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Lick Observatory

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A.E. Whitford: Directorship of Lick Observatory, 1958-1968

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Jarrell: It is March 4, 1986, and this is my first taped interview with Professor Whitford on his directorship of Lick Observatory. Professor Whitford, the way I'd like to start out—you mentioned in one of our conversations several weeks ago that in reading Donald Shane's¹ memoirs that he hadn't elaborated very much on his own retirement. You indicated that you wanted to talk about the situation during which he retired prior to your coming here.

Early Contact with Lick Observatory

Whitford: May I review my first recollections of knowing about Lick Observatory? And my visitations to it prior to the date when I was offered the directorship?

Jarrell: Certainly.

Whitford: As you know from the autobiographical chapter in the *1986 Annual Review of Astronomy and Astrophysics*,² I came into astronomy as a converted physicist without any formal training, and was drawn into it by my contacts and interrelations with Joel Stebbins, the director of the Washburn Observatory at the University of Wisconsin, when I was still very much an unschooled person in astronomy. While I was working for him in the summer of 1932, I went with him on an expedition to an eclipse of the sun in New England, August 31, 1932. There was an expedition from Lick Observatory and the Mt. Wilson Observatory in neighboring towns in New England. The Lick Observatory expedition was to Fryeburg, Maine. We set up shop in Lancaster, New Hampshire, a dozen miles away. Prior to the eclipse, we visited the Lick setup. It was there that I saw William H. Wright, later to be director of Lick Observatory for a short time. Stebbins had known Wright since his student days on Mount Hamilton, ending

¹Whitford refers to Shane's oral history, *The Lick Observatory*, edited by Elizabeth S. Calciano, (Santa Cruz, California, 1969). Charles Donald Shane was director of Lick Observatory from 1945 until 1958—Editor.

²A.E. Whitford, "A Half-Century of Astronomy" in *Annual Review of Astronomy and Astrophysics* edited by Geoffrey Burbidge, David Layzer, and John Phillips, volume 24. pages 1-22, 1986—Editor.

in 1903. I remember Wright saying he was trying to do something with a relatively small telescope, much smaller and less ponderous than some of the other traditional setups. He thought you didn't have to be the biggest dimensionally, that you could do it efficiently. This very much coincided with Wright's later opinion about the obligation to build a very large reflecting telescope. He did not in the end join the dominant group that thought it was necessary. That's my first recollection of knowing anybody connected with Lick Observatory.

The second point on the curve, as I might call it, came after I got my National Research Council Postdoctoral Fellowship for two years in Pasadena. I started out by helping Stebbins observe at the Mt. Wilson Observatory—a most stimulating experience for a young man who at the time had just come into astronomy as a kind of a sideline. After we had successfully carried out our first sessions with the big telescope, with the new equipment that I had developed . . .

Jarrell: This is the 100-inch?

Whitford: Yes. During an intervening bright moon period when we couldn't observe successfully, Stebbins scheduled a visit to Northern California, to Mt. Hamilton and later to Berkeley. This was the first time that I ever visited Mt. Hamilton. It was the place, of course, where Stebbins had served his apprenticeship; he did his Ph.D. thesis project under W. W. Campbell; and became the third Ph.D. in astronomy to come out of U.C. Berkeley. W. W. Campbell was still living in the director's house which was quite an elaborate establishment for a mountaintop and dated from the remodeling of 1923, when he left his active role as director of the Observatory to become president of the University. He wanted to retain the nominal title of director and to have a mountain retreat to which he could come and to which he could bring the U.C. Regents and potential benefactors of the Observatory and entertain them. He got it through his influence with the Regents. Although they denied him the right to start afresh and build a completely new director's residence, they told him he could remodel the old one, which went back to 1895 or thereabouts. The resulting residence was more or less a shell constructed around the old residence. The twin of that old residence is still there . . . crumbling. I was impressed with

the elegance of this residence. We were invited to lunch at Dr. Campbell's house. It was served with a great deal of style with servants. I think it was within a year of the time that Dr. Campbell realized that he was a victim of an incurable or debilitating disease, and finally took the courageous step (as his family thought) of ending his own life by jumping from the upper story of a hospital in Berkeley.

On that visit to Mt. Hamilton, Joel Stebbins found many reminders of his student days. During a very memorable visit in 1930, he got the idea from Robert Trumpler of the space reddening that Trumpler had proved to exist, the first undeniable observational proof in the history of astronomy and one of the landmarks of that decade, perhaps in the history of astronomy. Among other people I saw on that visit, I remember Robert G. Aitken, who had during Campbell's nominal directorship been assistant director, but in fact was in charge of the observatory, never receiving the prerequisites like the grand residence . . .

Jarrell: During Dr. Campbell's entire nominal tenure then, at the end here when he was president . . .

Whitford: Yes. During the years when Campbell was president of the University he was still nominal director of the Observatory.

Jarrell: Then Aitken was "acting" director although he never got the full title?

Whitford: Yes. I don't know whether I've mentioned to you that Aitken's son, who the last I knew was still alive; he is the grandfather of my across-the-street neighbor. He has many childhood recollections of growing up on Mt. Hamilton which could be recovered by oral history. I remember meeting W. H. Wright also. I don't remember much about any other people. I do remember that the main building of the Observatory and the original telescope were very reminiscent of the 19th century. This was not amazing to me because I recalled the first day I ever saw the inside of an observatory in Madison. There were bearded portraits in the hall and ticking clocks and ancient original photographs going back to the 1890s. There was a meridian telescope, not very actively used . . . all reminders of the old astronomy of position. Under Campbell the principal

activity of the observatory had shifted to photographing the spectra of stars. These were not for astrophysical purposes—how the stars are made and how their outer layers affect the spectrum. They were more related to how stars are moving relative to the sun or the sun relative to them. Well, I think that perhaps recalls that first visit to Mount Hamilton in the summer of 1933.

There's a corollary to that trip. I went to Berkeley with Stebbins where he visited his fellow student in Berkeley of those days—Tracy Crawford, then on the Berkeley faculty and perhaps the first Ph.D. that the University of California ever gave in astronomy. He was a classical student of orbits which was very 19th century. I remember that his wife, though of quite a mature age, was very well preserved, and an accomplished singer. We had dinner at their house. It was on this visit that I first saw Donald Shane, then a professor in the Berkeley department.

Well, that recalls some of my pre-migration contacts with Lick Observatory. I visited it more than once during the 1930s. Just before the war our Wisconsin associate Gerald R. Kron had been sent by Stebbins to Berkeley to finish his doctoral training. This recalls what had happened at the turn of the century when Stebbins' professor in Madison, a man named George C. Comstock told him: "You've got to get more training than I can give you. Why don't you go to one of the places where you can learn about modern astronomy." It turned out to be Berkeley. So Stebbins recommended the same for Kron. We were very good friends. I visited him at Lick when he was a very young member of the staff, still a bachelor. I think my next visit didn't come until after the war years—years in which I knew Kron in Cambridge at MIT and also Nicholas U. Mayall.

Jarrell: Were they at the Radiation Laboratory at MIT?

Whitford: Yes. They later migrated back to the West Coast to work at Cal Tech's rocket laboratory.

Jarrell: At JPL [Jet Propulsion Lab]?

Whitford: Yes. It was the parent of JPL, then located at China Lake in the Owens Valley district in California.

In the summer of 1947, I had a project to look for the center of the galaxy or its infrared light which might be shining through the dust clouds—a project that, as I recounted in my autobiographical chapter, I had begun in 1945-46 at Mount Wilson jointly with Stebbins. More or less at his instigation and with him smoothing the way, I decided to undertake an extension at Mount Hamilton. Mount Hamilton had a dark southern horizon which Mount Wilson did not. It was a little farther north which was not favorable. It also had some relation to the curdling, or cooling, of Stebbins' long, happy relationship with the Mount Wilson Observatory . . . a matter which needn't be detailed here. It doesn't affect me except that it explains how I got to Mount Hamilton in the summer of 1947 to make these observations from a temporary setup in the front yard of the then still unused 20-inch refractor which had only a blue lens in it.

Jarrell: Would you explain that?

Whitford: It had only a blue lens, so called, because it was corrected for blue light, but not for yellow or red. If you used plates that were sensitive to those longer wavelengths, you could not get a sharp photograph. The second lens planned for the other barrel was installed in the early 1960s. The Proper Motion Project, for which the 120-inch telescope was built, did not become an active program until Donald Shane became director, just after the war.

Because of my infrared project, I saw Lick Observatory in the beginning Shane years. The main hall still had the 1890 pictures, but the rehabilitation was underway; the plans for the 120-inch telescope were beginning to take form and the revitalization of the observatory that Shane undertook were being launched. I had a very happy and rewarding summer. Due to my lack of appreciation, first, of how to do infrared astronomy at two-micron wavelengths and second, aiming for the wrong thing—I did not discover the center of the galaxy. That is related in my autobiographical chapter. There never was a published paper, only an abstract. I think that reviews my contacts with Lick Observatory up till the

time when I received an approach from the University of California. I believe that was in late 1956.

Becoming Director of Lick Observatory

Whitford: They asked would I wish to be considered, would I consider being approached for the [Lick] Director[ship] to succeed Shane.

Jarrell: Now at this time you were Director of the Washburn Observatory?

Whitford: Yes, and I had been director almost ten years. I had succeeded Joel Stebbins who retired on June 30, 1948 in Madison, and then moved to California and had a post-retirement career of about ten years . . . you can read about it in his National Academy biographical sketch that I wrote.³ He was enlivening the mountaintop with his wit and urbanity and working quite fruitfully in association with Gerald Kron. I do not know whether he had any role in the approach to me . . . I think what may have accelerated the University of California into approaching me as early as they did before Shane's target date for retirement, which was mid-1958, was that I had been approached by the Radio Astronomy Center in Greenbank, West Virginia, now called NRAO [National Radio Astronomy Observatory]. It also operates the Very Large Array in New Mexico now.

I had been approached: did I wish to take over the directorship of this enterprise? I can't remember whether it ever had a resident director . . . during the building phase it had been run by the president of the Associated Universities Incorporated (AUI), the same conglomerate that operates the nuclear physics station at Brookhaven, Long Island. His name was Lloyd V. Berkner. He was not an astronomer. I guess his largest claim to fame was his discovery in the early 1950s that one could sound out the ionosphere by sending up bursts of radio waves and waiting for the echo. I remember the first time I saw him was at the

³ A. E. Whitford, "Joel Stebbins" in *Biographical Memoirs*, pages 293-318, volume XLIX, National Academy of Sciences, Washington D.C. 1978—Editor.

laboratory at MIT. He had very close contacts with the Navy. Naturally the echo response from the ionosphere was a forerunner of radar.

Jarrell: So you were actually in the process of having two offers very close together?

Whitford: Yes. Shane announced that he planned to step down as director about five years before his retirement date. If they wanted me to consider it, they had to get in an offer. The people who now were on the staff and know how all these things took place and are alive are George H. Herbig here on the staff, Stan Vasilevskis, retired, who sometimes comes down to visit at that desk in this office, and perhaps Gerry Kron who retired and lives in Hawaii.

Jarrell: Can you tell me in this initial contact, how was the job as Director described to you? Did you have a pretty full notion of what was involved?

Whitford: Well, yes and no. (Laughter) I was aware that the then president of the University, Robert Gordon Sproul, had been a strong supporter of Lick Observatory. And, as you will perhaps recall from Shane's interview with Mrs. Calciano, he and President Sproul had been very good friends from Shane's days as a faculty member in Berkeley. Shane had come up through campus politics in Berkeley, and Academic Senate committees, and was, in retrospect (as I now realize it—I didn't know it at the time) a very accomplished academic politician. Maybe his Irish ancestry helped him in this regard. (Laughter) That's just an aside, but . . .

Jarrell: But you didn't have a full appreciation of that part of him back then?

Whitford: No. And I didn't quite realize that there was a kind of specialized and personal predilection of the President of the University [of California] favorable to Lick Observatory. Well, the word "crony" is a little bit perjorative, but it has been applied to the relationship between Dr. Shane and the then ruling hierarchy of the University in Berkeley. University Hall was finished in the first year of Clark Kerr's presidency and the hierarchical organization of the University had

changed a great deal. You will recall that Dr. Shane was concerned that Sproul had called Mount Hamilton a campus,⁴ an equal on the organization chart [to the] Santa Barbara campus, or for that matter, Berkeley or Los Angeles. It was rumored that Clark Kerr didn't think this was proper and was going to change it. Shane got some kind of a promise that it wouldn't happen on the day that I came or immediately thereafter.

Jarrell: Yes. In Dr. Shane's memoirs he states that in his discussions with you, that you had been "promised" that Lick would remain as an autonomous campus. And of course your directorship coincided exactly with the coming of Clark Kerr. There was a big transition . . .

Whitford: Yes. It happened on the same day.

Jarrell: Yes.

Whitford: Yes. The initial step by which I came to accept the directorship was that I came to California during the Christmas holidays, 1956-1957, and had lunch at President Sproul's house in Berkeley. I think that Mrs. Hearst gave that residence to the University. I believe it is now occupied by the Berkeley Chancellor at least some of the time. Well . . . I think this was an opportunity for Sproul to size me up as a person, someone who had a sense of what Lick Observatory should try to become. I remember it was a somewhat . . . well, what shall I say, nervous experience for me. But in the end, he offered me the job. I had to decide among three choices: the Radio Observatory in West Virginia, a place that I had been to look at, (my wife went with me and considered our children's schooling); Lick Observatory (she did not accompany me to Mount Hamilton although she had visited it). Staying in Madison was the third alternative. I was director, I had grown up in Wisconsin and it was my native land. I was comfortable there. I was a little bit worried by the fact that perhaps the Wisconsin astronomical effort could grow into a full-fledged

⁴Lick Observatory had the status of an independent campus of the University of California until 1958 when it became part of the UC Berkeley campus; Lick became part of UC Santa Cruz on July 1, 1965—Editor.

graduate school and that would take leadership that I, who had never had any formal training in astronomy might not be the best person to lead.

Jarrell: In terms of the academic development of astronomy there?

Whitford: Yes. We had had occasional graduate students and some of them went on to quite successful careers, but only one at a time and there was no formal set of courses. The challenge that was before me if I took the Lick job was to exploit the resources of the 120-inch telescope. It lifted the observatory into unquestioned second place in the United States, Mount Wilson being the first. You will have to ask the people who decided that I should be the first choice. It was my understanding by word of mouth, and you may find documentation of this in things that Dr. Shane has communicated: the directorship had previously been solely the prerogative of the President of the University.

Jarrell: Yes.

Whitford: He had instituted his own search, and set his own criteria. But in this case he allowed the staff the privilege of nomination and they settled on me. I always thought, and I guess most people that you might ask would probably agree, that my reputation as a person who could handle instruments, perfect them and get them to work, was something that they felt they needed, with the 120-inch going into its first years of operation. Dr. Shane I believe told me (and this may or may not be in his notes) that he had planned his career so that he would complete what he agreed to do—to finish the 120-inch telescope; but he didn't wish in his last five years as an active astronomer prior to retirement, to try to set the research program of the Observatory that would use this new research instrument. Some people thought that I might offer some leadership in that direction. I don't know whether they did or not or whether it . . .

Finishing the 120-inch Shane Telescope

Jarrell: From my reading and conversations with your colleagues, it seems the fact that you were a very highly respected instrumental specialist was of great

importance to Lick because the last stages of completion of the 120-inch were not getting done. In fact was there some concern that it wasn't being finished according to schedule?

Whitford: It is my subjective impression from conversations with people like Stan Vasilevskis and George H. Herbig—you will have to ask them what their recollections are—that yes, there was this sense when the chief engineer on the project, Bill W. Baustian, a descendant of the 200-inch engineering group, got a chance to go to help start the telescopes at Kitt Peak. He resigned before I ever came. I remember seeing him on Mount Hamilton during one of those periodic visits in the post-war years. He resigned on the premise that well, yeah, he'd finished the job, that the telescope was virtually finished. And an aide, Dwight ("Red") Ludden . . . he had not had extensive engineering training. So I think this motivated some of the staff. Some of the long delays were due to the final choice as to the way the optics should be ground, polished, and tested which was very slow and time consuming.

