

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH ROBERT LEE WALKER

BY SHIRLEY K. COHEN

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Robert Lee Walker

by Shirley K. Cohen

Session 1 March 14, 1997, Pasadena, CA

Session 2 January 16, 1998, Tesuque, NM

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COHEN: Welcome to the Caltech Archives. Tell us a little bit about where you were born, what your parents did, where you grew up, and your education?

WALKER: OK. I'm told I was born in St. Louis, but I don't remember that. I grew up in Winnetka, Illinois, a suburb north of Chicago.

COHEN: So shortly after you were born, your parents moved to Winnetka?

WALKER: Before I could start remembering anything. I was always in Winnetka. My mother's family was from Noblesville, Indiana, and she used to take the children down there for summers most years

COHEN: Was that a farm community?

WALKER: Yes. Her parents had a big family with many children, so there were a lot of her brothers and sisters in that neighborhood. They tended to be farmers. Her father had been in farming, but he also became a banker. He was one of those who started a bank. Anyway, back to Winnetka. I went to New Trier High School.

COHEN: Well, you mentioned your mother. How about your father?

WALKER: My father came from Missouri. His family, I think, came from Wentzville, Missouri, but we never went there. I didn't get to know my father's family nearly as

well. My father's sister was married to Albert W. Hull, who was a physicist/engineer at the General Electric Research Laboratory, in Schenectady. He's a well-known person; he became assistant director there. So that was a connection to some science. I met Albert W. Hull a couple of times, but just briefly. I never got to know him well.

COHEN: Did he open up the possibility of doing science as a career or anything?

WALKER: No. My father was an attorney in Chicago, so he commuted back and forth to Chicago. My brother, who is younger than I am, became an attorney, following in our father's footsteps. And I never considered being an attorney for one second.

COHEN: I see. Well, you must have been interested in science, then.

WALKER: I was interested in science. In high school, I took courses in the sciences. They offered biology, chemistry, and physics.

COHEN: So it must have been a reasonably good high school in the sciences.

WALKER: It was a very good high school. *It is* a very good high school. I took math courses. The teachers I remember most as being an inspiration were my math teacher—one or more of my math teachers—and a biology teacher. Certainly not the physics teacher. I took a year of physics and realized later, when I was in college, that I hadn't the vaguest idea of what physics was.

COHEN: High school physics can be a problem. [Laughter]

WALKER: Anyway, I graduated from high school in 1937 and went to Harvard. I had a scholarship from the Harvard Club of Chicago.

COHEN: I see. Now, how did that come about?

WALKER: Just because I had done well in New Trier.

COHEN: I see. It didn't have anything to do with family or genealogy or anything?

WALKER: No. I had a very good record in school. So that scholarship—I'm pretty sure I remember it correctly—was \$500 a year. Things were not very expensive in those days.

COHEN: Did the \$500 carry you through the year?

WALKER: It didn't take care of all my expenses, but my father contributed, and it was OK. I only stayed there one year. What I say is, I did not catch the disease.

COHEN: [Laughter] What do you mean by that?

WALKER: Well, all Harvard people think that's the most marvelous school in the world, and I didn't. My excuse has always been that I did not go back as a matter of principle. The principle has to do with what happened when I applied for a scholarship. The Harvard Club of Chicago scholarship was for only one year, and they expected that in your second year, if you needed one, you would get a scholarship from Harvard—some other kind. So I applied for that. And in the application, I guess as they always do, they inquired about my father's finances, and so we supplied the information. And then they had a question at the end, "If you do not get this fellowship, can you return to Harvard?" And I had discussed that with my father; I didn't just answer it blithely. After careful consideration or something, he decided no, and so I wrote "No." Now, when they looked over the financial data, they decided that the answer should have been "Yes," and they didn't give me a scholarship, and I didn't go back, because am I supposed to be a liar?
[Laughter]

COHEN: But obviously you didn't care that much about going back.

WALKER: Well, now you've hit upon the real reason. [Laughter]

COHEN: So your father just made it convenient.

WALKER: Yes. I had this highfalutin' excuse of a matter of principle, but actually I did not have a good time. I did very well academically, but I didn't do anything else, and I didn't enjoy myself. So I was not at all unhappy to not go back. In addition, I had gone there with the intention of majoring in biology, partly because the teacher of biology in high school had become a great friend and we used to go on field trips together, and so on. But slowly during the year I found that the course that interested me more was chemistry. I wasn't even taking physics. And then, even more slowly during the year, I learned that the part of chemistry that interested me most was actually physics. So then I formed the plan to switch into physics. Anyway, I don't think that had much to do with where I went. But I transferred to the University of Chicago and started to major in physics instead of biology. I spent three years there. Because of the fact that I had not enjoyed Harvard, I intentionally did all kinds of dumb things, like joining a fraternity. I took up skiing.

COHEN: I thought the University of Chicago had no fraternities.

WALKER: Oh yes, it does. It had and still does, I think. I took up skiing, which would have been a lot better from Boston than from Chicago. I did a number of outside things, and I enjoyed my life there pretty well.

COHEN: Did you live at home?

WALKER: No, I didn't. It's quite a long way; it wouldn't have been a good idea. So then I graduated with a BS from Chicago in 1941.

COHEN: And they had a very good physics department there, as I recall.

WALKER: Yes, it was quite good.

COHEN: Would [Enrico] Fermi have been there yet?

WALKER: No, he was not there yet. And so then I started graduate work at Chicago. I just barely started, because I was there for only one quarter. I started in the fall, and on December 7th the Japanese bombed Pearl Harbor. Actually, I had learned to fly a small airplane as part of a civilian pilot training program.

COHEN: While you were at the university?

WALKER: While I was at Chicago. So I decided I should go join the air force. I expected that probably I would not be a pilot but that because of my physics training I'd be in meteorology or something like that. Anyway, I told that to a couple of my professors, and they said, "Don't do that. Go see Sam [Samuel K.] Allison." So I went to see Sam Allison, and he hired me on the spot. And I joined the Manhattan Project.

COHEN: Right at that moment? Would it have been 1941 or 1942?

WALKER: It was January of '42.

COHEN: Was it called the Manhattan Project?

WALKER: Yes. A lot of the work had been done at Columbia. Anyway, they had just made a decision to concentrate a lot of that particular work at Chicago. And Fermi moved to Chicago from Columbia—Fermi and his group—very soon after I started work.

COHEN: Did you know the Goldbergers [Marvin L. (Murph) and Mildred Goldberger] there?

WALKER: No. They were not there yet; they were later. I did meet quite a few people. Anyway, I worked under Fermi's program. I wasn't too happy. Before I had even gotten into that, I had gone and visited their cyclotron and, boy, that seemed really exciting! I thought that maybe I'd go and operate the cyclotron. Well, I didn't do any physics at all. They put me to work building these graphite and uranium piles. So I spent a lot of time

drilling holes in graphite and pressing uranium oxide into lumps. I pressed tons of uranium oxide into little cylinders.

COHEN: Was there any danger from radiation doing that?

WALKER: There probably was some, but we paid no attention to it.

COHEN: People didn't have bad effects or medical trouble?

WALKER: As far as I remember, I didn't even wear gloves when handling all this uranium oxide. I took showers. These activities are very efficient in getting you dirty, especially drilling holes in graphite, and so I took a shower every day after work right there in the building. Anyway, these so-called exponential piles—their purpose was to test out the effectiveness of different lattices of uranium and graphite and test purities of the materials, and so on. So the only physics I learned then was from some lectures on the theory behind these measurements we were doing. After we made one of these little piles, we would put in a neutron source and some indium foils and then measure the activities in Geiger counters.

COHEN: So it was really a technician kind of thing.

WALKER: Very much so. And I would operate a crew of high school kids doing some of this pressing work with a hydraulic press. Well, eventually it got closer and closer to the time when Fermi was satisfied with how to make a big reactor that would be supercritical. But as that time approached, there became some uranium metal available. So far, we had used uranium oxide, and then a couple of places in the country started producing uranium metal. One of them was in Beverly, Massachusetts—a company called Metal Hydrides. So they shipped four people from Chicago out to MIT. Two of them were supposed to learn how to melt this uranium metal and turn it into ingots. I was supposed to run a crew of high school kids and press it into little lumps.

