

JAMES A. WESTPHAL
(1930–2004)

INTERVIEWED BY
SHIRLEY K. COHEN

July 8–29, 1998

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California



Subject area

Geophysics, planetary science, astronomy

Abstract

An interview in six sessions in 1998 with James A. Westphal, engineer and instrument designer who became research associate and later professor of planetary science at Caltech (1961-2004); and principal investigator for the Hubble Space Telescope's original Wide Field and Planetary Camera (WFPC 1, 1977-1994). He was born in 1930 in Dubuque, Iowa, to parents of German ancestry and raised in Tulsa, Oklahoma, and Little Rock, Arkansas. Receives BS in physics from the University of Tulsa in 1954 and works for seven years in geophysical research for oil companies before coming to Caltech in 1961. He recalls early work in geology division with C. Hewitt Dix, H. Lowenstam and B. Murray; with the latter on chemical differentiation of the lunar surface, his first involvement with planetary science. Works with B. Kamb on Blue Glacier; also with M. Schmidt and J. Gunn in astronomy. Recollections of Caltech colleagues G. Neugebauer, R. Leighton, R. Feynman. Comments on history of 200-inch telescope at Cerro Tololo and Caltech's relationship with Carnegie Observatories. He recalls work in early 1970s with J. Kristian for Palomar Observatory on highly sensitive electronic detectors (silicon vidicon photometer) leading to the evolution of CCDs [charge-coupled devices]. Joins NAS's COMPLEX committee at invitation of chairman G. Wasserburg; involvement with NASA's Galileo mission. Subsequent involvement with Hubble Space Telescope (HST) imaging project;

proposal for original wide-field and planetary camera put together with J. Gunn at JPL. He comments on early attitude of HST astronomers toward planetary scientists. Installation and testing of WFPC 1 in telescope; 1990 launch from Kennedy Space Center in Florida. Trouble with HST's solar panels and subsequent repair efforts. Westphal receives MacArthur award, 1991, and succeeds G. Neugebauer as director of Palomar, 1994-1997. With J. Miller of Lick Observatory becomes acting co-director of the new Keck Telescope; comments on instrument building. Earlier work (1983) with former grad student S. Kieffer, of USGS, on dynamics of Old Faithful geyser resumed; builds camera to send to the bottom of the geyser. Comments on R. Leighton's contributions to X-ray and infrared observations and planetary science. Further comments on instrument building.

Administrative information

Access

The interview is unrestricted.

Copyright

Copyright has been assigned to the California Institute of Technology © 2002, 2005. All requests for permission to publish or quote from the transcript must be submitted in writing to the University Archivist.

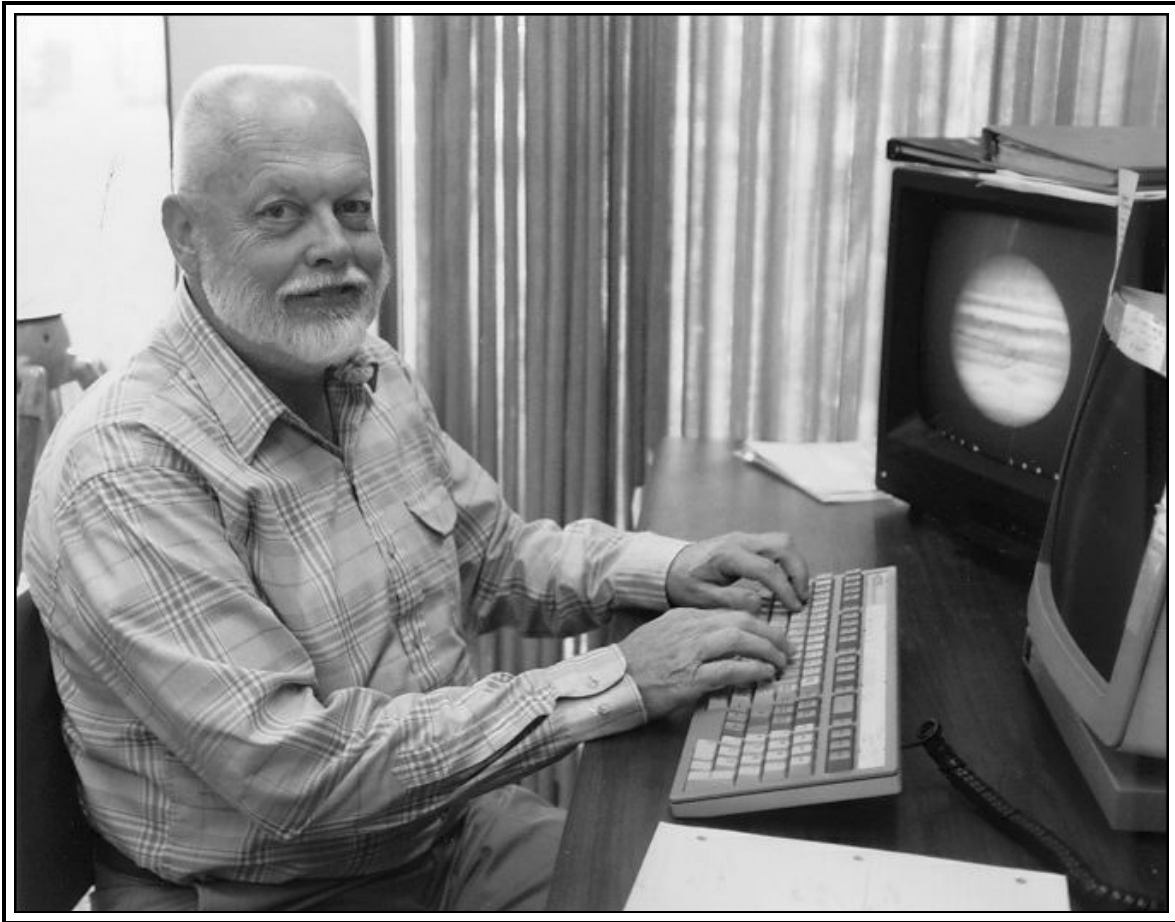
Preferred citation

Westphal, James A. Interview by Shirley K. Cohen. Pasadena, California, July 8-29, 1998. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Westphal_J

Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)793-8756
Email: archives@caltech.edu

Graphics and content © 2005 California Institute of Technology.



James Westphal processes an image of Jupiter taken by the first Wide Field and Planetary Camera (WFPC 1) in 1991. The original camera designed by Westphal's team at JPL was used on the Hubble Space Telescope (HST) from April 1990 through November 1993. During a spectacular in-space servicing mission, it was replaced with WFPC 2. The second instrument was modified to correct for optical problems in the HST mirror. Photo Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH JAMES A. WESTPHAL

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

Caltech Archives, 2002
Copyright © 2002, 2005 by the California Institute of Technology

TABLE OF CONTENTS

INTERVIEW WITH JAMES A. WESTPHAL

Session 1

1-16
Family's German background. Move from birthplace (Dubuque, Iowa) to Tulsa, Oklahoma, during Depression. Move to farm in Arkansas during WW II. Early schooling. High school in Little Rock. Builds a telescope as a teenager. Graduates from high school 1948; returns to Tulsa.

17-21
Goes to work for Seismograph Service Corporation in oil exploration for a year.

22-32
Goes to University of Tulsa. Works for SSC in summer on oil exploration crews. BS degree in 1953. Goes to work for Well Services, SSC subsidiary, in Mexico, for a year.

Session 2

33-45
Returns to Tulsa and works for Sinclair Oil Company's new research laboratory. Works with first mass spectrometer for organic material.

45-55
Meets C. Hewitt Dix, of Caltech, who is interested in his work in exploration geophysics. Works with Dix during the summer. Comes to Caltech in 1961 as research engineer. Takes applied mechanics course with J. Miklowitz. Builds Fourier analysis machine with Dix. Works with H. Lowenstam and B. Murray. Offered permanent job by R. Sharp to "decrease research resistance."

55-60
Works with B. Murray on chemical differentiation of lunar surface, his first involvement with astronomy. Works with B. Kamb on Blue Glacier. In 1971, becomes an associate professor of planetary science; in 1976, full professor.

Session 3

61-75
Work (as senior research fellow) in the 1960s with M. Schmidt and J. Gunn. Work with E. Becklin on infrared observations of comet Ikeya-Seki. Recollections of G. Neugebauer, R. Leighton, R. Feynman. Schlieren technique with H. Lowenstam. Unofficial thesis supervisor for A. Goetz. Works with J. Kristian and A. Sandage; works on IR observations of Eta Carinae with G. Neugebauer in Cerro Tololo. Comments on history of 200-inch telescope at Tololo and

Caltech's relationship with Carnegie Observatories. Works there with G. Neugebauer and T. McCord; there during 1969 Mars opposition.

76-89

Works on silicon vidicon photometer for Palomar. T. McCord meeting with N. Hinners of Bell Labs; history of attempts to develop picturephone. Various papers in planetary science including one with his first graduate student, R. Terrile. Comments on evolution of CCDs. Joins NAS's COMPLEX committee at invitation of chairman G. Wasserburg; involvement with NASA's Galileo mission.

Session 4

90-100

Recollections of H. Lowenstam's work on hard-shelled marine animals. Plans to collect animals with navy's bathysphere *Trieste*, foiled by *Thresher* disaster, 1963. In 1964, trip with Lowenstam to South Pacific.

100-105

1973 oil crisis. Successful attempt to reduce Caltech's use of electricity with phantom fluorescents. NACUBO award.

105-115

Involvement with Hubble Space Telescope. NASA meeting called by N. Roman to discuss detectors to be put on HST. A year later, committee meets at JPL; he discusses CCD. Ideas for wide-field camera discussed. Camera project taken away from Goddard space center; Gunn wants them to build it, and at JPL; he resists involvement initially. But they put together a team, including R. Lynds, D. Currie, W. Baum (later A. Code and S. Faber) and write a proposal.

Session 5

116-132

Hawaii vacation, fall of 1977; informed by N. Roman that JPL proposal is accepted. K. Seidelman joins team. Comments on early attitude of HST astronomers toward planetary scientists. Becomes chairman of AAS Division of Planetary Science. November meeting of instrument teams at Goddard. Comments on HST protocol. Comments on attitude of JPL toward Caltech people directing its projects. The golden rule. His work on the camera at JPL. Contributions of J. Kristian. Friendship with Lockheed's D. Tenerelli.

132-150

1986 Challenger disaster; HST on temporary hold. Installs WFPC 1 in telescope. Various tests. 1990 launch from Kennedy Space Center in Florida. Trouble with HST's solar panels. Preparation for first light. Comments on spherical aberration. Recollections of L. Allen. Marshall SFC's pushing of Perkin-Elmer. Repair efforts.

150-161

Receives MacArthur award, 1991, still at STSI in Maryland. Returns to Caltech. Impact of MacArthur award, for five years and \$360,000. Asked to succeed G. Neugebauer as director of Palomar, 1994. Initial reluctance. Comments on MacArthur reunions.

Session 6

162-188

Comments on Palomar directorship and bitterness of G. Neugebauer. Relationship with J. Miller of Lick Observatory. They become acting co-directors of Keck. Flap at Palomar over cooks. Help on Keck's LRIS; work of J. Cohen and B. Oke. New relationship between Palomar, JPL, and Cornell. Retires from directorship after three years. Comments on instrument building.

188-200

Earlier work (1983) with former grad student S. Kieffer, of USGS, on dynamics of Old Faithful geyser resumed after MacArthur award. Builds camera to send to the bottom of the geyser; finds out how geysers work. Other awards. Comments on R. Leighton's contributions to X-ray and IR observations and planetary science. Abortive astrophysics lab at Caltech. Further comments on instrument building.

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with James A. Westphal
Pasadena, California

by Shirley K. Cohen

Session 1	July 8, 1998
Session 2	July 13, 1998
Session 3	July 17, 1998
Session 4	July 23, 1998
Session 5	July 27, 1998
Session 6	July 29, 1998

Begin Tape 1, Side 1

COHEN: Good morning, Professor Westphal. Perhaps we could start with your telling us about your family background and your education.

WESTPHAL: My family is second-generation German.

COHEN: That means your parents were born in this country?

WESTPHAL: I guess it must be third generation; their parents were born in this country as well. But [my great grandparents] came from Europe in the late 1860s, as best we can tell—nobody has made any great effort to find out the details. The lore on the Westphal side is that seven brothers left Germany right at the time when Germany first became Germany, in order to beat the draft.

COHEN: A lot of people came for those reasons, I'll tell you.

WESTPHAL: You can't blame them, I guess, in those days in that time. But beyond stories like that in the family, I know very little about what their background is. I know where my great grandparents were born in Germany. And my son Andrew, about three or four years ago, went to that little town in northeastern Germany—Schwerin—[where the Westphal side came from] and toured around town and looked in the phone book. There were an awful lot of Westphals in the phone book.

COHEN: Is that right? That's a common German name?

WESTPHAL: It is, actually. Andrew didn't speak German, so he finally went into the church; he thought maybe he could find somebody there who spoke English. He showed his driver's license to a lady at the door—kind of a gatekeeper, I guess—and she became very excited. She took him alongside one wall of the church and said, "Look." And there was a portrait of a man by the name of Edward Westphal. It surprised me that the name was Edward, of all things, but of course that's the anglicized spelling of it, I suppose. At any rate, [the inscription, which was in German,] said that this was Pastor Westphal, "who was a good man." So Andrew took a picture of it—because he couldn't read the German either. I gave [the photograph] to Hermann Engelhardt [senior research associate in geophysics], who speaks German, and he could read it straightaway. And so we really did understand what it was about. It turned out there were so many [Westphals] around that this person was not necessarily our ancestor.

COHEN: It's not an uncommon name.

WESTPHAL: Yes, exactly—particularly in that part of Germany, apparently, which is not really in Westphalia.

My mother's father came on his own from Germany as a young man, to Titusville, Pennsylvania, and then went on to Kansas. The last name was Lobsinger. The Westphal family

first went to Nebraska, amazingly only thirty or forty miles away from where my mother's family lived. But they didn't have contact at that time. My great-grandfather Westphal apparently had a fair amount of money, for those days, which he brought with him from Germany. Those were the days when there were a lot of little ethnic settlements scattered all over. Italian towns, German towns. He moved to a small German town called Lutherville, in central Arkansas—I guess these things were advertised somehow. When he got there he really didn't like the place, so he moved to the next German town—I don't remember ever hearing it with a German name. It was already a settlement, called Index, and it was on top of Petit Jean Mountain, [in central Arkansas]. The reason he moved there—a common reason in those days, apparently—was that the girls from these German towns would go and visit the boys in another one, and vice versa, so as to spread the genes around. And that's how he came to go to Index.

COHEN: It was time for a wife.

WESTPHAL: Time for a wife, that's right. So his wife—my great grandmother—had the last name of Jöns—which, of course, immediately became Jones [laughter] as soon as they hit the ground.

COHEN: Oh, that's interesting.

WESTPHAL: So that was that. My [great grandparents] on my mother's side I know very, very little about. Their name was Weiss, in modern times. We know that they came from Kassel. The rumor in our family—and all I can say is that it was a rumor, a story that was told—is that there was a strong suspicion that he was half Jewish. That was mainly tied, I think, to the spelling of Weiss in some documents. And again, when they got here, somewhere along the line the name got changed to Wise.

COHEN: Were they farmers when they came here?

WESTPHAL: They were all farmers by trade. And apparently the Westphal great-grandfather's family—which lived, as I say, in the north of Germany—was by those days' standards doing very well. I think his father—my great-great-grandfather—[was doing all right] because he was a fairly young guy, along with his older brothers. But at any rate, there was no history of anything more than those people being farmers of some sort. When they came to this country, the Westphal branch continued to be in the agricultural business. But the Wise branch of the family became mechanics—people who would fix things. As usual in those days, why, the mothers [stayed home].

COHEN: Working very hard. [Laughter]

WESTPHAL: Yes, very hard. Oh, boy, there are some awful, awful stories about how all of that was. Those were tough times.

I was born in Dubuque, Iowa, where my father had a job as an accountant for a company that was headquartered in Tulsa, Oklahoma, called Central States Power & Light Company. They had a branch in Dubuque. I was born on the thirteenth of June in 1930; my family had moved there sometime around Christmas of 1929. It's a wonderment to me that they stayed [laughter], after moving to Iowa at Christmastime. Then they moved to Tulsa, as I remember, sometime in the fall after I was born. The company brought my dad to the home office. Of course the Depression had started, but it wasn't really full-up at that point. So throughout the Depression my dad had a very good job, which paid \$250 a month. That was a lot of money for the time. It was enough money, for example, that we bought a brand new 1934 Pontiac [laughter] right in the middle of the Depression. He did very well.

COHEN: He was a bookkeeper for the company?

WESTPHAL: Yes. He was actually an accountant, not a CPA.

COHEN: He kept the books.

WESTPHAL: He kept the books in a serious way. I remember one time when I was—I guess I was already out here in California—I said something about some problem. I can't remember what the problem was, but things didn't fit right in a numerical way, and I was moaning that somewhere there was an arithmetic error. He said, "Let me tell you a story that changed our whole life. You probably wouldn't ever have existed. In fact surely you wouldn't." He had gone to what nowadays would be called a junior college, which was associated with the Lutheran Church; it was in Winfield, Kansas. He got a job as an accountant in Winfield after he got out, and before he learned accounting. The first month he did the books, he was off by one penny—it didn't balance. And he said, "I was so gung-ho I was damned if it wasn't going to balance. And before it was over, I found \$600," roughly—I don't remember the number he had—he had it right down to the penny. Still, that he balanced within one penny. And he said, "If it had balanced by accident it was sure that nobody would ever believe I hadn't cooked the books, and I would have surely lost my job." And I'm sure that's true.

Backing up a little, my mother's family lived in Braman, Oklahoma, which is very near the Kansas border, about due south of Wichita. [Her father] was a mechanic, and the work he did in his day was primarily on farm equipment and things like that. That's big wheat country.

COHEN: I was just going to say that was the Dust Bowl area, wasn't it?

WESTPHAL: That was farther to the west, in Kansas and western Oklahoma. I can even remember having to wear wet handkerchiefs over your face. These big, black rollers would come in. Cold fronts would come in full of dirt and they just rolled, like this. You couldn't see

across the street. That was in '36 or '37. At any rate, my mother was born there, in this small town. She went away to Winfield to secretarial school, and that's how my parents got acquainted.

COHEN: Well, then your parents both had higher education for those days.

WESTPHAL: For those days, that's right. Anyway, they got married in due time.

COHEN: And you were the first child born?

WESTPHAL: I was the only child born. And as I said, at the end of 1930—somewhere along there—we moved to Tulsa. We bought a brand new house, and that was a horrible problem for my dad, because he could not break the deeply built-in tradition that if you didn't have money to pay for it you didn't go into debt—for anything. But he had to get a mortgage on that house, and he was very unhappy about that. [Laughter]

COHEN: He had to learn to be an American.

WESTPHAL: Yes—well, he never did learn. [Laughter] Until the day he died, he was uncomfortable about going into debt, and that was really the only big thing he ever went into debt on. Anyway, he went about his business in Tulsa, and I went about going to school.

COHEN: The regular public school?

WESTPHAL: Right, the regular public school. It was only a block away from my house. And it was brand new.

COHEN: There was a lot of building going on?

WESTPHAL: A lot of building was going on, in all kinds of directions. And there were lots of people moving into town from out in the country. Then it got even worse, of course, as the Depression came along and the crops all failed. So my mother continued to be a housewife. She was the person in the family who had interests in cultural things. Although she was not a musician, she was a great lover of classical music, which was not a tradition in our family. Part of that lack may have been artificial—the fact that people lived out in the country, where there was no electricity and no way to easily go to big towns. You just didn't do that. But that was the extent of my education—as little as there is of it—in cultural things.

COHEN: Your mother's love of music?

WESTPHAL: Yes. Well, and in other things, like plays and things like that. [So we] toggled along until sometime in 1940 or '41. Then the company was bought out by somebody and suddenly my dad had no job. The Depression was still with us. It was not nearly as bad as it had been, of course, but it was still there. He was a bit of a miser, and he had money put away. So he leased a filling station in downtown Tulsa. It was associated with a parking lot that was almost a whole city block, and near enough to the main part of downtown so that it was very popular with people who were working there. They paid ten cents a day for parking. And when I was not at school, during the summer, I was a collector of the dimes. [Laughter]

COHEN: I see. So your first real job was—

WESTPHAL: Collecting dimes for a parking lot. [Laughter]

At any rate, then Pearl Harbor happened. My dad was born in 1905, so that means he was thirty-six [by then].

COHEN: And I think thirty-eight was the cutoff for the draft, wasn't it?

WESTPHAL: Thirty-eight was the cutoff for the draft. And he was very, very embarrassed by what he did. He had two brothers—[both younger]—one of whom was in the army at that time. The other brother, who was just five years older than I was, had moved up to Indianapolis, where his sister—my father's sister—lived, and he moved up there to go to high school, because there was no reasonable school in that part of Arkansas. It was far from being even a small town, much less having a real school. That left my dad's parents—my grandparents—alone, and my grandfather was not in the best physical shape. It was a German tradition—maybe it's a European tradition—to pour salt all over everything before you eat it. So he was quite overweight and his blood pressure was really high. Usually blood pressure problems don't surface in any vivid way—people die without even knowing they have a problem. But he couldn't do some of the hard work that needed to be done on his farm, which was large—600 and some odd acres which, had it been all in one piece, would have been a square-mile section. So he and my dad formally went into partnership; they formed a partnership in the legal sense. My dad was very goosey about how this was all going to work. Remember, he hadn't had a job for a while, and he'd run this filling station. He did not hit a gold mine in any sense, but it was certainly more than just subsistence. And he had some money in the bank, so he went into this partnership with my grandfather. And we all lived in the same house. It was a great big two-story house out in the country, overlooking the Arkansas River, 600 feet below, near the foot of Petit Jean Mountain.

COHEN: So you had some years of growing up in the country.

WESTPHAL: In the country, yes. But the immediate problems were two. One was that they couldn't expand this farming activity just by using mules—that was mule country—as draft

animals. So my dad went out and bought a used tractor. My grandfather had no money, really, so there had to be a barter enterprise for his half of the tractor. The tractor really revolutionized what could be done, but it still was farming and cash crops were not [doing very well]. The cash crop in the early days, when my grandfather was quite a prosperous farmer, was cotton. But the cotton was completely destroyed in the early thirties by the boll weevil. So only in those places where the soil was extremely good could people make any money out of cotton, and you had to hand-pick it. Well, as soon as the war started, that was the end of having any cheap labor around. Before then, my grandfather had had three sharecroppers on his farm. So those guys immediately went off to war plants and started making real money.

COHEN: Right. They weren't paid a great deal as sharecroppers, I'm sure.

WESTPHAL: Yes, they were always in debt. That was part of the game. My grandfather was famous for being a kind man and an honest and good man, and he didn't play that game. And I can remember visiting him there, before we went there to live, and hearing people griping at him—other farmers saying, “You shouldn't be treating those people like that. The next thing you know, they'll want to do that on my farm.” [Laughter]

COHEN: So you lived your adolescent years on a farm.

WESTPHAL: Well, it turned out that the education issue was very strong in our family. There were very strong feelings that people ought to get an education. So the first year that we were there—we moved there early in June of '42, so it was shortly after Pearl Harbor—

COHEN: Let me backtrack a bit. You said your father was ashamed of something, but I didn't get what he was ashamed of.

WESTPHAL: Oh, yes. [Laughter] What he was ashamed of was that his main motive for moving down there was so that he could be a farmer, so that he wouldn't be drafted.

COHEN: OK. But I would think that having to take care of his father—wouldn't that have...?

WESTPHAL: Well, that was real but it wasn't critical. If it wasn't for the draft, he would not have done that, and that would have changed the world, of course. You get to branch points, places where you can say, "If such-and-such had happened, things really would have been different." So he was always very, very uncomfortable about the fact that he had done this. He could have gone immediately to work in a war plant, too, but if he had tried to do that, he would have been drafted.

COHEN: He must have had some sense that he had to take care of his father also.

WESTPHAL: Yes, it was all very real. But a two-story house is not anywhere big enough to hold mothers-in-law and sons-in-law and daughters-in-law, and so forth. There was a fair amount of friction between my mother and my grandmother; she ran the house and my mother tried to be helpful.

COHEN: I can imagine that.

WESTPHAL: You can imagine better than I [laughter] all that kind of business. I remember lots of times, though, when my mother was very, very unhappy. Naturally that bothered my dad, because immediately he realized that if we had just stayed in Tulsa, this kind of stuff wouldn't be going on, which was of course true.

So there was a high priority to getting me into school. There was a school on the mountain; it had two teachers, a man and a woman, and about ten or fifteen students of various

ages. Almost a one-room schoolhouse, only it was really a three-room schoolhouse. And it had been built by the CCC [Civilian Conservation Corps] during the Depression, and it was a stone building. They used a standard set of plans. So in a large part of the South you'll see these abandoned stone schoolhouses. They weren't brick, because bricks cost a lot of money, and there were lots of rocks around.

So I tried this mountain school the first year. It was about three miles away from where we lived, and in the wintertime in Arkansas it gets cold, and in the spring and fall the roads are almost impassable because of the mud. So that was a pretty miserable business. So it was decided that we should somehow get me into a school in the nearest real town, which was Morrilton. That was fundamentally a farming-centered town, too, but it had a real school system. I was, I guess—

COHEN: If this was '42, you would have been about twelve and getting ready for junior high or high school.

WESTPHAL: Well, junior high, because I had graduated, essentially, from the primary school in Tulsa. That, in fact, was what had driven the timing of when we actually moved; as soon as my school was out, we moved.

So the idea then was, next year let's get him into the Morrilton school. There my grandfather's influence and reputation made a big difference, because in Arkansas at that time you couldn't move from one school to the other; you went to the school you were assigned to. My grandfather talked the Morrilton school's assistant superintendent into allowing me to go there. So that year I rode on my bicycle two miles to the house of a cousin—a second cousin, a relative—who lived only about a quarter of a mile from a paved road. She was a teacher in the Morrilton school district, and she—this was still up on top of this mountain where we all lived—would drive down to the bottom of the mountain, to where the school bus came. She would be picked up by the school bus, and I got to ride along. So I had to leave our house in the

wintertime in the dark and ride [to her house] on my bicycle, if it was cold enough so that the roads were frozen. And if it wasn't, if it was muddy, then I just had to walk. When school was out in the afternoon, the whole process was repeated, backwards. What this meant was that I wasn't seen at home except on weekends. My grade-school education in Tulsa had been so much better than it had in the Arkansas system that I was a couple grades ahead of [other students] my age.

COHEN: Of course, you may have been anyway.

WESTPHAL: Well, maybe. But that was a problem with the system. My dad had [another] sister, who lived in Little Rock. I wasn't in the loop in any serious way, but I knew they were talking about somehow trying to get me into the Little Rock school system. My dad's sister had two children, so it was not an idea that I was going to stay with them. But she recruited an elderly lady, a recent widow. In fact, she was a young lady—she was sixty-one or something, but older than the hills. Her family was, again, part of the German community. Her husband had just died, and he had been a very wealthy grocer in town. There is a big main street in Little Rock called Kanis Road. Kanis was her name. At any rate, she needed somebody to take care of her car, which was parked inside the garage. This car was a 1942 Ford, and it was absolutely immaculate. And the big problem at that time—wartime—was, How do you get tires? It was a big crisis—you couldn't get tires.

COHEN: That was when we still used rubber. [Laughter]

WESTPHAL: Yes. So here was this essentially new car with good tires on it. Her husband had left instructions that this car was supposed to be started and driven around the block once a week, so that the tires wouldn't always be sitting in the same place. Well, she wouldn't let me drive it around the street. [Laughter] Everybody in town was trying to buy this car from her;

they would offer her huge amounts of money, and she just wouldn't have it. It bothered her a whole lot, so she finally gave it to her next-door neighbor for nothing. At any rate, I roomed with this woman—room and board. She also had a bunch of bantam chickens, so I was the guy who cleaned up the chicken farm.

COHEN: You're saying all this without any emotion. Did it hurt you to be sent from place to place?

WESTPHAL: Oh, no. I thought it was a wonderful adventure. These people were all nice people, and I was used to hard work—by then I was thirteen or whatever. At any rate, I went to a junior high school in Little Rock, I guess for just one year, and then I went to Little Rock High School. This was in '44. That was where I first got seriously interested in astronomy. When I was going to school in Morrilton, there was a Carnegie Library. Of all the things that anybody has ever spent money on in this country, the Carnegie Libraries are probably one of the best investments ever. There is actually a book about them. When my father died, I went back there to the funeral, and I went to this Carnegie Library and told the librarian, who was of course not the person I had known, that it had gotten me interested in all sorts of things, that I had found this wide-open world. My mother kept a clipping from a local newspaper that said, "Petit Jean Boy Reads Every Book in Morrilton Library on Airplanes." [Laughter]

COHEN: That's wonderful.

WESTPHAL: So that's how I got used to thinking about a library. When I got to Little Rock, in a much bigger library—this was a city public library, not the high school library—I came upon a book called *Amateur Telescope Making*. So I thought, "My goodness, it's possible to build a telescope!" And I ran into a classmate who was also interested, and the two of us went off and learned how to build telescopes.

COHEN: The two of you by yourselves?

WESTPHAL: By ourselves, yes. Using ATM, as it was called. There came to be three volumes of ATM.

COHEN: Who wrote that book?

WESTPHAL: It was a compendium of articles by a whole bunch of amateurs. The editor was Albert G. Ingalls. The godfather of the whole thing was Russell Williams Porter. He started amateur telescope making in [the early years of this century]. Fascinating man, absolutely fascinating man. So we read the books, and we went through all sorts of machinations. We didn't have any money.

COHEN: I was just going to ask you where you got the money to buy this stuff.

WESTPHAL: Especially I didn't have any money. My dad was not doing well during the war, because he was still sticking with the farm.

COHEN: And then, of course, they were paying all this room and board for you.

WESTPHAL: Well, no, they didn't have to pay for that. It was a barter deal. I did the house and gardening work, and for that I got a room and board. At any rate, that's how I got interested in telescopes. And in fact the two of us built an eight-inch telescope from scratch and put it on the top of the bandstand tower, which was the highest point in the middle of Little Rock High School. We had to climb up a ladder to get up on top. And every Friday night the two of us—or usually just one of us at a time—had open house for the telescope, and lots and lots of kids came.

COHEN: Did you charge for this?

WESTPHAL: Oh, no.

COHEN: So where did you ultimately get the money for the parts to build the telescope?

WESTPHAL: I wished for almost everything.

COHEN: Oh, OK.

WESTPHAL: We went to the local glass shop and said, "Have you got a piece of old plate glass that is ideally an inch thick? We're not going to be picky." I think we ended up with only a half-inch thickness, which was pretty limber, but that taught us pretty quickly how to make a mirror support, which amateurs never did, because you'd just buy a piece of thick glass and not worry about it.

COHEN: I see. So you had all this wonderful early training.

WESTPHAL: Yes, you learned a lot just by virtue of the fact that you didn't have all the facilities right there. Anyway, I graduated from that high school in '48. I immediately went back to Tulsa, having been gone only six years. My mother's sister lived in Tulsa, and we had lots of friends left over from when we lived there. And to get started, I got a job in a filling station, pumping gas. It was a twenty-four-hour-a-day station, and I worked the day shift. There was one man who worked the night shift. I came to work at six o'clock in the morning and left at six o'clock at night, and rode the city buses back and forth. It was the first real sizable amount of money I made. And one morning I got off the bus at six o'clock and walked up to the place and

sensed something strange. I didn't understand what I was seeing until I was a block away—something just didn't look right. I got up there and by the pump island was a burned-out car. And lying there next to it was the night man. He was a notorious smoker. He apparently had been pumping gas and smoking. And I had started smoking, I guess a year before that, but not really seriously. It was a social kind of thing, because my buddies did. Boy, that cured me.

[Laughter]

COHEN: So he was a goner?

WESTPHAL: Oh, he was dead. He was burned up. So I never smoked. That's one good thing that came out of that.

Well, I was hell-bent on going to college, and my folks were really pushing hard. They still didn't have any money. By then they were having real problems with keeping my grandparents going; in fact, my mother's mother was also not in very good shape. She had had polio as a kid. Nobody knew what it was at that time. So she was [lame] in one leg, and that became a problem. At any rate, I was essentially on my own, but they were certainly rooting on the sidelines.

COHEN: But you had to find the means by yourself?

WESTPHAL: That's right. So I stayed in Tulsa for the first six months or so, or a little less than that. [Tape ends]

Begin Tape 1, Side 2

WESTPHAL: I was just saying that I was really hell-bent to go to college. My mother's sister lived in Tulsa, and her husband was an exploration geophysicist for a company in Tulsa called Seismograph Service Corporation, which was the biggest independent exploration company

around at that time. They essentially invented the commercial business. He had gone to Colorado School of Mines, which was, and still is, the center of training in exploration geophysics—not just oil, but minerals and various things. I thought he was a really good guy—a real neat guy. He was kind of a madman but a neat guy. He was clearly extremely bright. He had invented most of the analysis techniques himself. So through him I got a job with his company, working on a seismic exploration crew. My first day at work was the first day of December, 1948. There's a lot of jargon in this business, but the lowest guy on the totem pole is the guy who moves the seismometers—the seismic sensors that have to be placed along in a line, and then they shoot a charge of dynamite placed into the bottom of the hole, and then the line is removed and you go to the next hole, and so forth. So this is somebody who needs to have no technical competence whatsoever.

COHEN: You'd have to be fast to stay ahead of the dynamite, I would think.

WESTPHAL: No, no. Everything is under control in that sense. The dynamite can be left in the hole. I mean, it's shot electrically with a plunger like you see in the movies.

At any rate, I got this job. It was in Spearman, Texas, which is in the very top of the panhandle. I had never worked for a company before, so I had no idea how I was supposed to do things. I was just completely naïve about that. He had sent me to the company's person there, who called me into his office. The guy said, "You go to Spearman, Texas. Be there on the first day of December." He didn't tell me how to go. He didn't tell me anything about keeping track of my expenses or what to do if I had a problem. The question just never came up.

COHEN: No orientation.

WESTPHAL: No orientation. My only experience of traveling at that time was traveling on a train or in a car. Well, I didn't have a car, thank goodness, so I thought I'd get on the train to go

to Spearman, Texas. I called up the railroad station downtown and said, “Can I get from here to Spearman, Texas?” And they said, “Yes, you can get there. You have to go up to somewhere in Kansas called Caney, then through Tulsa and change to the Santa Fe. That will take you to Woodward, Oklahoma, which is way out near the Texas panhandle. And then there’s a “doodlebug” rail trolley that will take you from there to Spearman.” And I thought that sounded neat, so I did it.

COHEN: How long did that take?

WESTPHAL: A day—a little more than a day, actually. I got to Woodward, I think, at four o’clock in the morning, or something like that. The trolley was not really a Toonerville trolley—it was a single-diesel engine-driven thing. It had snowed three feet in Spearman in the previous three days, and just by pure chance the day I got to Woodward was the first day they were going to try to get to Spearman on the trolley. So four or five of us got in. These things had just a few seats in the front; they were mainly for carrying goods and stuff. So we all piled in and went toward Spearman. Well, they had a big steam engine, a locomotive, with a plow on it. I wondered about that. I remember at the time thinking that that was not a very effective way to get snow off the tracks, because what would happen if they came to a roadcut, where you’re going through a canyon blocked with snow; you’d never get through it. But I thought, “Well, they know what they’re doing.” [Laughter] “They’ve been doing it for years.” So off we went, and sure enough, when we came to the first place where there was a roadcut like that, why, here’s the back end of the snowplow stopped ahead of us, and they had been battering this [drift]. They’d back up and hit it hard, again and again. And they actually pushed the snow all the way through, in front of the train. I was so amazed, I couldn’t believe this. So we got through that and went on down the tracks. All of a sudden, *clunk clunk clunk clunk clunk*, we came to a screeching halt. We had fallen off the tracks, because the temperature had changed so much that the spacing between the rails had decreased, or increased—it depends on the way the bend

curves, so it's either one way or the other. Anyway, the wheels of the train were literally on the ground, not on the rails. And I thought, "Boy, this is going to be a long night out here." There didn't seem to be great agitation about it. The guy who was running the train had a fireman with him, and they got everybody out. They had big pry bars and they pried the train—

COHEN: They put the train back on the tracks?

WESTPHAL: They put it back on the tracks.

COHEN: I hope you got a discount on your ticket.

WESTPHAL: [Laughter] You know, it's interesting, because nobody would ever have thought of saying that we ought to get a discount for doing that. It was just what you did. It was part of the way of the world—which is really different now, I think, in a fundamental way.

