Harbor and start on San Francisco, or this area. And people were indeed panicked in some areas on the coast itself. There were air raid warnings, and blackouts when everybody had to keep everything dark, and so on. I got caught in one of those blackouts while I was on the campus finishing my thesis; I still remember that.

But I think that people had a variety of feelings. Some may have felt pressures, of course; everybody was encouraged to be a part of the war. It was a very different aspect of living. Living through that war was very different from living through, say, the Vietnamese war, Vietnam war. Totally different feeling on the part of the country. It was, of course, a much bigger affair, and everybody was involved. At Yerkes, war work was in progress. Louie Henyey and Jesse Greenstein remained at Yerkes and worked there. They were not doing astronomy; they were working on optics. Gerard Kuiper was away at MIT in radar work, as I remember. Dan Popper came here to Berkeley. He returned to work at the Radiation Lab on the project here. Lots of people came who had been here, came back and worked at the Rad Lab. And I finally ended up there too, after another job first, in the war. My feeling was that you had to just help out as much as you could, and do what you could to help the country.

JS: If that was the sense you felt, why did you initially take this position at Yerkes, and then after...

HW: Well, I guess I hoped I could escape it. I really didn't want to – I wanted to do astronomy, I wanted to try to do astronomy. It was the thing that I had most wanted to do. And I was

hopeful that perhaps it would be possible to continue. But it clearly was not. I suppose also that if I hadn't done something, I eventually would have been drafted. I was down the list quite a bit because of being married and having children. We had two children at that time. But eventually, you know, they took nearly everybody, so it was a question of serving one place or another. And there were a lot of opportunities to serve in useful capacities doing things that you knew something about, and perhaps could be helpful; so I finally, at the beginning of my war work, became involved with optics research.

War Work at the National Defense Research Committee

JS: And that was at Cambridge, is that right?

HW: At Cambridge. There was a very large organization of several layers that was started, I guess it was started by Vannevar Bush. The top layer was the Office of Scientific Research and Development. At this time I don't remember how many second-layer branches were under that, but one of them was the National Defence Research Committee. And under that were a very large number of subsections that dealt with all kinds of different scientific fields that the federal government needed, that the army and navy needed for their work. And lots of the people from the scientific and engineering community were involved in these groups, in these panels. For example, NDRC Section 16.3 was optics. There was another – I may have the numbers a little wrong – 16.1 was ultraviolet, 16.2 was infrared, and visible optics, etcetera, etcetera. And the chief of that division – the 16 was the division and then there were sections under that – well, the chief of the division was George Harrison, the physicist at MIT, who was the chairman of the physics department, I think – no, he was dean, he was dean of science at that time. And then, the optics division was headed by Ted Dunham, who was a great optical guy, who was always involved in that. He was an astronomer at Mount Wilson. He had built the coudé spectrograph, for example, and all kinds – he loved instrumentation. And he was the head of the optics section.

I had become acquainted with Dunham at Mount Wilson – during those summers at Mount Wilson. Now, he was writing and phoning to get me to join him at MIT as the technical aide in the optics section. I agreed to meet him at one of the railroad stations in Chicago when he was between trains on one of his many trips across the country. We talked for some time and he twisted and twisted my arm trying to get me to come to join him at the optics section. I knew I had to leave astronomy; this seemed like something useful and needed that I could do. So I left Yerkes in, it must have been, late spring or very early summer, to go to MIT. The staff of the optics section was very small; it consisted of Ted Dunham as chief of the section, me as technical aide, a secretary, and a typist who worked on the constant flow of reports and letters.

Cecile had followed in her father's tradition, and always liked to garden. And I remember when I came and got her and the children – again it had been one of these

arrangements that I left and found a house, and came back, etcetera; that seemed to be my fate – that she pulled up all the vegetables that she had planted, put them in a handbag, and took them to Cambridge. They were just barely starting out. But that growing season in Yerkes is wonderful short, and things grow very vigorously. So I went to MIT, and there assisted Dunham. And worked on all kinds of projects in optics. You didn't do optics in the section in that sense, in designing lenses. I never designed any lenses, or any of that. Our task was to take requests from the armed services and try to convert them into reality.

JS: So they would specify what they needed...

HW: They would specify, or sometimes the section would propose what they ought to have in order to do a certain task. And then the section would find a contractor to do the research, or make the device, or whatever. And then we would see to its testing, sometimes semi-field testing, and so on. So I ended up – Ted was always running around throughout the country on these things – and I found myself doing the same thing a good deal, visiting, oh, the University of Rochester, the optics department there, where there was Brian O'Brien. Each section had an advisory panel consisting of a group of scientists who had the responsibility of seeing that the section worked. And so, there was I. S. Bowen, and W. V. Houston from Caltech, and Brian O'Brien. Oh, and R. W. McMath of the McMath-Hulbert Observatory, for Section 16.3. And Ted as the chief of the Section. I guess that was the complete advisory group for the Section. So I got to know those guys fairly well, which was nice.

So there would be research, and manufacturing, and testing, and this would be at a large number of places throughout the country. At Dartmouth, Dartmouth Eye Institute was doing visibility tests. Very interesting, I sort of enjoyed that part of it. They were determining what properties of binoculars most improved vision under low levels of illumination, for example. Very interesting things. Then there were tests on resolution problems for aerial photography. A good deal of that was done at Mount Wilson, and Minkowski was very much involved with it. The Section arranged for the design of many optical devices – by Jim Baker and Perkin-Elmer, at the Eastman-Kodak Company, at the University of Rochester with Brian O'Brien and the whole optics division, at Polaroid where Edwin Land had lots of ideas, and many other places. We had projects all over the country working on cameras, gun sights, tank telescopes, – almost anything you can name. Ted and I did a lot of traveling.

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HW: Well, there were a lot of others too, but these are the ones that I always thought of as the, sort of, the ones that were fun to work with. The task that Dunham and I shared was to go around and visit all these places to keep track of what was going on, and then when something was coming to fruition, to try to arrange a service test of it. So I got to know a number of the air force places, and other military installations. Wright Field, in Ohio; and in

Rhode Island, some of the navy installations; and everything from submarines to airplanes. It was an education, I can tell you, quite different from the university life that I had known. It was more like a business operation. Well, finally, as the war wore on, things got tighter and tighter. There was a great push here at the University of California on the bomb, on the atomic bomb. There had been requests for me to come back to Berkeley. Shane was the personnel manager here at the time. And so I returned to Berkeley finally. I felt I could do more here. And furthermore, I must say, as a personal way, that the optics section was a management job; I enjoyed more directly hands-on research. And it was that, at Berkeley, that I was involved with. So I left that job at MIT and came back to Berkeley. And worked on, not on the bomb, but on getting the material for the first bomb. The separation of the uranium isotopes.

JS: Could I ask just a couple more questions about your time at MIT?

HW: Sure, yes, absolutely.

JS: Was your background in optics adequate for the demands that were put on it in that position?

HW: Yes, it was at that time. And, of course, the job at MIT was primarily that of a scientific manager. I'm not sure my knowledge would be adequate under all circumstances now. You know, I'd have to do a lot of work. But it was fine for that time. There were no extremely fancy optics. Jim Baker had some of the most elaborate designs for aerial cameras. And there were various interesting devices. But they were all moderately simple in general character. There was nothing radically new, or – things were inventive, in the sense that tricks on old things and improvements were used, but there were no wholly new developments. I guess the closest anything came – and even that was not really radical, it was only some better adaptation – there was a project at MIT itself to produce calcium fluoride in large single, or at least, good crystalline form for lenses. And that was an interesting development, and it was new in the sense that by combining fluoride and glass, you could get a better correction and better field. So that was rather interesting, but it wasn't radically new. It was just getting a different material, a material with different optical constants, to combine with various glasses.

JS: You mentioned that you had to travel quite a bit with this job. Were you under a lot of time pressure to meet deadlines?

HW: Oh yes, sure, all of that, yes. I must have spent about a third of my time on trains. No airplane travel at that time. That required very high priority. Normally you couldn't get that. But trains, yes. I learned to sleep on trains pretty well. [laughter]

JS: In coming to the Radiation Lab, you said that that appealed to you in part because it was more hands-on research?

HW: Yes.

JS: And can you describe in more detail what you worked on in that position?

HW: I guess it's all declassified now, I can't imagine... Yes, it was a curious arrangement in some ways. I was technically assigned to the information division, but in fact I worked with the people up on the hill, and with the theoretical group. And after a very brief time of sort of indoctrination, learning what was going on, I became involved with problems of scattering. Maybe the first was in shaping magnetic fields. And the problem was to have multiple beams within one vacuum tank, to have multiple beams on the mass spectrograph. And there were several. So there were several things. Now, if you just have a long slit between the poles of the magnet, you do not get a very good focus vertically along the beam. You have to vary the magnetic field. Furthermore, you want to get a big angle in this way [gesturing]. You want to process as much material as you can, as fast as you can. And it's fairly easy to show that by shaping the field within there, you can push more stuff out and into a little slot, and collect it, which is what you want to do. So you want to separate the isotopes, and catch everything you can in this little slot, and you don't want anything that's right next to it falling in.

Well, if you shape the field, which means that you'll curve it in various ways, you can compress the beams and bring them around. And so, there are also a lot of other interesting ways of doing it, and using not a complete magnet, by partial magnets. Anyway, there are lots of interesting problems in doing that. And there are two sort of problems that go together. One is to shape the field, and the other is to shape the receptor, because it's no longer a straight line. And it's also that you need to receive on an angle. There are all kinds of little tricks in this game, which one learns by, sometimes by straight analysis, and one sometimes by empirical observation. The general principles had all been started. I had nothing whatsoever to do with devising any wholly new schemes. But what I did become deeply involved with was the process of improving the shapes, and improving the process, improving the magnetic field shaping, and so on. Improving the receptor, the receiver forms. And so that involved a lot of observation of what was happening up on the hill in the experimental mass spectrographs that were all under the big dome part of the Rad Lab up there, with the 150-inch being the main one.

JS: The 150-inch cyclotron?

HW: The 150-inch cyclotron, which was used as a mass spectrograph at the time, no cyclotron stuff. And then there were other additional magnets off to the side. Vertical magnets, big magnets, that had tanks in them, and these were all experimental devices to improve the productivity of the process. Then there were at – the name that comes back is the nickname, Dogpatch – at Knoxville, where the plant was located, and the real mass separation was done.

JS: So that was at Oak Ridge?

HW: That's at Oak Ridge, yes. And so it was in working here on the things that would be modifications there, and so on. So I remember working on those problems. And a person with whom I collaborated a good deal was Oscar Bunemann, who became a member of the faculty at Stanford in plasma physics. And Oscar and I had some very – he was in the British group. There are many, many, many things that return to mind now that I think about it. So we had a number of experiments that involved shaping fields, and modifying the metal shims, they were called, that were put in to shape it. One of the most fantastic experiments we had was one in which we wanted to vary the magnetic field over these multigauss fields. We had to design – we had the engineers design, we just specified what we wanted – turned out we had conductors that were copper bars several inches by a foot or so in cross section to carry scads of amps and very few volts, to make magnetic fields that would permit us to change the field and watch what happened to the beam. There were some very interesting experiments. And I guess we improved things a little, but it was mostly to discover that there were lots of manufacturing faults that had left little corners and irregularities that would screw the works.

But also, the other thing that I worked on a good deal – well, I guess it seemed like a good deal – was the shape of the receptor. I guess there are some actual patents in my name on that from those old days. The Rad Lab had a big patent division. And the other thing I worked on was the problem of the scattering, particularly of the multiple beams that went through each other. And I remember doing a fair amount of work on that also, and determining really that there was a limit to how far you could go with the idea of putting multiple beams in one tank, because the scattering became so great that you were collecting in this little slot all kinds of junk you didn't want.

JS: From other beams.

HW: From other beams, and all of the other scattering sources. There were a whole series of experiments that I would still – Pan Jenkins and I worked on a number of those things together. And I still would like to know the answers to some of the things that we found doing spectroscopy on the beams. And certain lines would appear in various curious forms, and intensities, why we never did figure out. It would be a very good experiment still to learn what went on in some of them. Especially, that we found by introducing foreign gases, we could actually improve things occasionally. And so there was a whole series of experiments

that we did with different molecular weight gases, and different masses of the makeup of the molecules. There was really a very, a terribly interesting series of problems in excitation and control within an ionic medium like that, a plasma medium. Anyway, you see, I did enjoy that work. I was very lucky in being able to do it. I must say I was very fortunate in remaining in that project, because I was never drafted and I never went into fighting, into the war in that sense. It ended up that there were only two of us young people left in that project, at the end of the war.

JS: So you were considered sufficiently necessary to the project that you were not draftable?

HW: Oh, I guess they thought so.

JS: You were draftable?

HW: No, no, no. The project kept me out of the draft, and there were only two of us who they were finally able to keep out of the draft to the end. Often times the people at the lab who were drafted would come back to the project on special assignment. But they didn't want to take a chance. The lab didn't want to take a chance if they could. They always wanted to retain the people who were doing work that they were really interested in. Otherwise, I remember that the navy wanted me to come too. They tried to get me away, too, from the MIT project, to take a Washington desk job. Then I would have been commissioned, and would have been the Secretary of the Vision Committee of the Navy. That didn't sound very interesting. Anyway, it was an exciting period, but I was sure glad when it was over.

JS: Do you have any sense as to whether the technology you worked on for this isotope separation was actually incorporated into the device used for isotope separation on a large scale?

HW: I am not really sure that any of the improvements I was involved with made it to the Oak Ridge plant to be of any real use in production. There may have been some use of the improved receptor shapes. We tried some larger experiments at the Oak Ridge plant, which was a fantastic sight to see. But I am not sure that we did anything to produce the wanted isotope faster. As I think back about it now, mostly, I think, I may have helped provide some insights about why the device worked as it did, what some of its limitations may have been, and how some improvements might have been made.

JS: So you must have been sufficiently involved in the overall project that it was clear what the ultimate goal of this was for, the isotope separation?

HW: Oh yes, very clear, oh yes, yes. Oh yes. We all knew that. Sure. And knew about when it would happen.

JS: I believe there were some other displaced astronomers that worked at the Radiation Lab at that time.

HW: Oh, yes. Quite a few.

JS: Who were some of those people?

HW: Lawrence Aller was there. And Dan Popper was there. They're the ones that I remember most. Keith Pierce was there. Keith was very much involved with operation. Dan was an operator of the equipment.

JS: Dan Popper?

HW: Dan Popper. Lawrence was with the theoretical group. They're the ones I most remember. There were some who later became semi-connected to astronomy in a variety of ways.

JS: When you say that the people were involved in operations, it was in operations of equipment in these experiments?

HW: In actually running the magnet, running the mass spectrograph, the Calutron.

JS: Was George Herbig also at the Radiation Lab?

HW: No, George was not there.

The Atomic Bomb and the End of the War

JS: Did your work with the Radiation Lab end abruptly at the end of the war?

HW: Yes, at the earliest possible moment. At that time, Berkeley was just starting to come alive again, as far as normal things. When the bomb was dropped there were repercussions all over here of a variety of kinds. Very strange ones. There was hope to get back to normal at the earliest possible moment. Shane was in process of being appointed director at the Lick Observatory. And he wanted me to be on the staff, to join the staff. He was not formally director at that moment, though he was clear he was going to be the director. So Joseph Moore, who was the director, actually appointed me to the staff at the Lick Observatory; but on leave for the time being, to teach in Berkeley, because they were going to be short one person. Shane was not there. And Shane taught the astrophysics courses and so on, so they were sort of short-handed for a while. So I was on leave to teach at Berkeley. And I still recall that I taught that astrophysics course, which is now the undergraduate series, was called 117 then. And so I taught that course. And I was still working at the lab. And so I would go down and teach the course, and go back to work up there. But as soon as I could, I became disconnected from the lab.

JS: So did that arrangement start in 1945?

HW: Yes. It was very, very soon after the bomb was dropped, or the bombs in the plural were dropped. Yes. And the war ended.

JS: Working in the Radiation Lab, were you aware after the successful test of a bomb had been made in New Mexico? Did that information reach you?

HW: That it had – did what information?

JS: Well, that a successful prototype had been exploded in New Mexico.

HW: There certainly was a strong rumor to that effect, yes.

JS: And was it known that there was a strong possibility that it would actually be used in the war?

HW: Yes. Yes. And there was a great division of opinion – the one that occurred everywhere – should it be used as a test, or should it be used directly. Yes.

JS: I'm intrigued by your statement a little while ago that there were a variety of strange repercussions all around the University of California and Berkeley.

HW: Well, I'd better be careful. I don't know about "all around Berkeley," but certainly at the Lab. Some people were joyful. It had worked; they wanted it; it would end the war. And some got sick. So. You could take your choice of anything in between. And some who had very little reaction.

JS: Did you yourself have a strong reaction at the time to its use?

HW: Well, I don't know if I could call it – it certainly was none of the extremes. It was somewhere in the middle. I felt – I've thought about it since – I've felt that dropping the bomb was probably a necessity, as barbaric as it was. I think it was a terrible sacrifice of the people who were bombed. I think, however, that as a result, fewer people died than would have been killed if it had not been used. So I think in a sense, if one looks at it purely from the point of view of minimizing the number of people killed, it was ok. But, you know, that's a terrible way to look at things. But I presume that that is the way the military looks at things. And I think if I were a general controlling things, I would be forced to look at it that way too. People are going to get killed; how can I arrange for the smallest number of people get killed? And I do think in that sense it was an appropriate move.

But the humanitarians will say, we shouldn't kill people that way, it's too horrible. It is horrible. But perhaps the most devastating is the potentially long-lasting effects of the radioactivity spread around by the explosion. Though I must say, at Hiroshima as far as the bombed area is concerned, there do not seem to be any severe lasting effects. While at an astronomical meeting in Japan, I visited Hiroshima quite a few years after the bomb was dropped and was surprised to find so little effect on the activities of the rebuilt city. There is the reminder of the bomb in the form of the famous monument at ground zero, but people move about in a normal way all through the area that was devastated. I have not looked

into the situation directly and carefully, but I don't *know* of any great problems of residual radiation in the city.

Now, for the other part, those who were killed, and the many who were not killed but are now suffering from cancer and dying horrible, possibly lingering, deaths. They are truly the sacrifices to humanity's insanity. And as I grow older, I feel more and more that war is humankind's fatal insanity. Humans simply seem unable to stop fighting and killing one another for material gain or to gain power or for religion. I believe that the world is far from outgrowing the insanity of war, and that wars will continue to occur for hundreds or thousands of years, and that large numbers of people will continue to die in them. If I accept that premise, I feel that it is no worse to die under an atomic bomb than it is to die in a fire storm started by "ordinary" bombs. You get killed in both cases. In the best situation death comes quickly however it is inflicted, in the worst, one is maimed, perhaps terribly, and death comes after much suffering. Looked at in this detached way, it seems to me that the nuclear bomb and a fire storm started with ordinary bombs are not really different. It simply takes greater effort to start a fire storm, but it will be done if "winning" the war demands it. You may feel that my views are totally off base, out of line with right thinking. After the interview, you can tell me off and explain where I'm wrong.

JS: You mentioned a few minutes ago that your initial appointment with Lick was when you were actually working some at the Radiation Lab.

HW: Yes.

JS: After you had ceased working at the Radiation Lab, did you maintain a residence in Berkeley for a long period after that?

HW: Oh, there were several years, yes. There wasn't any house available for us on Mount Hamilton. And so I remained here, and I did the teaching. I did some of that. But I would then go up to Lick to start observing. So I would spend some time at Lick. And I just stayed in the boarding house at the time until there was – finally, several years – I don't remember exactly the year, it may have been '48, though you'd have to check that; I have no memory for dates. But it must have been around then that we decided to go. There was a house. It was the house that had originally been built for the Jeffers, and then the Neubauers were there. It was a house that I knew well, having stayed with the Neubauers. And it became available when Neubauer retired and left the mountain. So we went up for a summer, and we stayed. We stayed until 1951, that date I do remember, when we moved back to Berkeley. And that was when I became a member of the faculty here.

[Interview 4: September 6, 1991]##

Lick Observatory Staff Member

JS: Yesterday we talked some about your experiences during the war, your work at the Radiation Lab, and how at the end of that you phased out of working there and phased into working with Lick Observatory. You said there was an interval of time when you lived in Berkeley with that position because there was no housing at Mount Hamilton.

HW: Yes.

JS: A few days ago you were commenting about the living environment at Mount Hamilton and some of the difficulties that posed. Were you just as happy to stay in Berkeley with that position, or would you have preferred to be at the mountain?

HW: Well, I think I would have preferred being on the mountain. Cecile and I enjoyed it, and we had a very happy time on Mount Hamilton. It had, of course, been her home, so she knew it well. And in a sense it was somewhat strange, perhaps almost poetic, that we were reliving her family's life – that she grew up as a child, and then returned as a member of the community, a wife of an astronomer. So it had its poetic aspects. But we did enjoy it there very much. We both like the out-of-doors, and enjoyed the living under those conditions.

A potential difficulty, of course, of living on the mountain was school for the children. And there had not been a school there for quite a long time. Cecile, of course, had gone to school at Mount Hamilton, as had all the members of her family, all the children in her family. So when our children were of school age, Cecile was very active in getting the school on Mount Hamilton started again. There were two Mayall children. There was, at that time, Nancy Baustian, who was of school age. I think Ann [Baustian] was too young for her to start. And we had, I guess at the very beginning, two who would have been at school age. The third was, I think he was a little too young at that point. And there was another, a child of, I think it was of the truck driver. You see the difference? I don't know them very well. It's a curse of Mount Hamilton in a way. And so Cecile managed to, by beating on the door of the school department in San Jose, of getting the school underway again. The Mayall children had been going to school in their home. Let's see, it was the Calvert system, which is an in-home school system. The two Mayall children were the oldest ones on the mountain. So they then had public school, and that was an advantage. But we did move down at just about the right time, because the children were switching into junior high school, etcetera, at that point. So it was good that they started when they did; that we did what we did. But, no, in spite of some of the difficulties we certainly enjoyed the mountain very much. I think I would have been very happy to have gone there from the very beginning.

JS: You commented that in some ways it was poetic for your wife to go back as the spouse of an astronomer.

HW: Yes.

JS: Did you feel that when you took that position that you were sort of working in Trumpler's shadow? Were you identified strongly as his son-in-law, or were you accepted on your own terms as an astronomer?

HW: Well, I hope I was accepted on my own terms. I don't think that I lived in his shadow. We did, of course, write the book together after that. But that was just at the very end of my stay at Mount Hamilton, and was at just the time of returning to Berkeley.

JS: When you took the position with Lick Observatory, were you given specific responsibilities at the observatory as part of your job?

HW: No. That really wasn't – there weren't any responsibilities of that kind assigned to people, except for the job of time assignment, etcetera. Nick Mayall did the time assignments, went around each week and asked, "What nights do you want on the telescope?" and so on.

JS: So it was only scheduled a week in advance?

HW: A week in advance, yes. And by half nights. So you could have the first half, or the second half, or all night if you wanted it. But there was never a problem of getting time on the telescope. It was a little tighter on the Crossley during the seasons when the galaxies were up, because Nick Mayall had long exposures and needed all the time he could get. But there certainly was no problem of telescope time in those days. In fact, it might be the reverse almost, that you got too much.

JS: Did people that were not on the Lick staff at that time, or perhaps some sort of fellowship with the observatory, observe as visitors?

HW: No, no. There wasn't anything of that sort. There were a few, I guess you would call them fellows, who came, though they often didn't observe. There was the Morrison Associate. And there were occasional visitors. During summers there were a few visitors who came who would use the telescope. Edward Fath, who had been at Carlton College, came and used the twelve-inch telescope for photometry. Joel Stebbins became really a kind of associate of the observatory after he retired from Madison [the University of Wisconsin]. He really was effectively a staff member, and he and Gerry Kron worked together, and they used the telescopes. But there weren't visitors who would come, for example, from University X, somebody would ask for time and come. That didn't happen. It was essentially an in-house affair, with the staff members of the observatory doing all the observing. I think that was a long tradition of the place.

JS: While you were at the Lick Observatory, did you teach classes at Berkeley?

HW: When we lived there?

JS: Well, either.

HW: Yes, as I mentioned, I taught some classes here at Berkeley. I taught classes here at Berkeley during those years before we moved to Mount Hamilton. That reduced the amount of observing that I did at Mount Hamilton, because I was – you know, it would be weekends and odd times. And in summer I was freer. But I did – no, I taught the astrophysics classes for several years. I think it was about three, perhaps; I guess three years. And I wrote, most of the time I had to myself. I wrote that long article on photometry. I remember spending a lot of time in the library on that one. I think the only class I taught was the 117. I don't remember doing any others.

JS: In that period when you were commuting to Mount Hamilton to observe, in that era, what was the preferred means of travel? Did you drive the whole way?

HW: Oh, no. I would sometimes go by bus to San Jose, and go then with Mr. Roper, who drove the stage, had the Mount Hamilton Stage.

JS: That was a daily shuttle up and down the mountain?

HW: It was a daily affair, yes. Yes, that was a time when cars were hard to come by. We did not own a car. It was almost impossible to get a car after the war. For a long time after the war, you had to be on a waiting list for literally a year or years. Furthermore, they cost a lot of money, though they were very much less than they are now. But we didn't have a car until the year we went to Mount Hamilton, and then we absolutely had to get a car. We tried very hard – I still remember that story – we tried very hard to get one. Finally, some time after we moved to the mountain, we did buy a quite good used car. And so that was our first car. We then could – we were free to come and go from Mount Hamilton. But you see, at the very first, even with the family up there, we didn't have a car, and that made life quite a bit less mobile than it is at the present time.

JS: When you were a member of the Lick Observatory staff, did you maintain any connection to Mount Wilson through visits or observing?

HW: Yes. For a period of time, I observed at Mount Wilson, even though I was at Mount Hamilton. I did some work on clusters, and took a lot of plates for variable stars. But not much came of that finally. It was a program that would have required a great deal more time. Then when I came back to Berkeley, it was a different situation. I just didn't do as much observing any more.

JS: Was that sort of work at different institutions, was that agreeable to the staff at Lick Observatory?

HW: Oh, I think so.

JS: Or did they prefer that you should be using their facilities?

HW: I guess I don't have an opinion on that. I never thought of it that way, that it might make people angry. It was doing something that couldn't have been done otherwise; that I couldn't have done otherwise. As I remember, I was not alone. I think Gerry Kron also did some work with the telescopes at Mount Wilson at about that same time. I seem to remember that.

Lick Observatory Staff and Students

JS: Had the staff at Lick Observatory changed significantly from prior to the war, to when you returned as a staff member?

HW: No, not very much. There certainly had been changes. Prior to the war, I guess I named off the staff, starting with Wright and Moore and Jeffers, and going on down the list. I'm not sure I mentioned Jeffers earlier. After the war, Wright and Moore had retired. And Wyse had been killed. There were changes in that Shane had become Director. Stebbins was there at the time. Olin Eggen was there. And that was really about the change. Otherwise, there was still, of the older people, there was still Neubauer, and Jeffers, and G. F. Paddock, and Mayall, and Gerry Kron was there. He had been there actually before the war. Stebbins was a kind of member of the staff, though he was really a visitor, a very distinguished and honored member there. He worked, I believe, under some kind of a grant.

JS: Is it correct, he had effectively, supposedly retired at that time, and resided in San Jose?

HW: Yes. He had retired from Madison, and lived in San Jose, and came up with Mr. Roper and stayed several days, and then went down. And Olin Eggen came at that time. He came from Madison. He was one of Stebbin's students. And I came.

JS: Another name I believe I've read of in that period was Carl Wirtanen.

HW: Yes. Wirt was a very senior assistant. He worked with Shane on the twenty-inch telescope. They shared the observing duties at the twenty-inch. I had always felt that Shane probably took that on because they had to do something with the telescope, and everybody else was using something else. And so I think he took it on. It certainly had not been his field, at all. He did a very good job, I think, of working out ways of correcting the lens, seeing to the centering, and all the technical details of it. And then he planned the program, and he and Wirtanen carried it on. So Wirt was an assistant. Another person who was there at the time as an assistant was Stan Vasilevskis. Now, Vasilevskis came as a refugee from

Latvia, a very interesting case. He had been a professor at Riga. When he came, he was told he could not be a member of the staff. But the work he did was so good, that he soon became a member of the staff. He did lots of position work and so on. Finally he became the expert on the twenty-inch. But at first he was an assistant doing all kinds of odd jobs for the astronomical staff.

JS: I think I read an account somewhere that your wife's mother played some role in arranging for Vasilevskis to get to the U.S., is that correct?

HW: Yes, well, several of us did actually. It was through the Unitarian Service Committee. And I guess the Trumplers were his official sponsors.

JS: Were they acquainted with him prior to the war?

HW: No. No, not at all. The Service Committee had the policy of trying to bring over refugees who had distinguished careers and had done things in their own country. I don't know how many refugees were brought over by the committee, but it was a fair number, I think. I believe that was one of the more spectacularly successful ones, but probably there were many. He was a wonderful person, Stan Vasilevskis.

JS: Was Aden Meinel associated with the observatory in that interval?

HW: No, he was a graduate student, and he was a graduate student here. He was one of my early students. He did his observing at Lick. Aden was always very much involved with instrumentation; that's what he really loves. And he did, for his thesis, a design of a fast spectrograph, which would be used for observing the aurora. And we had agreed that if he didn't observe any aurora, the instrument and its development would constitute his thesis. If not, if it all came out all right, then the work on the aurora would do it. And he was very fortunate in that there were some very bright displays that were visible even at Mount Hamilton. None of the curtain-like things, but just a brightening of the sky, and so on. And that's where he discovered the Meinel bands. So it was quite a successful thesis.

JS: Now, those are bands of hydroxyl?

HW: I believe they are – I don't remember exactly, yes, but hydroxyl. And I think he also got, didn't he, some velocities of the motion of the particles?

JS: That may be, I'm not particularly familiar.

HW: That was a long time ago. I don't remember all those details.

JS: I believe another one of your early students was George Herbig, is that right?

HW: Yes. Not that George ever needed anyone to guide him. He was an extraordinary person. He had done a lot of work during the war years. You were asking if George had been at

the Radiation Laboratory. I believe a more complete answer is – I gave the answer no, and I believe the appropriate further remark would have been, he was at the Lick Observatory. He was not involved in the war, I guess because of his spinal problem. And he kept the observatory running to a very large extent. He was an inveterate observer, and he observed all kinds of things. Did a lot of work there. Had had a great friendship with Alfred Joy at Mount Wilson. Joy had really started him out in the T Tauri variables, and the sorts of things that George became famous for. But it was a pleasure to work with him. He had a great deal of his thesis done before coming to Berkeley, because he had had free rein of the telescopes, and he used his time very effectively and very efficiently there. Wonderful student in many ways. So it was a joy to work with him. It was mostly just encouraging him.

JS: Did he join the staff of the observatory upon completion of his thesis?

HW: Yes. I urged him not to come immediately. He was appointed to the staff immediately upon his graduation. But I felt that he ought to have a broader view of the world. I certainly had found it useful, and I thought that he should have that same experience. And so, he did leave for a year, and then returned to Mount Hamilton.

JS: What was the principal research that you pursued when you were on the staff at Lick Observatory?