COHEN: Why the high school kids? Because it was cheap?

WALKER: Yes. Cheap.

COHEN: Or because the others had gone into the army?

WALKER: Yes. They just needed some labor.

COHEN: Was there security?

WALKER: No.

COHEN: Did they realize what they were working on?

WALKER: They had three crews, so we could work twenty-four hours a day. The head of one of the other crews didn't even know what that stuff was. I did. He was a fake metallurgist, and he kept wondering what this was. [Laughter] Anyway, they told me when I went there that it would take about six weeks. It ended up being six months in Boston. The idea was that this uranium metal was more effective than uranium oxide, and they would put that in the center of the reacting pile. Well, the idea of pressing the stuff into these lumps ended up a failure, because all of them burned up sooner or later. They just burned. Uranium is pyrophoric. When it's in a solid ingot, there's no problem. You can heat it to a red-hot state and it will just cool off. But the uranium powder pressed into lumps with a hydraulic press—when you took one out of the die and held it in your fingers, it would just get hotter and hotter. You see, I was doing that all the time, too—holding uranium metal in my fingers; that never caused any trouble. You wouldn't do that these days.

COHEN: On the surface there was too much oxygen there.

WALKER: If you hold it in your fingers, it gets warmer and warmer and warmer, and pretty soon it starts to glow. When I say it burned up, there was no big flame. It glows red and slowly turns into oxide. There's nothing to be done about that. If you put it on a cement-concrete floor, it sort of just burns up.

COHEN: I appreciate what you're saying. I am a chemist by training.

WALKER: Good. If you spray it with carbon dioxide to put it out, it takes the oxygen away from the carbon and keeps going. If you put nitrogen on it, it makes uranium nitride.

COHEN: Which is not very safe either.

WALKER: And if you put water on it, it takes the oxygen out of the water. So you just put it on the cement and let it go, and it turns back into oxide. Well, to prevent that from happening, when you take one of these lumps outside of the die, you put it on a cake of dry ice. That cools it down. And then you put it in a can with dry ice. These were sent back to the factory, where they were sintered—a heating process—and then shipped in a hermetically sealed box filled with argon. It did not combine with argon. And then it was sent back to Chicago. But I understand that sooner or later they all burned up.

COHEN: So it never was of any use?

WALKER: It was of no use. Meanwhile, however, these other two guys learned how to melt it. And once they did that—this was in a vacuum furnace with a graphite crucible and induction coil heating. When they learned how to do it, they went back home and left me to run a crew of high school kids to melt the uranium. So I did that. And that stuff eventually did go into the middle of the pile and was a success. But I was not present when that pile became supercritical, on December 2, 1942.

COHEN: Then what happened? You went back to Chicago?

WALKER: I never mentioned to you who the fourth person was that they sent back to Boston. The fourth person was an elderly man named William Jesse, and he was a physicist. He knew my father somehow; they both had this connection with Missouri. But that had nothing to do with any of this. He was sent back there to administer the other three of us.

COHEN: You mean the high school kids?

WALKER: Well, they were always overly optimistic about how much uranium they were going to get from this factory, so they had established three crews. I had the evening crew. And the material came in from the factory just before four o'clock, whereupon I and my crew would melt the whole thing—all of it. And pretty soon these administrators of this program began to worry that maybe someday they really would get more material, and if they did, the other crews were getting no practice and wouldn't know how to deal with it. So they asked me to slow down and not melt all of it. And I said I would be very happy to go home and not melt all of it and leave some for the other crews, but that if I stayed around there I'd melt every bit and work as fast as we could work, because otherwise it was too boring.

COHEN: You didn't fit into your slot, huh?

WALKER: Right. You'd think that would have been a pretty easy way to handle it, but they were not able to handle that—to just send people home when they were supposed to be working. They could not deal with that. So I kept going. We always melted every bit of uranium.

COHEN: So you did that for about six months?

WALKER: When that was over and the pile worked—the reactor—I went back to Chicago. The place was now in kind of a flux, because that meant that the Chicago job was about done. Of course, they also had the responsibility of setting up the Hanford [Washington] project and making that go. Anyway, there were these other sites. There was Hanford, Oak Ridge [Tennessee], and Los Alamos—projects X, Y, and Z. Los Alamos was project Y. I'm not sure which letters went with the other two. And I went around and found that some of my friends were going to project Y—we didn't call it Los Alamos. And I found out that that was in New Mexico and that these particular people were going there. I told them it sounded interesting and I'd like to go there, so they arranged that I could go there.

COHEN: Was it known then that you were going to be building the bomb? Did people know what they were doing?

WALKER: Yes, although at Chicago there was not widespread information about what was going on. It was obviously nuclear energy. But talk of a bomb was not anything that was generally discussed—of course, you have to remember that I was only a bachelor scientist. I wasn't in the upper echelon. When I got to Los Alamos, things were entirely different. Everybody knew what we were doing.

COHEN: Who was your superior when you got to Los Alamos?

WALKER: At Los Alamos they had a physics division, which was divided into groups. The experimental groups were centered on a cyclotron, some Van de Graaffs, and a Cockcroft-Walton machine. I entered the group on the Cockcroft-Walton machine. I was in a subgroup of that, a subgroup consisting of only two people. One was Alvin Graves, who had a PhD [from Chicago], and the other was myself. We had the job of calibrating neutron sources—radium-beryllium and other types of neutron sources.

COHEN: What was your feeling when you saw this place where you had now decided you were going to spend the rest of your life?

WALKER: Well, actually, when I first saw it I was disappointed, because I had thought it was going to be in the mountains. And it is, but not if your idea of mountains is Colorado or something. This is not really that kind of mountain. But then I gradually got to like it very much. Anyway, I went there. I arrived at Los Alamos in June or July of 1943. The project there had started only a few months earlier. I think it started in April of 1943. When I went there, the people at Chicago knew I was leaving [and] they said, "How would you like to carry a sample of plutonium with you?" And I said, "Well, that's OK." And so they said, "Fine. We're trying to get this sample out to Los Alamos, and we need a courier, and we might as well use you." So with that, I got a high priority [status] on the airplane.

COHEN: So you didn't take that long, slow train ride?

WALKER: I didn't take the train, I went on a DC-3. In those days, it had to stop about four times between Chicago and Albuquerque. And I carried this little sample, which I think was a tiny speck in a glass vial. I was met at the airport by a GI driver—an MP. So we drove up there. After a while, we became friendly and started talking. He didn't like it at all, obviously, because those guys thought they were going to a war or something and here he was, an MP out in the middle of nowhere.

COHEN: Guarding nothing.

WALKER: Guarding nothing, as far as he could tell. And there wasn't anything to do, and he wasn't at all happy about that. Then he said at one point, "You must be very important." And I said, "Why's that?" He said, "Well, when I started to come down here to pick you up, Mr. [J. Robert] Oppenheimer told me that if anyone comes up to the car looking suspicious, I should shoot them." [Laughter]

COHEN: So, you mean Oppenheimer was looking for this plutonium.

WALKER: Once I got there and was met outside the lab by somebody—it might have been Bob [Robert F.] Bacher, but I don't really remember—I gave them this sample. After that, I ceased to be important.

COHEN: Well, that was a nice ride out. So then you were at Los Alamos for the rest of the war?

WALKER: I was at Los Alamos until the end of the war. I left there at the end of 1945.

COHEN: Then of course you got to know Bob Bacher.

WALKER: I got to know Bob Bacher, but not well. He was head of the physics division, and then, later on, the so-called gadget division, which was the bomb division. I worked

on all these neutron standardizations for most of the time. But then toward the end I worked on the Trinity test shot, making blast measurements with piezo gauges. So I was down at Trinity and saw that. When the war was over, everybody was planning to leave and go here and go there, and I was planning to go back to graduate school. I guess the most important considerations for me in deciding where to go were that [Hans] Bethe and [Richard P.] Feynman were both going to Cornell.