Anyway, I got to Spearman. First I had to find the company's office—these are temporary locations, when you do this kind of work. In fact it was very unusual that the one in Spearman had been there for a couple of years. Usually six weeks would be a long period—you just rent office space. Anyway, Spearman wasn't a very big town, so pretty soon I found the office. And the guy looked at me. I told him who I was, and he said, "How in the hell did you get here?" I said, "I came on the train." And he said, "On the train! Oh, that's going to be a big problem. You were supposed to come on the bus." And I said, "Nobody told me to come on the bus." And he said, "Everybody knows you're supposed to go on the bus."

COHEN: You mean there was actually a direct bus?

WESTPHAL: Well, no. I would have had to go from Tulsa to Oklahoma City to Pampa.

COHEN: So it would still have been the same circuitous route?

WESTPHAL: Oh, yes.

COHEN: But you wouldn't have had all that fun.

WESTPHAL: That's right. [Laughter] Well, I wouldn't have gotten there at all, because the snow was so deep that the highways were not open. So the very next day, the first thing was to get me into a boarding house so I'd have a place to stay. And I was supposed to report for work at 6:30 in the morning, which is the way that kind of business goes. And it was still determined by the party chiefs, the guys who were running that local group, that there was too much snow and we couldn't work. They had some war surplus army ambulances, and they used them to carry the dynamite out to the fields to the test holes—they were four-wheel-drive winch trucks and all this other stuff. Somebody said, "Let's go to Pampa." And I thought, "Gee, how the hell do we get to Pampa?" And somebody else said, "Yeah, we're really flat out of beer. If this is going to last a while we'd better go get another load." They invited me to come along. Four or five of us got into this ambulance and just started off cross country, going over fences and everything else. These guys knew the countryside well. Most of the time you could tell where the road was, because of the power lines and stuff like that. But there was nobody out there; we were the first tracks over the snow. And if there was a shortcut to be taken, we did—knocked down some fences and so forth.

COHEN: Got your beer?

WESTPHAL: We came back with a bunch of beer.

COHEN: How long did you work at this job?

WESTPHAL: I worked at it for a year.

COHEN: And that was full time? You lived down there?

WESTPHAL: Well, I lived in several places. I guess we were in Spearman only a couple of months and then they shut down that activity, which was a special kind of seismic exploration—refraction shooting, it was called. Then we went back to the more conventional stuff, which is reflection shooting. We went to different places. I was out there in the field a year and a half. I don't think I had started college.

COHEN: You were saving your money all this time?

WESTPHAL: I was saving my money, you betcha.

COHEN: And by then you must have moved up from the lowest position.

WESTPHAL: Yes, I kept working my way up. The first thing was being what is called a “jug hustler,” the guys that carry the seismometers. If they want to be fancy they say that they're electromagnetic detector and placement engineers. You've got to say something impressive to all the girls. At any rate, it was very educational. I learned how to handle dynamite, a ton at a time. I also learned how to run the electronics in the recording truck. So then I started college.

COHEN: You went back to Tulsa?

WESTPHAL: Yes, to the University of Tulsa. And again I needed a job—one that I could do at night, while I was going to school in the day. I got a job through another neighbor, who was a

big guy in a local wholesale grocery in Tulsa. I could work any time I wanted, putting in my forty hours—they didn't care when I was there. There was always something there for me to do. So I did everything, including hand-selecting—by pinching the tissue paper—the good apples from the apples that were already rotten. I'd come in and there'd be this big stack of crates of apples. And these were fancy apples.

COHEN: So you'd pick out the good ones.

WESTPHAL: They'd been there too long, so I was sorting them. I didn't have to take the paper off—you could just feel with your hand whether they were soft. And I went through a boxcar-load of apples over the course of several months. It was very educational to see how food is handled at that level. As an example, all of the kale that was being sold in Tulsa was in fact the outer leaves of cauliflower. The people who lived in town didn't know any difference. If it says "kale," it must be kale. [Laughter]

COHEN: It may taste the same too, after all.

WESTPHAL: I have no idea. I don't eat that kind of stuff. [Laughter] Some things you don't eat—like being in a sausage factory. We also handled all of the bananas for Tulsa. They came by freight cars from New Orleans and they were hanging in bunches inside the freight cars.

COHEN: I'm getting ready for a spider story [laughter], no?

WESTPHAL: Worse than that. The stalks of bananas are hung in a special room and they're sealed. There's one man in the company who is the keeper of the bananas, because he knows how to put the proper amount of ethylene gas in for the proper length of time to ripen the proper quantity of bananas for tomorrow morning's delivery load. They come in just as green as green

is green, and the whole thing is done with finesse—the guy has to know what he’s doing.

Ethylene is a poisonous gas. Not only do you have to put it in, but you have to make sure it’s all gone before you send somebody in there to get the bananas. What you did find, though, in the stalks of bananas—I don’t remember spiders, but there were a lot of little snakes about six inches long that somehow lived in there. And they were gassed. They’d been dead since they were put on the boat. They came from Central America, they were put on a boat, and then they were taken off the boat in New Orleans and put on freight cars. So they were long since dead. That was one of the problems, actually—

COHEN: Now I’m realizing why we enjoyed bananas so much in Puerto Rico, where they take them off the tree and you eat them. We haven’t had bananas that tasted like that since.

[Laughter]

WESTPHAL: [Laughter] That’s right. At any rate, so that’s how I made my money, doing that.

COHEN: So you were either working or at school. It sounds as though you had a long day and a long night.

WESTPHAL: Yes, that’s right. But school was pretty easy in reality.

COHEN: You were an engineering student?

WESTPHAL: Well, I started out in physics, actually, which was more engineering than it was physics. Then I went back to the field the next summer, after one year of school and working in the grocery. I had had enough experience in the field so that I was used as the vacation substitute instrument operator. You had to know technically what you were about, and you had to have certain skills. And in principle you were the boss of the crew out in the field. But when

you were as young as I was and they were a bunch of the toughest birds you've ever seen in your life, if you had to ask questions or [figure out] a special way to do something—you damn well better know the right way to do it. [Laughter]

COHEN: You learned how to get along.

WESTPHAL: You learned how to get along, that's right. And it was very educational to be out in the country and go to the various towns and meet a whole lot of people who lived in a different way than I had ever seen before. The guys in that business are called doodlebuggers. Some of them were elderly men—forty-five or something like that. [Laughter] It's hard work. They live hard and they drink hard. It was a whole different world.

COHEN: Did you feel you were sort of an observer in all this, Jim? You didn't imagine that this was going to be your life, did you?

WESTPHAL: Well, everybody kept telling me, "Once a doodlebugger, always a doodlebugger, and don't give me this crap about going to college." But I didn't think it had to be that way. It looked to me like it would be a lot more fun to do some other things besides that. I mean, it's hard work for little pay, it's not good for your health, and on and on and on. So then, having done that first summer's vacation substitution—actually, that was the first time I was ever on an airplane. I was flown in in a panic, because somebody had a death in the family in Alexandria, Louisiana, on a crew, and I was on a crew near central Oklahoma. Anyway, I flew from Oklahoma City to Alexandria, Louisiana, on a DC-3 through a series of thunderstorms.

COHEN: I bet you liked it.

WESTPHAL: Oh, I was fascinated by this, because the lightning was right near me. The airplane was bouncing all over everywhere. I can be in pretty rough weather in an airplane without it's bothering me at all. [Laughter] I think I got it all scared out of me. At any rate, so then the company said, "Look, you need to learn more about the electronics and how all this stuff is put together and so forth. Why don't we give you a job on the production line where we actually build the amplifiers and other electronics that are associated with that? This would be in Tulsa while you're in school. We'll arrange for you to get in enough hours, and we can give you a special task." So I'm an expert "lacer" of electronic wiring harnesses. Just as your hands know how to operate a typewriter, my hands know how to do this lacing.

COHEN: So when you went back to school that second year, then your work was your engineering. You were then professional.

WESTPHAL: It was much more professional.

COHEN: It was good to know about apples and bananas, but this wiring was a little more interesting?

WESTPHAL: That's right. I went back to the field again the next year during the summer and did the vacation thing again. This time, instead of being on a production line—since I knew all about that, having done it for a year or so—they put me in charge of the incoming and outgoing inspection. The incoming parts from the vendors had to be sorted into bins of certain values, capacitances, resistances, and so forth. And I had to check all that stuff before the guys put them in the actual chassis. And then when they got the chassis finished I had to go take the newly built chassis and put them under a test set to make sure everything worked right.

COHEN: As soon as you learned something, they'd have you doing something else. [Laughter]

Did they pay you well?

WESTPHAL: Yes. They paid me very well.

COHEN: And they were essentially supporting you all year long, because they were giving you work to do during the winter while you were in school.

WESTPHAL: Yeah, that's right. But they were getting their [money's worth] out of me, so I didn't feel bad about that at all.

COHEN: So you continued this all through school?

WESTPHAL: Yes. I went through that cycle until I got my BS degree in 1953. I actually have the sheepskin I got. I saw it the other day.

COHEN: Was this a state school?

WESTPHAL: It's private. The University of Tulsa started out as a Presbyterian college and then spread out into law and science. I think at that time it was probably the top school in petroleum engineering. Anything to do with the petroleum industry they were interested in. They were really the leaders in petroleum engineering, which has to do with design of everything from rigs to drill bits to whatever. We had a very large population of foreign students, and they were almost all from the Near East—Iran and Iraq and places like that.

COHEN: Was it just a men's school? Were there women?

WESTPHAL: There were women there, but it was largely male. I guess most schools were.

COHEN: Well, in engineering.

WESTPHAL: But they had a religion school and they had a music school and, as I said, a law school.

COHEN: And did you choose that school because of the engineering and physics, or because it was in Tulsa and you knew it and it was convenient?

WESTPHAL: It was handy, and it was in Tulsa, and I had already started down that path.

COHEN: Do you feel you got a good education there?

WESTPHAL: Oh, I think so. But it was just an undergraduate education, and it was certainly not anything that you could compare with Caltech.

COHEN: And you were always able to work essentially full time during this period.

WESTPHAL: Yes, but that was kind of fun, too. I took a lot of pride in the fact that I could do this all myself. I don't think my parents would have had it that way if they had had a choice—I know they wouldn't have, but I never felt that this was a bad thing at all. By contrast, it was very interesting when my son Andrew graduated from Rice. Let me step backward: When he graduated from high school—he lived in Tulsa then—he called me up and said, "I'd kind of like to take a year off before I go to college. You did that." [Laughter] And I said, "I don't have any problem with that. What are you going to do? If you want to do something useful, why, that's a great idea." He said, "Well, I want to just travel in Europe." And I said, "Well, that's a very useful thing and a good idea, but how are you going to finance it?" And he said, "Well, I've

been saving my money.” And I said, “Well, I’m not treating you.” He said, “I think I can do it OK.” And I said, “How much money have you saved?” And he said, “\$12,000, I think.” I said, “Where’d you get that much?” I just didn’t dream that he was making that kind of money. In high school, when he was a sophomore, he went out and tried to find a job, and he got a job with Wang Computers. That was the first business computer—PC-like computer. And he got a job with them. His job was to install the computers in the various businesses and change the formats of the printouts and all that kind of stuff to make them just the way the customer wanted. This was just a part-time job on Saturdays. And that had gone extremely well. The next year he did the same thing, only this time he somehow had a broader suite of customers. Whether this was accidental or whether he somehow finessed it, I don’t know. But he started doing this same sort of thing for petroleum engineering companies in Tulsa.

COHEN: This was all while he was in high school?

WESTPHAL: While he was in high school, yes.

COHEN: A chip off the old block.

WESTPHAL: Yes. He was appalled by the rudimentary way the petroleum refinery design—the petroleum pipeline design and pump station design and heat exchangers and all that sort of stuff—was done. It was done with nomograms. Do you remember nomograms? You had these lines, like this, and you took a ruler and went from here to here and there to there and so forth? That’s how they did the calculations. And so he convinced one of these companies to let him see if he could computerize that for them. And of course it was absolutely trivial to do, even then—conceptually, I mean the details. This company was a branch office of a bigger petroleum engineering company. The stuff he did worked beautifully, and it didn’t take him very long to do it. I don’t know that he ever got paid for doing it; he did it just because he thought it’d be

fun. But the thing spread, and the next I heard was that he had been sent down to Houston in the middle of the summer to install this stuff in another branch of the company. So pretty soon he was sure enough getting paid, and he was getting paid a lot. It was really kind of mind-boggling. The next year, when he was a senior, he decided he was interested in airplanes and wanted to go find something to do with that. Tulsa had—I think was even the headquarters of—Flight Safety, which is a company that makes aircraft simulators. So he went out and got a job with them for the summer. That's when he decided he wanted to take a year off. So that's how he got all his money. But when he actually got ready to go to Europe, why, the Flight Safety people said, "We have an office in London. It might be very interesting to those folks to see some of the work that you've been doing. Why don't we put you in touch with them?" So he was told to call some number, some guy in London, and he did—this is while he's still here. And the guy said, "Oh, we're very anxious to see this. Where are you going to live when you're here?" Well, Andrew was just planning to drift around for a year and see the sights and stuff. But the man said, "We also have an office in Australia, and the person who normally would be here is in Australia for six months, and he needs a house-sitter." And this went on and on. It was just unbelievable.

COHEN: So his trip got laid out for him.

WESTPHAL: Well, it turned out that he had to quit the company to be able to go do the rest of it, because they just had this pile, apparently, of things for him to do [laughter], and they were going to capture him.

COHEN: Did he come back after a year and go to school then?

WESTPHAL: He came back that fall, so he really was away only seven or eight months, maybe—something like that. He and a buddy from Tulsa bicycled all over Western Europe and even Algeria.

COHEN: Good for them.

WESTPHAL: They tried to get into the Louvre five times, and it was closed every time they went [laughter]—but he got to see London and Paris.

COHEN: Well, that's very good. But let's get back to you. You graduated, and then...?

WESTPHAL: OK. Well, I graduated. And the company was very anxious for me to stay working with them. They ran a subsidiary called Well Services, Incorporated, as in oil well—you put instruments down in oil wells to see what's going on. They said, "We have an opening down in Mexico for an operator for one of our well-logging systems. Would you have any interest in that?" And I said, "Boy, I sure would. I've been looking for an opportunity to go to some other country and do something and learn things about that, too." So they ran me quickly through a training program and sent me off to Mexico. This was in '53. So I went to Mexico—again on the first of December [laughter]—and was sent to Poza Rica, which was then the main oil town in Mexico. And it's right in the middle—as is almost every place in Mexico—of the most amazing bunch of archaeological stuff you've ever seen in your life. I did that kind of work for six months, and then our company lost a contract to Schlumberger, which was the big operator in the well-logging business. So then I was transferred to another one of these field crews down on the isthmus of Mexico, to a field camp that was actually out in the jungle. It was thatched huts and so forth. And we were there for, I would guess, three or four months, and then we moved into the nearest little town, which was called Huimanguillo, which was near the railroad, the only transport to that part of the world. There were no roads. We moved into town and continued the same work.

COHEN: Were you in charge of the group or just part of it?

WESTPHAL: I was in charge of the group. There were two of us—two Americans—to teach the others there. The work [went on] continuously, so you had to have two people as boss. And then they gave you a week off, flew you back to Mexico City for a week.

COHEN: So it was pretty intensive.

WESTPHAL: Yes, right. All sorts of wondrous and weird things happened there. I'll tell you one quick one. We ended up in the second story of the pharmacy in this town. It was the only two-story building in town. The pharmacist spoke quite a bit of English, but of course the main part of the crew were all Spanish-speaking people. And we had a medical intern in Mexico who was working for the government for a year after he had gone through medical school, which was free—they would assign you to these government activities. So we had one of these interns, and he was a very sharp guy. So the question was, "Can we drink the water?" The answer from our medico was, "Don't you dare." The answer from the pharmacist was, "Oh, sure, the water's really good. You don't have to worry about it at all. It's called *agua puro*." And we said, "What is it, really?" And he said, "This kid will take you down and show you where they make the stuff." We went down the street four or five doors or so, and there was a guy behind his storefront with rows of five-gallon water bottles. They were all out under a tree and they were filled up almost to the top. And he told us, "Well, I make it right here." We said, "Show us how you do it." He said, "Well, I've got this stick. It's got a piece of extension cord wire. One of the wires is stripped off near the bottom of the stick, and the other wire is stripped off up near what's going to be the top. I just plug [the cord] into the wall and stick one end of it right into the bottle." And he [did that and] said, "Watch." Then he had a whole bottle of wildly bubbling water. It didn't get hot; it just generated enough hydrogen and oxygen to [bubble]. But he thought that that guaranteed that the water was being purified. We said, "Thank you very much. We'll consider it." And we never came back. No way!

COHEN: But most people were drinking it?

WESTPHAL: Oh, yes, the whole town was drinking it. But they all had immunity.

COHEN: Of course. If they didn't, they'd be gone already.

WESTPHAL: Long ago.

COHEN: How long did you work for that company, Jim?

WESTPHAL: I worked for that company almost a full year, from the end of November.

COHEN: Did you learn Spanish?

WESTPHAL: I tried really hard, and I was so embarrassed by the way I was treated by the Mexicans, because they love their language and they'll do anything, anything to help you learn. It's just not a skill I have a natural gift for. Even now, I have a big vocabulary of nouns, but I couldn't work with the verbs. When we were doing this well-logging business, why, a lot of the time we would be sitting there in our instrument truck, waiting for a chart to be made. And they'd drill me and drill me. The guy who drilled me was the hoist operator—he didn't have any education beyond grade-school level. I really was embarrassed, because they were so nice about it. In contrast, the way we treated the people from Iran and Iraq at the University of Tulsa, that was just 180 degrees different from [the way the Mexicans treated me]. We were not nice to the people from Iran and Iraq. [Tape ends]

JAMES A. WESTPHAL

SESSION 2

July 13, 1998

Begin Tape 2, Side 1

COHEN: Last time we were talking about your being in Mexico, working for the oil company.

WESTPHAL: It was the Seismograph Service Corporation. It was a contractor to PeMex, the Mexican government's oil company.

COHEN: Now, we talked about the one year you were there.

WESTPHAL: I was there within a few days of one year. In fact, one of the things I discovered in one of these letters that reminded me was that I sure didn't understand about tax. Because if I had stayed beyond a year, I wouldn't have had to pay any kind of tax on the first year. I went down on the first day of December and I came back on the twentieth, I think it was, of November. If I had stayed ten more days, I would have saved myself a lot of money, by my standards.

COHEN: And the company didn't tell you that?

WESTPHAL: No one told me that. If you don't know what questions to ask, you don't learn things.

COHEN: Particularly about money.

WESTPHAL: Yes, right.

COHEN: Did you continue to work for this company?

WESTPHAL: No, I didn't. I resigned from that company.

COHEN: Was that because you were tired of doing this kind of work?

WESTPHAL: No. I had to come up to the States about the middle of that year for a draft physical—I was about to be drafted, because now I was out of college and I didn't have a deferment. So I had to do that. The company had lost their contract with the Mexican government, but I had to pick up some equipment, up in Veracruz. So they gave me a pickup truck and said, "If you'll haul that stuff up there, you can take the truck and go to Tulsa and do whatever you have to do, and then we'll fly you back down when you're ready to come back, after you get your draft physical." And I said, "Well, if I pass the draft physical I probably won't be back." [Laughter] In my memory what happened was that if you passed your physical, two hours later you were in the army.

COHEN: Of course, there was nothing going on at this time. This was now after Korea.

WESTPHAL: It was just after Korea, but they were still drafting people, so it was a very serious issue. So I went through this draft physical. The first thing they found out was that I was color-blind. I think that's the first time I knew that I was red-green color-blind. And it was because they used the little Japanese dot patterns to see numbers. I think there were fourteen of those cards: one of them everybody could see correctly and the others showed just how color-blind you were. I missed, I think, twelve out of the fourteen or something. So I had it pretty bad—I do.

COHEN: But it never bothered you up to that time?

WESTPHAL: It had never bothered me particularly, no. I was really not very aware of it. But then I realized, “Ah ha, that’s why there was this mystery and that mystery.” [The disability] is more complex than meets the eye.

COHEN: More than just not seeing the color.

WESTPHAL: That’s right. Well, a lot is known about it. It’s an inherited trait, down through the female path, so every generation of males is susceptible.

At any rate, it didn’t bother the army, which kind of surprises me, thinking back. Then we were all undressed down to almost nothing so that they could examine anything they wanted to examine. I did all of the tests. At the end you came to an army doctor sitting at a desk who looked at the results of the exam. Somebody had noticed that I had bandages on my two big toes. He said, “What are the bandages on your toes for?” And I said, “I’ve got very bad ingrown toenails.” And he said, “How long have you had those?” I guess at that time it had been ten years. And he said, “Well, the rules around here are that if you’re not in good enough physical shape to take basic training, I can’t draft you. So you’re 4F.”

COHEN: For ingrown toenails?

WESTPHAL: For ingrown toenails. [Laughter] Isn’t that amazing? And I never heard from them again.

While I was up here, of course, I went to see my folks and the people in Tulsa where I had been living before I went down to Mexico. I had been very big in the amateur astronomy club in Tulsa, so I had a whole suite of friends who were involved in that. And of course I saw all those folks and we had a big star party. And I talked about how many more bright stars you

could see down in Mexico than you could up here. One of the guys who was one of my close friends worked for Sinclair Oil Company, and he said, “Sinclair has started a new research lab in Tulsa. I bet I can get you a job out there. In fact, a guy who would probably be interested in you is a member of this club.” But that guy wasn’t there at that point. At any rate, I made contact with him and we talked for a little bit. And he said, “That’s a hell of a business—to be down there in the jungle all the time, like that. Come back and I’ll give you a job in our research lab.” And I said, “I’ve got to go back down there.” And he said, “Well, just go down and give them notice and then come back. Whenever you come back I’ll give you a job.” So I went back down to Mexico and gave them three months’ notice. Three months later, I turned around and came back to the States, and that’s how I got to be at Sinclair. The lab in Tulsa had just been formed.

COHEN: Sinclair is a US company?

WESTPHAL: Oh, yes. Harry Sinclair—famous for the Teapot Dome [scandal]. He tried to steal all of the oil out from under Teapot Dome by drilling wells around the outside. And he had cohorts all over, and it was a scandal high in the government in the Department of the Interior. The guy, I think, actually went to jail over it. A mean, mean man, Harry Sinclair. He was famous for being a mean man. And the reason there was an oil company research lab—a Sinclair research lab—was that the financial people in New York had told Sinclair himself that no oil company, no corporation the size of Sinclair, could do without a research lab. They said that it would hurt the cost of the stock and that they would have problems. So they said, “OK, that’s easy. We’ll just start a research lab.” And they decided to do it in Tulsa, because the research lab was going to be oriented toward oil exploration, in contrast to refinery chemistry. So the lab had just been formed; it had maybe been there a year or two. They were still recruiting people, and the way they recruited people at the very beginning was they said to the financial people, or their consultants or whatever, “What do we need for people?” Well, you needed twenty-five-percent PhDs; fifty-percent people who had a college degree, preferably a

master's degree; and twenty-five-percent workers. Or whatever the arithmetic was. And that's what they did. They just went out and put ads in newspapers and around universities, and people just poured in. It was amazing. They actually got a bunch of very good young people by that process. They got some turkeys, but the turkeys disappeared rather quickly. The joke around the place was that somebody said, "Well, where's your library?" "Library? Why do we need a library? Harry Sinclair has taught us that books are the work of the devil." [Laughter] Whether that was true or not I don't know, but that was the story. So the zinger in the story was, "How big a library do we need?" "Well, we need fifty feet of bookshelves full of books." This story is probably not really exact. But it certainly was the attitude. But the story was that they went out to the Salvation Army and got twenty-five feet of bookshelf with books. Just any old books. It was true that there were some pretty weird books there when I came.

At any rate, it was a brand new enterprise and it was looking for people. So they hired me, really just on the basis of the recommendation from the guy I knew. I had been building telescopes, so [I was hired] just based on the fact that I managed to build telescopes that worked. That was about all the qualifications I had, really.

COHEN: Well, you had done all the work for the other company. That must have counted for something.

WESTPHAL: It did.

COHEN: So what was your job when you went there?

WESTPHAL: I was just in the lab. It was a very disorganized place. The guy who actually hired me—his name was Jimmy Johnson—I think had a master's degree from Colorado School of Mines. The Colorado School of Mines, then and now, is the source of most exploration geophysics people; there are a few from Texas A&M, but not many. Anyway, he was a very

flexible guy, this Johnson. He ran a very loose, flexible organization. He, I guess, was the person who decided what we should do. Within a year or so—maybe less than that, even—I was the leader of a group of four other guys, and our task was to look at unconventional ways to find oil. So we advertised in all the newspapers in the oil patch in the south-central part of the country. We advertised that we were interested in unconventional ways to find oil, and if you thought you had something that was a way to find oil, then we'd like to at least talk with you. A flood of people responded to that. One of the old official ways to find water is dowsing. Well, that got converted by many of the people in the oil patch. They'd hang a little bottle of oil on a string from the bottom of a "Y" and then they could go out and dowse for oil. This went on and on, in all kinds of variations. Our group had one million dollars a year, in 1955. Here I was, twenty-five years old with a million dollars and four people, and we could do anything in the world we wanted to do, because the company didn't give one damn what we did.

COHEN: It was just this front of having to have a research place?

WESTPHAL: It was so they could say that they had a research lab. Well, we lucked out by having some really bright, trained guys. And Johnson was very supportive of all of this, and anything we wanted he'd sign up for. We went through several cycles of looking at some new ways to determine where oil was. This was not crazy stuff; it was good physics, as a matter of fact. One of them had to do with [the concept that] oil essentially floats—it doesn't really float, but it diffuses on top of the water that's underneath it. The whole earth, down to five kilometers or something: if there is any permeability at all then that's a path for stuff to move. It's full of saltwater from about 3,000 feet down everywhere, and in lots of places it's much more shallow than that. So there were various things we did associated with that idea. One of the things that's fairly easy and quick to talk about is the La Brea tar pits. There, oil is coming out of the ground, right? And all on its own. There's a big oil field there. You don't see much of it anymore, because it's drilled out to some extent, but it also just got covered up with houses. So it's not a

big leap of faith to say, “Gee, if we could get a hundred times more sensitive way of seeing oil or natural gas in the soils, we might be able to find oil with that technique.” A lot of people have tried that in some simplistic kind of way—everything from sniffing the ground, literally, which is not a bad way to do it as a matter of fact. You can really smell an awfully small amount of material of certain kinds, particularly some of the stinky oil kind. What we did was to go out and find a guy at Penn State who was making the first mass spectrometer for organic materials. We had him build us a wonderful machine. Did we talk about the mercury problem?

COHEN: No.

WESTPHAL: OK. Well, we paid a lot of money for this wonderful machine—I think \$100,000 or so. We really put this guy in business building these mass spectrometers. There were a lot of problems with getting it going. We finally got it running, after lots of false starts. It was on a Thursday afternoon, as I remember, and we left it sitting there running. On Friday morning there were mad, panicked phone calls to many people, including me, but mainly to the guy who was the chemist, who was going to be the user. Calls from the lab at, I don’t know, three o’clock in the morning, or seven o’clock in the morning, or whatever. And when he talked to me he said, “There’s something weird in that house.” It was a house on the grounds. This lab was built in an old bulk plant, where trainloads of gasoline and oil came in, and we’d hose them into the tank trucks and so on. And yet it had about three wooden houses on it, where people lived and worked. He said, “Weird stuff is going on in there. Lights are flashing and all kinds of stuff.” And I said, “Well, why don’t you just get away from it and I will come down and take a look. Get far enough away from it so you feel comfortable, and if it starts burning, call the fire department.” [Laughter] So I got down there and, sure enough, there were blue lights inside. I immediately realized that there had been some sort of electrical problem and perhaps even more than that. So we turned the power off on the outside of the building. The pumps that were in this mass spectrometer were all mercury-diffusion pumps, and there were several gallons of

mercury in these several sets of pumps. It didn't take long to realize that these blue arcs we were seeing had to do with mercury vapor, and that surely there must be some massive vacuum leak in the system that was letting air in and mercury—

COHEN: And vaporizing your mercury.

WESTPHAL: So when the power went off—

COHEN: Did you know it was poison at that time?

WESTPHAL: Oh, you bet we did. So we opened all the doors and got away from the thing and left it until the vapor dissipated. And we had the old-fashioned kind of mercury sensors—little conical things with a candle in the bottom that caused air to flow through it, and if the mercury changed the color of this paper that was inside—like a litmus thing...that was the original mercury detector. So we took that and it was responsive to a certain level, but nobody knew how much mercury there was.

COHEN: You just knew you had a leak in the system?

WESTPHAL: Yes. So we finally let everything go, and then we went in and started trying to find where the leak was. And we started, of course, at the end farthest away from where the pumps were. We worked our way along, valving stuff off. There were plenty of valves. It didn't take very long to get to the point—even the very first valve from the mechanical forepump didn't stop it. You'd close that valve and you still had the leak, which told us that the leak was in the forepump. I wasn't right there when they discovered this, but somebody looked at the top of the forepump, brand-new forepump—and it was a fancy one, the best you could buy—and here in the top of it was a brass plate about four by four inches square from Sinclair Research

Laboratories: equipment numbers such-and-such and such-and-such. And in the four corners of this there were four holes and there were four nails. Some guy from the property had come in while we were having a big celebration the previous day and—

COHEN: He just came in and put—

WESTPHAL: It was a perfectly reasonable thing to do. He didn't know anything about pumps. He came in with a hand drill and *zt, zt, zt, zt*, drilled the holes and put the plate down and nailed it in. It was wonderful. [Laughter]

COHEN: Oh, wow. At least you found the problem.

WESTPHAL: We found the problem.

COHEN: How long did you stay with this organization?

WESTPHAL: Until I came here.

COHEN: I see. So that would have been quite a few years.

WESTPHAL: Five years. So we went out doing various things. That was one of the simple ones. It was looking for micro oil seeps. And we tried to do it really right, by using this organic mass spectrometer and looking for traces of some of the organic materials that only exist in oil. It doesn't exist in decaying leaves or any of that stuff. I can't remember the names of them now, but they smell godawful. At any rate, that didn't lead to much.

COHEN: But you enjoyed that work?

WESTPHAL: Oh, sure. It was great fun. You never knew what you were going to find.

COHEN: You had your own say about what you wanted to do.

WESTPHAL: Yes. And through all of this I was a young guy, with a whole bunch of fresh PhDs.

COHEN: And you still had your connection with your amateur astronomy people?

WESTPHAL: Yes, I was having a good time. So it rattled along and we looked at various things. One that was popular at that moment was something called Radoil—it was a trade name. And there was a guy in Shreveport, Louisiana, who was an independently wealthy person with no technical training. And he claimed that radio waves, unlike what theory tells us, were able to go long distances underground in the earth and that if you did the right sort of measurement you could find the edges of oil fields. You could find the place where the interface between the water and the oil was. And the physics of that was just nonsense. We didn't spend much time at all with the physics of the thing. The first thing we did to show that the waves didn't go through the earth was to go up into northeastern Oklahoma in the zinc-mining area where there were a lot of old, abandoned lead zinc mines that were so old they didn't even have any wires. We'd take a radio receiver down in these old mines and then we'd put the radio transmitter up on the surface and see if we could hear it. Thinking back, that must have been dangerous, but we did it. I don't remember any concern that we had about it being dangerous. Well, we couldn't hear anything.

COHEN: As you had expected.

WESTPHAL: As you would expect. But he quite properly zapped us, because we hadn't really done our homework quite well. And he said, "Well, what in the world do you expect when

you've got twenty feet of lead between you and the transmitter?" [laughter] which maybe was about right. There was an awful lot of lead in that rock. So we later went to a place that everybody agreed upon. It was up in Illinois in a coal mine. And everybody agreed that there were no wires and no way for [waves to be transmitted] down there. We even blocked the mine shaft top so stuff couldn't channel down and give you a false reading. And the guy was along with us—we were playing this game absolutely straightforwardly. And there was no sign of it down there. Well, that didn't bother him a bit, because he was a very practical fellow.

COHEN: He had faith evidently.

WESTPHAL: Well, not only did he have faith but he was making a lot of money. He was wealthy before this ever started—I don't know how much money he made out of this, but probably a lot—but in any case, he made a deal with big oil companies. Not Sinclair, but Texaco and Standard of New Jersey and various individuals. It was a win-win situation. If they would show him a place where they thought there was some possibility of oil, he would do a survey for free. And if they found oil, he got fifteen percent of the profit from that oil. And if there wasn't any oil, he got nothing. So it didn't cost them anything unless he found oil. And they'd be happy getting the oil at fifteen percent. So everybody said, "Well, that's a neat thing, but I'll bet that will never happen." And he would show checks from the various companies of royalties he was getting from the oil he found. So that was a big deal, and that drove us to finally kill this whole thing. We did a bunch of fun stuff, including going down to the Florida Keys. This was in May of 1960. We found a place just offshore, to the east of Marathon in the Keys, where there was a large area of ocean that had a sandy bottom before it fell off into the gulf stream. So we could keep our instruments totally immersed in seawater. And we made transmission measurements through the seawater, because that was the critical thing. And everybody agreed, including this guy, that that was a good experiment. That was an even better experiment than the one we had done in the coal mines. Well, we got exactly what you would predict using the equations.

COHEN: It sounds like you had a good time traveling around.

WESTPHAL: Oh, we had a great time. That one was particularly popular. Almost everybody and his uncle came down to visit us. We taught a lot of people how to snorkel over the course of two months!

COHEN: So there you are, doing things that are very interesting. How did Caltech find you? How did that happen?

WESTPHAL: It happened that Tulsa had then three or four oil company research labs—maybe even five. When you have these kinds of facilities, why, you have to have a Geophysical Society of Tulsa. That's so you can all get together and whoop it up and tell each other secrets and lies—mainly lies. At any rate, at a meeting in Houston of something called the Society of Exploration Geophysicists, which is really a trade group but a quasi-technical organization—I heard C. Hewitt Dix, who was a professor of geophysics at Caltech and was the father of exploration geophysics. Not only of seismic stuff—gravity, magnetic, and various other techniques of learning about things. There's a whole world of exploration geophysics having to do with ores and stuff, not petroleum at all. So he made a presentation about some studies that he was making out in the Mojave at the Old Woman Spring area, in Soggy Dry Lake. He was trying to measure the depth down to the bottom of the earth's crust—the Mohorovicic discontinuity, which was a big new thing in those days. And it was normally measured by using earthquake waves that were refracted through the earth. But he applied the technique to some oil exploration, in which they do reflection seismology. And he thought there was likely to be a high enough difference, contrast in the velocities in the Moho below and above that each reflects.

COHEN: And you went to this meeting?

WESTPHAL: Yes, well, we all went there, because that was our trade organization, as it were. And I had never met the man. I certainly knew his name. Somehow or another—it was probably purely by accident—I did meet him, but there was nothing special about that. I mean, he didn't know me from Adam, and there was no reason in particular—except purely accidental—that we met. But when we came back home, why, several of us in these different oil companies said, “You know, I bet we could do some of that stuff.” We had all the equipment lying around, all over everywhere. We had more people than anybody knew what to do with. The guys at Sinclair had all this money that they couldn't spend. [So we thought], “Why don't we see if we can do some of this?” And in those days one of the big problems was communicating between whatever was the source of your seismic waves and the recording equipment that, in this case, was going to be thirty or forty miles away. Well, it was very hard in those days to get radios that would go that far that were in any sense portable. So we had to figure out a way of being able to do it without direct communication. Just east of Tulsa there was a very large limestone quarry, and every Friday afternoon around five o'clock, quitting time, they would blow up enough limestone for the next week's use. They used ammonium nitrate; this was the first time I heard about ammonium nitrate as an explosive. They used ammonium nitrate because it was cheap and easy—you'd just pour it out of a sack into a hole and pour diesel fuel on top of it and stick a 1-inch by 8-inch stick of dynamite down there and a blasting cap and away you went. So there was this thing that went on, essentially every Friday night, if the weather was decent at all. These were large amounts of explosive—probably two or three tons. So it occurred to me that we ought to be able to go out there and put a wire down into one of these holes that they were going to blow up and run a small current through this wire and through the resistor and in turn put that voltage onto an ordinary tape recorder, a stereo tape recorder. And on one channel of it we put WWV, [the signal of the atomic clock at the Bureau of Standards]. On the other channel we put the voltage that was across this little resistor. Now, there was a constant voltage there until the shot went off and broke the wire. So at each step

down it would make a good click on the tape. So you could time the explosion immediately, to within a few milliseconds, to the WWV signal. Then we took these recording trucks—and we had several of them—and we spread them all out over a big piece of eastern Oklahoma. We didn't talk to Hewitt or anything about this—we just did it. If it didn't work, why, we could all go back into our holes and not be embarrassed. If it worked, we would be heroes.