Part of the delay was due to the fact that there were mechanical problems, particularly with what's called the worm drive, the thing that keeps the telescope turning to follow the stars as they rise in the east and set in the west. This was a great vexation to Shane and his hands were somewhat tied. The attorneys for the Regents (or engineering people who were connected to them) would not allow him a free hand for fear that he might somehow absolve the contractors responsible for that part of the mechanism from their responsibility to deliver on their contract.

Jarrell: Things were sticky?

Whitford: Yes. The problem was finally solved. Shane broke the logjam and things were done to fix it. They did three things all in parallel—so exactly which one did it was never identified in unambiguous terms. Well, I've talked very freely. You asked me a question and you get a lot of answers.

Jarrell: Well, my last question today is what was your notion of some of the dimensions of the directorship. First and foremost was completion of the 120-inch telescope and getting it into night-to-night scheduled operation. I see from your autobiographical chapter that very shortly after you assumed the directorship, you really must have made things happen because by 1960 it was operating. There seemed to be not just the telescope but all the auxiliary machinery and devices that you had a personal hand in overseeing.

Whitford: Yes. Well, this was why I accepted the job. I thought I had the right background. You never asked me that. (Laughter)

Jarrell: I would like to ask you that. How interested were you from the start and why did you accept the job?

Whitford: Well, it was a challenge. And too, it does wonders for one's ego to be offered such a job. There's a kind of euphoria about it that I've seen happen to other people since. The other half of our marriage was emphatically footdragging. Moving away from the community in which we had roots, where my wife had a job and the children were going to school, and so on, had no attractions. Especially the official hostess role that went with the job, in which Mary Shane had set a very high level of performance and hospitality. That was something my wife felt she had no preparation for. But anyway, I accepted, and she came, footdragging or not . . . and we set out. So I guess the answer was that the challenge, the recognition were persuasive. Remarks that George H. Herbig made on probably the second day impressed me that if it was going to go forward, I had to be in personal charge of the technology and the engineering. He felt that these parts were not being directed effectively.

Jarrell: Several people have told me that there was a kind of a cronyism extant in the running of the Observatory?

Whitford: Yes, yes.

Jarrell: And that you had to overcome a certain status quo in terms of personal relationships, and the way things had been done in the past?

The University of California under Robert Gordon Sproul

Whitford: Yes. Well, I think Shane inherited this favoritism or bias that Robert Gordon Sproul had toward Lick Observatory, whether or not it was in fact the kind of world leader that the University of California ought to have, or at least a member of the first half-dozen observatories in the world. This rather paternalistic attitude I think had made it too easy for Lick Observatory. Going back to Shane's years in Berkeley these were personal friendships that he had formed with the members of the Sproul inner circle—the head of architects and engineers, a man named Weaver was one of them. James M. Miller who later went to Kitt Peak was another. He was a fiscal administrator. Shane had tentacles throughout the statewide administrative setup. In my first weeks on Mt. Hamilton, he took me down to Berkeley and introduced me to all these people that he had worked with in negotiating the contracts for the erection of the building for the 120-inch, the erection of the steel dome, and mechanical mechanism that made it move, the installation of the dials that told where the telescope was pointed and so on. He had relied very heavily on these people. President Sproul had believed the very modest proposed budget would be sufficient to get the telescope on the air when he agreed to find the money in the state budget at the end of the war.

You may recall that there was an interview between Kron, Mayall, and Sproul? I thought that this meeting perhaps had more of a initiating impact than it really did . . . but it emphasized to Sproul that the young members of the staff didn't want a modest telescope; they wanted a really big one. He said he would work to make sure that it would be as big as could be feasibly financed. But the cost that they were figuring was under one million dollars in 1946 dollars. The inflation rate wasn't very high in the late 1940s. But when George H. Herbig came on the staff, (I knew him as a bright student in those 1940s visits to Mt. Hamilton) he campaigned heavily for expanding its bright-moon use spectrographically, adding what was known as the coudé spectrograph. It involved two to four new mirrors, mechanisms for moving some of them and

for a very large appendage, a large fixed spectrograph. He planned all of that, and saw the execution.

Jarrell: Did he design it?

Whitford: Optically, more or less. The mechanical design was done by others. So Shane had to go back to Sproul and the Regents and say, "We can see ways to make the returns on the telescope very much larger for the investment if we add this, that and the other thing." And in the end the total appropriation was 2.8 million dollars. As the history of the University of California is recorded, Sproul was a master hand at keeping friends of the University in Sacramento friendly towards strong support of the University. This, plus some encouragement of private benefactions was Sproul's contribution. When he became President, the University was a good state university, and when he finally retired in 1958 it was unquestionably the leading state university and a very great one. And that's his monument. He wasn't an academic at all, as you know; he was a business manager, a business man. He had a canny sense of academic excellence, of who could perform, who was performing. The thing that I did not realize when I came to the University was that the relations between Clark Kerr and Robert Gordon Sproul had been very, very chilly when Kerr was Chancellor of the Berkeley campus. I attended the inauguration of Clark Kerr and Shane told me as soon as it was announced that Kerr was going to be the successor to Sproul, that he thought the University had made a very wise choice. But I did not realize that there had been this very biased attitude and perhaps favoritism by the University which came out of this personal relationship between Shane and Sproul. It would not carry over into the Kerr administration. Perhaps more important was that in Kerr's determination to administer the University in a way in which these personal, crony-like relationships didn't dominate things. I don't know whether it was generally agreed that they did dominate all over, but anyway, in the case of Lick Observatory they certainly had an influence. Kerr determined to replace these key people in the Sproul administration with his own men. There was a list of . . . well I wouldn't call them heads that should roll, but the people who should be eased out. It was known as the "Dirty Thirty."

Jarrell: The Dirty Thirty. I mean that's common enough that the new administration comes in and wants to replace . . .

Whitford: Yes. It didn't involve anything Machiavellian or a desire for personal dominance on the part of Clark Kerr. In general I admired him although he wasn't . . . perhaps we can go into more detail . . .

Jarrell: Next time.

Whitford: He wasn't a particular friend of Lick Observatory. Well that gets us to Mt. Hamilton and the problems before us.

Jarrell: To start off today would you discuss the transitional period during which you learned about Dr. Shane's style of directorship and running the observatory, his lifestyle up on the mountain, his philosophy, just the general impressions that you got?

Whitford: Well I could do that. My realization of how we approached the lifestyle from a different background and philosophy came rather slowly. The immediate problem that I confronted on arriving on Mt. Hamilton on the first or second of July in 1958 was the problem of getting the 120-inch telescope into an operating instrument. Also I reviewed the problems that I confronted while I was gradually becoming acclimatized to life at a mountain observatory and seeing how people thought and worked. That will lead into the answer to your first question. Is that a satisfactory order of things?

Jarrell: Yes, certainly.

Whitford: I came to Mt. Hamilton alone, leaving my family in Madison with plans to move about September first. In fact my wife and two daughters arrived on the mountain with a lot of possessions and the family car while I was at a meeting of the International Astronomical Union in Moscow . . . (Laughter) An experience which my wife never forgot. The mountain staff were very helpful

and hospitable in getting her established in the director's residence which was a rather formidable mansion created by W.W. Campbell when he was President of the University.

Jarrell: Kind of like a Camp David for astronomers?

Whitford: Well, or for university presidents. And I recounted in my last interview how in my first visit to Mt. Hamilton in 1933, I had lunch with Joel Stebbins at this mansion and saw the lifestyle of directors in the palmy days. But those days had gone. The Shanes had made it a rather legendary center of social life on the mountain, a guesthouse for visiting foreign and domestic astronomers. They later got a guest apartment built on the mountain to take some of that load off them, but Mrs. Shane had a fund of anecdotes which I hope she recorded for your tapes. They included foreign visitors putting their shoes out in the hall to be shined as was done in all European hotels and Mrs. Shane did it as well.

So this was a rather formidable challenge for which Mrs. Whitford had no great enthusiasm. She loyally tried to fill it and always doubted that she could do as well as Mrs. Shane. The Shanes were very helpful, invited us to occupy their weekend retreat in Belvedere, off the Marin County peninsula.

Completing the 120-inch Telescope

Whitford: But the problem at the Observatory was to advance the date when the 120-inch would be working. It was already years late due to the series of mechanical, electrical, and optical completion dates which had been missed. Perhaps lack of competent, top-level engineering leadership in the Observatory organization was responsible. Shane was an excellent organizational man; he could handle the details, the legal, the administrative intricacies which had to be carried on with the Sproul administration in Berkeley. But I had grown up in a kind of an instrumental school in Madison and Pasadena; some of these things are recounted in my autobiographical chapter. I thought that my background had prepared me to make a personal contribution. I was perhaps a little too

much of an egoist. I thought I could handle it better than the next person. I didn't delegate very much to high-level technical advisors.

The first problem was the mirror. The procedure that was adopted to bringing it to the correct figure or shape was very much like that which was used in the last days of the 200-inch telescope on Palomar. We inherited many revised designs almost literally from that telescope, concepts that went back to the 1930s. Some of them were outdated. But it involved final testing on star images on the telescopes without a reflective aluminum coating, just the bare glass which reflected only four percent of the light; but that was enough for bright stars. Of the two testing procedures, one was one the opticians use, the very traditional so-called Foucault cutoff. It enables opticians to read highs and lows, hills and valleys, relative to the desired final shape. This could be recorded photographically. The optician in charge was Donald O. Hendrix, the chief optician at Mt. Wilson Observatory: a legendary Texan and a great artist.

The second was a mapping procedure which was more quantitative in giving the heights of the hills and valleys. It was called the Hartmann test, carried out by Nicholas U. Mayall. He was a protégé of some of the Santa Barbara street astronomers before he came to Mt. Hamilton and a favorite son, I might say, of Dr. Shane. Mayall believed that the Hartmann test should be followed. There were sometimes disagreements. Hendrix was on a half-time arrangement. He spent the good months on Mt. Hamilton when we were ready for another test and it was fairly certain to be a clear night. This meant more or less June to November. The rest of the time he was in Pasadena. Collaborating with him was our own optician Howard Cowan, now still alive, but retired. But while an excellent optician, he didn't have quite the breadth of experience and self-assurance that Hendrix had. When there was a disagreement between the two tests, Mayall was insistent that his way was right. It took about two days of calculation with an old-fashioned desktop calculator to evaluate the test. And it led to some rather tense disagreements. It was later found against the cocksure certainty of the then head engineer on the mountain, Dwight J. "Red" Ludden, that the test screen that was used was precisely made to specifications.

This is something that I think should be recorded. You can get more recollection of it if you need it from Stan Vasilevskis, who had a very big share in this process. When the disagreement came, Stan, I think was one of those who insisted that there must be something wrong with the spacing of those holes because it was just one part of the mirror where it looked like there was a kind of a dip that didn't make any sense at all. They finally got Ludden to remeasure the holes and he confessed yes, there had been a mechanical mistake. It delayed things. But by going all through the summer of 1958 and into the fall, progress was made. It took about a week to evaluate a test, come to a committee agreement on what should be done to make it better, get the mirror off the telescope and into the optical shop in the lower floors of the dome, onto a polishing machine with the standard optical tools, which were a mixture of pitch and optician's rouge (a red powder); very, very small changes, a few millionths of an inch, I think, could be instituted. This was done and then the mirror had to be hoisted back up onto the telescope, mounted; it was done in the outdoors, not an air-conditioned room. It had to be brought to temperature equilibrium, with night-time temperatures in the dome, the test made, two days more evaluating, and the cycle was about seven days long. So you spent two or three hours actually doing something.

We kept it up for twenty weeks or so after I got there; fifteen or twenty weeks in 1958. There was a hiatus during the bad winter months. Work resumed in 1959, about late May or June, I can look up the exact date when the mirror was aluminized, I've got a note on it here . . .

It was in *The Astronomical Journal*, the official journal of the American Astronomical Society. The date was June 24, 1959. There was a series of final committee meetings in which Hendrix and Vasilevskis said that they agreed. The mirror is not completely rigid; it floated I think on eighteen equalizer supports, each of which bears its proportional share of the weight as the mirror is facing straight up and shifts it into a sidewise support as it is tipped. The mirror is more or less like a waterbed except there isn't any water. It receives proportional support. If it didn't it would bend. There are temperature strains. Although a pyrex mirror has a lower expansion coefficient than old-fashioned glass, it's only about seventy-five percent reduction of the coefficient for ordinary glass.

When we got the best tests we could allowing for all those things, it appeared that there wasn't any improvement after the next polishing. There was enough uncertainty in making all these factors 100 percent perfect, e.g., the temperature history and so on. We were just spinning our wheels, weren't getting any better. And Hendrix assured us, "You have got one of the really good mirrors of that size, it's better than the average quality of the 200-inch." It was the same kind of glass, pyrex, and the same kind of support system, just fewer pads. But Mayall said, "We've got to strive to get it perfect." And Hendrix said, "How will we know when it's perfect? We don't know whether we're making it any better. The tests don't show that we are."

So Mayall was overridden and we decided to aluminize. It was put on the telescope. We had agreed to accept the French visitors André Lallemand and Maurice Duchesne, for September 1959. The visitors had an electronic device, which was an advanced thing for those days. It had been the rule before I came that the University looked the other way about overtime put in by people who lived on the mountain; well there weren't any bright lights in the evening to take them off to pool parlors or . . . anything like that. They didn't mind the extra pay. I don't think they got up to time and a half, but they got a lot more money because of working fifty or sixty hours a week. And that went on through the summer. And the French team came. We had our working spectrograph, one of a number of them that were later finished, to which their device could be attached. The first scientific results came in October, 1959. They were good. Merle F. Walker collaborated with the French team.

Jarrell: Were they opticians?

Whitford: They were from Paris. Well one of them was a real astronomer who had pioneered this. The other, Duchesne, was more . . . a technical person.

Jarrell: And this device . . . it was a test then?

Whitford: Well it was the original scientific observation with the telescope and they made sort of a discovery. It had to do with the way the center of a

neighboring galaxy, which is flat, goes round, very close to the center. It was the combination of a good telescope, a good spectrograph, although in rather preliminary shape, and this device, which was better than photography, much more efficient. Meanwhile, during that fall we did have the means of taking pictures of the sky. Nick Mayall was a very skillful photographer with this kind of equipment at the upper end of the telescope. He took some magnificent pictures to prove that the telescope did have high quality. It did. Then the telescope was taken out of circulation to do a lot of new things. The pointing indicators for where the telescope was really looking depended on a very old-fashioned scheme called syncros going back to the 1930s, and the 200-inch design, transplanted about that time from some battleship gun pointing mechanisms that the Navy used. (laughter)

Jarrell: Makeshift?

Whitford: Well it wasn't makeshift but it wasn't state of the art. It'd just been carried over. It worked, but it wasn't as good as it could be and now has been discarded. And that had to be done. And I don't remember all of the other fix-up jobs. But in March, 1960 we said the telescope was now open for general use; we began receiving requests, mostly from our own staff, and they got very generous assignments. The more talented ones reveled in the opportunity. Examples would be George H. Herbig, Nicholas U. Mayall, and Merle F. Walker . . . and a year or two later we had two new young staff members, George W. Preston, a Berkeley Ph.D., and Thomas D. Kinman, a British-born astronomer who came from South Africa. They made very good use of the telescope. So with the talent that we had on the mountain it began to perform world-class astronomy that we'd never been able to do.

But there were projected four to six new auxiliary instruments . . . until finished it was very limited in the ways it could be used. It was more or less my job to see that with the aid of the engineering design group, the shop construction group, and such contracts that we could make outside for hardware that was too big to handle in our shops, that it all got started.

I was perhaps not delegating enough. I was quite clear that the talent we had on the mountain wasn't up to it, particularly the then head engineer Ludden. This is a delicate matter. He was not schooled in electrical controls and indicators, and he compensated for this by saying that no one could touch it without his approval. He had the key; he would leave the mountain for a long weekend and we couldn't run the telescope because he had this rather, oh what should I say, domineering attitude. I got a competent control engineer a couple of years later. His name was Lawrence Berg, and some of these things were all fixed up; we ran on kind of a temporary regime until that was all done. I had inherited the faint object spectrograph (a prime focus instrument) from a group of designers who were no longer around. I reviewed the whole optical design and made optical calculations myself with a desktop calculator, which helped our optician Howard Cowan to set up some test procedures so he could see that the optics were correct; they were difficult optics, he had never done anything like it. We devised tools, I watched the tests, and it came out to have high performance. In the end, it lasted only a few years because non-photographic recording devices began to be substituted.