HW: I mentioned that the other day, that I think there were several things that I did or tried to do. I worked on clusters, and I had wanted to continue that project that I'd started before I went into war work. And that did occupy me for quite a while. I made a lot of observations with the thirty-six-inch [refractor] on spectra of stars in clusters, the purpose being to observe their spectra and to get accurate H-R diagrams for them; to determine what the turnoff was, and whether or not there were peculiarities in any of the stars, particularly in the upper parts of the main sequence. So, it was spectra, and then photoelectric photometry on the clusters. And I did publish a few papers on the clusters, but not very many. It was really interrupted by return to Berkeley.

Photoelectric Photometry at Lick

JS: On the topic of photoelectric photometry, that period, I believe, was an important time for the development of the techniques of photoelectric photometry.

HW: It was, yes.

JS: And particularly by people at Lick Observatory. Could you comment some on who was working on that and what their contributions were?

HW: Well, there were several people working. Gerry Kron was sort of the lead. He had been involved particularly in the construction of amplifiers. That was certainly what he liked to do. Olin Eggen was there, doing the enormous amount of work that Olin always does. He always does things wholesale, you know, not one star but thousand stars! And he was just a fanatic when it came to using the telescope. He worked all night, every night, at the telescope. He did reduce his data, of course, but that was also done at night. He was definitely a night person. He did not build amplifiers, or was not involved in the electronics. That was Gerry Kron, and usually an assistant that Gerry would have working in the field of electronics. Stebbins was there. And Stebbins and Kron did projects together on a variety of things, including multicolor observations of certain standard stars, and so on. Gerry had done his thesis on an eclipsing variable star, and he did continue to pursue the topic of various eclipsing stars, and so on. That had been another of Art Wyse's fields. It was Art who worked out the mathematics of eclipsing binary orbits, particularly ways in which one would get the limb darkening. That was the great feature of Gerry Kron's thesis, that he had, in fact, detected limb darkening in a star during the eclipse phases.

Another person who was there at the time, but not on the staff, was Harold Johnson. Harold Johnson came as an assistant. He worked in photoelectric photometry and built equipment. He was a multifaceted guy, in that he was a splendid designer of electronics, and also a pretty good astronomer. And he did lots of work. There developed two sorts of views of the world. Kron and Eggen, who had PV as the color system, and then Johnson (later Morgan and Johnson), who had UBV as the color system. And there was some competition for that for a number of years, but finally UBV won out as the better system. And then it developed into all the other letters that come: R, I, J, K, L, M, N – all the other different bands started by Harold Johnson – I think one has to give him credit – and pursued by him very vigorously.

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JS: You mentioned Olin Eggen as being one participant in the business of photoelectric photometry. And I guess it was after he'd been at Lick Observatory and published some of his observations that some of his work became rather controversial.

HW: Yes.

JS: Was it controversial from the start, or was that something that took time for people to perceive?

HW: No, I think it was not controversial from the start. It was considered rather remarkable from the start. Well, I better be careful. I guess that some people considered it controversial from the start. But it was not a widespread view, the controversial nature of it was not a widespread view. It seemed quite remarkable for the time. Eggen said that there were two quite different types of clusters: the Pleiades type or the Hyades type. Each type of

cluster was defined by several very narrow lines or bands in the upper part of the color, magnitude array. These locations of these lines – the blue dwarf sequence, the bright blue dwarf sequence, and so forth – were distinctly different in the two types of clusters. You have probably read about these sequences, or looked at them.

JS: A bit; I'm not familiar in detail.

HW: The remarkable feature of the whole thing was that the stars in all clusters followed these lines with extreme precision. And they were either one or the other, but essentially nothing in between. If there were things in between, that would be attributed to binary stars, so that you were looking at a combination of two, not a single star. Yes, it did become quite controversial.

JS: Did that lead to disagreements among staff members at Lick, when some of Eggen's results began to be called into question?

HW: Yes, it did lead to some problems. And certainly, by the time Harold Johnson was observing some of the same clusters. He and Morgan worked together. Harold Johnson was first at the Lowell Observatory – I hope I get it straight – yes, first at Lowell, because I remember I helped get him that job, and then on to Yerkes where he worked with Morgan. And they produced quite a number of cluster studies – I would have to look up the exact number. He/they produced magnitude-color diagrams for some of the clusters Eggen had worked on, and they didn't agree with Eggen. There were not just Pleiades or Hyades types of clusters; the clusters differed from one another; their stars did not fall on a few specific, sharp, narrow sequences. There were many discussions and arguments about it. Finally Eggen's hypothesis fell apart. We know now, of course, that the shape and locations of cluster sequences are primarily a function of the age of the cluster; they are not quantized.

JS: Eggen was a member of the staff and not just a fellow, is that correct?

HW: Yes, oh yes. He was an Assistant Astronomer, which I was also – Assistant Astronomer, which was the same as Assistant Professor here.

JS: He left after a relatively short interval at the observatory, to take a position with, I believe, the Royal Greenwich Observatory, is that correct?

HW: Yes.

JS: Do you know at all the reasons he chose to leave?

HW: No, no. You see, that all occurred after I left, so I have no insights at all into that.

JS: The photoelectric photometry measurements that were done, which telescopes were primarily used for that kind of work, at Mount Hamilton?

HW: Olin's work was done largely with the twelve-inch refractor. Gerry's work and the work that Stebbins did was largely with the Crossley. I was trying to think of whether or not Gerry used the thirty-six. He did his thesis with the thirty-six-inch [refractor], but now I don't remember that he used it very much after that. It was the Crossley, or the twelve-inch. I think Olin used the twelve-inch largely because it was so available. It was really his telescope, except for visitors nights. And it was big enough for many of the things that he was doing at that time – observing these enormous numbers of stars for colors and magnitudes. The work that I did was almost all done with the Crossley. I much preferred it as a photoelectric telescope because of the mirror, instead of a lens. It was always worrisome to know what happened to colors when one used a refractor. So the work was divided between two telescopes. There was later a small, photoelectric reflector that was installed. I have no direct knowledge of that, because again, all the work was after I had left the mountain.

JS: Are you referring to the twenty-four-inch that's over by the water tank?

HW: Yes. The Tauchman telescope. At least, that's what it was called then. It may have been a smaller instrument at that time. The original one was built by an amateur astronomer in the Bay Area.

JS: Right. Ok. At Mount Hamilton now, there's a twenty-two-inch telescope referred to as the Tauchman, which is near the schoolhouse.

HW: Yes.

JS: And by the water tanks there's a twenty-four-inch, that I believe was built in the 1960s.

HW: Oh, a twenty-four-inch – in the 1960s, yes.

JS: To digress for a moment, did you know George Tauchman?

HW: Yes. He was a very short, little, bald-headed man, who loved to make telescopes. I didn't know him well. I had met a lot of the amateurs because of going and giving talks for them at the Chabot Observatory – East Bay Astronomical Society.

JS: Were you involved at all with the plans for the 120-inch telescope that were underway in that period?

HW: No, no. Well, I better be a little careful, but not – no, when you say plans – I was involved, George Herbig and I were involved, somewhat. George, finally, much more than I, in trying to get some of the spectrographs on the telescope. At first, the 120-inch was planned without

a coudé spectrograph, because the astronomers who had been at Lick Observatory had never used coudés and didn't see any need for them – they were radial velocity people, and they always wanted to hang the spectrograph on the telescope. And, there was a lot of discussion, and finally there was a coudé focus. I think George should be given most of the credit for that, probably all of it. I did make some studies of the transmission of prisms, and use of gratings, and things like that, and I did publish one paper, maybe two papers. One in the optical journal on the transmittance of a prism[2], I remember that thing. And another one on efficiency of spectrographs.[3] And I had really hoped, and I talked, as I remember now, as strongly as I could, in favor of a coudé spectrograph, but also of a cassegrain spectrograph. Because for some purposes, I felt that they were easy to use, efficient, and could be made very efficient. So to that extent I certainly argued in favor of certain instruments on the telescope. It had largely been settled. All of the main outlines, and so on, had been settled earlier on. So I did not play much of a role in that instrument, if any.

JS: Did construction for the 120-inch begin while you were at the mountain?

HW: Yes, yes. And it was a great experience for the children. Watch this thing be built, and get acquainted with all of the ways a big thing is built. Yes. Yes, it was really wonderful to watch.

JS: That project was directed by Baustian, is that right?

HW: Yes. Bill Baustian.

JS: What was his background? Was he an astronomer?

HW: No, he was an engineer. He was an engineer, and had known Mark Serrurier, who was at Caltech. And that mounting that is there, that way in which the mount is made, you know, with a frame and tubes going up for the 120-inch?

JS: Yes.

HW: That's a Serrurier mount, Serrurier tube. It deflects, but the ends remain parallel this way. And, it was through that connection with Caltech that Bill Baustian came. I do not know exactly how they located, how – I guess Shane – located Bill Baustian, but it was done. Probably Nick Mayall, and Shane, I would have guessed. With perhaps some help from Gerry Kron. But there I'm speculating.

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[2] "The Transmittance of a Prism," *Journal of the Optical Society of America*, vol. 41, p. 331 (1951).

[3] "The Relative Efficiencies of Spectrographs of Moderate Dispersion at the Prime, Cassegrain, and Coudé Foci of Large Reflectors," *Publications of the Astronomical Society of the Pacific*, vol. 60, p. 79 (1948).

JS: The 120-inch telescope is somewhat unusual in that it has a rather long f-ratio, $f/5$, which gives it a rather long appearance, say, compared to the Palomar telescope. Was that intentionally selected, or driven by their choice of using this mirror blank from Palomar, or – do you know why that was?

HW: I'm not really sure of that at all. You know, you ought to talk to Bill Baustian about that one. Though he's fairly old now, he's still as active and as alive as he ever was. He's a very vivacious fellow. And he would be able to tell you. Another one would be, of course, Nick Mayall, who was very much involved with the telescope.

Solar Eclipse Expedition to Brazil

JS: I believe it was while you were at Lick Observatory that you participated in an eclipse expedition, is that correct?

HW: Oh yes, yes, that's right. Yes.

JS: And could you describe a little about that?

HW: Yes. I guess that was a test of the past history of the Lick Observatory. You know, Lick originally did a lot of eclipse work. I guess it was true of most observatories, and Lick was no exception, that the astronomers, even though they weren't specialists in the field of the sun, went on eclipse expeditions to photograph the corona, and do all those things. And there was a question, I think it was a question in Shane's mind, as to whether or not the Lick Observatory should continue in that past type of work. So there came an opportunity (what was it – was it 1947, I guess. Have to look that up, too. You see, my failure with dates.) – an opportunity for the Lick Observatory to participate in the National Geographic / army air corps (because the air force was then a part of the army) eclipse expedition to Brazil. That opportunity arose, I think, because the person who was very much involved with it was C. C. Kiess, a spectroscopist at the Bureau of Standards. He worked with William Meggers on the analysis of spectra. Kiess had been a student at Berkeley. He took his Ph.D. here. He had been at Mount Hamilton, was very fond of it, had great memories of his years, his time at Mount Hamilton. And I think it was the connection of Shane and Kiess that made it possible for the Lick Observatory to see if it wanted to go on and perhaps become the lead organization in future eclipse expeditions.

So I was the low man on the totem pole, and so I got the job of participating. So I went east, and met Kiess, and learned what I could about the equipment. And, finally, there was these spectrographs. They were two large, very large spectrographs, about the size of grand pianos. And stood up on edge. Very large dispersion, high dispersion things, with a long tube for the light to come down. And, I finally – there wasn't too much for me to do on any of the instruments. That was being done in the east, at the Bureau of

Standards. Finally I did leave on the eclipse expedition. I was away about three months. We flew from Washington to Brazil. It was two days to get there. I think, no, three actually. I think we stayed overnight in Florida one time, then went on to the coast of Brazil, and finally across the Amazon to Rio de Janeiro. Then trans-ship to smaller planes to go to a site in Bocaiuva, of Bocaiuva, in Minas Gerais, which is the state in Brazil. And there the air force had already set up camp. I can bring you a *National Geographic* writeup[4] about this if you'd like to see it.

The army had set up quite an extensive camp. There was a large mess hall and meeting room, which was all built for the purpose. And then, lots of tents around. And there was a long line of instruments, different experiments that were in progress. And there, C. C. Kiess and George Van Biesbroeck and I shared one tent. And that was really how I got to know Van Biesbroeck quite well. So we lived together in about as much room as somewhat, maybe, the square of this room – this way, that way, you know [indicating dimensions]. And that was our living quarters for a long time.

So we started and set up all of the instruments. There was a whole series. I think I can remember them all, but I'm not sure. I'd have to look at the picture. It was a line of equipment like this. First, there was a sky spectrometer, a device to measure sky brightness. And that was from the Bureau of Standards, and that was E. O. Hulburt and an assistant of his that was from the bureau. Hulburt became the director later. And he's the person for whom the E. O. Hulburt Space Laboratory is named. I came to like him very much. He was a very interesting and cultured person, who was fun to talk to. He did watercolors. And, he had been a friend of Jeffers. So there were always lots of connections. I think they may have been students together. I'm not sure of that.

Then there was a long-focus telescope; that was Van Biesbroeck's. And that was to measure the Einstein shift. The comparison field was to be done in a different way, in that there was a forty-five-degree flat in front of the lens. So he photographed through it to get the sun, and this way to get his comparison field at ninety degrees. And then that comparison field would be photographed six months later, and get the Einstein shift. Let's see, and then came one of the two big spectrographs, which I operated. And then another of the spectrographs, which Kiess operated. These looked at different parts of the spectrum, these two grand pianos on side. And then – wait a moment. I guess there was another instrument somewhere in here that was just a long-focus-length camera to get some coronal pictures. And then there were some short-focus instruments that were going to be used for just general photography of the eclipse. And this one, in particular, was used for photography of the southern Milky Way at night. So they did make a whole, sort of,

[4] F. B. Colton, "Eclipse Hunting in Brazil's Ranchland," *National Geographic Magazine*, vol. 92, p. 285 (1947).

early and much smaller scale map, or series of photographs of the Milky Way. And that was operated, I guess that was operated by Father Francis Heyden, who became Vatican Astronomer, and was a Catholic project. I think that's the complete line, but I should–.

Anyway, it was a great experience. We spent all this time setting them up. Very crude sorts of setups. The first thing we had to do was establish our position accurately. So Van Biesbroeck and I were the observers and data reduction people, so we would observe stars and determine latitude and longitude by old-fashioned standard methods with a theodolite. It was quite something to keep up with Van Biesbroeck. I would devise new methods of data reduction, and Van Biesbroeck would, with log tables and so on, would compute so fast I just had a terrible time keeping up with him. That had been his life work, and it was not mine. There were, of course, no computing devices, or anything. How we would have loved to have had that. Then, at each day – there was not much, then, to do after we then had the thing once set up – except to aim it each day. So we would carefully aim the spectrograph each day at the time the eclipse was to take place. And then with a marker there on the foot of the instrument – we'd have to swing the instrument – there was a marker, an index, and we would make a pencil line where it was. And the next day, we'd make a pencil line, and the next day, and then we would predict where we had to point to get the eclipse. Precision!

Well, the instruments were designed, those spectrographs were designed, very badly in my view. There was, for example, no dark slide. They were photographic, with huge, big pieces of photographic film. The idea was that you would load it up at night, and then leave the film, and that section would be exposed and useless. Then you'd move the film through. It was a roll of film, just like your camera. Roll through, and expose a fresh piece of film for the eclipse. I don't remember now how you started the exposure, but it was a simple little thing in front of the tube. And then you would roll onto the next piece, and so on. And it was supposed to work automatically, or semi-automatically.

Well, as an indication of the character of science in that time, there was not enough money available, in spite of this enormous expedition, to have regular rolls of film to try out ahead of time. There were substandard, sub-sized film rolls. So, we had practised with the automated part, and so on, in these days. So the night before, we loaded up the two rolls of full-sized film – I mean proper width and so on. And at night, and went and put it in the camera. Covered everything up, put it on the spectrograph, and protected from light as much as we could in all ways. And then, just shortly before eclipse, removed all this stuff and got it ready for the first exposure. And I got one exposure, which was not very good. It was of the chromosphere, and it had beautiful arcs and so on. And the film didn't move after that. Absolutely stuck, because the full-size film for some reason wouldn't go through the slots, the ways, so the darn thing stuck. And Kiess never got a single exposure. Same problem.

On the other hand, one of the instruments next to us started in the automated sequence, and the film wouldn't stop! It just rolled through. Well, it's a sad joke. It was a miserable thing. Very little came out of that whole expedition. Certainly Kiess and I got nothing. I had that film for a long time just as a memento. It wasn't scientifically interesting; it was interesting as a memento of the expedition. I finally threw it away. All the arcs were there, but, you know, it was fogged, and everything else, because we couldn't even take the darned stuff off until the night. Then we had to take it off in the dark and quickly cover it up, and so on. What a business. So everybody had trouble. Well, nearly everybody. Hulburt did very well. He published a very interesting paper on sky brightness. In fact, I used it much later to predict visibility of stars under various conditions. And so there is a paper on the visibility of stars at night, and how faint you could see, and all kinds of things.

And van B. had trouble because there was some warpage of that flat. You know, it had been under sunlight, and then was suddenly in darkness, or at least rather quickly in darkness, and so there was some warpage and he had trouble with that. He tried again at another eclipse in Egypt, and I think he had better luck there. He had learned to protect it in different ways. And I think some of the coronal stuff didn't work because of the film going too fast. And there were some nice pictures from some of the little cameras, but – you know. The main thing that came out of it, I believe, was the photographs of the Milky Way, which had been done during the month before. And the knowledge that the Lick Observatory didn't want to go into eclipses. And so that ended. Certainly, the experience was just too awful to contemplate, doing this again. It would have meant redesigning everything and rebuilding everything. I think the National Geographic would have continued to sponsor and find means, perhaps along with the air force or some other institution that had airplanes and travel potential, to do it. But it's mostly a travel affair. The amateurs now go and love to see this thing, and travel to strange places, and so on.

It's exciting, but it was a large amount of work, expenditure of a good deal of time, for no result. Anyway, I saw the backwoods of Brazil, and I spent time in Rio de Janeiro, and all kinds of things. And as a result of that, I was invited to give, for the National Geographic Society, their annual lecture at the meeting of the American Association for the Advancement of Science. I gave it on the eclipse. And I gave a lecture for them in their annual series of lectures in Constitution Hall in Washington, D.C. And I got to know some nice people from those months in Brazil, and especially – he was then one of the secretaries of the National Geographic Society – Mel Payne, who was the representative of the society on that trip. He stayed with us. He was right with us at the camp. He later became president of the National Geographic Society. Died a few years ago. So those are my eclipse experiences. It was a good eclipse! I saw it, you know, after the first few seconds. There was nothing more to do except stand and watch it, so I did.

[Interview 5: September 9, 1991]##

JS: You said the army helped sponsor the expedition. What was their interest in encouraging it? Do you have a sense of that?

HW: Not very deeply. But I think it was the experience of setting up camps. It was a kind of exercise for them, transporting lots of stuff from the United States to a foreign country, at some distance, and setting up a camp, which they did. I dug out the reprint of the National Geographic article, and then I, as running to the car this morning, I left it right there. I'll bring it for you tomorrow. And it has a picture of the camp and some of the operations. It was the National Geographic, that was the sort of prime sponsor, but much of the transportation, etcetera – *the* transportation, etcetera – was supplied by the military. I think it was just the experience – a field exercise in setting up a large camp in a distance place, and operating it.

JS: You said this was quite a large investment of time, about three months, I believe.

HW: It was about three months, yes.

JS: Was most of that required for setting things up for this experiment you were involved with?

HW: Yes, there was a good deal of that. Yes, we had to assemble the equipment, and put it up, and perform these varous not-quite-adequate tests, and things of that sort.

JS: One further thing you said was that part of the reason you were selected to do this project was because you were the low man on the totem pole at the time.

HW: Well, that was the way I said it. I was the most recent appointment to the staff, and had just begun there.

JS: When you arrived on the staff, was the hierarchy pretty clearly defined at the observatory among the staff?

HW: I never felt it was. Clearly, there were the senior people. I guess there was a hierarchy; I guess there was a hierarchy, yes. It never bothered me. I don't recall any sadnesses as a result of it.

III. Berkeley Astronomy and the Origins of the Radio Astronomy Laboratory

Appointment to Berkeley

JS: I believe it was in 1951 that you were appointed to be on the faculty at Berkeley?

HW: Yes.

JS: Could you say a few words about how that came about?

HW: It came about, I guess, for – well, it's several pieces. One thing, perhaps *the* thing, was that Struve had been appointed to Berkeley and was the new chairman. He had come from Yerkes, and there were staff appointments to be made. There was one retirement, namely Trumpler. And so Struve asked if I would come, would join the Berkeley faculty. Again, it was somewhat the same story as with Cecile's parents. By that time, the children were beginning to run out of school to go to on Mount Hamilton. And so it was an opportune time for us on that basis, to come. So we did come.

JS: Do you know why Otto Struve left Yerkes Observatory, an established center of astrophysics, to come to Berkeley?

HW: I think there were a great many difficulties, staff difficulties, there at the time, with some variety of disagreements among the staff members. I never tried to pursue exactly what they all were, but there were a number of departures at that time, in that general era. Chandra [Chandrasekhar] went to Chicago. He had been living previously at Williams Bay. Gerard Kuiper went to Arizona. And Struve came here. That was all at roughly that same era. So there must have been a number of disagreements among the staff. And of course, the Burbidges [Margaret and Geoffrey] were there at the time, and they left. That wasn't all instantaneously, I mean, it was over a period around that era.

JS: Right. Now did John Phillips also make that move, from Yerkes?

HW: Yes, he did. He came with Struve. And Margaret Phillips had been working for Struve as a secretary, and she, of course, came as Mrs. Phillips. And there was another Yerkes person at the time, and that was Lillian Ness, who became the department secretary here, and she had been the observatory secretary there. She had worked very closely with Struve for a good many years. So it was a kind of University of Chicago group here. Struve, Lillian Ness, John Phillips, and of course, Louie Henyey was here.

JS: Henyey had arrived a few years earlier.

HW: A few years earlier. So it was very much a Chicago group.

JS: From that description, it sounds like the department had changed significantly from when you had been here as a graduate student.

HW: Oh, very definitely. It was the beginning of the current department.

JS: I guess by that time Leland Cunningham was also a member of the department?

HW: Yes. He was a member of the department. He was the one tie to the distant past.

JS: In terms of his research.

HW: In terms of his research, and the sort of thing in which he was interested.

JS: Which was orbital calculations?

HW: Orbital work, yes. Comets.

JS: In terms of your own research, how did that change when you came to Berkeley?

HW: It remained the same for quite a period of time. But then it began to switch more and more into Galactic things per se, rather than clusters, which I had looked upon primarily as astrophysical objects. So I began to do – though at first when I came here, there were several small astrophysical sorts of things that related to spectral lines, and dilution effects in atmospheres, and things of that sort. And I guess there were still a few papers relating to clusters that came that I had started at Lick, and that were published after I came back to Berkeley.

JS: Were there particular reasons you were drawn into working more on Galactic topics?

HW: Yes. It was the advent of radio astronomy, I think, that did it. I particularly remember the IAU [International Astronomical Union] meeting in – it must have been Rome – in '52. And that was when radio astronomy had – when the hydrogen line had just been discovered, when the first real indications of spiral structure had been uncovered, and when the group from Leiden presented the first picture of spiral structure. And I remember, I still remember, returning by boat, by ship – airplanes were still not so commonly available – and thinking very much about it, and planning a whole series of tests of spiral structure using stars and other ways of doing it. And when I got back, I very shortly thereafter started the study of the distribution of B stars. It was more a distribution than plotting out specific entities that indicated spiral arms, but finding the same patterns in velocity. My effort was to find the same patterns in velocity that were found in neutral hydrogen. And lo and behold, they are there. And so, spiral arms are also indicated by the B stars treated statistically, rather than individually. And that started me off, then, on a whole series of different things. But it was testing various models to determine the distribution of B stars. And there were other things that related to breakup of clusters, and a variety of problems that were really, I think, more Galactic and statistical than they were astrophysical as such. So that started me off in that direction again, which had been the first field in which I had worked.

JS: It was approximately in that time when you published your book with Trumpler on statistical astronomy.

HW: Yes. And that was an impetus in that direction.

JS: How did that project get underway?

HW: Trumpler had been wanting to write a book in statistical astronomy. It was based on the course that he gave, though as it developed when we wrote it, it was a good deal more extensive than the course that he had given, so there was a lot of material that we worked up for that. As I say, he had wanted to publish a book in that field for some time, and he proposed that we do it together. So we did. We divided up the chapters, just doing alternate ones. And then each had a chance to go over and to criticize and suggest changes in the other's work. It developed quite a bit as we wrote it. We had wanted to make some changes still. I think we should have done some more work on a few of the things in it. But the fear was, at least it was Trumpler's fear, and I think he was probably right, that we would never get the equations all numbered correctly if we started making modifications somewhere in the middle of it. That we'd never get the references really fully correct. So we didn't make a revision that perhaps would have helped. But it was a useful book, I think. It served its purpose and put a lot of material together.

JS: How long in total did it take you to finish that project?

HW: It was about a year. Actually, I worked on it not here, but at Lick. It was really completed before I left Lick. It was published while I was here. So we did the correcting of the proofs, and so on, after I left Lick. And there too, we did it jointly. Each of us read the proofs and tried to catch all the errors. There are still a couple in it, I'm sorry to say. One in particular is the limits of an integral; it always annoys me when I see it.

JS: After Trumpler retired from the department, did he remain active in research?

HW: Yes, he did. He wanted to finish up his big project in radial velocities. Mostly he had measured the plates, though he still did a little bit of measuring. He would come to Berkeley and work, stay for a while. His task was mostly to test for internal consistency in the system of radial velocities on which he had wanted to publish. He did work up until the last day of his life, almost. He still had things in his briefcase when he went off to the hospital and never came out of it. Unfortunately he never saw that through.

After Trumpler died, several students who were here, Mort Roberts among them, and I worked on the thing, and put it pretty much in final form. Actually I submitted it for publication as a Lick volume, or as a publication. It was a thick manuscript, and

I remember once using it to participate in an international colloquium on radial velocities, where I provided all the information on it.[1] But one of the referees for the darn thing wanted to have all of the binary stars done too, and I had always thought the binary stars that are in the clusters would be a separate publication. And that really stopped it, and it never did get published. I feel guilty that I did not push harder for the publication. I did supply a few manuscript copies to people who were interested. But it's still very much on my conscience that that did not get published. It might still have some value. Of course, you can do a cluster now in a night if you really work at it. Whereas then it was really a tremendous job to do it. To do almost a hundred clusters was really years and years and years of work. He also was doing magnitudes and colors, and he did a little bit of measuring on photometric work after he retired. But it was not much.

JS: You said that he would come to Berkeley to work in that time. Did he no longer reside in Berkeley at that time?

HW: No, he did not reside in Berkeley. The Trumplers moved to Rio del Mar, where they'd had a house on the beach for quite a few years. Ever since – I guess they got it in '42. It was just at the time of the war. Trumpler had had an accident at Lick Observatory with the thirty-six-inch [refractor]. In reversing the telescope – have you ever seen it or done it?

JS: I haven't seen it; I'm familiar a little with what happens.

HW: He didn't get it moving in quite the right direction, and it had an enormous momentum, and it was rotating around the polar axis. Now, when it's doing that, you have to have it way out in declination at the same time, or you have no leverage if you're doing it from the floor. The safe way is to do it from top with the wheels – with the, looks like boat-steering wheels. So he was underneath the telescope trying to clamp it, so to get it to stop it, to put on the brakes. The telescope struck him on the knee, and drove his heel right into the floor. So his bones were the brake that finally stopped the telescope; it crushed part of his foot. He did have a limp, a slight limp, for the rest of his life. But the doctor had recommended that he go for walks on the beach on sand, so in bare feet to walk on sandy soil, sandy stuff. And that would, the doctor said, help him regain the use of his foot. So they were quite often, then, at the beach, and Trumpler would go for long walks on the beach. One of the times, one of the houses that they had been renting came up for sale at a very good price, because the person who owned it was afraid that the Japanese were about to invade that beach. And so they bought it for a very good price, and then they were there quite frequently to try to improve Trumpler's foot. They finally retired there. It was also a wonderful place for a

[1] "Robert J. Trumpler's Work on the Radial Velocities of Galactic Star Clusters," in IAU Symposium No. 30, *Determination of Radial Velocities and Their Applications*, ed. A. H. Batten and J. F. Heard, Academic Press (1967), p. 153.

garden, which was his second great love. And they moved there as soon as he retired. So they would come periodically and stay with us, and Trumpler would work at the observatory.

JS: Did you collaborate with other faculty at Berkeley when you arrived on the staff here?

HW: No. There really wasn't much collaboration. Everyone sort of worked by himself. I was trying to think; the two who did work together, but then they were in a different relationship, were Struve and Su-Shu Huang, who was not a member of the faculty, but was sort of hired as Struve's collaborator. If you wish, a kind of super-postdoc, in a sense. Su-Shu was a very gentle and wonderful person, and very bright. Quite often did things by himself, but very often he was Struve's theorist and helped Struve in the interpretation of data.

Transition in Research, and Origins of Berkeley Radio Astronomy

JS: You left Lick Observatory a number of years before the 120-inch telescope was completed. Did you ever end up working with that telescope at all?

HW: Never in my life, no. I'm very sorry to say I never used it. I would have enjoyed very much doing so. But I've never used it, and I've not even seen it in use. It's a telescope in a different world, as far as I'm concerned. Because, for one thing, I had changed to radio astronomy, and so I wasn't using optical telescopes anymore. I've sometimes thought I would still like to go back and do a few things. I want very much to get the distance to a cloud of hydrogen that has some dust in it. And I keep thinking, I've got to go back and get some spectral types for a hundred stars or so, and do the job, but I haven't done it.

JS: Well, since we've got onto the topic of radio astronomy, you mentioned earlier how you became interested in Galactic topics in response to what was being learned from radio astronomy. How did it come about that you yourself actually became an active participant in working in radio astronomy?

HW: Well, soon after I came – and again, my bad memory for dates, I'd have to look up somehow the exact time – Ron Bracewell was invited to come to Berkeley as a visiting professor. He gave a course in radio astronomy. He came from Australia, where he and Joe Pawsey had just finished and published a book on radio astronomy that they did together[2]. Clearly, radio astronomy was becoming a very important topic in the field, so it was important for Berkeley to have a course in it. It became clear that we really should have a permanent presence in the field of radio astronomy.

Under Struve's pushing, the Dean of the College appointed a committee to investigate whether or not Berkeley should go into the field of radio astronomy. There were three

[2] *Radio Astronomy*, by J. L. Pawsey and R. N. Bracewell, Oxford Clarendon Press, 1955.

members of that committee. I was one, and was the chairman of the committee. Sam Silver, who was in electrical engineering and interested in that field, was another member. And Louie Alvarez was the third member of the committee. So we had a number of meetings, thought carefully about the project, looked into what was being published, sort of surveyed what was going on, and concluded, yes, absolutely we ought to be into the field of radio astronomy here on the Berkeley campus. In fact, I think there's a piece of the original report here. I gave it to – I was trying to find things for Radio Astronomy [the Radio Astronomy Lab] a while ago. And this turned up. I don't know where the other pages are; I think I gave them to Radio Astronomy. The report dealt with, first, the question whether we should be in it, and then what field. And in fact, we decided – here, in fact, I'll just read you a section of the report.