COHEN: And you had gotten to know them?

WALKER: I didn't know Bethe personally. I had actually talked to Feynman on a few occasions in connection with a calculation I was doing. But I knew their reputations and had heard them talk. Of course, Bethe had a tremendous reputation, not only from the Los Alamos work. In those days, the main sources of information on nuclear physics were three large *Reviews of Modern Physics* articles that Bethe wrote, together with Stan [Milton Stanley] Livingston and Bob Bacher.¹ But all of the theoretical stuff in those articles was Bethe. Those were called the nuclear physics bible in those days.

COHEN: So did you have to apply at Cornell?

WALKER: No. I just—

COHEN: Went?

WALKER: That's an interesting question. I think I just went. No. I was accepted. But I think I did that through Bob Bacher. Bob Bacher was also going back there.

COHEN: So you said, "I want to go, too," and he said, "Come on"?

¹ H. A. Bethe & R. F. Bacher, "Nuclear Physics. A. Stationary States of Nuclei," *Rev. Mod. Phys.* 8, 82-229 (1936); H. A. Bethe, "Nuclear Physics. B. Nuclear Dynamics, Theoretical," *Rev. Mod. Phys.* 9, 69-244 (1937); H. A. Bethe & M. S. Livingston, "Nuclear Physics. C. Nuclear Dynamics, Experimental." *Rev. Mod. Phys.* 9, 245-390 (1937).

WALKER: I don't even remember how all of this happened, except that my friends who already had PhDs—my close friends, some of whom I lived and worked with—said, “When you go back to graduate school, you should use for a thesis your experiment on the absolute calibration of a neutron source,” which I had done [at Los Alamos] using a method suggested by Otto Frisch. It was a nice little experiment in which Otto Frisch kept coming around telling me to do this and that, but I actually did all the work. And so they said, “You should be able to use that as a thesis.” So somehow or other, in negotiating with Bob Bacher, I had an agreement worked out where that would be possible.

COHEN: So your thesis advisor was really Otto Frisch, and you had done the experiment already?

WALKER: Well, that would have been the case. It didn't turn out that way. Anyway, my memory is, for some reason, very vague about this. Exactly how I got the agreement—and as far as I know nothing was ever signed. But anyway, as a part of all that, it was just assumed that I would go to Cornell and be in the graduate school. I started graduate work then in, I suppose it would have been, January of '46, at midterm.

COHEN: Was Bob your advisor?

WALKER: No. Bacher did not stay there very long. Bacher went to the Atomic Energy Commission—he became a member of the first Atomic Energy Commission. And that happened very quickly, so I guess I signed up to be the advisee of Philip Morrison. I knew Phil Morrison and had a high regard for him—he was very intelligent. Also, I was sort of an experimentalist, but I had a strong interest in theory and he was a theorist. But I wasn't going to be a pure theorist, like working under Bethe or Feynman. In fact, the plan was that I was going to use this neutron-calibration experiment for a thesis, and Morrison would have been a good advisor for that. The first year I was there, I just took courses—a lot of courses—and that worked very well. When I was starting graduate work at Chicago before the war, I don't think I was learning very well. But now I went back to Cornell and I knew why I wanted to learn quantum mechanics. I had already,

from the lectures at Chicago and Los Alamos and so on, a fair amount of mathematical techniques.

COHEN: And you were more mature.

WALKER: And I was more mature. And I knew what I wanted to do. And so I just took courses, since I had my thesis in hand, so to speak. But then, toward the end of that year, I started snooping around in the laboratories, and I found very interesting work going on under Boyce McDaniel at the cyclotron. [This work] tied in well with a higher-energy interest. They were in the process of building an electron synchrotron there. This work had some connection with what you might do later on. That was all very interesting to me. So I went and joined McDaniel. He had a postdoc from Sweden working on a trial. It was an idea for a new piece of equipment—a gamma-ray spectrometer. And he just picked pieces of junk out of the junk pile and put something together. Then I joined, and we made a few measurements and determined that, indeed, it would work very nicely. That was just a preliminary test. So I decided it was a dumb idea to use this other thing at Los Alamos for a thesis. “I’ll do this,” [I thought]. So then I started working with Boyce McDaniel. We built the entire apparatus, which involved a big magnet and some electronics and some special counters. Then I took data at the cyclotron. It was a little tiny cyclotron, about the same size as my spectrometer. I operated that all by myself, really.

COHEN: It’s amazing what people did then.

WALKER: And then I wrote up the measurements. We found some interesting results on gamma-ray spectra—from a couple of reactions. I wrote up the results in a fairly thin thesis.² I did all that in about one year.

COHEN: And that gave you your PhD? You make it sound so simple. [Laughter]

² R. L. Walker, “Gamma-ray spectrometer measurements of fluorine and lithium under proton bombardment,” Ithaca, NY (1948).

WALKER: [Laughter] So I did this work—building all the apparatus, taking measurements, writing a thesis, and doing all my coursework and everything else—in two and one-half years.

COHEN: Now, I have you getting married in 1946, so you did something else besides. Where did you find time? Where did Dorothy find you?

WALKER: Well, we had met at Los Alamos, actually, and had already decided to get married. She went back to Cornell—preceding me by a couple of weeks, probably. When she arrived in this new, strange place without knowing anybody, there was Dick Feynman on the street. And housing in those days was impossible everywhere. But he said, “Well, there’s a room in the house I’m living in.” Some old woman had a house with a few rooms, not many. He said, “Come and you can rent that one,” which she did. When I got there, I rented some other room. And then pretty soon I was kicked out of my room. And then we got married and moved into her room. Of course, nowadays I guess, you wouldn’t do that.

COHEN: You wouldn’t bother with two rooms?

WALKER: You wouldn’t bother with the getting-married part. [Laughter] Well, anyway, everything was different.

COHEN: And there was another sense, after the war. It was different. How long did you stay, actually?

WALKER: I got my PhD in June of ’48. Then I went to work. I stayed there as a research associate, or whatever they called them, and worked on their synchrotron, which was still under construction. And during that year it started to operate. In fact, I was on shift the night we found the first beam. So I stayed there one year. I might have stayed there longer, but they didn’t— I thought I should become an assistant professor, and usually I never told anybody what I wanted or what I thought I should get, I just left them to figure that out.

COHEN: Sometimes people can't figure things out.

WALKER: Anyway, they didn't offer me anything other than what I already had. Meanwhile, Bob Bacher was leaving the Atomic Energy Commission and coming to Caltech as division chairman [of the Physics, Mathematics, and Astronomy Division]. He had known Lee DuBridge [Caltech president 1946-1969] from the MIT radar project, and DuBridge was already here. So I imagine DuBridge hired Bacher. You must know that. And Bob Bacher already knew about me from Los Alamos and for having arranged to do my possible thesis. And I'm sure he talked to people there, too. So he came through and offered me a job. He offered me a job as a—it was called a research fellow, I think. I still had this funny idea that I should be an assistant professor. So I guess, after I thought that over... **[Tape ends]**

Begin Tape 1, Side 2

WALKER: I phoned him. Maybe he was at Caltech [by then]; I think he was. He saw me at Cornell, on his way to Caltech, I believe. So I called him up and said, "I'm very interested in this, but I would like to be considered for an assistant professorship." And he said, "Well, let me think about that," or something like that. So then—now, this is really different from nowadays—it was a very short time that elapsed when he called me back and said, "OK, you can be an assistant professor." The way he did that—in those days—is that he obviously had to get approval from, probably, DuBridge. But the thing that would matter was this. He always consulted with a few people in the physics department. One of them, for certain, was Charlie [Charles C.] Lauritsen [professor of physics, emeritus; d. 1968]. And what other ones he talked with, I don't know. Maybe Charlie Lauritsen was sufficient, in my case. But Bob [Howard Percy] Robertson [professor of physics; d. 1961] was another one he consulted with. He had a few people he consulted—they were the ones who decided things in those days. They didn't have to have all these committees.