I'd go out to the quarry and at five o'clock, or whatever time they told us, I'd be listening to WWV and I would tell the guy to push the plunger, and he'd shoot the charge. And then everybody out there at that same instant, or maybe ten seconds later or so, would start making their recordings. They could tell whether there was a signal, whether the charge had gone off, because you could see it on the recording. And if for some reason we didn't shoot it during that five minutes, why, you'd try it every five minutes between five o'clock and 5:30. And they would give up, because that meant that the quarry blast didn't occur. Well, we got a bunch of records straightaway the first few times we did this. So we talked about it and I said, "Look, why don't we send this off to Dr. Dix and see what he thinks?" Because we thought we could see this reflection really nicely. So I called him up and told him what we were doing and asked him if he'd like to see this. And I think he was a little reluctant. I think he didn't really think this was something that he could be sure had been done right.

COHEN: Nonacademics in the field, is what you mean?

WESTPHAL: Yes, right. And quite properly. He didn't know any of us. So we sent him the report by airmail. And he told me later on that he didn't get the second roll unrolled until he decided that it was absolutely real. He called me up and said, "This thing is real. Can I come and spend the summer with you guys and we'll do a whole lot of this?" And I said, "Gee, that's the most wonderful idea I've heard in years."

COHEN: So you were really in charge of this whole operation, that particular—?

WESTPHAL: Kind of, yes. There were other people. I wasn't the only one. What I said to Hewitt was, "Gee, we'd love to have you here and we would love to have you as a summer consultant." And he said, "Oh, that would be just great. There are probably other interesting things you guys are doing, too. I'd like to look in on what you do." Well, we never dreamed we could hire this man as a consultant; it never occurred to us to do something like that. It turned out that apparently Standard of New Jersey, who had been in the business in Tulsa for years, had been trying to hire him as a consultant for the summers for years, and they never could do it.

And we did exactly what he asked us to do. We helped him in every way. We got a lot of data like that, and during the course of the summer he and I got very well acquainted, and we talked about various things that were hard to do those days, which are now trivial, and settled on a particular way—which actually was very clever, I must say—of doing a cross-correlation between different channels of the seismic record, and in fact doing Fourier analysis so we could see what the frequency spectrum looked like. So, as time came for him to come back, he said, "I've got \$4,000 that a widow woman gave me to do whatever I want to do with it. If you think you can build this thing we've been discussing"—which was a physical piece of hardware—"for that or for less than that, I'd be tickled to death for you to come out to Caltech this winter sometime." Actually, I planned to do it the next year [1961], in the spring. And just about that instant, we discovered that my wife was pregnant.

COHEN: You didn't mention your wife. When did she come on the scene?

WESTPHAL: That happened immediately when I came back from Mexico. I had known her since I could remember, because she was the niece of the next-door neighbor where I was staying when I was going to college, and I always thought she was the neatest thing on toast. We had corresponded and so on for several years.

At any rate, we were, at that time, pregnant. And the baby was going to be born sometime in June, which was about the time Dr. Dix was talking about us coming out to Caltech. And that didn't seem to be a very good time. So I thought this was probably going to kill the whole thing. He said, "Can you come earlier, before the baby is born?" And I said, "Well, I hope I can." I talked to my wife about it, and she didn't see any reason why we couldn't do that. It was now six months before [the baby was supposed to be born]. The idea was that I would probably be there just three or four months, so there was a real window there. Then another problem was that I didn't know whether I could get a leave of absence from Sinclair. And Dix said, "Oh, I've already taken care of that. You don't have to worry about that." [Laughter] And I'm sure he had. I bet he didn't get the first four words out of his mouth before somebody said, "Sure." So I came [to Caltech] on the second day of January, 1961. [Tape ends]

Begin Tape 2, Side 2

WESTPHAL: So we came here on the second day of January 1961. I contacted Hewitt one way or the other when we arrived. My son was born in June of '61, and [for that] we went back [to Tulsa]. But meanwhile we built an analog Fourier analysis, and it worked.

COHEN: Was this your first time out here? Was it the first time you had seen Pasadena?

WESTPHAL: No. I had been here, I guess, twice during the previous summer, trying to get a company in the movie business to build us a forty-eight-channel analog tape recorder to record this data. But I hadn't spent any real amount of time here.

COHEN: Because Pasadena must be quite beautiful compared to Tulsa.

WESTPHAL: Yes, although in the springtime Tulsa is a pretty nice place, and in the fall as well. In the winter and the summer it's not too good. At any rate, we settled in. The first thing Hewitt wanted to do was try to get some of the holes in my education filled up. So he sent me off to [Julius] Miklowitz to take AM [Applied Mechanics] 116. I never worked so hard in my life. The deal that I made with Miklowitz—or what I agreed to—was that he would treat me like any other student. He would grade my papers, or have them graded, like any other student. And I was expected to act like any other student. And although I would never get any credit, he would grade it all and he'd give me a grade at the end of the quarter. So I worked for three or four or five hours a night on that. It was tough, because I had never had that level of mathematics in the first place, and certainly I had forgotten almost everything I had ever known over the few years. But I tried like hell. And when the quarter was over, he called me in and said, "Well, you did your part of the deal. Here's my part." He gave me a D+, and I was the happiest man on the face of this earth. To believe I could make something better than an F in this course. But the deck was stacked, because I didn't realize until years afterward that he sat students on each side of me in class who were really sharp guys. And it's crystal clear to me now that he encouraged those guys to help me, and be tutors essentially. And they did. That was a fabulously neat thing that he did.

COHEN: Did it ever occur to you to go back to school when you came out?

WESTPHAL: Oh, Hewitt was very anxious for me to go back and get a graduate degree. But I think that that experience taught him that that probably was not a realistic notion. We talked about my going down to Scripps, because I was very interested in the ocean. The work we had done in Florida really hooked me on the ocean. I had built an underwater camera in the early days, when almost everybody used ordinary flashbulbs under water, and the fish moved so fast that you always got smeared fish. I realized that you had to use a strobe. So I built a strobe-illuminated camera, and when we were down there doing this work in the ocean, I'd go on

weekends and at other times and take pictures of fish and so forth. And then at the end of it, one of the guys I had recruited to go down there started a scuba-diving enterprise in Tulsa—that was very early in scuba diving, when there were just a few people you could trust in the water. We went over to the Bahamas. So I had this whole suite of absolutely gorgeous pictures of the reefs, particularly the ones in the Bahamas.

COHEN: Ah, that's where your scuba diving comes from.

WESTPHAL: Yes. And I was very proud of the pictures. So when I got here, why, I mentioned something about having taken a bunch of neat pictures in the Bahamas. And somebody said, "Gee, why don't you show them to us some lunchtime?" So I did that. Fifteen minutes after it was over, I got a call from Heinz Lowenstam [Caltech professor of paleoecology], who had not been there when I showed the pictures. And he said, "I hear you've got some really good pictures from the Bahamas." And I said, "Sure, would you like to see some of them?" And he said, "I'll be right down." So he came and I showed him a few of them, and he said, "Can I borrow them?" And I said, "Sure, you can borrow them." He sat down at least four or five undergraduates during off-hours in, I guess, what's now called the Buwalda room. And they did a survey of animals and plants that they could identify in these pictures, just like an archaeologist. They actually had a framework with strings on it, like an archaeologist might have. And they put that up on the screen and projected this thing on it. It was really a fun thing to see them in action. But I didn't know one animal from another, of course [laughter], much less a plant. I had discovered that Heinz had an aquarium up on the third floor, but the problem with the aquarium was that everything he put in it died, and he couldn't figure out what was wrong with it. He was using an ordinary commercial hobby-shop aquarium, which was fine, because those are made with stainless steel and glass. But he was very anxious that things not fall into the water, because he was spiking the water with different kinds of isotopes of strontium. He was very anxious to understand the calcium-strontium balance primarily as

functions of temperature and concentration—that is, the original ratio in water. I had built an aquarium at home in Tulsa, but in those days you couldn't go out and buy tropical fish, so I had other kinds of fish. And I took one look at his aquarium and saw that it had a lid on it that was made out of Lucite—and that was fine, but it had brass hinges on it. And the hinges were the most beautiful green color that you ever saw in your life. I said, "Heinz, that's copper." And he said, "Well, copper will kill everything." [Laughter] And I said, "Yeah, copper will kill everything." So we right quick got rid of all the brass and everything started growing like mad.

COHEN: You really established a reputation for yourself.

WESTPHAL: That's right, yes. And he was cooling this aquarium with an old coke box—the kind that had water circulating inside. Originally there was ice inside; he'd put refrigeration underneath. By the end, things were going like mad. You'd get off on that floor and everybody was very interested and there'd be seawater going all over the building. So it was agreed that we'd move everything down to a room in the sub-basement of Arms [Charles Arms Laboratory of the Geological Sciences]. And I said, "Heinz, if you want all of these animals to be at the same temperature, why don't we just air-condition the whole room?" Because he had had water circulating through copper pipes first. I got rid of those and got the stainless pipes.

COHEN: For some reason we have moved from Dix over to Lowenstam. You did both?

WESTPHAL: I did both.

COHEN: And your course every night?

WESTPHAL: Yes. We haven't talked about Bruce Murray [professor of planetary science and geology] at all yet. Anyway, a lot of things were happening and it was all just tremendous.

COHEN: You were having a good time?

WESTPHAL: Oh, I was having the time of my life.

COHEN: Did your wife like Pasadena? It sounds as though you didn't have too much time for her.

WESTPHAL: Oh, yes, she liked Pasadena.

So that was the path the visit took. The official deal when Hewitt brought me here was that I was going to work on Hewitt's Fourier machine. But Hewitt being Hewitt, if there was something else I needed to do and it didn't take forever and it didn't totally derail everything, why, great. So as we got near the end of the four months and the machine was working—we built a little darkroom in Hewitt's lab—I was sitting one day at the workbench where we were building stuff and somebody came walking in and came up behind me. I turned around and said, "Can I help you?" And he said, "Are you worth a shit?" And I said, "Pardon me?" And he said, "Do you do good stuff?" And I said, "Well, some people think I do." And he said, "Oh," and turned around and walked out. He was a fairly short fellow with black hair—you can guess.

COHEN: Some introduction.

WESTPHAL: Wasserburg [Gerald J. Wasserburg, John D. MacArthur Professor of Geology and Geophysics]. Wasserburg checking out whether—well, it turned out that ten minutes later I got a call from Bob [Robert] Sharp [chairman of the Geology Division, 1952-1968]. So I went to his office, which was just down the hall. And he said, "We want you to stay." And I said, "You guys can't afford me."

COHEN: So Gerry was putting his OK on it?

WESTPHAL: Sure. He always does that. It doesn't matter who it is either. [Laughter]

COHEN: Or who you're going to go work for?

WESTPHAL: Exactly. From Gerry's view, he is doing that as a service. And it is a service. At any rate, none of that did I appreciate at all. But this was an unusual place, so nothing surprised me much. Anyway, Sharp said, "We want you to stay." And I said, "I can't stay. We're going to have this kid sometime within the next couple of months." And he said, "Well, we want you to come back then." And I said, "Well, I don't think you can afford me. I make too damned much money at Sinclair." And he said, "I know how much you make at Sinclair. You make eleven thousand dollars a year, and I'll offer you thirteen." So that's how it came to be. We went back to Tulsa, we had the baby, sold the house, and I got a leave from Sinclair.

COHEN: And you haven't looked back.

WESTPHAL: I haven't looked back.

COHEN: So you came out and you were a research engineer?

WESTPHAL: I was a research engineer.

COHEN: And was there something specific you were going to do?

WESTPHAL: Well, after Sharp called my bluff I said, "What's my job?" And he said, "Well, your job is to decrease research resistance around here. I don't care who it is—whose research it

is—that you’re decreasing the resistance to success. It doesn’t even have to be in this division. It doesn’t even have to be in this university. If you want to follow up with your astronomy stuff and work with the people at Santa Barbara Street [Carnegie Observatories], God bless you. Do it.”

COHEN: What a job description.

WESTPHAL: It was just awesome. And it has been, to this very day, a monkey on my back. Every time somebody comes into my office and says, “How can I do this? I’ll do thus and so,” I hear Bob Sharp saying, “Go decrease this research resistance.” It happened to me on Friday with Paul Wennberg, our newest—I guess he’s an associate professor [of atmospheric chemistry and environmental engineering science]. He had heard someplace that I knew how to build dewars. And he came in and wanted to know if he could do this and that and the other thing. We spent two hours at the blackboard.

COHEN: So you must have had a unique position—not only at Caltech but anywhere.

WESTPHAL: Probably anywhere.

When I asked Bob Sharp that question and he gave me that answer, he also said, “Now, your salary is going to be financed by—” Well, it was something like twenty-five percent from Heinz, twenty-five percent from Bruce Murray...

COHEN: From their contracts?

WESTPHAL: From their contracts. And fifty percent was from Hewitt, I guess.

COHEN: OK. So what were you doing for Bruce?

WESTPHAL: Well, the main reason, it turned out, that Sharp wanted to get me to come here was that, while I was here that four months, Bruce was a postdoc at that time. And Bruce was very anxious to find out whether the moon was chemically differentiated—a very fundamental question about the moon, which we did not know the answer to. And you might imagine that you would be able to find that out if you could see that there were places where the rocks had different colors. And that had been tried by some people at the USGS [United States Geological Survey] by taking pictures. But they couldn't find any clear evidence that any color contrast there was more than ten percent, or something like that. And Bruce was very anxious about this, so I suggested that maybe we could get data, somehow or another, with a real photometer—maybe the 60-inch at Mount Wilson or maybe the 20-inch down at Palomar, or whatever. This was while I was here the first four months. Bruce had talked to somebody, I suppose, like Bowen [Ira Sprague Bowen, director of the Hale Observatories]. I don't really know. At any rate, [Bruce] had some 60-inch time, and he didn't really know what to do with it. He was trying to put a 35-millimeter camera on this thing. And I suggested that they probably had a real photometer for it, and that we should go and see if we could use that, because then you could get the one-percent kind of numbers. It turned out that they did. But we found that it was a bigger task than we could do just by running up there, because you had to know just where you were looking. So you needed something that would allow you to take a picture of the moon where you were getting the different intensities of color. Well, that was a straightforward thing to do, but it was not something that the Mount Wilson/Palomar Observatories had any interest in doing. But I suggested to Bruce that it could be done without too much trouble. And then there was the Madman of Cornell, Tommy Gold. [Laughter] He had proposed to NASA that the moon was covered with a kilometer of levitated dust—electrostatically levitated dust—and that when the astronauts got there they would just [sink], *psssh*, like that, and that'd be it. And NASA went into full-up panic, as one would imagine they might. So they were very, very anxious to find some way to know whether this was so. After some discussion between Bruce and me it seemed

that it was clear that the thing to do would be to try to see the nature of the surface—whether it was solid rock or dust or pebbles or something else—and I had had a little bit of experience at Sinclair. We'd built an instrument that worked in the infrared—down in the 2-micron region, where there are many absorption bands in the infrared—that you could handhold in a light aircraft and fly down a pipeline and see methane leaks, because the sun shines on the ground and is reflected back up through, double-passing the methane, and it would look like a black spot. And that was an absolute wonderment, because the biggest problems Sinclair had economically in those days was pipelines that were leaking and blowing up. I knew that little bit about infrared—and that was about all I knew about it, really. So Bruce and I discussed this. In those kinds of discussions, you don't know who said what to whom and when, but, at any rate, between us we came to the idea that a neat thing to do would be to look at the sunset terminator of the moon just after the sun has set. When the sun is up on the moon, why, the surface is hot. It's about 300 degrees centigrade or something, so it's really hot. It emits a tremendous amount of 10-micron infrared. But in the shadow, if [the surface is] dust, the dust doesn't have any finite amount of conductivity, because the grains just barely touch each other. So the only heat transport is by radiation, through the dust. In fact, that used to be the way they made dewars. They'd put powder inside for that purpose. So our idea was that we would watch an area that was just going out of the sunlight and see how fast the temperature fell.

COHEN: If it fell really quickly, it was dust.

WESTPHAL: It was dust. If it fell slowly, then it was like the concrete sidewalk out here when the sun goes down. It looked to us at the time as if this would work like a charm. But of course it meant that we had to have a telescope, and we had to have one that we could control ourselves, et cetera, et cetera. So when [this idea] was being proposed, I guess, among the faculty or among all the players [before I came] back, why, Bruce was pushing it extremely hard. He really wanted to do it. So that was one of the higher-priority things. And then it also turned out

that just in those few months between the time I left and the time I came back, you could get analog-to-digital converters for the first time. And that, essentially, put my neat Fourier machine out of business, which was OK with me. We [had] made it work. So the priority of that and the [other] things I had been doing was much lower than it had been before. And by then, of course, [Robert] Leighton [William L. Valentine Professor of Physics, emeritus, d. 1997]....The story I've always heard was that Leighton was getting a million dollars a year from NASA with no strings attached—money that he could use to start any kind of activity at Caltech that he wanted to and that he thought would be useful to NASA. And out of that came the X-ray astronomy world, out of that came the infrared world, out of that came all kinds of other things I don't even know about. And I've been curious whether that story is true or not.

COHEN: But you did do your experiment with Bruce Murray?

WESTPHAL: Yes. After I came back, that was a high-priority thing. A big problem we had was finding a telescope somewhere that we could use to do this. And we thought—it turned out incorrectly, because we weren't very good physicists and we really hadn't thought it through very carefully—but we thought we had to go to the highest altitude that we could possibly get to, to get rid of the emission from the atmosphere. It turned out our emission problems were in our telescope, not in the atmosphere. And so we went up to White Mountain, up to 13,000 feet, and built a dome. The first thing we did was just try to see how this whole world worked. We talked to Ike Bowen and he made this flat statement, "That's interesting for this planetary stuff. I don't know much about that, so maybe you can do some good stuff there. But you'll never be able to do any real astronomy at that wavelength, because any object in the sky that is bright enough for you guys to see in ten microns, we would see it as the brightest thing in the sky in the visible wavelength." It was just fascinating that he would think that. Ike was an awfully, awfully sharp guy. It apparently was just totally out of his imagination that there could be cold stars—stars that were cold enough so that most of their energy, or some large amount of their energy, was

coming out at that wavelength. And later on, when we got involved with the 2-micron sky survey, the prediction was that we would only see thirty or forty stars. We saw thirty or forty stars within the first week that you couldn't see on the chart, because they weren't there.

COHEN: So now here you are. You are finally doing astronomy. [Laughter]

WESTPHAL: [Laughter] So I weaseled my way into astronomy. Anyway, there's lots more to that story.

COHEN: We can go on to your next big project when you were out here—the Blue Glacier project.

WESTPHAL: Yes, that was my thesis project. [Laughter] At least that's my interpretation. Anyway, there came a time—and all of this was invisible to me as it happened; I had no inkling that anything was being discussed—when I had been a senior research fellow for some time, for several years, and there apparently was a serious discussion among the faculty about whether I should be an associate professor. And Sam Epstein [William E. Leonhard Professor of Geology, emeritus] had made a statement that professors had to profess. I had in fact been teaching some at that point. But his second statement was, “Where are the papers?” I had my name on a lot of papers, but where was a paper with my name on it with work that I did that looked like something real? So I heard that, and I thought, “The man's right.” So one of the things that Barclay Kamb [Barbara and Stanley R. Rawn Jr. Professor of Geology and Geophysics] had been doing was work on the Blue Glacier, along with Bob Sharp. Bob Sharp had started this whole thing right after the war. At any rate, Barclay came to me and said, “People have tried for a long time to use seismic techniques to measure the thickness of the ice in various places, in Greenland, and around, on the Blue Glacier, and nobody could ever get a number that they could

trust, that they believed was right, because they really didn't have any idea how deep it was. We are now drilling these holes all the way through the ice so we can watch and see how the glacier moves over the rocks underneath it. It's a window to watch what's happening. So we know how deep the ice is in a few places. Can you think of any really good way to measure the thickness of the ice?" So I said, "The ice should be a really extremely uniform acoustic medium. It should be very clear acoustically. And there's really not any reason to believe that you couldn't use a very high frequency—say, 50 kilohertz, which is the wavelength you use in fish finders in boats, depth sounders in boats. So why don't I get one of those and set it up for you? You could go look there next year. Go find a puddle of water and stick it in there and see if you see anything." He said, "That's an interesting idea." So that came to be, and they didn't see anything. It was a real mystery why they didn't see anything. By then I was intrigued by this problem and it was right up my alley. I was the obvious person for it. So they invited me to come along on the annual trip to the Blue Glacier, which was always in August, because that's the only time it doesn't rain. We chose one of Barclay's drill holes, which was by then full of water, and put a hydrophone down in this thing. I jerry-rigged an oscilloscope with a camera on it that could be run with a converter on 6-volt batteries, which they had because they used them for their drills. I measured the acoustic attenuations as a function of frequency through the ice and discovered that it dissipated as a function of frequency with λ to the fourth. Well, λ to the fourth—if you say that to a physicist, they immediately say, "Rayleigh scattering!" just automatically. When I first saw this stuff, I was out there still, and Barclay was sitting out there on the ice with a little tripod in front of him and a great big pair of Polaroids and some slices of ice, and he was measuring the size of the individual crystals and their orientation. And I said to him, "This is the result." And he looked at this and he said, "And there's why." And I said, "The ice is not a uniform velocity in all three crystal directions." And that was true. Then the question was, "How can we prove that that's what it is." And he said, "Well, there are places around here—I can show you one—where there's a single ice crystal as big as this table. You ought to be able to see the difference." So I did a bunch of things like that. I did this whole thing on my own. I

started it all myself. I wrote up the paper ["In Situ Acoustic Attenuation Measurements in Glacial Ice." *J. Geophys. Res.*, 70, 1849 (1965)] and got various good people—especially Gerry Neugebauer [Robert Andrew Millikan Professor of Physics], who is one of my heroes, because, particularly in those days, every time I wrote a paper I'd send it to Gerry. He was a friendly but tough, tough reviewer. It was very interesting: I never had a paper rejected once it had gone through Gerry. He volunteered to do that.

COHEN: So that was your thesis.

WESTPHAL: Well, I didn't think of it in that way at all. I just thought, "Here's the answer to the question. Now we know how to do it." So the next year, why, we went up there and it turned out that we could use the sounding at 500 hertz, because the Rayleigh scattering wasn't so severe, so they could make it happen. But apparently—and it really has only been quite clear to me recently—they were looking at that as a thesis. There were people high in the administration who had to agree that this was a good thing to do.

Now, the other thing that I learned fairly recently—probably fifteen years ago—was that Bob Leighton was a very strong advocate of doing this. And Bob Leighton was a person who was not very kind to dumb animals; he wasn't very tolerant of dumb folks. So that really amazed me. And I heard that from somebody who knows absolutely everything that's going on. And then next thing I knew [1971], I was an associate professor [of planetary science]. And then, a few years later [1976], I was a full professor. [Tape ends]

JAMES A. WESTPHAL**SESSION 3****July 17, 1998****Begin Tape 3, Side 1**

COHEN: I'd like to backtrack a little bit, because I want to pick up, in some detail, your turning into what I will call an astronomer—from an amateur astronomer. Maybe you can pick up with what happened after [your work with Bruce Murray].

WESTPHAL: OK. That was the first nonplanetary thing I did. It was great fun. And it was fascinating to me, because I'd already been here for four years. I ran into Maarten Schmidt [Francis L. Moseley Professor of Astronomy, emeritus], just by accident. And he had this little, tiny spectrum. It was a piece of glass about the size of my thumb. These were the little plates that came out of what was known as a nebular spectrograph down at Palomar. It was what everybody used who was interested in very faint things in real astronomy—that is, in far-away stuff. It had an optical design that had been done by Ike Bowen by hand. I remember going to Bowen's office and seeing his table full of pieces of paper with sixteen-digit numbers, all hand-done.

COHEN: Those were the heroic days.

WESTPHAL: Those were the heroic days. And, boy, it really gave the advantage to people who had intuition and didn't have to do all that stuff. [Laughter] At any rate, it ended up that to be able to make the kind of an image that was possible, you needed a very unusual optic material that was not something that you would normally think about building optics out of—namely, diamonds. The story, as I remember hearing it, came from Arthur Vaughan, then at Mount

Wilson, who was much closer to Ike than I was. Ike called up some diamond importer down in Los Angeles or Beverly Hills or wherever, and told this person what he needed, which was a piece of diamond about a centimeter long and about 5 millimeters wide and a couple of millimeters thick. This is not what you would imagine that some diamond dealer would have, but again, it was his intuition. The guy said, "I've got some scrap pieces around here kind of like that. I can't sell them for anything, but people like to see what a diamond is really like and they scratch glass with it. So I'll send some of these pieces out, and if any one of them works for you, that's wonderful. Then we can discuss how much it might cost, or not." So some pieces came out, and one of them was appropriate. Apparently Ike sent the others back. The story was that this guy said, "Oh, keep it, it's not worth anything." [Laughter] So buried in this nebular spectrograph is this diamond field flattener.

COHEN: Field flattener?

WESTPHAL: It's an optical element that lets you take a flat piece of glass and put a curved image on it by changing the optical path length in the right places so that it flattens out the image. And these images had to be on flat plates, because they were little, tiny glass plates. At any rate, I thought that was a wonderful thing. So this was a plate that Maarten had taken.

COHEN: He was just walking around with it in his hands?

WESTPHAL: Oh, yes. He was terribly proud of this thing, and properly so, it turns out. This was sometime very soon after he found out about quasars. On it was this wonderful little spectrum. It had only a single emission line in it. That makes it very difficult to get the red shift, because you've got to guess what the line is. But he thought there should be a broad line a little farther along the spectrum. And he thought he could maybe see this if he held the thing just right so the light would be just right. So I said, "Why don't we digitize this plate?" because Jim [James E.]

Gunn [professor of astronomy 1970-1980] had just finished building a little single-channel digitizer for those kinds of plates. For whatever Jim was doing at that point.

COHEN: Now, the idea of a digitizer was to be able to put the data on a computer?

WESTPHAL: To get it onto a computer, that's right. And the digitizer put out a punch-paper tape, of course, in those days. And that was converted into punch cards. Then I did a cross-correlation between the tracing of the image and a Gaussian curve as a smoothing function. And when we did that, it came out on a plot from the computer center. It was a nice curve. And here was this very big, broad band right at the wavelength Maarten said it ought to be, which was magnesium II. And Maarten was just tickled to death with this.

COHEN: That must have been sometime in the early sixties.

WESTPHAL: Well, the paper was dated 1965. ["Some Astronomical Applications of Cross-Correlation Techniques." *Astrophys. Jour.*, 142, 1661 (1965).] And it was my first real astronomical paper.

COHEN: And would that have been your first time working with Jim and Maarten?

WESTPHAL: Yes.

COHEN: OK, because the three of you went on to work a great deal together.

WESTPHAL: That's right, yes. So that was a fun thing. And a lot of people—many of the astronomers—were interested in it. This was a technique we had used in the oil industry [ever since computers came in], which was only five or six years before that. It was a cross-

fertilization of ideas. And that was my point, really—that I was kind of flabbergasted that these people didn't know all about this, because it's trivial stuff, especially nowadays. It's so totally trivial that you don't even think about it. Then there was a series of papers that was a collaboration. And then there were a bunch of papers I was involved in in real astronomy, but I didn't do much except help with the equipment—

COHEN: And lower the resistance to the research?

WESTPHAL: That's exactly right. Then [1966] came the paper with [Eric E.] Becklin, called, "Infrared Observations of Comet 1965f" [Ikeya-Seki] [*Astrophys. Jour.*, 145, 445 (1966)].

COHEN: This is the first time you mentioned Becklin. Was he a postdoc here, or a student?

WESTPHAL: No, he was Gerry [Neugebauer]'s graduate student. This was done in the thermal IR [infrared], from 2 microns to 20 microns.

COHEN: Was Gerry always just involved with infrared work?

WESTPHAL: Yes, pretty much. Well, the first thing he did when he came here, I guess, that took a huge amount of time, was that he was one of the editors of the Feynman Lectures. And that was an immense task.

This was a comet that had been already seen—comets are almost always discovered by amateurs. There were two Japanese who competed with each other to be the world's greatest comet finders. I think at least one of them is still around and still discovering comets. And very soon, people got enough of the orbit so that they could see that this thing was going to be a sun grazer, a very elliptical orbit. There had been, I don't know, four or five of these sun grazers before, spread back in history. As best people could tell, they were all in the same orbit. So

everybody concluded—I think quite accurately—that these were all pieces of a single comet that had broken up, and the tiny, little velocities of the fracture were enough to spread them out over all these years. So this was kind of an exciting thing. The prediction, several months before it actually went around the sun, was that it might be a pretty spectacular comet. Pretty soon somebody realized that it was going to stay essentially one astronomical unit—that is, one radius of the earth's orbit—away from the earth for almost its whole path. It doesn't seem as though that could be possible, but it turned out that the earth was moving in such a way and the comet was moving in such a way that it was always about the same distance from us. Well, that was neat, because that meant you had a lot of time to make observations—it was fairly predictable. So Eric [Becklin] and I suggested—I think we started out with Neugebauer—that we go up to Mount Wilson and use the equipment I had up there—

COHEN: From this other infrared work on the moon?

WESTPHAL: Yes, on this planetary stuff. And Eric had some equipment, which was on the other arm of the telescope, that he was using to do things like finding the center of the galaxy at 2 microns. The view, particularly when we talked to the real astronomers, was that we were out of our minds and just wasting our time, and that there was no way there was going to be any significant infrared energy from this comet—it was just a lot of dust and so forth, and it would be cold, and we were just not going to get anything. But of course, since we owned the telescope, why, we could still do it if we wanted to. And Gerry certainly didn't discourage us. So we went up as soon as we thought there was any chance of seeing the signal. And my God, there was a huge signal!

COHEN: In the infrared?

WESTPHAL: In the infrared. And this was first at 2 microns, the wavelength that Eric was doing. And then, within just two or three days, we went up each morning, because this thing was in the morning sky. And then when it passed behind the sun and came back, it was still in the morning sky—all an artifact of this strange coincidence of the orbit. It turned out that there was some 10-micron irradiation. So by the time that had happened, there was a whole lot of interest down here about this. [Laughter] So we started recruiting any student that would help us, because it would be thirty days, as I remember, before it got to the sun. And then we had five days, or something, when it was so close to the sun that we couldn't point the telescope toward it. And then there'd be another thirty days afterward. So that's a lot of going up to Mount Wilson at three o'clock in the morning and then coming back down here and going to school or doing your job or whatever. So we had kind of a crew. I think there were five or six or seven people.

COHEN: And these were mainly students?

WESTPHAL: They were mainly students. And either Eric or I was always there. We were able to show that the reason we could see the stuff in the thermal infrared was that this dust had a special property. And there's still an argument, which has been talked about till everybody's gotten bored with it, about how come this dust acts this way. It's very black, so it emits radiation very well. But it's also shiny somehow—"white" enough so that it absorbs a lot of sunlight. It's like laying a pair of pliers out in the sun and coming back in thirty minutes to pick them up and they almost burn your hand. It has to do just with the reflectivities and the various wavelengths. This dust had to be special stuff. So that became a big deal—we all got lots of brownie points for that.

COHEN: And you still didn't have your position as an associate professor. You were still a research engineer, or whatever they were calling you?

WESTPHAL: Well, I was a senior research fellow from '66 on. That was '66, and that may be how that came to be; I hadn't thought about that. One day, I happened to wander into the whole Neugebauer infrared army. At that time it was at the basement level of West Bridge. Leighton had his office down there at that time. And another person who was there, and who was another one of my heroes in the world, was Vic [Henry Victor] Neher [professor of physics, emeritus]. He was at the very west end of that hall, and he had a lab and an office right there. Right about this time he retired, and I inherited a massive supply of very pure nickel wire of many different kinds of diameters.

COHEN: You saw it was there, so you took it?

WESTPHAL: Oh, no. I asked Leighton if I could have it and he said, "Sure. If you're willing to store it, keep it. We'll remember where it is." So I was walking down the hall. I don't remember why I was there or anything about it. And there were Neugebauer and Becklin, and they had the strip chart. It was unrolled and lay all the way down the hall, right in the middle. And they were talking. I walked up and said, "What's going on?" Gerry said, "Eric found the center of the galaxy." And there was a beautiful big signature on the strip chart at 2 microns. Again, everybody had said that there was not going to be anything there. So we were all standing there talking about this. Leighton had come out and we were discussing what it meant. And along came Dick Feynman, purely by accident. Eric was very excited that he had done this thing. [Leighton] explained to him [Feynman] what it was about [instead of letting] Eric do it. But Feynman fixed that. He started talking to Eric. [Laughter]

I didn't know Dick well at all. I knew him well enough to call him Dick, but that was about all; we hadn't had a lot of interaction. I said, "Dick, what's special about the center of the galaxy? Why should we see something like this? Is there something special about that?" He was standing there and looking down at this [chart on the floor]. He said, "That's where God lives."

COHEN: [Laughter] Well, maybe that's right.

WESTPHAL: Maybe that's true. [Laughter] He probably would know better than me.

Let me go back to the nickel wire. There's a paper here called "Schlieren Technique for Studying Water Flow in Marine Animals." [Westphal, J. A., *Science*, 149, 1515 (1965)] This was a Lowenstam activity. There's a class of shellfish that are very ancient—some of the earliest shellfish around in the fossil record. They had a very strange structure inside the fossils—there are almost no modern ones. They're very rare, and Heinz probably had the world's supply of captive brachiopods. He was very anxious not to kill one of them. He wanted them all growing, because they were growing shell, which was what that whole business of having the cold room and all of that [was about]. He said, "Is there any way in the world we can trace how the water flows through this animal?" Nobody knew how it worked. I mean, you couldn't tell from the fossils, and he didn't want to dissect an animal. But he was very interested in whether this thing cycled water in and out, or back and forth, or what. It has two openings—it's a Shell Oil Company-shaped shell, vaguely, except that one half of it is flatter than the other. I thought about it a little bit and I said, "Well, it looks to me like we ought to be able to set up a Schlieren system." A Schlieren system is what is being used when you see pictures of bullets going through balloons and things like that—where you can see an explosive wave path coming. And it all has to do with just changing the index of refraction of the gas in that case. And since I was using that kind of technique for building telescope mirrors—that's the so-called Foucault test—why, I said, "Look, let's get some little piece of wire or something that we can heat up with an electrical current, and put that around—if the animal is suspended at a level off the bottom like that, we'll put this wire around, like that." This was a piece of Neher's nickel wire. The wire went around like this, in a vertical plane—in the same plane as the main body of the animal. And then we just set this thing up in a tank of seawater. And I could pulse the current and make a little ring of warm water, and then we could see it blow the water out. And we could

see that on one side it was sucking water in, and on the other side it was blowing it out. So that work got published in *Science*.

COHEN: Now, this was still in the period of time when you were doing some things with Lowenstam and some things with Bruce Murray? Or were you done working with Bruce Murray at that time?

WESTPHAL: Oh, no. I've never been done with Bruce Murray! And Heinz is gone now. The first grad student I had anything to do with directly in planetary science—whom I worked closely with—was Alex Goetz. Alex Goetz's father was an associate professor in physics here; he's the guy who invented the aerosol particle spectrometer used by smog monitors, and he had a big lab on the top floor of West Bridge [Norman Bridge Laboratory of Physics]. At any rate, it was Alex Goetz, and he did a thesis with equipment we built.

COHEN: And you supervised him directly?

WESTPHAL: Essentially I was his supervisor. Bruce [Murray] was his official supervisor, because I wasn't a faculty member then. But it was really an enterprise between Alex and me. He did spectroscopy in the infrared—way up in the deep infrared, which nobody had ever done in astronomy, including Bruce and me. He was trying to answer the question—independent of the question we were talking about earlier, about whether the dust was on the moon or not—of whether the moon was chemically differentiated, because he could take spectra in that wavelength where you'd see different mineral species, if there were, and so if there were big areas where there were more silicates than in other areas, why, that would be his thesis.

COHEN: Did you have to build special equipment for this?

WESTPHAL: Yes, we had to build a spectrometer. And we had to learn all sorts of crazy things neither of us knew. And every day it didn't work, we'd have to figure out why it didn't work that day. And tomorrow it'd find another way to fool you.

Along about this time I started working with Jerry [Jerome] Kristian and Allan Sandage [of the Carnegie Observatories]. The first paper was called "Rapid Photometric and Spectroscopic Variations of the X-ray Source Cygnus X-2." [*Astrophys. Jour.* 150, L99 (1967)] This again was a case where I was the guy who ran the equipment. Allan was the guy who decided what was good to do. And Jerry was the guy who was supposed to be analyzing the data.