"In answer to the second question, the committee recommends that in the field of radio astronomy, the university should concentrate on the study of the sun. Solar work is not now strongly represented in the Department of Astronomy. Specialization in this field would accomplish two goals." The idea was that it would bring us into solar astronomy, which has not really been represented ever in this department. It's the one great missing piece of it. And the other thing is, it wouldn't be so expensive to go into solar radio astronomy, because there are many ways to make those observations.

Well, that report was accepted, and the university agreed that it should go into the field; the dean's office did agree. And Ron Bracewell was going to be invited to remain in Berkeley and develop the field. Well, it never came about somehow. Into radio astronomy, yes, but it never worked out with Ron Bracewell. So it came around to finding someone else, and finally they asked if I would take it over. Well, I wasn't very anxious to do that. And finally I said yes, I would, if we could switch the field to Galactic astronomy. And there were two important reasons for that. One is that that was a field in which I was interested. And at just that time, Bart Bok was leaving Harvard. Now, Harvard had been the leading twenty-one centimeter and Galactic radio astronomy source in the United States. Radio astronomy was not very strong in the United States. In fact, it was very, very weak in general. That was just one reason why we should go into it here at Berkeley, to try to assist in bringing it up to speed in the country. And Harvard had been the important source of Galactic studies under Bart Bok. But Bart was leaving for Australia to become the director of the national observatory there, at Canberra, at Mount Stromlo. And so I felt that someone in the field of Galactic studies should be in radio astronomy again. Harvard, I didn't know what would – nobody knew what would happen at Harvard. So it was an opportunity for us.

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HW: Well, so the arrangement was that we would start Galactic radio astronomy here. We would concentrate, at least at the start, in the twenty-one centimeter range and look at neutral hydrogen. The university was pretty generous. Clark Kerr, who was then the chancellor

of the Berkeley campus, but about to become the president of the university, was very supportive. He felt strongly that we should go into the field, and agreed that there would be a certain amount of money – I don't remember exactly, something like a hundred thousand dollars for things to start with. That was a lot of money in those days. And the university would supply – I don't know – seventy-five thousand, or something of that order, a year for maintenance and running the place, which was to be considered half of the original operating budget. The point was, we had to go out and raise money for equipment and for operations, but the university would certainly provide us with money for building houses, doing that sort of thing, and would match the operating budget supplied by any outside organization.

So one thing was that I took a sabbatical and went to Harvard, and worked with the equipment there. That was my first introduction into radio astronomy. So I got to know the people who were there at the time in the field. The students there were – well, there were quite a few of them, but ones you might know would be Dave Heeschen, and Frank Drake, who were there. And there were quite a few others, but their names are perhaps not so well known. Anyway, I learned some radio astronomy. I divided my time between Harvard and DTM [Department of Terrestrial Magnetism, Carnegie Institution of Washington]. Mostly at Harvard, but also at DTM in Washington, D.C., where Bernie Burke was on the staff. So that was my first acquaintance with Bernie Burke.

So it was a question of learning some radio astronomy, and finding funding for the place. So a lot of my time was spent writing grant proposals, and visiting various foundations and organizations. I tried very hard with NSF [National Science Foundation]. They were not very helpful at the time. And the Office of Naval Research. And they were sympathetic, but they didn't have any large sums of money. After all, they had lots of money out in somewhat similar projects, and they were a little bit more oriented in that it should have some relationship to their mission in life. Well, there were lots of nibbles, but no very hard bites, until Sputnik went up, and then money became much freer.

We were instantly funded, then, by the Office of Naval Research, who put up the money for the telescope, and in fact, with whom we had a wonderful relationship. First of all, there was no large report writing. I used to get the budget for the year from a four-page letter, which said we want to work on the following general problem, which I then described. And after they funded us the first bit for, I don't know, a couple of hundred thousand dollars to start the telescope, they said in a few months, "Couldn't you use some more money?" So the money flowed in the beginning quite easily. Until they began to have troubles much later, the Office of Naval Research was really very generous and a very wonderful place to work with. They were very good to us. Supplied not only money, but they supplied some of the equipment. For example, the crane out there was originally navy. And the bulldozer, and jeeps, and cars, and trucks, and all sorts of things like that, came directly from the navy. And that was a great help, because there was a lot of building to do.

Well, once we got funded, the thing to do was to start. And I think we had already been doing some of the work, but we had to locate a site. And that required the better part of a year. In order to locate that site, I would study maps. I was looking for a place that was remote – as close as possible, but nevertheless remote; protected to the greatest extent possible by mountains for direct line-of-sight radio interference; as large a flat area as possible for – though we didn't plan it then – for future possible expansions through interferometry; and free of overflights from airplanes; etcetera, etcetera. This was to find the place that was most suitable for a quiet radio site: a radio-dark sky. Well, that required a good deal of investigation. And I used to think, at least for a while I thought, that I must know the backroads of northern California better than anyone else, because I've driven almost every one of them at some time or another, looking at sites that showed some promise with those particular criteria in mind.

Well, if a place looked sufficiently good to warrent a real investigation, then two or three of the fellows – we had then a small staff – would set up equipment, and go and monitor radio signals over as wide a band as we could then have available, but certainly concentrated in the twenty-one centimeter range, and looking at as much of the spectrum as we could, for at least a twenty-four hour period and preferably more. And if a place looked reasonably good, showing radio quiet, we would go back and look again. Well, the search narrowed down to two or three places. There was one that really looked very good; that was in the Madeline Plains. That's farther north and east of Hat Creek. And Hat Creek. Well, it turned out that Madeline Plains would have had lots of, miles of, flat area. But it would have been very difficult. It was also very bad land on which to build. It was the bottom of an old lake, and had very little foundation. At Hat Creek we had less area for interferometry, but the foundation was good, and it was also very quiet. In fact, it turned out originally to have more radio quiet than Madeline Plains. So everybody was pleased we stayed away from Madeline Plains. It would have been a much more difficult place than Hat Creek.

So, the first thing were all the negotiations for the land from C. M. Bidwell, who owns part of the land, and is one of the local ranchers, and the Forest Service. And I must say it was interesting because, as it turned out, the university didn't do much of it. I finally wrote all the contracts. The reason was that the university moved very slowly. They also originally wanted to condemn and buy the land. That would have created an atmosphere of great hostility toward us in the community. The farmers of the area were already a bit suspicious of the installation. They were afraid of the telescope and the electronics; there were many rumors. They were afraid that the equipment would somehow cause the cattle in the area to become sterile. But we just had to get the operation under way. I gave as many talks to local groups as I could, explaining the nature of the future observatory and trying

to assure them that there would be no danger from it. And I just went ahead and started negotiations for the land. The real estate people in the university administration were very unhappy about that and raised a lot of objections but finally the university administration and the regents approved the arrangements. And so at long last we started to build roads and houses and everything all at once. It was very interesting. I remember that I drew the plans for the dormitory – not the ones they built by, but, you know, the schematics and so on. And I think the observatory turned out to be really quite nice. I often thought of Mount Hamilton and things to avoid at Hat Creek.

JS: So the contract that you drew up was for a lease on the land?

HW: Yes.

JS: And what was the period of that lease?

HW: My recollection now is that it was for twenty years. I never renegotiated it, but Jack [Welch] had to renegotiate it. It was already stipulated the maximum that they could increase the rental fee, and so on. It was tied to the cost-of-living index, various things of this sort, so that they couldn't rob us. Because, quite frankly, what I was afraid of, that it wasn't so awfully expensive to rent, but if the observatory were successful, and we had lots of equipment on it, I was very much afraid that we could be held up at a future negotiation. And so that's why I was anxious to specify somehow a maximum amount, which would be the same amount that we were paying, increased by the cost of living change between the two periods.

Instrumentation at Hat Creek Observatory

JS: What was the initial complement of observing equipment that you set out to build there?

HW: There were two telescopes. A thirty-three foot telescope – I'll describe first, a little bit, the observatory area. Come in off the road, and then there's an area where there's a shop, and the housing, and so on. And then you go down – the idea was to keep all the housing, etcetera, at least a little ways away from the telescopes – go down to a meadow, which is a kind of natural flat area that must have been a lake at one time, in amongst the lava rock. So there was real foundation to build on! And then it extended out – that area, the meadow – extended out into Bidwell's fields, and so we could extend out into that way. There's a much smaller baseline north and south on the same flat land. It's mostly east-west. Though the north-south baseline is not bad.

In the meadow then, in this area, there were originally planned two telescopes: a thirty-three foot telescope and its laboratory building, and an eighty-five foot telescope and its laboratory building. The thirty-three was the first one built, though we had already signed the contract for the eighty-five. The eighty-five was designed and built by Philco. The thirty-three was designed and built by us. There was a small engineering staff here.

Temple Larrabee, who had been at Caltech, was the main engineer we had here. And we had draftsmen, and people who could build things, a shop and so on. The plan was to learn – we were going to do part of the eighty-five ourselves, the controls, for example – we were going to learn on the thirty-three. So the thirty-three was completely designed and built here. The dish and the gimbal on which it was mounted were not made here. They were built elsewhere by Philco, but brought in and put on the mount, which we built. And we built all the controls. I've often thought it was a wonderful thing that we did, because not a single aspect of the controls on the thirty-three foot were built onto the eighty-five foot. They were entirely different. We learned a lot.

The experience was really very telling on the thirty-three foot telescope. It was a useful instrument, and it did one Ph.D. thesis. And it taught us an awful lot about telescopes. We also tried out the receivers there. It was the first telescope in operation, and was there as a learning experience instrument. The eighty-five foot came along later. We built the controls for the eighty-five foot, and finally built a variety of receivers. The first receivers were built for us by Doc Ewen and his company, Ewen-Knight.

Observations with the thirty-three foot were made at wavelengths of 21 cm and 22 cm with equipment built in the Lab. Observations at 22 cm were made to examine the distribution of ionized hydrogen in the galaxy. But, at the start, the thirty-three foot was mainly used for engineering studies.

When the eighty-five foot antenna became fully operational, we had a variety of receivers available for several wavelengths: 3.75 cm, 10 cm, and of course, 21 cm. The 10 cm receiver and the 21 cm double comparison, single channel receiver had been made by Ewen-Knight. We observed at all of those wavelengths. We used the 3.75 cm receiver to make observations of the Galactic center and various radio sources, in part to test the efficiency of the antenna at short wavelengths. The antenna was useable at that wavelength, but was not very efficient.

During the first months after the eighty-five foot antenna was put into operation, a great deal of time was devoted to engineering tests. The first real "just for science" observing program made use of the single channel scanner for studies of 21 cm absorption lines. That was a particular interest of David Williams. We soon learned that the single channel scanner was terribly slow – an observation required a couple of hours as I recall – and that we would have to look to new methods of observing if we were to make enough observations to be useful. That is when we started thinking of a multichannel device. (We built and put a 100 channel receiver into operation early in 1965. Three channel bandwidths were available: 30 kHz, 10 kHz, and 2 kHz. The design of the multichannel receiver was primarily the work of David Williams.) A hastily built receiver for OH was used on the thirty-three foot antenna in December, 1963 to confirm the discovery of the OH molecule; a better OH receiver was

later used on the eighty-five foot, and a then state-of-the-art, low-noise OH amplifier was used on the eighty-five foot antenna starting in September, 1964. That, combined with the multichannel receiver, made many OH studies possible. With the multichannel receiver the data were recorded digitally, and could be reduced on a computer. The single channel scanner produced a graph on a strip chart recorder; the graph had to be measured and worked on by hand. It was very slow work.

We did hydrogen work, and hydroxyl work. That launched us into several different fields. Nan Dieter was here as a radio astronomer. She had been at Harvard, did her thesis at Harvard. I tried valiently to get several of the people who had been at Harvard to come onto the faculty, or onto the staff here. But we still weren't quite a big enough draw to get them, and we didn't succeed. Tried very hard to get Dave Heeschen here for a while. But he became – he really was the operational head of the National Radio Observatory then, so he had a lot more freedom of operation than he would have had here.

JS: When you were getting the Radio Lab underway, and in its early years, were other members of the Astronomy Department supportive of this effort?

HW: Yes, oh yes. Very definitely. Louie Henyey, who was the other senior member of the group, was very supportive, and I think John Phillips was supportive, but he was very much in his own field of molecular spectroscopy. He didn't pay much attention to us. While Struve was here, he certainly was supportive, but it all developed really rather after he had left Berkeley.

JS: Since you came from a more traditional astronomical background rather than an electrical engineering background, there were presumably many things in the technical side that you weren't familiar with. Was this a big impediment to progress?

HW: Well, there was lots of learning to do. But working at it, one can learn. Necessity is a great teacher; if you have to learn, you will.

JS: Did you work very closely in the actual development of hardware?

HW: Some aspects of the hardware, yes. Certainly not in the technical electronics development. I left that very much to David Williams. And Tap Lum soon became extraordinarily proficient in it. Tap Lum was a graduate student in astronomy, traditional astronomy. He did lots of celestial mechanics, and orbits, and all that sort of thing. And I think he is largely, perhaps entirely, a self-taught radio engineer. But he's extraordinarily successful at it, and extraordinarily adept. He's a very, very splendid, low-noise amplifier man. He really understands it. And David Williams too. David had been trained in that field, though. He came from Manchester, England, where he did a thesis really in electronics, on the radio telescope. He then went into electronics at the Canadian Marconi company. I had learned

of him through the group in England, at Manchester, and then pursued him in Canada and offered him the job here.

Graduate Students and the Discovery of Interstellar Hydroxyl

JS: You mentioned the increasing involvement of graduate students as the Radio Lab took off, and that several dissertations were done in that time. What was some of that work?

HW: Well, the first one on the thirty-three foot was done by Kimball Hansen. And that was the only thesis on the thirty-three foot, as I remember, because we used it for various tests and things of that sort. The ones that I think were important and that I remember very well are ones by Miller Goss, and by Barry Turner, who went on to be at the National Radio Astronomy Observatory. And Miller, of course, has traveled all over and been everywhere. They grew out of some work that had been done at the eighty-five foot telescope.

At that time, the OH line had just been discovered. In fact, I recall that I was at a meeting in the east, and at a dinner party at the Gart Westerhouts' in Washington. And there was talk after dinner about the OH discovery, the absorption line of OH. And the discussion got around to confirming the thing. So we were all going to try to confirm. And I remember telephoning early the next day, and telling the story of it, and getting people here to build a receiver, or to modify a receiver, that would – I think we did that with the thirty-three foot – that would make it possible to confirm in the direction of the Galactic center. The discovery of the OH absorption line; that was Al Barrett's work. And Al was there at that dinner. And there were several confirmations of it. We all got it about the same time. I don't remember whether we all published in the same issue of *Nature*, or we may have been one issue later.

So it looked as though a good project would be to investigate the presence of OH in the Galaxy, and to try to determine how it was distributed and what its astrophysical nature was – more than we knew at that time. It was a great excitement to have a molecule like that. So we were dividing the telescope time up at that time. Each of us would have a certain period of a couple of weeks, and whatever work was in progress. And I remember – in fact, I got a little bit mad, because one of the staff members didn't want to take the time, so I did; and I think I had a double dose of it that time, maybe a month on the telescope. And I had decided that a good thing to do would be to look at each of Westerhout's sources, which he had done for his thesis, in the twenty-two centimeter continuum radiation, and look for OH absorption against those radio sources. And almost immediately – I should say how it worked. The telescope was – there were observers at the site. And so one didn't have to be present at the site to use the telescope. Had to supply the program, all the settings, and all that sort of thing, and then the observers, who did nothing but use the telescope – they were electronic technicians – would set it, and observe.

Well, after the program on Westerhout's sources started, in which I was looking for OH, I got a terribly worried call from the head electronics guy, the head observer there, at three o'clock in the morning. Something was wrong with the equipment, that there was an absorption line, but there were sharp emission features within it. And he didn't know what to do about this. So I said, continue observing, and let's talk tomorrow – I don't know what to do either, at three o'clock in the morning, being wakened from bed, from sleep! And it turned out that that was the first maser. That was the OH maser. And that was very exciting, and that really exercised everybody here for quite a long time. And it was discussed – we used to all meet together at lunch, at that time; it was very pleasant in many ways. And it was the topic of discussion constantly, of course. Everybody thinking what it was. Nobody came up with the idea of a maser at that point. In fact, knowledge about masers was only a few years old.

Well, the first thing was to discover what type of source it was visible in; it didn't appear in every source. And what were the characteristics. I remember Minkowski was very helpful in suggesting sources, and he suggested some of the good ones that he had seen as HII regions. So that was how OH masers were discovered, and it was really on my observing program. The discovery of the maser stopped the general program of looking at all the sources. So Miller Goss took on, as his thesis, observing the Westerhout sources, and he took over the program, and I went on to work on OH, and Barry Turner went on to interpret it.

[Interview 6: September 10, 1991]##

JS: Yesterday we were talking some about how things got started at Hat Creek and the type of work that was going on. You were describing the work on OH in which you had some graduate students get involved.

HW: Yes. The two who were particularly involved were Miller Goss and Barry Turner, both of whom ended up, or have ended up, at the National Radio Astronomy Observatory.

JS: Was it your initial discovery of the OH maser that resulted in the paper that referred to this as "mysterium"[3]?

HW: Mysterium? That was a kind of a joke, though it perhaps was an interesting name. I gave it that name because I was thinking really of nebulium[4], and the fact that nebulium was

[3] H. Weaver, D. R. W. Williams, N. H. Dieter, and W. T. Lum, *Nature*, vol. 108, p. 29 (1965).

[4] Nebular sources are spectroscopically observed to radiate a pair of strong green emission lines that are generally not detectable in laboratory plasmas. Early researchers in this field suggested that the lines were produced by an element, labeled "nebulium" (M.

not disentangled and identified for a while. And this one, too, was unidentified at the time. That is, mysterium is now known to be a maser, but we certainly didn't know it then. The suspicion was that it was OH; it always occurred at OH. But the nature of the spectral line, with its sharp features, which turned out to be variable, polarized, everything you can imagine, made it deserve the name mysterium for sure. So, it was done just to have a little humor in the paper. People still refer to it as that – mention the name occasionally.

Other Research at Hat Creek Observatory

JS: What other sorts of science did people get involved with at Hat Creek when you were in charge?

HW: Well, there were two large surveys that I think have survived and been useful, right up to the present time. The one was made by Carl Heiles and H. J. Habing, and the other one by David Williams and me. We divided the sky up. Carl had, for his thesis and for some of his earlier work in radio astronomy, had worked in high latitudes. And so he, rather naturally, took the high latitude stuff above ten degrees.

JS: Now, you're referring to Galactic latitudes here?

HW: To Galactic latitude in this case. And I had been interested in the nature of spiral structure in the Galaxy, and so David Williams and I did the plane, the Galactic plane, between plus and minus 10 degrees. Those were two large surveys that have, I think, proved their usefulness, and still provide information to people who want to use them. I use them everyday here, though I ought to get off on something else for a change. Nan Dieter worked on dark clouds in OH, and in some velocity anomalies in the edge of the Galaxy that she had gotten interested in. Carl Heiles had started, I guess, at that time already, his magnetic fields. He was mapping interesting areas of the sky, and worked on magnetic fields.

I think that the output of the telescope was worthwhile. Not that it couldn't have been a lot better, a lot more. But I think it proved itself as a useful instrument that did produce useful results. I remember some of the Australians said, when the OH maser was discovered, "well, you've already amortized half of the telescope, that's worth half of it right there." And then, of course, other lines were discovered, other molecular lines: water and ammonia. Jack Welch and Charlie Townes did those. And so I think that the observatory, even in its beginning, produced some worthwhile things. And now, under Jack Welch, has entered important new fields that make it worthwhile. So I'm glad we started it.

L. Huggins, *Astrophysical Journal*, vol. 8, p. 54, 1898), that did not occur on earth. Bowen (*Astrophysical Journal*, vol. 67, p. 1, 1928) later demonstrated that these lines could be understood as forbidden transitions of doubly ionized oxygen.

JS: At the time you were doing your large surveys, were there other groups elsewhere that were doing similar work?

HW: No, at the time there was no one else doing very large surveys. I think that was, in some ways, a monopoly of ours because we owned the telescope, and we could devote whatever time was required to the project that we had all decided to do. And that was not always possible elsewhere. For example, one could not have done that at a national observatory, because there are too many users clamoring for the telescope. It was a relatively small number of users here, that could command whatever time they really required.

That's an interesting problem in general in modern astronomy at the present time. And I mean not only with respect to large surveys of the sky that take a lot of time, but to an individual project, as for example, the observations of a variable star, or a small number of variable stars, that must be observed intensely and over a long period of time in order to produce the type of observational material that you need to understand what's going on. It's somewhat of a hit-and-run process at the present time, because no one owns a telescope, effectively, that can be used for a long period. So only certain types of projects are available at the present time. Now, there have been some instances where observers have pooled their time to work on a generally similar problem, or generally similar group of objects: group A using one aspect of the plates, and group B using another aspect of the plates, and group C another aspect. But it's not quite the same thing as really owning the telescope, and doing what you want with it.

JS: Going back just a moment on OH, do you recall what period of time it was before the OH maser that you discovered was interpreted correctly?

HW: Was interpreted correctly? Well, it was certainly within a relatively short time. Within a year, I'm pretty sure, but that's not a very firm statement. I'd have to look at the literature again on that. I really haven't worked in that field since that period. But it was not too long.

JS: Do you recall who did that work?

HW: Several investigators were involved. Soon after the discovery, Weinreb and a group of collaborators[5] suggested that "mysterium" was anomalously excited OH. Litvak and his

[5] S. Weinreb, M. L. Meeks, J. C. Carter, A. H. Barrett, and A. E. E. Rogers, *Nature*, vol. 208, p. 440 (1965).

collaborators[6] as well as Perkins, Gold, and Salpeter[7] invoked maser amplification as the explanation of the very high brightness temperature of the radiation.

Hat Creek Director and Fund Raiser

JS: When you were the director of Hat Creek, and after the initial construction phases were over, did you travel frequently to the observatory?

HW: Oh yes. We were all there, this small group that was here at the time. Oh yes, I spent a good deal of time at the place. Working on equipment, and observing. Yes. I know it well.

JS: Was your teaching load reduced because of your responsibilities with the Radio Lab at that time ?

HW: Yes. I was half-time Radio Astronomy, and half-time in teaching and the department as such. The department and the laboratory have separate budgets. And so, the director is half-time – I suppose he could be more-time if he wanted. I don't know what the arrangements are now. But then it was half-time on one, half-time on the other.

JS: At the time, or at least part of the time, that you were director, I understand the university's alcohol policy was rather different than it is now, in that alcohol was not to be had on university property.

HW: Oh, yes. [laughter] Yes, that's right.

JS: Was that a hassle you had to confront sometimes?

HW: Yes, it was indeed a hassle I had to confront. Mostly because there was one graduate student, who will remain nameless, who was insistent on flauting this rule because he didn't believe in it at all. And properly so, but it was nevertheless the rule then. And I had to make an effort to try to enforce it. When he was there alone in the dormitory, and so on, there was just nothing that anyone could do; he did as he wished. But it did cause some trouble, and some worry. On the other hand, the rule was flauted by the authorities. In particular, I recall when we had the dedication of the observatory, and the chancellor of the Berkeley campus was there, and the director of the Lick Observatory, who was also the chairman of the review committee for the laboratory – it always has a, as all these organized research units have, there's a university committee that's supposed to look over them, and make sure they're running right, and roast the director periodically, and things like that. So, we were

[6] M. M. Litvak, A. L. McWhorter, M. L. Meeks, and H. J. Zeiger, *Physical Review Letters*, vol. 17, p. 821 (1966).

[7] F. Perkins, T. Gold, and E. E. Salpeter, *Astrophysical Journal*, vol. 145, p. 361 (1966).

all there, and our respective wives, and as soon as we got into the dormitory, the then-chancellor pulled down the shades, and brought out a bottle of whiskey, and said, "Let's have something to drink!" So, I couldn't say very much, except, "Yes, but I'll have soda in mine, please."

JS: Who was the chancellor at that time?

HW: Well, you really want to know? It was Ed Strong.

JS: And was Al Whitford –

HW: C. D. Shane. Shane was the director [of Lick Observatory]. Or – yes, he was still the director then, yes.

JS: You were the director of Hat Creek until 1972?

HW: I think that's right. I think it was altogether fourteen years, so that sounds about right.

JS: What prompted you to leave that position?

HW: To quit? Well, I'd been doing it a long time, and I just wanted to get out of the job of preparing budgets and doing all of the large amount of paperwork that is required. There was at first a reasonably modest amount. Fortunately, in the early days, as I mentioned, the Office of Naval Research did not require large amounts of paper. But as time went on, and we became a part of the National Science Foundation, and many rules and regulations changed, it was necessary to prepare larger and larger – in the sense of amount of paper and reports and so on – budget requests and reports of activities. It just seemed to me that I was spending more time doing that than I really wanted to spend; that I had other things I wanted to do.

I had never wanted to do administration to any extent. I enjoyed building the observatory, I must say. I enjoyed seeing things grow, and seeing them produce, and so on. So I must say, I enjoyed that aspect of it, but I did not enjoy the paperwork and administration aspect of it. And finally I decided after – in fact, I think it was within a year – no, it was a little longer than that. It was after I'd had a particularly long squabble with the administration about some budget problems, and the need to hire certain individuals who I believed would not be good for the observatory. The administration tried to force a certain business manager, who had been laid off from another department, onto us. I had known him in his former capacity, and also as a friend, and I just knew that that would not be a good deal for us, and so there was a long, long fight about that. And this just added to my frustration with administration. So I decided the time had come to get out.

JS: Could you say a little more about how the transition in funding from the navy to the National Science Foundation actually came about? I understand that was a period where there were many changes elsewhere in how funding was given.

HW: Yes, there were a great many changes in funding everywhere because the Office of Naval Research really changed the pattern of its funding. Congress, through the Mansfield Amendment, was demanding that research sponsored by the navy be demonstrably shown to be relevant to the mission of the navy. And that put a number of projects, quite a few of them in radio astronomy – the Office of Naval Research had been a vigorous sponsor of radio astronomy projects – in jeopardy. The National Science Foundation then had the task of finding money to take over some of those projects from ONR.

So there was a kind of rather rapid diminution, as I recall the amounts, in the budget supplied by the Office of Naval Research, and a ramp-up by the National Science Foundation. So we became, at that time, a part of the National Science Foundation, and all of our connections, all of the observatory connections with ONR, came to an end. They simply abandoned to us, to the university, all of the equipment that they had supplied, and all of the things that had been built as a result of their sponsorship. So it was a change in the rules of the ONR that changed the sponsorship of the observatory. There had been some small projects that were sponsored by NASA. They continued, and continued off and on quite regularly, but the big amount of money came from the navy, and then finally, from the National Science Foundation.

In the early days, National Science seems not to have been too interested in radio astronomy as such. They did not support university radio astronomy very vigorously. They were, of course, involved with the national radio observatory. That, perhaps, took care of things. But then eventually they became effectively the sponsor of all of radio astronomy.

JS: When the Office of Naval Research phased out its funding, my understanding is that some places actually lost out on funding completely then, and weren't picked up by the NSF.

HW: Yes, that's right. There were a few. There were some. We were fortunate in not being one of them.

JS: Do you think there were particular reasons that Berkeley was successful in maintaining funding?

HW: Well, I hope it's the quality of the work that it had been doing. It had been producing, and it was a very promising installation. I hope that's the reason. There certainly was no inside pull. It was necessary to fight every inch of the way to get the funds.

That brings to mind an aspect of the university, of differences between universities that I found interesting when I was fighting for money, spent so much time fighting for

money, as Jack Welch does now. I think he spends a great deal of his time trying to get money. I should think he would tire of that sometime. But, the University of California, at least on the Berkeley campus, is – I'll say right out – is not in any way helpful in providing, or searching out funding, getting funding. Obviously, it took care of all the administrative aspects of grants; it did all that. But the university bureaucracy was somewhat negative – some of the bureaucrats in the organization were somewhat negative at the very beginning, and I did have to fight with the business manager quite a bit. He was always wanting to put it off, and, "well, you can do it next month, and do it..." so on, and we needed the money right then and there. They would always find excuses for somehow delaying the process going on. It was just necessary to exert a constant push to get anything done. And the university did not ever send any administrators to Washington, or speak up in our favor. Robert Sproul, who was president of the university, did help a little in the very early days, and he did speak to some people in Washington.

On the other hand, at Caltech – and I think it is still very much that way, though it may not be quite so good – radio astronomy used to have a great deal of help from the administration. The president of Caltech would go and talk to the people at ONR. And he would have the admiral over, and they would talk about how much they needed this, that, and the other at the radio observatory at Owens Valley. And there was a great deal of assistance from some of the other organizations that were trying to build up radio observatories at that time, a great deal of assistance from their administration. That was never the case here. You're sort of thrown out on your own, or a little like being thrown into a pool and being told, "Now, go ahead and swim!" It was something that used to irk me, but there was no use getting uptight about it. You just had to go on and do your work.

JS: You commented on how, in many ways, it was a relief to get out of having to squabble for money, as the director.

HW: Yes.

JS: On the other hand, you relinquished a lot of control over a large organization.

HW: Yes.

JS: Was that something you missed?

HW: Oh yes, I think I missed it. Not terribly seriously, but I missed it. I think I still miss it occasionally, you know. There is a great deal of difference from having clout to having no clout. I think the part that I missed most, at least the first few years, was having no help in computer programming. I certainly had some – well, I better be careful how I say that – I had some for the first year or so after I quit as director. And then finally I had no help. I think that Jack felt that they had a lot of work in some of the instrumentation

programming that they had in progress at the time, and Art Setteducati, who helped me in the programming, was gradually shunted off into that, or there was an attempt to shunt him off into that particular field. It ended up finally, because he didn't like that sort of work, that he quit. He retired early, is what he did. And so other programmers were hired, and I never had any help after that.

So I guess that's why I now spend so much time of my time at the computer, as you know, as you see me around. For many years the only thing that I did aside from the work here on the programming and so on, was to teach the classes that I had, and worry about the astronomical societies, things like that. The societies sort of, in a sense, took the role of the observatory from the point of view of time. I try not to show it, but I chafe occasionally because when there are things that we really – that I think we really need, in the computing setup and so on, there's no way to get them. There are certainly hardcopy devices and ways of producing pictures, and so on; I think we're really very primitive in some ways here. There are a lot of things that are not quite, I think, as good as they ought to be. But people look at things in different ways, so one has to get used to it.

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Leaving the Directorship; Observatory Joint Ventures

JS: When you retired from being director of the Radio Lab, and Jack Welch took over, was there a significant change in managerial style that went with that transition, or with the sorts of work that was done at the observatory?

HW: Well, the sorts of work. I'm not sure about the managerial style, because I have not participated to any extent. I don't know quite how Jack runs it, in that sense. But there certainly was a change in the emphasis of the observing program, and I think that was due. That is, the time had come to do that. The eighty-five foot telescope has, in a sense, outlived its major usefulness. Now, I'd better – there again, that requires a little analysis. It certainly is not a telescope that is at the forefront of anything at the present time. It was at that time, when it was built. We would need a much bigger one if we were going to compete on size. I tried that for a while, and we did try to get a much larger telescope, a hundred-meter telescope. That eluded us; no one got it. There was quite a contest for that for a while.