COHEN: Well, there were fewer people.

WALKER: Anyway, I said, "OK," and came out, and that's how I got started at Caltech. I arrived early in the fall of 1949.

COHEN: Had you been out to California before?

WALKER: Only once. When I got my degree in 1948, that summer we made a trip to California to see Dorothy's parents, not knowing that we were soon going to be coming back.

COHEN: Where was Dorothy from originally?

WALKER: Well, at that time her parents were living on the Newhall Ranch. It was at the Newhall Land & Farming Company, near Saugus. Her father was a bookkeeper and office person at that ranch.

COHEN: So she had grown up in this area?

WALKER: She grew up, I think, in Burbank. Yes, she grew up in Southern California. She was not born there, she was born in Pueblo, Colorado. Well, I think this is a good place to break it off. **[Tape ends]**

ROBERT LEE WALKER

SESSION 2

January 16, 1998

Begin Tape 2, Side 1

COHEN: Last time we spoke of all your activity up to the time you thought about coming to Caltech. Could you talk about how that came to be? How did the invitation come about and what enticed you?

WALKER: I got my degree at Cornell in June, 1948. Then I started working on the little synchrotron there that they had at Cornell. They were building a 300-MeV [million electron volts] electron synchrotron.

COHEN: Who was in charge of that project?

WALKER: The person who had been in charge, or who was supposed to have been in charge, was Bob Bacher. But he never really got started on that program. That was a program that started right after the war, after people went back to Cornell.

COHEN: From Los Alamos?

WALKER: From Los Alamos. And in those days, there were three electron synchrotrons in the country being planned and built, and they all had about the same energy, roughly 300 MeV. One was at Berkeley, where it was being done under Ed [Edwin M.] McMillan, who had invented the synchrotron idea. Another one was at MIT, under Ivan Getting. The third was at Cornell. Bob Bacher went back to Cornell in the position of head of the nuclear laboratory there [the Laboratory of Nuclear Studies], but then he didn't stay. He was there only a very short time.

COHEN: Could we backtrack just a minute? Could you briefly describe what a synchrotron is supposed to do?

WALKER: Well, the idea for a synchrotron was invented by Ed McMillan and independently by a Russian named V. I. Veksler. The reason that McMillan invented it was that he was from Berkeley, and, as everybody knows, E. O. Lawrence had a big program there, building one machine after another. These were cyclotrons, because Lawrence invented the cyclotron. Before the war, Lawrence had plans for a much bigger cyclotron than had ever been built before. And I think he had even gotten as far as getting the magnet for it, or ordering the magnet for it. Some people realized that that big a cyclotron wasn't going to work in the same way that the lower-energy ones did. The trouble was that a cyclotron depends on a property of the frequency of a charged particle in a constant magnetic field, where it goes in a circle. The frequency is independent of the energy. So as it gains energy, it retains the same frequency. The radius gets larger, but the velocity gets larger and it compensates so that it will go around the circle in the same time.

COHEN: And what is the purpose of it?

WALKER: That was important for the operation of the cyclotron, because the cyclotron accelerated the particles using a radio-frequency field on the D, so-called. But it was a constant frequency. And that constant frequency matched the frequency of the particles going around—usually they were protons or deuterons, or they could be alpha particles or other things. But the problem is that if you try to build a machine like that at a higher and higher energy, after a while relativistic effects come in and the velocity increases a little more slowly than the radius, because, obviously, the thing can never go faster than light. So eventually, as it gets very high-energy, it would approach the velocity of light. And then the bigger the circle, why, the longer time it would take. And it would just get out of sync. So the cyclotron would no longer work as it had been.

COHEN: And what was the idea? What did you ultimately want to do?

WALKER: Well, the purpose of the machine, of course, was to accelerate protons, deuterons, or whatever to higher and higher energies. And the maximum energy of a given machine was dependent upon the radius of the machine, how big it was, and the

maximum magnetic field. Magnetic fields of iron magnets were pretty much limited. But they could go with some strain to, say, 15,000 gauss. The field magnitude is limited just by the way iron works.

COHEN: So the synchrotron was going to compensate for some of that?

WALKER: Yes. So a cyclotron would accelerate, say, protons to a certain energy, depending on the size. [Interruption in interview]

COHEN: We were talking about the purpose.

WALKER: I was trying to explain what a synchrotron is and why it was important. The reason was that the way the old cyclotrons worked, with a constant radio-frequency acceleration, was limited, because of relativistic effects as the energy got high. E. O. Lawrence apparently didn't worry about that, or he figured he'd build this big machine anyway because somebody would figure out how to make it work. Anyway, when the work at Los Alamos had been sort of finished but Ed McMillan was still there, I think McMillan started thinking about how to make that cyclotron work. And he came up with a scheme whereby, in the case of that cyclotron, you would modulate the frequency of the RF [radio frequency] for acceleration. That is, you could no longer accelerate particles continuously with a DC current of particles. But you would inject a batch, and then that batch—

COHEN: A batch of particles?

WALKER: Particles. And they would go in and increase their radius as they got faster and faster. And then as they got so high in energy that the velocity increased less rapidly than the radius, you would start decreasing the frequency of the RF. Therefore, by keeping the radio frequency synchronized to the rotational frequency of the particles, you could remain in sync and the thing would work.

Now, it needed a couple of other ideas to do that in a stable manner. Otherwise it would be too difficult to get everything matched exactly right. But by letting the field

fall off—there are a couple of tricks, and we don't need to go into that. That principle would allow this big cyclotron, whose magnet Lawrence had already gotten, to work. That was then called a synchrocyclotron. Now, the synchrotron was kind of a cousin of that. A synchrotron works by having— Instead of having the entire region inside the circle of the particle orbit filled with magnetic field, like the cyclotron does—the cyclotron is just a big magnetic field, more or less constant; it falls off slightly with the radius—the synchrotron was made by having a doughnut-shaped vacuum chamber, with the magnetic field confined to that region. So it can become very, very large; it can be much larger than you would ever make a cyclotron. And the magnetic field is just where the particle orbits are, not inside the middle of that. But it's the same idea. But the difference between the synchrocyclotron and the synchrotron is that in the synchrotron the magnetic field is also changed with time, not just the radio frequency. The particles are injected when the magnetic field is very small. So the radius of the orbit is what it has to be; the particles have to stay inside that doughnut. So if it's a big radius and a small field, that corresponds to small energy. Then as it increases in energy—being kicked each time it goes around by the radio-frequency field, or electric field—the magnetic field now increases, so it has to keep a match between the radius, which remains constant, and the magnetic field value and the radio-frequency frequency.

Partly to test that idea and partly to build a useful machine, there were these three electron synchrotrons designed right after the war. The first one to finish construction was at Berkeley. The second one was at Cornell. But by the time the Cornell one was finished, the Berkeley one was still not working. They still didn't have a beam circulating. And when the Cornell one was finished—and by that time I was working on that—

COHEN: I was going to ask you when you started working on that. Was that part of your thesis work?

WALKER: No, it was not at all my thesis work. After my thesis, I stayed on in a research position—I forget what they were called at Cornell. I just stayed on there, working with the people I knew very well—my thesis advisor Boyce McDaniel and Bob [Robert R.]

Wilson and so on. And there were a lot of others—John DeWire, Dale Corson—who were on that project. When we started to look for the beam, we, too, had trouble. We were struggling away. The particles would be injected and they would go around a few times—you could tell that by looking at their traces; there were detectors to detect them and their signal was displayed on an oscilloscope and you could see a blip each time they went around. It sort of looked like noise, but you could see; we knew what it was. It would go around a number of turns but then get lost. I think it was Dale Corson who had the idea that maybe it was eddy currents in the iron produced by the fact that the magnetic field was increasing rapidly. A change in magnetic field induces eddy currents, and maybe those were causing problems. So he suggested that we reduce the so-called excitation of the magnet, meaning that the magnet still cycled and increased with time, but it didn't go to such a high field. So the rate of increase was reduced, the eddy currents would be reduced, and maybe it would work. Well, it was very soon thereafter that we did see that it worked. I happened to be on the shift when it first worked.