By 1961, 62 observers from the other campuses began to ask for time. We made it plain that as a part of the University of California we were obligated to give them access to the telescope. I think without saying so we assumed that the Mt. Hamilton staff would decide how the time would be allocated.

Jarrell: Yes. You said in your autobiographical chapter that astronomers from the new astronomy departments and old astronomy departments in the system who had not previously . . .

Whitford: Yes, they had been able to get better instruments at that time in Pasadena; they were beginning to be available at Kitt Peak.

Users included Paul W. Hodge then on the Berkeley staff, E. Margaret Burbidge from San Diego, Roger Lynds, originally from Berkeley. Rudolph Minkowski a retired astronomer then centered in Berkeley, Hyron Spinrad, George Wallerstein, Peter S. Conti and some Berkeley students. Others were Lawrence

H. Aller, a new faculty member lured to Los Angeles from Ann Arbor, and Wallace L. Sargent from San Diego. We thought it proper that they should use the telescope. By 1965 or so some 47 percent of the available nights were being used by astronomers who did not belong to the Lick staff.

Conflicts Over Observing Time

Jarrell: Now can you tell me, this is a separate question. When this trend started to be recognized, of increasing use by non-Lick staff, but people with entitlement to access, how was the time divvied up? Who allocated the nights and how? Was it you as Director?

Whitford: (Laughter) Well, yes. I'm afraid I was rather viewed as a dictator by some people, particularly in Berkeley. I sent out announcements for I believe it was a three-month period, during which time there would be some 91 or so nights or something like that, on the average. It was so many two-week periods when the moon was dark and two-week periods when the moon was bright. People would send in applications and say what they wanted to do and we would divvy it up as best as we could. At first we were able to give everybody a chance at the telescope and not pinch our own resident staff very severely. Perhaps we should stop at this plain factual historical review at this point, because as you may have sensed it led to considerable strains within the University in the end and led to our move to Santa Cruz. And make that a separate chapter.

Jarrell: I agree. I was going to ask if this was the opening wedge of the whole shift.

Whitford: Yes. But as I indicated in my autobiographical chapter, I considered it my principle duty, my highest priority or task, to get that telescope completed and working as it was supposed to, as a high-performance instrument. It took just about all the time that I had for about four or five years to finish off the planned focal-plane instruments.

Jarrell: Yes, you said in your chapter, in fact that your involvement in all of these instrumental problems took about four years and that was it. There wasn't much time for anything else. That was your number one priority.

Whitford: Yes, that is perhaps the place to say let's back up and talk about other things.

Jarrell: Something that branches off from this was that during that four years you no doubt became acquainted with the modus operandi of the staff, not just the astronomers, but the technical support staff, the engineers. You had first-hand dealings with all of them in doing this very complicated, laborious series of tests, and you started seeing, how things were run, and how the lifestyle on the mountain influenced things.

Whitford: That is a fair statement. I would perhaps put as a supplementary comment that I came from a less complicated university background, nowhere near the total size nor operating budget, but a good one, and one in which I had a very comfortable arrangement with the administration. They trusted me and I trusted them. But the challenge, as I said, in the previous interview—well I guess you could call it this opportunity—was something I didn't think I ought to spurn and I didn't. There were two parts of the modus operandi that were different from what I was used to. One, the small town, the company town on a mountain. I knew I was getting into that. It was one of the very few places where this still prevailed. George Ellery Hale decided in 1905 not to do it that way at Mt. Wilson because of the terrible personality clashes and feuds that he saw in the early Mt. Hamilton days.

But as I said in my chapter, living on the mountain was a logistical necessity for Lick Observatory when it started and it had its points. It meant that there were astronomers there for site observing. You could change the schedule without running special vehicles up and down the mountain and taking nights away from somebody and giving them to somebody else, always a touchy matter. The first impression that I got was that this combination of a rather sheltered life, which in Campbell's case had its baronial aspects, meant a great deal of fanning

the brow about the hardships and different lifestyle that living at the end of this winding road imposed on the inhabitants of the place. It had engendered a sort of a dichotomy amongst the then resident staff members. On one side was Shane and his loyal, and very much loved and admired wife, Mary Shane. I would say the two other staff astronomers, Hamilton M. Jeffers, the older brother of Robinson Jeffers, had some family means (Shane certainly did) and Gerald R. Kron, who married a rich heiress from the New York area. He was the son of a chauffeur to a beer baron in Milwaukee. I knew him (laughter) as a student in Madison. Well I'm letting gossip get into this, but why not?

Residential Life on Mt. Hamilton

Whitford: But this group believed in the genteel life, which involved weekends at country retreats in the Carmel area and in Marin/Belvedere. What I called (rather disrespectfully) the long, sacred weekend was a part of the lifestyle. Dwight J. Ludden was picked by Shane as an example of someone who should get jobs on the mountain and good housing, the best housing he could find anyway. This was the consequence of his feeling that employees should be comfortable members of this happy family community. There were very few bitter feuds, because Mary Shane saw to it that people felt that somebody cared about them. But in the view of the other wing on the mountain, this had led to underweighting the competence and performance of some of the people who were hired. Some of them were really excellent people, the photographer Ronald Watson, his wife Connie, the librarian. We had to get couples because of the housing shortage. The machinist, Ray Greeby, now retired and living in Clear Lake, and his wife Rete, who was an illustrator and many other things, were very, very competent. Those were excellent appointments.

On the technical and engineering side I guess there was disagreement. The other wing, which you might call the Calvinist wing felt you had to work hard. The fleshly pleasures of lush weekends (I use the the word lush in a non-pejorative sense) subtracted from the scientific potential of the observatory. I called them the astronomy-first wing. The leader of this contingent was George H. Herbig, a dyed-in-the-wool Calvinist and rather mercurial in his intense disapprovals.

Jarrell: One of your colleagues has in a rather vernacular way characterized what you're describing here, a little more succinctly and maybe pejoratively, as cronyism.

Whitford: Yes, that was Herbig. That was his word for it.

Jarrell: So that the scientific or technical competence, in a certain sense, came second?

Whitford: Yes. I got an orientation session on the second day I was there and a kind of memorandum of what Herbig thought ought to be done. I said, "I will feel my way. I'll be my own chief engineer." I wasn't going to just dump the present staff and replace them with my own selection. I didn't know how to select the people very well.

Jarrell: You didn't know the environment sufficiently yet?

Whitford: Yes, I had to feel it. But there was no question that I joined the astronomy-first wing, as opposed to the happy family, genteel astronomer wing, who, in Herbig's view were not as competent, up-to-date, well, not forefront astronomers. They were doing more pedestrian, but useful astronomy.

[Inaudible]. . . In his publicly released comments Shane was quite aware of this and he mentioned that in the first year I was adopting a different style, which he called placing great weight on what he said was a "burning desire." I was interested because, while our relationships remained friendly, they were not warm until the end of his life. Well, the second part of this was getting oriented in the politics of the University. Shane attempted to introduce me to the movers and shakers that he knew in Berkeley who had held office under the Sproul administration, that ended on the day that I came.

A New Era under UC President Clark Kerr

Whitford: There were persons in the University-wide department known as Architects and Engineers who were very good friends of Shane, and part of the cronyism that affected his approach to life and personnel selections on the mountain and also affected his connections with the administration of the University. I had not realized, didn't realize for some time, that there had been a rather paternalistic relationship between Robert Sproul and Donald Shane, going back to the years when Shane was high in campus politics as a member of the Berkeley faculty. Those were the years when Sproul didn't ask staff on Mt. Hamilton who should be the next director; he just decided.

I don't know where he got his advice or whether he just followed his, probably quite good instinctive sense, but as Shane recalls in his memoirs, he was approached about his becoming director when some of the old-line astronomers were getting past retirement age. He said he could not accept because he was committed to work at the Los Alamos Weapons Laboratory. But at the end of the war he quickly got rid of that, accepted the job, and did yeoman service in catching up on a rather neglected, low-budget, let's-not-be spendthrift attitude of some of the old astronomers. W.H. Wright belonged somewhat to that school. I in no way dissent from the decision to name the telescope after Shane, because he brought it into being, he maneuvered it through the administration of the University, and the Legislature, with the help of Sproul.

It was quite clear that Clark Kerr didn't mean to do it the same way. He got a management consultant firm to draw a new administrative plan for the University. It was a time when enrollment was growing very rapidly, and the possibility of new campuses was being considered. He was talking about multiversities, that the different campuses didn't have to be carbon copies, they'd each have a distinctive emphasis. But he didn't see Mt. Hamilton as a proper campus. And Shane comments on the fact that he tried to stave off what he saw coming, this hierarchical downgrading from campus to something called a research station. He did get Kerr to delay making the change for one year until I could get my feet wet. But it was inevitable. I didn't see any reason to fight it. President Kerr, with whom my relations were mostly pretty warm (though I

didn't see him very often) quite correctly, I think, defined a campus as a place where students studied a variety of subjects, in a rather broad academic community, and Mt. Hamilton certainly wasn't that.

Jarrell: It was really a misnomer to have ever called it a campus. It was an autonomous unit.

Whitford: Yes, it was a euphemism. But it had pleased Sproul, who had a soft spot, for some reason that I cannot trace to its origin. The second thing was that Kerr had had a rather unsympathetic relationship with Sproul as President of the University when he was Chancellor at Berkeley. I remember hearing him say, or Mrs. Kerr say, at some social gathering that Clark had a second desk (perhaps going back to Swarthmore or some Quaker forebears) that he went and sat at when he felt that he was just going to boil over. (laughter) He could be quite decisive, even ruthless. He proceeded to clean out the Sproul-Shane cronies in what later became University Hall, but wasn't finished yet. I have mentioned there was a blacklist called the Dirty Thirty, which included some of Shane's cronies. One of them was James M. Miller, in the statewide business section. He realized he was being pushed out and resigned. Shane was instrumental in getting him a very important job at AURA [Association of Universities for Research in Astronomy] in Tucson, and he practically ran the place for five or ten years (laughter).

Lick Observatory's Changed Status

Jarrell: Well, you came during that first year, that interim year, during which Lick Observatory retained its autonomy. Then in 1959 it became a part administratively of UC Berkeley.

Whitford: Yes, I could not confirm the date of the official transfers without consulting documents that I would have a hard time finding. But I told you I was going to speak from my memory, and such records as I have here.

Jarrell: I'm interested more in your state of mind.

Whitford: Yes, well Kerr was cordial. The Regents met on Mt. Hamilton in 1959, their last visit. They had rotated [their meetings] amongst the campuses and this one was, while it was the state of a campus either just ending or having ended. They came and it was quite an occasion. We had a catered meal at our house for about fifty people. A traumatic affair for us but I got to meet some of the Regents and came to respect some of them more and a few of them less. As a result . . . the first step was that President Kerr was trying to unload the separate details that he had to handle; to unload part of the oversight of the University on his Vice-President, Harry R. Wellman, who later became Acting President of the University when Kerr was fired by Ronald Reagan, or he was fired by the Regents, with Reagan putting on the pressure.

Then we were assigned as a subunit of the Berkeley campus under Chancellor Edward W. Strong. That was where the gathering storm took place. Perhaps this punctuates the fairly satisfactory relationships with University-wide and Berkeley administrations. We did get budget supplementations when we said we're back in the major league, we've got the second biggest telescope in the world and it works very well.

Jarrell: And then also you were putting all of these auxiliary instruments into operation.

Whitford: Yes, the financing for that had all been arranged by Donald Shane. He was a past master of it. He told an anecdote which is perhaps worth repeating. The telescope that had been envisioned by Kron and Mayall was a very simple one with few attachments. They didn't even plan to use it much during the bright of the moon. George Herbig said that the real astrophysics is done on bright stars during the bright of the moon. You can't do such fancy stuff on very faint distant galaxies. We should have a high-resolution spectrograph that will work on bright stars during the bright of the moon. It would cost hundreds of thousands of dollars. He provided the design. It is a magnificent spectrograph, continually being re-outfitted, even now. A very large one, it's stationary in a dungeon room and the starlight is brought to it, rather than it following the telescope around as the telescope follows a star or finds a new one. The cost overruns, which always happen, and changes in design and augmentation of

these very large expensive, auxiliary instruments, considerably exceeded the original estimate of about one million dollars for the telescope, an amount which Sproul said he could find in the state budget, from the regular appropriations that the University would get in the post-war years. It rose up to 2.8 million, the final cost. And Shane had maneuvered these supplements through the legislature, and they were in the bank.

He said that he thought I could (and I agreed that I could) finish this program on those appropriations. But when the last one, oh some \$270,000, if I remember correctly, was being put in the University budget and being approved as a capital expenditure, the legislative committee, having a rather dull afternoon, began asking some very hard questions. They were having some fun with Shane; they were hazing him. And he sweated, and they finally said, "Well we intended to give it to you all along but you do have to say that this is the last appropriation that you will need to finish the project." He said, "Well, I can assure you that." They said, "Well we're glad to hear that. It's been back here for this and back here for that. We've been nicked and dimed to death."

Well, there is one development that I might mention that was a part of this gathering storm. As soon as it was quite apparent to astronomers in the sea-level campuses of the University that they had a great resource here, a research tool, they began using it and we expected they would. But at Los Angeles and at San Diego and also in one case at Berkeley, the campus staffing pattern began recruiting very good astronomers with the promise that they'd get to use the 120-inch telescope, that they had access to it, and very vague promises as to how much.

Jarrell: These were enticements for faculty recruitment?

Whitford: This was all done without any unified consideration of how many astronomers could be given sustenance via observing opportunities in the number of nights per year that that telescope could work. This ran between 60 and 70 percent depending on what kind of sky quality needs were satisfied for different tasks. George Herbig became alarmed; I became alarmed that the

camel's head was just going to crowd the Mt. Hamilton tent; it could crowd out the Lick astronomers who had suffered through almost twenty years of conception, planning, do-it-yourself monitoring, sacrificing personal careers, committing knowledge and talent to finishing the telescope. We thought it was ours. The other people thought we were . . . well, this gets into the gathering storm . . . that we were mere telescope jockeys and perhaps not as noble astronomers as they were, or as competent. And this developed into what I called the gathering storm.

Jarrell: The gathering storm. Is that a term that you've come to use in just the last couple of years or did you conceive of that back then?

Whitford: Yes, that's my word chosen almost today, borrowing from Churchill's famous . . .

Jarrell: Yes, at the time, during 1958, 1959, 1960 . . .

Whitford: Well, it went up to 1965. It was mostly between 1962 and 1965 that these . . .

Jarrell: So there were indications . . . First there was the pattern of increasing use and then eventually it culminated in a critique . . .

Whitford: Well, whose telescope was it?

Jarrell: OK. That's really the way you're phrasing it, is whose telescope was it?

Whitford: Who controlled the maintenance, operation and guaranteed readiness, accessibility and performance? And could this be delegated to some telescope jockeys who didn't know much about astronomy or . . .

Jarrell: Well as a member of what you called the astronomy-first group on the mountain, how did you conceive of this question: who owned the telescope?

Whitford: Well, we thought it was ours. It was a perhaps provincial . . . this story can't be told in the framework of the good guys. There aren't any heroes and there aren't any villains. It's just one of those power struggles that was inevitable and it came.

Jarrell: At the time, from 1962 to 1965 you were in the midst of a major change in this institution of Lick, of Mt. Hamilton. Did you see that the University of California was becoming a very large and massive bureaucracy and the way it was organized and run would inevitably touch everything, including Lick? Did that make sense to you then?

Whitford: Well, it was a gradual realization and I think perhaps how it all came to a kind of crisis and a focus in our mind might be reserved for another day, if that's . . .

Jarrell: OK, we can take that up . . .

Whitford: There is also the matter of the Southern Hemisphere telescope, our Australian adventure . . . and the national politics of who would get to build and administer telescopes which had federal financing and therefore an obligation to serve the national clientele. That was all mixed up together. So the two topics that I think might well be considered for another session are: how this interrelation with the national picture, our set of mind, how astronomers in other parts of the country saw us as provincial, and then the internal struggle within the University-wide astronomy community.