But Jack's interests were always – they started at higher frequencies. He had worked with Sam Silver on the sun, and certain problems on the sun, with the little telescope at the third site, the third lab, lab three. And they had tried some experiments in interferometry between that and the thirty-three foot, and in a variety of ways. Clearly, Jack's interests were in these technical aspects, and I think he clearly saw that the future was going to lie in the direction of high resolution. So he became very much interested in interferometry. Hat Creek had been sited from the beginning, the observatory had been sited from the

beginning, to have the potential for interferometry. Hence the original search for as large a flat area as we could find to permit telescopes to spread out, and so on. So Jack has, from the beginning, pushed the observatory in the direction of interferometry, and molecular work at high frequencies. And I think he's done a splendid job of that. I think that he has really renovated the observatory, and pushed it into the forefront again.

I have been interested in one aspect of his efforts in that, and that is his effort to bring together a consortium of universities in order to improve the funding potential and to provide, in the modern world, the drive and the energy of a large enough group to get funding for the thing. So I think he's really done well in that. There was an earlier attempt at the same sort of thing, and we did have a consortium that was built up of Berkeley and Caltech and Michigan. And the effort there was to get a large telescope. That effort failed, and really there wasn't any very large telescope that was funded. I wanted to build a hundred-meter telescope.

JS: Would that have been sited at the Hat Creek site, or somewhere else?

HW: Well, the plan was to site it at Hat Creek, but because of the nature of this consortium, a variety of compromises had to be made, one after the other. And it would have been sited at Owens Valley so that it could be combined with the array that they wanted to build. They had two telescopes, and they always wanted to build an array of these large telescopes. They thought in terms of longer wavelengths. It didn't change into the higher frequencies and shorter wavelengths until much later. So, it's really wonderful that Jack has put such an effort in it, and has been so successful. I think that Hat Creek, at least for another decade, is going to be a very important observatory. I mean, a forefront observatory. It will always be, at least for the forseeable future, will be an important observatory because, with the equipment that they're installing now, it will be doing very good and very useful work. But whether or not there is a – in another decade – a bigger telescope doing things faster, with more dishes and so on, is what we'll have to wait and see. So I think the new directions of the observatory are just splendid, and that it's doing extremely well.

JS: Do you recall approximately when this attempt was made to acquire a larger dish?

HW: A hundred-meter telescope?

JS: Yes.

HW: No, I'd have to look up the dates. You know, I told you I was terribly poor on dates, and I am. It was quite a few years before I quit as director. It was a good attempt, but it didn't succeed. It was very hard to bring the group together. Caltech is hard to deal with.

Supervision of Students

JS: Well, you commented on some of the students who did early work with you at the radio observatory, and also students you had before then. Do you know the total number of dissertations that you supervised in your career?

HW: No, I don't. Do you? Did you count them all?

JS: I don't have an actual tally.

HW: No, I don't remember. It's not a huge number, you know, but I certainly am very proud of the people that I did work with. It was a joy doing it. That's really the fun of being a faculty member, a professor. Working with the graduate students, and having thesis students. Yes. And it's very nice that so many of them are still really very close friends and associates. Two of them were visiting us just the other evening. Amelia and Bill Wehlau, who are in Canada. That was an interesting pair, because – that was in optical astronomy, and Galactic structure – because both man and wife were my students at the same time. Husband and wife, I should say. And it was a great race to see which one was going to finish first. As I remember, Bill finished a little ahead of Amelia. And then, there was Mort Roberts, who has remained a very close friend. And Aden Meinel, and George Herbig, and – there were lots of good students. Really, they have made remarkable contributions to astronomy in this country.

JS: You talked some, a little bit of detail, about what Miller Goss and Barry Turner worked on. Are there other students you've had who you think their scientific contributions were particularly interesting or noteworthy?

HW: Well, I think those were probably two that were particularly noteworthy. Some of the others, I think, have been good contributions, but they haven't been as important a contribution at the time. I think that those were very timely contributions. They were just at the right moment, and just the topic that was under discussion, and was a hot topic at the time. The others – well, I guess the one that I would say that has lasted the longest and has led to the most important and large field is George Herbig's, and that was on T Tauri stars. Many of the stars one hears about, and came, were first – were worked on, not necessarily first worked on, but were worked on in that thesis. And an effort was made to bring them together in some kind of meaningful class.

George was a remarkable student who really needed no help. All one had to do was wish him well. It was a joy to work with him. In fact, it's the only time I've known it: George gave a part of one of the graduate courses while he was a graduate student! He had done a great deal of work at the Lick Observatory during the war years, and he had had the run of the telescopes to a very considerable extent. So he had a marvelous opportunity to

gather information, observational data, and to then get it ready for discussion. He also was a graduate student only two years, and Harold Johnson was a graduate student only two years. Each of them finished in two.

IV. Berkeley Astronomy Through the 1960s

Berkeley Astronomy Personnel Circa 1960

JS: The late 1950s and early 1960s were a time of a number of transitions in people and facilities at Berkeley and for the department.

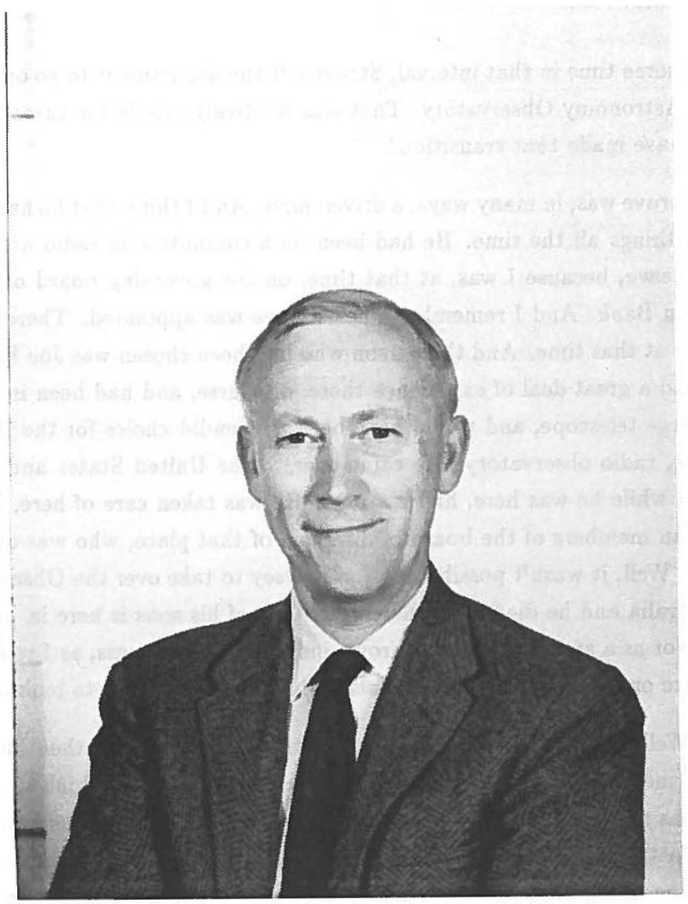
HW: Yes.

JS: And at some time in that interval, Struve left the department to go on to direct the National Radio Astronomy Observatory. That was relatively late in his career; do you know why he would have made that transition?

HW: Well, Struve was, in many ways, a driven man. And I think that he had to be proving himself in new things all the time. He had been on a committee in radio astronomy. I recall when he did leave, because I was, at that time, on the governing board of the radio observatory in Green Bank. And I remember when Struve was appointed. There was an effort to get a director at that time. And the person who had been chosen was Joe Pawsey, from Australia. He'd had a great deal of experience there, of course, and had been instrumental in building their large telescope, and would have been a splendid choice for the then small, but rapidly growing, radio observatory. He came over to the United States and looked it over and so on, and while he was here, had a stroke. He was taken care of here, actually by one of the physician members of the board of directors of that place, who was at the Harvard Medical School. Well, it wasn't possible for Joe Pawsey to take over the Observatory, so he returned to Australia and he died soon thereafter. One of his sons is here in Berkeley at the present time. Not as a student, but as a grown individual in business, as I remember. I've talked to him once or twice, and I'd like to talk to him again; I've got to look him up.

Well, the whole problem of the getting a director was then blown wide open, and I don't remember exactly how Struve's name came up. But I think it was primarily because there was needed someone who was a very well known astronomer who was respected in the field of astronomy, who had had experience in building things. And Struve fitted those, and I guess he was getting restless here at Berkeley. It certainly was a small place compared to the sorts of things that he had been doing at Yerkes, where he ran the show, and a large number – quite a large number – of research astronomers, and this was not at all that type of place. It was growing, and it was beginning to do the research for which it is now famous, but when Berkeley started in its new era, which was in the 1950s, it started from essentially zero.

Well, Struve, I think, was restless, and he had all the other qualifications, although he had no knowledge of radio astronomy. But he did take the job, and I think that he helped them out in a great many ways, though he certainly didn't help them out as far as building



Harold F. Weaver in 1969.

went. They had several disasters, including one large telescope that was a complete and total failure. It was essentially buried, gotten rid of. He was not a man to lead a large building program. That was just not his strength. It was really the young people who were there that ran the observatory. Dave Heeschen, who became the director then, and did a wonderful job of directing it. And Frank Drake, who also was very active in instrumentation and all of that. So, it was an observatory with very good young people in it, who had great ideas and were forward-looking, and an old person who had the prestige and authority, and would tell them to go ahead. Struve died not so long after that. He returned to Berkeley, where he had a house, and he died. I think he was not at all a happy man for quite a few years before his death. He simply wasn't doing all the things and wasn't in the forefront, the way he liked to be.

JS: In the time that he was associated with Berkeley, were you acquainted with him very much personally, or just professionally associated?

HW: Well, yes, personally as much as anyone. Struve did not have an active social life. And his wife was not at all interested in social things. It was a very strange pair. When we would be invited out by them when we were at Yerkes, for example, he would pick you up, or maybe you would come to the house, but then he would take you out to a restaurant or a hotel for dinner, and that was essentially it. He was not a person who was very good socially that way, or very active socially that way. I can remember a few times at the house that they – just off Solano Avenue – can remember a few times at the house there. But not very many. He simply didn't do much entertaining. His life was one of astronomical work, with his eye to the eyepiece, and measuring plates. A very, very, strange person in many ways, in that sense. Lots of interesting stories, lots of things.

I think he didn't like the administration here at all. He was used to running Yerkes Observatory, and just specifying what would happen and the University of Chicago would do it. Even in regard to salaries, and promotions, and all sorts of things – bonuses, which were never heard of here. And here he had to abide by all the rules laid down by the Academic Senate, and all of the things here. I remember him *ranting* one time, "No little Professor Brown is going to tell me what to do!"

JS: During the early to mid-1960s, there were a number of other personnel changes in the astronomy department, and a number of people that came and went not so long after. I think that included John Brandt, Paul Hodge, George Wallerstein, and Richard Michie. Do you know why there was so much coming and going then among the department faculty?

HW: Well, it was a bit odd. It was a bit odd. And I think they all left because they saw that they had greater opportunities somewhere else. I think that in each case, there was a real loss. For *us*, for us. I think that some left because they – well, George Wallerstein went really to establish a department. Became chairman of the department [at the University of

Washington], and he established a department. I think that the others also saw that there were greater opportunities in the sense of doing new things in new places. And that their chances were better there. And I think that in a sense maybe they were, in that at least in some instances they had greater freedom of action. That is, in some cases they had only research to do and no teaching. And in spite of the fact that it is – I will confess, that though it is wonderful to have graduate students, it is not necessarily always wonderful to teach Astronomy 10, for example. It does have its difficulties, and it is time-consuming, and it can be rewarding, but it isn't always as rewarding as one would wish. So in several instances, they left for jobs that did not involve them in teaching, or that involved them in beginning a department, or managing an organization, or doing something like that, that would, they felt, provide them with greater opportunities. Certainly there was no feeling ever of shoving them out. As I say, their departure, the departure of each of them, was a loss for this place, and meant that we had to search and find replacements. I think that the department was extremely fortunate in finding very good replacements, and that it has grown into a marvelous department that is so far from the one that I knew as a student, that I do not know words how to describe the difference. It has become a department of very substantial importance in the astronomical community.

The Student's Observatory and Construction of Campbell Hall

JS: I don't know the exact year, but in the early sixties or late fifties was also when the astronomy department saw a change of home to Campbell Hall.

HW: Yes.

JS: Were you involved for the planning for that move, or the arrangements for the new locale?

HW: Yes. Yes, very distinctly. I was a member of the Building and Campus Development Committee at that time, that was involved in all new buildings, and all changes in buildings. And I guess Astronomy got its building because of my presence there at the time. So I was very much involved in it, and presented the case for Astronomy, and succeeded in getting it onto the list very early. Unfortunately, one thing had to be changed – these are always the compromises – we were to have the entire building, and it was to be a little smaller than it is. It was to be five floors and not six. At just that time, the computer center was developing, and desperately needed space. So we were delayed in Campbell Hall for a year. It was enlarged; a sixth floor was added to the plans. And we had to share the building.

We had to share the building with mathematicians, and the computer center, which used to be in the basement of this place. And that proved to be very difficult from the point of view of expansion into a lot of new space. We had lots more than we'd had previously, but the idea was that we would have really a very large amount of space in the basement for shops, and laboratories, and all kinds of things. But we were all squeezed together with

these other guys. That was most unfortunate, and it's taken up until now to have the space somewhat eased. And we never have had the whole building and, I presume, never will. That necessitated several other changes in plans. For example, a part of Radio Astronomy for a long time was located in Space Sciences. And Stu Bowyer was in Space Sciences. There was never room for him here. I'm sure that there wouldn't have been eventually for him anyway, with his whole organization, which requires a large part of a whole building over there, in the center of Berkeley [the Center for Extreme-Ultraviolet Astrophysics]. But it did cause a lot of difficulties, having things spread around. Radio Astronomy was not in this building at first – not *all* of Radio Astronomy was in it at first.

At any rate, yes, I was very much involved, and the reason that the department got the building was because of my efforts on the committee that was involved with granting priorities, and ordering the buildings. But I must say, I failed miserably in keeping the whole building. It just was absolutely impossible under the circumstances to retain the whole building, as originally we planned. We beat out several of the others, of the big buildings around here, in order, and we could have waited for another ten or fifteen years in the building program if there had not been a strong effort to get it onto the early part of the list. That was an interesting part of the work I did at the university on this campus at that time, and I must say I feel a very fatherly interest in many of the buildings around here which I was instrumental in getting started. It's a very different campus from what it was in, say, the 1940s. It's many times the size it was.

JS: Campbell Hall gets its name from...

HW: W. W. Campbell.

JS: W. W. Campbell, former president of the university, and also director of Lick Observatory.

HW: Yes.

JS: Did you suggest that name, or did it arise naturally as a likely name?

HW: I think it arose from the whole department, as I remember. I was not involved in the – I would have gladly have selected it. I think it's a very good name for this building. But I was certainly not the only one who was interested in it.

[Interview 7: September 11, 1991]##

JS: Prior to the move to Campbell Hall, was there considerable shortage of space for the astronomy department?

HW: Yes, space was in fairly short supply. But I think it wasn't desperately short. I guess that's not quite right. Yes, it was. I'll go back. Yes, it was. As I think about it, we were spread over

three different places, and that did make it quite awkward. There was the old observatory buildings on top of the hill. Have you seen the plaque that's recently been put there?

JS: Yes.

HW: Those buildings were all filled up. There was a large part of one floor of one of the nearby T buildings, some of which are still in place, that was used by astronomy. And there was a place, there was a house, one of the many houses the university owns on Piedmont Avenue across, in general, across from International House, and down this way to the north, that was used by Radio Astronomy. We had one of those houses as our first office. So, yes, I correct myself, we were short of space in one building, but everybody was housed somewhere. So it did seem very important to get everybody, all of the operations, together in one place. So it was important to get a building.

JS: Concerning the old observatory buildings on, I guess it's referred to as Observatory Hill, there are some fragments of those buildings still in existence there at this date. Is there a story as to how they continue to persist in part?

HW: Why they're there? Yes, it's surrealistic. It's bizarre actually, in many ways. I think the only excuse for having those pieces of building there is that they hold up the wisteria. The wisteria, which grew profusely over those buildings, was a glory of the area. It was wonderful in the proper season, when all of those flowers were out. So to the best of my knowledge, the only reason for leaving those bits and pieces of building up is to keep up the plants. But I think they're gradually dying out. I'm not sure that the plants are maintained at all. So I think the that normal decay of plant and building will soon remove all remnants. I went by earlier, a few days ago actually, and I noticed that more and more of the pieces of the building were collapsing. In particular, one of the large railings is now down on the ground. I suspect that all of that will disappear. I hope it doesn't become a parking lot, but is somehow extended into the park-like setting that there is now present on that hill.

The old observatory was really very comfortable. It was almost like a campus unto itself. It was a very homey place, and it's just too bad that there weren't enough rooms and spaces. Of course, it really had to go. It had to be replaced because it wouldn't have lasted. But it did have a character about it that this concrete building lacks.

JS: You mentioned a minute ago the new plaque that is over there, which indicates that this was the former site of the Student's Observatory, later known as Leuschner Observatory. Do you know the circumstances how that plaque came to be put there?

HW: No, I don't, and as far as I am aware, no one in the department knew about the placing of that plaque. It was discovered by Dave Cudaback, and he came over and asked if I had seen the plaque. I said, "What plaque?" No. As far as I know, it was placed in the middle of the

night, and by some mysterious characters. I don't know how it got put there. It's too bad that the department didn't participate in some kind of a ceremony to place it there. Maybe there was a ceremony, and they just forgot to notify the department. [laughter]

JS: When did the Leuschner Observatory, in its current form near Lafayette, come into existence?

HW: Well, there you have me on those dates again. It was sometime either during the transition from the old observatory on Observatory Hill to this place, or shortly thereafter. And I honestly do not remember that date. I was not very much involved in that at all. And so I never participated in any of the big decisions, or any of that. I think the reason for that is that I was fully occupied with the radio observatory. My recollection is that the move over there was sometime after we were in this building, and after I was very busy with Radio Astronomy. The person who would know about that was John Phillips, who has always been very much involved with that observatory and its operation.

JS: Did the renaming of the observatory to Leuschner Observatory occur shortly after Leuschner died?

HW: Yes, it did. It may have been just before. He had retired. He retired in, must have been 1938 that he retired. And sometime after that. Quite honestly, from the stories that I heard, the hope was that he would leave some money to the institution, and that naming it for him might encourage him. But he never did.

Rudolph Minkowski

JS: One person who was associated with the Berkeley Astronomy Department in the 1960s was Rudolph Minkowski, whom I believe you had some role in bringing to Berkeley.

HW: Yes, I invited him to Berkeley. I had known him at Mount Wilson, and admired his work and the joint work that he and Baade had done. And when he retired from Mount Wilson, I invited him to come to the radio observatory here. He had been very much involved in radio astronomy during the last years of his career at Mount Wilson. He had been to Australia on a kind of sabbatical – they really didn't need sabbaticals at Mount Wilson – but he had spent some time in Australia with the radio astronomy group, and was very much involved with radio astronomers all over the world. And I thought it would be a very good idea to have someone of his stature and knowledge involved in the radio astronomy here.

And I think it was really a very fruitful period that he spent here. He didn't do very much writing, but he did one or two papers after he came here. But he was a great inspiration, and a *great* source of information of all kinds. He had a tremendous encyclopedic knowledge of all kinds of objects, and things, and processes, and operations, in astronomy. He had a deep physical insight into things, which was his great strength. And all of that was a great advantage and help to us here. I was really very pleased when the administration

agreed to give him an honorary doctors degree a few years after he was here, and also there was a symposium for his eightieth birthday, which unfortunately he did not live to see.

Student Politics in the 1960s

JS: Berkeley in the later 1960s is commonly associated with a lot of student unrest and political activity. Did that find its way into the astronomy department?

HW: Oh yes, it did. I think – now, these are my recollections – it certainly found its way into the department. There were not terribly many students who were involved in a deep way. I remember from my own personal experience only one, and that was Ashley Cunningham, whom I guess I mentioned after we ran out of tape last time. Ashley was a student in radio astronomy, took his Ph.D. in radio astronomy, and he was vigorously involved in the protests. He was, I remember, put in jail, and he phoned up here to the department and to Radio Astronomy, and asked for help to get out. And as I remember, the then-secretary of the radio observatory or radio astronomy group went over and bailed him out. But there were certainly repercussions within the department, in the faculty, and classes were disrupted. George Field was then the chairman of the department, and George was a very strong liberal who did participate at least to the extent of cancelling classes. I think he taught classes off campus; that is my recollection. I remember that. He was very much involved. But the rest of the faculty, to the best of my knowledge, did not participate in that direct and personal way. Of course, the unrest upset everything. There was nothing normal on the campus at that time. But there certainly was an effort here to keep operations together. I remember the famous incident of the tear gas, which I watched from this window; here you could watch the helicopter fly over and drop it on Sproul Plaza.

JS: Today the Berkeley graduate students in the astronomy department have what I think many people might find a surprising amount of say in departmental matters.

HW: Yes.

JS: Has that always been the case?

HW: No, I'm sure that came as a result of the problems in the 1960s. Going back to that for just a moment, I should mention that there was later one student who was associated with the department who participated in a number of demonstrations. I cannot now remember his name. I don't recall ever having him in class. But he was a student in the department. And as a result of the difficulties he had, he dropped out of the university, and to the best of my knowledge, ended his career in astronomy.

But I'm sure, yes, that certainly the current strong participation of students in the affairs of the department stems from that time. It certainly was not so at an earlier stage. And I cannot imagine that the earlier members of the group, of the faculty – for example,

those who were on the Observatory Hill there when I was a student there – could ever have contemplated the role that the students currently play in the affairs of the department. I think it has been a good move to make the students a part of the decision-making process, because I think they are mature enough to participate in a meaningful and reasonable way.

I had some unfortunate circumstances, or unfortunate experiences, I felt, in an earlier stage, when some of the undergraduates participated and did things; not in the department, but at one of the newly built residence halls. When the residence halls were built, faculty members were invited, and in fact asked, to participate in the operations or in the living of the place. So they were called – I don't remember – Faculty Fellows, or something or other like that. And we participated; I was assigned to one of the residence halls, those tall buildings over on the south side of campus. And, to participate in meetings with the students, and have meals with the students, and all kinds of things like that. And I really became quite involved in some of the operations, in improving the way in which the students would participate. And one of the students was really very vigorous in his pursuit of this. And he graduated – he was a senior, and he graduated – and I tried to continue to interest him in the projects that he was so involved with, and he said, "Oh, forget it. I'm out of here, I couldn't care less," and he never would participate any more. That is, it was something that I felt showed no interest in the continuation and *future* of the place.

I think the most important thing in the participation of governance, and specification of administration, and so on, is that there be an eye to the future. If you wreck the organization right now, those who are in and out, as, for example, the students, will not really suffer from it because they're gone. It seems to me that to participate meaningfully and appropriately, the students must think not only of themselves – or the faculty for that matter – the individuals involved must think not only of themselves and their own benefits or whatever then, but they must think about the future and the continuance of the place after they're no longer involved. Otherwise, the whole organization will collapse and fall apart.

So I think the students here are now – the graduate students in the department – are sufficiently mature to think of it as an ongoing, continuing institution, in which they are now participating, and building for the future. But I'm not sure that, you see, all of the undergraduate students and so on have always had that view. Some certainly have, but many have not; for them it's a thing of the moment, and that's always very dangerous for the organization. I'm not sure anyone here has ever talked to the students about the future in that way, and about building for the future, but perhaps they think of it themselves. I don't know. I'd like to talk to you about that sometime, and see if you feel the students have any view of that kind.

JS: Before we leave this general era, I think 1961 was a noteworthy year for the Berkeley astronomy department because that's when Berkeley hosted the IAU General Assembly meeting.

HW: Yes.

JS: Were you heavily involved in the preparations for that?

HW: Unfortunately, yes. [laughter] It was really – the meeting was chaired by C. D. Shane, in the sense that he was the person in charge of the meeting. And he and Mrs. Shane were heavily involved with that meeting. And Cecile and I worked with them, and in a similar way were heavily involved. So sort of the Shanes and the Weavers were the ones who bore the brunt of that thing, with Shane being the chairman of the whole affair. The department members also played a role, but it was mostly the four of us. For example, Mrs. Shane – Mary Shane – and Cecile went on all of the tours and did all of the things before the meeting took place. That is, it was all, everything, all the operations of the organization had been gone through in practice sessions at least once. Unfortunately, even that wasn't enough, and there were a few disasters, or at least accidents, but it went pretty well. There was about a year of work on that.

There was also the problem of raising money for it, and I was fairly heavily involved in that operation. Interestingly, the money was used for – I should explain that, perhaps. The money was used not only for some of the affairs of the meeting, special receptions and all that sort of thing, but as travel grants. There was a large number of travel grants for foreign astronomers. That's always a problem. The astronomers always have great plans, but not great amounts of money. So they did need that. We did get enough. I think no one who really wanted to come was denied an opportunity. It meant, on my part – for that particular aspect of it, which I remember very well – it meant contacting lots of foundations, both local and national, and building up a fund for primarily travel, but also all these other things. In fact, I think there was a fair amount left over. There were quite a few thousand dollars that we didn't use. And that formed the nucleus of a fund in the National Academy [of Science], which is used for IAU affairs at the present time. So in that sense the Berkeley meeting has a legacy that continues to support the IAU.

It was a very interesting affair. It was the last, I guess, of the sort of small meetings. I enjoyed the meetings very much, though I got to attend fewer sessions than I would have normally. There were always things to take care of.

JS: As part of that meeting, I think there was a visit to Hat Creek.

HW: Yes, it was the official dedication of Hat Creek. And what we did was to arrange for a group of astronomers, who were primarily in the field of radio astronomy. And we arranged for a visit to Hat Creek, an overnight visit, so one of the big jobs was just to find enough housing in the nearby area, in Burney. I think everyone was finally housed in Burney. Of course, we had every room we had at the observatory site in use at that time. It was a very pleasant affair. There was one surprise at it that I'll always remember, and perhaps some of the astronomers who were on the trip will also remember. That season in that area, at Hat Creek, is really very nice. It's by and large warm, pleasant. It's an out-of-doors time.

JS: What month was this?

HW: This would have been in July. July, August. We could also look up the exact date by a special method that I'll tell you in just a moment. We arranged for an outdoor barbecue. The observatory site, I guess I already mentioned, was a kind of an old lake bottom, or at least there must have been in the lava an indentation there. There's lava all around it. And then there's quite a few acres of very flat land in amongst that lava. So the arrangement was that they would visit the two telescopes, and see them in operation, and so on, at that time. And then we would have a big outdoor barbecue, which was up on the lava, which is a few feet in that place, a few feet above the flat land on which the telescopes are built. So there was a stairway built up, and gravel was put up there so it was not so rough, and so on. It was a beautiful evening, which started before sunset, and had outdoor festivities. And then the moon rose. There's a beautiful view to the east from there, and just the sort of high lava plateau that is a few miles, a mile or so, off to the east. And the moon rose. And everybody started talking about it, "Isn't that a funny color? What is going on?" And it turned out that there was an eclipse of the moon, and not a single astronomer knew anything about it. There was Jan Oort, and all the big shots from everywhere. No one had an inkling of it. So we could date it, you see; we had to have an eclipse of the moon.

JS: I believe Oort at that time was the president of the IAU?

HW: Was he the president? No, I think at that – he may have been. He was president, and he was secretary; he was lots of things at different times. He may have been the president. That's a bad failure of memory. The person I remember very well from that meeting – I can tell you some Oort stories sometime, or later, maybe. The person I remember very well as being with quite a number of times was V. A. Ambartsumian, and the Russian group. We had developed quite close ties with a number of Russian astronomers, and we had dinner parties for them, and so on, at that time, at the IAU. It was really wonderful. We had a dinner party every night that there wasn't some official activity. And there were all the official thank-yous, and so on.

The Russians were always involved in that. It was Ala Masevich, who was then, I'll call her, the crown princess of Russian astronomy. She just disappeared from it later; that

would be an interesting story. She gave the thank-yous. She was the one who spoke English best, and always was representing them in these meetings. And she gave elaborate thank-yous to all of us, and especially I remember she mentioned our children, who had somehow taken care of some of them, and acted as guides and all kinds of things, and were helping them out. The reason for this close friendship dates from a much earlier meeting in Italy, when two of our children, the two older ones, were with us for a summer in Europe.

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HW: In the meetings – they were meetings in – that was the year the meetings were in Rome – and on one of the trips we were actually going to the Blue Grotto. And somehow the children, our two children, had gotten separated from us, and we were very worried about it, but they were fine. The Russians had taken care of them; the Russians had adopted them. And so after that, we were always very good friends with the Russian group. And that was nice, because they always remembered us, and asked about the children, and felt very parental towards them, I guess. Those meetings do develop many interesting and close friendships. I think that they represent an important part of international astronomy, though I do think that the need for the General Assemblies is diminishing, and I guess maybe the General Assemblies are diminishing in size. There are so many other meetings of a specialized nature, that these large, general meetings are less important than they used to be.

Relations with Jan Oort

JS: When I brought up Oort a moment ago, I believe there is at least one story I've heard you tell about ...

HW: Which one?

JS: ... your arrangements to convey him to Hat Creek for this special occasion.

HW: Oh, yes. That was an interesting part. Well, you treated Oort with kid gloves, and so on. He was, in many ways, Mister International Astronomy. And I think he wasn't a great admirer of me, for a variety of reasons. So I was always very careful with him. Oort had very strong likes and dislikes. And though he would tolerate people, he might dislike them. And some that he collaborated with on many instances, he really did not like very well, for unknown reasons. But in this case I was very anxious that he have everything that would be for his convenience, and so on. So to get him to Hat Creek, while most of us traveled in buses and so on, we arranged for Oort to go in his own car, with a driver.

So we borrowed the chancellor's; it was a Packard, a big Packard car. And my younger son, who was then in high school, just barely able – he had been driving for a year or so, but he was an expert in it. He likes things like that, mechanical things. And he was the chauffeur. And he just took them around, and did whatever they wanted to do. One of the

rules that we'd been given about the chancellor's car was, for heaven's sakes, don't get it dirty. And Oort, and Mrs. Oort, who were in the car, and also W. M. Christiansen, who was from Australia, was the originator of the Christiansen cross, was with them, and he wanted to go off – they, I guess, all maybe, all of them – wanted to go off on some road and see some sight. And my son Kirk had been given strict orders, for heaven's sakes, do what they want to do and take care of them, and so he drove off on the dusty, unpaved road, and – we had to wash the Chancellor's car when it came back. [laughter] There are lots of stories of a variety of kinds.

JS: You said that Oort had fairly strong opinions about people, sometimes for reasons that were unknown.

HW: Yes.

JS: Do you have any sense of why he might have not held you in high regard? Was it for personal, or scientific reasons?

HW: Well, I *think* it was because I one time wrote a paper that had some words he didn't like. It had to deal with Galactic rotation, which, of course, is Oort. And I had pointed out in this paper – there were some mathematical models; in fact I think there were three papers that followed one, two, three – pointed out that one had to be very careful in determining the constants of rotation, because there were, quote, "biases in the results." There were built-in mathematical biases in the procedure. Now that's a perfectly good statistical term, that all the statisticians use: mathematical bias, observational bias, everything. And I think that Oort interpreted that as that *he* was biased. And I think that caused a problem that existed for a very long time.