COHEN: That must have been exciting.

WALKER: Yes. The oscilloscope had this noise that suddenly went way out in time, much farther. So the beam was going around a lot farther. So that was exciting. Anyway, to get back to one of your earlier questions: Although Bob Bacher had gone to Cornell with the idea of being head of the lab, he didn't stay. And when he left, Bob Wilson was appointed. I think Bob Wilson had gone to Harvard after the war.

COHEN: Wasn't he at Los Alamos?

WALKER: He was at Los Alamos during the war, yes. Bob Bacher went to Cornell with the idea of being head of that lab and building a synchrotron. But when he left, they got Bob Wilson. He was at Princeton before the war, but then he went and got the Harvard cyclotron, which he took to Los Alamos. He was head of the cyclotron group at Los Alamos, using the Harvard cyclotron. None of that has much to do with me.

COHEN: So you didn't work on any of that at Los Alamos?

WALKER: I didn't work on the cyclotron at Los Alamos, or any of those things. I did my PhD thesis on— I don't know whether I told you this before, but anyway, when I was at Los Alamos I did an experiment with Otto Frisch. It was a very clever experiment. The cleverness was the idea of Otto Frisch; he thought of the idea. I had been in a tiny group consisting of Al Graves and myself. We worked to standardize neutron sources, find out how many neutrons were emitted per second from various sources—radium-beryllium neutron sources. But at the beginning, all we did was inter-calibrate sources: We'd say, "This one has 1.32 times as many neutrons per second as that one." So we were relying on the fact that somebody somewhere had measured roughly how many neutrons were emitted from one of these sources. But it wasn't very accurate, and we wanted to have a better value, and Otto Frisch had a great idea of how to do that. He had the idea, he talked to our little subgroup—or to John Manley, who was leader of the group. So we all decided to do that. I'm the main person who did it. I did the work, and Otto Frisch came around frequently and—

COHEN: —said, "How's it going?"

WALKER: And said, "How's it going?" And he bugged me [laughter], because he told me how to do things that I already knew how to do. So I was young and irritable. [Laughter] That's one of the things I've been sorry about: I've been sorry that I did not get closer and more friendly with Otto Frisch, because he was a great man.

Because this was a pretty good experiment, and I had done it more or less by myself and it was a nontrivial experiment, some of my friends there told me I should go someplace where I could use it for my thesis. So I went to Cornell, because Cornell had agreed to that, I think through Bob Bacher. I went there because of that. I signed up with Phil Morrison as my committee chairman, because he was familiar with things of that nature. I had a considerable interest in theory; I was thinking I wasn't going to do an experiment anyway, and it seemed like a good combination. Following on that, I took courses my first year at Cornell. I did nothing else. I just took courses. One of them was a lab course. It was in the second term of '46, because I went there probably in January or February of '46. Well, after that first half year, or first semester, when summer came,

I started prowling around in the basement looking in the labs to see what was going on, and I found Boyce McDaniel and a Swede, Guy von Dardel, doing an interesting experiment at the Cornell cyclotron, not the synchrotron. They had one of the first cyclotrons there; it was a very tiny one. It was probably the smallest one in the world on which significant physics has been done. This experiment involved building a gamma-ray spectrometer—a pair spectrometer. Gamma rays would make electron-positron pairs in a metal foil. The foil was in a magnetic field, and two particles would then start out on circular paths, and by knowing the sum of the radii of these two particles, you could determine the sum of the energies of the electron and positron, which was the energy of the photon, or the gamma ray. I had already been thinking that it might be fun to build a pair spectrometer, and I had had a couple of ideas, but I didn't have a good idea. When I went to see what they were doing, they were building one following an idea that Boyce McDaniel had, which I recognized immediately as a wonderful idea that would really make this thing practical. And so I asked if I could join, and they said, "Sure," whereupon I did. They had already started throwing together a trial model of this idea—just to test it—out of a magnet that was lying around and a couple of Geiger tubes that were lying around in the junk pile and whatnot. They put it all together and did an experiment. I joined them at that stage. So we got it assembled, and we did a little experiment, and it was great. So then Mac and I said, "Well, let's build a real one." And so we did. We sat down and we designed the whole thing and then we built it. Most of it, I built. By that time, I had decided that my previous idea of using what I did at Los Alamos for a thesis was a dumb idea. And anyway, I was interested in this.

COHEN: This was more fun at the moment.

WALKER: This was a lot more fun. The other one—it was a dumb idea. So Mac and I built that thing. This involved a big magnet about the same size as the cyclotron magnet. So these two things were side by side. Anyway, we built all the apparatus, including the special electronics for it and the special Geiger tubes and a special magnet—everything. We built it all, did an experiment, and I wrote my thesis all in one year. And my thesis was not very long, compared with what many of them became later. It was relatively

simple. We did some interesting measurements with that machine, and it worked just beautifully. That was finished, though. I got my degree in June of '48. I just stayed on. I liked working with these guys, and the synchrotron was interesting to me. But sometime during that year I started looking around for a job.

COHEN: You already had a wife by this time.

WALKER: I had a wife by that time and a baby coming along—planned to come after my degree. But somehow I had a peculiar idea, which would be considered exceedingly strange these days, that I should be an assistant professor by now. So I started—I don't even remember; I don't think I did very much—but I must have inquired at a couple of places, because I went to visit Princeton and [the University of] Minnesota. Minnesota is where John [Harry] Williams was. Maybe I visited Indiana, too. I forget. I interviewed for jobs.

COHEN: Now, did you come out to Caltech at that time?

WALKER: No. I guess I wasn't terribly interested in whatever I got offered. I did a couple of follow-up experiments at the cyclotron using this pair spectrometer; I ended up with about three or four papers based on that. So I was not only working on the synchrotron for my pay, so to speak, but I was continuing to do experiments with the apparatus that I had built and the cyclotron. One fine day I got a phone call, while I was sitting there running the cyclotron, and it was Bob Bacher, who happened to be, I think, at Cornell. Or he had just visited there. And he was sort of on his way to Caltech, and he wanted to offer me a job at Caltech. And so he offered me a job at Caltech as a research fellow. And he told me they were planning to build a higher-energy synchrotron than any of these three that were in existence. And I sort of liked the idea of going to Caltech. I didn't know anything about it, but I knew it was very famous in astronomy, and it sounded like a good place.

COHEN: Now, Dottie [Dorothy Walker] was from California.

WALKER: She was from California, and of course she would have been happy to go anyplace I wanted to go. So I replied to Bob Bacher and said I was interested but that I was hoping that I might get an assistant professorship somewhere. As far as I know, I had never said that to the Cornell people. And I'm sure the reason I didn't was that I figured they knew me very well. If they wanted to offer me a position as an assistant professor, they should be able to.

COHEN: It doesn't always work that way.

WALKER: It doesn't work that way, but that's always been my approach. If somebody doesn't give me something that's attractive enough, OK, I'll go somewhere else. And that's what I did. Because Bob Bacher got back to me in a few days—this was different than it is now—

COHEN: He made up his mind himself, essentially?

WALKER: Probably, but not solely. He contacted the people at Caltech. Now, that doesn't mean the whole faculty in physics. He was already signed up as the division chairman, so he had the most to say about it. He would be happy to make decisions of that sort himself, but he always consulted a few people. He consulted, I'm sure, Charlie Lauritsen and probably Carl Anderson [Board of Trustees Professor of Physics, emeritus; d. 1991] and a couple of other people. The thing that's different is that in those days, those guys could say, "Well, sounds good to me. Go ahead." So he goes and contacts DuBridge and says he wants to do that, and they did it. So he came back very quickly and said, "OK, we'll change the offer to assistant professor." So I said, "OK, that sounds good. I'll take it." So then I went to Caltech. I was then going to be sort of in charge of the experimental instrumentation for this proposed synchrotron. And Bob [Robert V.] Langmuir [professor of electrical engineering, emeritus; d. 1993], who was always known as Joe, was in charge of the machine. He came to Caltech [1948] as a senior research fellow in physics, for the purpose of being in charge of the design and construction of the synchrotron.