Jarrell: Yes, I see that is definitely the larger context, the national picture and the whole beginning development of big money astronomy, international and national, the federal government and all of the funding agencies, National Science Foundation and others.

Whitford: Those two subjects I think might well be reviewed in the next session and perhaps I'm talked out for today.

Jarrell: Oh, that's fine.

The Ground-Based Astronomy Report

Whitford: The survey was undertaken at the instigation of The Committee on Science and Public Policy.

Jarrell: . . . at the National Academy of Sciences?

Whitford: . . . there was an interagency counsel, of maybe all small letters, of federal agencies which financed scientific research, which tried to coordinate and not compete in covering the needs and not leaving any gaps. How the proposed survey of needs for astronomy in the next decade came to be organized in the study, perhaps the annals of COSPUP or the National Academy would tell. Robert Green, who became the secretarial agent of this committee of this inquiry is now dead. The chairman of the committee COSPUP at the time was George B. Kistiakowsky, an immigrant Russian and a very much respected professor of chemistry at Harvard. He was also quite instrumental in the successful technique that was used to trigger the fission bomb at Hiroshima. He was, incidentally, a leader in trying to contain and mitigate the added tension and danger that the success of the bomb brought upon the world. At any rate, at a date which may appear in the preface to that report, [1962] which I'm sure is available . . .

Jarrell: The Ground Based Astronomy Report?

Whitford: Yes, the ten year program, I'm sure is available in the library catalog.⁵ I was asked, at the suggestion of Leo Goldberg, whose post at the time I cannot exactly pinpoint, if I would be willing to become chairman. Well of course it was somewhat flattering to be asked. I felt responsible, if I could do it,

⁵ *Ground-Based Astronomy, a Ten-Year Program* a report prepared by the Panel on Astronomical Facilities for the Committee on Science and Public Policy of the National Academy of Sciences. Washington, National Academy of Sciences-National Research Council, 1964—Editor.

to try to see what was possible. I was given carte blanche to choose the panel. I tried to get representatives from across the country. As will appear later in these remarks, I was definitely of the West Coast group; the eastern astronomers felt that we were somewhat provincial and arrogant, and questioned our self-assessed competence to run mountain stations and big telescopes which the benighted people in the eastern part of the country did not have.

I tried, as well as I could, to respect this long difference of opinion which, from the accounts, goes way back to the beginning of Lick Observatory. I've heard about this from the spoken word of Joel Stebbins, who was on the mountain in the year 1900 as a graduate student. If Olin J. Eggen, now at Cerro Tololo, ever gets to write his historical account of American astronomy of that period no doubt he will discuss this east/west conflict. It set the dominant thread of how American astronomy developed in the way it did. He is a good archives man but is not afraid to take forthright views. But this will perhaps get any future archivist or historian some leads as to the set of mind which I brought to it.

There were radio astronomers and optical astronomers but no space astronomers on this panel. Dr. Kistiakowsky said that because of the clout which NASA had, a far greater sum of money was already under its control for the space effort, more than then ground-based astronomy would ever get. The actual figure when the Apollo program was being launched to put man on the moon, as President John Kennedy said before 1970, was toted up and this was going to be so much larger that all we ever asked, though that seemed rather ambitious at the time. It would not be more than one-half of one percent. That is in the preface of the report.

Well the inquiry went about two years. There were meetings and hearings at various places. A good part of the optical review and formulation of the proposals fell upon Allan R. Sandage and me. All of the radio portion was directed by Frank D. Drake, now our [UCSC] Dean of Sciences, who at that time was from either Greenbank or Cornell, or in the transition. Both were very articulate, even eloquent proponents of the emerging potential in ground-based astronomy, of what could be done with extended support. We tried to make our recommendations seem not too fanciful, something that was realizable within

already established existing engineering limits and within the competence of the people who would have to carry out the work.

There was also a section which I *ex post facto* accept the responsibility for writing, and perhaps for the basic set of mind that is in it. It's called, "Under Whose Auspices?" There were the two large consortia—one for radio astronomy, which became the National Radio Astronomy Observatory, NRAO, centered in Greenbank, West Virginia, at the time, and AURA, for optical astronomy, centered in Tucson. Both were governed by boards of trustees, or boards of directors that came from constituent universities, eight or ten in each case: not exactly the same but many universities in common. They had a gap to fill, a role to fill, and a claim upon public monies that the existing observatories probably were not going to get because the consortia were what I might call ecumenical. This meant they were meant to serve the whole astronomical community on an equal access basis, not by any quota or rule, but by a review of applicants based on their experience and competence and the merit of their requests. Time with the telescope was reviewed by a panel of peers. This was the same process used by all government agencies for grant funding. On the one hand there were the single institution observatories, mostly existing. Lick Observatory and the University of California represented one; the Mt. Wilson and Palomar Observatory was another; at that time this was a marriage of the Carnegie Institution and Cal Tech, later called the Hale Observatory after George Ellery Hale. Two other university observatories were that of the University of Arizona, and of the University of Texas, McDonald near Ft. Davis, on Mt. Locke in Western Texas. The argument for the second approach was that although research at these institutions did not necessarily have immediate yield, they had demonstrated enduring long-term value by maintaining a long-term program in which there was guaranteed time to their existing staff members. Nationwide requests from the national observatories did not do that and tended to favor the more opportunistic [projects]; a few nights would prove something, it was claimed.

Jarrell: Kind of flashier?

Whitford: Yes, and it was the position of the report first that the national facilities, which were likely to get almost certain federal support should not thereby be allowed to have what I wrote in the report, “automatic unreviewed preemption.” Second, that it was probably in the national interest to have a pluralistic system where both approaches to providing telescopes and a corps of observers from across the country went in parallel, a view to which I still subscribe and perhaps many others do.

Jarrell: About this effort at the national level, was this the first time that such a defining process had been initiated in astronomy?

Whitford: Yes it was. Astronomy was not the only discipline which was going through this. There were parallel ones for physics, for biology. In connection with some of these trips to Washington D.C., I first met Kenneth Thimann, who later became a member of the faculty at Santa Cruz. He was then at Harvard and was a member of the panel reviewing biological sciences.

Jarrell: So this was a very large national effort in the sciences generally to establish priorities and also to coordinate all of these efforts?

Whitford: Yes, that is true. I believe there was also one for mathematics, one for chemistry and perhaps others. When our report was formulated there was a final hearing before COSPUP in which Allan R. Sandage and Frank D. Drake appeared and they made an impressive presentation; the report sailed through and was approved for publication by Kistiakowsky and the then president of the National Academy, Frederick Seitz. It was published in 1964. Not all of what was recommended was immediately approved for funding. The history that follows might be reviewed in the remainder of this interview.

As was remarked in my autobiographical chapter, before the decade was out, there was the need for an updated report which did include space science and which was led by Jesse Greenstein of Cal Tech. Then perhaps a decade after that, around 1982, as I recall, there was a third one, called the *Field Report*, chaired by George Field of Harvard for which there were many subpanels. Sandra M. Faber

from here was involved in that effort. So it was one of three, and I believe that while not everything came out just the way the panel members hoped, it did indeed guide federal agencies in what the leaders of the astronomical community thought were the needed pieces of equipment, facilities, and support that would go farthest toward rapid advancement or realization of potential advances in the field of astronomy. This may be a self-justifying and vain view on my part, but that's the way I see it now.

Along the way, I should mention one thing, that this resentment of the East Coast versus the West Coast became very obvious in the field of radio astronomy in the early venture into the field by Cal Tech, which resulted in the Owens Valley Radio Astronomy project near Bishop, California. That was pushed by Lee A. DuBridge, the President of Cal Tech, formerly the leader of the radar project at MIT during the war. They were early in the field and in competition with the national effort at Greenbank, West Virginia, called the National Radio Astronomy Observatory the leaders of which were very much from eastern and middle eastern universities like Harvard and Cornell and MIT. This smoldering resentment—what was called the Pasadena provincialism (laughter) came to the surface in a very open day of bloodletting at a hearing in Washington. There was a kind of compromise; the report said both approaches should go forward in parallel. According to the testimony of Geoffrey Keller, then an official of the National Science Foundation, it did handicap the getting of millions of dollars that Cal Tech wanted in order to carry out their program. It was very hard to get support for the idea of a competitive alternative at Greenbank.

Jarrell: Did the eastern bloc try to put up hurdles? How would they argue against it if they didn't have comparable facilities?

Whitford: You could get a personal account of what really happened from Frank Drake, who was in the midst of it. But when it came to the choice of funding new and expensive radio facilities it did end in what became the array (VLA) in New Mexico. It was done for the national facility and not as a Cal Tech project. Cal Tech has later come forward with another unique development.

That's not part of my special knowledge or purview but what I say is historically true, I think.

Jarrell: I'm a little unclear on the core issues at the heart of this east-west schism. Would you elaborate on this?

Whitford: I have mentioned it in my New York Academy review of western observatories: "Astronomy and Astronomers of Western Official Observatories." With the wonderful facilities that were developed first at Lick Observatory and after the turn of the century by George Ellery Hale at Mt. Wilson, culminating with the 100-inch telescope at Mt. Wilson, a succession of quite epochal discoveries by some quite perceptive astronomers just automatically fell in their lap. Harlow Shapley showed that the sun was not the center of the universe of stars; it was off on one edge of the galaxy.⁶ Edwin P. Hubble⁷ showed that, yes, the unresolved blobs, called nebulae, were indeed made of stars and that they were stars like those around here. They were external stellar systems which Shapley called galaxies. Hubble, (who did not admire Shapley very much after he left Mt. Wilson and went to Harvard), liked to call them extra-galactic nebulae.

There were also some very solid contributions from Lick Observatory which, for a long time, had much smaller telescopes. Perhaps the two most notable ones were a long program of charting the motions of the stars as indicated by radial velocities from the spectra. W.W. Campbell and J.H. Moore had the leading roles in this program. The second contribution was the study of R.J. Trumpler, which showed for the first time conclusively that there was interstellar absorption which made stars seem dimmer than they otherwise would be and led to an overestimate of their distance. Their faintness was attributed to the fact,

⁶Harlow Shapley (1885-1972) was a U.S. astronomer who deduced that the sun lies near the central plane of the Galaxy some 30,000 light years away from the center. His work led to the first realistic estimate of the actual size of the Galaxy. Shapley was director of Harvard College Observatory from 1921-1952—Editor.

⁷Edwin Powell Hubble (1889-1953) was an American astronomer who is considered the founder of extragalactic astronomy and who provided the first evidence of the expansion of the universe—Editor.

wrongly, that they were quite far away and not to the fact that we didn't get all the light that they were omitting. It was absorbed by the dust that was in the way. This came in 1930. These triumphs, which were almost pure descriptive discoveries, related to some historic speculation going back to Immanuel Kant. You can read about the latter in Hubble's *Realm of the Nebulae*.

This was accompanied by what the East Coast astronomers felt and may in fact have been quite real, a kind of condescension. East Coast astronomers didn't have very good telescopes, didn't have a very good climate, didn't know how to adjust and to focus these telescopes. (Laughter) Shapley was cited as an example; he did wonderful work at Mt. Wilson but couldn't seem to bring it off in his later work. The Easterners had some stations—Harvard had a station in South Africa, and a good one, the one in which the great wealth of information that came from the Magellanic clouds was discovered.

Meanwhile, there was the development of the information that one could get from the new knowledge of how atoms were made, how they were ionized, and excited to emit spectrum lines or absorb them, mainly a European development. A Hindu astronomer, working at Cambridge, England, named Saha, wrote down a great relation in 1922 that explained everything that had been discovered about the spectra of stars as being primarily a temperature effect, somewhat influenced by how compact the star was, whether it was a giant or a dwarf. And Cecilia Payne, a British woman, (who married Sergé Gaposchkin and in later years was known as Cecilia Payne-Gaposchkin) wrote a very important monograph as her Ph.D. thesis at Harvard called *Stellar Atmospheres*. Most of the other developments came from Germany and from Britain.

The Eastern astronomers stated from their side of this great schism that the West Coast astronomers were benighted, ignorant hermits on mountains so wedded to their telescopes, [but which they admitted they used quite well] and too heavily committed to the notion that no clear night hour should go unused. But their criticism was the Westerners did not know what atomic physics and spectra told us about what made the stars go, why they had the spectrum lines that they did; why some elements came out in the hot stars and other elements came out in the cool stars. You can read in the New York Academy historical accounts—

Donald E. Osterbrock has the details and more of the final documentation than I have—about how Donald Menzel, a product of this eastern block, later a long-time faculty member of the Harvard College Observatory, during his short term occupancy of a position at Lick Observatory applied some of this atomic physics to historic eclipse spectra that had been around in the Lick Archives since Campbell got them years before, but still unanalyzed. He could not get his papers past the acting director of Lick Observatory, Robert G. Aitken, because Aitken was just unable to understand it. That may be an inaccurate and unjust characterization but if it is, you can get the straighter story from Donald E. Osterbrock. But you asked about the development of this schism, this difference of view between the East Coast and the West Coast, it certainly was perhaps the central fact of the astronomy of the United States ground-based effort, up to the time of this first ten year study.

Jarrell: So by the time that the Ground Based Astronomy Report was published in 1964, this schism had not abated?

Whitford: I would say it was beginning to be ameliorated, that an accommodation was reached about what the program should say about optical astronomy; there was a pluralistic approach, and there was room for both Cal Tech and a national effort in radio astronomy, that both should be funded. It's not my job to review the whole national history of this. But I think one could say that both sides came to understand each other and to acknowledge and to overcome any lack of a complete and well honed approach that each faction had. Well . . . end of comment.

Jarrell: Yes. Now, to move over a step, even as you were overseeing this first national effort to coordinate this research in astronomy and to see that a set of priorities was articulated, you were, at the same time, director of Lick, and seeing that there was another kind of struggle, which you described last time as becoming strong. This same set of dynamics in terms of growing astronomy, of people wanting to take advantage of resources in a new way; this was happening in your own institution. I would like to know how you started to view the changes in Lick, which we started to discuss last time, in the sense of the

administrative existence of the Observatory. And we can talk sequentially about the various aspects of this.

Lick Observatory's Interest in the Southern Hemisphere

Whitford: May I take a detour that may be a rather extended one? How Lick Observatory became itself involved in this effort to expand facilities, and that had to do with the Lick interest in a Southern Hemisphere station. I have not mentioned the fact that there was an early effort, which resulted in the southern station in what is now the suburbs of Santiago, Chile, called the Lick Southern Station. The telescope is still there. It was called the D.O. Mills Expedition. It was established by W.W. Campbell. It was occupied by William Wright, Heber Curtis, and finally somewhere between 1910 and 1920 after it had finished its announced program, abandoned and donated to the Catholic University of Chile, which now owns it and uses it to a certain extent. But this had been forgotten. It was widely noted, not just in our ten-year program that there was a great imbalance in the number and size of telescopes in the two hemispheres, that the indigenous nations with capitals in the Southern hemispheres were probably not wealthy enough or perhaps sufficiently advanced in their scientific endeavors to fill this gap on their own. South Africa, the South American nations, and Australia, might be mentioned as potential sites. It would be to the advantage of everyone if Northern Hemisphere nations, especially the United States were to establish a large telescope somewhere in the south.

AURA established a site survey centered on Chile. It was led by Jurgen Stock, a native born German. This must have begun not long after Nicholas Mayall, a longtime member of the Lick staff went to see President Sproul about the 120-inch telescope. He became the first long time director of AURA Kitt Peak Observatory. He was hand-picked by Donald Shane. He had a prominent place; he may have been president of the Board of Directors of AURA. I mentioned earlier that they had a very sympathetic relationship. It was about 1960 when Mayall was offered and accepted the directorship. He had not had any directorial experience. On the side of policy and scientific politics, he leaned heavily on Shane's experience and judgment; they spent hours on the telephone every day between Tucson and Mt. Hamilton until he got his feet on the ground. I think

under the influence of Shane he employed James M. Miller, one of the Dirty Thirty in Berkeley that I mentioned last time, as his business officer, and Miller lent great strength to the management side in those early days.