I think also that there was a great change as a result of bias, and largely because of changes in distance scale. Distance scale in astronomy is always changing. It is like a rubber ruler; sometimes it's long, and sometimes it's short. And don't worry, if it's long now, just wait a while and it'll be short again, because of problems that arise, and understandings that become clarified, and so on. And there's nothing worse than the distance scale. And in part because of mathematical bias, and in part because of changes in the distance scale that was then current, the value of A was changed greatly. And Oort had always advocated a value of 19.5 or 20 for the Oort A constant. And I was coming out with something like 12 or 13; which, it turned out, was more nearly 14 or 15 later. And that, Oort didn't like, because it was a value very different from what he had advocated. And he, I think, reluctantly accepted a smaller value. But it's interesting to look at his papers after this, and to notice that he is always pushing it up, and he is trying to get back to 19, or 19.5.

So I always thought that that's why he sort of had it in for me. And I was sorry for that, because I had very much wanted to collaborate with him, and at one time had

thought of going to Holland for a sabbatical and spending the time working with him. I admire him greatly. He's an extraordinarily productive and imaginative astronomer, and I've always been very sorry that there wasn't a close relationship with him. I would have enjoyed knowing his insights and knowing his operations more closely than I did. So that's why I think Oort always had it in for me.

V. The Weaver Family

The Weaver Children

JS: I wonder if we could digress for a moment, and come back to something that's come up a few times in this conversation, and that is with reference to your children. Could you tell me a little bit about your family, and your children?

HW: Yes. There are three of them. They've all had interesting lives. Our oldest child is a girl, Margot. Our second child is a boy, Paul. And our third child is a boy, Kirk. Margot, Paul, and Kirk. They are all doing interesting things. They have – especially Margot and Kirk – have lots of interests in out-of-doors. I think that that started in part through our living at Mount Hamilton. They all seemed to enjoy their lives there. We did lots of hiking, and nature, and they had all kinds of – in the summer, at least – had all kinds of pets: lizards, and horned toads, and once a turtle. All sorts of things that you can find at Mount Hamilton. So they gained a lot of insight into nature from that period.

And it continued through backpacking in the Sierra, which we used to do every year. I guess we missed a few because we'd be away, or something like that. But there was always backpacking in the Sierra, from the time that Kirk was about six. We had – at that time, Margot and Paul could carry packs and hike, and we did with Kirk when he was six one overnight, in which he carried the lunch, so everybody had a backpack. And then we went from Tuolumne Meadows down to Glen Aulen as the first overnight with Kirk. And that was fun, and thereafter the trips got longer and longer, and finally they could be almost anything. I guess we never were out more than ten days at a time, but that's a fair time to be out. It was something that all of the members of the family enjoyed.

Margot Weaver Garcia

The children are all doing interesting things. They have made interesting switches in some ways. Margot started in political science when here at Berkeley, but she shifted over to botany, and she has a bachelors degree in botany. Then she married rather young, and her husband, J. D., was an officer in the air force. And they were for some time at the air bases and so on in the Southwest. They were at Albuquerque. And so Margot did graduate work at the university there. And she has a masters degree. She did field work on certain kinds of plants in the desert. Then, like Cecile, she was busy with two children for a long while. When the children got older, she started doing more things. They had gone finally to Wisconsin, where J. D. got his Ph.D. in physics. And now he is professor of physics at the University of Arizona at Tucson. So Margot did all kinds of things in Tucson of a civic nature, and she finally became a member of the council, the City Council.

It was quite an interesting group; there were some other faculty people, or faculty wives, on it, on the council. The city had a lot of water trouble, as that area always has. And the council had to change the structure of the water price structure. And the council raised the fees on the water cost, including instituting what they called a lift fee. So the higher in the mountains that you were, so the water had to be lifted, you paid more money. Well, this caused a great uprising, and the council was actually recalled. I think there was only one member left after that, and a whole new group put in. They never changed the fees, by the way, which was the basis of all this political trouble. Well, I think Margot was terribly disillusioned. What seemed to be a proper and normal sort of thing did not go over with the populace. I think that's because the council didn't introduce the problem in the right way, and have hearings, and do all that. So Margot became very acutely aware of the need for citizen participation, and so on, after that.

Well, she was very hurt and shaken by this, I think. So she immediately went back to school, and she got her Ph.D. in planning. Her thesis involved how to get citizens to participate in planning problems. And she became a consultant in that after she got her degree, and she helped in particular, was very active, with the Forest Service – again, the out-of-doors and so on – in all of their citizen's participation programs. And she organized the meetings, and so on. She is currently the chairman of the Department of Planning at Virginia Commonwealth University in Richmond, Virginia. And she maintains an apartment there, and a house in Tucson, where her husband – where they have a beautiful adobe house. So she has a commute of rather large size every month to go home and see how things are running. So I think she's very busily involved, busy, and heavily involved in teaching and running a department.

Paul Weaver

Our middle child, Paul, started in physics at Cornell. He became involved as a high school student with Telluride Association, which is an interesting organization running out of Cornell, where they have a house, that is, a large living house, for a group of students who come from all over the country, and are on full scholarships. And it's a wonderful place; very intellectual group. Paul started in physics in Cornell, and after the first year decided that wasn't for him. And I can see why. When I talked to him about it, and we looked at some of his things, it seemed to me it was simply deadly dull physics. It just wasn't the sort of thing that would draw a student in. And so he went into government, which was, I guess, much more to his liking, in which he did very well. All the children have done very well in their schooling.

He went on as a graduate student, and he took a Ph.D. in government at Harvard. And he remained there as assistant professor for a few years. But he became very disillusioned with the student protest, which he thought the faculty had not handled very well. So he

went into publishing, and magazine work, and so on. At first he was the – I don't know what the title was exactly, maybe managing editor – of *The Public Interest*, which is a small conservative journal, heavily involved with faculty. There are a lot of Harvard people on it, and so on. Moynihan publishes in it regularly. And Paul had known Moynihan and quite a few of the people through Harvard/MIT, where he had worked with them. And from there he went on to be an associate editor of *Fortune* magazine. He went on to open the Washington office for *Fortune*, and ran the Washington column for quite a while. And then felt that there wasn't enough future for him there, and became associated with Ford Motor Company, with the headquarters office in Detroit, where he was kind of their – well, what? I never have known exactly what the term would be, but corporate public relations. And that didn't last very long; he was there for a few years. He felt that they weren't vigorous enough in pursuing their own interest. In particular, that they acquiesced too frequently to Washington rules and regulations which were not good for them.

So for the last few years he's been at Hoover Institution at Stanford. He's been one of the fellows there. He's written a book, called *The Suicidal Corporation*, and he's just finishing up another one now, relating to the media and to the role that the media play in the work and agenda of the country. I think he may eventually be going back into some kind of corporate work, or magazine work, or something like that. As an editor of a magazine, perhaps. Or perhaps he will continue to write books related to the media, business, and the government.

Kirk Weaver

Our younger son, Kirk, went to college here, and he was in economics and things relating to business. Kirk has always been interested in business things. As a child, he always thought he'd like to run a big farm in California; run it from a business point of view. So I can see him running a thousand-acre farm or something like that. He did some interesting things when he was in high school. He went and worked on a farm for several months, and became involved in all aspects of a large farm. He also was in Telluride, which I think influenced both boys very considerably. By the way, Paul's roommate at Telluride House in Cornell is now dean of engineering here on the Berkeley campus, Dave Hodges. Dave was several years older than Paul.

Kirk went on to business school at Harvard. He graduated with an M.B.A. from there, and went into business operations. He is now the chief executive officer and chairman of the board of an electronics manufacturing company – now it's becoming more and more a service corporation, a servicing organization. They have one division that manufactures, but Kirk is changing it more and more into a service organization that services the equipment, mainly telecommunications equipment, of large corporations. He's been quite successful at that. He has been involved in investments for various concerns. One of the investments

was this company, of which he has now become the head of. It's a very interesting family, that one. His wife is professor of law at the University of Houston, and has become a national/international expert on law, on oil and gas law. And now she's very much involved with water problems. So she and Margot have interests in common.

Family Reunions and Grandchildren

It's an interesting group of children who have all gone on to do interesting things, and we very much enjoy them and their families, and our get-togethers. The whole family gets together, certainly at Christmas, and we try to get together at other times of the year also. Over the last few years Cecile and I have usually been going on some exploratory vacations of a week or so during summer with Kirk and his wife Jacqueline and their two sons Kyle and Kenyon. It's the K family: Kirk and Kyle and Kenyon. So we enjoy the children and *their* children very much.

JS: In total, how many grandchildren do you have?

HW: We have six. Margot has one girl and one boy, and Paul has two girls, and Kirk has two boys. So it's evenly divided. And they really span a generation. Margot's children are older. They are – well, in fact, in their twenties and late twenties. And Paul's children – Paul had children last, and one of them is only seven now. So it's like two generations of children.

Cecile Trumpler Weaver

JS: You've said before, when we were talking about when you got married, and your wife's interests in college, and her later interests, that she had a sort of change in interests and careers when she went back to school at Berkeley.

HW: Yes.

JS: And that was to study social work?

HW: Social welfare. Yes, after our children had grown up and flown the coop, so to speak, she went back to school in the field of social welfare and took a degree there, and has done social work ever since. She started – well, she also did quite a bit of work of essentially a psychiatric nature with children, and she ran a service for that for several years to help troubled children. Then as a social worker, she was involved in adoptions for quite a while, for several years. And then became involved with the chronically mentally ill. And she ran a program in Oakland, and built up what is called the Towne House Creative Living Center from a small program to one that was in operation every day, and now has altogether of the order of a hundred people associated with it. She is now the Director of Creative Living Centers. There are five of them in the Alameda County area, in Oakland, and over in Livermore, and around in the area. And she's very much involved in administration now.

I think that she enjoyed more working with people, but somehow she's gotten shunted off into administration, trying to run all these programs.

And I think now, as much as anything, she's a real estate agent – finding housing for these people, and raising money. If you have an extra million that you'd like to give, Cecile would be delighted to get it. They are involved in buying houses, converting them into apartments, and places where the chronically mentally ill who are well enough to go out into the community, to live an individual life and not be in a housing cooperative or other housing area where there is someone who's living with them. When they're well enough to go out on their own and to work and so on, they need some houses. Their income is not very large, and they need something that's quite inexpensive. So her big task lately has been to develop housing for them. That takes all of her energies. I see her occasionally now, though. [laughter]

[Interview 8: September 12, 1991]##

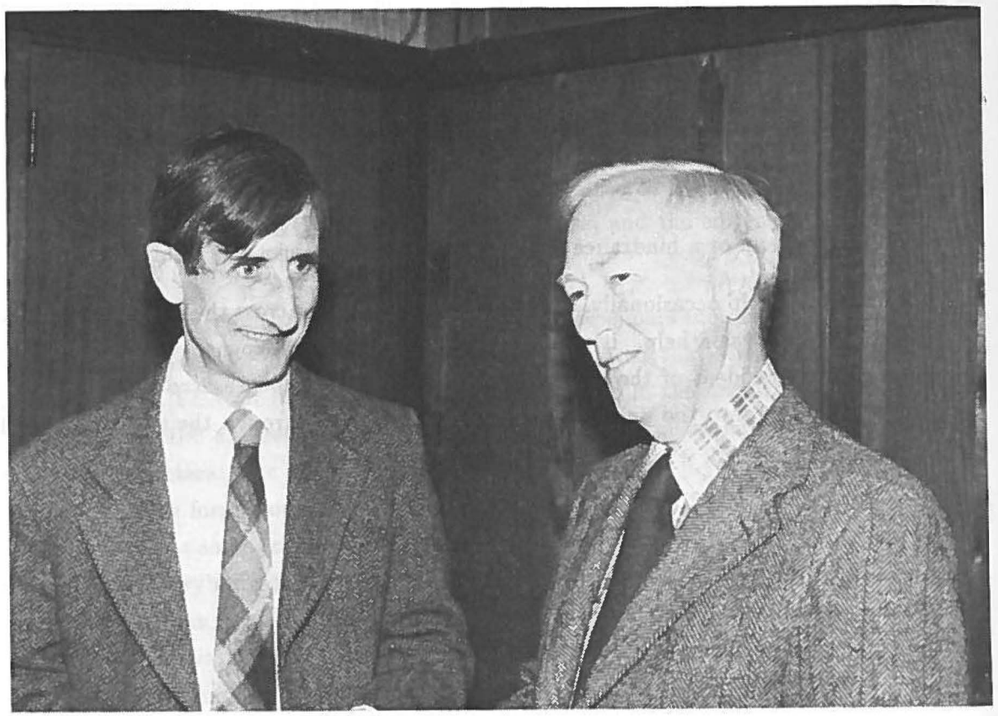
Postscript to the Brazil Eclipse Expedition

HW: After your kindness in taking me up to hear you play yesterday [the carillon in Sather Tower], I thought of another thing vaguely related in regard to the eclipse that I went on. I think I told you that I was invited by the National Geographic Society to give a couple of lectures for them as a result of that. They were – the one in their regular lecture series in Washington in Constitution Hall was a film of the eclipse with narration; I was the one who narrated it. And then a similar lecture, the annual National Geographic Society lecture at the AAAS [American Academy for the Advancement of Science] meeting. And they paid me for those. It was a reasonable amount of money for that time; it would be a pittance now. But when I came home, we bought a piano with my lecture money. And all of the children learned to play the piano on it.

JS: Wow. Have any of them continued with music?

HW: Yes, yes. My daughter plays the flute. When she was here, she played with various small ensemble groups. And my younger son plays and practices with his two boys quite often. Kyle and Kenyon are remarkable musicians. Kyle has composed music since he was about six. Both boys have many awards for their piano performances. For Cecile's and my fiftieth wedding anniversary, Kyle composed a piece which was performed by a string quartet that played during the party – which was held outdoors on Mount Hamilton! And Paul is quite a good one on the piano. He occasionally plays it for relaxation. When he gets too tense from his writing, he'll go and play the piano.

JS: Did you yourself play the piano?



Physicist Freeman Dyson with Harold F. Weaver, on the occasion of Dyson's visit to Berkeley as Hitchcock Lecturer, circa 1985.

VII. The Evolving Astronomy Student Community; New Work with Chabot Observatory

Berkeley Astronomy Students and the Blues Chaser

JS: There's one more topic I wanted to ask you about, which is sort of a jump in subject, but it's prompted in part by something you showed me before this discussion started, and this is a daybook from the Student's Observatory. And you also had told me about the Blues Chaser. Could you tell me a little more about the Blues Chaser?

HW: Yes. This daybook, I think, is not so interesting, because it wasn't maintained for very long. But it was a day-to-day account of what was done in the observatory. It starts in 1903, if I remember correctly. And it has a few – I guess really only a couple – of clippings in it. But it was not maintained, and you really don't get a feeling for what went on over a long period of time. But there was a book that used to be taken care of by the graduate students of the department. It was called the Blues Chaser. You were supposed to read it and laugh when you had some sad occasion, hence the name. And it was full of all kinds of – *is* full; it still exists in an office here, somewhere in this department. And it's full of all kinds of pictures, and accounts of activities, and jokes, and funny things that happen in the department, and all meetings of the humorous society that used to exist here. It was called Delta Alpha Delta Delta. And those are the residuals from the positions of a comet, because originally this place was a comet-orbit mill. And everybody computed comets, and were always worried about the residuals, the differences between calculation and observation. So there was a society that met once or twice a year, and people were inducted into this society, and they were supposed to give humorous papers for the assembled crowds at the time of the meeting. I don't know if any of the papers still exist, are still available. But some of them were pretty funny, and some of them were pretty sad.

Anyway, that volume, which I think we – as I was mentioning to you before we started – I think we should probably put in the Bancroft Library so it is preserved as a memento of the early history of the students in the observatory. It has things from really a great many astronomers who are well known, or used to be well known, because it covers a long period of time. And has all kinds of personal stories, and jokes about them; things of that sort. Interestingly, Lawrence Aller was one of the ones who really had some pretty good jokes and things in there. One might not think of Lawrence as being very funny, but he does have a very good sense of humor, and there are quite a few good things that he put into it that we ought to look at together sometime, if we can break the document loose from its current possessor.

There was another thing that I mentioned to you, from those early days, and I ought to get them down so you can look at them. And that is the recordings of heights of astronomers

HW: Yes, I learned to play, and Cecile also. But I never learned to play terribly well. I played the violin, as she did actually too, more than the piano.

JS: Have you continued with that at all?

HW: No, no. Unfortunately it's all disappeared. In principle I wouldn't mind doing it, but in practice it would be just too difficult to find the time.

VI. Work within the University and Astronomical Community

Relocation of Lick Observatory

JS: Well, over the last couple of discussions we talked about a number of things that were changing around the University of California and Berkeley in particular in the 1960s or so, and one topic that I think we touched on but haven't really talked about in detail was the move of Lick Observatory facilities from Mount Hamilton, and ultimately to Santa Cruz, though there was some discussion apparently about, first of all, whether that should occur at all, and second of all, whether the move should be to Berkeley or elsewhere.

HW: I think that earlier I had mentioned that some time before the war there had been a university committee established to consider whether the Lick Observatory should move down from Mount Hamilton. There were members of the Berkeley faculty on the committee and I presume that there were Lick astronomers on the committee also. I was a graduate student at the time, so you must realize that my information is not very direct. I did hear the decision of the committee discussed at later times. I believe that the committee had C. D. Shane and R. J. Trumpler as members. R. T. Birge, who was also a member of the committee, was probably the chairman, though it is possible that that job was held by Shane. The decision of the committee was that the Lick Observatory should move its offices and center of activity to Berkeley. Mount Hamilton would remain as an observing station.

After the war, there was even a building picked out for the combined Lick and Berkeley departments. I do recall hearing some of the Lick astronomers say that they were not in favor of moving, and I remember discussions of plans for housing the Lick library on the campus. The library was to be closed off, not accessible to the students. Surprisingly, when Shane became director, the plan to move Lick operations to Berkeley was apparently abandoned in spite of the fact that Shane had earlier favored the move. The observatory, as an institution, became, if anything, more firmly than ever attached to Mount Hamilton with the construction of several new houses fairly soon after Shane took over.

There was a second discussion of a move from Mount Hamilton when Shane retired and Albert Whitford became director.

JS: Was this the Brode Committee?

HW: It may have been the Brode Committee, yes. And the proposal was that they move here, but that did not go over well with the Lick astronomers. They were firmly against it. I do not know the details of their discussions; I took no part in them. I only know what a few of the participants reported later. There was fear – I remember a few astronomers mentioning it – that there was fear that they would be gobbled up by the Berkeley campus; that physics would take over all of astronomy. There was a variety of fears for the future of

the Lick Observatory. So there was no move. I mentioned that there was then a – the plan had come up to go to the Santa Cruz campus, which was then in process of formation. A major act of Clark Kerr as president was to enlarge the university, to start a number of new campuses. Santa Cruz, I think, was anxious to have the Lick Observatory, because it gave them a distinguished department, a distinguished group, immediately. It would add to their prestige and position. And it was a place at which the Lick astronomers somehow thought they would feel comfortable. Ostensibly, the reason was that it was closer than Berkeley. Now, I think it turns out it is, but by about two miles. So it's not a significant distance thing.

The move was not accomplished immediately. It was not approved immediately after it had been proposed. I did recommend to Clark Kerr that it not be made. I thought it was to the disadvantage of the future of the Lick Observatory in a lot of ways, in that while they would be a distinguished department there, they wouldn't have the advantage of being with a group of active research organizations in the field of physics, and chemistry, and in science in general, which they would find on the Berkeley campus and at the Lawrence-Berkeley Laboratory. So there would have been, I had always thought, a much better environment for them on the Berkeley campus.

Well, they finally moved to Santa Cruz. I guess it has been a good move for them. It certainly is an advantage being down from the mountain. I think the necessity of living on the mountain, in the way that it was a necessity in the old days, has disappeared. And that being in a community of some size has made it much better for everyone concerned. So I think the move was very much to the advantage of the Lick Observatory group. But I still am sad that they didn't move to Berkeley because I think it would have been an advantageous move from a lot of points of view, in that they would have been involved with a very active group of research now in physics and in astronomy, and have access to lots of opportunities here. And the Berkeley department would have had the advantage of close association with the Lick astronomers. Now perhaps it has worked out well, and conceivably even better, there; you have to ask the director of the Lick Observatory about that. I just don't know. But I wish Lick had moved to Berkeley, because I think it would have been a good move. I think it would have been a good move for them and for us.

JS: When the Lick astronomers expressed opposition to moving to Berkeley, as you indicated earlier, was that primarily because they were afraid of a loss of autonomy and control, or because they also were simply opposed to the geographical relocation?

HW: It's probably both, and very likely, other reasons also which I don't know, and which you haven't suggested; which neither of us knows, I guess. They had, of course, from the beginning been autonomous in that they were even considered a separate campus. For a while, they were associated with the Berkeley campus for business operations. The

purchasing was done through here, and a variety of business operations were done through the Berkeley campus. That may not have gone too well. I haven't any specific or unusual insights into that. But I think they chafed under that necessity. At least, if they were at Santa Cruz, they would go through Santa Cruz and be present and have all of the opportunities to do things quickly, and so on. But so would they have had those opportunities if they had been here on the Berkeley campus. I mean, somehow we've all managed to live with the Berkeley purchasing department, though it is stupid at times. I'm sure, I think they probably all are. Anyway, it was a move, it's done, and I think one makes the best of it. And it may have been the best move. But it didn't seem to me to be the best move.

Los Alamos - Lawrence Livermore Advisory Committee

JS: Well, among the many activities you've been involved with as part of the University of California community, I believe you've also been involved with the committee that oversees the administration of the national laboratories.

HW: Yes.

JS: Los Alamos, Livermore – and is Lawrence Berkeley Lab also included?

HW: No, LBL has a separate committee. Only the two national laboratories that are really operated for the government. Yes. LBL is not a part of it. It has its own committee.

JS: How did you come to be involved with that sort of work?

HW: Well, I think – the committee was first formed – oh, it was a good many years ago now, probably of the order of twenty, maybe a bit more. The president of the university wanted a committee of that sort formed. And I think that one of the advisors that he was depending on to get the committee into operation was Bob Brode in physics. Bob spent about half of his time in that early period, when the committee was in formation, in the president's office, I suppose on various special tasks that were assigned to him. I had worked with Bob Brode on a number of committees; Educational Policy, and quite a few different things. And I guess he simply suggested me as one of the members of the original committee, which was under the chairmanship of a UCLA chemist named William G. MacMillan. He had been very much involved in military operations, was well known among the military group in Washington, had been, I guess, the science advisor in Vietnam, and certainly had his finger on all of the activities of the laboratories and so on.

There must have been about twelve members, perhaps. I think I have still a picture somewhere among the things, of the original committee at one time. The members came partly from the campuses – partly from the university community, partly from the outside – corporations, NASA, a variety of organizations. A retired general. There were, in fact – over the time that I was there there were several retired generals and admirals who played roles

in the committee. So I think that my activity on that committee started up just because Bob Brode had thought that I might be a satisfactory committee member on it.

JS: So do you recall the approximate year that committee was started?

HW: Well, I could find – there's a plaque at home that was given me for when it was I finished my chairmanship, I could check it. Let's say it was – it probably was of the order of five years ago that I quit, when I resigned from it. And I had been on it from fifteen years, so that's, say, twenty years. So it would have been around 1970, '71, sometime around then that I started.

JS: What was the sort of time investment that that committee required from you?

HW: Well, a fair amount. During the first years when I was a member of it, it probably wasn't more than ten or fifteen percent of my time. It may have been fifteen-plus percent. It certainly involved being present at the laboratories for several times a year for a few days each time. There were all the reports and things in between. There were various special tasks for the president's office, where the committee would do certain things and report. When I was chairman of the committee, for the last five years of my membership on it, it was much more. And then it was a third or more of my time, at least, on that committee, because there were all the secondary conferences with the directors of the laboratories, preparation of agendas, before – I had at least one meeting a month with the president of the university or the executive vice president. It was with the president originally, before David Gardner became president, but Gardner never played a direct role in it. He turned over all of the activity of that committee to the executive vice president, and so I would meet with him. Normally there was a report before each meeting, a kind of a briefing of what we were going to do. This would be a two-day, occasionally a three-day meeting at one of the laboratories. And then afterwards a debriefing on what had happened, and what we had found out, and what recommendations there were, and so on, for the laboratories.

Our task was, as I always understood it, was to form a judgement as to the quality of science and the quality of work that the laboratories were doing. It was not – it did occasionally get into the programmatic things, but I always thought of them as the quality control group for the science that was in progress. And we viewed really everything that the labs did. Not at each meeting, to be sure; there wasn't time. There are *hundreds* of projects in those labs. At each meeting we would choose a certain category of things to look at. And then we would have reports, and direct visits to the laboratories where the work was being done, and so on, to view what they were doing.

JS: Did that result in sort of small perturbations in the development of projects at the lab, or were there larger results that came from this review?

HW: Well – you must explain a little bit more. Do you mean...

JS: I guess I'm just wondering sort of the magnitude of the consequences that came about from these reviews.

HW: You mean whether we killed anything, and whether we started something new?

JS: Well, that could be included in your answer.

HW: I think that we did have effects in a few instances of modifying programs. Actually, it wasn't necessary to do too much about that. By and large the programs are very well thought out. And very generally, I think what we succeeded in doing was simply keeping them on their toes. You see, we did not try to modify their programs. That is, we did not say – to use an example that I'm sure some faculty members here on the campus would like to use – we did not tell them they had to stop making a certain model of a bomb. That was not our task. We certainly reviewed the bomb programs, but we did not tell them to stop or to start, either way. That was not the purpose of that committee. We did occasionally suggest that they get into certain types of operation that had nothing to do with bombs. They do all kinds of other things. For example, from everything from ways to recover more oil out of oil wells, to ways of generating electricity that are not going to do anything to the environment. And it's a very broad program that does nearly anything you can imagine.

We did encourage them in various technology transfers. It is not the task of the labs to develop marketable products. But there have been lots of things developed at the labs for lab use that could be taken over by private companies and developed for commercial use. For example, the lab might develop new measuring instruments that they need and that might be useful to others. The government might then arrange under an appropriate license for the idea and models to be transferred to some company or companies that would develop the device for commercial use. The labs developed very neat ways of separating isotopes of carbon, oxygen, and nitrogen. These could be very useful in industry. A benign and unusual isotope of one of these elements might be built into, say, fertilizer. One could then readily trace the path of the fertilizer after it had been spread on a field. There have been many useful technological developments at the labs. It is important for these to be transferred to the industries in the country. If I were the vice-president for research of a large – or even a small – company, I would be beating on the doors of these labs to look for products to be developed by my company. There have been many transfers; there should be more.

JS: In recent years the fact that the University of California runs these labs has become controversial.

HW: Quite controversial.

JS: And in fact, there's discussion as to whether the amount of oversight that the University of California exercises is appropriate.

HW: Yes.

JS: Do you have an opinion about that?

HW: Opinion about it. Well, I think that it is a – I'll use the same word – it's a matter of opinion. The question is, what do you mean by oversight in this case? Certainly some members of the faculty who have been very vigorous in their opposition, and vigorous in their suggestions as to what ought to be done, would like to see a great deal of oversight. And I think that what those faculty members would like to do is to control the laboratories. Now that has never been the role of the University of California in the strictest sense. It does control the laboratories in the sense that it specifies, or at least it talks about, the accounting methods and what they do. The university is the manager of the laboratories. It is not the owner, or specifier of the program of the laboratories. Those are really quite different things. The program has been specified by – the major program has certainly been specified by the military services, or by the civilian Atomic Energy Commission, which has military people involved in the military aspects of it. And the requests for development of certain military hardware come from, eventually, if you follow it back, come from the military services.

It has not been the role of the university to specify those programs. It was never invited to do that. Though there have been a variety of faculty demands, and faculty requests, and faculty committees to have the university play that role. There was one group that I remember we had to deal with quite extensively. And they wanted to specify – they wanted to have a faculty committee, a university committee, that would really specify what the lab was to do, and would recommend that they make changes in programs, and changes in what they were doing, and all kinds of things. Well, in talking to the most vocal of those faculty members, I finally discovered what he wanted to do. He didn't like the way they were making one of the bombs, and he wanted to change the whole technique. And that was it, that's why he wanted the committee.

It has seemed to me that – I guess I'm rather pragmatic in that. It has seemed to me that the citizens of this country, through their representatives in the Congress, have decided to make these bombs. And given that, then it is not, I think, wrong for the university to be involved in the managing of the plant. It has always seemed to me that the university in that sense was a very benign manager. It gave the people there more freedom than any other place, than any other group that I know of.

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JS: So you were commenting that the University of California is in many ways a benign overseer.

HW: Yes, in the sense that it gave the workers at the labs opportunity to speak against what the labs were doing. In fact, I think that in some cases the university went almost overboard in that. There were some individuals who were protesting the work of the laboratory. They were among the scientists at the laboratory and were spending, in fact, a good deal of their time, on laboratory payroll, protesting. I'm sure that they had greater freedom under the management of the university than they would have had, say, under General Motors, or EG&G, or name any other big company that you wish. In that sense, I think university management is an advantage for the people there. I don't know whether all these advantages are as needed any more, and I suspect that the laboratories as atomic bomb plants will kind of wither away in the current climate of the world. But at the time when the laboratories were started, the attitude of everyone, the attitude of most people in the country, was very different from what it is now. After all, there's been a great change in attitude towards nuclear weapons.

They were started by scientists here on the Berkeley campus. And the original growth of those laboratories was quite normal. I'm sure that if those laboratories were to be started now, they would not be managed by the University of California. There would be no connection between them. But the historical connection, I think, came about quite reasonably and normally, given the problems of World War II at the time. And certainly – for some long time after that – there was no problem for university management. Having the university connected with it, and having the authority of individuals like E. O. Lawrence and others who were involved in the starting of the laboratories, provided the labs with an opportunity to employ a class of scientists better than they would have gotten if a company had been managing the laboratories. So I think that the university connection – that was always one of the stated advantages of being connected to the university – gave the labs opportunities for hiring personnel that they would not have had otherwise.

And the university has gotten some advantages. Some would probably say that is making a pact with the devil. But the university has gotten some advantages out of it. The Livermore Lab and the Davis School of Engineering have a joint program for Ph.D. degrees. The students work part time at the lab. At the Hertz building at Livermore and on the Davis campus are classrooms directly connected by wire. The professor can lecture at either place. Television gives the students in each location the same view of what the lecturer is doing and saying. Voice connection is direct and immediate. A student at Livermore asks the lecturer in Davis a question. The lecturer responds as though the student were in the same room at Davis as he is, effectively. It is a program that has given many students the opportunity to earn higher degrees while working. Faculty members and students on the university campuses use lab computers and lab equipment they would not otherwise have available; there are many joint projects between lab groups and campus groups – and there is the IGPP [Institute of Geophysics and Planetary Physics] lab that Claire Max runs.

One thing that I tried very hard to develop was the idea of – especially during the years I was chairman – was the idea of cooperation between the university and the laboratories. IGPP is a good example of that. Actually that was suggested originally by the chairman from whom I took over. He was the director of the state-wide IGPP, a geologist in Los Angeles. And there are many ways the labs provide funds for people on any of the campuses. They can use equipment and so on. One of the things that I tried to push as the chairman of the committee was the idea of cooperation. And finally that got written into the contract for a few million dollars, so that the lab could provide funds and opportunity for campus researchers to use facilities at the laboratories that would not have been available to them otherwise. So it seemed to me that there were a lot of possibilities for advantages of the university – by which I mean faculty and students of the university.