COHEN: This was in 1949?

WALKER: I went there in September of '49.

COHEN: So you didn't even come out to visit before you accepted this position?

WALKER: No.

COHEN: It was all done on the telephone?

WALKER: It was all done on the telephone. So the other people hadn't met me, either. I had gone to Princeton and a couple of other places to meet the faculty and be interviewed and whatnot, but it wasn't that way [at Caltech]. So I went out there and we started cogitating about the synchrotron. And then one of the things Bob Bacher did was raise a question about the energy they had been considering. This machine had been initiated probably by Charlie Lauritsen, or at least by the Kellogg [Radiation Laboratory] group—Charlie Lauritsen, Willy [William A.] Fowler [Institute Professor of Physics, emeritus; d. 1995], and company. They had been talking about building a 600-MeV synchrotron. That's twice the energy of these other three. Bob Bacher then raised the question, when he got involved, of what the energy should be. He thought that we should maybe go higher than that. So that drawing that you have from Russell Porter [associate in optics and instrument design; d. 1949] is of the projected 600-MeV synchrotron.

COHEN: So that wasn't even the machine you were going to build?

WALKER: No. That ended up being discarded very early, before any details had been worked out for it. So, Bob Bacher and I came roughly at the same time. He became the division chairman, but he also was director of the synchrotron laboratory.

COHEN: Now, there's something I don't quite understand about Bob Bacher. He left Los Alamos to come to Cornell.

WALKER: Yes.

COHEN: But you said he never really worked at Cornell after the war.

WALKER: The reason he left abruptly was that he was appointed to the Atomic Energy Commission, and that was the first Atomic Energy Commission. And he had no idea that that was going to happen to him. At the same time, he felt obligated to accept it, because he obviously had a lot of the background that would be very useful for that first commission. A lot of the other members of the commission did not have that background. That's why he left Cornell before really doing anything. He was there on the commission for a couple of years. Then he felt that things were under enough control that he could afford to leave the commission. DuBridge had by then gone to Caltech. DuBridge knew Bob Bacher from the Radiation Laboratory at MIT and hired him.

COHEN: I see. So he [Bacher] appointed you before he even got there.

WALKER: Yes, before he really got there. He was kind of on his way out there. I think we both went there that fall. He beat me by a few days, I'm sure. [Laughter]

COHEN: Where did you live when you first came to Pasadena?

WALKER: Well, we rented a house, a fairly elegant new house—I forget, the owners must have been going away for a year or something—in the hills over on the other side of Arroyo Seco. Anyway, it ended up that that cost too much; it was about \$125 a month, and that was pretty high, because my salary was not very high, it was very small. So that was too high, and we stayed only one year. And then we found a place in Sierra Madre that was a very nice place. This time it was not elegant; it was sort of primitive and very simply built. It was a small house in a big, huge yard filled with eucalyptus trees. That was about \$75 a month. So we lived there until 1955-1956. That year I took a sabbatical, and we spent it mainly in Italy.

COHEN: Let's talk about you suddenly having an academic position. So you had to do more than just your experiments. Did you have to teach?

WALKER: Yes.

COHEN: And how did you find that? Was this your first experience teaching?

WALKER: Yes. I think the first course I taught was a course for undergraduates—juniors—called math physics or something like that [Introduction to Mathematical Physics and Differential Equations—ed.]. I forget. It was a course in classical physics: mechanics, electricity, magnetism, and thermodynamics, I think. It was a course that had grown out of another one, taught by [William V.] Houston, a professor [of physics] who wasn't there any longer. [William V. Houston left Caltech to become president of Rice University in 1946—ed.] But he had written a book that was still used as a text for that course.³ It was Physics 6.

COHEN: And all the students had to take it—all the physics students?

WALKER: Yes, this was a course that all the physics majors took. It was mainly a problems course. And I remember that the person in charge of the course was Carl Anderson. Somebody assigned me to go work on that course.

COHEN: So you had a section of that?

WALKER: I had a section. And there probably were five sections or something. Because we had about twenty students apiece. The way the course was conducted was different from anything I had met before at Cornell. There were no lectures in this course. Mainly there were problems assigned out of that book. The class would come together and the instructor would call on different students to put their solutions on the board. So there might have been five or six problems for the day. And they would go up there and write out the solutions for these problems on the board and then talk about them. And the

³ *Principles of Mathematical Physics* (New York: McGraw Hill, 1934).

teacher would make comments about this or that or how this might be done differently or whatever.

COHEN: But then the students received no instruction before they had to sit down and do the problems?

WALKER: As far as I remember.

COHEN: They had to figure it out for themselves?

WALKER: [Laughter] Yes.

COHEN: Well, that may have been the Caltech way.

WALKER: Yes, it was. It was. There was this famous course by [William R.] Smythe [professor of physics, emeritus; d.1988]—Smythe's course [Electricity and Magnetism]. That was the terror of all of the graduate students, and it was sort of conducted that way. Smythe had written his book, and so on, and he had all these problems in his book.⁴ When I taught a section of Ph 6, I didn't give any lectures, but I talked to the students about some things, and then we did this problem-solving business. Anyway, that was a tradition at Caltech, to operate like that.

COHEN: But you had good students and they did it?

WALKER: And they were good students, yes. I remember one student I had in that course was Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus; d. 2011].

COHEN: [Laughter] And he could figure out the problems?

WALKER: [Laughter] Yes, he didn't have much trouble. I did that for probably a couple years. I don't remember all these things. Fairly soon, though, I think Bob Bacher wanted

⁴ *Static and Dynamic Electricity* (New York: McGraw-Hill, 1939).

to introduce a course that did not exist here at Caltech but which he knew about from Cornell. And that was called Mathematical Methods in Physics. **[Tape ends]**

Begin Tape 2, Side 2

WALKER: I had taken a course like that at Cornell from Dick Feynman, and that was a very interesting course, because, as usual, Dick Feynman would tell you about the way he solved mathematical problems, not the way it was written up in any book. Anyway, I was very pleased to have a chance to do that, and I had my notes from Dick Feynman's course. I did get several textbooks on the subject; there were quite a few. So I read these rather carefully and made up a course that was not just a copy of what Dick Feynman had done but that had some of the things I had learned from him in there, plus things I got out of these other books and so on. But it was not a math course, in the sense that it did not maintain what the mathematicians call rigor. It was a practical course in how to apply mathematics to physics problems. So it was not a physics course, either.

COHEN: Somewhere in between.

WALKER: Many of the examples of the use of the mathematics came from physics.

COHEN: Did all the students take this, or was it an elective?

WALKER: That was a rather popular course. It was primarily for first-year graduate students. It was also taken by seniors—quite a few of the seniors. And it was taken by quite a few engineering graduate students. The engineers sent them over.

COHEN: Now, were you the only one teaching this?

WALKER: Yes. I taught that a few years, not a lot of years. In fact, before too long, Jon Mathews [professor of theoretical physics; d. 1979] took over teaching that course.

COHEN: Now, didn't you write a textbook for this course?

WALKER: Eventually. But that was not early on, by any means. Yes, we did write a textbook. Jon Mathews and I wrote one, which is still in print, strangely enough, after many years.⁵ The way that came about was that I wrote up fairly detailed notes for the course for myself, and then when Jon Mathews taught it the first time, I loaned him my notebook. So he used that, plus he naturally built onto that with some of his own things. And then he went one step further. I think he had mimeographed copies, or whatever they were, of some notes that he made. And then eventually we decided to write a book. One would write one chapter and the other would write another chapter—or maybe the way it was was that whoever was teaching the course that year enlarged on those existing notes—

COHEN: So this really evolved over several years.

WALKER: It evolved. And Jon and I saw eye-to-eye on practically everything we wanted to put in there.

COHEN: But this was after some years. Meanwhile you must have been pursuing the research.