The development in Chile looked toward the construction of a large telescope. It was generally thought it would be a duplicate of the large one already approved for Kitt Peak, near Tucson, the so called four-meter telescope. But somewhere along the way . . . I cannot give you the exact date, but it may have been in the end of the Eisenhower years in 1958, or the beginning of the Kennedy years in 1960, or as late, although I don't think so, of the early Lyndon Johnson years beginning in 1963. The budget crunch in Washington took the telescope off the list of approved planning projects which might lead to large-scale financing. The cost in dollars in those years was on the order of ten million dollars. I review all this because it left a vacuum. There was agitation from two quarters towards filling that vacuum.

One from Pasadena, and the Carnegie Institution under the leadership of Horace W. Babcock, who followed I.S. Bowers as Director. There were conversations with the Ford Foundation, I do not remember the name of the then director, but the assistant director was Carl W. Borgmann. He was oriented towards the physical sciences, whereas I think the head of the Ford Foundation was more interested in perhaps biology and the social sciences. Some people felt that Borgmann had made some sympathetic remarks. This period is reviewed by Frank Edmondson of Indiana University, a retired member of AURA from the founding days, who has archival access. You will find that he has materials which give names and dates. I have made inquiries myself because of my own historical interest. I hope I can correctly speak to those issues.

They, meaning the Carnegie Institution, examined Southern Hemisphere sites, including one in Australia called Mt. McKinlay and one in Chile that was later to become the Las Campanas Observatory; it means "the bells" in Spanish.

Somewhere along the way, I think it was early 1966, the Ford Foundation got a new director. His name was McGeorge Bundy. He had been dean at Harvard at

the time that Leo Goldberg came to Harvard from the University of Michigan as head of the Harvard Observatory. I do not have a note as to the date but I can place it approximately as late 1965 or early 1966. I received confirmation of the fact that Bundy was reviewing what the Ford Foundation had best put its money into, what it could afford to put its money into, and what would be good scientific and public policy. He called me at Mt. Hamilton and said, "We're looking at this and there is a proposal from the Carnegie Foundation. Does it concern you that if it were consummated that the two largest telescopes in the world [they were proposing to build a duplicate of the Palomar telescope, a 200, not a 158-inch telescope, in Chile, at a cost of more than ten million, maybe 20 million dollars] would be under private control, with perhaps less general access than there might otherwise be?" I said no, indicating it did not concern me. I was not worried because they had been used to granting access to their telescopes on Mt. Wilson and Palomar to visiting astronomers. Anyone that got a new gift for what was going to be a unique telescope in the southern hemisphere would feel obligated. Babcock had already implied that they would grant general access. I thought you had to go with the first team, the people that had built or operated very large telescopes in good sites. That meant that I had absorbed some of the California provincialism. (Laughter)

Jarrell: So Bundy went to you as the head of a public institution to see if he could find some kind of prejudice against a private monopoly in that sense. And you were not concerned?

Whitford: No, I was reflecting the prevailing Western sentiment. I have gotten from two sources (Olin J. Eggen and Bernard Burke, a radio astronomer who was then associated with the radio effort of the Carnegie Institution centered in Washington), the intimation that an influential factor in the Ford Foundation's final position was a letter that Leo Goldberg wrote to Bundy. They had had a long, sympathetic relationship since Goldberg had served under Bundy as dean at Harvard. I had heard there was a conspiratorial theory around Pasadena, which doesn't seem to be supported by the historical facts (laughter) that this was known as the "CARSO steal," (CARSO was the acronym for the Carnegie Southern Observatory) that some of these Easterners smarting under this condescension from the western observatories had said, "Well it's time to put an

end to this arrogance.” Well I don’t think it happened that way, but it was a widely held view. I have since asked Horace W. Babcock over the telephone about it. He said that the words “CARSO steal” were not common parlance at the time that he got the bad word. The bad word was that on March 15, the Ford Foundation tabled the CARSO proposal, saying that in view of all their other commitments they were not in a position to put that much money into it. Goldberg said he was asked by Bundy, as an old friend, to give his opinion. He said although there was a proposal to have a kind of a joint national arrangement in which Carnegie would administer the national interest and so on, he didn’t think it would work. It was preferable to give the money to AURA, which had the mechanism to do all this. Later Goldberg became the director at Kitt Peak after all this happened, after Mayall retired.

Well, one can try to look behind what set of mind Bundy could have brought to this decision, being the head of a private foundation, like Carnegie. The Rockefeller Foundation, which had financed Palomar was similar. They had the view that somehow private money made for independence of the political pressures that came with government money. It preserved an organization’s independence which enabled it to be selective, to go its own way, to be bold. The same apology was made for the non-public universities like Harvard, Stanford, and Chicago, that there ought to be a pluralism and they still say so. But the purity of their argument has been greatly diluted by the fact that all of these universities get about half of their operating costs from government grants now, in some cases well over half. This is true of MIT.

Bundy might well have been sympathetic to an application from a private source and he might also have been sympathetic to what he could have viewed as the quite justifiable judgment of the Eastern astronomers who felt that they were wiser and more deeply rooted in an understanding of both the theoretical and the observational science of astronomy. Their view might be that it doesn’t take an awful lot of wisdom or artisanship to run a telescope; (laughter); maybe they don’t know what they’re finding, or don’t understand how to interpret it.

At any rate, somewhere along the way, when this vacuum was not being filled, there was an effort generated internally in the University of California to see if

we couldn't seize the opportunity, find the funds to establish a southern station. Since all the other efforts were in Chile, we thought Australia was a good place to go, away from the political instability of the South American nations. It seemed likely that Chile was climatically superior, but Australia was a very stable English-speaking country. Some time during the year 1963-64, President Kerr made available about \$80,000, (but that would have to be verified), from non-tax funds; it was essentially the overhead money, from, I think, the Opportunity Fund. It supported a site survey in Australia. The leader of the first survey was a young professor at Berkeley, C.R. O'Dell, called Bob. He was later hired away by the University of Chicago. There was a two year occupation of Mt. McKinlay. It was in South Australia, halfway between the south coast and Alice Springs in what's called the Red Centre. And somewhere along the way, and you can find some dates in a historical review of what's now called *The Anglo Australian Observatory*, regarding a long term plan generated over the years by Bart Bok, the director of the main observatory in Australia, Mount Stromlo. It is located at the Australian National University at Canberra. The plan would form a consortium with the British to establish a large southern telescope in Australia. The Australians became rather disenchanted with the lack of progress, and especially of funding from the British side. The then Minister of Science and Education, then known as Senator John G. Gorton, (later Prime Minister of Australia,) said that the Australians ought to see if the Americans were interested in coming in because the British might not do it. There was a visit to the United States by a naturalized Australian known as E.G. Bowen, usually called Taffy because he was a Welshman. I had known him during the war during the radar effort. After visiting three or four possible American sources of participation he found the only interest was at Lick Observatory.

I went to Australia, talked to a council there of which Senator Gorton was president. This may have been in the early days of Olin J. Eggen, as Bok's successor as director of the Australian observatory, Stromlo. We didn't have money in the bank at the time. It was clear that we didn't, but the place where there was money at the time was NASA. They had made grants to the University of Hawaii, and to the University of Texas for ground-based telescopes in the range of 88 to 106 inches. They would be operated by these university observatories with free choice as to how the telescopes were used for 75 percent

of the time, but with 25 percent at the option and direction of NASA as support of their planetary exploration efforts from unmanned vehicles. When we went to Washington to see if NASA would be interested in giving major support, something on the order of ten million dollars for a telescope which might be a copy of the 200-inch in Australia, it quite surprisingly seemed that they did not immediately say no. I cannot now recall the date when these overtures were going on. In the review I mentioned you can find the date when emissaries from Britain found out that this was happening.

At any rate, I think it was some time in 1966, I attended a meeting in Washington where the leading section chiefs of NASA told me that no, they could not fund it, they did not have the money. This is what they would say, whatever the contributing reasons, perhaps the unannounced reasons. Since then I learned that there might have been two contributing reasons. One was some inquiries to leading astronomers: a possible one was Martin Schwarzschild, gathered from a remark that Martin later made to me. These proposals were not explained by us to the American astronomical community so they were caught rather unprepared when NASA made inquiries about us—"were we competent, could we handle it? Could I handle it?" The second very decisive influence was (what shall I say?) the bigamous approach of the Australians, seeking "two partners." This upset and stimulated the British no end! A leading British civil servant on their Science Research Council was Sir Hermann Bondi, an Austrian, a Hitler refugee originally. He was an early associate of [Fred] Hoyle and [Thomas] Gold in the continuous creation idea came to Washington. He said the U.S. Government had no business meddling in internal affairs between friendly nations in the British Commonwealth family. This was a very touchy point at NASA and it may have been the decisive point. Anyway, it all came to naught at NASA.

Coming back to the thread of what happened after the CARSO proposal was laid aside by the Ford Foundation: I'm not sure just where the motivation came from but at any rate, Bundy called up Leland Haworth, the then head of the National Science Foundation, and incidentally an old graduate student friend of mine, at the University of Wisconsin, Madison. Bundy said that the Ford Foundation thought the proposal to have a large Southern Hemisphere telescope had merit.

While they could not do it on their own, they would be in a position to contribute five million dollars, which would be about half of the cost. There was a rumor that President Johnson was going to a South American conference of Latin American nations at Punta del Este in Argentina and he wanted to demonstrate goodwill. So he asked his advisors on cultural and educational affairs: What could we do? Well they said, we have this proposal and if you can say that we'll go the other half, it would be something that you could mention. He did. The financing came along and it was announced that there would be a duplicate four-meter telescope in the Southern Hemisphere under joint Ford and NASA auspices. It was built. The director of Cerro Tololo observatory then being considered had been selected, and there would be a southern station. That had long been assured but a really big telescope was not. Its cost was far more than the total investment of everything that had gone there before. The largest was a 60-inch telescope.

Jurgen Stock had a short term as the first director.⁸ He was very fluent in Spanish and knew the Southern Hemisphere, and the Latin mentality there quite well. But he wasn't a very good organizer and bureaucrat. The search led to the selection of Victor M. Blanco, Puerto Rican born, a Ph.D from Berkeley, then at the U.S. Naval Observatory. He is an American citizen and had the other two requirements, that he be a good scientist and that he be, quite independent of his Latin American sensitivity, a wise scientific administrator. They couldn't have chosen better. He's now not quite retired. He was the one who made Cerro Tololo a reality and in the view of most people far and away the more effective part of the the AURA complex, now known as the National Optical Astronomy Observatories (NOAO). That is a personal view. These events all happened in a few short months and our dreams of Southern Hemisphere glory were thus ended.

⁸"Jurgen Stock conducted the site survey to the time of the site selection in December, 1962. He was then made Astronomer in Charge during the early stages of building the observatory. He was appointed Director in November, 1964 on the same day he became a U.S. citizen. About one year later he resigned and was appointed Astronomer at the University of Chile, where he remained until 1971."—from the Donald Shane autobiography in progress as per Dorothy Schaumberg, Curator, Mary Lea Shane Archives of the Lick Observatory—Editor.

It was sometime around 1966 that it all came together. I think I talked to George Herbig, who as I told you was the strong scientific leader on our staff, a Calvinist, one who thought that good astronomy was perhaps above all what we had to do, independent of when and where we had telescopes to do it with. I said it looked as if U.S. financial support for a U.S.-owned large telescope would only come from the U.S. government. The way it happened was then probably the only way it could have happened. I said that we shouldn't feel that any stronger or prior claim had somehow been ridden over roughshod and that I personally accepted the inevitable. You could ask the opinion of other Lick astronomers then on the staff as to whether I was too, well, what shall I say, detached and did not allow local patriotism to be my most important consideration.

Jarrell: So it seemed during part of your tenure that Lick/UC might be overseeing a southern facility?

Whitford: Well it did for a brief time and looking back on it I see how anyone on the East Coast might have questioned our breadth and depth of experience and my limited competence, because I hadn't done anything except finish the 120-inch telescope, not raise it from the ground up. But I did have to formulate a kind of tentative proposal to NASA as to how it might all be done. I did it quite personally, with no committee reviews or consultants . . . I worked very hard on it and those who read it said that it was good. One thing that didn't shine through was where were we going to find the leading project engineer.

Jarrell: Today we're going to focus on Lick Observatory's headquarters being moved administratively to UC Santa Cruz. I would like you to discuss events prior to the move to UCSC.

Tensions between Lick Observatory and the University of California

Whitford: Ah, yes. I repeat what I've said before, that I am speaking from memory with such small clues as to actual dates as I can find in the public literature. The full, what you might call archival substantiation of what I say

exists in the Director's files that are now stored on Mt. Hamilton. I have not looked at them but anybody who wanted to write a documented history could get dates and perhaps some substantiation, and perhaps some correction of poor memory on my part. Starting from that point, what should we talk about first? I think it might be how the relations between Lick Observatory, located centrally on Mt. Hamilton, and astronomers in other parts of the University developed strains.

Jarrell: I guess the way I was looking at it, or one way to get into the whole subject would be, that when you took office, as Director, there seemed to be two major developments. On the one hand you had a new University President, Clark Kerr, and his new vision of how the University should be structured and run. On the other hand, you had the growing influence and almost, we might say, pressure, of the proliferating astronomy departments on other campuses, primarily San Diego; you already had Berkeley, and UCLA. The growth in those departments seems related to the fact that the 120-inch Shane telescope was there; that there were surely a lot of people interested in sharing control of this very, very superb scientific instrument. So that there was emerging a conflict over control of the telescope. There's a conflict over that that I see. And you happened to come in just at this juncture.

Whitford: I think you have a very good sense of the overall divergence of viewpoints. It developed slowly. Reviewing it, I think I came to the University of California with a generally held view that its standards as an absolutely first-rank university with strong research emphasis were not in any way tarnished by the transfer of the presidency of the University from Sproul to Kerr. It took me two or three years to become aware of the fact that the rather sheltered status of Lick Observatory and perhaps its very favorable treatment under Sproul were somewhat the product of a personal relationship between Sproul and Shane, going back to their . . . well what some people would call cronyism growing out of the years when Shane was on the Berkeley campus and engaged in University and Academic Senate politics. So when the vicissitudes came I always thought that Kerr was going to try and judge us by the same standards that he properly should for a component of a very strong and a very great university. But I never thought that he had any animus, any disenchantment with Lick Observatory.

Jarrell: It sounds like the Sproul era was a pre-bureaucratic era when things were based more on personal relationships . . .

Whitford: Yes.

Jarrell: . . . and with the coming of Kerr, just looking retrospectively it seems that you're saying he wanted Lick to be accountable in a way that it hadn't previously been: there hadn't been a need for it. Or it wasn't perceived as such, maybe.

Whitford: That is right. I wouldn't go so far as to say there hadn't been a need for it; if there were a need it wasn't exercised. It may very well have been that this somewhat too-rosy view of the success and scientific reputation of Lick Observatory, a view which may come up in some things I will say later, was not appreciated or allowed for, either by the old-time members of the Lick staff, or me as a new one. Now, to get to the chronology—is that what you would like to do?

Jarrell: Certainly.

Whitford: Here my exact dating could be corroborated. I mentioned in our last interview the various 120-inch auxiliary instruments that could be attached or taken off and alternate ones substituted. It took about four years for some of these things to get made. I had a very large part in the intimate technology of some parts of the optics and in a few cases the electronics. The usefulness of the telescope as a simple one, or with new devices, particularly a scanning spectrometer that Joe Wampler commissioned resulted in its increasing use by astronomers from Berkeley. I mentioned Hyron Spinrad and Leonard Kuhi; from Los Angeles; Lawrence Aller, and Daniel Popper, and from San Diego, particularly E. Margaret Burbidge. Of the total available, assignable nights on the 120-inch, the fraction of assigned people who were not on the staff of Lick Observatory grew to somewhere between 45 and 50 percent, while we were still living on Mt. Hamilton. How this time was to be allocated was based upon I think 90-day requests, (that could be checked) which were mailed to me. I

personally, perhaps with some consultation, ad hoc with staff members, added up the total, decided how it had to be adjusted . . . But a rather high percentage of what people wanted was granted both from within the staff and from other University of California observers who came to the mountain for a few days each month. This was in contrast with an application to award level of maybe one in three, in places like Kitt Peak and Cerro Tololo, as of the present date. Nevertheless there were some disappointments.