The university connection does not seem as bad to me as it does to some people. But maybe that's because I do see some good coming from it; and I believe that severing the university-lab connection will not stop the laboratories, it will not stop the manufacture of atomic bombs, it will not stop anything. Severing the university connection might make some faculty members feel better because they then imagine that they are not associated with the bomb. But in a very real sense, they are just as much associated with it as ever, because their taxes are paying for everything that is done in the labs. As long as they remain citizens of the United States, they are fully involved; they have not withdrawn from supporting the labs, paying for the bomb. Sweeping the dirt on the floor under the rug so we don't see it does not eliminate the dirt. As citizens, we are all associated with the labs and with the bomb no matter who manages them.

And I would rather see the labs run well than not run well. I would rather see them run in such a way that the people there have as much freedom as can possibly be given to them, which the university does. And that somehow as much good would come out of it as could come out of it, and that would be through these cooperative ventures, and the providing of funds, opportunities for research, that has nothing to do with bombs. Furthermore, there is an awful lot of non-nuclear-bomb research in progress there. And that can be done only because those laboratories are so large, and so good, and so well equipped, and so splendid in so many ways. They are wonderful laboratories. And they do all kinds of things – many people, I think everybody, would feel are very good things. They also do things that some people feel are very bad things, that is, they design bombs.

As to oversight on a larger scale but in character similar to the present oversight, I really do not know what the university could reasonably do, more than it is doing now. The contract does not involve the university in the programs carried on; it is not for the university to specify what the labs do. It is the university's job to manage the labs in a business-like way to accomplish the tasks specified by the government. And that means manage from a financial and scientific point of view. It might be possible to rewrite the contract in such a

way that the university could specify certain programs or have certain inputs, but it would be a different kind of business and philosophical arrangement than has ever been made. I do not think that would be an improvement.

The university might consider a larger oversight function in regard to the environmental impact of the laboratories on the sites they occupy and possibly on the surrounding areas. That would be a much larger involvement for the university and would no doubt involve considerable contract modification. But, again, I do not think that such additional official involvement would be appropriate for the university.

Having the opportunity to be on that committee and work with the people, both on the committee and in the laboratory, certainly changed my view of a number of things. One is about the top military brass. I had known one or two of them through odd connections; even through that National Geographic eclipse, I had met the general who was the head of the air force at the time. But on this committee I had an opportunity to meet a good many generals and admirals. And you know, some of the people, some of the generals that I had met, were among the most able of managers and people that I have known.

Andy Goodpaster, for example. He was a general; he was Eisenhower's chief of staff when Eisenhower was president. He was the head of the NATO forces in Europe. When West Point had trouble, and there was a great uproar about cheating, Andy Goodpaster was called in to become the head of it, and he cleaned it up. He is simply a remarkable man. He has a Ph.D., for example. He's extremely level-headed, very straight-thinking, very even in the way that he does things; has a wonderful philosophy of life, the way that he does things. I would be happy to be associated with him in any capacity. I wish he were the president of the university, or at least the chancellor of the Berkeley campus! He is an absolutely splendid person.

There was Jimmy Doolittle, whom I came to know quite well, who ran the raid on Tokyo. He was of the same type. I think he wasn't quite as wonderful as Andy Goodpaster. But he was one of the clearest-thinking members of the committee. He was certainly better than some of the faculty members, I can assure you. And it's just that these guys, many of them, are really very good. It changed my view of the military. That isn't to say there aren't wild generals and wild everybody you want, but at least many of them who were doing the big things are really splendid people. It gave me a new view, and a considerable amount of confidence that maybe something would go right. Maybe the orders wouldn't get carried out, but the orders would be all right.

Also it gave me a view of the university, since I came to know several of the presidents quite well. But I better not comment on that. They're not all the same.

Campus Planning and Other Committees

JS: You mentioned earlier that before even getting involved with the laboratory committee, that you were involved with a number of other committees on campus.

HW: Yes.

JS: Are there other committees for which your involvement was noteworthy in some regard, that you have some comments on?

HW: Well, the one that I – I have mentioned the one that I remember very well; I remember very well because it was a hard-working committee, and some of its deliberations are on the campus permanently. Those are the buildings on the campus. I enjoyed working on that committee – the BCD (Building and Campus Development) Committee – and seeing things develop, I suppose because I like to see things go forward and develop. That doesn't mean I'm a great believer in – quote – "Progress" – unquote. I mean, I wouldn't build everything up solid, but I do like to see things done that are useful, worthwhile, and appropriate. And I think an important part of that is the word "appropriate." I enjoyed building Hat Creek. Have you ever been there? No?

JS: I must confess I haven't.

HW: Oh, indeed you should go. I think the houses are very nice. I think that they're well done, and well placed, and all that. And I enjoyed doing that. I enjoyed working to get things done here on the campus, to see things appropriately built, and properly built. I think not all the buildings turned out very well. The BCD Committee recommended what was to be built. The administration looked after funding. The details of a building would be worked out by a specific building committee and architects and so on; the university architect was always deeply involved. And some of the buildings, I think, have not turned out very well. I think Evans Hall is not very nice. It's too big, it's too overpowering, it's – they shouldn't have let that get away from them quite so much. It was overbuilt, I think, at the time that it was built. On the other hand, I think some of the others around here have been very nice and have worked well. This building is a little too small. I think that – well, there's the education/psychology, that was one that's O.K. And geology, geophysics is another. There are quite a few of them that have come out, I think, all right. And it was fun to feel that something was being accomplished. So I liked that.

I was also on Statewide Educational Policy. I did not feel that that was a committee that had as important an impact as it might have. And on Educational Policy for the Berkeley campus. That solved lots of small problems, but I don't think it solved any big problems. And, well, those are the ones I remember most.

Work with the Astronomical Society of the Pacific

JS: One aspect of your astronomical work that wasn't explicitly through the University of California has been with the Astronomical Society of the Pacific.

HW: Yes. And the American Astronomical Society. Yes.

JS: I understand you have a long association with both, actually. In particular, I think you had a fairly key role in the Astronomical Society of the Pacific around 1970, when they underwent some degree of reorganization.

HW: Yes, yes. Well, I had been involved through a committee of – it was called Committee on Aims – that preceded that time. And what we did was to examine the activities of the society, and to try to understand how the society had interacted with the astronomical and potential astronomically interested group; and what it might do to improve the interaction, and to improve its ability to make astronomy known and interesting. Well, there were quite a few recommendations. One was to have an active executive director, who would be full-time, and organize the place. There ought to be some more popular journal. There was a little thing that came out, monthly leaflets – ASP leaflets. Each leaflet dealt with a single topic in a few pages, and interestingly the format was just about the size of an envelope, a little bit smaller; yes, rather smaller than a business envelope now. And it was made – and this will give you an insight into the nature of the organization – it was made to fit in a gentleman's inside coat pocket, when he was commuting from his home to his business in San Francisco. It had grown up, the society had grown up really, among a certain group in San Francisco and had been fostered by them. But it wasn't doing a very significant job in reaching the larger public.

Well, soon after that – in fact it may have been after the committee's report, with all these suggestions – I became the president of the society, and so I then simply started instituting these changes. In a real sense, I guess it redirected the way in which the society works, and operates, and so on. One thing that it became is a much larger, more active organization than it ever was before. So I'm glad to see that succeed and progress. I think it needs to do a lot more, but I'm finally out of it after many years. I was also the chairman of the finance committee, and was directly involved with the investments of the society. And I'm very happy to say that we made lots of – the committee, which did manage the finances in a very direct way – made lots of money for them; and including buying a building, which was about the last thing that I did.

JS: When you were the president of the ASP and went about implementing some of these suggestions of the Aims Committee, did you find the membership receptive to these changes, or was there a good deal of resistence?

HW: No, I don't think that the membership was resistant. I think the membership is sort of bland and accepting. I think that the new members are thankful, because, in the sense that they were brought in and they were given something that at least many of them must enjoy, since the membership is certainly much larger than it was, and is, I believe, growing reasonably well now. No, I don't think the members objected. The members are rather placid about the whole thing. It's rather the few people who may be active, or the board of directors, or whatever. I don't recall any serious fights about that.

Well, there were a few; there were a few, among them Sturla Einarsson, whose name you've heard before, who was professor of astronomy here when Leuschner was the chairman of the department. Einarsson became the secretary-treasurer of the society. And I think that he did dislike the idea – in fact, maybe I should say that he vigorously disliked the idea – of increasing the level of activity, and spending money to do things. He was very much against spending money to do anything. And that was one reason why it was a kind of sleepy society at the time. It gave out a few prizes. It published the *Publications of the Astronomical Society of the Pacific*, which were, in a sense, too technical for the amateurs, and not technical enough for the professionals; and I think it's getting away from that a little bit, but not too much. So there were one or two people who, I guess, felt that the old ways and the old procedures were fine, and they didn't see any reason for changing anything in the modern world. But I think we have got to change, you know; we have to grow, and change, and learn new ways, and learn new things, and keep trying.

I think I'm not altogether happy with all the things the ASP does now. I'm very happy with some of the things it does. I think that its going into catalog sales is a great thing for it. First of all, having those things, all those items – slide sets, and posters, and all this sort of stuff – is very helpful for the teachers that buy it. I think that's great. I think it should go into the whole thing more vigorously than it does at times. And I think it is changing into an organization that is pushing that more vigorously. I always wanted to get a special manager, and there has been a manager for it. But someone who is really a professional, and perhaps could get it into much bigger time than it is now, with a national mailing, and involved in some of the very large mail catalog things. There are a few of the products of the Astronomical Society that are in catalogs now, but there ought to be more. For one thing, that is the only way that the society is really going to finance itself to help teachers, to help amateurs, to help all the other – the members that belong to it. It really cannot expect to raise enough money from memberships to pay for all the things it does. So its catalog sales, which can do lots of good things, can also support the society financially. And I hope that it will grow.

Yes, that has been an interesting project to – the whole bit of the Astronomical Society has been an interesting project. I had to learn a lot about that, especially about managing finances. And that's how I got into the job of treasurer of the American Astronomical Society.

The Burbidges – Geoff Burbidge had been the president of ASP while I was involved with a small committee in managing the finances. And Margaret Burbidge was the president of the American Astronomical Society when it was necessary for Bill Howard to resign from his job – the job to which he'd been just elected – as treasurer of the American Astronomical Society. So Margaret asked if I wouldn't become the treasurer, which I did. And I think I had that job for ten years after that. One year, or maybe twice actually, as acting treasurer; and for three terms, that'd be nine years, as treasurer. So that was an interesting stint.

[Interview 9: September 13, 1991]##

ASP Finances

JS: You commented yesterday on your long involvement as treasurer of the ASP.

HW: Well, I really wasn't the treasurer of the ASP. I was the chairman of the Finance Committee. There was a combined job of secretary-treasurer. And the treasurer was not very active when it was a combined position. So I was the chairman of the finance committee for a long time. And it was that committee that really handled the finances of the society. Then later on, the job was divided so that there was a secretary and then a treasurer; there were two positions. And then I was treasurer. But that was only a few years. That position was created to get me to stay on another few years, another year or two, which I did, and then turned the job over to Eugene Epstein, who is now the treasurer.

HW: As part of your involvement with the ASP finances, I think you did a lot to improve their financial situation. Could you comment a little on the details of how you went about doing that?

JS: Well, it was done by working with a number of brokers, and simply investing the money in stocks that went up. There were far more gains than there were losses. There were not many losses, and they were normally small. When I took over as chairman of the Finance Committee, the society was in a very – very bad position, I guess I should say, in the sense that its portfolio was a very old-fashioned one, in the sense that there were large numbers of bonds. And bonds, at least in my view, are somewhat dangerous instruments in the sense that they will return you a small percentage income, but they will be worth only what you paid for them at the end of their terms, at the time of their maturities. The normal increase of the cost of living will then diminish that money by a very substantial amount. So in fact, under normal circumstances, I think bonds, while safe, are not very good investments in general in the modern world if you plan to hold them until maturity. And the society was doing that.

The society had had a large number, several years in a row, of deficits which were increasing. I was looking at some reports – I've been throwing away papers and things, and

looking at some reports I had written for the committee, the Finance Committee – really bemoaning the fact that we couldn't continue in the society this way, or we'd be absolutely broke. And something had to be done. We had to sell off a large portion of the bonds, in fact, finally all but one, in order to meet deficits and cover deficits of the past years. That was a forced sale, in the sense that we had to cover the deficits in order to keep in operation. But because the bonds were fairly old, and interest rates had risen very considerably during the life of the bonds, they had very low value compared to their original investment cost. That, just because of the change in interest; to a first approximation, the price of a bond is simply the ratio of interest when you bought it to interest now when you're selling it. That's not the total story, but it's a large part of it. So the society lost money in order to keep in operation.

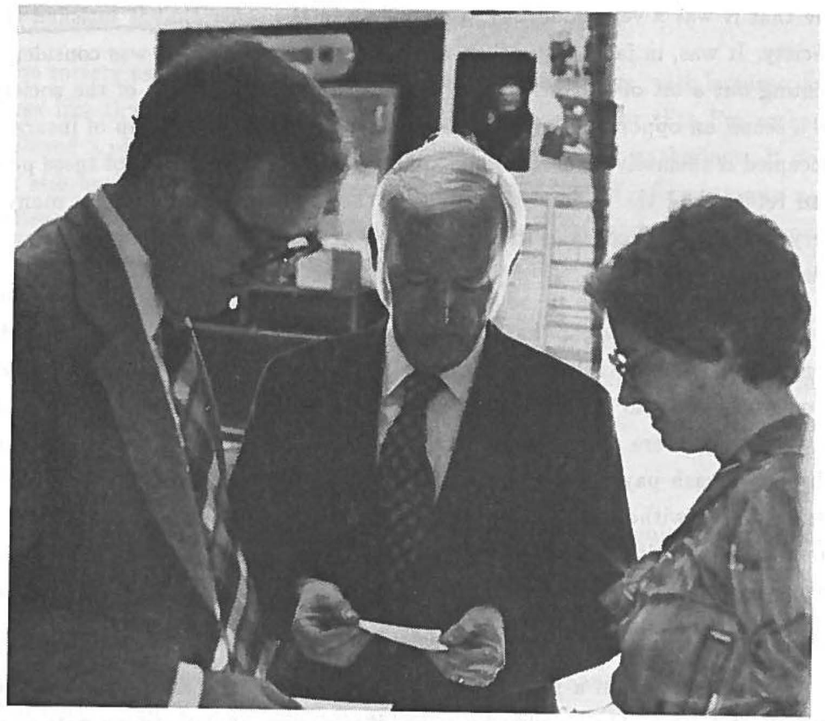
Well, we couldn't continue that way for very long, so we started a rather vigorous campaign of investing the money with full safety – you have to be very careful when you're working with a society like that not to go outside the bounds of prudence in any sense, but actually one can do quite well with very safe stocks if they're bought at good times, and sold when they have increased in value. And we simply did that. Eugene Epstein, in particular, and I worked together with brokers, and spent a good deal of time in working with stocks, and increasing the portfolio of the society. It recovered; of course, the market recovered too. We couldn't have done it if the market were going down like a falling stone. But the market was good, and we were able to recoup all of the monies that were lost, and in fact increased the value of the portfolio very substantially. Witness the fact that we finally bought a building for seven hundred thousand dollars, and still had a half a million or so in stocks, whereas originally we had only a few hundred thousand left in the endowment of the society. So the society is now doing quite well. I have stopped muddling in the finances of it at all. And Eugene Epstein is very busily engaged in it, and, I think, doing an excellent job.

JS: When the stock market took a fairly large tumble a few years back, was the society able to avoid losses from that?

HW: Yes, in fact, we had gotten out of a large portion of it before the fall. It seemed to me that there was going to be a market decline, and we did liquidate a large portion of the stocks of the society and avoided that. Not entirely, because we weren't completely out of stocks, but certainly very largely. That was good luck.

JS: What did you then put that money into?

HW: Oh, for the time being it was simply in money market funds, and things that brought in interest without having any downside of risk.



Astronomers Robert P. Kraft, Harold F. Weaver, and Sidney C. Wolff, at a meeting of the ASP Board of Directors, December 1981.

ASP Building Acquisition

JS: You mentioned part of your involvement was in the purchase of the ASP building.

HW: Yes.

JS: And I understand that you were involved in a fairly close way with the negotiations leading to that purchase.

HW: You really have been investigating me, haven't you? [laughter] Yes, I was. It seemed to me that it was a very good buy, a good buy in the sense that it satisfied the needs of the society. It was, in fact, larger than the society need, and there was considerable talk about renting out a bit of it if needed, in order to improve the funds of the society. But it was, in a sense, an opportune buy. The building was owned by a group of insurance agents who occupied it themselves. It was some kind of a loose confederation of these people. And they had refurbished the building, and redone the building – rebuilt it, in many ways – into a very nice set of offices, and then after a few years, had a falling out. They were anxious to get out.

Finally it was clear that they just wanted to get out of that place, and so I proposed that we offer them "cash on the barrelhead" a couple of hundred thousand dollars less than they were trying to sell it for, and really, less than I think the building was worth. That is, I think we were really undercutting a little bit. Well, the point that there could be an absolutely cash payment – we had lots of money in money-market funds then; we took a small loss by withdrawal slightly early. But we had lots of money available to us, and we could just hand it over to them. And after a day or so in which they must have argued vigorously among themselves, they finally took the money, and I think we made a very good buy on that building.

So it was again a great opportunity in the sense that it satisified the needs of the society, and more. The people who owned it were very anxious to separate one from another, and go away from each other. And we had the cash to make that possible for them, and get the building that the society needed. So it was simply a lucky set of circumstances that the society could take advantage of. And it did. The society has never rented any of the space. Like all organizations, it rapidly expanded into all available space, and is doing fine. So I think that was a happy conclusion to a long-standing need of the society for space.

ASP Administration and Activities

JS: At the time of the society's reorganization around 1970 or so, one of the changes that was instituted was the hiring of a full-time executive director.

HW: Yes.

JS: And were you involved in hiring that first director?

HW: Yes, I was. That occurred, as I recall, when I was president of the society. And I was involved in it. That was a part of the implementation of the proposals of the Aims Committee, and the idea was that there would be, I always said, a Mister ASP, who would become very active in touting the society in all possible ways, at getting into the public eye, and making known the things that it could do for individuals interested in astronomy; provide them with information of all kinds. And also assist in teaching astronomy. Not at the university level, but providing information for grades K through twelve. And simply making the joys of astronomy and the marvels of astronomy known to the public.

The society used to hold quite a number of public meetings, with lectures, and shows, and things like that. It's done less of that in recent years. For that I'm rather sorry. I wish it played a somewhat more prominent role in direct public gatherings. It does have a – I think still has – an astronomy hotline, and it supplies lots of information to newspapers and publications. It's always a source of information on new things. When anyone who's a reporter or writer or whatever wants information, the ASP is a ready source. But it's a little less involved with direct public things than it used to be. It does lots of work with teachers, and it has, as I mentioned the other day, the *Selectory*, as it's called. That is, it's a catalog business in which it supplies all kinds of things for teachers and amateurs and individuals interested in astronomy.

JS: The first executive director, I think, was Leon Salanave?

HW: Leon Salanave. You may have seen him yesterday at the colloquium. Leon and I were students together. We came to Berkeley at the same time in 1936, and graduated together in 1940. Leon did some graduate work, but then went off into teaching and other things. He did not finish a degree, and became interested – really, more interested – in amateur-like activities than in professional astronomy. He has taught at San Francisco City College, was there until fairly recently. And he's still actively involved in building an observatory for the California Academy of Sciences on some property that the Academy owns up in the region of the Russian River.

JS: Who was his successor as the director?

HW: Well, there were several. And the current one, of course, is Andy Fraknoi. Andy has held the job for a long time, and is particularly strongly involved with teachers and teaching. He was a student, a graduate student, here. And he did take a master's degree, though he never went on to get a Ph.D. He's a very vigorous, active fellow, and I think is doing a good job at the ASP.

JS: You commented a few minutes ago about the work that the ASP has had with public lectures in the past, and I believe you personally were quite involved with that sort of work.

HW: Oh yes, everybody was at some time or another. Yes, I usually gave a few. They were called Morrison lectures, and they were supported; they would pay for transportation and expenses to talk to amateur societies in particular. The fund, the Morrison fund, is still there. It was a gift of – well, I think many things were given by Alexander Morrison, and then May Morrison. I think it was Alexander F. Morrison; yes, Alexander F. Morrison gift. That's the same foundation that provided the music building here, for example, on the campus. The lawyer who was the trustee for the fund was interested in astronomy, and it was he who really directed the money to a considerable extent. So there was originally a gift of twenty-five thousand dollars to the ASP to set up this lectureship for amateur societies, or schools or other groups.

We tried over the years a number of different methods of reaching the public. For a while there was an effort to go out to schools, and talk to science classes, and special meetings, and so on. That somehow didn't work out too well. I don't know exactly why. Perhaps it was the wrong way to reach those classes. Anyway, it didn't work out too well. But there were many lectures for amateur societies, and groups of all kinds that would simply ask – knowing about the gift to the ASP – would ask for a lecture, especially if there were some astronomical event about to take place. I'm not sure it brought in too many members to the ASP, but I think it was a good public service to let the public know about what was happening in astronomy.

JS: Did you yourself enjoy giving lectures and interacting with these groups?

HW: Yes, it was sort of fun. Yes. The people are generally so enthusiastic about astronomy. It's a pleasure to meet them. And I must say, I made many friends during that time. I had the opportunity to meet many of the amateur astronomers from this larger area; from, oh, probably over into the Valley, and up as far as Sacramento, and down as far as San Jose, and over in Marin, and Palo Alto. Various places around here. And Oakland – the Chabot Observatory.

The Trumpler Award

JS: One of the ASP's activities is the awarding of the Trumpler Award, which was established as a memorial to your father-in-law.

HW: Yes.

JS: Were you involved in establishing that memorial?

HW: Yes, I was. It's not a large sum of money. Most of it came from the Trumpler family in Switzerland. The family there had, at that time, spinning mills and weaving mills, and were quite well established. And they gave a few thousand dollars to a fund for Trumpler. It was not specified as to what it would be. But I had proposed – not immediately, because the funds simply stayed in the ASP as an undesignated fund for a number of years – but during this time of change that you were asking about a little earlier, it seemed to me that it would be better to use those funds for things that would bring some recognition to the Astronomical Society of the Pacific, and that would accomplish something – some good of one kind or another – for astronomy somewhere. Well, Trumpler, though he started out as a pure research astronomer, became very interested in teaching. I think he probably enjoyed – toward the end of his life – he probably enjoyed teaching more than he enjoyed research. Though he always did research, as I mentioned; right up to the day he died. He did enjoy teaching very much. He enjoyed working with students, and having Ph.D. students, and so on.

So it seemed that it would be appropriate to use that fund somehow to commemorate, or to reward, good work by students, and to somehow commemorate Trumpler's interest in teaching. And so there was a proposal to have a Trumpler Award for the best – quote, "the best" – thesis of a year. And there are certain stipulations about publication, and so on. But I think it's been a successful award. I think it has been very nice for the students, and I think that the recognition of the students who are chosen for receiving the Trumpler Award, do have a first sort of feather in their caps. And then they give a talk at the annual meeting of the ASP. So I think it has worked out pretty well. And I think Trumpler would be pleased if he could know about it.

JS: I believe one of the early winners of the Trumpler Award was a member of this department, Hy Spinrad?

HW: Yes, yes. The first of many awards that he's won.

JS: Beyond the initial establishment of the memorial fund, have you had any real involvement with the Trumpler Award, or deciding how that would be used?

HW: No, I've never been involved with it. There is a solicitation that goes out quite early among the various departments of astronomy that award degrees. Departments meet – we met here just a few weeks ago – to nominate a candidate. The nominations then go to a committee that reads the theses, or reads papers that have been the result of the theses, and reach a decision as to who will get it. I've never been involved with its award at all. But I've always been pleased with the people they gave it too.

JS: Well, among your other financial assistance or oversight to astronomical organizations, in addition to the ASP, you worked with the American Astronomical Society.

HW: Yes.

JS: And you were the treasurer or acting treasurer for various intervals of time?

HW: Yes, I guess it was ten or eleven years altogether. I really had no intention of doing that for such a long time when I first became acting treasurer to take over from Bill Howard, who had to give it up. It was an interesting job, and the thing that I most enjoyed was going to the meetings. I went to all the AAS meetings during those years, and that was a real pleasure. On the other hand, it was a real chore to have to do all the financial things, especially to keep the accounts straight. At first I did a lot of the bookkeeping. That was drudgery, but it had to be done. Then gradually we got this shifted over, and had a professional bookkeeper in the society. Again, at that time, it seemed important to sort of increase the level of activity of the society. In fact, we got a very much more active executive officer.

It started – when I first came, there was an executive officer. There had been one for several years. But again, it was a slightly inactive society. The executive officer, who was a very nice guy, Hank Gurin, simply sort of kept the place running, but there wasn't any great push to do things, and to make it an active society. That gradually changed; we managed to get that changed, and then Peter Boyce became the executive officer. He has certainly – it's now centered in Washington, and it participates in all kinds of activities to further funding of astronomy, and in keeping the astronomical community informed as to what is going on in the way of funding, and in grants, and things of that sort. It's just become a very much more active society, and I think it's going fine.

It also is quite well funded. The journals are doing extremely well. They were always problems, and at one stage in the game the *Astrophysical Journal* was technically broke. That caused, at least – not that the public knew anything, or the astronomers knew anything about it – but it caused great difficulty among the officers of the society. And we had to make a variety of changes, and we had to change the whole way in which budgets were reached, and were determined, and set up, and so on. At that point there was a marked change in the way that the place operated.

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JS: So you were commenting how the financial difficulties of the *Astrophysical Journal* led to some changes.

HW: Well, in the ways the budgets were set up, and the sorts of analyses that went on before the budgets were finalized for the publications. To the best of my knowledge – I haven't been

treasurer for three or four years now; four years; maybe five – I think they have an adequate reserve fund. For these publications there really needs to be a reserve fund that would keep them running for at least a year or a large part of a year, if they ever had to shut down. There are real dangers in the funding of these journals, because they involve large amounts of money. By now it's probably about two million dollars a year for the *Astrophysical Journal.* It was a million and a half or so when I was there, and it was going up rapidly. In large part because, well, costs are all increasing, but the amount of publication – you know how many pages there are in the *Astrophysical Journal* each year.

It used to be that the *Astrophysical Journal* for the year would take up four inches, or five inches, or maybe even six inches on your shelf. And now it's more nearly six feet! You know, there are just so much paper that the cost is really very substantial. And that is paid for almost completely – there are certain formulas about: the subscription has to pay for a certain part of it, etcetera, etcetera – but most of it comes from the page charges. Now, the page charges come out of grants. And at one time there was a rule set up, or a new procedure set up, that at least grants from certain of the military services would not pay publication charges. You had to publish anything that was published in their own journals. Well, you can imagine how much circulation that would get in the astronomical community. Fortunately we had friends in the right places at the right time. The secretary of the air force was, in fact, at that time a good friend of a lot of us, and we managed to get that squashed. But that could have wreaked havoc with the astronomical publications.

JS: Approximately when was that an issue?

HW: Oh, probably of the order of ten years ago or so. And though the publications are doing fine as long as the money continues to flow, in those categories that support the publications, it's fine. But in a very real sense, they're on a tightrope – they're walking a tightrope. One doesn't realize that, but it is true. And for that reason I think it's terribly important to have an adequate reserve in case of this kind of emergency, if in periods of financial stress the federal government, which in the long run supplies most of the money, would change its rules of operation. You have to publish all your astronomical papers and astrophysical papers in some government publication that deals with something or other – it could be difficult. So there were always tense moments; there were tense moments in some parts of that. And it was necessary to analyze budgets, and write reports, and do all kinds of things of that sort. And not everyone was sympathetic to some of these things, some of these ideas of having enough money to take care of emergencies, and there were internal struggles of a variety of kinds.

The *Astronomical Journal* has had some problems in the last few years. Its problems arise because of its success. The volume of publication in that journal has increased enormously in the last few years, and that means that costs of printing go up. Everything

goes up. And while the page charges pay for most of it, there has to be a balance between page charges and subscriptions and so on. And the sum total has provided a deficit at times. So that means it's necessary to increase both – subscription prices, and no one wants to do that, especially libraries, which pay the large sums of money. And so there's always a great problem.

I think the whole scheme of publication has to change within the next few years. I've been trying to get changes made – no, I *was* trying to get changes made while I was still an officer of the society – but there is great reluctance to change anything that exists. It's very hard to get people to move. It's also very hard to get people to think of things in new ways. We may have to give up some of the consistency of type face and consistency of things in the *Astrophysical Journal.* We may have to have a less beautiful journal that is cheaper to produce. But there is great reluctance on the part of the – has been, maybe there isn't any more – on the part of the University of Chicago Press, and the editor of the journal, who loves it the way it is.

I think sometime we'll have to think about having papers submitted in printable form, just the way you do for abstracts, and so on. We may insist that – whoever it is in control may insist that TEX, or LaTEX, or some of those be used in the preparation of it, and that it conform to rules and regulations as to type and form; it can be edited and be redone by the author, etcetera. The journal can still edit the thing. And if you turn it in yourself this way in printable form, you have a very low page charge. And if you don't, it's a thousand dollars a page, or some enormous and unreasonable amount, to try to get people to go over into the way, everybody does it in a way, that will make the publication of the journal cheap, much cheaper than it is now. Or that it be available on-line on your computing system, or a whole variety of ways that we have to think about to change the cost of these publications, which is really going out of sight in many ways.

Well, there are all kinds of things that need to be thought about for the future by people who are involved with these things. They are being thought of to some extent, but I would certainly love to see them looked at in a more vigorous way than they are.

JS: When you took on the job of treasurer for the AAS, did the condition either of the finances or the way the finances were kept track of require improvement or change when you took them over?

HW: Oh yes, they were improved and changed. Witness the fact that in accounts, we went over to a professional accounting system in the office. And the investments were changed around. I must say, I was treasurer at a very fortunate time, that interest rates were particularly high, and so it was possible to improve the finances of the society with no risk, and to improve them very markedly. So mostly, I simply rotated the funds available to the society through very high-paying certificates of deposit. There is some stock, that have been given to the

society as gifts, and I did retain those. There's some IBM stock, though I think they should have gotten rid of it a little earlier. And there were some investments that were made in various funds that were especially designed for nonprofit organizations. But the society was in pretty good shape. And the most important thing, I think, was that after many battles the reserve funds were up into the amounts that were required. We got several changes made in by-laws to accomplish that. The percentages of budget are stated in the by-laws of the society, but they were originally very low.