WALKER: Oh yes, all the time I was working, first of all, on the apparatus for the synchrotron. Also, this neat dichotomy of one person being in charge of the machine and one being in charge of the apparatus for experiments and so on—that dissolved quickly. And the group got bigger, naturally. Pretty soon we were all working on building it and making it work. One of the first people who was already in it before I was was Alvin Tollestrup [professor of physics, 1962-77]. I think he started on it just after he got his degree; he was a research fellow. But he started working with Joe Langmuir on the injector; he built a pulse transformer for the injector. And after a little while, Matt [Matthew L.] Sands [professor of physics; d. 2014], whom I had known at Los Alamos but who had gone to MIT and worked on their machine there, had to leave Massachusetts because of marital legal problems [laughter] or something. He wrote me a letter

⁵ J. Mathews & R. L. Walker, *Mathematical Methods of Physics* (New York: W. A. Benjamin, 1964).

expressing interest in a job at Caltech. I showed that to Bacher. Pretty soon we hired him [as a senior research fellow, 1950]. Anyway, we all worked on making the machine work and then doing experiments with it. So that went on all the time. At any one time, I only had one course. I taught various other courses, too, besides that one.

COHEN: Did you have to apply for grants for this work, or was the money just there?

WALKER: No. Bob Bacher provided the money from the AEC.

COHEN: So he was the one that did the grant.

WALKER: Yes, he did. And it wasn't much of a grant in those days. He got the money. None of us had to worry much about that, until later.

COHEN: Now, you must already have had graduate students working on this project?

WALKER: Yes. I had quite a few graduate students over the years.

COHEN: From these early times, do any of them stand out? Do you remember any in particular? Maybe they made their mark somewhere.

WALKER: Well, some of them have made their mark. One that I just read about the other day in *Science*, I think, is Mike Hauser [of Johns Hopkins University]. He's become a well-known astronomer. Gerry Neugebauer [Millikan Professor of Physics, emeritus; d. 2014] was officially my graduate student; actually, he did his thesis with me. What happened to Gerry was that he started to do his thesis with Carl Anderson in cosmic rays. I've forgotten. He had had a couple of unfortunate starts on theses that didn't pan out. I don't remember what had happened, but I do know that by the time he got associated with me, I felt—although I don't know whether he felt the same way—that he had done enough work on the usual things that a beginning graduate student does, struggling with apparatus and making this work and that work and trying this and that, that he just needed to get a thesis done as quickly as possible. And I had the apparatus and a very good

experiment. And I said, “Although this might not satisfy your desire to do the most original thesis, I think it would be a good idea for you to do this and get your degree.” So he and another student named Walter Wales did a measurement using the apparatus I already had in hand. It turned out to be a very interesting measurement, probably more interesting to me than it was to him. [Laughter] Anyway, he did then do a nice thesis⁶ and got his degree. And you know what’s happened since.

COHEN: He’s done very well. [Laughter] Very good. So was there anything from those days that you remember in particular? You had to be promoted and get tenure. Did that happen?

WALKER: Yes. Again, that was like the money: I didn’t worry about it. Bob Bacher seemed to come through at the appropriate times. Pretty soon I became [an associate] professor [1953].

COHEN: When did you go on your first sabbatical?

WALKER: My first and only was in ’55-’56. We went to Italy. We sublet the house in Sierra Madre to a theorist named [Yoichiro] Nambu, who was a very famous particle theorist.

COHEN: Why Italy, by the way? I know all the Cornell physicists were always going to Italy.

WALKER: Well, one of the first people who went to Italy was Matt Sands. And Matt was there the year before I went, or maybe two years. When he came back, he pressured me into doing the same thing. He went there and was in on the beginning of their design of the Frascati synchrotron [Laboratori Nazionali di Frascati]. He said how wonderful it all was there, so I decided I might as well go. It sounded good.

⁶ Neugebauer, Gerry (1960) “The ratio of negative to positive pion photoproduction from deuterium by photons with energies between 500 and 1,000 MeV.” Dissertation (Ph.D), California Institute of Technology.

COHEN: By then you had two children?

WALKER: Yes. Our children were six and four, I think, when we went to Italy.

COHEN: Did you live in Rome?

WALKER: I had arranged somehow to spend half the time in Rome and half in Padua. So we went to Rome and then—Dorothy thinks this was good, but I think it was a mistake—rented an apartment out in Ostia, on the sea. She didn't like the idea of living in town with these two children. Anyway, that meant a lot of commuting for me. And it was the coldest winter they had had in a long time. Our apartment was marble, which is a very good conductor of cold. [Laughter] And we didn't have any heat in there, except for one electric heater of 1.5 kilowatts. So it was a very cold, uncomfortable several months. That was a winter that was so cold that they had to turn the water off at the Piazza Esedra in Rome, because the water froze on the street and made ice. Anyway, then we went to Padua. There we rented an apartment in a brand-new apartment building. It was very small but—

COHEN: You had heat?

WALKER: It had heat and was very nice. I don't think I contributed much to the Italian programs. But anyway, we had a good time over there.

COHEN: There was a tradition of Cornell people going to Italy.

WALKER: A lot of the Cornell people went there, yes.

COHEN: So then you came back in '56?

WALKER: We came back, and soon after that we ran into a snag with our nice cheap little house, because the owner was selling the property. That was only maybe a year after we came back. By then, I had decided that I should buy a house, and we bought a house on

Dale Street, which is only a few blocks from Caltech. It was a dead-end street, and we were at the end of the dead end. It was a very nice two-story house. It cost \$17,500. I was a bit shocked, because by that time I had been there for some time. I had had my degree for several years.

COHEN: And you were a professor?

WALKER: I was an associate professor by that time. I had never worried too much about money, except I do remember that in those days if we had a \$25 expense during a month, that was out of the ordinary; it was a big deal. [Laughter] Anyway, I borrowed \$5,000 from my mother. But what shocked me was that if I hadn't been able to do that, then I wouldn't have been able to buy that house, and it only cost \$17,500.

COHEN: So the reality of your financial situation hit you?

WALKER: I learned that I was not in a very lucrative occupation. [Laughter] And these worries continued for a while. But after a while, my salary started going up faster. We had that house for ten years, and during that ten years my situation seems to have changed completely, because we then bought another house. I had been thinking of getting another house, up toward the mountains, and Dottie found this one. We bought a new van in 1967. Yes, I remember that—it was 1967. And what you do with a new car is you drive it around a while. So we were up there towards the mountains of Altadena, and Dorothy said, "Oh, drive down this street. I saw a house for sale." I had not ever mentioned to her that I was thinking of getting another house. We drove down there and it looked like a nice house. It was Sunday. So I said, "Call them up tomorrow and find out about it."

COHEN: This was on Rubio Canyon?

WALKER: Yes, that house on Rubio Canyon. You've probably seen it.

COHEN: Well, yes, I've been in that house.

WALKER: So she made arrangements, and we went out to see it. We liked it on the inside as well as on the outside, so we talked to the real estate agent. The only snag there was that the lot for that place had been subdivided by the owners who were selling it to us. They wanted to sell off a side piece where the boundary was, right next to the house. And one of the charms of the house is that it's all by itself out there in that debris basin. So I told her that what I'd really like to do is buy the lot as well as the house. Anyway, we ended up deciding to make a bid for the two of them. It was appreciably less than what they were asking. It was \$50,000.

COHEN: Things were cheaper then.