There was also a point of view, perhaps not unlike the view that I mentioned of the East Coast astronomers, going back to the turn of the century, directed against these mountain hermits, who knew how to run telescopes and how to get superb results out of them, but who perhaps had less extensive knowledge of the astrophysics behind what the stars could be telling us. A built-in plus that these campus-based astronomers had was that they were already members of teaching departments with their graduate students. Some of these Ph.D's got their degree under an astronomer who was on the Lick staff. This could be George Herbig or George Preston. In the first case Leonard Kuhl would be the candidate. In the second case it would be Sidney C. Wolff, now the Director of Kitt Peak National Observatory.

Jarrell: Now these would be graduate students based at Berkeley?

Whitford: Yes, they were . . . there was another one from Los Angeles, Donald Hayes, under Daniel M. Popper. Some of them actually lived on the mountain for a year or so.

Jarrell: Shane, in this little written memo fragment that I have to track down, talks about the whole question of teaching and the granting of Ph.D.s vis-à-vis Lick. Did Lick teach graduate students?

Whitford: It did teach, but not in a big obligatory percentage of anybody's time, not because anyone had membership in a teaching department. Going back to the rather favored days of Lick Observatory, beginning under Campbell, Lick Observatory astronomers resident on Mt. Hamilton had the title astronomer,

not professor, but they were voting members of the [Academic] Senate, had all the rights and privileges there unto appertaining. I believe these are the words that are used. (Laughter)

They were the only persons in the University who had that privilege without the title professor. This rankled somewhat. The campus people who liked the teaching relationship liked to take credit for it. Some of them realized that the Lick Observatory people had somewhat of a free ride. The point of view of the Lick people was, yes we teach when asked, we are glad to guide students when we have students that want to, when we have what we think is the proper background, and our quid pro quo, in service to the University is not teaching but seeing that the telescopes are there, that they work, that the instrumentation is updated as fast as we can do it.

Well, there was some rankling in a member of the Berkeley department referred to in Shane's handwritten memorandum as Harold Weaver, who had been on the Lick staff, a son-in-law of Robert Julius Trumpler. He rankled under what he felt was less than full recognition on Mt. Hamilton, and when offered an appointment in Berkeley, (I think by Otto Struve, but that I couldn't swear to now,) he accepted it immediately. Shane confessed to me privately that he was relieved, but he tried not to show it to Weaver. Weaver was an ambitious planner and Shane, perhaps conscious of this less than sympathetic relationship he had with Weaver when they were both on Mt. Hamilton, may have ascribed some dissatisfaction that Struve felt because Weaver was an ambitious planner and into campus politics. He engineered the entry of the University of California into radio astronomy at Hat Creek after a year's leave at Harvard. Hat Creek is in the northern part of the state, about 70 miles east of Redding in a rather isolated area. A radio observatory still exists there. It's south of Mt. Lassen. We do not know how it all got started, but more than one person at Lick Observatory suspects that some of the rankling that the campus-bound astronomers felt about our perhaps favored status, not having to teach, our self-appointed feeling of elitism, may have had a focus in that history on the Berkeley campus.

The other focus could well have been Geoffrey Burbidge, at San Diego. His wife, E. Margaret Burbidge was the observing astronomer. He was a theoretical astrophysicist himself with not too great sympathy for people who actually stay up all night and use telescopes. He later became the Director of Kitt Peak National Observatory and was rotated out when the superdirector, John Jeffries was appointed. He is now at UCSD. Here I think one has to say that he was a very ambitious astronomer who desperately wanted to direct astronomy in what he considered a less pedestrian approach to what problems were addressed. He has always taken a maverick position in regard to the leading observational astrophysicists, as opposed to theoretical astrophysicists. This may have just been power hunger: he sensed that there was a soft underbelly in the political capital that the Lick astronomers had.

Outside Critiques of Lick Observatory

Jarrell: Now I know that he figures very prominently in the whole critique of Lick that came from San Diego which I guess was part of his critique articulated in the Revelle report?

Whitford: I do not know about it. I've not read the Revelle report.

Jarrell: I haven't either but I learned about it from a number of your colleagues here. Elitism was certainly an implicit sort of charge against Lick and the idea that a campus ought to have professors . . . It was a very harsh critique of Lick and then in the same period was the Brode report that was generated out of Berkeley.

Whitford: What's the date that you have?

Jarrell: I don't have the exact date for either of them although it was probably in the mid-1960s. I couldn't find copies of either report. But the significant thing about both the Revelle and the Brode reports was that they they never bothered to get responses from Lick Observatory people. The points of view in these reports seem to have been political, one-sided documents. I wondered if either of

these reports had influenced you. They were part of the ammunition, part of the controversy.

Whitford: You have perhaps (laughter) a little broader documentation on this than I do on some of these things. I have never read the Revelle report that I can recall. The history is something like this. (I cannot give you exact dates.) During this period we felt that we were meeting the needs of observational astrophysicists from other parts of the University of California, admittedly on our own terms. There was an incentive for these campuses to appoint new, young observing astronomers, with a promise that here was this great telescope, the second biggest in the world, already performing very, very well. Examples of these appointments were Lawrence H. Aller to the Los Angeles campus, and C. Robert O'Dell to the Berkeley campus. Aller was in mid-career; O'Dell a very fresh Ph.D. All of these appointments and all of these promises of observing time on Mt. Hamilton were extended without any consulting mechanism with Lick Observatory, and so we found about them after the fact. This created some alarm on Mt. Hamilton, most keenly felt by George Herbig, that we would get to be like the old woman that lived in the shoe.

I saw the problem. We were by this time no longer reporting directly to the President of the University; this shift from a campus status that Shane knew was going to end came within about a year. He managed to get it postponed from the first day of Kerr's presidency.

Jarrell: Yes, you got a year's reprieve.

Whitford: We were already assigned to the Berkeley campus. The Chancellor at the time was Edward W. Strong. I wrote a rather bristling letter and sent copies to the Berkeley Chancellor and the other campuses where there were research astronomers, saying this was no way to run a railroad. I think unconsciously I was reflecting this proprietary attitude which only raised hackles higher than they already were among the off-mountain astronomers in the University. Here was proof that we were elitists. We were trying to give time, but we had the

power to decide how much time could be given and this policy could only lead to catastrophe.

Well, I think this stirred up the animals and probably led to the assignment of the problem to Robert B. Brode, whose assignment was scientific advisor to the President of the University, then Clark Kerr. He was a cosmic ray physicist, a well-respected member of the National Academy of Sciences. I didn't know much about what he did. He never gathered data or statistics from inside our operating domain. When the report came out it claimed that, yes indeed there was limited capacity of the Mt. Hamilton telescopes to serve astronomers, that the highest responsibility was to the campus-based teaching astrophysicists, and that since it wasn't likely that there could be any expansion of the number of useful observing nights on good telescopes, the number of astronomers that had access would have to be reduced. I'm paraphrasing this from memory but I think it is fairly accurate. As a way of accomplishing this, the number of resident staff positions called the junior tenure ladder tracks later, the astronomer equivalent series to professor, should be reduced by three. So the people on Mt. Hamilton who stayed, those with tenure and rank, would have the ability to do whatever they could. There was implied the idea that their strongest contribution was as builders, instrumentalists, and maintainers of the telescopes, essentially telescope jockeys, technicians.

The three people who would have to go under this view if it became adopted as University policy, were Merle Walker, Thomas Kinman, and George Preston. Maybe I should say George Preston and Thomas Kinman. The first order was the order of age. They were assistant astronomers, all of whom were up for tenure review. I think Walker was already in the process of being reviewed for tenure. The other two were in sight of it but hadn't been yet recommended.

Well this hit us as being not in line with the standard University of California policy, the sort of matter that comes before the Committee on Privilege and Tenure of the Academic Senate. It monitored the rights of professors, whether they be tenured professors or not. Our young astronomers were, we thought, most prominently in the case of George Preston, fully as promising and productive as the people who were on the campuses. We tended to think of the

astronomy of the latter as perhaps worthy but somehow not quite as recognized or achieving as our own. This was rank provincialism, looking back, I'm willing to concede. But in any case, there had been no external review of their performance, the standard procedure that is due every assistant professor, more than a year before he comes up for the recommendation for tenure review. And it hadn't happened. They were simply being thrown out because there was no guarantee by the University that they had to be reappointed. They didn't have tenure. I remember hearing Vice President Harry R. Wellman say, "The University has no obligation towards these people." He held up to us as an example Harvard University's policy, where assistant professors are almost never advanced to tenure; tenure appointments are made from a nationwide slate of candidates in which Harvard assistant professors are not on the wish list. This was not a bad idea, Wellman said, and maybe we should think about it.

Well it hit us very, very hard and in looking to how Brode could have gotten that solution there were two diagnoses—that he had asked around amongst the dissatisfied people on the campuses, perhaps leaning heavily on Harold Weaver. The only confirmation of that is what Robert O'Dell said. He was one of those who was temporarily unhappy because he didn't get as much time as he wanted. But he remained a loyal friend of Lick Observatory; he thought we had contributed. This may be a somewhat wishful recollection on my part.

Another view is what Shane gave in a later review of it. He had known Brode as a Berkeley faculty member when they were both in Berkeley. He thought Brode was a person with a rather limited "bookkeeping mind," that he didn't understand what makes an observatory go, the kind of motivation, recognition, selection of talents that contribute to the common good as well as advancing the science of astronomy. The exact date when this report was delivered to me I do not know. I think it may have been sometime in 1965.

Meanwhile, there had been for some years a meeting of all of the UC research astronomers at one campus or another. There was a meeting at San Diego. I think there was a proposal that the assignment of the time on the valuable 120-inch telescope be given to a committee which included a majority of non-Mt.

Hamilton people, or at least included a heavy representation of non-Mt. Hamilton people.

Jarrell: So at this meeting of all the research astronomers in the UC campuses emerged the establishment of a committee . . .

Whitford: No, it did not. That was one of the problems.

Jarrell: Oh, it did not?

Whose Observatory Was It?

Whitford: Yes, I was feeling some acrimony, or persecution complex, if you want to call it that. I was not the only one on Mt. Hamilton that felt that way, so we were less than friendly. I guess I would summarize it by asking how did we view the 120-inch telescope? Whose telescope was it? Whose observatory was it?

Jarrell: That's a very fundamental question.

Whitford: Yes. Did it belong to the people who had put blood, sweat, and tears into it over twenty years, and had made it come to fruition, or had we been merely custodians who had prepared it for these nobler and more worthy characters who lived on teaching campuses?

Jarrell: A very painful and deeply felt issue.

Whitford: And so I said no. I was angry. But when we got back home and I think this was in May 1965, but I do not know for sure. That was the time that probably during the year 1964-65, George Preston had heard Geoffrey Burbridge's remarks to Thomas Kinman, both British-born. These three staff members, assistant astronomers, including Kinman, were kept from the knowledge of their possible discharge. The Brode report may not yet have been officially rendered at the time. But Geoffrey Burbridge in George Preston's

hearing, said to Thomas Kinman, “Tom, we’ve got you on the run. You haven’t got a chance. We will bury you.” Preston, who was a very emotional person who very easily blew up, seeing possible future scenarios and Armageddon, came home very upset and over the following few days he himself generated the plan of moving to Santa Cruz.

Moving to UC Santa Cruz

Whitford: He secured without any solicitation on my part the consent of all of the Lick astronomers; two foot-draggers were Gerald Kron and Merle Walker who loved life on Mt. Hamilton. Kron resigned before we moved. Merle Walker is still here. Neither one of them craved the opportunity to teach but they signed it under duress. The establishment of a new campus here at Santa Cruz had been in the works for several years. The campus had been acquired by the Regents. There was a campus plan of the colleges generated by the dream of McHenry and Clark Kerr. In geographical distance it was the nearest campus to Mt. Hamilton, in driving time no closer than Berkeley because of Highway 17. I had thought that it was a way out but I thought that there was no chance that the staff would go for it.

Jarrell: But you saw the handwriting on the wall too?

Whitford: Yes. Meanwhile, there were these interviews with Harry Wellman, the Vice-President of the University, whom Clark Kerr had asked to look out for Mt. Hamilton. He was a very friendly person, an agricultural economist by trade, and later when Clark Kerr was fired by Ronald Reagan, the successor Acting President. He acquainted me with Clark Kerr’s joint appointment rule, which I may have spelled out before, but the general thrust of it was that of the functions within the University, teaching had to be very high. Although there were some entities such as the IGPP, the Institute of Geophysics and Planetary Physics, headquartered at Los Angeles which was an example of a place that had research appointments where scientists with high rank and high salary did not teach; they were given full time for research. This was considered a come-on. It got some very prestigious people to sign on. Clark Kerr said that was not the way to have

a university be a university. There were precedents because the Rockefeller Institute of Medical Research changed into a university.

The Institute for Advanced Studies at Princeton has some seminars. I don't know whether they have formal classes. It still exists. The way this policy could be achieved here would be while nobody would have any rights that were granted when the person became a member of the University of California would be taken away from him, they would be the last persons so appointed. Any new appointments could only be made part-time in research institutes, sometimes called ORU's, organized research units. The other part would be in a teaching department. For us this could not possibly be fulfilled. We were not a campus. We could not hold regularly scheduled formal classes on Mt. Hamilton and have any student body. Another example was the Scripps Institution at San Diego which had a number of appointments with no teaching duties.

Well it was the shadow; the handwriting was on the wall. Somehow we would have to one day accommodate to it. The conversations with Dr. Wellman made it clear that this would happen. That was an influence that we saw that would find its resolution if we did move to a teaching campus and become the nucleus of a teaching department. This could be either at Berkeley or at Santa Cruz and be within commuting distance of Mt. Hamilton. But by going to Berkeley we would first, be coming automatically into rivalry with the existing astronomers; second, whether we had that rivalry or not, being a sort of separate institute that either did or didn't have special status, would almost certainly be source of friction. By coming to a new campus we could write our own prescription more or less. When the proposal was put to Dr. Wellman, he took it up with the President's Council and with the head of the Santa Cruz campus and got approval from both sides. He said, "It's good for Lick Observatory and it's good for the Santa Cruz campus."

McHenry wanted us to move right away. We set the date for the fall of 1966. The move happened in November, 1966. There is a sort of prospectus that I wrote for *Sky and Telescope* in that year telling about what our plans were. We advertised for graduate students, a group that would arrive in the fall of 1967. We got a very good group. We filled a vacant staff position with a theoretical

astrophysicist, Peter H. Bodenheimer, who became a very respected member. He is an astronomer, not just a professor. I think we felt that this was our manifest of good faith in that we wanted to emphasize the theoretical astrophysics side of this pairing of observers and theoreticians on the campus, a group that we hoped would be amplified by new appointments from campus budget support. We brought our own ORU budget with us, thank God. What we thought is that the campus might come forward with half-time appointments for the arriving astronomers out of the campus budget. Well the campus budget did come forward with 20% teaching support for astronomer titles.

Jarrell: 20/80 then?

Whitford: 20 percent teaching, 80 percent research. Reviewing all the budget strains at the time and the amount of time that we do have to teach, I don't think it was unfair, but it was less generous than we hoped because it would produce less new money for theoretical appointments. Must we use some of our ORU money to do that?

Jarrell: I see it as a very creative decision to come to Santa Cruz, that the internecine past history aspects of going to Berkeley would have been much more difficult?

Whitford: Yes, some parts of the University thought we were rather secretive and hasty in this. I think this had its origin in George Preston's rather ju-jawed determination—"We'll show them! We can beat them at their own game." (Laughter)

Jarrell: So in your directorship once the decision had been made, it was a loss of a certain kind but it could be turned into something positive to the degree that you could influence and shape the changes in Lick's status.

Whitford: Yes, residence on Mt. Hamilton as a self-contained community living together had created a certain degree of comradeship and a bond that doesn't happen when professors commute to Orinda to their homes. So we kept our

identity and we felt that if we went to an existing campus, and Berkeley was the only candidate—I'm rationalizing it after the fact, I suspect—we could not see that that identity would be preserved. That was the strong motivation for coming to Santa Cruz, a new campus, where we could help to write some of our own ticket; some of the problems that arose will come out later. Well that's some of the rivalry and what you might call power politics, scientific politics in the University that drove us, (the word drive is not an overstatement) to Santa Cruz.