The next [paper] is one that Gerry [Neugebauer] and I did together. It was called "Infrared Observations of Eta Carinae." [*Astrophys. Jour.* 152, L89 (1968)] It will probably be the next supernova close to us. It's this beautiful thing that looks like a dumbbell. You've seen the HST [Hubble Space Telescope] pictures of it. My favorite—

COHEN: Your favorite source?

WESTPHAL: I own that object. [Laughter] But what we learned from those observations, which had to be done in Chile, was that there was a tremendous amount of dust and stuff associated with it. It was, in fact, the brightest object in the infrared, after the sun and the moon.

COHEN: Already in the sixties there were telescopes in Chile to work at?

WESTPHAL: Oh, yes. This work was done in Tololo [Cerro Tololo Inter-American Observatory]. And it was done on the 60-inch, which belonged to Kitt Peak [National Observatory]. Gerry and I had gone down there to do a whole bunch of stuff, including following up on Eric's center-of-the-galaxy stuff, which would be right overhead down there. Soon after I came [to Caltech], and probably about the time I started working with Maarten,

Horace [Babcock] became director [of Mount Wilson and Palomar (1964-1978)]. And Horace was hell-bent on building a telescope in the Southern Hemisphere. Well, let me back up just a bit. Somebody at Carnegie—as far as I know it was only at Carnegie, but I don't know this; it seemed a little illogical for it to be only Carnegie—decided that they should try to build a sister to the 200-inch. So there started to be serious technical discussions about what you'd have to include in a 200-inch design to be able to use it in the Southern Hemisphere. And I began working with Bruce Rule, who was the chief engineer at Caltech in those years. It was decided finally, after he had a bunch of studies done, that all you had to do was make the tracking clock motor turn in the other direction and build another one. But a big problem was that none of the detailed drawings for the 200-inch existed anymore.

COHEN: They were just gone?

WESTPHAL: Well, Westinghouse had built the mounting, and this was for the mounting. And for reasons unknown—I guess nobody dreamed anybody would ever want to build another one—why, Caltech didn't capture the drawings, which it could have, I'm sure. All that Caltech got were some old assembly drawings, so they could see how all the pieces fit together. Everybody was very concerned about how in the world we really would do this if somebody gave us some money. Well, the next thing we heard—this had to be early in 1963—was that a proposal had been made to the Ford Foundation to do this, and this was really going to happen.

COHEN: Was it going to be just a Carnegie effort?

WESTPHAL: Well, that subtlety I don't know. In those days—

COHEN: Caltech and Carnegie were still supposedly getting along?

WESTPHAL: Well, they were, to some extent. And certainly if Carnegie wanted to do that, why, they'd have to cope with Caltech somehow, because Caltech had the drawings. At any rate, since I had worked in Mexico, I knew a little Spanish, which was rapidly dissipating. Bruce didn't know a word of Spanish, so he asked me if I would go with him down to Chile. And I, of course, was tickled to death to do that. It sounded like a real adventure. So Bruce and I went down. The word was that maybe even while we were down there, the Ford Foundation was going to sign this thing up. It was soon after Kennedy was assassinated, in November '63. The first thing that happened after everything settled down was that McGeorge Bundy became president of the Ford Foundation. So Bruce and I were down there after all of this. Bruce sometimes worked at a pretty high level. The first thing I knew, I was being introduced to the president of Chile, Eduardo Frei, who was a chemist from Havana who spoke perfect English.

COHEN: So much for your Spanish.

WESTPHAL: Yes, right. Well, thank God. [Laughter] And the questions were logistic questions: Could we get a boat big enough with a telescope on it anywhere near the site, which hadn't been chosen as yet. Bruce knew a lot about that part of it—about how much the thing could weigh, and how many pieces there would be, and all that kind of stuff. So as soon as that subject opened up, Frei said, "What kind of help from us do you need?" And Bruce said, "We need some kind of a crane, preferably a floating crane, that can pick this heavy thing up out of the boat and set it down on whatever." Because if we didn't get it off the boat, why then, it made a huge difference in whether or not we'd have to cut it in small pieces. So there was a quick conversation between Frei and an orderly in Spanish. It was much too quick for me, but clearly he had sent him off to get somebody. And Frei turned to us and said, "We have three floating cranes. One of them is on the bottom of the sea floor off of Antofagasta, one of them is sinking slowly in Viña del Mar, and one of them is down in Concepción and we think it's a good one. Maybe it will survive the trip to La Serena, or up to Coquimbo." So this went on and on and on.

And that led to me being involved seriously in the site survey in Chile, in anticipation of building a telescope down there. But indeed, when Bruce and I were down there on this very first trip, the word came to us that McGeorge Bundy had stopped all grants and everything, and he was in fact very much more interested in [funding] direct television satellites for India. And of course, that's what's happening. This was a way to try to bring education to the masses.

COHEN: Bundy was interested in education.

WESTPHAL: Yes, that's right. I think it was a noble cause. I wouldn't argue for a second that that was not a better thing to do than build a 200-inch telescope in Chile. Some of my colleagues didn't think so.

So there was a whole series of trips over several years where I helped the folks in Carnegie to put up towers with very sensitive thermal sensors on them and to do site surveys and to measure the water vapor in the air and see how good it was for infrared astronomy.

COHEN: But ultimately they did get the money to build a telescope, didn't they?

WESTPHAL: Yes, but they got it internally.

COHEN: Ah, Carnegie gave the money for that?

WESTPHAL: Well, I don't know the subtleties of it. They first built a 60-inch, and that money came from—I can't remember her name, but she was a quite famous woman astronomer at Santa Barbara Street [Henrietta Hill Swope]. And then later they built a 100-inch telescope. So I was involved in that. And I inevitably got involved with the Kitt Peak folks and the Tololo folks. And Horace [Babcock] had set up a facility on a mountain called Morado, which was a nice flat-topped mountain like Palomar is. It's the next mountain south of Tololo, which is a very sharp-

peaked mountain. And there was a big argument between Horace and the people associated with the site survey for Tololo as to whether you should build your telescope on a peaky mountain that's so small at the top you couldn't get your telescope on it, or you should do it "right like Palomar," where you have all the space in the world. Well, that was a technical argument you never could get enough data for. I spent a lot of time doing that sort of stuff, and it fit right into Bob Sharp's view. I mean, if he hadn't expanded to say, "I don't even care if it's at Caltech," I probably—

COHEN: He just wanted to get more astronomy into his program.

WESTPHAL: He just wanted to get good stuff going anywhere he could.

COHEN: So you spent a lot of time there. But from your oil days you were used to being away from your family?

WESTPHAL: Oh sure. And then as soon as Tololo got on the air with the telescopes, Gerry and I started doing observations down there—and also Tom [Thomas B.] McCord, who had been a student of ours. There are some papers here with his name on them having to do with Mars. There was an especially close opposition of Mars in 1969. During the week that it was the closest to Earth, the wind blew eighty miles an hour night and day at Tololo. And on the night it was going to come the closest, the director of Tololo decided, "To hell with this. We're going to look at Mars." And so he got everybody who wanted to go up on Tololo in the dome, turned the dome around—closed—to the right position, set the telescope to where Mars was, waited until the wind was blowing in the opposite direction, and opened the dome up. Somebody—I think it was Tom—was looking in the eyepiece. He focused the telescope right quick and then everybody got twenty seconds to look. [Laughter]

COHEN: You got something though?

WESTPHAL: Oh, it was just sightseeing, pure sightseeing. No science. But I thought it was a neat thing.

Here is the business with the electronic detectors.

COHEN: OK, that's quite interesting.

WESTPHAL: That came about in a fairly convoluted way, as [these] things always do. The first paper is McCord and I: "A Silicon Vidicon Photometer." It was presented at an Advanced Electronic Systems for Astronomy meeting, at a conference in Santa Cruz in '71 [*Pub. A. S. P.* 85 (1972)].

COHEN: Silicon sensors. Is that what we're talking about?

WESTPHAL: Yes, silicon sensors.

COHEN: And you developed those for Palomar?

WESTPHAL: Yes, we used them to do planetary science with, essentially. This was the path headed toward CCDs [charge-coupled devices], and it was the path toward the way modern astronomy is done now. Tom McCord, who by then was at MIT, met somebody at some cocktail party, or meeting, or who knows what, who worked for a company called Bellcomm, a splinter of Bell Telephone. [Tape ends]

Begin Tape 3, Side 2

WESTPHAL: We were just speaking about how we got started with silicon detectors for astronomy at Caltech. And this man—his name is Noel Hinners—later on was a big gun in NASA. He's now a great big gun in Martin Marietta. He's originally a Caltecher—a Caltech PhD. He was a very important guy in a lot of things that went on—things that most people don't know anything about, as often is the case. He told McCord that there was somebody at Bell Telephone who had a vidicon, which is a vacuum-tube type thing with a silicon target inside. They had developed this at Bell Telephone. It was a horrible experience. They had spent many dollars—millions, no doubt—trying to develop a picturephone that they could put on people's desks.

COHEN: Oh, so people could see who they were talking to?

WESTPHAL: Yes. It's something you can do now with almost any computer, but only in the last two years can you do it. [Laughter] They had built this system and they had tested it in the lab. Everything worked great, so they decided to make a field test. They made one, I believe it was somewhere in Ohio. They installed in an office building some fairly large number—like ten—of these things, and finished with it Friday afternoon. So on Monday morning they were going to turn all this on and be huge heroes. Monday morning came, all of these phones were turned on, and all but one of them had a great big white stripe right across the picture. And [the Bell people] could not figure out what the hell was going on. They had happened to put these things on the sunny side of the building, looking out through the window, because there was something interesting to see out there, and the sun came up and burned the stripe right across the target of this vidicon.

COHEN: The tenth one was not facing the sun?

WESTPHAL: Right, it wasn't looking at the sun. So that made them realize that if there was any way in the world that this could be made not to work, people would find out how to do it. Therefore you had to build something that could stand to have an image of the sun on it indefinitely without cooling or anything funny. So they developed this silicon-target vidicon, which was a thing that would allow them to do this.

COHEN: They did a lot of good things at Bell Labs.

WESTPHAL: Do you realize that essentially all fundamental things in electronics and in information theory came from Bell Labs in 1927, 1928, or 1929? Somebody ought to really do a history of that before it's too late, and it's rapidly becoming too late. It's really a fascinating story, which will, I'm sure, be written by somebody.

So we said, "Gee, that would be a very interesting thing to us," because immediately we understood its properties, which would be that it was a linear device. That is, if you put in fifty photons, you'd get a certain signal, and if you put in five hundred photons you'd get a ten times bigger signal. Regular vidicons, like the kind they were burning up, don't have that [property] at all. They have a curve that flattens out at the top, and that makes it miserable to do photometry, or real measurements, with. So our ears got this big. And Hinners said, "Now, remember, this thing is just silicon, and it's not going to be the most sensitive device you've ever seen in your life before." And we said, "We've got big, bright planets out there. We don't have problems with that." [Laughter] So Tom followed this up and we got an appointment to see the guys who were actually building this thing. He got to see some of them work. Weeks later, I met Tom at Murray Hill [New Jersey, Bell Labs headquarters] and we went to the lab. We were given the royal treatment. They were so excited that somebody would use this thing for something that was "worthwhile" [laughter], that they actually gave us three or four of these things. I still have one of them; I noticed it the other day in my storage. But we didn't know how to run them, and

Bell Telephone wasn't running them in a slow readout mode—they were running them at a standard TV rate. So their electronics really weren't much help to us. But by then I had a contact up at JPL [Jet Propulsion Laboratory], by pure accident. It was Gary Bailey, the guy who was the electronicer responsible for the vidicons that were on the *Mariner* spacecraft, and he knew exactly how to make these things run.

COHEN: Was this project your first contact with JPL?

WESTPHAL: Probably at the technical level—I would imagine so. So we said, “Gee, can you teach us or show us how to make it go?” He said, “Yes, but you’ve got three of them, right?” And I said, “Yes, we’ve got three of them.” He said, “I want one of them.” [Laughter] It was a little barter job here. So that’s the way it was. We gave him one. And of course that was the best thing we ever did, because he got it working long before we got ours [working]. All we had to do was just copy what he was doing. And he knew that, of course, too. There’s a 1972 paper, “Silicon-Vidicon Astronomical Photometry,” [McCord, T. B., and J. A. Westphal, “A Two-Dimensional Silicon Vidicon Astronomical Photometer.” *Applied Optics*, 11, 522 (1972)] and I put my name on that one. And then sometime later there was another one with Gary Bailey’s name on it.

Now, I’m going to jump back quickly for just one other thing here, having to do with [Heinz] Lowenstam. There are lots of papers having to do with things that were going on in which I was just a partner to the act. Here’s a paper with Becklin and a whole list of graduate students called “Infrared Diameter of IRC+10216 Determined from Lunar Occultations.” [Toombs, R. I., et al., *Astrophys. Jour. Letters*, 173, L71-L74 (1972)] Actually, this was my enterprise. Do you know the radio technique of measuring things by what goes in front: occultation?

COHEN: Occultation? Yes, I do.

WESTPHAL: Well, this was the same exact idea. On an object that was the first really neat thing found in the Leighton-Neugebauer-Marx sky survey at Mount Wilson, where Ike [Bowen] and everybody else had said that the survey was never going to find any—

COHEN: This was their infrared survey?

WESTPHAL: Yeah, their infrared survey. And this object was found on the second night or something. You could barely see this object in the visible well enough to get a spectrum of it with the 200-inch to find out what the hell kind of a thing it was. It was a star buried in a lot of dust, of course. But that was fun, because somebody had to ride up in the prime-focus cage, and we had to do a whole lot of mucking around and bollix up on the 200-inch. And all of this happens whether you're ready or not. [Laughter]

COHEN: The occultation happens?

WESTPHAL: That's right. Anyway, it worked. So we could get a diameter of this object, and that was fun. And then there were a lot more papers, on planetary things—here's a paper with Fred Gillett, who was at that time at Kitt Peak, as a collaborator [“Observations of 7.9-Micron Limb Brightening on Jupiter.” *Astrophys. Jour.*, 179, L153-L154 (1973)]. Here's one with Keith Matthews and my first graduate student, Rich Terrile—he did his thesis on 5-micron emission from Jupiter [“5-Micron Pictures of Jupiter.” *Astrophys. Jour. Letters*, 188, L111 (1974)].

COHEN: By this time you had your appointment as professor?

WESTPHAL: Associate professor. Terrile was, in a sense, my first formal graduate student; he was the first grad student I was entirely responsible for. A couple of years earlier than that, I had

just been lucky. Lots of luck. In 1969 I started a program on the 200-inch of measuring the actual profile of the light coming from planets—so-called limb-darkening measurements. Because if the planets have an atmosphere, then the nature of the intensity curve you'd get as you measure—the profile of that light—tells you something about the nature of the atmosphere. And nobody had ever done that, because nobody had had the hardware to do it and certainly nobody had had a 200-inch to do it. One of the first things I discovered was that there were dark spots on Jupiter—dark things that oftentimes were kind of finger-shaped and at times round with a little white dot in the middle. And I found out that there was a tremendous amount of 5-micron light coming out through those holes. I actually measured temperatures as high as room temperature in some of these holes. Nobody much believed this at that time, but that's what the data showed, and I published the data. "Don't confuse me with the facts, my mind's made up." So that led into Rich's thesis on that subject. And recently there was a probe that went into Jupiter. It happened, by pure chance, to go into one of those holes. You'd think, "That's neat." It is, but it caused a terrible problem, because everybody was predicting how much water there was going to be as a function of altitude. Everybody was assuming it was a regular planet and not a special place. It had way less water than it should have. And there was a lot of thrashing around and revisiting of this whole business again before everybody agreed that that's a place where there aren't any clouds and therefore it's probably dry." This was the *Galileo* probe.

COHEN: So it was just happenstance that it ran into one of those holes?

WESTPHAL: If they had tried to hit it—

COHEN: Or the hole that it hit was pretty big?

WESTPHAL: Well, they're big, yes. They're bigger than the United States. But Jupiter is a big place. If they had tried to hit it, there would have been no way in the world that they could have.

COHEN: When you were doing these things, were you talking very much to the astronomers?

WESTPHAL: Well, I was talking mainly to the infrared people, which was Neugebauer and company.

COHEN: And the physics department?

WESTPHAL: Yes, and then of course to my own colleagues in planetary science. Most of my colleagues are people who do what is really modern nowadays, and that is sit at computer terminals making models. The only other planetary person who was an observationist was Dewey [Duane O.] Muhleman [professor of planetary science], and he and I talked a lot—you know, crying in each other's beer. And then there were some papers with Peter Young. We first looked for black holes, and first published a paper having to do with the M87 black hole. [Young, P., Westphal, J. A., Kristian, J., Wilson, C., and Landauer, F., "Evidence for a Supermassive Object in the Nucleus of the Galaxy M87." *Astrophys. Jour.*, 221, 721-730 (1978).] These were measurements we made with CCDs by then.

COHEN: Do you want to talk a little bit about the CCDs? Because that's a good story, too.

WESTPHAL: The CCD thing grew out of our interest in two-dimensional photometry. The evolution of the silicon vidicon, the natural evolution of that, was that it went into industry—well, Bell Telephone turned it loose. So RCA started building the things, and then RCA decided that what they really needed was an image intensifier in the front of it....As soon as the CCDs appeared, we got one of them, because they seemed to have the potential to be able to have almost ultimate sensitivity—that is, you could count the individual photons as they were collected on this silicon target. We never got quite that good; we got down to 2.5 photons or

something. But it was a wondrous device. It used high-voltage intensifiers, so you could adjust that voltage and control the sensitivity of the system. It was a vacuum tube about 6 or 8 inches long and about 2 inches in diameter. You had to cool it down in a cooler, but it all fit very nicely in the prime focus of the 200-inch.

COHEN: Where had you heard about it?

WESTPHAL: As soon as RCA started building these things, it was in every magazine you saw. And they became—and I guess are today to some extent—still the detector of choice for surveillance for parking lots and stuff like that. And by then we knew the people at RCA, having worked with them on this old vidicon, because the ones we got from Bell Telephone actually never were good for us. For their purposes it didn't matter, but for our purpose the filament inside, which generated the electron, wasn't shielded. So the thing was illuminated by the light from the filament inside the tube. But we learned how to make them work that way. Jerry Kristian and I were doing that. We were still getting help from Gary Bailey up at JPL. We built an electronic rack that would plug into the plate in the prime-focus cage, made to hold various electronics. So we could just roll that thing out on the 200-inch prime-focus elevator and use the hoist in front of the telescope. It had a little TV, a little commercial 5-inch TV set, so you could see what you were looking at. That led to all sorts of funny things happening. But when you're developing something like that, you don't want to use up good telescope time. So the directors through the years have always been very sympathetic about that and they'd give you twilight time, with the agreement that whoever had the whole night could say, "No, I don't want those guys out here." But it meant that you had maybe thirty minutes of a decent dark sky. You can do an awful lot in thirty minutes, if you're all ready to go and you've got everything that you need working. So we were in that situation. The first time that we were really going to look at something, Jerry brought along a bunch of Polaroid Schmidt plates for a field that he thought would be a fun place to look. As soon as it got dark enough, he took a picture of this spot.

There were just stars, I recall. And Jerry was looking at this two-piece screen and he had this Polaroid. We had forgotten to make sure that the TV was oriented the same way the Polaroids were going to be oriented, so we had to look through the back of it, which was OK—common problem, common solution. He couldn't identify the star field. He asked Tuton—Gary Tuton was the night assistant that particular night—“You got this damn telescope pointed right?” And Tuton says, “I put it right where you told me to.” So Jerry looked through it and said, “This isn't the right place.” I said, “What are the chances that we're seeing stars that are not on your picture?” And he said, “Oh, no. No way. That's not possible.” And I said, “Well, why don't we turn the high voltage down until we get something that is about the same exposure?” And when we did that, why, it fit his picture perfectly.

COHEN: So you were seeing a lot of stuff that you had not seen before.

WESTPHAL: Deeper in the sky than anybody had ever seen before.

COHEN: I can just imagine the excitement of that moment.

WESTPHAL: Jerry said, “I peed in my pants, I was so excited.” [Laughter]

COHEN: The magic of electronics.

WESTPHAL: The magic of electronics—science in action. By today's standards, it had lots of problems. But, boy, for the first time, it was so infinitely better than anything we'd seen.

COHEN: Was this the first time that something like this had been put on a telescope?

WESTPHAL: To my knowledge. We were kind of in at the ground floor, thanks to this pure accident of Tom McCord meeting some guy at some sort of a party and getting this connection.

COHEN: And now no telescope is built without [these detectors], is it?

WESTPHAL: Well, we don't use that kind anymore. This was before CCDs, you see, but this was the path toward them, because now we had the little silicon chip, first in the silicon-target vidicon without the intensifier, and then [with] the intensifier. The next obvious step was to make it a free-standing thing, and that was a big fat step for electronics and in the construction of these devices. Tricky, tricky business with all sorts of problems. I was first aware of these devices in '73 or something like that. There was a meeting called by NASA's chief program person for astronomy. She was a very good astronomer—Nancy Roman. Almost everybody knew Nancy Roman. She called this meeting in anticipation of a space telescope to see what's out there that might conceivably replace the Princeton vidicon—this thing they had been spending a huge amount of money on up at Westinghouse. I went to that meeting and there was a guy from Bell Telephone there talking about the CCD. And I remember vividly thinking to myself, "With that kind of performance, we'll never be interested in that thing." Two years later the performance was such that—

COHEN: They had improved it somewhat.

WESTPHAL: [Laughter] A bunch. A whole bunch.

COHEN: What's interesting here—for the record—is that you were working with Jerry and you were really working very freely with other people from Carnegie. Did you have a sense that they were in that organization and you were in this organization?

WESTPHAL: No.

COHEN: It just went very smoothly?

WESTPHAL: Yes, it did. I was a resource for them. I could produce things that they couldn't handily produce. And I had this freedom to do whatever fun thing I wanted to do that seemed useful. At any rate, that device we started using like mad.

COHEN: Was Jim Gunn working with you yet?

WESTPHAL: No. He was on the scene, but he was in the midst of being a professor and getting started, I guess. I don't know the details of timing in his career, but he had been a student here and then, I think, he went away. I think he came back here from the army [1970], but he hadn't been at JPL very long as an army person before he came down here. So anyway, we learned that these devices existed. And by now I had a contact working within the inner workings of JPL in their imaging world—this guy Gary Bailey. I soon became a friend and a buddy of his boss, Fred Landauer. And my wife became a good buddy of Fred's wife. Then—less than a year later, probably—Fred quit JPL. He had had enough of this high-level place, and he went off to Menlo Park to run a rope factory that his brother owned. They made specialty ropes of various sorts.

COHEN: Industrial ropes?

WESTPHAL: Industrial ropes. But Fred was terribly important in this, as you'll see in a minute. So I was aware of what JPL was doing, and then I heard from Fred that they had a contract with Texas Instruments to try to build CCDs to be used in the *Galileo* spacecraft. It was at that time called JOP, for Jupiter Orbiter Probe. They had recognized that that was almost an ideal device

to have in a spacecraft, because it didn't take any power; for most of the kinds of things they were doing, they could cool it with just radiation cooling from the spacecraft. It would be a really wondrous device, which, of course, it turned out to be. The person who was the brains behind that idea, of going to Texas Instruments, was Jerry Smith [Gerald M. Smith]—our very own Keck tech, Jerry Smith. I talked to him about it, of course. He did that.

COHEN: Do you mean improving the performance?

WESTPHAL: Well, getting CCDs that would be usable in a spacecraft. It's very different: We can tolerate junk on the ground, because you can always fix things or get around them or something, but on a spacecraft everything has to be right, and it has to stay that way. The way I actually came to know about this was when Gerry Wasserburg was made the chairman of the National Academy subcommittee called COMPLEX, which stands for "Committee on Planetary and Lunar Exploration." The committee was populated by some pretty high-level folks: Jim Van Allen of the Van Allen Belt, Al [Alastair G. W.] Cameron, who was a planetary theorist from Harvard. At any rate, a bunch of really big guys. A committee like that, like most advisory committees, can't make something happen, but they can kill something really easily, and you need to realize that that's the case. It's cheaper, from the viewpoint of the user, if they say, "Kill that project." Somebody out there will want to do something else, always. At any rate, there was a guy from Princeton whose name was [Robert] Danielson. He was a planetary type, and he was the planetary person for COMPLEX—the instrumentation-type person, a very, very able guy. He had some very severe illness just at this time, and he died. So Wasserburg asked me if I would replace him on the committee. I said, "I don't know what I'd do on the committee, but if you want me to come sit there, I'll sit there and listen and try to be responsive if somebody asks me a question I can answer," [laughter] "but I don't think this will be a very useful thing." And he said, "Come on." So the committee, very soon after that, had a meeting at JPL. One of the things that committee does, I guess to this day, is that it conducts reviews on scientific, and to

some extent technical, levels on future NASA programs—programs that are being generated at NASA. And the big thing at that moment was JOP—the mission now known as Galileo. So it was being described at this meeting. Gerry was very anxious that I should be there. But before that subject came up—that was an afternoon agenda item—on the morning agenda there was a discussion about various ways to learn more about Mars. The military had let it be known that there existed—classified at some level—things called penetrators, which were cylindrical devices about three or four feet long that they dropped out of an airplane in the Vietnam jungle. And each one had a radio transmitter and microphones on it. And they claimed to be able to recognize the detailed sound signatures of individual trucks coming out of North Vietnam and going down to South Vietnam. They could tell you how fast the trucks were bringing materials in and so forth. And this was a very effective undertaking. It was a lot better than having some guy sit in a tree wondering if he was going to get shot. So the idea had occurred to some people at NASA who knew about this—I had never heard of this thing—to drop an instrumented penetrator onto the surface of Mars. It would be able to measure all kinds of interesting things. We could raise the antenna on the outside, have a little pole in it, and measure the wind velocity just like that little kiddie car did, the Pathfinder. One of the big things that they wanted to measure—we still want to measure it—is the heat flow coming out of Mars, because that tells you something very basic about the internal structure of Mars. So there was a guy from Ames [Research Center] making a presentation about this penetrator. In the penetrator was also a seismometer and some alpha chemistry stuff. And he then started talking about how they were going to do this heat flow measurement. Well, he had had an idea about how to do that that was just absolute madness. I knew something about that from my oil days. If you want to get a sample of something on the side wall of an oil well, there is a very well-defined way to do that. You shoot a bullet—literally—it's the old gun with the bend in it. You shoot a bullet which is hollow and made out of hardened tungsten or something. You make it bend around the corner and go into the side wall and then it has a string on it and you pull the thing back out and you've got a sample. This guy had never heard of such a thing. He said, "I don't know how to do it.

I'm interested in the problem." And I said, "I don't think the way you want to do it is going to work at all, but there's an easy way to do it." And I described it. Well, this guy clearly didn't like this. And I noticed in the back of the room—this was in the main conference room at JPL—a whole row of guys in suits. At any rate, there was a coffee break. Wasserburg went away to talk to two of the suits. He came back to where I was and said, "Come on, Westphal." I thought, "What have I done?" He said, "Do you know what you just did?" And I said, "No, probably something terrible." He said, "No. It isn't what you did that was terrible. It's what you triggered. Do you see the guys in the back of the room with the suits?" And I said, "Yes." And he said, "One of those guys is the chief guy from NASA headquarters, and he just took the penetrator job away from Ames and gave it to JPL, because clearly Ames didn't know what they were doing. All of this was based on your comment. You damned well better be right about that." [Laughter] And he said, "You know, you've got to do your homework. And if you do your homework in the NASA world you'll always be ahead of the game, because the NASA people don't do their homework. This is not because they don't realize they need to—it's because they haven't got time to do the homework." And, boy, was that ever true, because I saw NASA people with goodwill, smart people—you hear about the bureaucrats that go home at 4:30 and that kind of stuff?—I've never seen such a hardworking bunch of people in my life. They'd sit in these damned meetings all day long and then they, the NASA people, would meet for dinner together and they'd be up till midnight figuring out what to do the next day.

So my time on the Hubble was a lot smoother than it would have been if I hadn't had that kind of experience. But as part of that same meeting, we did hear about the CCD instruments that were being made at JPL. And it was a tremendous delight. It was such a pleasure to be around Jim Van Allen especially. Wasserburg sat Jim Van Allen right next to me, and that was really a neat experience.

COHEN: Well, they were lucky to have you, Jim.

WESTPHAL: It certainly was great fun, and very educational. [Tape ends]

JAMES A. WESTPHAL**SESSION 4****July 23, 1998****Begin Tape 4, Side 1**

COHEN: I thought we might backtrack a little bit before we get to the [Hubble] space telescope, because you've spoken mostly about your astronomy project, and I know other things were going on during this time. So maybe we can talk geology first.

WESTPHAL: OK. Let me broaden the word "geology" to include the stuff Heinz Lowenstam was doing, because this was one of the things he was very anxious for me to do at the time the division was making a decision about whether to hire me or not. He had an ONR [Office of Naval Research] grant. That was where all the money was in those days. The navy was apparently very interested in putting some sort of electronics in something—they never went into detail about it, but it was something that was going to be put on the bottom of the ocean at deep depths, not just offshore a little way—some sort of sonar thing that would put out sound or listen to sound, or whatever, or both. And their concern about it was animals burrowing on the outside of it. Well, Heinz was extremely interested in how animals change their hard shells. All the animals we are talking about are ones that end up being shell-based; it could be tube worms—you might say that's not a shell, but anything that makes calcium carbonate. So he was very interested in the question of how that evolves: the stable-isotope chemistry of those shells as a function of temperature and as a function of pressure and growth rate and all the kinds of obvious things, with the hope that he could utilize the animals. Remember, we talked before about how we generated a whole room that was essentially one temperature and held a very large number of aquariums. That was the low-atmospheric-pressure part of this task.

COHEN: Now, you had done that work with him already—with the flow of the water through the animal and all that?

WESTPHAL: Yes, we're really going back quite a bit here. But we won't be back there very long. So the navy found out somehow that he was very interested in this. They were very interested in this. So suddenly he had a big grant. I wasn't involved ever, in those days, in who got grants from whom and how much money was being spent. I was just a person being paid to do fun stuff and good things. So he was very anxious for me to build a high-pressure aquarium, something that he could raise animals in under high pressure and otherwise controlled conditions.

COHEN: It would simulate the pressure at the bottom of the ocean?

WESTPHAL: It would simulate various depths in the ocean, including as deep as you could go, which is 36,000 feet or something. I had had a little experience in the oil industry with certain kinds of pumps; this allowed us to pretty easily use pumps we could buy locally. So we set up such an aquarium, and then the next step was to populate the aquarium with animals that came from those kinds of depths. Caltech was a partner with USC [University of Southern California] and UCLA [University of California at Los Angeles] on an oceanographic vessel called the *Volero IV*. It had various dredging capabilities. So the idea was that after we got the aquarium going we would start dredging....and it's a blind thing. In those days you didn't have televisions that showed you what you were looking at; you'd just drag a dredge along and pull it up after a while, and you took what you got. So there were many mysteries still at that time about how animals could be brought from very deep depths up to the surface and survive for any time at all. These were really basic questions we were able to solve rather quickly this way. We were able to get animals from 8,000 feet, about 200 miles offshore.

COHEN: Where did you keep them? Did you have some kind of pressurized situation until you brought them back into lab?

WESTPHAL: We didn't get quite that far. But we learned that the really important thing is that they have to be kept cold. What kills most animals when they come up to the surface from the depths is the heat—the water's too hot. And we learned that rather quickly, just by accident. We had a big dredge full of animals—we hit a gold mine down there of animals. And the dredge was full of mud, essentially. The ones that were on the outside of the dredge died quickly; the ones down in the middle of it, where it was still cold, were OK. We quickly realized what that meant.

At any rate, there came a time when if we were going to do what the navy wanted us to do, we needed to use some kind of submersible so we could see what we were doing. And at that time the bathysphere *Trieste*—somehow the navy owned that, or at least controlled it—was based in San Diego. So we had a visit from the navy people and we were talking about what went on, and one of them said, “Hey, why don't we put these guys on the *Trieste* and let them go down there and get whatever they want?” It seemed like an obvious thing to do, and it was certainly fun and exciting to contemplate. So then there were issues about how we could get the animals, and how we could get them into the aquariums, and how we could screw the lids on the aquarium—the aquarium was a cylinder about eighteen inches long with caps on each end. Well, once again my oil industry experience [came in]. I had solved that problem trivially. If for example you cut a thread on the end of a piece of pipe, and you cut it with a taper like this so that the male threads are like this and it mates into a tapered hole in the next piece of pipe, you can just turn it one turn and engage all the threads at once. That was something we could clearly do. So I was in San Diego talking with the engineering people, and they hadn't thought of that idea. They immediately said, “That will work just great, because it's very easy with our actuators to turn maybe three or four turns. We thought maybe you wanted to turn fifty turns on a thing that—”

COHEN: Where was this pipe? This was on the bathysphere itself?

WESTPHAL: The aquarium would be on the outside of the bathysphere.

COHEN: OK. And the bathysphere, where you would be, was pressurized and all that?

WESTPHAL: Yes. It was just a balloon, essentially, in which instead of helium it had gasoline, which is lighter than water. Remember, this all started with [Auguste] Piccard, a Frenchman who was doing all sorts of high-altitude balloon work. I don't know the details, or whether bathyspheres were his idea or not. But at any rate, that's how the thing worked.

COHEN: I see. How many people were in it.

WESTPHAL: Four, as I remember.

COHEN: And then it would be submerged?

WESTPHAL: It would be towed out to someplace we decided was a good place—based on almost nothing, in those days. And then down it would go, with somebody inside who knew what he was doing. And of course both Heinz and I thought that we ought to be at least two of the people aboard—me to run the machine that screwed the lid on the aquarium and him to [choose the] animals.

COHEN: How did you go down? What forced you down?

WESTPHAL: Just like a balloon, it has lead weights, so it has to be supported with floats. And then when you're ready to go down, it's like a submarine: you fill tanks with seawater. And there are more lead weights on the bottom. For safety purposes, those are arranged so that if something happens and the vessel doesn't want to come up, you can disconnect it from those lead weights.

COHEN: Tell me about the pipe.

WESTPHAL: Well, the aquarium was like a pipe. It was about eighteen inches long and it had very thick walls. It was made of a special kind of stainless steel called 316 stainless, which corrodes the least, particularly in seawater. It was strapped on the outside [of the bathysphere] in a place where a mechanical arm would be. It was not operated by me; it was operated by an arm operator, and on the end of the arm was the cap that was going to seal it up. And then Heinz would—

COHEN: Say, "I want that animal"?

WESTPHAL: They would then scoop up this animal with a scoop thing, dump it into the end of the tube, and then screw the lid on, and all we had to do was make one turn [of the screw]. One turn was all you needed, because of the kind of a thread we had on the end of it. So I made two or three trips down to San Diego, discussing exactly how to do this. And then we built pieces of hardware to fit onto it, and everything was just fine; we were ahead of schedule, and we were going to do a nearby run first, just to find out if everything was—you know, OK to go down to 10,000 feet or someplace a little deeper than we'd been.

COHEN: What year was this?

WESTPHAL: In the mid-sixties. There was a disaster around then [1963] in which a US nuclear submarine with hydrogen bombs on it sank out in the middle of the Atlantic. It was called the *Thresher*. Of course, the whole military just went completely out of its mind over that. They had to get down there first to make damned sure the Russians didn't get there first and find out something. There was no hope for the crew or anything like that. It was in very deep water—totally beyond saving people. So the only way in those days to get down to it was with the *Trieste*.

COHEN: They took it away from you? There was just one?

WESTPHAL: There was just one. And they put it on a military transport plane and flew it to the East Coast and took it out there, and it was successful. They did whatever it was they wanted to do, and who knows what that was, because the *Trieste* didn't have an awful lot of capability to do much. But maybe they just wanted to blow the sub up. I mean, if they really wanted to make sure nobody could learn anything from it, then probably the safest thing to do—with hydrogen bombs on board, too—was not to blow it up [so much as to] blow it all to pieces, and then the seawater would eat it all up over the course of time.

COHEN: You suspect that that's what happened?

WESTPHAL: We only suspected; we never knew. Nobody talked about this at all. I mean, we knew the *Trieste* went away, and it didn't take long to imagine [what was happening], particularly since Heinz and I saw pictures of it being loaded off the airplane on the East Coast. Anyway, so that was the end of that neat thing, because the navy never brought it back. So the whole thing collapsed, which was a dirty shame, but it was fun.