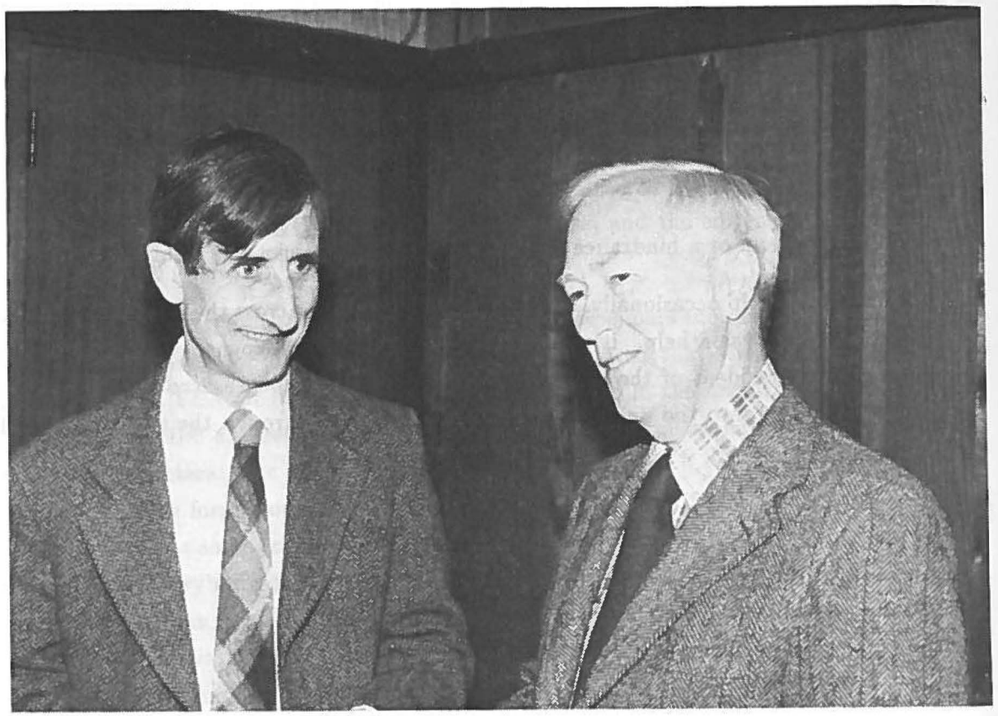
Well, that was a long sojourn into the operations of the society. As I say, I did enjoy going to the meetings. That was my benefit from the work.

JS: During part of the interval you were treasurer, I believe Ivan King was the president.

HW: He was president for two years while I was treasurer, yes.

JS: Was that a help or a hindrance to have the president so close at hand?

HW: Oh, it was a help occasionally, because I could just walk across the building and talk to him. But – yes, it was a help. It was very easy to set up his expense accounts and things like that. The president of the society always has a small expense account, and Ivan chose to put it directly into the university, and he then worked through the secretaries in the office here.



Physicist Freeman Dyson with Harold F. Weaver, on the occasion of Dyson's visit to Berkeley as Hitchcock Lecturer, circa 1985.

VII. The Evolving Astronomy Student Community; New Work with Chabot Observatory

Berkeley Astronomy Students and the Blues Chaser

JS: There's one more topic I wanted to ask you about, which is sort of a jump in subject, but it's prompted in part by something you showed me before this discussion started, and this is a daybook from the Student's Observatory. And you also had told me about the Blues Chaser. Could you tell me a little more about the Blues Chaser?

HW: Yes. This daybook, I think, is not so interesting, because it wasn't maintained for very long. But it was a day-to-day account of what was done in the observatory. It starts in 1903, if I remember correctly. And it has a few – I guess really only a couple – of clippings in it. But it was not maintained, and you really don't get a feeling for what went on over a long period of time. But there was a book that used to be taken care of by the graduate students of the department. It was called the Blues Chaser. You were supposed to read it and laugh when you had some sad occasion, hence the name. And it was full of all kinds of – *is* full; it still exists in an office here, somewhere in this department. And it's full of all kinds of pictures, and accounts of activities, and jokes, and funny things that happen in the department, and all meetings of the humorous society that used to exist here. It was called Delta Alpha Delta Delta. And those are the residuals from the positions of a comet, because originally this place was a comet-orbit mill. And everybody computed comets, and were always worried about the residuals, the differences between calculation and observation. So there was a society that met once or twice a year, and people were inducted into this society, and they were supposed to give humorous papers for the assembled crowds at the time of the meeting. I don't know if any of the papers still exist, are still available. But some of them were pretty funny, and some of them were pretty sad.

Anyway, that volume, which I think we – as I was mentioning to you before we started – I think we should probably put in the Bancroft Library so it is preserved as a memento of the early history of the students in the observatory. It has things from really a great many astronomers who are well known, or used to be well known, because it covers a long period of time. And has all kinds of personal stories, and jokes about them; things of that sort. Interestingly, Lawrence Aller was one of the ones who really had some pretty good jokes and things in there. One might not think of Lawrence as being very funny, but he does have a very good sense of humor, and there are quite a few good things that he put into it that we ought to look at together sometime, if we can break the document loose from its current possessor.

There was another thing that I mentioned to you, from those early days, and I ought to get them down so you can look at them. And that is the recordings of heights of astronomers

on the door frame in the old observatory, which I was telling you I had saved, managed to go in and pry off the walls just before the building was demolished. I didn't get quite everything that was there, because there were a few on the door. I didn't want to walk off with a door; I could walk off with a couple of pieces of wood, but not a whole door. Those are interesting. Again, there are heights, just marks for the height, and the date, as I recall, and initials. And the initials are identified in the Blues Chaser. So one could trace back the height. It goes over quite a range. And of course, the students couldn't refrain from a few jokes, so T. J. J. See is up somewhere off the diagram, and used to be up near the ceiling, and so on.

JS: Maybe you could briefly say just who T. J. J. See was, since I don't believe he was a student here.

HW: No, he wasn't a student. T. J. J. See was an astronomer. It must have been from – oh, I'll guess – 1910 to perhaps late 1930s, or maybe even to the '40s. That, I don't know about for sure. I remember seeing him occasionally in the late 1930s here on the Berkeley campus. He was an astronomer who started out as a good one, and did some very good work. He was at the Lowell Observatory. But he became, to put it bluntly, a little bit nutty. And he wrote many books on all kinds of topics in which he would solve the most horrendous equations, and he was always talking about wave theory. Everything was waves. And he would write wave equations for everything and solve them in these many, many books that he published, each of which was quite thick. They were always specially placed in the Student's Observatory, and they were worth going and reading when you had the blues and wanted to have a laugh, maybe. But, interestingly he started as a good astronomer, and he did some things – I was trying to think – he did some things in stellar structure, and so on. He was the astronomer at the Naval Observatory at Mare Island. He ran that station.

JS: In charge of their timekeeping service?

HW: Yes. And he just went off the deep end a little bit, and was an interesting character after that. He had a motto, a medallion and a motto on his books. Now let me see if I can remember that. It was written in Greek. "God is in geometry – T. J. J. See is a geometer."

JS: My understanding is that in the end, through the course of his career, he managed to alienate a good deal of the astronomical community.

HW: Oh yes, of course. Of course. Yes. And people would hide when he came here. That is, at least the faculty would try to hide. They didn't want to talk to him. Yes. One of the many characters at the Department of Astronomy at Berkeley.

JS: Coming back to the Blues Chaser and other things that the students did in that time, the marking of heights and that book are two of the things that have disappeared from amongst

the graduate students today. Do you think there's been a big change in the atmosphere amongst the graduate students at Berkeley?

HW: Well, I think, yes. Of course I can't know – I don't know the graduate students as well as when I was a graduate student. So I can't say for sure about that. But I do think there is, yes. I think they have always worked hard, but I think they work harder now in lots of ways. And the reason is that there is such a vast difference in research. I guess we've talked about that before; that the department was not originally, or at least in the last years of Leuschner's reign – the Chief's reign, he was always called "the Chief" – there wasn't very much research in progress then, and there is an enormous amount now. And the students now participate in that, and I think are expected to do research, and expected to publish papers and so on.

Earlier students would publish very little. There might be some notes in the *PASP*, the *Publications of the Astronomical Society of the Pacific*, but almost never in a journal like the *Astrophysical Journal*. Lawrence Aller may have had a paper in the *Astrophysical Journal*, some work that he had done under Menzel. But you just didn't do things like that. You just didn't publish. The faculty didn't publish to any extent. Now there's this drive for publication, and so on – I think makes a great deal of difference in the students. I think the students are also much brighter. I think they're much better prepared and much brighter. The students now are a wonderful group. I think there's never been such a group.

Whether or not they have any of the fun things or not, I don't know, you see. Maybe you all have parties every weekend, I don't know. But you don't do it here, at least. There are not many things here. Whereas there used to be parties, or meetings – supposedly humorous meetings – at the observatory. As with this humor organization, Delta Alpha Delta Delta, where the student, for example, one of the things was that when a student was inducted into this after his humorous paper, there was an oath, and everyone had to take this oath. And I can't remember all of it, but some of the things: "I swear by the bones of Napier that I will ... ," something, "I will not put my feet on a log table." And, "I will not get sunspots on my clothes. I will ...," and a variety of things like this, you see, that went on. Something about, "I will not close the open clusters. I will ...," etcetera, etcetera, etcetera, you see. I can't imagine anything like that here at the present time. It's a different atmosphere. Perhaps there was a kind of innocence then that isn't present now. It was a different atmosphere.

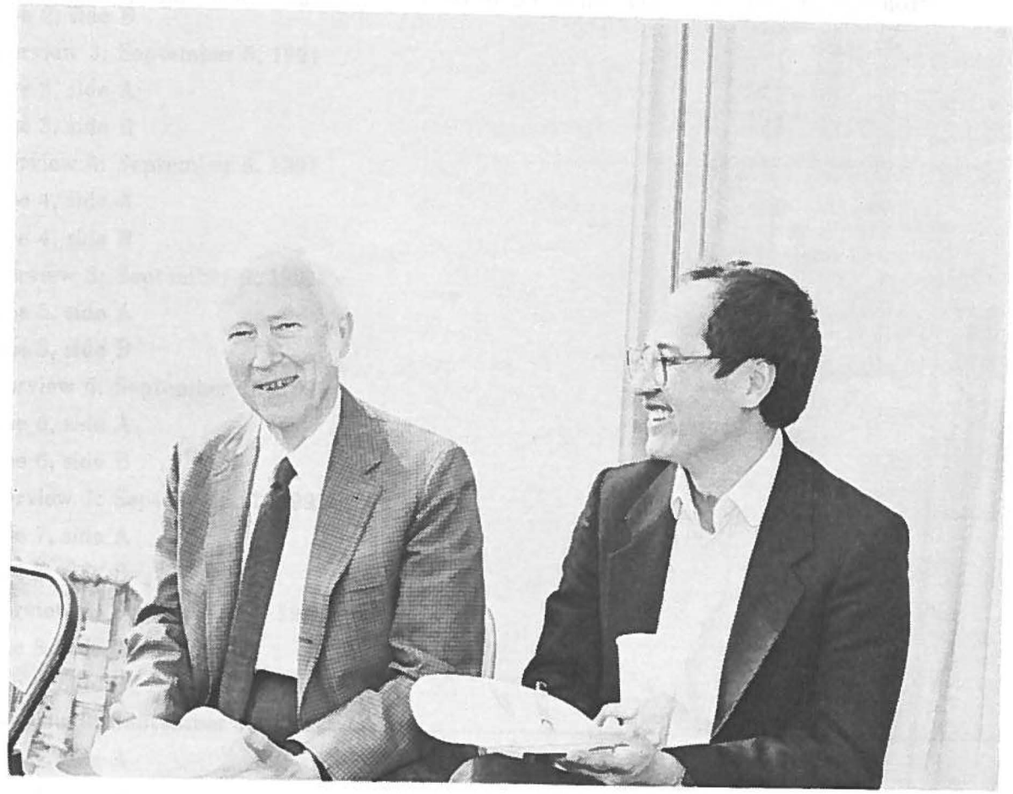
Chabot Observatory and Science Center

JS: Well, I think we've gone through my entire list. Have we touched on anything on which you'd like to comment further?

HW: Well, you certainly have covered things thoroughly. You know too much about me, maybe. [laughter] The one thing I might mention is, I seem to be at it again, in that I have become deeply involved in the rebuilding of the Chabot Observatory and Science Center, which I think, in its new manifestation, will be an important adjunct to the Oakland public schools, and perhaps to a lot of other K through twelve schools around here. I've been on the board of directors of that now for a year or so, and was the chairman of the committee to choose the new director. We've got him; he'll be here on the twenty-third of this month to start his job. And now we have to raise ten or fifteen million dollars to build a new building. So that'll be an interesting problem. I'm going to try to get not too deeply involved, though I seem always to do it. Because I think, in this case, I'm willing to do some work because I think it is important for the improvement of science opportunities for the students who are in this area. So that's my next big project.

JS: All right. Well, thanks very much.

HW: You're welcome.



Berkeley astronomers Harold Weaver and Frank Shu, circa 1985.

Addendum: March 29, 1993

HW: In reading this oral history and, in a sense, reliving the moments and incidents described, I often thought how much the success of the Radio Astronomy Laboratory was due to the devoted work of the staff. I would like especially to acknowledge the efforts of David Williams and Tap Lum. They came to the laboratory in 1959, shortly after it began operation. Their contributions to the laboratory have been very great. David Williams retired as a Research Astronomer in 1990; Tap Lum still works in the lab as a Senior Development Engineer.

Tape Guide – Harold F. Weaver

Appendices

Harold F. Weaver

A.B. University of California, Berkeley 1940

Ph.D. University of California, Berkeley 1942

Lick Observatory Fellow 1941-42

National Research Council Fellow, Yerkes Observatory, University of Chicago and at McDonald Observatory 1942-43

Technical Aide, National Defense Research Committee, Washington, D.C. 1943-44

Physicist, Radiation Laboratory, University of California, Berkeley 1944-45

Assistant Astronomer, Lick Observatory 1945-1950

Associate Astronomer, Lick Observatory 1950-51

Associate Professor of Astronomer, University of California, Berkeley 1951-56

Professor of Astronomy, 1956-1988

Director, Radio Astronomy Laboratory, University of California, Berkeley 1958-1972

Chairman, Department of Astronomy 1979-1980

Professor Emeritus of Astronomy, 1988-present

Member of:

The International Astronomical Union
- Member U.S. National Committee 1947-1950

American Astronomical Society
- Councilor 1954-57
- Acting Treasurer 1977-78
- Treasurer 1978-1987
- Acting Treasurer 1987
- Member, Executive Committee 1977-1988

Astronomical Society of the Pacific
- Member, Board of Directors 1969-1972
- First Vice President 1969-1970
- President 1970-72
- Finance Committee
  - Member 1960-1990
  - Chairman 1972-1990
- Treasurer, 1987-1990

Union Radio-Scientifique International

Associated Universities Incorporated
- Member-at-Large, Board of Directors 1958-1962

American Institute of Physics
- Member, Governing Board 1980-83
- Member, Fiscal Advisory Committee 1981-83
- Audit Committee
  - Member 1980-83
  - Chairman 1983
- Investment Advisory Committee
  - Member 1984-1990
  - Chairman 1987-1990

National Science Foundation
- Member, Division Panel on Astronomy, Physics, Chemistry, and Mathematics: Advisory to the Director 1962-65

Special Panel to Review Canadian Radio Astronomy for the Canadian Government
- Member

Squaw Valley Creative Arts Society
- Member, Board of Directors 1980-present
- Treasurer 1980-84

Exploring System Earth Consortium of Universities
- Domain Expert in Dynamics (Computer aided teaching) 1983-present

Chabot Observatory and Science Center
- Member, Board of Directors 1990-present

University Service:

Statewide Service
- Scientific and Academic Advisory Committee for the Livermore and Los Alamos Laboratories (Advisory to the President and Regents of the University of California)
  - Member 1972-1986
  - Chairman 1981-1986
- Committee on Educational Policy 1957-1960

Berkeley Campus Service
- Committee on Educational Policy 1955-58
- Committee on Buildings and Campus Development 1953-56
- Various Building Planning Committees 1953-56
  - Earth Sciences
  - Education
  - Social Welfare

- College of Letters and Science Committee on Courses

Departmental Service
- Chairman 1979-1980
- Departmental Committees
  - Teaching
  - Admissions
  - Curriculum Revision
  - Space Planning and Allocation
  - Building Plans
  - Obtained outside grant to rebuild and furnish room for Faculty-Student Commons
  - Obtained outside grant to furnish meeting room for Theoretical Group
  - Obtained outside funding to furnish Chairman's Office

Other Professional Activities:

World Book Encyclopedia
- Member, Physical Science Advisory Panel 1970-1988

Berkshire Technologies, Inc.
- Secretary Treasurer 1982-present

Awards and Honors:

- Berkeley Citation 1988
- G. Bruce Blair Medal 1989 (Western Amateur Astronomers Association)

The Radio Astronomy Laboratory

Founding Director of the Radio Astronomy Laboratory 1958-1972.

The Radio Astronomy Laboratory was established and the Director of the Laboratory appointed by the Regents in July 1958.

The period 1956-1958 was spent developing sources of funds to establish a radio astronomy observatory and an operating unit on the Campus. Funds obtained: $206,000 from the University; $368,285 (1958), $250,000 (1958) from ONR.

Extensive site tests during 1958-1959 led to the choice of Hat Creek as the location of the observatory. Construction was started in August 1959. This involved construction of roads and wells, installation of power, siting and building of three houses, one fully equipped dormitory for six persons, one shop, two laboratory-control buildings, and antennas of 33 feet and 85 feet aperture.

The first stage of construction was completed in July 1960 with the tests of the 33-foot antenna.

The 85-foot antenna structure was completed in April 1961; tests and installation of controls continued until June 1962, when observations were first made.

The budget of the Radio Astronomy Laboratory was shared originally by the University and ONR. In 1960 the budget was $233,600 ($158,770 ONR, $74,830 University), and increased annually to $530,000 in 1966 (UC, ONR, NSF, USAF), to $650,000 in 1972 (UC $184,000, other sources $466,000).

Policy called for the Laboratory to be open to all qualified users in the University System. Participation in the work of the Laboratory was expanded to include faculty from Physics, Chemistry, Electrical Engineering, the Space Sciences Laboratory.

An important step was taken in 1965 when S. Silver, Director of the Space Sciences Laboratory and Professor of Electrical Engineering, with the cooperation of the Radio Astronomy Laboratory, moved a 10-foot antenna to Hat Creek and developed a third radio telescope site at the observatory. This project and its growth through the efforts of Professor W. J. Welch (now Director of the Radio Astronomy Laboratory) developed into the current major program of cm/mm interferometry at Hat Creek.

Among the earlier significant contributions from the Radio Astronomy Laboratory were the extensive surveys of neutral hydrogen (by Weaver and Williams, 1973, 1974, and by Heiles and Habing, 1974), the discovery of the first maser in space (by Weaver, 1965), and the discovery of ammonia (by Cheung, Rank, Thornton, Townes, and Welch, 1968) and water (by Cheung, Rank, Townes, and Welch 1969) in the interstellar medium.

PUBLICATIONS

by Harold F. Weaver
Radio Astronomy Laboratory
University of California, Berkeley, California

1941

1. Aspect of the Heavens (Monthly Feature), *PASP*, **53**, 286-288, 325-327, 1941.

1942

2. Aspect of the Heavens (Monthly Feature), *PASP*, **54**, 29-30, 105-106, 1942.
3. Aspect of the Heavens (Monthly Feature), *PASP*, **55**, 96-98, 147-148, 1942.

1943

4. The Solar Parallax, *ASP Leaflet*, No. 169, 1943.
5. "J. J. Thompson" by Lord Rayleigh (review), *Sky and Telescope*, No. 26, 16-17, 1943.
6. Calcium II Emission in ν Sagittarii, *ApJ*, **98**, 131, 1943.
7. Classified reports for NDRC on Optics and Vision.

1944

8. The Spectrum of Nova Puppis 1942, *ApJ*, **99**, 280-294, 1944.
9. "Electronic Physics" by Hector, Lein, and Scouten (review), *Sky and Telescope*, No. 36, 15-16, 1944.
10. Classified report for NDRC; also for Radiation Laboratory on ion beams, magnetic field problems, and spectroscopy.

1945

11. "Optical Instruments", by E. B. Brown (review), *Sky and Telescope*, No. 50, 16-17, 1945.
12. Classified reports for NDRC and Rad. Lab.

1946

13. The Development of Astronomical Photometry, *Pop. Ast.*, **54**, Nos. 5, pp. 211-230, 6, pp. 287-299, 7, pp. 339-351, 8, pp. 389-404, 9, pp. 451-464, 10, pp. 504-526; reprinted as No. 11 in the series "Astronomical Summaries", 199-293.
14. New 'Metallic Line' Stars (abstract), *PASP*, **58**, 246-247, 1946.
15. "The Milky Way" by Bok and Bok (review), *PASP*, **58**, 68-69, 1946.

16. Optical Techniques, Chapter 9, pp. 389-435, of Summary Technical Report of Division 16, NDRC, Vol. I, Optical Instruments, Washington, DC, 1946.

17. "Scientific Instruments" ed. by H. J. Casper (review), *Sky and Telescope*, No. 65, 16, 1946.

1947

18. The Visibility of Stars without Optical Aid, *PASP*, **59**, 232-243, 1947.

19. The Determination of Magnitudes by Comparison with a Standard Field, *ApJ*, **106**, 366-379, 1947 (Contributions from the Lick Observatory, Ser. II, No. 20).

1948

20. The Relative Efficiencies of Spectrographs of Moderate Dispersion at the Prime, Cassegrain, and Coudé Foci of Large Reflectors, *PASP*, **60**, 79-97, 1948.

21. Chapter in *The National Nuclear Energy Series* of the U.S. Atomic Energy Commission (63 pp., material classified).

22. "Elementary Nuclear Theory" by H. Bethe (review), *Sky and Telescope*, **7**, 278, 1948.

1949

23. Color Excesses, Total Photographic Absorption, and the Distance of the Dark Cloud in the Aquila Region of the Milky Way, *ApJ*, **110**, 190-204, 1949 (Contributions from the Lick Observatory, Ser. II, No. 24).

1950

24. Spectral Anomalies in F and G Dwarf Stars, *PASP*, **62**, 50-55, 1950.

25. Neutral Filters for Photographic Photometry, *PASP*, **62**, 167-171, 1950.

1951

26. The Transmittance of a Prism, *Journ. Opt. Soc. Am.*, **41**, 331-335, 1951.

27. The Identification of D'Agelet's Nova Sagittae of 1783, *ApJ*, **113**, 320-323, 1951 (Contributions from the Lick Observatory, Ser. II, No. 33).

28. Galactic Orbits of Stars in the Vicinity of the Sun (with R. J. Trumpler), *PASP*, **63**, 172-174, 1951.

1952

29. Notes on the Spectrum of Gamma Ursae Minoris, *ApJ*, **116**, 541-545, 1952.

30. Spectral-Type, Magnitude, and Color-Index Relations in the Galactic Star Cluster in Coma Berenices, *ApJ*, **116**, 612-639, 1952 (Contributions from the Lick Observatory, Ser. II, No. 44).

31. "Astrophysics" ed. by J. A. Hynek (review), *Sky and Telescope*, **11**, 255, 1952.

1953

32. Regional Variations in the Velocity Dispersion of Early B-Type Stars, *PASP*, **65**, 132-138, 1953.

33. Magnitude, Color, and Spectral-Type Relations in the Galactic Cluster M 39, *ApJ*, **117**, 366-376, 1953.

34. B Stars and Spiral Structure in the Larger Neighborhood of the Sun, *AJ*, **58**, 177-195, 1953.

35. *Statistical Astronomy* (with R. J. Trumpler). The University of California Press, XX + 644 pp., 1953.

1954

36. The Distance to the Galactic Center and the Zero Point of the Cepheid Period-Luminosity Relation, *AJ*, **59**, 375-384, 1954.

37. Motions of Stars in the Galaxy, Berkeley Conference Notes, 257-265, 1954.

1955

38. The Value of the Oort *A* Parameter Derived from Radial Velocities of Cepheid Variable Stars, *AJ*, **60**, 202-208, 1955.

39. The Value of the Oort *A* Parameter Derived from Radial Velocities of B Stars, *AJ*, **60**, 208-210, 1955.

40. Mathematical Bias in the Oort *A* and *B* Parameters Derived from Proper Motions, *AJ*, **60**, 211-216, 1955.

41. The K Effect in Stellar Motions, pp. 228-238 in *Vistas in Astronomy*, vol. 1, ed. by A. Beer, Pergamon Press, London, 1955.

42. Numerical Values of the Oort Galactic Rotation Parameters (abstract), *AJ*, **60**, 181-182, 1955.

43. "The History of the Telescope" by H. C. King (review), *Sky and Telescope*, **15**, 83, 1955.

1956

44. The Galactocentric Circular Velocity and Corrections to the Precession Constant and the Motion of the Equinox (with H. R. Morgan), *AJ*, **61**, 268-271, 1956.

1957

45. Robert Julius Trumpler 1886-1956 (with Paul Weaver), *PASP*, **69**, 304-307, 1957.

46. "Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability," Vol. III: Contributions to Astronomy and Physics, ed. by J. Neyman (review), *PASP*, **69**, 370-371, 1957.

47. A Physical Model of N. Aql. 1918, *Science*, **128**, 1149, 1957.

1959

48. Spiral Structure in the Galaxy, pp. 40-64 in IAU Symposium No. 7, *Second Conference on Coordination of Galactic Research*, ed. by A. Blaauw, G. Larsson-Leander, N. G. Roman, A. Sandage, H. F. Weaver, and A. D. Thackeray, Cambridge at The University Press, 1959.

1961

49. The Scale of the Galaxy: A Symposium. I. Introduction, *PASP*, **73**, 88-94, 1961.

50. Photoelectric Observations of Northern Cepheid Variable Stars (with D. Steinmetz and R. Mitchell), *Lowell Obs. Bulletin*, **5**, 30-53, 1961.

1962

51. Photographic Photometry, pp. 130-179 in *Handbuch der Physik*, vol. 54, ed. by S. Flugge, Springer-Verlag, Berlin, 1962.

52. *Statistical Astronomy* (with R. J. Trumpler), (reprint) 644 pp., Dover Publications, Inc., New York, 1962.

53. Linear Diameters of Galactic Star Clusters (with David Steinmetz), *PASP*, **74**, 125-128, 1962.

1964

54. Non-Circular Motions in the Galaxy as Exhibited by Very Young Stars, pp. 92-99, in IAU-URSI Symposium No. 20, *The Galaxy and the Magellanic Clouds*, ed. by F. J. Kerr and A. W. Rodgers, Australian Academy of Science, Canberra, 1964.

55. On the Comparison of Spiral Structure as Delineated by Gas and by Stars, pp. 158-160, in IAU-URSI Symposium No. 20, *The Galaxy and the Magellanic Clouds*, ed. by F. J. Kerr and A. W. Rodgers, Australian Academy of Science, Canberra, 1964.

56. Mars Observations from Stratoscope II (with R. E. Danielson, J. E. Gaustad, M. Schwarzschild, and N. J. Woolf), *AJ*, **69**, 344-352, 1964.

57. A Recalibration of the Absolute Magnitudes of Early Type Stars Classified on the MK System (with A. Ebert), *PASP*, **76**,6-13, 1964.

58. The OH Absorption Profile in the Direction of Sagittarius A (with D. R. W. Williams), *Nature*, **201**, 380, 1964.

1965

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61. Some Problems of Planetary Radio Astronomy, pp. 371-399, in *Solar System Radio Astronomy*, ed. J. Aarons, Plenum Press, New York, 1965.

62. Observations of a Strong Unidentified Microwave Line and of Emssion from the OH Molecule (with D. R. W. Williams, N. H. Dieter, and W. T. Lum), *Nature*, **208**, 29-31, 1965.

1966

63. Secular Variations in the Radio-Frequency Emission of OH (with N. H. Dieter and D. R. W. Williams), *AJ*, **71**, 160, 1966.

64. Values of the Einstein A Coefficients for the Λ Doublet Transitions of the Ground State of OH (with W. M. Goss), *AJ*, **71**, 162-163, 1966.

65. OH Radio-Frequency Emission Lines Near Very Bright HII Regions (with D. R. W. Williams and N. H. Dieter), *AJ*, **71**, 184-185, 1966.

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1967

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1968

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1970

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1971

74. Award of the Bruce Gold Medal to Professor Jesse L. Greenstein, *PASP*, **83**, 243-247, 1971.

1972

75. Search for Interstellar Furan and Imidazole (with R. L. Dezafra, P. Thaddeus, M. Kutner, N. Scoville, P. M. Solomon, and D. R. W. Williams), *Astophysical Letters*, **10**, 1-3, 1972.

76. The Award of the Bruce Gold Medal for 1972 to Professor J. S. Shklovsky, *Mercury*,**1**, no. 4, pp. 6-7, 1972.

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1973

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1974

79. Some Aspects of Galactic Structure Derived from the Berkeley Low Latitude Survey of Neutral Hydrogen, pp. 573-586, in IAU Symposium No. 60, *Galactic Radio Astronomy*, ed. by F. J. Kerr and S. C. Simonson, Reidel Publishing Co., Dordrecht, Holland, 1974.

80. Space Distribution and Motion of the Local HI Gas, pp. 423-440, in *Highlights of Astronomy*, vol. 3, ed. G. Contopoulos, Reidel Publishing Co., Dordrecht, Holland, 1974.

81. The Shell of V603 Aql and the Early Stages of the Nova Event, pp. 509-532, in *Highlights of Astronomy*, vol. 3, ed. by G. Contopoulos, Reidel Publishing Co., Dordrecht, Holland, 1974.

82. The Berkeley Low-Latitude Survey of Neutral Hydrogen: Part II. Contour Maps (with D. R. W. Williams), *Astron. and Astrophys. Suppl.*, **17**, 1-249, 1974.

83. The Berkeley Low-Latitude Survey of Neutral Hydrogen: Part III. An Extension to Latitude ±30°; Profiles and Contour Maps (with D. R. W. Williams), *Astron. and Astrophys. Suppl.*, **17**, 251-445, 1974.

1975

84. Steps Toward Understanding the Large-Scale Structure of the Milky Way; Part I. *Mercury*, **4**, no. 5, pp. 18-24, 1975.

85. Steps Toward Understanding the Large-Scale Structure of the Milky Way; Part II. *Mercury*, **4**, no. 6, pp. 18-29, 1975.

86. Steps Toward Understanding the Large-Scale Structure of the Milky Way; Part III. *Mercury*, **4**, no. 7, pp. 19-30, 1975.

1979

87. Large Supernova Remnants as Common Features of the Disk, pp. 295-300, in IAU Symposium No. 84, *The Large-Scale Characteristics of the Galaxy*, ed. by W. B. Burton, Reidel Publishing Co., Dordrecht, Holland, 1979.

1985

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1988

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UNIVERSITY OF CALIFORNIA
GRADUATE DIVISION

PROGRAMME OF THE FINAL EXAMINATION FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

OF

HAROLD FRANCIS WEAVER

A.B. (University of California) 1940

ASTRONOMY

THURSDAY, MAY 14, 1942, AT 9:00 A.M., IN ROOM 2
STUDENTS' OBSERVATORY

COMMITTEE IN CHARGE:
Professor ROBERT JULIUS TRUMPLER, *Chairman*,
Professor RAYMOND THAYER BIRGE,
Professor CHARLES DONALD SHANE,
Professor RUSSELL TRACY CRAWFORD,
Professor WILLIAM FERDINAND MEYER.

BIOGRAPHICAL

1917 —Born in San Jose, California.

1940 —A.B., University of California.

1940 (Summer)—Volunteer Assistant, Mount Wilson Observatory.

1940–1941—Teaching Assistant in Astronomy, University of California.

1941 (Summer)—Temporary Associate, Mount Wilson Observatory.

1941–1942—Lick Observatory Fellow in Astronomy, University of California.

DISSERTATION

The Absorption of Light and Space Distribution of Stars in the Aquila Region of the Milky Way

A knowledge of the structure of our galaxy must ultimately depend upon detailed studies of many sample areas distributed over the whole sky. The present work is such a detailed study of 14.98 square degrees located at $\alpha=19^h\ 1^m.6$, $\delta=+8°\ 19'$ in the dark rift of the Milky Way in the constellation Aquila. In this investigation the law of absorption of light, and the space distribution of stars are determined to a distance of 1500 parsecs.