The delivery of the Brode report and our fear that it might indeed become the policy of the University, happened in the last weeks, the last months that we were on Mt. Hamilton. I wrote protesting letters to Wellman. I talked personally to President Clark Kerr and I said we had felt that we were succeeding in doing what we were supposed to do as a distinguished research arm of the University. This may be paraphrasing it in perhaps more detached terms than I did at the time. I meant to imply that if we have overestimated ourselves in our self-evaluation, the University certainly can find out about it. There are plenty of astronomers around the country whose opinion could be sought. Are we a strong part of the astronomical effort in the University of California or not so strong? His reply was, "Yes, I know. But we do not like to do that. We feel that we have enough strength inside the University to make those judgments." I appealed to Chancellor Dean McHenry who was to become our boss. I was urged by Shane and by Kenneth V. Thimann, whom I met when I visited the campus before we moved here, to go to President Kerr and lay it on the line and say, "This is no way to run a railroad. Do you want me? Tell us." I demurred because there was a feeling, a rumor I got, that President Clark Kerr came to rather final judgments that were perhaps argumentative and he never backed down. Anyway without that ever happening, it did get resolved. Along the way Chancellor McHenry said, "Well this comes at a time when I am coming to the culmination of the dream of years, the opening of the Santa Cruz campus. It is some of the most crucial days of my life. I am going to ask the Vice-Chancellor for Natural Sciences, [I believe that is the correct title], Francis H. Clauser . . ."

Francis Clauser

Jarrell: Yes. He was the go-between between you and McHenry.

Whitford: Yes. He was to look into this. He and his wife came to Mt. Hamilton. He was a charming person. He had been in the engineering sciences at Johns Hopkins, one of two twin brothers. The other one was at the Naval Postgraduate School at Monterey. He had a good deal of charisma, and married a sister of Edmund M. McMillan, the famous nuclear physicist and Nobel prize winner at Berkeley. I felt, well, we're going to get some help. But when we got moved down here in November, 1966, it became clear that what we felt was good for us, what we felt was good for the University of California and we thought was good for the Santa Cruz campus became, (this is a personal judgment, but it's not mine alone) mixed with Clauser's personal, political and scientific ambitions, which had some Machiavellian aspects. He had, before he went to Johns Hopkins, a long background working for Douglas Aircraft, later McDonnell Douglas. He in fact designed one of their widely used planes personally. I don't remember which one. He had many friends in the aerospace industry, among whom was a man named Bradley Young, Brad Young, a high scientific politician at Lockheed, in Sunnyvale, and also an aerospace engineer. Lockheed was expert in lobbying R&D contracts with the Pentagon and with NASA. Clauser invited us to have dinner with Brad Young, or Brad Young invited us to have dinner at some fancy Peninsula restaurant, I think it was Caesar's Palace . . . (Laughter)

They weren't spending corporate money. They were spending contract overhead, I suppose. I don't know. Anyway, there was already a good deal of talk of the space telescope then. The first unmanned space telescopes were already in orbit; the second launch was called the Wisconsin Smithsonian package. They were looking forward to the time when there could be a really large telescope in space perhaps occupied by manned operators, or at least visitable. Whoever got the contract would have a very rewarding, one might even say juicy bit of R&D "pork." (Laughter) These are disloyal and derisive opinions but Clauser felt that we could somehow grab the ball and run away with it ahead of the pack. We had the astronomical and optical expertise. Lockheed had the inside track on new aerospace engineering, which we did not. This was in use already in some of these unmanned astronomical vehicles, two of them. This seemed absolutely insane to us. Even if his fondest dreams were true, that we could be the chosen agent, the right group of astronomers to do all this

stuff, one could think of other likely groups: American Science and Engineering which was getting started in X-ray astronomy; the Princeton group under Lyman Spitzer, which had the Copernicus vehicle, not launched at the time but the third and most productive model of success; they had a lot of money and developed a satellite group at Princeton that knew aerospace engineering.

The Princeton group expected to get the final contract for running the Space Science Institute. They didn't. It came to a group centered in AURA. The institute is located on the Johns Hopkins campus. I think the general opinion is that they got the contract because they were better at writing a proposal than the Princeton group. It wouldn't have been many months before the actual launch of the telescope by the shuttle it hadn't been for the Challenger disaster. It would be postponed at least a year.

Getting back to 1966-67, when we became very obvious foot-draggers in this wild dream. This wasn't our forte; it was a good thing, but we were ground-based astronomers, and that's what we do best. Whatever was done from space, ground-based astronomy was going to remain fundamental and that's our direction.

Jarrell: It sounds as if Clauser wasn't sufficiently appreciative of the fact that you were ground-based astronomers and had a very long historical tradition of doing a certain kind of thing?

Whitford: That may be a correct evaluation. It may be unkind to him. What I started to mention quite apart from his ambition to get to perhaps a personal high place in this venture, was our awareness of how these things had been done in other branches of science. If there were to be a national facility, and a very expensive one, the operating unit which built and maintained all of this had the responsibility of seeing that access was guaranteed to properly qualified users from all over the country. An example would be the great accelerators like SLAC [Stanford Linear Accelerator], and Fermi Lab outside of Chicago, and indeed the AURA ground-based telescopes in Arizona and Chile. We saw that there might be in any case more than one consortium of universities or a potential

consortium of universities which would like to undertake this role, which felt that they were peculiarly qualified, or just wanted the job. (Laughter) To try to grab the ball and run away with it by lobbying in the way that Lockheed knew how to do and was *de rigueur* in especially Pentagon R&D, was just not something we wanted to have any part of or thought was right that we should.

Jarrell: It wasn't your style?

Whitford: Yes. That it would be something that some kind of a screening committee would review and the funding agency, be it NASA, be it National Science Foundation or whatever, would finally take the responsibility of making the choice and the most qualified group of scientists or consortium of universities would get the job. That's the way we thought it should be. It was the honest and non self-serving way to go about it. If we wanted it and were the best qualified we should go after it that way. Right then we didn't think we wanted it. It wasn't our dish. But this led to growing coolness, one might almost say acrimony between Clauser and Lick Observatory. As a way of fulfilling his role as an administrator, putting it in the most detached language I can, he appointed an ad hoc blue ribbon visiting committee to examine Lick Observatory as a growing institution in the first year that we were on this campus. Its members included Leo Goldberg, later Director of Kitt Peak, Allan R. Sandage, a well-known name; I don't remember for sure the other members. Anyway, they came here for a two or three day visit. I never saw their report. When I asked Sandage what he remembered about it he said, "Well generally it was favorable."

George W. Preston's Resignation

Whitford: It's somewhere in the campus files. But meanwhile George Preston, who was a rising and promising astronomer, had somehow induced Bob Kraft to move to Santa Cruz, because Bob was dissatisfied with the relationship between Mt. Wilson Observatory and Cal Tech, which became joined in a thing called the Hale Observatory. He was vulnerable and we got him to come here on George Preston's suggestion. Preston became less and less satisfied with the way things were going here. He was approached in the summer of 1967 with the

offer of a high-level appointment at Mt. Wilson observatory. The Carnegie part, not the Cal Tech part. He turned it down and said: “This is my spiritual home but I will be watching how things develop here.” As things got more and more acrimonious during that fall of 1967, some things happened that made him take a rather emotional evaluation.

Jarrell: I’m unclear as to the nature . . .

Whitford: There was one other factor coming in here. Ronald Reagan had become governor. He had fired Clark Kerr. He had said he was going to cut, squeeze and trim the University budget and the organized research units fared rather poorly. The Regents felt this was wrong and they dipped into their non-tax funds to help out for one year. But as a part of the University budget structure there is a grant to the campus of a certain amount of monies for the support of the scholarly and research activities of every faculty member. Added all together this was a rather attractive sum in the extreme straits in which we found ourselves, where pink slips weren’t so far away.

I was afraid. I was under the control of Clauser. I said, “Can’t you make it available to help us through this?” He said no, that it had to be treated just the same as all the other parts of the campus and he was not going to make any exceptions. He wasn’t ready to decide how the total sum would be disbursed. I cited some of the things that we had done to prove our good faith—brought down surplus-and-used machine shop equipment from Mt. Hamilton, had not asked for new machine tools, whereas the other sciences had got a spanking new shop. And I said something about envious eyes; I thought I said it in a negative sense, but anyway he picked it up and wrote back saying it was time for Lick Observatory to go out and get some grants. Now I said that just now with some rancor in my voice. I take it out. No rancor. There aren’t any heroes. There aren’t any villains. This is just what happened.

Well the fact is, starting from the days on Mt. Hamilton, we had all applied for support grants for our research, mostly with the National Science Foundation. I think, compared with other faculty members on the Santa Cruz campus at that

date, our standing would have looked very good and perhaps better than any other scientific group. He didn't know that or he hadn't bothered to find out and I rather plaintively said that maybe he ought to look at what we'd already done. We had already applied for the Southern Hemisphere telescope and been turned down. I said: "We had missed it but we didn't miss it by a mile."

Somewhere along the way George Preston saw some of these letters. I circulated them. And it flipped him. He was a rather emotional person. He wrote back to Carnegie right around the turn of the year, the beginning of 1968, and said, "I don't know whether you still have a position or not. If you do, I'd like to hear about it." Pretty soon came the offer, yes they did. And without telling anybody here he accepted it, resigned. It was a terrible blow. He was a promising young astronomer.

Jarrell: Several people in consultative interviews that I held before we started asked me to ask you why he left.

Whitford: It was in the last days of January, 1968.

Jarrell: I could tell from the people with whom I spoke that he meant a lot to them and that it was very difficult when he left.

Whitford: Well it was very difficult for everybody, especially for me. As I told you, he is an emotional person. He tends to overdramatize things into a kind of Armageddon situation. After it had all happened I asked him what went wrong, asked him to tell me about it. He said, "Well I've had a lot of second thoughts." And he intimated that maybe he had been overwrought.

Well, let me back up several weeks in my own bewilderment, perhaps frustration, and failure to cope. I think one would have to say that that's not unfair. During this growing estrangement with Clauser I realized that I was not going to cope with the situation unless we got some outside help. Appeals to McHenry, (I don't remember any exact laying it on the line by interview or by letter) led to a sense that his priorities were not going to get us anywhere. This

was recognized most vocally by what Stan Vasilevskis said at my birthday party last October (1985) when he reviewed some of these things in the hearing of Dean McHenry, whose beaming smile didn't tarnish the slightest during these rather critical remarks. You know him I'm sure. We are on very friendly terms still.

But it was a sense of the staff here that we had grown up in a scientific world where research prowess was our objective, our reason for being. We expected to be judged on a national scale. We thought that that was what the University of California and certainly the Berkeley scale of values was; that the University in its scholarly and research achievements wished to be judged on that scale, that it was aiming to be one of the three or four best universities in the country. And it was. We came to a campus where we recognized there was going to be an experiment in undergraduate education, the college system, which was the dream of Kerr and McHenry going back many, many years. Graduate education was going to have a smaller role, at least at first, than it had on any other campus. But we knew that they were glad to get an advanced research institute handed to them with a budget brought along on a platter. We thought that for the fraction of the graduate effort on the campus we would be living under the same rules, the same set of priorities that we were used to on Mt. Hamilton and that scientists at Berkeley and some of the other research campuses like Los Angeles and San Diego expected. Either McHenry didn't realize this or he was so committed to this dream of the new undergraduate experiment that he didn't want to spend any of his rather limited total resources and political capital on somehow resolving this crisis.

Jarrell: This was about graduate students?

Whitford: Yes. And about the acrimony with his appointed Vice Chancellor, Francis Clauser. This was sensed among the staff. I was sensing that we weren't getting anywhere. On a visit to Tucson, in January of 1968, I talked to a Vice President of the University, Earl C. Bolton, who was in charge of a certain part of the business contracts of the administration. There were two representatives to the AURA board of directors from each university. I was the scientific one and he was the administrative one. I said that we were having trouble at Santa Cruz.

“I’ve got to find some way to go around our Chancellor. It’s a delicate matter. How do I do it?” He said, “I’m not surprised. That’s just the way things seem to be going at Santa Cruz. I would advise you to go directly to Harry R. Wellman, tell him what the problems are, and see if he can’t find a way to assist.” I was wondering whether I should do it without telling McHenry or to tell McHenry, I’m doing it, and I’m doing it alone.

Jarrell: Delicate.

Whitford: Yes. But I didn’t ever do it. Because I came home and the next day George Preston’s resignation was before me and it was too late. Over the weekend there were councils between the senior members of the Observatory staff. They made it clear to me that although I had done a lot of things that they very kindly praised, they didn’t think that I was going to be able to . . . resolve the impasse between me and McHenry and Clauser. Maybe it was time to let the directorship pass to fresher hands. Bob Kraft was in the wings. He had just come, hadn’t gotten enmeshed in all these things . . . he was a Berkeley Ph.D., originally.

Whitford’s Resignation

Whitford: I resigned secretly. I thought it was best for the University not to make a big deal out of it. I took a sabbatical. Bob Kraft became Acting Director with me on leave without a public announcement about my resignation. McHenry accepted it immediately. Then I asked Preston what went wrong. I found he had been to see McHenry before me. He went to see McHenry in a very charged atmosphere and told him about resigning. He told me that he said to McHenry, “I suppose you know what you were doing. Maybe you wanted to get a first class son-of-a-bitch for your Vice-Chancellor for Science. You got one.” Well now, these are the most poisonous words I’m going to put in this transcription but those are almost literal quotes.

I know myself where I fell short in those six months. I should have been holding consultative meetings with the Lick senior staff members about the impasse and

how could we get out of it, instead of retreating to this lone secret consultation with Vice President Earl Bolton. Well anyway, that's the end of the story as far as I'm personally involved. You'll have to ask the people who resolved the impasse. Bob Kraft I think had the biggest role in it. Relations with the other campuses were made respectable and friendly. There was a long period in getting a permanent director and I never knew whether they just never found a person who wanted to take it . . . George H. Herbig filled in for a year while Bob Kraft went off on some kind of leave, or whether the potential candidates somehow sensed that things weren't yet resolved in the University of California. The person who finally accepted it was Donald E. Osterbrock. And you can ask him why he decided to accept. He was asked more than once. And I think that comes to the end of the line.

Astronomical Research After the War

Jarrell: This is April 15, 1986 and this is my concluding interview with Dr. Whitford. I'd like to ask you about the economics of astronomical research after the War and in particular during your directorship of Lick Observatory.

Whitford: This is not a question for which I have a prereviewed and considered answer. So I will speak ad hoc, from memory. In the era up to the Second World War astronomical research was supported mainly inside universities from whatever resources universities were able to get from their standard budget sources, local in almost every instance. There was in addition a conspicuous example in the Mt. Wilson Observatory, founded by the Carnegie Institution, and by the time I first went there in 1933, funded by the interest on the Carnegie endowment. And one could say it was rather adequately funded. The tradition of founding new observatories, telescopes and observing stations, not adjacent in those years to the in-city or near city locations of the headquarters, came up against the need for rather large grants of money and in particular, Hale's dynamism. His uncanny ability to represent the potential and the promise of such facilities led to the great success of the reflecting telescopes on Mt. Wilson. It also justified the expenditure of a then very large sum—it was about seven million dollars—for the 200-inch telescope. The money to do that came, I think, in 1927 or 1928, and the whole story is told by Helen Wright in her official

biography of George Ellery Hale. That ends a chapter, a tradition in how astronomy in the United States graduated from the European tradition, which did not emphasize large telescopes.

It did emphasize a close correlation between knowledge of laws of radiation and the way atoms absorb radiation, a graduation of the United States from this tradition, which was in effect at the turn of the century, to a position of leadership and dominance that came with Hale and the American genius for these very fine telescopes. The 100-inch was finished in 1918. The 200-inch appropriation came about 1928. Its completion was of course delayed by the hiatus of everybody scientific becoming involved in the War effort. As you know, the funding came from the Rockefeller Foundation and was given to Cal Tech.