Then in 1964—just at election time, when Johnson was running against Goldwater—Heinz and I took a trip out to the South Pacific. Heinz knew of a place in the South Pacific, out

beyond Guam, called Palau. There was, in fact, a PBS program on TV the other night about Palau and these notched-in islands. Palau was an island that the Japanese controlled from World War I to World War II; it was a League of Nations kind of thing. And there had been a Japanese oceanographer from Sandai University in Japan who was a resident there. Every morning he went out to the end of the pier that ran out into the lagoon a good distance, and he took a water sample off the end of the pier and measured its temperature, and then came back to the lab and measured the chemistry—the salinity, and a little bit more sophisticated chemistry than just that. He had these careful records, and Heinz found out that the temperature over the period of about twenty or thirty years had not changed enough to show on the thermometer—there was less than a one-degree change in the temperature of the water, and the chemistry was essentially identical as well—though it would change a little bit during the rainy season, when there was a lot of fresh water on the surface. And Heinz's interest in all of this was that here was a place he could collect animals of various kinds, ages, and so forth, and know that the ecological environment they had grown in was constant. There's probably not another place on the face of this earth, even today, that had a record—

COHEN: Is it an island where there aren't any currents or anything?

WESTPHAL: Well, there are doubtless currents, but it's an atoll, so it has an internal lagoon, and the measurements were made in the lagoon. And there was a lot of in-and-out of the water out of the lagoon. Tides are high in that part of the world. It's a little nearer the equator than Hawaii is, but it's in the Southern Hemisphere, and the tides in Fiji and New Guinea and Palau and the whole area are large—about six or eight feet. Whereas in Hawaii, eighteen inches is it—or two feet at the most. I wondered about that, and the answer is that it's because the water is shallower there, so the tides are being built up because of that very fact: think of them as a wave going over a bump in the bottom of a river. So there was lots of water coming in and out of the lagoon. The lagoon was well-mixed and well-flushed.

So Heinz wanted to go first to Sandai and look at the animals that this guy had collected. The guy himself was no longer alive. I don't think he was killed in the war, although godawful things went on in Palau during the war. It's a wonder he wasn't. Maybe he wasn't even there when the war over there broke out. At any rate, we went to Sandai, using ONR money.

COHEN: Sandai? Now, where is that?

WESTPHAL: That's in the north of Honshu, the main island of Japan. Heinz was a very big man in Japan in the academic world and had had several students from there. So we were given a tour. That's where the story starts. [Laughter] Wondrous things happened to us just because of being in Japan that way. From Japan, we were to fly [down to Guam] on a military plane. We had flown to Japan on a commercial plane but on a government ticket, and there was no way to fly from Japan to Guam except in an old Lockheed Navy Constellation—the one with the triple tail in the back that Howard Hughes designed right at the beginning of the war; it was the greatest airplane around in those days. So we started out from Tachikawa Air Force Base in Tokyo on one of these things with about fifteen people or so on it, with the seats backward. And off we went to Guam. And we'd grind, grind, grind, grind, grind. I was sitting by the window and pretty soon I noticed—it was a four-engine airplane—that the outer engine had been feathered and that it had stopped. Nothing was said or anything, and three engines out of four is not bad in that kind of situation—

COHEN: You were a little nervous?

WESTPHAL: Well, it's not a good thing. [Laughter] So finally I poked Heinz, who was sitting next to me, and pointed that out, but I didn't say anything to anybody else and they couldn't have heard me anyway. Finally, after maybe half an hour or so, the intercom came on. I had noticed by then that we were gradually turning—slowly, slowly, slowly, turning. Clearly we were going

back to Japan. The guy came on the intercom and said, “You may have noticed we’ve lost an engine. We’ve got an oil pressure problem on another one of the engines. We’re headed back to Tachikawa and we’ll be there in three hours or so.” And he said, “We’ll have an angel in twenty-two minutes.” Twenty-two minutes later—from where, I couldn’t see—popped down an Electra off the end of our wingtip and it said “Okinawa Air Sea Rescue” on the side of it. And that plane was just essentially welded to us until we touched the runway in Japan. So then he pulled up and rolled his wings like that and away he went.

COHEN: That must have been a nervous flight.

WESTPHAL: Well, it felt a lot warmer after the angel got there, I must say. [Laughter] So that fouled up our trip. So then we went on a commercial plane. There’s a wonderful story about Heinz, when we were waiting overnight to get on this plane. We got in a taxi and went out to the US Air Force Base. And there was a marine kind of drooping around in the guard booth. He said, “What the hell are you doing here?” And I happened to be sitting at the window on his side, and I said, “Dr. Lowenstam and I are supposed to be overnighing here on our way down to Guam tomorrow.” And he said, “Hmm. You got a pass?” And I said, “Well, we have our navy IDs.” So I handed him mine and I didn’t impress him with that. And then Heinz handed him his. The guy got down and looked at him and then looked at it—and he held it so that he could really see it—and he said, “Yes Sir. Yes Sir.” And he turned 180 degrees into his kiosk to get on the phone. Pretty soon, within minutes, we could see a pickup truck coming with a red light going. The marine guard wouldn’t talk to me—he was terrorized. A guy got out of the pickup. He was in full dress, with a hat and everything. And this was nine o’clock at night or something. It turns out that Heinz’s ID card, the one you get from the naval ordnance test station office here, made him a rear admiral. [Laughter] And Heinz didn’t know that. So we drove off to our quarters. I was going to be dumped off at the bachelor officers’ quarters and Heinz was going to be dropped off at some much higher-level housing.

COHEN: [Laughter] As befitting his appointment.

WESTPHAL: That's right. Heinz refused. He said, "I'm going with him." And this guy just—you know, in the military they expect you to do what they tell you. Heinz wouldn't do it.

Anyway, so we went to Palau and did all the things Heinz wanted to do. We had a lot of fun. We saw a lot of beautiful reef, and we saw a lot of godawful destruction that had been done by our own troops after they took the island, just because they were bored. The island had been the Japanese equivalent of Bermuda—for rich people from Japan before the war to vacation. And there were the remnants of these beautiful, gorgeous houses with tile baths and all kinds of stuff out into, by then, jungle. The story we were told, and I had no reason to believe it wasn't true, was that—you see, even at that time it was a trust territory of the US. Anyway, we were told that the people there in 1953, I guess—it was just before Heinz had been there the first time—a colonel or somebody said, "I'm going to clean this place up." And he just said, "Bulldoze this," and they did.

COHEN: So Heinz got his animals, or whatever it was that he was looking for.

WESTPHAL: Yes. But one of the neatest things about it—did you see the [program] on Palau the other night?

COHEN: Not the other night. I saw it a few years ago.

WESTPHAL: Well, the other night [on the program] it was said—it's still wrong—that the notches on the island shorelines are made by waves or by solutions in seawater. There are many of these notches in places no bigger than this building, where no waves could ever have enough energy to do that. And Heinz had some intuition that it was being done by animals. As soon as

we got there and could get a boat and go out to one of the notches, we went in the water and went snorkeling. And here were chitons. Chitons are little beasts about two inches long or so. And there was a little piece of rock where one of these chitons had gone up and over and down the other side, cutting a millimeter of rock; they were eating up the limpets. It turns out that chitons have teeth on a radula that they scrape with. The teeth are three-pointed and they scrape algae. After we got back to Caltech, I was over in Heinz's lab. He had a very nice Irish woman who did all of the dissections of this stuff. And I hoisted myself up on the table next to where she was working, waiting for him to come or something, and she had some stuff she had been picking out of the chitons in a watch glass sitting there. I had a magnetic flashlight, just by pure chance, and I laid it down and I thought I saw something move. That magnet made whatever was in the glass move—and it was those teeth. They are the most pure magnetite known anywhere—now used for X-ray diffraction standards. So, out of all of the geology I've been involved with, that was a fundamental process going on in geology and the erosion of limestone materials in the ocean. And you can find chitons everywhere, once you've seen them. When we got back to Guam and landed at the airport, you could see the same notches I'd seen in the Bahamas and Fiji.

COHEN: Did Heinz consider himself a geologist?

WESTPHAL: He was a geobiologist. He was fundamentally a biologist, of course, but he was very interested in geological things and he was really interested in paleoecology.

COHEN: So that was your biology work?

WESTPHAL: Yes. And people like Joe [Joseph L.] Kirschvink [professor of geobiology] did their theses on that whole business.

Anyway, was it in 1973 that the oil embargo came and all the gasoline...

COHEN: Oh, yes. That was '73.

WESTPHAL: Something like that. Bob [Robert] Christy [Institute Professor of Theoretical Physics, emeritus] was the provost then. At any rate, the city of Pasadena was of course furnishing power to Caltech, and as the oil thing got worse and worse, why, there was more and more potential of having rolling blackouts and all that sort of stuff. It was at this point that I got a phone call from our division chairman asking me to be on a committee that Christy was forming to see if we could cut down on the consumption of electricity on the campus by a third, on our own. He wanted a one-third decrease, because he was sure that he could then get the city of Pasadena to agree, since we had these huge wires running down California Street to run the synchrotron, which was not in operation anymore—to actually feed the whole Caltech campus, independent of all the houses around it, because it would clearly be a disaster if they were going to turn off the power four hours a day or something. It was a terrible potential problem. So each division was asked to put somebody on a steering committee to do this.

The first thing the steering committee did was ask where all the power was going: Let's not do in somebody for 1/100th of a percent of the power in the system; let's find the places where all the power goes and see if there is anything we can do to help. [Francis H.] Clauser [Clark Blanchard Millikan Professor of Engineering, emeritus] was in this act immediately, and Leverett Davis [professor of theoretical physics, emeritus]. So Physical Plant was sent off to measure where all the power was going. Other people were sent off to look at potential strange places the power might be going. That happened because somebody knew—I don't remember what time of year this was, it may have been the cool time of the year—that almost every secretary on the campus had an electric heater underneath the desk, because Physical Plant never could quite figure out how to use thermostats to keep from having that happen. And every one of those was pulling a couple of kilowatts. It was a huge amount of power. It was a real problem too. You couldn't say, "Well, to hell with those. Come in your bunny suit." In the end

what we learned was that a third of the energy was going into heating and ventilation, a third of it was going into lights—people not turning them off, and this sort of stuff. The other third was distributed all over the campus, doing all kinds of stuff. So it was clear that if we could figure out some way to cut the heating and ventilation by half and the lighting by half, we'd be home free; we'd have our third and away we could go. So Clauser said, "I can fix the heating and ventilation really easily. All you've got to do is change the pulleys on the fans in the buildings and cut down the air flow, which goes with the square of the speed at which the fan's running—that is, the wasted power does. I'll bet we can get that much just out of the heating and ventilation without doing anything except changing the pulleys on these things."

COHEN: Would you have just as much air and heat coming through?

WESTPHAL: Well, you would have more than an adequate amount of air, because everything was overventilated. They should have been doing that for years, because they would have saved all this money. So I was sent out to see if there was anything to do about the lighting. I believed that the only way you could solve the lighting problem was to selectively turn lights out and turn them out permanently, if you could—at least for the duration of the event. By then, most of the lights on the campus were fluorescent lights. Just standard 4-foot bulbs. So I thought about that a while. I understood how they worked ten years before, but I wasn't sure that there hadn't been some change in the way fluorescent lights worked—that is, how they were powered. But it turned out they were built the same way. So I went down to one of our labs and pulled a fluorescent fixture down where we could get on it and get to the wires easily. I had realized that the lights were run not with a transformer, which everybody imagined, but actually with a choke—that is, with a magnetic core with a winding on it. That had two bad effects: one was that it was very inefficient, because you were essentially throwing away a bunch of power; and secondly, it made the power factor very bad. The voltage and the current didn't follow each other, because it was a reactive load. So I thought, "Gee, we ought to be able to fix the power

factor just with a capacitor of the right size.” So one of my technicians and I were doing this. There are two bulbs in each fixture, and they were in series with each other. So if you took a bulb out—and that was the very first notion; everybody said, “Oh, we’ll just take one bulb out of everything”—the other bulb would go out. So we thought about this for a few minutes. Devere Smith—he’s not with us anymore, he’s dead now, but a wondrous man—then picked a capacitor box, with various-sized capacitors and a switch on it, off the shelf right there on his lab bench, and some clip leads. We took one bulb out and clipped one lead of these capacitors to one end and one to the other—we essentially replaced the bulb with a capacitor. And then we just twisted the knob until the other light came on to a reasonable brightness.

COHEN: And you didn’t lose any luminosity? You still had as much light as you needed?

WESTPHAL: My memory is that by putting in the capacitor you got two-thirds of the light with one-third of the power. So the next thing was, well, nobody was going to accept that, it was going to be too dark. I was completely skeptical about that idea, so I got somebody from Physical Plant to come over with the light meter, and somebody else to look up the illumination the engineering community thought was the proper light level for various things, and we discovered that we were putting out three times as much light. Still, nobody believed that. So we could buy glass tubes, fluorescent tubes, we could buy the sockets, and the capacitors we could buy in a local place here. So we made up roughly twenty of these things, which were immediately called phantoms—because they were phantom fluorescents. And we made a deal with somebody who will remain unnamed, because there are probably still people mad at him. [Laughter] We went over to the bullpen in the Business Services building, on Sunday morning, when there was nobody there, and we changed every one of the fluorescent fixtures by taking out one bulb and putting a phantom in its place. And didn’t tell anybody. And to my knowledge they are that way to this very day. Nobody noticed.

So then the next step in this was to convince people that we could do it. The first thing we did was change all the fixtures in the stairwells, because clearly those were massively overlit. We went into people's offices and said, "We'd like to make an experiment," and we told them what it was about, "and see how you would react to having your light level decreased somewhat. This would save a lot of energy." People were very energy-conscious. They had to wait in those dang gas lines. And we said, "If you don't like it immediately when I do it, I'll take it out and put it back like it was. If you want only to have the ones that are not right above you in the working area that way, we can do it that way. Any way you'd like. But we think this will help a lot." There's a lot more to the story, but in the end the institute made a study of how much we were saving, because we had a company start making these things. There was a patent for the idea, and there was a patent deal with this company. I don't remember for sure the numbers, but there was a memo that said how much we were saving. It was on the order of \$35,000 a year in lighting. [Tape ends]

Begin Tape 4, Side 2

WESTPHAL: OK, so the end product was that we were able to get a third of our consumption down—and more. I guess in the end it was forty-five percent or something, because once we got going with it, why, it saved money. And this thing finally came to the attention of an organization called NACUBO [National Association of College and University Business Officers]—that's probably in here.

COHEN: Let's see. "Energy-saving fluorescent tubes special committee grand prize in cost reduction." You won an award.

WESTPHAL: Yes. It was a \$10,000 award. I didn't know anything about this award—they put me up for it. Actually, Physical Plant put themselves up for it, but I got a phone call from the head of Physical Plant saying, “Hey, you won a NACUBO award.”

I said, “What's a NACUBO?”

“Ten thousand bucks.”

I said, “Gee, that's nice, where's my charge number?” And there was this dead silence. And the guy said, “Well, we turned in the proposal and stuff, so the money should be ours.” I said, “I invented it, not you.” And they said, “OK, I don't know what we're going to do.”

So I called up [Caltech treasurer] Dave Morrisroe. And I said, “Dave, where's my charge number?” And he said, “What are you talking about?” And I said, “Well, apparently we won ten thousand bucks for the fluorescents—the phantoms.” And he said, “I'll get you a charge number.” And I said, “Well, let me warn you that Physical Plant thinks they're the ones that are supposed to get the charge number.” And he mumbled something obscene under his breath and said, “OK, you'll both get it.” [Laughter] So he gave them \$10,000 and he gave me \$10,000.

COHEN: That's a good way to settle it.

WESTPHAL: So that was a little side issue. But it did do another thing. The institute had a policy that you could get fifteen percent of whatever royalties they got. So once the phantoms started being commercially made, it just exploded, not in the United States but in Canada.

COHEN: They light for more hours in Canada.

WESTPHAL: Yes, that's right.

COHEN: By this time, you were busy with all the duties of being a professor—continuing with your students and committees, I suppose.

WESTPHAL: Oh yes.

COHEN: It must have been about now that you were approached about the camera in the space telescope.

WESTPHAL: My first contact with the space telescope was a meeting at Nancy Roman's in which CCDs were talked about. She called that meeting in Washington, for the purpose of trying to identify the very best detectors to use on the space telescope for imaging. And I went and made a presentation on the SIT [Silicon Image Tube] vidicons we were using at Palomar and said that I didn't think it was viable, given how much it weighed and the magnetics and the cooling and all that sort of stuff. So maybe a year later—or maybe even longer, I don't remember—there was a temporary science working group formed at NASA headquarters, by Nancy Roman, to discuss the instruments that were going to be put on the space telescope. This was a standard way NASA operated, and still operates today. And it's a good way, because it gets the potential users interested and keeps NASA from going off on tangents in one direction or another. [The working group] was populated by some of the very top people in astronomy: Gerry Neugebauer, Lyman Spitzer, John Bahcall. He was a major potential user and hard at work selling the space telescope to Congress. And Bob O'Dell was the chairman of this committee. He was by then at Marshall Space Flight Center. He was the Official Space Telescope Scientist, and he was running the committee. It had met several times. The committee decided they ought to have a meeting in Pasadena, because JPL had shown some interest in building the wide field camera for the telescope—I didn't know that at the time, but that's apparently why the meeting was here—and I was invited to make a presentation. I was again going to talk about the silicon-target vidicon. And just then, why, I learned about the CCDs through this National Academy committee we talked about. I hadn't known that JPL was involved in CCDs at all. So O'Dell asked me to come to the meeting and bring along the JPL CCD people. It was all on very short

notice. So a question rose immediately in my mind: they had a requirement for the camera—that it should look at a 3x3 arc-min. field of view in the sky. And even with the CCDs that JPL was proposing to build for *Galileo*, a single one of those detectors wasn't big enough to cover that. So various people had talked about the ideas of butting up four of them fairly close together, or laying them purposely wide and then taking four exposures. And the other problem with them, which was obvious for the space telescope, was that they didn't work in the blue wavelengths, especially in the ultraviolet—they essentially were totally dead.

COHEN: The CCDs didn't pick up enough photons or whatever?

WESTPHAL: Well, it picked them up in the wrong place in the silicon, it turns out—this can get very deeply into jargon here. So Bob [O'Dell] and I were standing up by the blackboard before the meeting, and I said, "The CCD is a wonderful device, at least from what I've learned so far." I was just in the midst of trying to find out if the thing was really going to do what they wanted it to do on *Galileo* as a task for my committee, the [National Academy] committee. And I said, "Things really do look extremely encouraging, but they're not going to have any ultraviolet response, and they're not going to be big enough." So we talked about this idea—putting four of them down somehow and overlapping them. He said, "Why don't you put a four-faceted pyramid—four mirrors that form a four-faced, very flat pyramid—right in the focal plane of the telescope and reflect off a fourth of the field in that direction, a fourth of the field in that direction, a fourth of the field in that direction, and a fourth of the field in that direction?"

COHEN: In all four directions?

WESTPHAL: In all four different directions, one each. And then put a CCD in each one of those. And I said, "Well, that certainly, I guess, would work. I've never seen such a thing before, but it's a nice idea. I suspect we may have some problems being able to match everything together

in an adequate way, but it's a possibility." That was kind of the end of the discussion. We started to discuss it better, and I remember saying that Bob O'Dell had suggested a very elegant idea but I wasn't sure even that was viable, and we still had the problem in the ultraviolet. This was all in the morning. There was a lunch break, which I didn't participate in. But there was a meeting apparently, at that lunch, to discuss what they were hearing. The normal way that NASA builds instruments to go on spacecraft is that some national center, Goddard or JPL or Marshall or something, gets assigned to do it. In the space telescope's case, it was Goddard that was assigned that task, but they were going to have subcontractors to actually build things. But all of the instruments that JPL has flown for imaging were all built entirely under JPL's control by JPL people, with subcontractors doing special functions. And they were called facility instruments, which meant that they didn't belong to—they were not specified by—a scientific principal investigator [PI]. And that was the way Goddard was going to handle the camera for the space telescope. It was going to be a facility instrument, which meant that if the committee was even going to seriously consider doing something else, it would have to change the ground rules and allow the PIs to propose to build a camera for the space telescope.

COHEN: Now, was this camera the most important thing on the telescope?

WESTPHAL: You could hardly use anything else on the spacecraft [without it], that's right. So the view at JPL was that this was all the more reason not to let the damned PIs get into the act. Besides, they don't know how to build anything that goes into space, and if you want to make sure something works, you'd better get JPL to build it. And you'd hear the same thing from Goddard. But since [the camera had already been assigned to] Goddard, that meant that JPL was out of it entirely. Goddard was to take over the responsibility and make sure that it really happened. They were already doing that. They had a group at Princeton trying to develop—along with Westinghouse, who was to be the manufacturer—the actual camera. It was a thing called an SEC vidicon. SEC stands for Secondary Emission Camera. It was a tube that had been

built and was used on Apollo—the television tube. And if you put too much light on it, you ruined it. I can remember on one of the early Apollo missions when the astronauts would run around on a little cart. This camera was mounted on a pole on this cart, and then they could point it whichever way they wanted. And one of the astronauts reached up and took the camera off of this pole, and he was waving it around like that, and I thought, Oh, my God, he's going to get the sun on it, sure as hell! And I thought that the whole camera would go blank.

COHEN: He faced it into the sun?

WESTPHAL: He faced it into the sun and that did it in.

COHEN: But I thought they were so carefully trained before they went up there.

WESTPHAL: He wasn't even supposed to take it off the pole. But these guys are test pilots, and they run their own world; they don't follow all of the rigid rules. And it's good that they don't, because if they did, why, not one of them would live to be five years older in the business of testing airplanes. You know, wings were falling off and all sorts of things in those days.

Anyway, Goddard already was going down that path, and of course a Princeton thing means that Bahcall and Lyman were very deeply interested in it; it was mainly Lyman's thing. Part of the deal was—and it was perfectly natural for it to have happened—once Goddard was going to run it, in fact even if Goddard hadn't run it, it would have been a natural thing to ask Lyman's people to work on it, because they knew damned well that Lyman really wanted it to happen, for a lot of reasons. There was already a fairly well-known detector group at Princeton, and they had been working several years on this, and they had spent \$5 million by then, trying to make this detector work. And it did not work well at all—all the horrible problems you run into trying to make something that's beyond your experience. So when the [committee members] came back from lunch, why, I was just waiting to hear some of the other presentations, because

there were people there from all over the country who were interested in detectors. And Bahcall asked O'Dell if he could make a statement. This was no surprise to O'Dell, who doubtless had been in this meeting; otherwise it could never have happened. And [Bahcall] announced that over lunchtime it had been decided that, based on the fast evolution in the development of these kinds of [devices] for space telescopes, the committee was going to recommend to NASA that the wide-field camera become a PI instrument.

COHEN: Which means everybody could put in a proposal for it.

WESTPHAL: That's right. I just saw the blood go out of Lyman's face. He clearly hadn't been in on the conversation, but Bahcall clearly had. And Neta Bahcall [John Bahcall's wife] was working for Lyman in those days. I always thought that—boy, talk about a ingrown sociological problem! [Laughter] Anyway, so that's how it came to be that the wide-field camera—

COHEN: Now, was that because they wanted to bring in new blood? Or did they just feel that this was such an important thing that they needed to have more ideas? Why do you think they did that?

WESTPHAL: They did that because it was crystal clear to everybody that the SEC camera was never going to work. Without something, they weren't going to fly. There was no way in the world they were going to fly the space telescope without a camera in it. The Europeans were building a camera. It was a very old-fashioned kind of camera, which never worked well. And they saw that this CCD, with all of its potential problems, as at least some new path they could go. The right way to do that was to open it up to competition and see what other ideas were out there—that nobody was thinking about, because nobody was going to pay attention to something

that had already been pipelined to Princeton. If Princeton wanted advice or something, we'd certainly give it; that would be no problem at all.

So I still didn't think this [CCD] thing was going to work. I started working very closely with JPL engineering—the people who were working on CCD devices. From their viewpoint, this was all a *Galileo* issue, but my viewpoint was that it was a broader issue. Because these [CCDs] were already ten times better than any other detectors around. They covered a tremendously wide wavelength region, down into the near-infrared, which nobody's detector came close to—including the Princeton thing; if it worked perfectly it still wouldn't have done that. And *Galileo* didn't care about the ultraviolet; it was not an issue for them. They didn't perceive that there was any planetary science in the ultraviolet. They were wrong [laughter], but that was the lore of the day. Soon after that—and I don't know what it was that triggered Jim Gunn off. I remember calling him up and saying, “Hey, they've got a CCD up here that has a readout noise of only thirty electrons.” And Jim said, “Are you sure?” I said, “Everything I can see about it says yes.” This meant that it was thirty times better than the best detector we knew of. I don't know what happened in detail, but sometime soon after that, Jim came to my office in South Mudd and went up to the blackboard and said, “Jim, we have to build the camera for the space telescope.” I said, “Jim, no way.” I didn't want anything to do with the style of NASA. I said, “It's too messy. It's not your style. It's not my style. We'll build some nifty thing and the damned thing will have a bad solder joint in it someplace and we won't be able to reach up there and fix it, and it'll drive both of us into the booby house. It's just not our thing.” And he said, “If we don't do it, we're going to be out of business in astronomy, in serious astronomy, the forefront of astronomy, on the ground, in five years.” And I said, “Oh, I don't believe that for a second. It's a little telescope. The bloody thing's only 94 inches in diameter. And we own a 200-inch.” And he said, “Yeah, but remember those good images that you've been advertising.” [Laughter] “Those are a hundred times better—more compact than the images you can do from the ground under the best conditions.” So we talked about it a little bit then; I don't remember all the steps, but at one point, finally—he just kept at me—and I finally said, “Well, what do you

want me to do?” And he said, “I want you to be the PI.” And I said, “No chance, Gunn. No way. No chance at all. I’m having too much fun to start commuting back and forth to the Marshall Space Flight Center for the next four years of my life!” [Laughter] Actually, it was seventeen years. It was going to be a four-year thing, and everybody that knew anything about it knew that was nuts. But that was the calendar. Anyway, so he started working on the blackboard, doing the sums. Have you ever seen him do that?

COHEN: No.

WESTPHAL: He does it in a way that I’ve never seen anybody else do it. When he writes down exponential numbers, like 10^6 , he writes a “10” like this with a “6” down here.

COHEN: A subscript?

WESTPHAL: A subscript. That’s right. That’s the way Jim has always been. It makes it go so much easier. [Laughter]

COHEN: Jim changes the world to suit himself.

WESTPHAL: Jim changes the world to make it better in many ways.

So we went through this, and I couldn’t argue with his numbers. And by then his numbers were even better—we were down to twenty electrons. And we weren’t done yet. In fact, in the end, why, we flew things that were eleven electrons. And then flew ones that were six electrons. So they were just as near an ideal detector as you had ever hoped for. This business was about the noise performance of the detector—the intrinsic noise in the device that you can’t get rid of.

COHEN: I see.

WESTPHAL: So he finally came over, and had at me again. And I said, “Well, I’ll tell you what: Let’s write on the board the names of some people we’d like to have on our science team and see if we can identify people who can really help us make this thing happen, and who have real experience and fill in the holes.”

COHEN: Now, this is in anticipation of a competition?

WESTPHAL: This was in anticipation of a competition.

COHEN: It wasn’t as if you had been asked to be PI at this point.

WESTPHAL: That’s right. We were just getting our act together to see if we wanted to have anything to do with this. So we both wrote names on my board, and it was not a hundred-percent overlap. I knew people he didn’t know; he knew lots more people than I did who might be useful in this. As I remember it, we had ten names on the board—some even number—and it included Roger Lynds at Kitt Peak, whom we both knew well, and Doug Currie, who was at the University of Maryland and was an applied physicist who had been a Carnegie fellow here for a while at Santa Barbara Street. We tried to find an ultraviolet guy, and the only ultraviolet guy in the country was Art Code, at the University of Wisconsin. He was at Caltech for quite some time—he was a wonderful guy and a real ultraviolet pro. And Bob Bless, who was also at Wisconsin, was going to be the PI on another instrument and so Code, naturally, was involved with that instrument. So we didn’t actually ask him at that stage at all. We were really worried that we couldn’t identify a real ultraviolet experimentalist. So the agreement was that we’d split this list into two pieces and that Jim Gunn would call the people he knew best and I’d call the

people I knew best and we'd see what the reaction was. So Jim went away, and I started making some phone calls.

One person who I remember vividly was on my list was Roger Lynds. And Roger was one of the people who was reasonable. He just is on top of what's going on, in a way. He can drive you right up the wall worrying about trivia, but he was very realistic about the real world. So I was damned sure I was not going to get Roger to sign on to some madness like this. So as I went down the list, why, everybody I called said, "Oh, yeah, I'll be a member." Bill Baum. He was at Lowell [Observatory], probably, when you came; he used to be at Santa Barbara Street. The last one on my list was Roger. Roger was married to B. T. Lynds, who was also an astronomer—Beverly something, but she's always been known as B. T. She was the deputy director of Kitt Peak at that point. Roger you could never get on the phone directly. You always had to get somebody to chase him down. I knew that, so I called B. T. and said, "Do you know where you can find Roger?" And she said, "Yes, what do you want?" And I said, "I want to talk to him about a crazy enterprise that Jim Gunn and I are looking into. We may be building an instrument to go on the space telescope. I'm sure Roger will not want to have anything to do with such madness, but I thought I'd like to give him a chance to do this." She said, "Don't be too sure." [Laughter] It wasn't two minutes till the phone rang. I picked it up and on the other end of the line all I heard was, "Yes," and then he hung up. [Laughter] And that's when I knew I had had it.

COHEN: So you guys put together this committee? And then you wrote a proposal?

WESTPHAL: And then we wrote a proposal.

COHEN: How long a period of time was it before it was accepted?

WESTPHAL: Well, the proposals were essentially due on the fourth of July in 1977. The discussion we have been talking about occurred maybe six or eight months before then. The selection was made sometime in late September, I believe. I actually have the telegram—that was before email—from Nancy Roman saying, “You’ve been selected.”

COHEN: And then your life changed?

WESTPHAL: And then my life changed. Seventeen years. [Laughter] Four years stretched out to seventeen. [Tape ends]

JAMES A. WESTPHAL**SESSION 5****July 27, 1998****Begin Tape 5, Side 1**

COHEN: Let's start by talking about when you knew that you had gotten the project. Now, your team was already made up at this time?

WESTPHAL: We had made up our team. We ultimately added two more people to the team later on. One was Art Code, whom we were very anxious to get at the beginning, but we assumed that since he was working with Bob Bless on a different proposal, he didn't want to be on two teams. It's bad enough being on one. And then fairly soon after, the selections were announced, and then we did approach him about this, because what [he and Bless] were going to do was not anything like as big a task as what we were about. And we were very anxious to get him, because he was the person who understood how to do ultraviolet instrumentation and what the interesting ultraviolet science actually was.

COHEN: And that was the really new thing?

WESTPHAL: That's right. It couldn't be done from the ground. One of the primary reasons the space telescope had to be in orbit was to be able to see the ultraviolet. The second main reason was that you got very much better images. Maybe a factor of ten smaller than you'd get even on very good nights on the ground in those days. We've improved that on the ground substantially in recent years.

So we turned the proposal in. Then my family went off to Hawaii to take care of a couple of pieces of property we had there and to play in the water. As my medico said several times in the course of all of those years, "It's time for you to go to Hawaii and take the warm

saltwater cure—total immersion twice a day, three hours each.” [Laughter] But it certainly was true that one needed, as time went along, to get away from these things. My doctor’s position was that two weeks was not enough, but I couldn’t possibly stay away two weeks. Even then there were phone calls several times a day. We had this nifty place in south Kona where I could hide from the phone. But as time went on, people discovered what the phone number was. In any case, I got a telegram from Nancy Roman, who was the space telescope project scientist at NASA headquarters. I can look it up and let you know what the date was; it was some months later, the end of September or maybe even into October 1977. It was just a short telegram. I think it’s the only telegram I have ever received. I thought it was kind of strange for her to send a telegram. She wanted something that she had a real record of, so it was a reasonable thing to do. Bob Christy was our provost in those days at Caltech; he was one of the people who encouraged us to do this and essentially approved our doing this, because, especially in my case, I was very concerned that—

COHEN: That it would take all your time?

WESTPHAL: I knew it was going to take all my time, and I was concerned that my colleagues would get pretty tired of that after the four years it was supposed to take, much less the seventeen it actually did. And my colleagues had always been tremendously supportive about this.

COHEN: So you got this telegram from Nancy Roman.

WESTPHAL: I got the telegram from Nancy Roman saying that we had won, with the following two conditions. One was that we add a team member, Ken Seidelman, from the US Naval Observatory. Through those years he was the chief guy who put out the nautical almanac that all of us would use to find out where things are in the sky. He was interested in astrometry with the

space telescope. There was already a science team to do that, but Ken wanted to deal with planetary objects like satellites and even planets themselves. He wanted to improve the ephemerides of the planets by being able to have sharper images. And we were perfectly happy to do that. None of us knew him, but Doug Currie, who was on our team, was at Maryland, so I asked him to snoop around and see whether this guy was a good guy or not—whether he was going to be a problem to us somehow. And he reported back—almost within the day, I think—saying, “I can’t find anybody who doesn’t think this is a really good idea.” So I called Seidelman. I was down at Palomar; I remember talking to him from the data room. There were a lot of people around, so it wasn’t a very handy place to be. But I asked him if he’d like to join up with us and he said, “Oh, yes, I’d like to do that.” So that was our first new team member, and it was really a person selected by NASA. And then our second was Art Code. And then, many years later on, we added Sandy [Sandra] Faber.

COHEN: Now, why did Nancy Roman want you to have a certain number of people on there? Was your first number not big enough?

WESTPHAL: No, her problem was that Ken Seidelman had made a proposal to work on these planetary things, and Nancy was very sensitive to the fact that the space telescope needed the service of the planetary community as well as the deep space community. And there was even a meeting of the planetary division of the American Astronomical Society in Honolulu, at which there was a famous and powerful East Coast astronomer who was not a planetary person and who made a public presentation saying, “OK, you’ve got all of this money. You’ve sent all these spacecraft off to the planets. ST belongs to us, the real astronomers.”

COHEN: That sounds like George Field.

WESTPHAL: We're talking about George Field. He clearly had not thought this through for ten milliseconds. I don't know what triggered it. Fortunately, Bill Baum was the chairman of the Division of Planetary Science. He knew Field. I didn't actually know the man—I was not involved in this, except as a spectator. Somehow or another, he and Bill ended up sitting next to each other on the airplane going home. I think they had talked to each other, and by the time George got home he had decided that that wasn't one of his better things to have done. So he apologized. I think by then I was the chairman of the DPS—it fell to me just at that meeting. My memory is that I ended up with a letter from George apologizing and saying that he really hadn't thought this through very well, or something to that effect. But at any rate, it all blew over. But Nancy was really concerned by this, because she was a stellar astronomer, she was a spectroscoper, not a planetary person. And she knew about the politics of NASA headquarters—that in the end what NASA headquarters wanted out of the science program was pretty pictures for the public. Planets tend to be that. And there was already a long history of it, you know. Many of the planetary science guys at that time thought the space telescope was really great, because you could get very much better images of the planets. And because many of the planets change [appearance] rapidly, because they have an atmosphere, why, it was really the only realistic way at that time to monitor what was happening. An example would be the cloud structure of Jupiter, or what was happening in whatever cloud structure there might be on Saturn, and then finally Neptune. This was before *Voyager* got there. So it was a real issue. And in fact one of the little flurries that occurred later was when a planetary astronomer at Kitt Peak, a fairly powerful figure, notified NASA that he wanted two solid months of ST time to monitor Jupiter, Saturn, Uranus, and Neptune—to monitor those with the ST as the *Voyager* spacecraft was passing by. And that didn't go over well at headquarters. It could have been made workable, if he had presented it differently, or if he had chosen some better way to go about that.

So the first thing that happened after I got the telegram and we got the Seidelman thing settled was that a formal letter came from NASA headquarters saying, “The following people are on the team,” and that there would be a kick-off meeting of all of the instrument teams at

Goddard sometime later. That meeting occurred at the end of November, as I remember. That was the first time that the teams talked together at all.

COHEN: So for the first time you were meeting people that were doing the other aspects of the science on this?

WESTPHAL: That's right. We were familiar with these people. Some of us knew every one of them, but many of us didn't know a lot of them. There were a couple hundred people, and that didn't even include the Europeans, who were off building [an instrument] of their own. But it was not competing; it was a facility instrument, in which the European Space Agency decided who was going to build this thing. That was kind of interesting, too, because they picked [H. C.] van de Hulst. He was the chief scientist for that instrument, and he wouldn't go away.