The law of color absorption has been determined by three methods. The first method requires magnitude, color indices, and spectral classes for individual stars; the second method employs star counts according to photographic and visual magnitudes; and the third method requires magnitudes and color indices for individual stars. The results obtained from all three methods are accordant. The first and third methods have higher resolution than the second, which is very sensitive to errors in the magnitude scales.

On the basis of the law of color absorption determined by the above methods and an assumption of constant average space density of stars as far as 800 parsecs, the ratio of photographic absorption to color absorption, χ, was obtained from the star counts and van Rhijn's luminosity functions. The law of photographic absorption resulting from the transformation of the law of color absorption through χ, indicates that a cloud with an absorption coefficient of $7^m.20$ per kiloparsec begins at about 130 parsecs and extends to 270 parsecs. Its total absorption is $1^m.1$. From 270 parsecs to 390 parsecs there is only normal absorption. At 390 parsecs a second cloud with an absorption coefficient of $3^m.64$ per kiloparsec begins and extends to at least 1500 parsecs, the extent of the observations. To a distance of 1500 parsecs the total absorption is slightly greater than five magnitudes.

The true space density of the stars, obtained by removing the absorption effects from the fictitious or apparent density law determined from the star counts, is constant as far as 350 parsecs, where it begins to decrease. It reaches a minimum of 0.5 at about 550 parsecs, rises to a maximum of 2.4 at 800 parsecs, and decreases to 1.5 at 1400 parsecs.

A new method for the reduction of photometric plates is devised and discussed.

GRADUATE STUDIES

Field of Study: ASTRONOMY.

Statistical Astronomy. Professor R. J. Trumpler.
Advanced Astrophysics. Professor C. D. Shane.
Celestial Mechanics. Professor C. D. Shane.
Theoretical Astronomy. Professor R. T. Crawford.

Other Studies:

Advanced Physical Optics. Professor R. T. Birge.
Differential Equations. Professor Thomas Buck.

THE BEGINNINGS OF THE HAT CREEK OBSERVATORY AND THE 85-FOOT TELESCOPE

Invited Talk Presented at the June 1993 Meeting

of the American Astronomical Society, Berkeley, California

by Harold F. Weaver

The establishment of the Radio Astronomy Laboratory was not an isolated event in history, simply something that happened at Berkeley. In the global view, it was a part of the rapid evolution of astronomy that took place after World War II; in a more restricted sense, it was one of a whole series of changes in the Department of Astronomy at Berkeley that occurred after 1950. I would like to describe the start of the Lab and the building of the 85-foot telescope in the light of those events.

The current Department of Astronomy at Berkeley began in the year 1950. There had been an astronomy department of considerable distinction at Berkeley since the 1880's, of course, but by the end of World War II it had pretty much run down. Its principal research activity had been the determination of the orbits of comets and asteroids. In 1950 Otto Struve, a power in American astronomy and, many would say, the best observational astrophysicist, joined the faculty at Berkeley with the triple title: Professor of Astronomy, Chairman of the Department, and Director of the Leuschner Observatory. He brought with him from Yerkes John Phillips in spectroscopy, his theorist collaborator SuShu Huang, and the observatory secretary. Louis Henyey, who worked in stellar structure, had come to Berkeley from Yerkes three years earlier. Leland Cunningham, who was appointed to the faculty in 1946, represented the department's traditional fields of orbit determination and celestial mechanics. Struve invited me to transfer from the staff at Lick to Berkeley, which I did in 1951.

It was a time of tremendous change at Berkeley. The entire department turned over in a very short period, mostly in a single year. Course emphasis changed to what was then modern astrophysics. The faculty was strongly research oriented; graduate student hands-on research became an important part of the curriculum. We managed to switch funds that had been allocated for renovation of the old observatory buildings to provide the department with the basic equipment that would then be found in an active, working observatory. The 20-inch telescope was ordered and the design of auxiliary equipment for it started. It was a time of vigorous rejuvenation and of change of direction.

There were other changes. Visitors came. Visiting faculty, invited for a semester or two, provided new points of view. Ron Bracewell came from Australia – the Pawsey, Bracewell book on radio astronomy had just been completed – to give the first course in radio astronomy at Berkeley

during the 1954-55 academic year. W. H. McCrea has written in a very flattering way about his semester at Berkeley in 1956 in volume 25 of *The Annual Reviews of Astronomy and Astrophysics.*

Astronomy itself was undergoing a great change. World War II had taught members of the scientific community how to work in groups and how to spend money. Technological advances made during the war were just waiting to be applied to astronomy. There is no better example of that than radio astronomy, a direct descendant of war-time radar. The first radio detection of the sun was as radar interference during World War II. The first radio astronomers came from outside the ranks of astronomy; they were physicists and electrical engineers who had worked in radar during the war and now saw great opportunities for new astronomical discoveries through the use of radar equipment in both active and passive form. Strangely, the United States played only a minor role in the early post-war growth of radio astronomy, which was an activity primarily of Great Britain, Australia, Holland, and Russia. We had a lot of catching up to do.

By 1954 discoveries were flowing from radio astronomy at ever increasing speed. It was abundantly clear that no aspect of astronomy was going to be untouched by this new development. Berkeley had to become involved in radio astronomy. Struve, with the strong support of the members of the department, requested that the Dean establish a faculty commitee to determine how the University should enter this new field. The members of the committee appointed by the Dean in December, 1954, were Louis Alvarez from physics and Sam Silver from Electrical Engineering (he later established the Space Science Laboratory). I served as the third member of the committee and as chairman.

In mid 1955 we presented a report that strongly urged the University to establish a new faculty position in radio astronomy, and to establish a solar radio observatory for research and teaching purposes. The report was very favorably received. It appeared that we were going to get what we had asked for. But it soon became apparent that the idea of a solar observatory, interesting at first because it would be relatively inexpensive (we innocently thought), and because a new field would have been introduced into the astronomy curriculum, and because there were related interests in several departments, was simply not feasible.

Meanwhile, I had become more and more interested in the 21-cm results on spiral structure produced by the Leiden group. I was convinced that the galactic research I wanted to do in the future would involve radio astronomy. Struve urged me to take on the task of establishing the new observatory. I finally agreed to take the job, but with some provisos: The new radio observatory would be devoted to Galactic studies. It would be a first class research establishment with a large radio telescope and an adequate staff. It would be available to the staff and faculty and to the students we would train in radio astronomy. It would be a much larger observatory than the one we envisioned in the original report. All parties agreed to the proposal. The year was 1956.

The next two years were filled with committee meetings, report writing, discussions of organization charts and possible lines of reporting for the new observatory, planning preliminary

budgets, calming fears, soothing bruised egos, talking to mid-level university administrators, searching for funds. Thankfully, there were some happy times. I had a sabbatical during the 1956-57 academic year which I spent mostly at Harvard. I worked with Bart Bok's radio astronomy group and observed that project close up. The graduate students working with Bok were a truly remarkable group of pioneers. Among them were Dave Heeschen, Frank Drake, Kochu Menon – and there were many others. It was a pleasure to get to know them. I spent a part of my year at DTM [Department of Terrestrial Magnetism, Carnegie Institution of Washington], and I visited all the U.S. radio astronomy projects, met and talked to the people involved, and observed their operations.

The search for funding for the Berkeley project seemed endless. Washington became even more familiar to me than it had been during the war. Our proposals were favorably reviewed, but no one had uncommitted funds of sufficient size to meet the Berkeley needs. Our Russian colleagues came to the rescue. In October, 1957, the Russians launched Sputnik. The U.S. government was galvanized into action. Money flowed. The Berkeley radio astronomy project was funded by ONR [Office of Naval Research] in early 1958. The situation was unique in funding history. At one point ONR actually asked if we couldn't use some more money. Of course we could! Anytime they wanted to send some.

With funding assured, the Radio Astronomy Laboratory was established by The Regents in July, 1958. A small staff was assembled; we immediately started testing promising sites for radio quiet. Hat Creek was chosen. Site plans were developed, utilities installed, buildings started. Detailed equipment plans were developed; we would build a 33-foot antenna in order to gain experience and test equipment while waiting for the large dish – the 85-foot telescope – to be constructed. Philco won the bid for the 85-foot. They were to build and assemble the dish and mount; the Radio Astronomy Lab was to build and install the controls. Philco also built the dish for the 33-foot; Lab personnel designed and built everything else. We all learned a great deal in the process. Doc Ewen built the first two receivers at 1420 Mhz and 8000 Mhz. The first observations with the 33-foot were made in July, 1960, two years after the Lab was established.

The story of the 85-foot is a bit more complicated.

The contract for the 85-foot was signed in January, 1960 after a long hassle with the University administration about boiler-plate in the contract document. Work started promptly and everything was going along fine – and then – well, let me show you some pictures.

By late summer 1960 the major parts of the 85-foot structure were well along (Figure 1).

The dish had been started (Figure 2).

In the last stages of its construction the dish frame was tested and surfaced in a large airhouse (Figure 3).

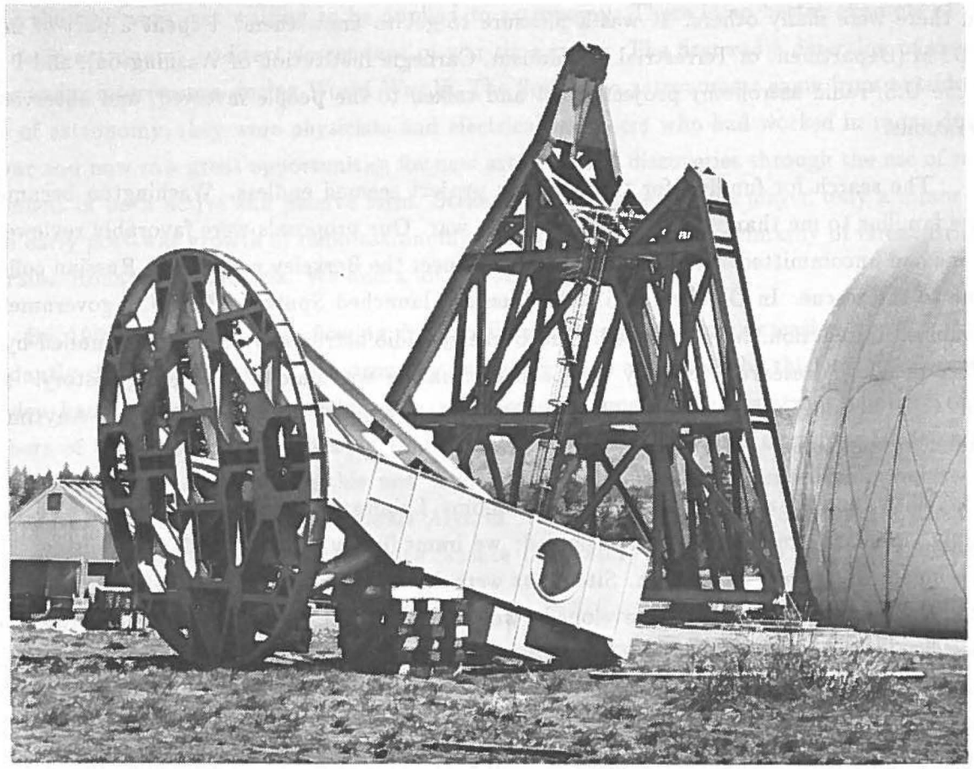

Figure 1. The major components of the telescope mount.

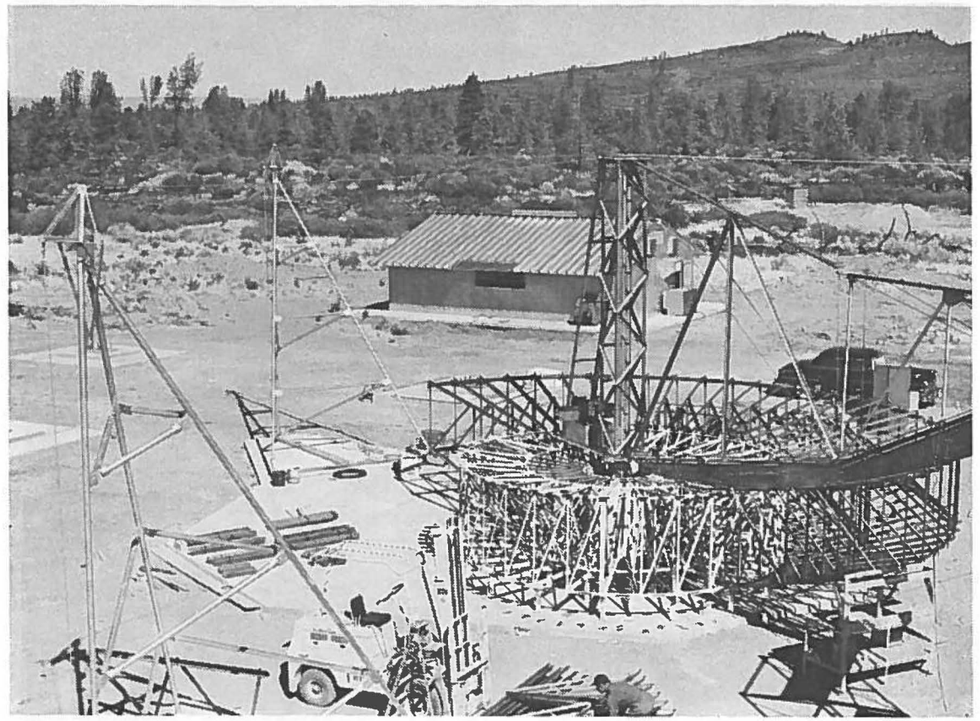

Figure 2. An early stage in the construction of the dish.

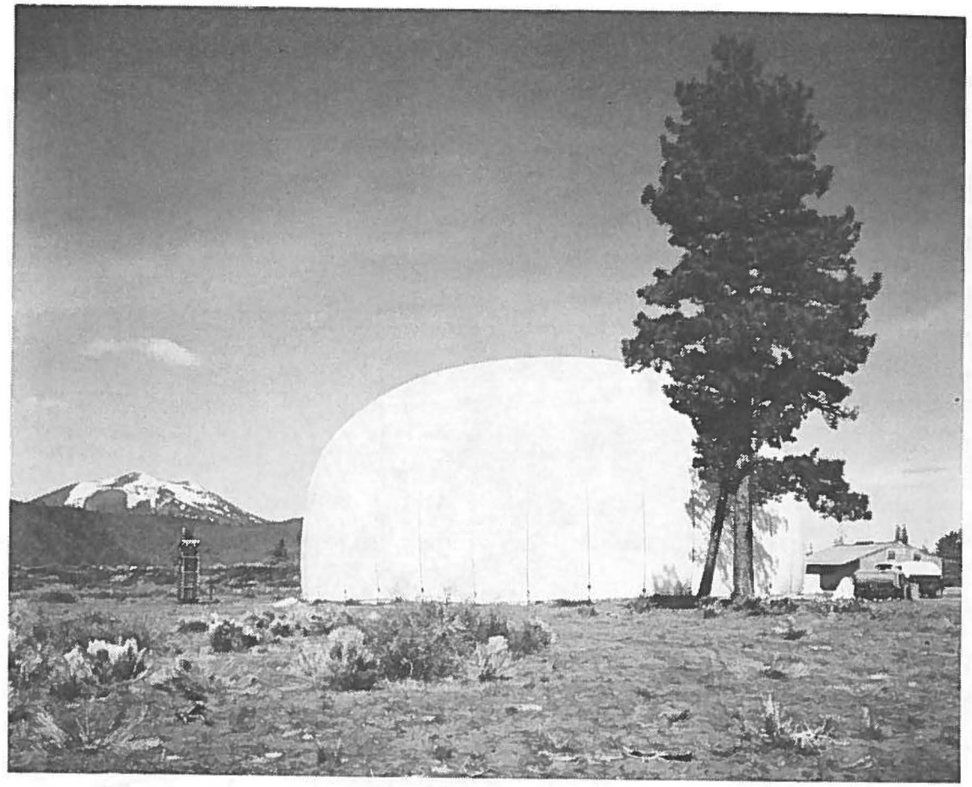

Figure 3. The airhouse in which the dish structure was completed, tested, and surfaced.

In October, 1960, as the season was getting late and the dish was nearing completion, there was a long weekend. The construction crew took off. A snow storm blew in accompanied by strong gusty winds. The power failed. Pressure decreased in the airhouse, which, under a snow load, sagged onto the partly surfaced dish. The airhouse was cut to shreds as the wind blew it against the sharp edges of the aluminum plates that formed the surface. There was a long delay as the surface was removed, another airhouse put up, the dish structure checked, and then resurfaced.

At long last, the dish was put on the mount in April, 1961 (Figure 4).

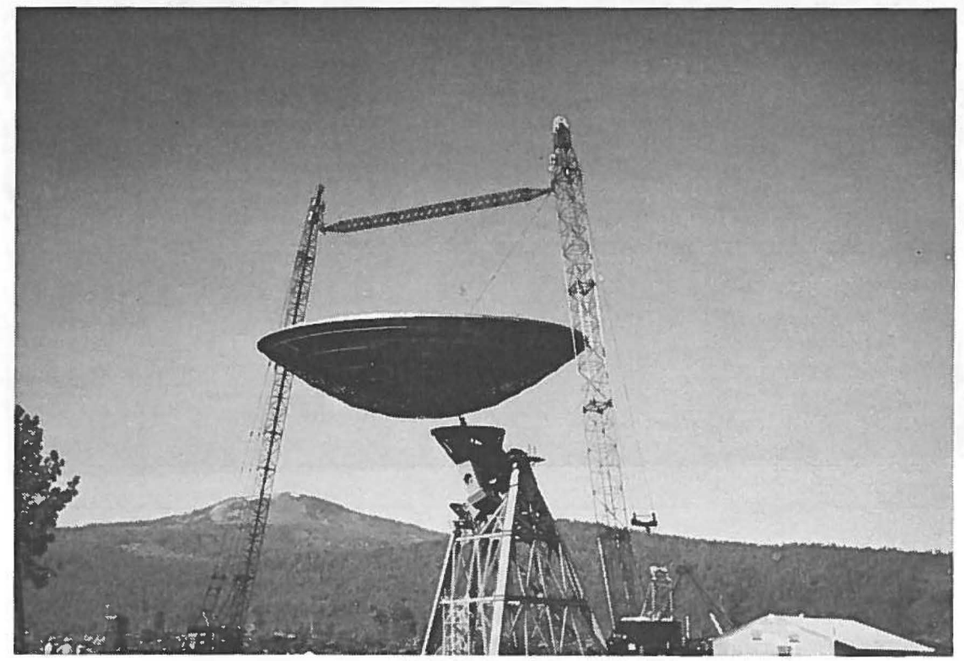

Figure 4. The dish being hoisted onto the assembled mount.

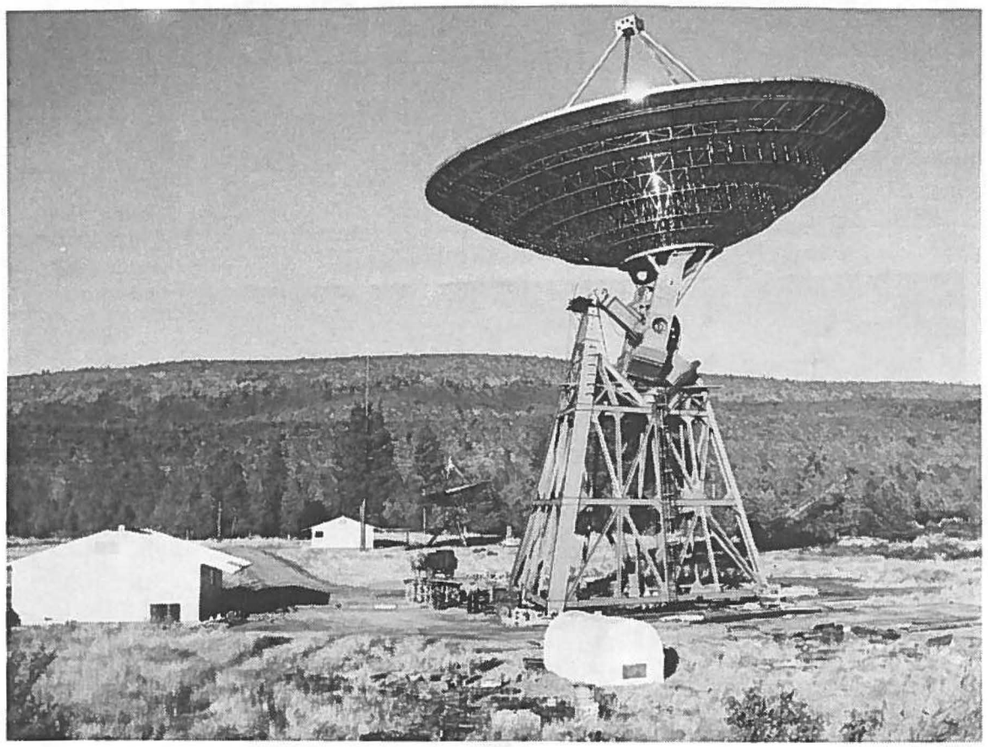

Figure 5. The completed telescope.

The 85-foot was dedicated and put into service on June 7, 1962 (Figure 5). A rather long series of engineering tests followed; then regular observations were finally started. Operations proceeded smoothly... and then...

In April, 1964 the south bearing suddenly and catastrophically failed (Figure 6). The design of the telescope required a very heavy counterweight on the stubby leverarm below the declination axis. The bearing was simply not adequate to take the load. It was replaced with a very large and strong bearing. At the same time, as a safety precaution, auxiliary bearings were placed on the declination axis.

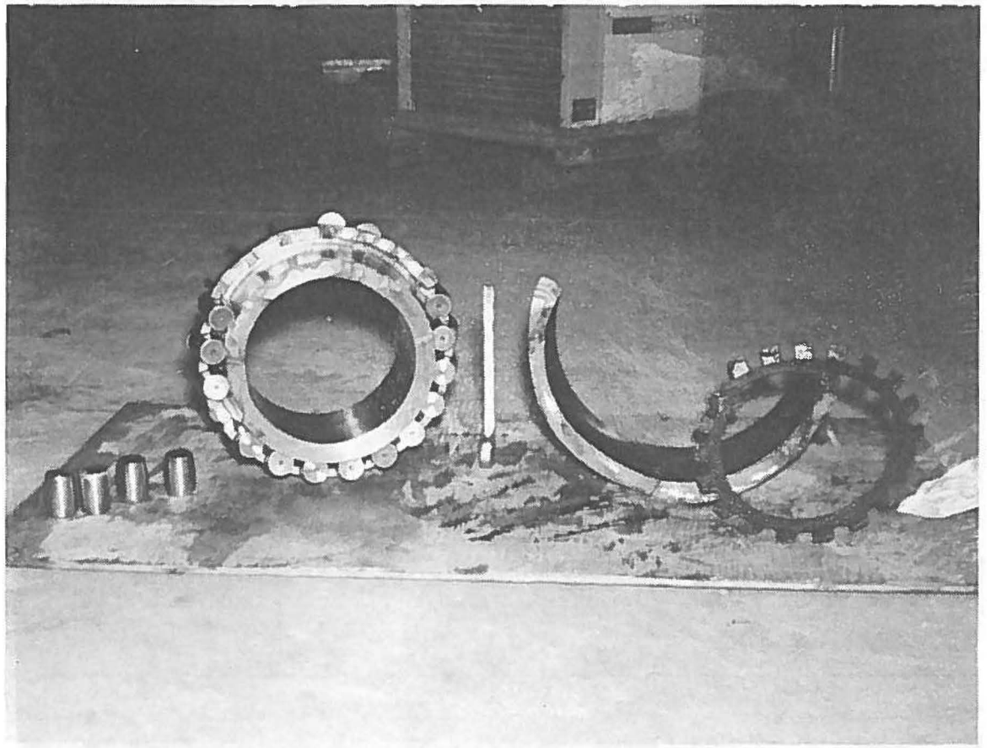

Figure 6. The failed south bearing.

There followed many upgradings, many changes of gear, many years of useful observations which many of you made or helped to make. And then...

On January 21 of this year the telescope was destroyed in an exceedingly heavy storm with wind gusts up to probably 100 miles per hour (Figure 7). A very strong gust caught the telescope which was stowed in the normal horizontal position, started it moving north and east against the brakes, back-driving the gear trains. The telescope must have been moving at a good speed when the gimbal hit the stops and abruptly came to a halt. The dish simply broke free and kept right on moving. It struck the ground on the east side of the mount upside down (Figure 8). Freed of the weight of the dish, the gimbal spun around the right ascension axis and ended up pointing to the west. Remarkably, the axes of the telescope held together, possibly because of the strengthened bearings installed after the accident in 1964. The reaction forces acting after the dish blew off must have been tremendous. The base of the mount moved several inches under some of the tie down bolts. Steel bolts an inch in diameter were sheared off.

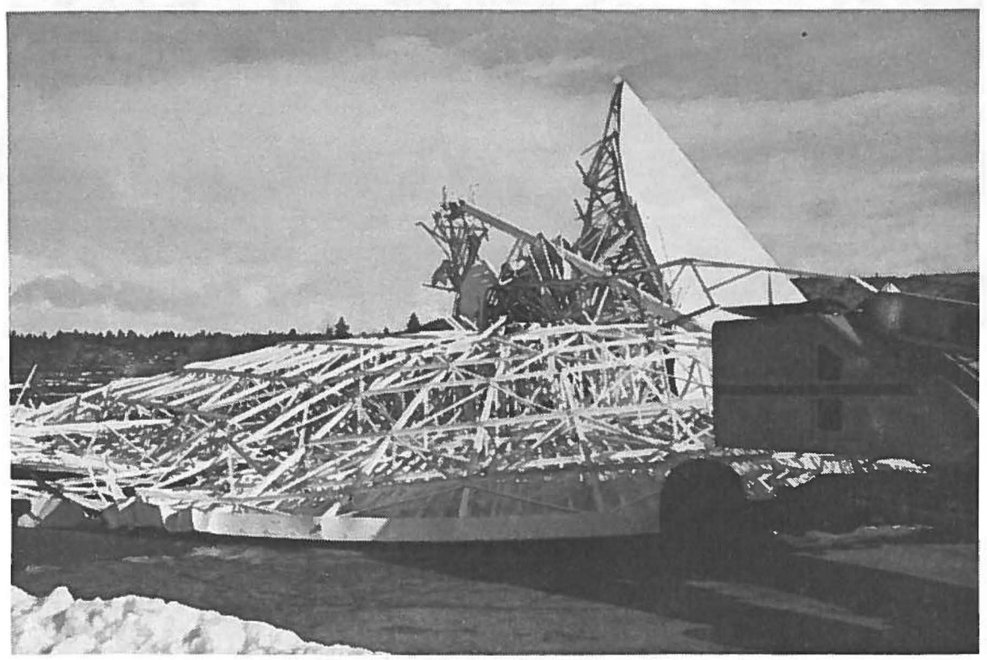

Figure 7. The dish as a twisted mass of wreckage.

Figure 8. A general view showing the gimbal pointing to the west.

Figure 9. View of the damaged lab building showing the wrecked cable runs.

The lab building was damaged as the dish fell (Figure 9).

The telescope looks very sad and forlorn, appearing somewhat as it did 30 years ago before the dish was hoisted to the mount (Figure 10).

We have lost an old friend who worked hard and produced many results for all of us.

RIP, or, to quote Jill Tarter, rest in pieces (Figure 11).

Figure 10. A bleak sight – cold, forlorn, and dishless.

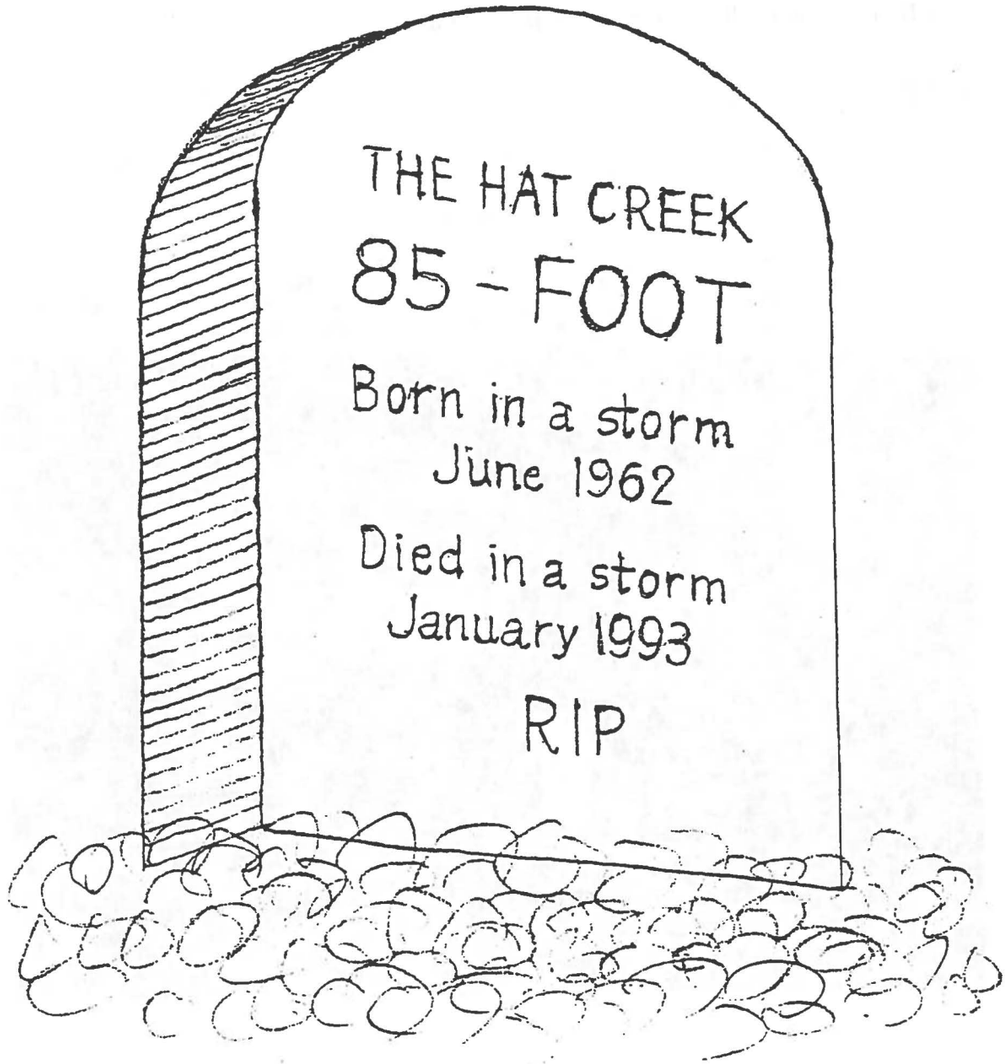

Figure 11. RIP

About the Interviewer

Joseph C. Shields is a native of Kansas and completed Bachelors degrees in Physics and Astronomy at the University of Kansas in 1985. At Berkeley he completed a Ph.D. in 1991 in astronomy, based on observational research in extragalactic nebular topics. In addition to his scientific pursuits, Shields maintains an interest in music, studying carillon performance at both Kansas and Berkeley. Since departing Berkeley, he has been a postdoctoral research associate in the field of theoretical nebular astrophysics at Ohio State University.