The transition to the new mode came in the post-war years, when it was recognized as a result of the very successful war-time efforts of the scientific community, of physical sciences principally, to a war effort. They were given enough money, and they proceeded to utilize it. Indirectly our defense potential had spin-off, rising from the justification that there were unrealized possibilities, by war-time developments such as radar. This development played a very large background role in the development of radio astronomy, an effort in which the United States had two pre-war pioneers, [Karl] Jansky and Grote Reber, but later advanced more rapidly in Holland, England and Australia than here. There were grants by some of the government agencies in Washington, the Office of Naval Research was the first, and they had as the armed services often do, still do, grant money that they did not have to wring out of Congress for some non-utilitarian purpose. There were grants both to optical, and especially to radio astronomy out of this fund. I think it was about 1952, that the National Science Foundation was established by Congress. I suppose some members of the higher scientific community did some lobbying. It went on from there and it began with grants to existing astronomers mostly at universities.

The story of how the National Observatory concept grew I've told in my autobiographical chapter in the annual review. Other archived materials having to do with that have been collected by Frank Edmondson of Indiana University

at Bloomington in a history which he is preparing. What the relation should be between these national facilities supported by federal budget items and the National Observatory has grown into quite a large item and between university-based and what are now called private observatories is a question that was addressed in our ten-year report. The general stance was that they complement each other and it would be a great mistake to let either one or the other dominate the picture, that diversity and civilized, or friendly competition was a good guarantee that we would have healthy development.

Now how did Lick Observatory stand in this spectrum? One, there was a long history of University of California support, a condition of Mr. Lick's initial gift, that stipulated that when completed by his trustees it should be given to the Regents of the University of California. I don't know that it was written, but it certainly was implied when they accepted it that they expected to see that it was staffed and maintained. And it was. And I don't know how the degree of support that it got from those sources ranked with respect to other University-based institutions, but it certainly might be the first.

Another institution with a long term of local support was Harvard Observatory, which leaned on the kind of resources private universities always do, donors, wealthy capitalists of one kind or another. As one looks back on it the Lick bequest was not by modern standards at all handsome and sometimes it was considered very frugal. Directors like W.W. Campbell were continually going to California, to capitalists like William Crocker, for example, or to D.O. Mills, a famous San Francisco capitalist of the early years of the twentieth century, to get grants for this or that or to finance an eclipse expedition.

Jarrell: But that was still based on individual gifts?

Whitford: Yes. Now when this sense that the scale of scientific support that had prevailed in the pre-war years was really quite inadequate to realize the potential that was now open at the end of the war, given some of these war-time developments, there was something of a scramble to find large sums of money to do some of these things. The University of California was unique, and I think

fortunate in that Robert Gordon Sproul, the then President of the University, had rather complete, practical control of where the budget money came from and where it went. At the time of his interview with Mayall and Kron that I tell about in my chapter, in the last year before they came back after their war-time work, it wasn't the first time that a large telescope had been mentioned. What was new was the possibility that the money to finance it could come out of State of California budget appropriations; there was quite a bit of money set aside during the war when tax receipts were high and priorities prevented spending any of it then. I believe there was a post-war rehabilitation fund, or something like that, and a good deal of the money came from that. Of course the rather simple telescope that Mayall and Kron had envisioned turned out to be grossly underpriced; there was a huge overrun which Donald Shane had to see got financed through state appropriations. He did. Well, that brings us up to the period when the telescope was ostensibly finished in its bare bones without outside support other than state support. The state of California is the only state that has put that much capital money on the line. It's unique and it was quite handsome for those years.

As the years went by it became quite normal, *de rigueur*, for faculty members in all leading universities to go to the National Science Foundation or NASA, or some other arm of the government to ask for term grants of one year, three years possibly in the extreme, to do research that would require two things—support for additional skilled personnel, often Ph.D.'s in their post-doc years, and quite expensive new equipment, either attached to the telescope or for analysis of observations.

Jarrell: Computer support?

Whitford: Well it would be plate-scanning equipment, evaluation of the original photographic plates, finally spectrometers that are based on photoelectric registration. I speak in my chapter of the photoelectric revolution. This meant more and more complicated equipment, and quite expensive equipment at the telescope, and quite expensive computers which interacted with it and made it possible to actually do it. All of the leading universities, some of which had telescopes, some of which did not have telescopes, had ideas for instruments

which they themselves would build in their headquarters, with a view to attaching them to some large telescope, when completed. A lot of the operators of the big telescopes tried to make information available so that there would be a good fit to such instruments. As this happened, I think Lick Observatory was probably on the lower side of the spectrum in our degree of extramural support. We relied more heavily for just normal operating on our already quite substantial budget support as an organized research unit, and viewed all this soft money as a valuable supplement, but one to which we should not become so addicted that we would curl up and die if it disappeared . . . However these grants have become more and more substantial and you would have to ask the present director or Joe Calmus how much the annual expenditures are state funds and how much are extramural.

Jarrell: There's another topic in this area. In 1967 the National Science Foundation apparently gave us here about 600,000 dollars to start up a theoretical astrophysics adjunct . . .

Whitford: Yes, that was initiated before my time as director ended. The paternity would belong to George Preston and Robert Kraft. I reviewed last time our initial disappointment in the beginning of 1966 when we came here, that the degree of state support, particularly in the first Reagan government year, squeezed and trimmed organized research units and how equivalent funds were somehow dug up by the Regents, so we never lost all the money that we might have otherwise lost. This was money that we thought would come from the state to support the non-observing theoretical astrophysicists who would be teaching members of the newly established department, only that wasn't the word we used. Board of studies is still the code word at UCSC, though nobody else that I know in the academic community uses that term.

Jarrell: You said that most often these NSF grants were short term for specific expenditures.

Whitford: Yes, independent of any initiative on our part, the National Science Foundation said that, (well, I'm trying to rationalize it with hindsight) that the

scientific potential and productiveness of the country's scientists and scientific establishment depended partly on facilities and partly on developing the best and most creative people. It involved encouraging new ones to choose high-level scientific careers as a direction they wanted to go. How that all developed must be documented in some historical account in the National Science Foundation archives. It became apparent that there was going to be this effort, and that a handful of institutions were going to compete, not for one year, \$2000 or \$10,000 projects, but for really major grants that would allow them to advance their structure, their power, their staff. I think it was Preston and Kraft who decided that UCSC as a new, young department with a good observing arm stood a good chance of getting one of these. We applied for it. A previous interview records our initial disappointment in the reception and support that we got when we came to this campus; it may be a rationalization of some personal disappointments, you could even say self-justification for not coping. I mean to be humble. But that at any rate, this initiative blossomed, you just have to ask Kraft how it all came about . . . It required them to secure a rather large commitment from the administration of this campus. Now whether they went after it and secured this commitment after the shakeup following Preston's resignation and mine, or whether it was in process before, my recollection doesn't tell me. There was a period of expansion and recruitment of theoretical astrophysicists.

Jarrell: I believe it was five FTE's in theoretical astrophysics?

Whitford: Yes, you've got the facts better than I. The campus agreed that it would continue the support. To the best of my knowledge. I believe this rather large and strong commitment was kept. It is through that grant that the double barreled potential of astronomy on the Santa Cruz campus developed. It is upon that base that it was built and has achieved its present prestige. Whatever that is, I think it's quite good.

Jarrell: From my reading of the record, I am trying to reconstruct this, it seems that postwar astronomy, was advanced with this other wing, this other emphasis on astrophysics which hadn't previously been important . . .

Whitford: Yes, well another way of phrasing it might be that the importance of it wasn't previously implemented. As I told you in the really old days (laughter) going back to the thirties, there was the charge that the Lick Observatory staff was devoted to just plain recording and observing and not enough to understanding what it all meant in terms of the best physical theories. There was the charge that it was somewhat limited. But by the post-war years it certainly was recognized, and I suppose the best example of one of those who tried to realize it would be George Herbig. But because we lived on a mountain we reflected the old adage that we had an advantage in pursuing our observations as our first priority. Since we didn't have any students who could sustain and justify this effort in theoretical astrophysics, all the budget money was spent on the side of observational astrophysics. Due to this pressure within the University—the joint appointment rule, jealousy from other departments that felt that we had some kind of favored position, a position that impelled us to come to Santa Cruz—it was to our advantage to have such a wing. It was also a motivation or justification for getting more money out of the University, that turned our thoughts in that direction. I guess that's the comment that I'd make.

Jarrell: I'd like your thinking on the recruitment of graduate students who came here.

Whitford: Well it wasn't especially original or creative. I told you about the recognition of places like the Rockefeller Foundation for Medical Research, which saw that without the interaction with young minds, the strong, surging creative energy that comes at that time in bright young people's lives, that without that, there was danger of fossilization. I'll say that was a danger Lick Observatory didn't have. I don't think it ever got out of hand. This was a truism. Clark Kerr believed it was the reason for the joint appointment rule. That was one of the reasons we gave the University for why we wanted to come to Santa Cruz, but it was partly a way of preserving our coherence and identity which might otherwise become somehow dissolved or confused. We advertised for our first graduate students in the fall of 1967 when there was not much in the way of a theoretical staff yet. Peter Bodenheimer was the first person recruited, and he filled in an observationalist's post. We never got back that observer's position.

We were quite pleased that we got good applications from potential graduate students. As the staff expanded we got very capable theoreticians.

Some of the students, it's now close to half, decided to undertake dissertations that were clearly theoretical. The advantage from the point of view of a student who wanted to become an observational astrophysicist, was that Lick Observatory was there. They would have access to the smaller telescopes in their own name. There was pressure on the 120-inch; when it started to operate it was still one of the great telescopes of the world. It has shrunk somewhat in rank but it is still a very, very good telescope. The pressure on it was such that it couldn't be granted to graduate students in their name. But by working with staff astronomers on the Santa Cruz Lick staff they could get to go along, and the staff member would support it out of his assignment of nights. Thus enough observing time was granted for the student to get a dissertation, and it has worked very well.

It doesn't need any chauvinism on my part to say that there are two indications that this has worked. One is in the nationwide competition for the very best graduate students. There always have been about ten that apply lots of places at various universities and research centers. Lick Observatory has fared very well. We always get some of the first ten. I recall on a trip to Chile meeting a senior graduate student from Yale University who said, "Well I thought Yale had the inside track on that chap, and he decided he'd go to Lick." Yale has a department, heavy on the interpretation and not very long on their own telescopes; hence they use a national observatory. The second is that the students that we've gotten, trained, and given degrees to usually have a very good choice of first jobs, in general as post-docs; there have been very few, almost no examples of people that have not gotten some opportunity that is quite respectable. I think this is somewhat above the national experience. I don't know very many people that don't get some chance, but sometimes they have to take jobs in computer outfits or government treadmills.

Jarrell: How would you assess our national reputation for recruiting graduate students?

Whitford: Well our reputation is excellent and the potential entering graduate students are not unaware of this. They think, “Well if I go there and do well I can expect to get a job that I want.” Well I don’t need to call the roll, but Cerro Tololo National Observatory in Chile will, beginning next fall, have three staff members, all of them fairly young, from here. One is just going this September. So it’s sort of a Southern annex of this place. It’s certainly one of the two best Southern Hemisphere observatories in the world; it has the biggest telescope.

Jarrell: Now I just have only a few more questions, but I notice that you have some notes that you’ve made. Are there any other topics that you would like to include?

Assessment of Lick Observatory

Whitford: I think I reviewed the fact that at the time I resigned there was a kind of a crisis. In hindsight, I saw it as my failure to cope, to plan. The failure can be divided between the University administration, the UCSC campus administration and the Lick Observatory directorship. The sense of the staff was that perhaps other leadership could bring about a reversal and that really did happen. A large part of that credit goes to Robert Kraft. The support of the University, the amicable relations among the four UC campuses that have graduate programs in astronomy, is really quite good now. What we thought should happen, that the primary responsibility for the operation and instrumentation of Lick Observatory on Mt. Hamilton should be centered here, has not been taken away from us, although there’s cooperation from other campuses. What initially went wrong I don’t know. I will leave that to the judgment of others. You ask why did Preston resign and the words that he used were, “How much do we have to give up in favor of undergraduate togetherness?” Those were the nasty or pejorative words that he used—“undergraduate togetherness”—which was in his view the overruling priority of UCSC during our formative years. End of comment, and perhaps a bitter and personal one.

The other question that I might like to raise is the ideal, the model of the kind of observatory that we thought we wanted to be and I think still, to a large degree, are. This model says that you get the best results, if the end-user conceives and designs and guides the construction and tests and makes the first use of some observational facilities. Of course it is a truism that if you keep on using the same old ones you will lose out, you will fall behind . . . This is a model for ground-based astronomy only. I think it is acknowledged that in space astronomy there is so much collaboration with NASA, a mixture of aerospace and cryogenic engineering, that it may be beyond the single person capability of one instrumentally inclined astronomer, though he knows what he wants to do with the instrument.

I recognize that radio astronomy is also a separate case. There's a Very Large Array in New Mexico, thus far I think the single largest one, I'm not sure whether it's in competition with space telescopes. There certainly have been some key people who have played a role in bringing it into being, but no single person can be on top of all the extremely complex interrelated, computer-controlled devices.

Jarrell: It sounds as if it is a much more interdisciplinary, team coordinated effort today?

Whitford: Yes. Well it's computer-organized processing of information, maybe an analogue of the CAT scanner in hospitals, where a bunch of X-ray exposures are assembled into an image which you don't get from a single picture. In optical astronomy some places have tried to set up a technical branch independent of the end-user. Kitt Peak is an example; I think they have not distinguished themselves, because of the insulation between the technicians and the astronomers who feel that they are being elbowed out of control by non-observing engineering types and hierarchical people who say they are going to make the instruments. You tell them what you want and they'll make it for you but you can't meddle.

Jarrell: This is a fundamental conflict?

Whitford: Well, it has been. I speak of it from one side of the fence. The parallel observation that has to be made is that the Ford Foundation decided not to fund the Carnegie venture in the Southern Hemisphere because they rebelled against the elitist picture of people who had the experience, knew best how to do it and to extend it. Our cultural inheritance from the Southern California observatories, Mt. Wilson and Palomar, was at that time so strong, and my own personal upbringing was all in that direction, so that I was inclined to accept that view; it was called the elitist view.

I now recognize that it was wrong, that other places perhaps with monumental infusions of money and the opportunity can find their way with a certain amount of cost overrun, (not that we didn't also have cost overruns in making the 120-inch telescope). But they were puny by comparison. It's been proven in several places around the world, Australia, Britain, Germany, but thus far not in the Soviet Union, that lots of people can make a very fine telescope and make it work and do very good astronomy. The Canadian-French-Hawaiian collaboration on Mauna Kea, where our big ten-meter telescope will one day go, is a good example. I think the model of the end-user participation and control is something to give up very, very slowly, as you have to. It has worked so well, and I think in the examples where organizations outside of the West Coast of California have succeeded in making very fine telescopes, it has been a component that has been effective.

I don't know whether in the future, as things get more and more complicated, with the very complex detector arrays we now have, if this is now a fading nostalgic ideal. In the competition for money, and there are a lot of people who want to build large telescopes now, 7 or 8 meter, 10 to 15 meter—these are ground-based—the competitors get divided into what are known as the private observatories and the public or purely tax-supported observatories. Lick belongs to the private wing. Texas and Palomar and Cal Tech would be examples, Arizona perhaps, although they have very, very large grant support so the percentage of their money that comes from the state of Arizona might not be enormous . . . are considered private. If they are built, and the ten meter will be counted on that side, if they are built and completed, to the extent that they have been financed without federal government money, they are primarily for the

clientele that created them: the California clientele in our case, or the Arizona clientele . . . there is some obligation if you have a fine facility, to make it available on a limited basis to anybody who has a very good idea. But that this commitment should be 50 or 60 percent, as in the case of the National Observatory, is considered slicing it too thin, and nobody gets enough to do anything very fast. That is the competition between the ways of doing astronomy. I think it will work itself out. There will be failures and disappointments and triumphs and unexpected victories. My original view that it would be a mistake not to maintain some diversity in approach, is still what I think. I guess that's the end of my view on the way astronomy stands in the United States today. One assumes that military R&D, things like Star Wars, will not become the tail that wags the dog and I'm very concerned about that.

Jarrell: Thank you very much Professor Whitford. This has been a most enjoyable series of sessions.

Whitford: Well I hope it won't sound too . . . prejudice-laden, personal.

Jarrell: It's going to be personal. It has to be.

Whitford: Yes.

Jarrell: Well thank you.

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