[Laughter]

COHEN: Let me backtrack just a minute, because you refer often to the black box. The black box is when the instrument is in there, and you don't have to know what's in that black box.

WESTPHAL: Yes.

COHEN: Here it is; you use it. If something's wrong, you come back to us, you don't look in the black box. It's yours to use but not to touch.

WESTPHAL: That's right. You can specify what it is supposed to do, but not how it's supposed to do it. That's a very logical and very reasonable [approach]. But the fatal problem always is that nobody is smart enough to specify what it is that they really want to have happen. Because these guys take the specifications and they'll go off and start building stuff. And if they come to something that's hard to do, or costs lots of money or takes a long time or maybe is impossible—

because somebody who helped to make the specifications didn't realize that, or nobody realized that—then the whole thing just stops or they spend all of their money trying to make this one little thing happen. So internally, it's just a chaotic [approach]. The two major contractors, Lockheed and Perkin-Elmer—

COHEN: This was not for just the camera?

WESTPHAL: They were the contractors for the space telescope: Perkin-Elmer made the optics; Lockheed made the physical framework of the telescope. And the Marshall Space Flight Center, which we haven't dealt with at all, was the overall center for all of this.

COHEN: Now, this was the first time everybody came together. Is that something Nancy Roman did? She got all these people together?

WESTPHAL: I have no idea; it was an obvious thing to do. My guess is—and it is just a guess—that Marshall Space Flight Center did it, because they were officially the overall NASA group. And that led to horrible problems, as you can see through the history. Everybody knew that was a terrible idea, but they tried to do that kind of thing with the Einstein X-ray instrument [High-Energy Astronomy Observatory]. Marshall did that, and they did some of the stupidest things anybody ever heard of. They lost a spacecraft very early on, because they used some cheap gyroscopes that nobody had ever tested, and the gyroscopes burned up when they got out in space. And on and on and on. There were all sorts of problems associated with that.

COHEN: You had that initial meeting at Marshall.

WESTPHAL: No, it was at Goddard, because Marshall had given the instrument task to Goddard. But we needed the telescope people to answer technical questions about how this fit into that and

how much power there was and all that sort of stuff. It was clear that all of us were going to have to meet endlessly and in small technical groups. They were called TIMs, Technical Interface Meetings. These people love acronyms. I remember one time early on—once a quarter we had a full-up meeting of all the representatives of all the functions and all the pieces of the telescope instruments that they were going to use at Marshall. There were headquarters people there. They had the Marshall director there—he was the big gun of the system. And Nancy got to sit at the main table. Lots of protocol—oh, boy, was there protocol! The PIs all had to sit back against the wall, all in a row. [Laughter] So Nancy happened to be sitting right in front of me. And somebody said something full of acronyms. She turned around to me and said, “Jim, what are they talking about?” And I told her what they were talking about. And she said, “How did you know that?” And I said, “It’s now my business to know what all those acronyms mean.” And then, when I walked out of the place I thought, “Boy, that’s a hell of a comment—that that’s what I had to learn and I’m now the expert.” [Laughter]

So there were these endless meetings, and they all were appropriate things to do that could have been done in a third of the time if they had been organized and if the people who were attending those meetings were the people who were really going to do the work.

COHEN: So who was at these meetings?

WESTPHAL: Well, the PIs were at the meetings.

COHEN: Did any of your group have to come, or just you?

WESTPHAL: No, nobody else in the group could come. That was the protocol. It was a huge room, and it was stacked full of people; it was a real problem to keep the number of people down. Of course, everybody wanted to come and see what was going on. And a big problem internally at Marshall was that everybody at Marshall, from the directors to the deputy on down,

had to come in and see what was going on. And each of the PIs had a contractor. My contractor was JPL. Now, that's another interesting aspect of this.

COHEN: When you organized your group, the supposition was that it would be at JPL?

WESTPHAL: Oh, it had to be, because they had the detectors and they had the expertise. There was no question about that. And I never would have hired some outfit where I would have had to commute back and forth for a long distance. The fact that it was five miles away was bad enough. We wouldn't have done it at all if we had had to deal with, say, Ball Brothers [Ball Aerospace] in Denver, which is what two of the other teams had to do. Now we're going to talk about the sociology of people on the campus dealing with people at JPL.

COHEN: OK, good.

WESTPHAL: Remembering the background we talked about before, where normally those engineering people at JPL would be going about their business, and here suddenly was a PI and a deputy PI from down here [on the Caltech campus]. The JPL people—to this day, to the best of my knowledge—are terrorized by faculty members from this campus. They can tell you horror stories about why they were terrorized.

COHEN: Do you mean they have reasons?

WESTPHAL: Yes, they have reason to be terrorized. I mean, some of my colleagues don't have a lot of social skills. And I don't either, a lot of the time. [Laughter] But mainly this just was foreign to them, and they were very uptight about it. Particularly, they got uptight when I started keeping track of how much money they were spending and what they were spending it for.

COHEN: They weren't used to being held accountable?

WESTPHAL: They were used to it, but only to their own bosses. And so one of the things that had to get straightened out very soon was, you know, the golden rule: those what's got the gold makes the rules—that's the golden rule. So I had to get things straight with Goddard, whom I reported to, as to whether I had the power to control the money or whether JPL could just spend money in their normal way. What happens very often in a situation like this is a thing that is in reality not a bad idea—but I think probably wouldn't stand up very well if somebody had to defend it—and this isn't just JPL: the whole aerospace industry works this way, and it's expected to work this way. That's why I got in trouble—I didn't expect it to work this way. If you have a whole flow of projects that are moving through your system, some just being designed, some being tested, some being put on spacecraft and ready to be launched and so forth, why, nobody—especially those folks—can foresee how much money they will need at each stage of this process. The highest priority is money for the guy who's just about to put his instrument on the spacecraft. And then the priorities fall, below that. Well, the way that the accounting system is run is such that there is a lot of flexibility in shifting monies from one end of the process to the other. And I had taken it very seriously, although the word from NASA headquarters was that I had to stay within my budget. The next thing I knew, why, somebody had taken some money out of my budget and used it for something else.

COHEN: Do you mean someone at JPL? Do you mean moving things around like they were used to doing?

WESTPHAL: Yes, and they just couldn't imagine why I got uptight about this. So we had a little flurry for a while about that. It finally did get straightened out. They just quit doing it that way, since it was my project. But, of course, they hated the thing every second because of that—because I was destroying their system.

COHEN: Well, you were coming from outside and telling them what to do.

WESTPHAL: That's right. The only way I could control that was by the golden rule. So I got it clearly established between the two center directors—the JPL director and the Goddard Space Center director—that I had the gold. And I set it up that they couldn't spend over \$20,000 without my approval.

COHEN: And they weren't used to this kind of thing.

WESTPHAL: Oh, no. Oh, no, not a bit. They were also terrorized—particularly some people in middle management at JPL—that JPL was going to build this instrument. They wanted to farm it out to somebody, so that they could have somebody to blame when it didn't work. They didn't have a lot of confidence in their own business. What they wanted was a fall guy out there. But what I'm saying is that it wasn't just JPL that worked that way; that's the culture of the aerospace industry. But the thing that bothered me so much about it was that they said they were doing one thing, and they were doing something else. And they were putting me in a position that I felt was untenable.

COHEN: Do you mean that you didn't know what was going on at any one moment?

WESTPHAL: Oh, no, I could find out what was going all right, because all of the engineering people loved me. I had so many spies around you wouldn't believe it. And I didn't go out and recruit any—they'd come around and say, "You ought to know such and such and such and such."

COHEN: Did you have an office at JPL, or did you keep your office down here?

WESTPHAL: I kept my office down here. I commuted up there back and forth four days a week, or something like that, not on a regular schedule. And that was another problem: they never knew when I was going to pop up out of a door someplace.

COHEN: [Laughter] It sounds like you weren't the favorite person up there.

WESTPHAL: Well, not among a certain group of people. Anyway, things began to go very well at JPL. Probably six months into it, I went to Marshall for the quarterly review, and the JPL systems engineer went with me. He's the guy who was supposed to understand everything technically that was going on. Then there was a financial person who went along who presented a sketch of how we were doing on our budget.

COHEN: Now, you must have done the actual design of the instrument.

WESTPHAL: No.

COHEN: You just said what you wanted it to do, and then they had to design it?

WESTPHAL: Well, no, we didn't black-box it either. We sketched up what we thought we wanted to have. We didn't worry about what size screw they used, or anything like that. We worried, though, about what kind of metal they would use, and we worried about the testing of things like that ahead of time, so we didn't have a surprise when they picked it up and put it on the table and shook it, which is what you do just before you ship it down to the Cape to be launched—you make sure it doesn't fall apart. At any rate, there were the three of us there. And I made a little presentation that I thought explained where we were. And just in passing I mentioned that we were under-running our budget by about \$200,000. I was very proud of that.

I thought that was neat as hell. We were really serious about staying within our budget, having a reserve, and being able to take care of problems that popped up. I noticed there were some frowns around the table, especially from the JPL financial person. I was so innocent in all of this that it never occurred to me that I shouldn't have said something like that. The immediate conclusion when that happens—when somebody goes under budget—is one or both of two things: one was that you were way, way behind, because the fact that the money hadn't been spent meant that the work was not being done. Well, I made crystal clear that technically we were ahead of where we were supposed to be, but nobody believed it, because that just never happens.

So there was this educational process going on for probably the first six months or a year, and it never ended, actually. It was certainly a different world. It was a world that my science team really couldn't help me very much with.

COHEN: Now, you said one of the [possible perceived] reasons the money was not spent was that you were behind. What was the other reason?

WESTPHAL: The other reason was that you were so incompetent when you made the proposal that you hadn't even known how it was supposed to go. [These were] both completely logical conclusions. [You could] make the assumption that people weren't competent to make a proposal. And nobody was, because nobody ever did it before. Then there's a side issue to all of this, and it's really an overwhelming side issue. The issue is that JPL is a NASA facility: it can't make a profit. All the other contractors were private companies, and the object of their exercise was to make money—not to do science but to make money, and if you can do some good science along the way, that makes it more fun.

COHEN: And you get the next job.

WESTPHAL: Well, maybe, and maybe you don't. That depends on some other kinds of politics at different levels. But the Marshall folks, especially, never ever built anything in-house after [Wernher] von Braun left. Because by then some of those guys were getting toward retirement age and they had a lot of expertise. So when they got ready to retire, why, any smart aerospace company was going to hire them. They went to work at twice or three times the salary that they had been making with the government. And that inevitably was seen by those who were working their way up, so you want to be good to your potential new employer. You want to encourage them.

So we worked our way designing the instrument at JPL, with Jim [Gunn] and I watching what they were doing. Pretty soon it was clear that we had identified the right people. We got rid of two or three people.

COHEN: Now, Jim was still at Caltech?

WESTPHAL: He was still here, yes. But he didn't go up there every day, like I was doing.

COHEN: He was doing other things, too, I would guess.

WESTPHAL: Well, one of the things he was doing during that time was building something called a four-shooter. The four-shooter became the visible-light CCD instrument for the 200-inch for many, many years. Jim loves to do that sort of thing. And he's very, very good at it. And he loved it so much that he moved over into a little darkroom in South Mudd and holed up in there with a drafting table and designed this instrument. He made all the machine drawings the machinist could work with. And I had convinced NASA—Goddard, to be specific—that it was very important for the users of this machine that we were going to fly to have some experience, particularly because it was unusual, in that it had this pyramid device in the center of it that spread the light out and reimaged it; how do you put those together, and how does it work? And

so we built an optical system with almost all of the details of the wide-field camera, only on a much bigger scale, for the 200-inch at Palomar. It comes to mind right now because it was the instrument that Jim and Don [Donald P.] Schneider and Maarten Schmidt used all those years. It's now obsolete and we're trying to figure out what to do with it, because we need to get it off of the floor. It lives in a little garage, as they call it, on the observing floor of the 200-inch, to keep the dirt and stuff off. And now we need that for the other optics.

COHEN: Send it to the Smithsonian.

WESTPHAL: Yeah, well, there's a bunch of stuff going to the Smithsonian, including a lot of pieces from WFPC 1 [Wide Field and Planetary Camera 1].

COHEN: Well, let's get back to our story.

WESTPHAL: Yes, OK. I had three helpers who were there at certain times. Jerry Kristian was worrying about whether we would ever be able to get our hands on the data coming down from the spacecraft, with adequate quality so that the pictures didn't look like somebody had sprinkled pepper all over them. And there was a big fight between our teams, essentially, and Jerry was the leading producer of the graphics that I used to convince Marshall that they really didn't want to have to explain to the public why their pictures looked like they were full of black sand. And Jerry made simulations of what this was going to look like. It now seems like a little thing, but it was a huge, big flap. Almost everything was a big flap. Anytime somebody wanted to change something, or determined that something wouldn't work, why then there was a big flap.

COHEN: So here we're really getting into the philosophy of spacecraft. They wanted it all done in the beginning, they didn't want to change it, and that was that.

WESTPHAL: Right. That was that.

COHEN: It didn't matter if something new and better came along.

WESTPHAL: Well, especially if it was better. In fact, there's another NASA saying: "Better is the enemy of success."

COHEN: [Laughter] That's terrible. They actually say that out loud?

WESTPHAL: Oh, yes [laughter], proudly. Jerry was taking care of that particular problem. He was just wonderful in a meeting, because he would sit there and not say a word and listen carefully and you wondered if he was paying attention or not. And then at the appropriate time, as the meeting was finally about to come to a decision, he would say, "Well, I think you haven't thought about this and this and this, and therefore it won't work." And he was just great at that. But I don't think he did any of that after the first year, because he was so discouraged by it; it seemed like such a useless kind of thing to do.

And then the other person who was involved, who was not a team member, was Ed Groth, who was another one of Gerry Neugebauer's students. He is now a professor of physics at Princeton, but he's really an astronomer. He was officially in charge of making sure the data was OK. So there were these splinter groups floating around. And this just ground on in its way, and ground on and ground on. It soon became clear that nobody was going to be anything like ready in four years.

There were various crises as we went along. I got to be a very close collaborator and friend of Lockheed's chief systems engineer. His name is Dominic Tenerelli. Anyway, he discovered that I had access to an awful lot of good talent around here that he didn't have access to. So pretty soon I became an informal consultant. He would raise a question that he felt maybe I could help him with, and I would either know what to do or knew somebody who did.

That, I think, turned out to be very, very important. There was a time, a way into the project, when... [Tape ends]

Begin Tape 5, Side 2

WESTPHAL: Anyway, one time Dominic called me up. This was very near the end of the project.

COHEN: After four years had gone by?

WESTPHAL: Oh, a lot longer than that. We essentially went into kind of a stand-down mode for the two years it took to get the shuttle fixed, and they diverted money from everywhere in sight, of course, to do that. Now, they actually didn't touch us, so that was a good thing.

Anyway, he called me up and said, "Jim, can we put an image of the sun on the shutter on your instrument without hurting anything?" And I said, "It's thin aluminum and it's got a black coating on it, so it's going to heat up in the sunshine. Why would you want to do that?" And he said, "We've just discovered by a simulation that if somebody doesn't deliberately change the software that controls the pointing of the telescope, about fifteen percent of the time when you say, 'Go from that star to this star,' you're going to swing by the sun. Of course, quick like that." And I said, "Dom, you have much bigger problems than worrying about my shutter, because what's going to happen the first time you do that is that the primary mirror of the telescope is going to make this solar image out there and it's going to melt its way right up the side of the telescope tube, right across the spider, and right across the secondary, depending on where it's going, and it's going to be vaporized. And when it does, you'll never know what's happened until somebody goes up there and looks. You'll never figure out what in the world happened." And there was this dead silence on the other end of the phone for a few moments, and then he said, "Well, that's one we've got to fix." [Laughter] And the only reason that came

to mind was the event that happened on Mount Wilson. That was a classic kind of thing that he would ask me.

COHEN: But if you hadn't had that relationship with this guy, he wouldn't have asked. It's so important to have these relationships.

WESTPHAL: That's right. Anyway, it went on and on. We could spend an unlimited amount of time talking about the details, most of which, thank God, I've forgotten.

COHEN: Were you essentially done with your piece of the work on time? Or did that go on longer also?

WESTPHAL: We were done with it on time. In fact, we were ahead of time by a little bit. So we were slowing down, because of the shuttle business [the *Challenger* disaster]. Then we ran into this problem that's detailed in this document from Robert Smith—in that oral history. That was a terrible problem.

COHEN: What was that called again?

WESTPHAL: QEH—quantum efficiency hysteresis. It was a thing that was just a horrible embarrassment to both Jim [Gunn] and me. Because Peter Young kept trying to tell us there was a problem, and we kept telling him there wasn't any problem. Years of this.

COHEN: Can you summarize what it was?

WESTPHAL: Well, it turned out that because of the details of the way a CCD is manufactured, and the physical fact that silicon is not very transparent in the ultraviolet, so that if an ultraviolet

photon comes down onto the silicon, which it would in normal observations, why, it would make the photoelectron very close to the surface of the silicon, where there's all sorts of traps—fundamentally mechanical traps, like little cracks and all kinds of things in the surface of the silicon—which grab those electrons and they have to recombine again with the holes. So the thing would work wonderfully well in wavelengths that were blue-green and longer. But shorter than that, it couldn't give the right answer, because some of these electrons would get trapped. And as you went bluer and bluer—that is, more and more into the ultraviolet—the photons got in more shallow and more shallow until finally you didn't get anything.

COHEN: And Peter Young saw this?

WESTPHAL: Well, no. Peter Young just kept telling us that he was using it on the 200-inch. The instrument was called PFUEI—"Prime Focus Universal Extragalactic Instrument." Just "phooey."

COHEN: Well, it's good to be creative. [Laughter]

WESTPHAL: Yeah, that was Jim's [idea]. Anyway, Peter was using this on a big scale, and doing wondrous stuff with it, but he kept telling us that it didn't work right in the blue. And we kept telling him that I could never find any problems when it was down here. And it was even more subtle than that: it worked poorly the first night, and then from then on it worked OK. It's really an extremely simple kind of idea, but it took a while to figure out what the problem was. It had a trivial solution that was already known, which was to put a lot of ultraviolet photons onto the chip and that would charge up the outer surface of the silicon—it was really not silicon open to the outside. It's a little, thin layer of silicon dioxide, which happens just because the silicon is oxidized out of the air. You could plant a whole lot of electrons on that surface—on the outside of that silicon dioxide surface. That would put the proper electrostatic gradient downward into

the silicon to push the electrons away from the surface. So it turns out in the end we pointed the telescope—with this special little pathway that was built into the instrument—we pointed not the whole mirror of the telescope at the sun but we pointed this path at the sun when we got into orbit. And we actually only had to do it once.

COHEN: From then it was OK?

WESTPHAL: And that was what the lab work all said. But it was a tremendous embarrassment to us, because here we had all these clues, and it turned into a horrible thing. I needed to go to Hawaii because of some problem I was having with a rental house, and it was close to Christmas. I decided not to go to one of these quarterly reviews. And I didn't make sure that somebody else who knew what was going on went. Nobody was there to explain why we had this problem. It was a real failure on my part. It cost us a year and a half of pain and suffering to recover from all that. Everybody wondered if we'd have another meeting. Everybody from Charlie [Charles H.] Townes to God himself was brought in to look at the problem and make sure that we knew what we were doing.

COHEN: But it came out OK?

WESTPHAL: Oh, it came out OK. But it was a very painful period.

COHEN: This was a new experience for you, not to be right instantly. [Laughter]

WESTPHAL: A learning cost. So *Challenger* happened. Strange things happened associated with *Challenger* that I never really understood—not anything really serious. But I was on an American Airlines plane flying between Los Angeles and Washington when it happened. I essentially owned the seat in that airplane—morning flight from Los Angeles to Dulles. And by

that time I had so much mileage I could fly first class all the time. So I knew the first-class crew, which tended to be kind of stable. But I had never discussed what I did for a living or anything else with any of these people. One of the flight attendants came down the aisle from the cockpit and she leaned over and said to me, “I think you should know that the *Challenger* has blown up, and apparently everybody was killed.”

COHEN: So how did she know—

WESTPHAL: I never could figure it out. And two or three years later she happened to be on that same flight, and I asked her how she knew to do that. And she said, “I don’t know. It just seemed like what I should do.”

COHEN: How interesting.

WESTPHAL: Isn’t that amazing?

COHEN: So you knew right away that, besides the terrible tragedy, this was going to put some kink in the project?

WESTPHAL: Oh, yes. So ultimately we went off to Lockheed and put my camera [the Wide Field and Planetary Camera 1] into the telescope. The telescope went into a great big chamber that you could pump the air out of so that you could simulate being in space. There were little heaters all around it and cooling things all around it so you could temperature-cycle everything. And that went on twenty-four hours a day for sixty-some-odd days. It was supposed to have lasted twenty, but they had so many problems with the spacecraft—and almost no problems with the instruments. There were problems with the spacecraft getting too hot in some places and too cold in others, and various other things. And that’s why you do that kind of test—it’s to find

everything. There were a lot more problems than anybody dreamed there would be. And there were a lot of problems with the various electronic boxes; they were getting too cold and getting too hot. Those tests are called thermovacuum tests. We were actually simulating trying to take observations. We found our own problems—we had one or two little ones—of which, since it was clear that Lockheed and Perkin-Elmer had real problems, why, we got them fixed. The idea was that they were going to do those tests and then they were going to ship the whole thing right down to the Cape and launch it. And it was clear there was no way that was going to happen. There were all kinds of reasons why that wasn't going to happen.

They did this test. They found problems with their part of the spacecraft—mainly thermal problems, which meant that they had to put thermal blankets on some things and heaters on other things and change thermostat settings and all sorts of stuff. And that was clearly going to take a big piece of the year for them to do all of that. The original scheme was that they were going to do this test and put it in a C5A and fly it off to the Cape and launch it. Now, that wasn't going to happen, because the shuttle wasn't back in service yet. So a lot of money was spent on the taxi meter. That's one of the problems of the system. One of the problems is that you have this huge bunch of people on the payroll and everybody is supposed to be gainfully employed and doing something. And you had a lot of people sitting there and waiting. And particularly in those days while the aerospace industry was still thriving, why, if there was somebody sitting there for a month with nothing to do, they would get put on another project and you'd never see them again. They'd disappear back into the black world, and that would be a total disaster.

COHEN: You'd get someone who didn't know what was going on.

WESTPHAL: Yes, and how much are they going to tear up and how much to fix it, to teach them? At any rate, we fixed our parts of our instrument.

COHEN: And then you just went back to Pasadena? You didn't stay there, did you?

WESTPHAL: We brought the instrument back here and we fixed our parts. And then it sat here for a while. We did a makework enterprise and sent it off to Goddard and let Goddard play with it for a little while. I was vehemently against that, but they were hell-bent on doing it. The golden rule held; they had the gold, they made the rules. And so it was sent to Goddard, and then it was sent back here again. And every time you ship something like, first, it's extremely expensive, because you use a special semitrailer truck to drive it across the country. And every time you do that you're taking a chance that somebody's going to run into you, or the driver goes to sleep and the truck gets hit, and so on. So it's a scary time.

COHEN: Oh, that must have been a really wearing year on you. It must have been terrible.

WESTPHAL: Well, more of the same. Anyway, so we launched.

COHEN: And that would have been what year?

WESTPHAL: The launch was 1990.

COHEN: Did you go down there for the launch?

WESTPHAL: Yes, we went down there for the launch. And the first launch was scrubbed because an auxilliary pump didn't come up to snuff. And we were all out on the causeway. Have you ever been to one of those launches?

COHEN: No.

WESTPHAL: Well, there's a causeway that runs across what's called the Indian River. It's the backwater of the ocean. Well, anyway, they tried to launch and had to scrub it. Two weeks later, we tried again. It worked this time.

Morton Dean of ABC News, along with a lot of other newspeople, had been at a news briefing about the space telescope at Goddard a month before launch, and we had gotten acquainted. He was a technically inclined guy, so he was asking all kinds of technical stuff. So it was agreed that he was going to be down at the Cape for the launch. He started a campaign to get me to sit next to him in the ABC booth and be the color man, or whatever it is they call it. And I was reluctant to do that, because after all these years I thought I owed it to my family and friends to be with them when this thing went. So that's the way we did it; he sent a camera crew out to where we were on the causeway. And it didn't go.

A few weeks later, the whole thing was to be done again, but by then the launch had slipped an hour later, to get it into the orbit they wanted, and that meant he couldn't do the thing live on *Good Morning America*. It was then going to be just the camera crew that was going to be with us. So that happened. It was launched. We saw the solids go and everybody cheered. All of my family was there, and lots of other people, a bunch of the people from here who had helped to make this thing happen. We were all yelling. So the camera crew came. The producer said, "Let's get some footage of your family first." She talked to me for a little while, and she talked to my wife. She had established, when she talked to me, that we had been at this for fourteen or fifteen years, or whatever it was at that time. Then she turned to Jean, my wife, and she said, "How did all of this go for you during all of those years." Jean looked dead into the camera and said, "No comment." [Laughter]

COHEN: [Laughter] Very good.

WESTPHAL: And I have never seen so much information transmitted in so few words in my life. [Laughter] We all immediately went and got on an airplane and flew back up to Goddard, or

Johns Hopkins—same difference. Flew back up to Washington. Actually, we didn't run right to the airplane. The shuttle was launched early in the morning. They were going to deploy the [space telescope's] solar panels in the afternoon and that was critical, because if somehow that didn't work, why, we were still in trouble. So we all holed up in my motel room—I don't remember the name of the little town, just next to the Cape—with a TV set and various bottles of champagne, et cetera. The room was just packed. People had to sit on the floor, and some were standing, to see this thing. And then they had trouble getting the solar panels to come out. They got one of them pretty well out—three-quarters of the way or so—but the other one didn't want to move. And that was one of the things they'd always worried about. An astronaut was prepared to go out to put a crank on it and crank it out—literally with a crank. He was already in the airlock and getting ready to go out when they managed to get the thing opened.

COHEN: A tense moment?

WESTPHAL: That was a tense moment. Little did we know why that problem arose at that time, but it ultimately became a terrible problem. The solar panels were flapping as the telescope was in orbit, and that was shaking the telescope. That was because those solar panels had been made in Europe and they didn't pay any attention to the temperature requirements. And every time you'd go in and out of the earth's shadow [the panels] would creak and wiggle for a while.

COHEN: Is that still happening?

WESTPHAL: No, because one of the things they later took up for the first repair mission was new solar panels. They fixed those in kind of a neat way, so they didn't have to build new ones. But the next time they go, they are going to put in ones that are solid. These first panels rolled up like a windowshade, which seems to me to be a really tough way to do things.

COHEN: When were you aware of the fact that you had real problems with the telescope?

WESTPHAL: There was a rigid procedure set up to focus the telescope. After they got everything working, they opened the front door. This was the thing that was to be done between Marshall and Perkin-Elmer—to get the damned thing in focus. And there was a document four inches thick with the steps they were going to take and the branches they'd take if something didn't work. So they started that process. Everybody was out there screaming to see the first-light picture. So they messed with it and messed with it for a long time. Finally they said, "OK, we think we know where focus is. We don't think it's perfect, and there's something wrong someplace, because the star is not as bright as it's supposed to be and we're not getting as much signal as we're supposed to get," et cetera. So there were a bunch of clues there, but the pressure coming from NASA headquarters to get a picture was very, very high. And we, at that time, had our team at Bowie State University, which is a small university that used to be a segregated black university just east of Goddard, next to the Bowie race track. We had rented space in that place. They had actually done a lot of renovation so that we could use it. So that's where our team was, as well as our computers and everything else. The action was all being controlled from Goddard, which was a fifteen-minute drive away. So headquarters decided to take a picture in spite of it all. We had been arguing for a week or more that the thing to do was to take a picture and see what the image really looked like, because it became more and more obvious that there was something wrong with the image. All indications led to that but nobody could imagine what it might be. There were all kinds of guesses. So there was a big argument between the NASA headquarters program manager and me over how we were going to do the first-light thing. He wanted it to all be done by government employees at Marshall and Goddard and at headquarters. And they were going to explain what the picture was, and why it looked that way. They would select what to do. And I said that I wouldn't have any part of that. There wasn't a person out there who knew how to interpret what the picture might look like, and he was just courting total disaster if he did that without somebody who knew what was really going on. I

remember I made four trips to NASA headquarters and Goddard in one day. Every time he would finally agree to let me do it, by the time I got back to Bowie State, why, he was on the phone and wanted me to come back, wanted to change it. So finally this did actually happen. My postdoc still at this time was Jeff Hester. And Sandy Faber was on our team by now, and her postdoc's name was Bob Light. He and Jeff ran the computers. I ran the microphone, explaining what we were seeing. Ed Groth was back in the place where the data was actually coming down, so that he could grab the mag tape they'd write and come immediately with it and stick it in our computer so that we could do the processing we needed to do. So all of that was done.

COHEN: So you really were the first voice to be heard on Hubble?

WESTPHAL: Yes. As soon as the first-light picture was in existence, there was a big scramble to get it to the press in a form they could publish. I didn't need to be involved in that part of it. There was a big press conference at Goddard, in a different building. So I left this TV room where we had the computers and stuff. Several of the team members were standing behind the cameramen, and they were looking at a TV screen on the wall which was displaying the pictures. They had comparison pictures of the same field of stars, which happened to be in the Southern Hemisphere—there was an advantage in that. And the idea was that we were going to see a ground-based picture that somebody from Santa Barbara Street had taken, and then we were going to put the ST picture right next to it and we would see this huge improvement. Well, it was an improvement, somewhat, when the picture finally came out, [but] we couldn't understand why it was like that. I started to walk out of the room, and Roger [Lynds] was standing there looking at it. He said, "Jim, that thing has spherical aberration."

COHEN: So he saw the problem immediately?

WESTPHAL: Immediately. And I said, “Roger, we’ve never seen an image from a set of optics that are supposed to be as good as these are supposed to be. So I don’t think we should panic yet, because this may be something strange about the way the focusing goes or something.” He said, “No, that’s spherical aberration.” So I went off to the press conference, and they were madly trying to figure out what this picture meant. There’s an absolutely wonderful video showing the top NASA PR person at the podium, and then this row of guys along a table—the biggest gun, and then the next biggest gun, and the next, and so forth.

COHEN: The people you call the suit guys?

WESTPHAL: Yes, right, the suit guys. The first question that the guy took was, “What does this picture mean?” And the top NASA guy, who was not the administrator but his deputy, turned his head to the next guy, and the next guy turned his head to the next guy, and it rattled all the way to the end of the line, where there was nobody. There was a chair down there, and that chair was for me when I got there. And then they just kind of came apart. I mean, they were so embarrassed. They didn’t know a thing about what they were seeing. And one of the guys said, “Well, it looks to me like that may be a star right there.”

COHEN: Meanwhile the whole world is watching this on television?

WESTPHAL: The whole world was watching this on television.

COHEN: That’s terrible.

WESTPHAL: Terrible, terrible.

COHEN: So did you go sit in your seat finally?

WESTPHAL: When I got there, why, then immediately the heads all turned toward me and said, “What is this?” And I discussed it. By then we actually had a little better picture up, which looked more like a bunch of star images. So I said, “This is the way it is. We don’t know what it all means yet. We’re working on it.” [Laughter] They didn’t like that worth a damn. But the press loved it.

COHEN: Do you mean because maybe something was wrong?

WESTPHAL: That we had a problem [was more exciting] than if it had just come out with this gorgeous picture. If you’ve seen one star, you’ve seen them all.

So as soon as we could, we got tapes of all the data that they had taken. They had taken several exposures. And we took them over to Bowie State, where we could work on them. Roger kept standing there saying, “It’s spherical, it’s spherical.” And they sure looked spherical. I couldn’t argue with that. Finally somebody went out and got a copy of the famous old optical book that was written back in the thirties. Jenkins and White, I think it was [*Fundamentals of Physical Optics*, by Francis A. Jenkins and Harvey E. White. New York; London: McGraw-Hill, 1937]. And in it we all remembered there was a picture showing a spherical aberration as you went through focus. So our conclusion was that we should do a focus run on the telescope and that they should quit fiddling around with all of their automated focusing stuff and find out—

COHEN: Just go directly to the telescope and see what was going on?

WESTPHAL: Yes, and see what really was going on. There were a million rumors floating around—not rumors, but a million do-you-supposes. “Do you suppose this is in the main telescope?” Or, “It couldn’t be in the main telescope.” Or, “How in the world could that

happen?" Or, "Maybe it's in the secondary of the main telescope." Or, "That is conceivably so, but maybe it's in WFPC 1 [Wide Field and Planetary Camera]." Or "Maybe it's in our instrument." There was only the other camera, but it hadn't been turned on yet, because pressure inside the spacecraft was too high and it would arc over. So there was a meeting every afternoon about what we'd do the next day in the testing—overnight actually. And we said, "We think the right thing to do is to make a focus run and see what it really looks like." "Oh, no, we can't do that, because what would happen, you know, wouldn't it be a godawful disaster if the focus motor somehow stuck permanently out of focus?" See, nobody dreamed that there was anything—well, we said that the problem looked like a spherical aberration and we thought [a focus run] was the easiest way to know if it was that or not. They said, "Oh, that can't possibly be, unless it's in your damned instrument." And I said, "Well, maybe it's in my damned instrument. But let's find out." And they said, "Oh, no, we can't do that. We've got to continue doing the steps in our four-inch pile of procedures." This went on for a couple of weeks. They did this test and that test and the other test. Finally they agreed to take a focus run. And they did. And it looked like the picture in Jenkins and White.

COHEN: It was exactly what Roger Lynds thought it was.

WESTPHAL: It was exactly what Roger Lynds thought it was. He was absolutely right.

COHEN: And there was no fixing it? It just had to be replaced?

WESTPHAL: Well, what you had to do was figure out some way to fix it. Once you came to understand that that was the problem, then you had to know if it was a spherical aberration in the main mirror or the secondary mirror. So an optical test was made to be able to separate those. And it was clear that it was in the primary mirror. So then everybody splintered out into great bursts of action in all directions. One of our team members, Tod Lauer, who had also been

Sandy's student in earlier times, now at Kitt Peak, had done some deconvolution. That's something the radio astronomers have to do every day; you can't do radio astronomy without deconvolution. He had somehow gotten into doing that on optical images from the ground. The next step was to get some images that were as near in focus as you could get from stars and then do a deconvolution, because you know the star is a point source, so you can play the game backward. Soon we were beginning to get pictures that looked pretty decent, actually. They certainly were a lot better than you could ever do from the ground, but they had this big halo of light around them. But I think—and there is some difference in memory about this—I believe that in that first discussion that we had when we got back to Bowie State, why, it was obvious that the way you would fix this problem in our camera—well, what could we do for our camera to fix it? Well, there was nothing we could do with the [one on the] telescope, but JPL was already building a sister WFPC 1—a clone, which was supposed to be identical to the original one. And that was because it was perceived, quite properly—it was the most important instrument on the telescope—that if it somehow failed, you would want to have a replacement that didn't take five years to build. So that was a low-level thing going on at JPL. It was something they could be doing that was useful while we were all waiting for the shuttle to go. So it was immediately obvious that the easy thing to do was just to change the shape on the little Cassegrain mirrors on the inside of each of the four—well, each of the eight cameras—so that they had spherical aberrations of the same magnitude but in the other direction.

COHEN: Put glasses on them?

WESTPHAL: Put glasses on them. And there was a bit of a subtlety, because Art Vaughan had designed this system. And just out of neatness, kind of, he had made the system such that there was an image of the primary telescope mirror being projected right onto the secondary mirror. That was exactly what you wanted, because now you could fix the thing essentially by measuring a star and making the secondary mirror's shape the opposite of the primary mirror but

scaled down to this little tiny size. And that's what was done; it was trivial, and it all worked perfectly well. But of course there was a lot of thrashing around.

There's one other facet that maybe we should talk about: the question of how it happened that ST had this bad mirror. NASA fiddled around for several days after it was clear the problem was in the primary mirror, without setting up a committee to find out what was really wrong. They named the chairman of that committee, I think, five or six days after the press conference when they announced the problem to the public. And it was Lew Allen [JPL director 1982-1991], who had been involved in this QEH thing—the problem we'd had internally with the WFPC 1. Lew's background, among many other things, was the black world. He was head of one of those agencies—I think one that you couldn't even know existed—the National Reconnaissance Office, or something like that. Anyway, a smart, smart man.

COHEN: Oh, I think everyone respects Lew Allen very much.

WESTPHAL: And when we were in trouble with the QEH thing, why, he played my game and, oh Lordy, he was so interested in this technical problem, because he'd had long experience with CCDs in the military, maybe three or four years before we even knew such a thing existed on a big scale. And he had also had a lot of experience with Perkin-Elmer. But, at any rate, when we were worrying about the QEH thing, he would appear unannounced in the office of the head engineer working on this problem at JPL, who was also a buddy of mine—a fellow by the name of Jim Janesick. That just caused endless problems, because he'd come in and sit down and they'd work on the blackboard, talking about, "Is it like this or this or this?"

COHEN: And they'd go to pieces?

WESTPHAL: The manager would sit down the hall, just dying out there. [Laughter] Anyway, the first I knew about what Lew Allen was doing was when I got a phone call—we were still on the

East Coast—at about three o'clock in the morning in my apartment. And it was Lew. He said, "Jim, what do you know about this stuff?" And I said, "I don't know anything about it, Lew. I never even heard rumors or anything." [Tape ends]

Begin Tape 6, Side 1

COHEN: Let's start with Lew Allen calling you up at three o'clock in the morning.

WESTPHAL: Yes. And the subject we are discussing is what was really wrong with the space telescope, and how did it come to happen. There's a report that talks about it, from the committee that looked into it. Lew was the chairman of this committee. As I mentioned, he called me at three o'clock in the morning and asked me what I knew about this. I said that I didn't know anything about it. So he said, "I'm up at Perkin-Elmer and I've been up here several days."

COHEN: Now that's in Connecticut?

WESTPHAL: That's right, Danbury. And he said, "I know quite a bit about this place." The unspoken thing is "because I used to run it." [Laughter] Not literally, but clearly they were building stuff for the military, stuff for the Reconnaissance Office, so he knew the people. He knew I knew that he had done that. I needed to find out whether the people who were working on a certain thing at Perkin-Elmer were really the best people Perkin-Elmer had, because it was holding up the whole project. Marshall would certainly not call up the director of JPL and ask such a question. And he said, "Tell me the names of the people who are working on this." So I did. I said, "Do we have the first team or do we have the fifth team?" He said, "You have kind of the first-and-a-half team." [Laughter] So I knew he knew an awful lot about it.

He said, "I went down into the lab and talked to so-and-so"—I didn't write the man's name down, I'd never heard the man's name; he was the guy who really put the thing together—"He showed me his log books on what happened." What happened is a little messy to try to describe, but it was one of those situations that happens to anybody that builds things. A precision piece had been made—a metering rod, like a ruler, but it was a specially made kind of thing, for use in polishing the mirror. And this rod didn't fit when it was to be put into a piece of test equipment. So they sent somebody down to the machine shop and said, "Take three-thousandths of an inch off the inside bore of this little cap and bring it back up here, and it'll fit." And that was a completely reasonable thing for them to do, except that they didn't check to see whether the cap was right after they had done that. They apparently didn't even look at it. This was at a time in the project when Perkin-Elmer was under tremendous pressure from Marshall Space Flight Center to get on with it and stop fiddling around—"Don't tell us about your problems," et cetera. In fact, Marshall was famous for having a center director at that time who didn't want to know about any technical problems. All he wanted to know was whether you were on schedule or not. Well, somebody had to tell him that, and somebody always told him that, and he knew damned well that it wasn't so—he wasn't a stupid man. At any rate, instead of checking this measuring rod after they changed it, why, they went ahead and used it. The result they got said that some other pieces of this device were the wrong length. They were spacer rods, and they were the wrong length by 1.6 millimeters. So they went out to the local hardware store and got four washers and lathed them down and put it together. They never tested it. But the thing that was really fatal was that this thing that they had to machine with the little cap had a hole in the center, and a laser beam was going to look through that hole and see the end of this rod inside. It had black paint over the outside so you could reflect the laser off this. And when they machined it, a fleck of that paint came off. And [now there was a] bare spot on the little cap, so that made the spacing 1.6 millimeters wrong. There was nothing wrong with the original spacer rods. And that's what caused [the spherical aberration] to happen.

COHEN: The whole problem of aberration came from that? It sounds like for want of a nail, the war was lost.

WESTPHAL: Precisely. It wouldn't have happened if they had followed the normal procedures, which were that every time something didn't fit, several people should have looked at it and decided why it didn't fit. Every time there was some irregularity in anything, why, you checked and you checked and you checked. But Marshall wouldn't have that. Marshall had a resident engineer up there [at Perkin-Elmer]. He claims he never knew about it. That's problematic, to say the least.

COHEN: Do you feel the fault lies with Marshall, not with Perkin-Elmer?

WESTPHAL: Fundamentally. They were pushing so hard that it was hopeless.

COHEN: Oh, dear.

WESTPHAL: Sad, sad business.

COHEN: So then how long did it take before it was fixed? They put your new camera in?

WESTPHAL: They put the new camera in from JPL. And then to be able to use the spectrograph, why, they built a quite optically elegant device called COSTAR [Corrective Optics Space Telescope Axial Replacement] that they plugged in, in place of one of the instruments—namely, the one from Wisconsin, the one that Bob Bless built. That literally did put lenses out in front of the—put glasses on it. In our case, it was [fixed with] the mirrors.

COHEN: So this must have been 1992 already?

WESTPHAL: Well, we launched in '90. I guess they launched the repair mission two years later, in '92.

COHEN: So then when did you go off this project?

WESTPHAL: After it was clear that everything was galloping along in the right way, there was not really any great need for us to all be on the East Coast. And by then I had been there two years, commuting back and forth. And Jean would come. We had an apartment near Bowie State, and then we moved up to Johns Hopkins, where the institute [Space Telescope Science Institute] was, and had an apartment close to the institute, and we commuted back and forth. I guess in the scheme of things here, the next thing that happened that might be of some interest was when I came into my office at Hopkins, which was down in the basement, a room with all the computers. My secretary here in Pasadena wrote out the classic yellow phone slips and she'd stick them on a sheet of paper and fax them to me. So every morning when I'd come in, there'd be a fax sheet with yesterday's stuff. On this particular morning I went to some meeting somewhere, I didn't go into my office at all. I got back at about three o'clock in the afternoon and there were several phone numbers and messages. There was one number that I didn't recognize at all, and I didn't recognize the name on it. I thought, "I wonder who that is." I didn't even know what the area code was, or the town. So I just called, and it turned out to be in Chicago. And the person on the phone said, "This is the MacArthur Foundation." I thought, "Oh, that's interesting. They're probably looking for an opinion about somebody." That occasionally happened; somebody would call me up and say, "What about this person?" But it was not some name that I recognized. So she hooked me up with this person. He said, "Are you Jim Westphal from Caltech?" And I said, "Yes, although actually right now I'm at Johns Hopkins." He said, "Oh my God, have you moved from Caltech?" And I said, "Oh, no. We're just here working on the space telescope; we have offices at Johns Hopkins."

“Oh, well, that’s sure good.” I scared him. Then he said, “I wanted to just tell you that you are now a MacArthur fellow.”

COHEN: Oh, wow!

WESTPHAL: And I said something unprintable, something that meant “No kidding.”

COHEN: That was in 1991?

WESTPHAL: Yes. Then I started apologizing all over myself for what I had said. [Laughter] He said, “I’ve heard everything. Don’t worry about it.” And I said, “Well, that’s pretty nice,” and who knows what else. He said, “You’ll get a letter in a day or two.” I had a big sense of skepticism that this was somehow—

COHEN: A hoax?

WESTPHAL: And I said, “Could you send me a fax of the letter?” He said, “Oh, sure. No sweat. I’ll do that. In fact, I’ll put the original in Fed Ex and send it to you,” and we got all of that straightened out. And then the fax didn’t come.

COHEN: So you thought it was a hoax.

WESTPHAL: Well, I really didn’t think it was a hoax, but that concern was certainly back there. It was about an hour and a half or so before the fax came. Of course, there was an hour difference in time. So by then it was past 5:00 in the evening. So I called Jeannie, who was in Pasadena, and told her this had happened. And then I called my son Andrew, who was up at

Berkeley, and told him. But they couldn't talk about it until the following Tuesday or something. It was on the thirteenth of June, which happens to be my birthday.

COHEN: What a nice birthday present.

WESTPHAL: A nice birthday present. So that came to be. It was a very nice thing.

COHEN: Oh, more than nice. Very special.

WESTPHAL: Yes. It was a good year for Caltech that year. Jacqueline Barton [Arthur and Marian Hanisch Memorial Professor and professor of chemistry] got one and Jim Blinn got one—Jim Blinn, who now is working for Microsoft. We lost a good man there.

COHEN: So what difference has that MacArthur made? You continued to be at Johns Hopkins doing what you were doing. Is that correct?

WESTPHAL: Yes, sure.

COHEN: And now I see your name on a lot of papers for the actual work that you were doing on the telescope. Has that continued?

WESTPHAL: Yes. And I actually still have a little ST money left. I haven't gotten the final papers, so I'm not absolutely sure, but apparently I can spend that money as a no-cost extension and pay myself with it.

COHEN: And the MacArthur's finished now? That was five years?

WESTPHAL: Oh, yes. That was a five-year thing.

COHEN: When did you actually come back from Johns Hopkins?

WESTPHAL: I don't remember, but soon after that. And then I just settled in, working on data. I managed to hide well enough. My office at that time was on a little side hallway and you couldn't see whether my office door was open or not from out in the hall.

COHEN: So you just wanted to look at the data and be a scientist?

WESTPHAL: Yes, right. And decompress.

COHEN: Now by this time Jim Gunn was gone?

WESTPHAL: Oh, yes, Jim had been gone for some time. Remember, this was now sixteen years after we started.

COHEN: Was Jerry [Kristian] still around in 1991? I'm trying to think when Jerry left us.

WESTPHAL: He left only two years ago. He continued to work on it. So did all the other team members. And until September of last year, I could still support them with the NASA money.

COHEN: So you had this group going here. Let's talk about the MacArthur a bit. What difference did it make in your life? It was a lot of money, but so what. You had enough money before then, I'm sure.

WESTPHAL: As I sat there after I made phone calls and stuff, why, I called Jim Gunn, who had gotten one [earlier], and I told him, “I detect the heavy hand of James E. Gunn in this.” He just kind of laughed—I could see him smiling. As always, I literally don’t know how it came to be. I can guess, but it’s irrelevant.

COHEN: Well, if you read the description [of the award] here, it’s a person who works on the edge and is innovative. Isn’t that who they look for?

WESTPHAL: Oh, sure, absolutely.

COHEN: You certainly fill the bill.

WESTPHAL: Yes, but somebody had to decide that that was a good thing to do, and I don’t know who that was. There are a number of candidates, perhaps. And I don’t really have any curiosity about that. I know now, having been involved in other people getting the MacArthur, that there are lots of people in the loop. They do a lot of searching, getting recommendations from people, so knowing who started it doesn’t really matter much. Well, as I said, when I finished the phone call I called Jim and said, “What do I do now, chief?” And he said, “Don’t do what I did. When I got mine, almost immediately I had a really incredible offer. I mean, an offer to do a really incredible thing. But before I could get my act together, the opportunity disappeared. It was way out of my field, but it was really a neat thing. Don’t do that. Go sit under your favorite palm tree and decide how broad a spectrum of things you are willing to do if you are given the opportunity. Think broadly. You’ll never have another such opportunity.” I thought that was really good advice.

So I came home in June. We had already been, a year or two earlier, to Fiji, New Zealand, and Australia, all on one trip. On Qantas, flying nonstop, all the good stuff.

COHEN: You guys were diving this whole time.

WESTPHAL: We were snorkeling, yes. My favorite place was a palm tree in Fiji. So we went back to that same place. It was actually a little bit of a disappointment, because the staff had changed, it wasn't near as neat as it had been the first time. Anyway, I sat down under the palm tree and started thinking about what I was prepared to do. Rather quickly I realized that it was an awfully long list and that maybe the better thing to think about was what I *wasn't* prepared to do. So I sat there and decided that I wasn't prepared to be the principal investigator of, or the director of, or the chairman of—and so forth down the line—certain things. So that was how that ended, except for the fact that soon after I got back I got a call from a guy I knew at the National Academy of Sciences who had been the NAS person associated with this committee that Gerry [Wasserburg] had put me on. And he said, “Jim, I need you to be here”—this was on a Monday or something—“on Thursday. We are going to have a quick meeting to decide the future funding level for all of the space exploration for the next twenty years.” And I said, “No.” And he said, “What?” And I said, “No,” and I told him the palm tree story. And he said, “That's neat. I will never bother you again.” And he hasn't. [Laughter] So that was a very, very real result of this. And things idled along nicely. I was hiding in my office. I used to hide in there for a good part of the year.

COHEN: Just working on your data from the space telescope?

WESTPHAL: Yes, that's right.

COHEN: Were you teaching again?

WESTPHAL: No.

COHEN: No. You were still on leave, in some sense.

WESTPHAL: Well, very few people knew that I was even here. I did a pretty good job of that. I didn't hide, but it was not widely known in the other two geology buildings that I was really here, they were so used to my being gone. So one morning somebody knocked on my door and I said, "Come in." It was Nick [Nicholas] Scoville [Francis L. Moseley Professor of Astronomy]. He said, "You're on the short list as director for Palomar." I said, "Go away, Nick," and I told him my palm tree story. And he said, "Yes, I know, but you've got to do this. You're the only person around here that's got any hope of surviving being director of Palomar. I mean, look what it did to Gerry [Neugebauer]." Of course, that wasn't mainly Palomar. That was mainly the LIGO [Laser Interferometer Gravitational-wave Observatory] business. Gerry didn't know when to split the job, it seemed to me.

Anyway, I said, "Nick, you don't want me to do this. It's out of my academic field." And he said, "I'm not asking you to be a mathematician. I'm asking you to go down and make Palomar work. You managed to make WFPC 1 work, you ought to be able to make Palomar work." So I said, "You guys think about it more seriously than you must have to do this." He said, "We've thought about it pretty seriously." And then he asked me if I'd be willing to meet with the search committee. I said yes, I'd be willing to meet, but that there was no hope that this was going to really work. I really don't want to do that. So I met with the committee. They were very friendly. I said, "You don't want me to do this. It's not going to be a good thing if we do." But everybody wanted it to happen. And I said, "I won't say absolutely no at this point. I'll ask a couple of other people about it. I respect their judgment a lot, I'll see what that leads to." And so I did. I talked with somebody I respect very, very highly in this institute. That person said, "It's a hopeless job, but you've got to go do it."

So I was very reluctant. Of course, Bob Sharp's monkey was still on my back saying, "You're supposed to decrease the research resistance." That monkey was right there.

COHEN: Then of course there was your big love, this telescope.

WESTPHAL: Yes, sure. So with great reluctance I decided to do it. I talked to Charlie [Charles W.] Peck [chairman of the Division of Physics, Mathematics, and Astronomy, 1993-1998]. I knew Charlie, not in a very deep way, but I taught in one of his classes a couple of times when we first started ST. He wanted me to do two or three lectures on ST and what it was going to be and all that kind of stuff.

COHEN: Let me come back a little bit, because the MacArthur is such a mysterious kind of thing. They want nothing from you. And I know you go to wonderful meetings in Chicago.

WESTPHAL: Well, the way it worked was that, as I say, I was on the East Coast when it happened. It was several days before I got back here. Dave [David J.] Stevenson was chairman [of the Geological and Planetary Sciences Division], and the word had spread here, so he set up a little party in our division. As I was walking from my office—the party was over in Arms, in a classroom—and as I was walking over there, I went by my mailbox and there was a letter in it and it said “MacArthur Foundation” on it. And it had a window in it with my name in the window, and I thought, “Gee, I wonder what this is.” So I opened it up and there was a check in it. There was no check stub, just a check—a machine-generated check. And I looked at it, looked at the number that was on it, folded it up, and put it in my pocket.

COHEN: Did you know how much money it was going to be for?

WESTPHAL: Oh, yes, it was public—\$360,000 over five years. So I went on to this party. And Dave said some nice words. He asked me if I had anything to say, and I said, “I really don’t have very much to say at this point. The thing hasn’t been very real to me until about ten minutes ago. Here, read what this piece of paper says on it.” He read, “MacArthur Foundation.

Pay to the order of James Westphal, \$18,750.” I said, “I believe it’s real now, folks.” [Laughter] And that was the quarterly payment. The only other time that I had something come from them was having to do with the health insurance that gave you a hundred-percent coverage with a one-dollar copayment for anything.

COHEN: That’s right. Many of the MacArthur people don’t have regular salaries or jobs.

WESTPHAL: There were two things in the original letter—when I got that fax and then the real letter. It mentioned that you could not transfer this grant to anybody else. You could not sell this grant, et cetera. And if you were to do so, the grant would be canceled. It also mentioned that there was health insurance that would be viable for the five-year period. My health insurance at Caltech wasn’t bad, so I thought, “Why should I do that?” So I called the guy up to discuss that with him, and also to ask him about the business of what he really meant. I didn’t quote the words exactly, but the gist of it was that the grant was to you personally and not to anybody else. Of course, if it’s to you personally, why then you can do what you personally want to do with it. It seemed to be kind of a conflict, and I couldn’t understand what that was really about. And he said that the reason they put it in was that in the early grants there were quite a number of people who, because it was so widely known, had employers who said, “Oh, that’s neat. We won’t have to pay you for five years. If you want to keep this job, that’s how it will be.” So [the MacArthur people] had to change it. And then I said that I wasn’t really sure that I wanted to take the health insurance, because if I got out of the Caltech plan I might have some difficulty getting back in, particularly if my health wasn’t real good right then. And he said, “The reason we do that is that a large percentage of our fellows have no health insurance at all, and they’re happier to have the health insurance than they are to have the money. That’s much more attractive to them.”

COHEN: But it’s just for the five years.

WESTPHAL: It's just for the five years, that's right.

COHEN: You do have to pay income tax?

WESTPHAL: That's right. They increased it enough to cover the tax from the original sum. The tax law changed before my time, so in my case it was \$360,000. Had that same grant been given before the tax change, it would have been substantially less than \$360,000. It got jacked up to take care of having enough money to pay the tax. The only other thing I get letters about is the announcement every eighteen months for a reunion, as they call it. It's a wondrous enterprise. The last three of them, I guess, have been held in what was the Niko Hotel in Chicago. It's now been bought by somebody else and it's not called the Niko anymore, but it's downtown, right on the river. They invite all fellows, and you are a fellow forever.

COHEN: I see. Even after your five years, you are invited?

WESTPHAL: Yes. They won't pay your airfare. You can bring a companion. They will feed you and put you up in the hotel. You get three meals a day, all done in buffet style. They have big round tables for, I believe, eight people, if I remember right. It's the style that you try not to sit at a table with the same bunch of people all the time. There's no rule about it—it's just a self-done thing. You have a name tag that doesn't tell anything about you except for your name. It doesn't even say you're a fellow.

COHEN: But everybody there is a fellow?

WESTPHAL: No, not everybody. Most people have companions with them. So Jeannie gets a tag just like mine.

COHEN: And they have a regular program?

WESTPHAL: No. Just meeting with all of these people. And they have five regular meeting rooms at the hotel. You can sign up if you want to make a presentation. There's a sign-up sheet on the door; if you put your name on it, that means you got it. People talk about the wondrous things they do. You sit down at the table and you start talking and you come to know what people do and who the fellows are, just inevitably in the conversation. But it's a win-win business. You never get bored at the dinner table or going to one of the presentations. It's such a diverse group of people. Everybody is first-order friendly.

COHEN: It must be fun.

WESTPHAL: Oh, yes. I wouldn't miss it for anything. I remember when I met Jackie [Barton] once after she was a fellow and I said something like, "I'll see you at that reunion, I hope." And she said, "Oh, no. I'm not going to go to that." And she doesn't. And then there's another activity that's associated with it, of which I don't understand the details. One of the early fellows is a lady who has used her money, apparently, to set up something called the Jefferson Institute, which is in West LA. I've never been to the facility—it may be her house, I'm not sure. She does have a staff, though, and she's an organizer. She organizes special MacArthur fellow trips, and she is incredibly good at that. There have been quite a number of them. Last year in June a bunch of us went to Turkey. We went to the Aegean on sailboats. We had thirty-seven people on three different sailboats, twelve to the boat. It was a wonderful thing. This spring we had a small group up at Yosemite of local fellows who were interested in the Ice Ages and what was going on in the evolution of polar caps and all of that. I took Jack [John Robert] Horner. He's the dinosaur guy—another MacArthur fellow. I guess it was three years ago now that he and I took another one of the organized trips. There must have been close to a hundred

fellows on the trip around Yellowstone. We meant to do dinosaurs, but it poured down rain and we couldn't do dinosaurs, so that was too bad.

COHEN: You did the geology part of it?

WESTPHAL: I did the geology part of it. I already had the video by then, so we sat in the Visitor Center auditorium at ten o'clock at night and showed the Old Faithful video. But because we didn't get to do dinosaurs, why, she and Horner organized a trip just a month ago to do dinosaurs for a few days.

COHEN: So even though your five years is up, you can continue to take these trips?

WESTPHAL: Yes. And you have to pay your own way. But somehow the MacArthur Foundation funds part of those kinds of trips. The next one is going to be in New York City on the second, third, and fourth of December, going backstage on Broadway.

COHEN: So now you're regulars.

WESTPHAL: Oh, you bet. I wouldn't miss it for anything. It's nice to be retired now, because my schedule is more open.

COHEN: That's right. First things first. [Tape ends]

JAMES A. WESTPHAL

SESSION 6

July 29, 1998

Begin Tape 7, Side 1

COHEN: When we left off last time you had just been cajoled—is that a good word?—into being director of Palomar. First of all, what does it mean being director of Palomar? What is this job?

WESTPHAL: Well, that's the number-one question I asked as we went along. I'd been observing directors for a long time, but of course each director makes the job to fit his mold, abilities, resources, and so forth. Each of the previous directors had different interests and priorities.

COHEN: You replaced Gerry Neugebauer?

WESTPHAL: Yes, that's right. And that was my very first problem. Gerry and I grew up together, as it were—as you may remember from the earlier part of our discussion. I consider him one of my best friends. And for whatever reasons—and I, of course, did not make any attempt to find out why—they just notified him. He had signed up for five years, I guess.

COHEN: He was finishing his third five years. [Gerry Neugebauer was director of Palomar from 1980 to 1994—ed.]

WESTPHAL: I think that's also correct, yes. And [for a time] he was simultaneously chairman [of the Division of Physics, Mathematics, and Astronomy, 1988-1993], of course. At any rate, they apparently called him up or something and just told him they were going to appoint a new director at the end of his term. And the term ended on March 30 [1994]. It was only a few weeks before that when they approached me. So I was under a fair amount of pressure to say yes

or no. If I said no, why then they'd go on about their business. If I said yes, there would be a high-pressure, high-level learning process. As I mentioned, when I talked to one of the people I respected very, very highly in this institute, this person said, "You've got to go do this."

COHEN: You don't want to say who that person was?

WESTPHAL: No. First I was interviewed by the search committee. That took twenty minutes or something.

COHEN: They had made up their minds?

WESTPHAL: Well, I don't know exactly how it was, but they said that I was on the short list. If the short list was one or more than one, I never knew. But it was clear that if they didn't find some problem with it when they interviewed me, they were prepared to propose to hire me. So I told them that if they were really serious about this, then the next step was for me to go talk to Charlie Peck, in order to understand what he thought this job was. Because that's the first question anybody asks. And you'd think that there have been many directors, so you'd surely know what the job was. But you don't, I'm finding. You don't know what the real job is. You know what a whole lot of the parts of the job are, but you don't know what the real problems are. So I went away and Nick [Scoville] called me in ten minutes—about the time it took me to walk back to my office—and said, "Go talk to Peck." So I did. I had known Charlie at some level for a long time, so this was not somebody I didn't know. I knew his style. I knew how open he was to discussions about things. As the history of that went on, I thought maybe he was a little more open than he should have been. [Laughter] That was his business, not mine. So he and I talked. I told him that I would like to have a letter of agreement between him and me—or whatever you might call it: a job description—that would specify what things there were. As an example, the director was also on the board of directors of Keck, so Gerry had three hats on, at least. And the

question was whether this was the way it was to be. It turned out that the original charter of CARA [California Association for Research in Astronomy], the formal organization, specified that the director of the Lick Observatory and the director of Palomar Observatory would be members of the board of Keck. Neugebauer was a member of the board, but it turned out that that was not specified—that he was automatically on the board. Caltech made that decision. Joe Miller was the Lick director. Joe I knew very well. That actually had quite a little bit to do with my accepting this job—that Joe was out there. At any rate, I drafted a job description.

COHEN: So [Peck] didn't give you a job description? You wrote a job description?

WESTPHAL: Yes. He said, "I think that's a great idea. Why don't you write a draft and we'll work on it." So we did. And he worked on it in a significant way. I had had some experience with that kind of a structure, in which people had fairly well-defined jobs. Given the sociology of the environment I was about to step into, I needed to have it all written down on a piece of paper that I could hand somebody and say, "See, it says right here that I make that decision and not you." Otherwise there would have been an argument over every little thing that went on, given the personalities involved. So [Peck] and I agreed upon that. He told me then that Gerry's term was going to run out, and that that was a natural time for it to come to an end. And I said, "Boy, you sure haven't given me much time. You know, I'm teaching right at the moment. I'll solve that problem somehow, which I probably can." So he said, "Why don't you and Gerry decide how to do this?" And I said, "Have you talked to him about it?" And he said, "No, but I will."

So I called up Gerry. I said, "I guess you are aware that I'm going to do this madness." He was just furious, he was absolutely furious.

COHEN: Do you mean that he didn't know that he was going to be asked to step down?

WESTPHAL: Well, he had just learned it. Apparently when I walked out of Charlie's office and headed off toward my office—

COHEN: That was the first they said to him?

WESTPHAL: That was the first they said anything to him, at least according to Gerry.

COHEN: This institute does these things so well.

WESTPHAL: Yes.

COHEN: He's very bitter.

WESTPHAL: Oh, he's terribly bitter. And this sure didn't help a bit. He said, "There's no need to have a discussion. I'm going to leave at five in the afternoon on March 30. The next day, you're it." I said, "OK, if that's the way." He said, "It's got nothing to do with you. It has to do with old history and all sorts of things around here." And boy, was he bitter about it. And he still is. Actually this happened in his house. I asked him if I could come over and talk to him about it. It may be that when I first called him and said, "I'd like to have a discussion,"—I don't know how that was—anyway, it doesn't matter. The point was, he sure didn't have very much time to digest it. As I was about to leave, he said, "I don't want you to get hurt in this crazy place. I'll be happy to help any way I can help. But you'll have to ask me. Otherwise I'm not going to be visible to you. I don't know what I'm going to do, but at any rate I'm not going to volunteer to do things. You'll have to ask me. But if you ask me and it's something that I can really do, I'll do anything I can." And I said, "That was the best I could have possibly hoped for. I'm sure there's going to be an immense amount of history." Because we were dealing with Cornell, and it was right at the time when JPL was becoming a partner, replacing Carnegie on

the 200-inch. It was at the time that the feds started deciding that they were going to take all of the assets of the California Institute of Technology, including the two Keck telescopes and Palomar and everything, and dump that all together and set the overhead rate at Caltech, so that we'd get a lower overhead rate. That festered along during my whole time.

COHEN: I don't understand.

WESTPHAL: Well, I'm just telling you the words, because that's about all I know about it, too.

COHEN: Do you mean that they wanted to reassess how they were charging you overhead on contracts for the government?

WESTPHAL: The government auditor, or financial person, wanted to decrease the amount of overhead money that the government was paying on rents and contracts. That's all based on the assets of the institute, which we are furnishing and which the government needs to reimburse us for. We don't work for free and we don't furnish free facilities for the government or anybody else. And that's a big mess. And, as I say, I just decided that it was nothing that the director of Palomar needed to be involved in, other than answering questions of fact and getting somebody to figure out how many nights of telescope time were spent by whom and when. Every time I'd produce something, why, they needed something more—they saw something branching. I finally told Charlie that this was madness. I said, "The trouble with this is that it's probably going to get dumped on you." And he said, "No, I'll fix that so both of us are somewhere else in the system." So it came to be that way. I believe I had less than two weeks to close off all the other stuff.

COHEN: In what state was Keck at this time?

WESTPHAL: Keck I was just about to go first light. There was no formal director of Keck at that time, because it was still under construction and they didn't need a director. That was the stance that Ed Stone [director of JPL] was taking. The only problem I had with it—and he had the same problem with it, which he recognized—was that it sure would be good to have a director there for the six months or year before he had to take over the telescopes and do astronomy with them.

COHEN: Was that Ed Stone's idea?

WESTPHAL: Well, I don't know whose idea it was. That was generally agreed upon—that it would be good to get a permanent director. So it turned out in the real world that they couldn't find somebody for director. There was a search committee formed. Several of us were on the search committee.

COHEN: Why was that true? Is that because someone would have to move to Hawaii and wouldn't want to do that? I mean, why should it have been such a problem?

WESTPHAL: A number of things. To my amazement, a lot of people didn't want to move to Hawaii, especially the big island. If this had been Honolulu, there would have been more interest. Secondly, this was correctly perceived to be a huge task, starting from scratch, without enough talent, and with a telescope that might work and might not. No one had ever demonstrated in any real way that it would work. There was a lot of skepticism about this new scheme actually working. So Joe Miller and I became—and there never was such a thing on any piece of paper—acting codirectors of Keck. [Laughter] We talked every morning. When they finally did get somebody in place as director—well, actually first we talked with Jerry Smith. But that was Ed's teleconference and that was once a week. But Joe and I talked to each other every morning, as it fit into the schedule. That was taking care of Keck at some level right at

that moment. So I didn't have an immediate Keck problem. I had internal problems—sociological problems—about Keck: Who was going to get to use it and who wasn't going to get to use it, and all of this sort of stuff. And one of the things—and I'm not sure it was written in so many words—was that the director of Palomar had no academic responsibility. It was a service job. It's a job to keep the trains running on time. So there were always overlaps in situations in which academic matters were overlapping or underlapping the director's job. The director's job could be viewed in the early days—in fact, that's how it's viewed to this day—it was all complicated in the really early days, but it was just a guy who got the telescope working right. Everything else was done by the academic folks—the selection of who got to use it, and that sort of stuff.

Now, the tradition had been—I don't really know why this was—that Ike Bowen was a dictator. It was his telescope and he would decide who was going to use it. And that, in many different facets and with different personalities, finally, ultimately, led to the divorce [between Caltech and the Carnegie Observatories]. At least part of that problem was when Allan Sandage tried to prevent Caltech from hiring Sandy Faber. The Caltech administration—I guess it was Jesse [Greenstein, Lee A. DuBridge Professor of Astrophysics, emeritus] at that point—

COHEN: That's right. They had an agreement that each could act on each other's appointment.

WESTPHAL: Well, no such agreement ever existed. That's what was trying to be made to happen. There may have been a gentlemen's agreement to that effect, but in any case—

COHEN: There was nothing written down anywhere?

WESTPHAL: Well, that's my understanding. Remember, I was totally peripheral to everything at that point. So you'll need to ask somebody else about how that really worked. At any rate, somehow the tradition was that the director owned the telescope time. I remember everybody

saying, “Wow! Boy, if you’re the director you can really keep these people under control, because you can keep them from getting telescope time.” Of course that wasn’t much of a tool to work with. That’s got pretty rounded edges on all sides and it’s greased. The last thing the director does is override the time-allocation committee. He appoints the time-allocation committee, so he can argue that through that process he controls it, but that’s not the way it works. And it shouldn’t work that way, because those decisions are academic decisions or scientific decisions and they should be made by people who are impacted by it. Maybe we’ll come to some of that later on. So, as I say, the Keck thing was [going along] pretty well after a month or so. Joe and I didn’t have an awful lot to do.

COHEN: Well, the construction was still going on, and there was a site to manage.

WESTPHAL: That’s right, and there were all sorts of crises associated with that, but they were engineering crises. And although Joe and I were both pretty good engineers, why, that was Jerry Smith’s business. Sometimes we disagreed radically with him about how to do something. He would get in trouble, as we all would. We always do in the real world of engineering. You forget something, or something doesn’t work as advertised, or whatever. So the next problem that was going on was one that—after it was finally all over I remember Tom [Baruch Thomas] Soifer [professor of physics] saying, “Jesus, Westphal, you’ve spent six months solving that problem.”

COHEN: Which problem was this?

WESTPHAL: Traditionally there had been cooks on the mountain at Palomar. And they had been resident people, usually, on the mountain. They were oftentimes husband and wife pairs, the husband doing one job and the wife being the cook. It was a wonderful system for many years, because Mary Brunsma was the cook. She was Dutch-Indonesian, and she made the most

wonderful food that anybody ever ate. There was nobody that could criticize her food.

[Laughter] After they retired, why, somebody by the name of Beulah—I don't remember her last name—became the cook. They had recruited her from the Baptist camp on the mountain. The only way she knew how to cook things was with charcoal on the outside or boiled. Oh, it was really awful. It was really bad. Through my ST years, when I wasn't going down there, there were various people there, and they kept trying to get good food. Everybody wanted Mary back. [Laughter]

Anyway, we had two cooks when I took over. One was Alicia Laura, whose husband was Luz Laura, who was the janitor and kind of a jack-of-all-maintenance-trades. Luz was a very good guy, who had been there from way back when—he was friendly and helpful in every way—but Alicia was a little hard to get along with. She just didn't work seven days a week, 365 days a year—another thing that people don't really appreciate until you have to make the payroll and realize that you have to have 1.6 people for every job in a remote environment like that. Pretty soon you get yourself into deep financial troubles if you don't realize that that's there. [Robert] Brucato was on top of that. And Brucato, just in passing, was extremely helpful in every way. Without him I would have never made it.

COHEN: Now, he was the administrator?

WESTPHAL: Well, he was the deputy director, or whatever title. He was an astronomer by training. He really liked to do that kind of work. And he knows where all the skeletons are and all that stuff. And then the other cook was a Native American, an Indian. She was a fairly young lady and very friendly. She apparently was a pretty good cook. Alicia hated her guts; I have no idea why. She'd poke at her and poke at her and poke at her until she finally poked back. And then Alicia would come running to the director or to Brucato and say, "See, she did this horrible thing."

COHEN: He said, she said?

WESTPHAL: It was that kind of situation. But by some mechanism a bunch of black hair cuttings ended up in food one lunchtime. And Alicia claimed that the other lady had done this. And the other lady denied that vehemently. It was pretty black hair. I suppose you can get black hair in lots of places. It wasn't brown hair. But you don't know, see? And you sure as hell don't want to accuse somebody of something without doing your homework. And because Luz and Alicia had been there for all those years, why, I felt that they had special treatment coming. So it became a really miserable business. I spent more than a half of each day for the first six months or so getting that problem finally solved.

COHEN: This was your first problem?

WESTPHAL: That was my first big problem. And I went down there and I dealt with our human relations people. I dealt with this and that and lawyers and so forth. You can't imagine how complicated and difficult it is to fire somebody these days. You can't just walk in and say, "You're fired," which is surely what I would have done, if I could have. I would have fired them both right on the spot. So in the end we bought them out—both of them.

COHEN: They both left then?

WESTPHAL: They both left. It's the only thing you can do. And I remember thinking I'd hear stories about so-and-so getting bought out. And I'd think, "Jesus, these guys are too lazy to work the problem." We worked that problem pretty hard, and in the end that was the only solution. And the only way we could make that happen with any grace at all was to farm out [the food service] to a contractor. We put big ads in the paper. Boy, we covered ourselves. You wouldn't believe it. All the local papers. And I offered to ask the superintendent, Bob

Thicksten, to advertise that he would give a presentation to people that were interested in bidding on this job—all the people together at one time on such-and-such a date, so that nobody could say that somebody else had an undue advantage. Alicia was so sure that she was going to get this job, and she didn't. She wasn't even close. She was nowhere close to being a viable bidder, because it had to do with the cleaning, the rooms—

COHEN: She just wasn't doing a good job.

WESTPHAL: And she really didn't ever recognize that she wasn't. And by this time it was clear that the Indian lady was not going to bid on it. In fact, she got transferred up here as a janitor for a while. I've lost track of her long since. She had a couple of young kids that were in school and she was anxious to get them into a good school. And personnel was very helpful with [at least] that part of the situation.

Anyway, that ate up my time on a tremendous scale. I had phone calls at three o'clock in the morning. Several a week, at various random times—screaming over the phone about how that so-and-so had done thus-and-so.

COHEN: Had Gerry had any problems like this?

WESTPHAL: Oh, yes. This problem actually started while he was still director. In fact the business with the hair had happened while he was still director, and he couldn't solve the problem either. I remember the Friday we finally got it solved. Saturday we had one of the Keck science meetings, a semiannual science meeting—it might have been the very first one of these. We were all over in this building and I saw Gerry up against the wall with his cane. I went over and said, "Well, I finally got the Alicia thing solved." He said, "I'm sorry to have left you with that."

COHEN: So anyway, there must have been some other things going on that first six months.

WESTPHAL: Oh yes, there was all kinds of stuff going on. Maybe an illustration of the tenor of the thing was that my first day on the job was April 1. My colleagues and friends at South Mudd left on my chair in South Mudd that morning one of these fold-up paper captain's hats. On each side of it it said, "April Fool."

COHEN: [Laughter] That's a cheery send-off, huh?

WESTPHAL: And I have it yet. I have it taped up in my lab, and every time I walk by, it reminds me not to get caught in that kind of a situation again. I offered it to Wal [Wallace L. W. Sargent, current director of Palomar] and he didn't think it was very funny.

Oh, backing up just a bit, in this memorandum of understanding, I agreed to take the job for three years instead of the normal five. That turned out to be one of the better decisions I've made. I notice also that Wal has taken it for three years. Maybe that will be the tradition. I don't know. Before I actually went to work I went around and talked to everyone on the staff privately, every one of the astronomers and engineering people, looking for what they perceived to be the difficulties—what we could do to make their job easier, more efficient, better, whatever. I kept careful notes of all that stuff.

COHEN: I bet you got lots of grievances, too.

WESTPHAL: From certain individuals, grievances were all I got. The only solution to the problems they were talking about was to fire everybody. [Laughter] But it was very useful and very educational. There had been an Observatory Council formed somewhere near the end of Gerry's time, essentially against his will, which had been kind of an internal revolt, because nobody could find out what was going on. This was a mechanism whereby the director should

report to the staff; originally it was once every six months, or something like that. I thought that that was a good thing, not only sociologically but practically. That's a way that you can have peer pressure around the table to make a decision when a lot of people around that table wish it weren't that way.

COHEN: Something else is backing you up.

WESTPHAL: Yes, and letting them understand why the decision is what it is, without spending all my time going to each one of them separately and forgetting to tell somebody one thing—whatever. But in the context, apparently, Gerry took it as being a massive affront to him. So the first thing I did on April first was to call an observatory meeting. And the place was packed. It was down in room 25 of Robinson, that conference room in the basement. And I walked in with my April Fool hat on. There were a lot of people in the room I didn't know, postdocs and all sorts of people. That was, I think, a good start actually. So I had worked with Brucato and picked up a bunch of things that had been going on for the last little while that people should know about. I mainly sat there and listened. People wanted to know what the plans were. I said, "It's way too early for me to understand. First I've got to understand what the job really is, how to do it, what we can do, how much money we have, where we are thin in resources to do the things we want to do. I would expect the Observatory Council to advise me on things that have implications for everybody or on things on which they're going to spend a lot of money to build a certain instrument or something like that. And I need your help. I propose that we do this once a month. Once we get started and we can see how this goes, why, we can decide to do more or less of it." And I instigated engineering staff meetings every Monday morning and various stuff like that.

COHEN: So you didn't necessarily continue what Gerry was doing. You had your own style of running it.

WESTPHAL: Yes, sure. Well, I had a different problem than he did. He knew what he was doing; I didn't have a clue what I was doing. [Laughter] I mean, I wasn't around at the time when he took that job. He may have gone through the same process, I don't know. The things I really needed to know were primarily historical kinds of things, it turned out, as we went along: What happened to this? How did this come to be? Where are the mud puddles that are arrayed around that problem? And so forth and so on. And Gerry was absolutely always right there and ready to tell me all kinds of things that I couldn't anticipate until I asked the right question. So I really appreciated that on a big scale. And I have done the same thing for Wal. I got out of sight. and—this is jumping far ahead, but again it was the thirtieth of March. [Laughter] So, at 5:00 on the thirtieth, why, I turned my keys in—except for my office key, which I needed—and at 7:30 the next morning I came with a crew from South Mudd and they moved me out of there. By 9:00 in the morning, why, my office was completely clean and ready for the next guy.

COHEN: Let's go back now two years and six months.

WESTPHAL: Yes?

COHEN: We've finally solved the cook problem—it was not satisfactory, but it was solved.

WESTPHAL: It was solved. And it has, in fact, worked out very well.

COHEN: A service comes in and just takes care of it?

WESTPHAL: Yes. And nobody believed this would work, because it turned out that the people who clearly had the best proposal were the Hare Krishna—the guys that wear the yellow—

COHEN: Oh! [Tape ends]

Begin Tape 7, Side 2

COHEN: OK, so you ended up having the Hare Krishnas run it.

WESTPHAL: The Hare Krishnas ran the restaurant on top of Palomar—the one right on top of the hill. They're vegetarians, see? So that was the big issue. They clearly knew how to run something. They had survived for a long time. They had a lot of free help, it turned out, that they could call on. So they could make a very attractive bid. And they were well-known all over the mountain to be really friendly folks. And they don't run around the mountain with their orange things on; in fact I'm not sure that this sect even does that. Their main office is somewhere down in Oceanside or somewhere.

COHEN: [Laughter] Did they give you meat?

WESTPHAL: The deal we made with them was that they could serve anything they liked and in whatever order, but that there needed to be meat on the table at the evening meal—and, you see, there's only an evening meal these days, the luncheon meal is long gone—but four out of the seven days there needed to be meat on the table. And they said, "That's fine" and they didn't have a problem with that. What's happened in reality is that every day there's meat, [but] they serve an awful lot more vegetarian stuff than meat stuff. The food's good. There's no funny business. It's worked out well.

COHEN: Now, these people also have to keep the rooms clean?

WESTPHAL: They also do the cleaning, yes. So in the end that all worked out OK. They live close by, I guess. I don't even know where they all live, probably not on the mountain. There's not that much room on the mountain.

Anyway, there were other things that were going on. And one of the things I asked Gerry to do at Bob's suggestion was to continue overseeing a new telescope control system for the 200-inch that Gerry had been very, very anxious to get in there, to get rid of all the old stuff, and have a modern control system. So I went over and asked him if he'd be willing to do that, and he said, "Oh, sure, I'd be happy to do that." As far as I know, he is still honchoing that task. It's taking him a long time to get it done. But one of the problems that we have is not enough people with enough talents to be able to support all the stuff that we really ought to be doing. Then there's a long trail, for the next three years now, of solving in an administrative way engineering sorts of problems. The other big problem that existed when I took the job, and I couldn't really believe it, was that they had delivered the low-resolution spectrograph—LRIS—that Bev [John Beverley Oke, professor of astronomy, emeritus] had been the PI of, and Judy [Judith G. Cohen, professor of astronomy] had been deputy PI. Actually, when I first came, Bev was still the official PI even though he had been retired, and Judy's original task was to take care of software. But Judy was very anxious to be PI. That was a big thing.

COHEN: She wanted to be in control of the whole thing.

WESTPHAL: That's right. And that's not an unreasonable notion. They delivered the instrument to the Keck Telescopes, and it really didn't work. And it was weird, because it didn't work on a telescope, but it would work on the floor down below the telescopes.

COHEN: So would this be a software problem?

WESTPHAL: Well, that was the question: What is this problem? I had never seen this machine. So in talking to Hal Petrie and Bill Douglas—Petrie is the Palomar chief engineer, Bill Douglas is the electronicer—they said that they thought the problem had something to do with the fact that when a certain printed circuit card had all the low-level signals, very, very tiny signals of three or four or five electrons, when that board was plugged into the chassis it was supposed to feed into, why, it was too long. So there had been a foul-up in the—

COHEN: Do you mean that the signal wasn't getting through?

WESTPHAL: Four inches of the board was sticking out in the blue sky. It should all have been totally encased. I said, "Boy, that's pretty bad business up there. It can't work like that." It was failing in the sense that you'd see these streaks all over the picture, like meteors had gone by, only it was clear that there wasn't anything like that. So I said, "Let's get the electronics back to Pasadena so we can work on it in some real way," because it's very hard to work on Mauna Kea. You don't realize what a bum you're being when you're up there. There are people who get really testy.

COHEN: Do you think that's the altitude?

WESTPHAL: Oh, there's no doubt that the altitude does that. And you do crazy stuff.

COHEN: So it was at 14,000 feet that they're working.

WESTPHAL: And it really is hard to work. For certain people it's absolutely hopeless.

COHEN: You can hardly walk.

WESTPHAL: That's right. Some people just can't navigate or anything. So we brought the stuff back and it was crystal clear that I could—well, the telescope has all these mirrors. Each one of those has a set of electronics that actuates the mirror, or tilts it in the direction it's told [to go]. And there are power supplies associated with that. All of that was implemented in a certain way to minimize the amount of heat generated, because that's what makes the images bad. And that particular kind of an implementation is called a switching regulator, or switching power supplies. It worked by putting out very sharp spikes something like 100 nanoseconds wide, at 50 kilohertz. And there were seventy-two of those [switching regulators] on the back of the telescope. And they were plugged right into the back of the telescope, right in among this stuff. And I said, "Boy, there's just no way. I mean, that's broadcasting like KCET." It was no surprise to me that we had this trouble. And when you put this instrument down on the floor, you weren't up there anymore. That's why it would work on the floor but it wouldn't work on the telescope. So we got signal generators and stuff in the lab, and I could reproduce exactly the effects and what was probably going on. The solution was trivial. We just made the box longer so we could seal the whole thing up. But that took a while, and there was a lot of pressure at that point, because we were coming up on first light and we sure didn't want to have the first-light picture look like that. So it seemed to work well.

But there was no sensitivity to that sort of a problem among the people who built the hardware. Once I said "Switching regulators," everybody's eyes lit up and they started saying, "Yeah!" It just hadn't occurred to anybody. The pro of the electronicers, Fred Harris, had run off, essentially, to the Naval Observatory in Flagstaff. It was just so hard to work here that when he got a job offer he took it. He had been an undergraduate here and Jim Gunn had brought him up. And I even tried to get him back.

COHEN: So he was a big loss.

WESTPHAL: Oh, you bet. I tried to get him back, but he wouldn't move back to LA, not after living in Flagstaff.

At any rate, we put the LRIS back on the telescope and it worked quite well. So I made several trips back and forth as we were getting all that to go. And of course that made life a lot more pleasant around the place, because the panic level [dropped]. And Bev went off to Canada and didn't come back.

COHEN: He seems to be very happy up there.

WESTPHAL: Yes. He still gets Keck time. That was one of the things that we agreed on in the observatory committee. There were some who didn't want to let anybody have any Keck telescope time who wasn't a full professor.

COHEN: Oh, I remember that.

WESTPHAL: "Certainly a professor emeritus shouldn't be getting any telescope time." But there was discussion. Everybody finally agreed that if a professor emeritus—Maarten [Schmidt] was just about to retire at this instant—

COHEN: And Marshall [Cohen].

WESTPHAL: And Marshall. So it was agreed that resident professors emeriti could apply for telescope time like anybody else could. And if they had a good proposal, and the time allocation committee gives them time, that's it. As far as I know it's still that way.

Anyway, a large number of things went on. I think the most important thing, from my personal view, that occurred in all of this was that I really had a tremendous amount of help from

Joe Miller. And these daily phone calls broadened out. When we first made contact on the phone in the morning, Joe would say, “Well, how are the cooks?” [Laughter]

COHEN: So you spoke to him every morning?

WESTPHAL: Yes. Well, not on weekends, but all weekdays. And he was immensely helpful in all sorts of technical, sociological, and management ways. Of course, he lives in a very different environment. But he, too—this is interesting, I didn’t realize this until we went up to visit last fall for a couple of months. He has no academic involvement. He’s not the head of the academic group. His director job is totally isolated, just like this one is.

COHEN: He does have to teach his wine course.

WESTPHAL: Oh, yeah.

COHEN: Marshall and I took that course.

WESTPHAL: Yes, that’s totally peripheral. That’s where his heart lies. He’s the only guy I know who randomly wanders around southern France, not speaking French, on his vacation.

[Laughter]

Anyway, Joe was just unbelievably helpful. I’d have never made it. I’d have fallen in one of these traps. And through one stage of the process I had three bipolars in the building. Manic depressives. They all managed to get in trouble one way or another. One of them was in the hospital for a while. One of them got sent back to Japan. I knew nothing about that [disease]. I knew the words, but I knew nothing about what it really meant. Judy [Cohen] brought me a book: it was a diary of someone—some physician, actually—who had personal experiences with that problem. That book was extremely helpful. But she [Judy Cohen] was

very good when I went in to interview her. I knew she was a very nonlinear person. You never could really predict what she might want to do. So that was a very enlightening thing. And it turned out that Charlie [Peck] hadn't really understood about it, either. So we were both reading this book at one stage, maybe a couple years ago. Charlie told me he had three more in his division.

What I started to say was that when I interviewed Judy before I took the job—I'd known her from the time she came here, as a grad student—she said, "I go crazy—I'm serious, now. I go absolutely crazy mad. I do crazy stuff. I can be dangerous. I can be dangerous to myself and I can be dangerous to you. It comes and sometimes I know—I'm under various medications, but I can't predict when it's really going to happen. You've got to be careful." Did you know Dimitri Mihalas, the theoretician?

COHEN: No.

WESTPHAL: He wrote a book about it in recent times. He has the same problem. In that book, he says that the only reason he didn't kill himself was that he was in such shape that he couldn't figure out how to shoot the gun.

At any rate, in the midst of all of this, good stuff was happening.

COHEN: Keck came on-line.

WESTPHAL: Keck came on-line. We got the 200-inch going even better. Gerry had done a lot of work on the 200-inch that improved the quality of the images. We followed that up by changing the 60-inch data room. The images got twice as good with the 60-inch as they'd ever been.

COHEN: And how did your new marriages work out, with Cornell and JPL getting time [on Palomar]?

WESTPHAL: Well, the Cornell thing went very well, strictly because of Jim [James R.] Houck. The only problem we had there was a problem he had, and that was that one of his staff members wanted to run Palomar, as well as everything else in the world. We kept getting all these suggestions by email about what we should do. There seems to be a fair number of [people who want to run things] around Cornell. At any rate, that all worked well. And in fact, we started having a monthly phone call.

COHEN: Between you and Jim Houck?

WESTPHAL: Well, with all three of us. One of the problems, and it was a very real one, was that this person at Cornell I'm talking about, or griping about, said that they didn't know what was going on. That's the truth; they didn't know what was going on. And so we started a monthly teleconference between JPL and Cornell and Caltech—and Palomar—about what was going on. Through that process, why, we made life a lot more pleasant for everybody.

COHEN: People like to know what's going on.

WESTPHAL: People like to know what's going on, and they had good ideas. So we could improve things. And the JPL thing went well and continues to do so, as far as I know. Their task was to build an AO system, an adaptive optical system, for the 200-inch. That's very nearly ready to go on a telescope. It makes the images much sharper.

So there were a lot of good things that happened during that time. We got a new faculty member whose previous position had been one in which he had to fight with the director every inch of the way. The director was always trying to take his office and lab away, and all kinds of stuff. When he got here, I guess he figured that with the reputation that Caltech has, he would be in even worse shape. And we were trying to help him get his lab set up and everything. Every

time we'd suggest anything, why, he'd be negative about it, until he thought about it for a while. But that, apparently, has worked out too.

COHEN: This is a new faculty member at Palomar?

WESTPHAL: Yes—Mark Metzger [assistant professor of astronomy]. A good guy. A really good guy. But you could see Don Hall's heavy hand on his back.

COHEN: So looking back at these years [as director of Palomar], what would you say were the real highlights?

WESTPHAL: Well, I think getting the LRIS going at Keck. We ultimately made some major modifications in the mechanical parts that were also not working right. That's the prime instrument at Keck during the dark time—well, actually all the way around the clock, and all the way around the month, since it's on Keck II.

COHEN: But would you say that by and large it was really a smooth three years, once you got the cook thing settled?

WESTPHAL: Well, it was never smooth, because of—

COHEN: Personalities involved?

WESTPHAL: Personalities involved. That's right. You always had to think very carefully about what you were doing so that you didn't fall in a black hole, and then you did anyway.

COHEN: So would you sum it up by saying it was a good experience, but three years was enough?

WESTPHAL: Oh, yes. I was ready for the end of it at three years. So that worked out well from my viewpoint. But I sure wouldn't want to—

COHEN: Do it again?

WESTPHAL: Not do that particular job again, that's for sure.

COHEN: So when you gave up the directorship you also then retired?

WESTPHAL: Well, yes, a year and a half later. I got out of the director's job a year ago last March. I retired at the end of June of this year.

COHEN: What do you see yourself doing now?

WESTPHAL: Oh, all the same stuff, only in a different environment. I'm fundamentally a person who likes to work by himself, in a way. On the other hand, most of the science I've done has been collaborative. But one of the things that went on somewhere in all of that was building a TV camera to go down into Old Faithful and take pictures of the Old Faithful vent.

COHEN: Now, were you doing that as part of a project? Or was this just for your own pleasure?

WESTPHAL: This was with the MacArthur money.

COHEN: Ah, OK. Is there anything else you'd like to say about the directorship?

WESTPHAL: I don't think so, really. It's fundamentally an unwieldy way to proceed, because the director always worries that he's going to step on the toes of the academic things, and vice versa. It's such a big job that it's hard, and I don't have any advice as to how you solve that problem, because a director needs a lot of independence.

COHEN: You have a difficult constituency.

WESTPHAL: You can have a difficult constituency, and if you do, you have difficult problems. I was heartened at least, in October and November of last year up at Lick in Santa Cruz. I saw how Joe [Miller] works, in detail, with a whole group of astronomers who were very much more sympatico. I don't know if that's the right word for it.

COHEN: Nicer to each other.

WESTPHAL: Yes, that's right. They weren't at each other's throat all the time.

COHEN: Caltech breeds some kind of competitiveness that's interesting.

WESTPHAL: Yes, and historically so. We select for it.

COHEN: It's not for the weak, or if you're waiting for someone to tell you you've done a good job. [Laughter]

WESTPHAL: Yes. [Laughter] Yes, that was one of the things. Judy [Cohen], even as recently as last month, was griping in the AAS [American Astronomical Society] bulletin. You don't go into these kinds of jobs building things with the idea that somebody's going to pat you on the

back. You do it because it's what you really want to do; you want to use it, or else you just want to build it just because you like to build neat things that people will use. I can remember a godawful thing. Many, many years ago, when we first got the four-shooter on the 200-inch, one of our senior astronomers was down there observing with it early in its history. He called up Jim Gunn at 3:30 in the morning—this was in the wintertime—and announced to Jim that he had to get in his car and get down there before dawn, because his damned instrument wasn't working right. Screaming and yelling at him, I gather. But good-hearted Jim said, "I can't get down there to do anything before dawn. That's only two hours from now. I can barely get down there at all in two hours. But I will come down to see what this is about during the day." And the problem was just that the guy didn't push the right button.

COHEN: That's terrible. But Jim went?

WESTPHAL: Oh, yes. When you're the designer of the instrument and it doesn't work, and there's nobody around that knows much about it—this was early in its history, and he didn't have much choice about it. I was off on an airplane somewhere.

COHEN: So anyway, did you want to come back and say something about the MacArthur?

WESTPHAL: You asked me the question: "How did having the MacArthur change things?" And I remember thinking, among the phone calls the day that I was notified about it, that this was really very nice but that it really wasn't going to change things. I had never been short on money to do things I wanted to do. I had never had anybody tell me I couldn't do something if I really wanted to do it. So [I thought that] maybe this money was wasted on me, and that there might be somebody else it could help an immense amount more. But who was I to make a judgment about that? Of course, that wasn't an issue—well, I guess I could have turned it down, but given the

amount of resources of the MacArthur Foundation, that wouldn't have been a very useful thing to do. [Laughter]

COHEN: Also it trivializes it for other people who it is important to.

WESTPHAL: Yes. So anyway, I did like Jim said—I thought about the things I wouldn't do anymore. And that was clearly a positive thing; it was a surprise to me. I could have done it without getting the MacArthur, but the MacArthur gave me a great excuse. But then, as time went on, I realized that it gave me a strange sense of freedom. I mean, it was not real, of course: I had had all the freedom that anybody could ever ask for, and more. But I didn't feel that was so inappropriate sometimes. And I've always lived, since I've been at Caltech, under this kind of shadow, in the sense that I didn't get to this place in the regular way. I'm very poorly educated in many ways that I shouldn't be poorly educated. I feel guilty about that, mainly because it keeps me from doing things that I should be able to do and I'd like to do. The MacArthur alleviated that feeling somewhat, and that was good.

So the next question was: "What did you do with the money?" Well, the first thing I did was put the money into some investments that were fairly safe while I was finishing off the space telescope. In the midst of that, why, I had done some work in 1984 with Sue Kieffer. She was a grad student in our division back in the seventies, I think. She married Hugh Kieffer, who was also a grad student at that time. They went off. I guess she was a postdoc at UCLA, as he was. And then they both went off to Flagstaff to work at the USGS with Gene Shoemaker and others. And then a random thing happened. I was walking toward the bookstore along by Millikan pond one day and she came from the other direction. I hadn't seen her for a while and I said, "Let's sit on a bench and see what you're doing and I'm doing and stuff." She had just finished doing a study of... She's kind of a fluid dynamicist in the end, really; she's much broader than that, but that's one of her real things. And she had just done a study of the fluid dynamics involved in the rapids in the Grand Canyon, and how that interacted with the erosional process. Good stuff, and

fun stuff. And then she said, “Bobby”—her son—“and I have spent the last two summers in Yellowstone, taking movies of Old Faithful, trying to understand how it actually works. We now believe that that flow, for the first eight or ten seconds, is supersonic. And we think we can even see, on some of our stuff, mach diamonds”—you know, the thing behind a rocket engine that's diamond-shaped, like this—they're shock waves. It's an indication of being supersonic. And she said, “In fact, the very best picture we have is Ansel Adams's picture of Old Faithful. And in that you can see those, if you know where to look. We are trying to get some good movies of them to see what their velocities were.” This was soon after Mount Saint Helens. That was in 1980; this was probably '83 or so. She said, “I'm trying to understand enough so that I can build a mathematical model of Old Faithful so we can see what could make it erupt. When does it erupt, why it has superlong periods and short periods, and so forth. It would be neat, though, to get some pressure data and temperature data down there in Old Faithful.”

COHEN: Ah, so the old oil engineer...

WESTPHAL: So the old oil engineer said, “Well, that's pretty trivial. Why don't we just do that? But you've got to convince the park superintendent to let anybody in this world put anything down Old Faithful.” She said, “Oh, I can take care of him. He's a good guy. Besides, we have Rick Hutchinson, the park geologist. He's got so much prestige and trust with the system, and I've worked with him now for four years, so that won't be a problem.” So we actually did that.

COHEN: You built something to go down into Old Faithful?

WESTPHAL: Just a cable that has to go into the water that's actually at the bottom. Before the eruption, it's 118° C, so it's pretty hot stuff. And then as the eruption occurs you end up with steam, which is 92° C because of the elevation. So it required some understanding of how to

handle stuff, and a lot of Teflon wire and stuff like that. And that worked out well. We got beautiful data.

We learned that the time to go to Yellowstone is the last week of October and the first week of November. That's Indian summer and there's nobody around and it's absolutely gorgeous up there—blue sky and white puffy clouds and all of that. Anyway, we did that for two years. We got a whole bunch of data.

COHEN: So you felt MacArthur paid for that venture? Or it just gave you the freedom?

WESTPHAL: Oh, no. This was before MacArthur. This was 1983. So she took that data and we worked on it and worked on it and worked on it, trying to understand what it was trying to tell us. And the pressure data especially was very mysterious, and we couldn't figure it out—and the temperature data was mysterious too. We just couldn't figure out what in the world was going on. And, you see, nobody knew what that thing looked like down beyond eight feet in the ground. That's what you can see just looking down. From then on you didn't know anything. The only thing you knew was that you could put a rock on a string and let it down and it would go down sixty-some-odd feet and then it wouldn't go any farther. And in fact somewhere around thirty-five feet, you sometimes have to bang it around a while to get it to go down, which we assumed meant that there was a crack or something. The thing at the top is a crack. We just assumed the crack got narrow and you had to find the right spot to get down through it. And all of this is an analog for volcanoes, where clearly they are not regular and you can't depend on when it's going. Secondly, if there's anything going on, the temperatures are infinitely too high to be able to work with. So this was about the best analog that we could probably find. So that went on and finally just kind of died out, because she couldn't make the mathematical model fit so that it would actually erupt.

COHEN: Too many mysteries in there.

WESTPHAL: Well, it was clear that a problem in the model was that she assumed it was just a cylindrical pipe, which was the natural thing to do, of course. This was saying that it was something more than a cylindrical pipe that was required to make it erupt. We were talking about it, and she said, "It would sure be interesting to know what the shape of that thing is down there." And I said, "I don't know. It's probably arbitrarily complicated, as most things are in nature." I know how to do it the way the oil well guys do it, with spring fingers on actuator ends and things that tell you how far out things are as it scrapes along the wall till you pull it back up. But I don't think we'd ever understand what we were seeing, and that assumes that things are roughly cylindrical. I said, "The real way to do that would be to be able to see it." She said, "Yes, it would be." And then along came the MacArthur. About the same time, why, I was watching a television program—on PBS probably, which is about all I ever watch. It was about some skydivers. There were six or eight of them, and they would dive out of this airplane and make a circle. And there was some guy above them, also a diver, who was clearly making this video. He was following them down. So when they got down to the ground I could see that he had some very small thing on his helmet. It was a couple of inches long. I thought I'd chase around to see if I could find out where that came from and what it was. Well, I didn't have to chase very long. Somebody knew about it. It was made in Japan. It was a little TV camera. So I had this MacArthur money and I could walk down to the Good Guys and buy a camcorder. And we could use that as a recorder. We bought the camera. It cost \$2,200, with its power supplies and everything, but I had to put it into an environment to keep it cool, obviously. It would burn up easily in any temperature over 40° C or so. So that was a straightforward and fun thing to do. Build a vacuum-insulated camera housing and put a little camera in it. And we went up there and put it in, and it worked. We got this gorgeous footage of what was going on in between eruptions. You've never seen this?

COHEN: No.

WESTPHAL: Well, it's sixteen minutes of video in which we made two trips down. We made one trip down and then got scared, when we got down near the bottom, that it would tear the camera into pieces. There was such wild and boiling stuff going on down there. So I pulled it back up quickly, before it got killed, and I contemplated what we should do. We finally decided, "To hell with it. We'll go down again, now that we know what it looks like, and see if we can figure out what's happening. If we do in the camera, it's \$2,000. We've got MacArthur money. We can build a new one." [Laughter] There was nobody to account to. You could never have gotten a grant for that—not in a million years. The video has been on television several times now, and a segment of it is in the Yellowstone Imax film. We actually went back the next year with a little bit bigger and better camera.

COHEN: Still MacArthur money on the bigger and better camera?

WESTPHAL: Oh yes, sure.

COHEN: Was she able to make her model then?

WESTPHAL: It turned out that we understood how it worked without a model. It's simple. It's the most amazing thing. Almost everything turns out to be more complicated than you originally imagined, but this is a situation where it's simpler than you had imagined. It's so simple that nobody would have imagined it would be like that, and yet once you know how these things are, it's absolutely obvious that it had to be this way. And it all had to do with the water being superheated by a significant amount. The pressure in the cracks and crevices, let's say down to fifty feet or so, is going to be whatever the weight of a fifty-foot column of water is. And the boiling point there is going to be whatever the boiling point is for that pressure of water. And

then if it starts coming into this open hole through the cracks and crevices, why, it's coming in there, it turns out... [Tape ends]

Begin Tape 8, Side 1

WESTPHAL: OK. The point here is that the water, once the thing erupted, why, the hole was open. So the boiling point is 92°C at that point, because of the air pressure and the altitude. But here comes this water in from the side, which is hotter than that by a lot. It turns out that by the time it actually gets into the hole—well, that's what I was saying when the tape stopped a minute ago. We were smart the second time. We put two thermocouples out in the field of view of the camera, so that we could see—

COHEN: Oh, at which point the pressure and the temperature were doing this?

WESTPHAL: Well, this was just temperature that we put out. Not the pressure, because the hole is open. So we recorded those two thermocouples by converting the little, tiny voltage that comes from the thermocouple into a frequency in which if the voltage increases, the frequency increases. And we recorded that on the two stereo channels of the video. And then when we played the video back, why, we had a thing that converted the frequency back into a temperature. So we could in fact find out exactly the temperature of whatever was going by the front of the camera. And what we discovered was, first of all, a big blast of water coming in from the sides. And we saw that in the first year's film. And we couldn't figure out really what it was we were seeing. But the minute that we saw that the water coming in front of the camera was 98°C —six degrees above the boiling point, and of course that's what's driving the water out of this thing in big blasts—and as the vent builds up, these cracks and crevices are everywhere present. So water's coming in everywhere like that, but it's all superheated by some amount. And when it

gets up to about fifteen feet, why, these blasts are enough to throw water out the top. Have you been to Old Faithful?

COHEN: Yes.

WESTPHAL: Do you remember how it blows water out for a little while, kind of randomly, before it actually erupts?

COHEN: That's right. It kind of bubbles.

WESTPHAL: Yes, it bubbles and gurgles. Well, that's the product of the blast of superheated water turning to steam and blowing that water out. And when the vent fills up to about fifteen feet from the surface, it begins to throw water out the top. And the process continues on and continues on and continues on, filling up a little bit at a time. Finally it comes to someplace, and it's not always in the same place, and a small steam and water blast comes. And in the meantime, remember, the whole column is full of water, and it's heating up down at the bottom now. And it's going to end up at 118° C down there. At the top, it's 92° C. Finally a large bunch of water is thrown out, which depressurizes the whole column suddenly, and the whole column starts boiling suddenly, because it's all above its boiling point. And away it goes. And that's all there is to it.

COHEN: And that's probably how a volcano works. It's probably the same story.

WESTPHAL: Some kind of the same sort of business. Yes. So that's what I did with the MacArthur money—I found out how geysers work. [Laughter]

COHEN: [Laughter] So the MacArthur people know this? Of course. You've been taking some groups to Yellowstone.

WESTPHAL: On that first trip to Yellowstone with the MacArthur people, it rained all the time. Adele Simmons, who is the president of the MacArthur Foundation, was there and saw this film. And I took it out of the VCR and handed it to her. They are very, very careful not to ask you to do anything, but they're very, very appreciative of any kind of information that MacArthur supported.

COHEN: It's a good deal. What other honors and awards have pleased you?

WESTPHAL: Oh, there are the standard ones that you get when you're doing a project with NASA—if it's successful. [Laughter] There also was one from the American Institute of Aeronautics and Astronautics, which is the trade organization for the aerospace business, recognizing the development of the CCD detector for spacecraft use and the like.

COHEN: You are in the American Academy of Arts and Sciences, I notice. So it's been a good run.

WESTPHAL: Oh, it's been a very good run.

COHEN: Is there anything you'd like to say about Caltech in general?

WESTPHAL: Well, it was a place that just fit, or I just fit into, at that instant. One of the reasons I wanted to look at the [Robert] Leighton oral history is that I have a memory that he was given a NASA grant for a million dollars a year, starting probably in '61, that he could use to do anything he thought would be an enhancement of the space business. So from that came Gordon

Garmire and X-ray astronomy—that part of the X-ray astronomy world. From that came Gerry down here doing infrared work at JPL. He supported Bruce [Murray] and me in the stuff we were doing. Harrison Brown [professor of geology, d. 1986] was also somehow in that loop.

COHEN: So you're giving Bob Leighton credit for getting all these things going?

WESTPHAL: Yes. He made only one mistake really, and that was that he organized something called the Astroelectronics Lab, with the concept that one could develop instruments for telescopes, ground-based telescopes, by the black-box technique. And he hired a PhD astronomer from Princeton named Ed [Edwin W.] Dennison to run that. Ed was interested in running things. They ultimately spent about a million dollars a year—not of Leighton's money but other money. I mean, they had all the money in the world coming through in those days.

COHEN: In the sixties?

WESTPHAL: Yes. Very, very little came out of that endeavor. There was one instrument that came out of it, which Bev [Oke] built, which was a wondrous thing. It had, I think, something like fifty photomultipliers that allow you to do photometry simultaneously at many wavelengths. But it took years and years for the thing to happen. So it finally got to the point where it was clear—by then [Horace] Babcock was director—that this [lab] was a black hole for money. Horace was more interested in other kinds of things, and everybody else was too, really, except maybe Bev. Dennison got into a personal problem that made it appropriate for him to resign. And I was asked by Horace to see if I could go make the [lab] work. In those days there were Observatory Council meetings, too. There was an Observatory Council meeting about what we should do. And I did a little bit of homework and was able to show that every major instrument on ground-based telescopes at that time, starting with the Condé spectrograph and on and on, had been developed by a very small group—usually one astronomer—who needed an instrument that

would do that and found some group of people that he could work with that would make it come to be. It was his thing, and he would push it as hard as it could go. And that was true of every instrument you could identify.

COHEN: Things that were too cutting edge to package out.

WESTPHAL: That's right. And I said, "I think that's a lesson that's real, and it means that those of you who want to have the instruments need to get involved personally and be really interested in what's going on, and not just come in every once in a while and say, 'How's it going?' Pay some attention to it, because you're likely to be able to make it work a lot better in the end than if you just turn somebody loose who may be ever so good an engineer but who doesn't have the background or interest to know how this thing should really be." You see, perhaps the fatal flaw was that they hired Dennison, who really wasn't interested in astronomy. He wasn't interested in the instruments as they applied to the tasks. He was interested in managing. Sometimes it's good to have a good manager, but not when you're trying to do something the very first time.

COHEN: Would you like to comment briefly about instrument building by people who are really interested? It's national money, so it has to be available to everybody. The problem is that it's your instrument and you're so busy building it. Sometimes you're the scientist, but you don't have time to do the science. And then you get scooped. Do you want to comment on that?

WESTPHAL: Well, I think it's a situation where you have to first recognize that that's what's going to happen. The world is not going to change just because you think you're mistreated. An example is the case of the wide-field camera. I didn't build a wide-field camera because I thought that people were going to pat me on the back. I knew damned well that three months after the first pictures, nobody would even know that I had anything to do with this, because it was the people who were analyzing the data who were getting all the publicity. That's the

natural way these things work. I don't think you can change that. But I see, as an example, Sandy Faber right now, doing exactly the right thing. She's the PI on this huge, big spectrograph that goes on Keck II, called Deimos. It's an instrument being built that should be able to take huge numbers of very deep spectra; it's kind of the next stage of Jim Gunn's big deal. And she's right in the middle of it. She has meetings once a week for her whole staff. While I was up there I was invited. She is such a quick study. That thing's going to be a wondrous instrument. If it had been just handed off to somebody to build, it wouldn't work nearly as well as it's going to work.

COHEN: But then when it's done everybody will have a chance to use it.

WESTPHAL: That's right.

COHEN: And who's going to get the data to work it out first?

WESTPHAL: Well, that's up to the University of California system, but she accepts that. I don't know—she may have some deal about that. First there will be a shakedown time, and there will be a lot of good stuff to come out of that. Gerry attempted to get guaranteed time for the instrument builders on the 200-inch telescope. Somehow it didn't work.

COHEN: People who are busy working don't have time for any science perhaps. [Laughter]

WESTPHAL: Yes. The situation in Robinson at the moment is that there's nowhere near enough people to do the engineering work, the construction work, the testing, and all that sort of stuff. I mean, you go up to Lick [Observatory], and there are people all around that place. And they only have to pay half of their salaries for those engineering people.

COHEN: And the state picks up the rest of it.

WESTPHAL: And the state picks up the rest of it—or Keck or somebody does. Now one of the things associated with the agreement between Caltech and UC is that each organization would get half of the instruments, or half of the value of the instruments. There would be competition for who got to build it. But over some indefinite amount of time, like over ten years, why, it should come out even. And we're way behind. And we're going to stay way behind, because we don't have the people to do the work.

COHEN: That's the big problem—finding the people.

WESTPHAL: And the only way you can do that is to have some source of money to finance it. Caltech, at some fundamental level, has to decide where to put its resources. And it's not that people don't know that this is a difficulty. It's a fact of life. Priorities get set as priorities get set.

COHEN: And probably the best bet in the next years is going to be biology here.

WESTPHAL: Well, I suspect that's largely true. Although, somebody told me that the first time [President David] Baltimore came onto campus—the day his appointment was announced—he met with the biologists and said, “If you think this is going to be the California Institute of Biology, you're thinking wrong.” [Laughter] Now, whether that really happened, I'm not sure. I was certainly not there. But I heard the story independently several times.

COHEN: So it's been a good run. And I'll probably have to do more of an interview with you in a couple of years, because you've done six or more instruments that are doing fantastic things someplace.

WESTPHAL: [Laughter] Right. OK. It's been great fun.

COHEN: Thank you.