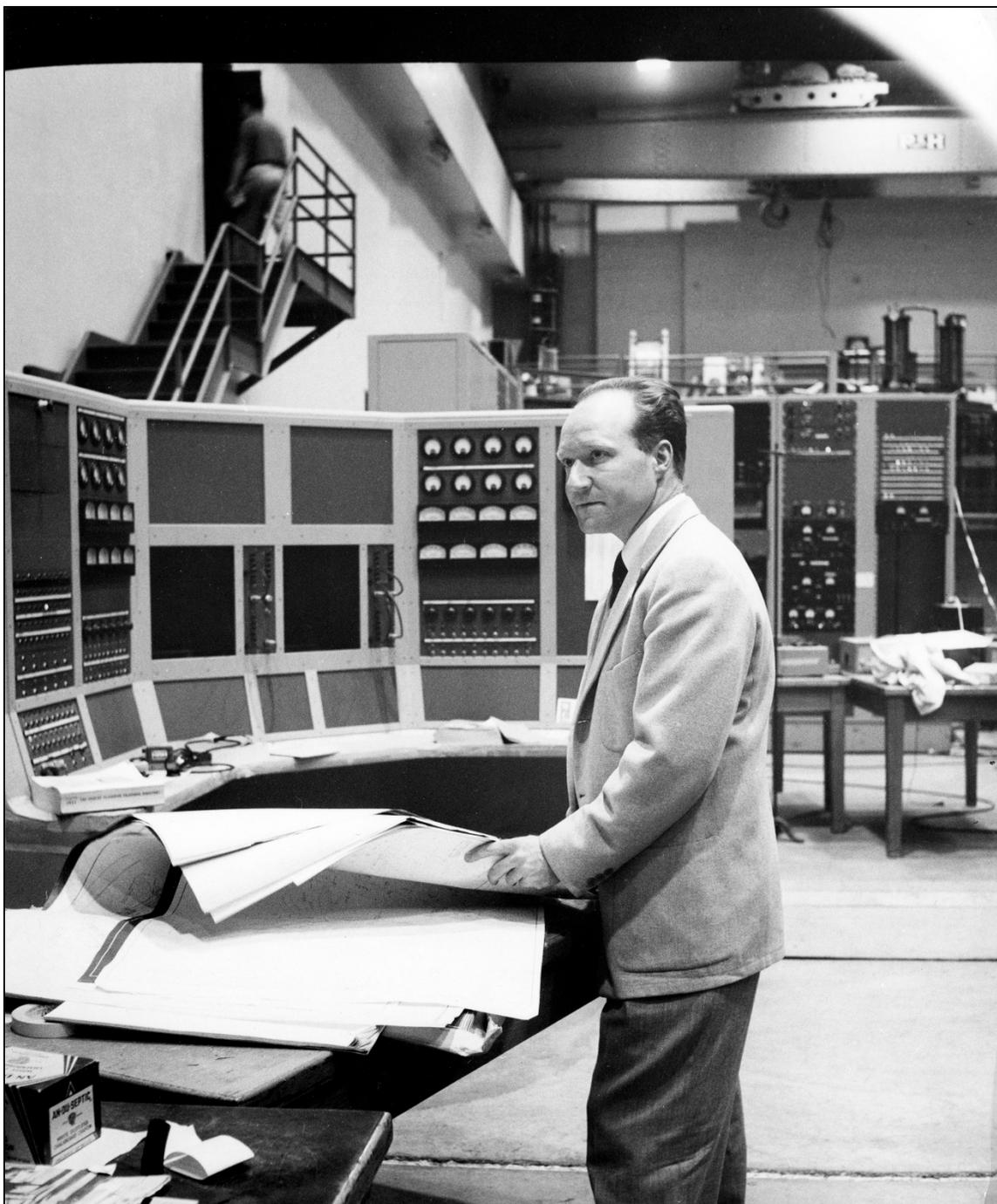
WALKER: But you know, from \$17,500 to \$50,000 in ten years.

COHEN: Ah, so you're just explaining how your situation had changed.

WALKER: I was unable to buy the cheap house, and ten years later I was able to buy the other one with a mortgage that I said, "Oh, make it ten years." The real estate agent was just shocked. We ended up with a fifteen-year mortgage, but I could have done a ten-year mortgage.

COHEN: Now, during all those years did you ever go back to Los Alamos to do research?

WALKER: I never went back to work or consult or anything at Los Alamos. I wasn't really interested in that kind of work, and I had a lot to do with what I was doing. I stayed at Caltech and worked on the synchrotron with a series of experiments over many years.



Robert Walker in Caltech's Synchrotron Lab, May 1955. Photo by W. W. Girdner.

And then eventually when the synchrotron was turned off [1970], I started working with Alvin Tollestrup and others doing experiments at other big accelerators, like the one at Fermilab.

COHEN: So did you have a group and propose an experiment? Is that how it worked?

WALKER: Yes. Alvin had a group. Caltech had about three groups that proposed three different experiments at Fermilab for the first round.

COHEN: And Bob Wilson was already the director?

WALKER: Bob Wilson was the director. We had made a study of a big machine like Fermilab's at Caltech. We were about the first ones to do that. That's something that should be in the Archives. **[Recording ends]**

BeginTape 2, Side 2

COHEN: You have something interesting you wanted to say about some of these searches that you've been involved in. Go ahead.

WALKER: I forget which year it was, but it was probably around 1958 or something.

COHEN: You were well ensconced with the new synchrotron?

WALKER: We were doing experiments with the new synchrotron. A fellow had come as a postdoc to Caltech—Alan Wetherell, from Europe. He was a Britisher. He worked for CERN [the European Organization for Nuclear Research]; at least, when he went back to Europe [1959], he did work for CERN. It was at the time when a new CERN machine called the Proton Synchrotron was just starting to work. So he sent me a letter describing the first results they got from the beams from the Proton Synchrotron. This machine worked at about 28 BeV, or GeV.

COHEN: BeV means?

WALKER: Billion electron volts, or giga electron volts—they're the same thing. "Giga" is, I think, the official term, but in the U.S. many people use "billion" instead of "giga."

Anyway, this machine ran at about 28 BeV or GeV. One of the most striking things he reported was that the fraction of antiprotons they were getting in the secondary beams was very much larger than what had been obtained at the previous machines at Brookhaven [National Laboratory] and Berkeley. So I passed this letter around, and I said, "Gee, it looks obvious that somebody should build an even higher-energy machine." So Matt Sands said, "Well, if you want to build a higher-energy machine, I've already designed one." [Laughter] So anyway, he then told us about something he'd worked on. He went on a summer study to a consortium in the Midwest called MURA [Midwestern Universities Research Association], and they were thinking of what new machines they might build in the Midwest. And Matt Sands had gone there for a summer study. He wondered what to work on. There were all these experts doing the popular studies of what they were thinking of building: a very high-intensity machine of some sort or else some colliding-beam machine. So he decided he would do something different, and that was to see how you might build a machine of a much higher energy than any that existed. His idea was to have what he called a cascade synchrotron. You would inject into one machine of intermediate energy and then use that as an injector into the very high-energy machine. The advantage of that is that by injecting into this high-energy machine at already fairly high energy, the aperture could be quite small. And that's where a lot of the money goes, because the aperture determines how big the magnets have to be, and so on. So by allowing a small aperture, by using a high-energy injection, you could save a lot of money; that would be very cost-effective. So we all thought that was a great idea: "Let's work on it and make a proposal." We had a very active program at Caltech for about a year, I would say. And we wrote many reports.

COHEN: That was you and Matt Sands?

WALKER: Yes, and Alvin Tollestrup. We were the three principal people, but we had a lot of others, including visitors. We had Hartland Snyder visiting from Brookhaven. We had quite a number of machine experts from other places. And when Berkeley found out about that, they decided that we should do that together—that was what Ed McMillan said. So we started having joint meetings. Anyway, that eventually developed into—

Well, a lot of other people found out about this, and they wanted to get in on it, so before too long we had big competition between various labs that wanted to build this big machine. Included were the two big labs, Berkeley and Brookhaven. And here we were at Caltech, a pretty small outfit. We didn't have huge support from the administration. So pretty soon we just sort of joined with Berkeley. In the end, Berkeley got a contract from the AEC to study this thing and make a design study. I remember we had many joint meetings with Berkeley and we didn't see eye-to-eye at all on how the thing should be built. I think Berkeley kind of blew it, in the sense that they wanted to build the machine and build it themselves, and they didn't want to cooperate too much with anybody else. As a result of that, they kind of lost it all.

COHEN: So then it went to Chicago?

WALKER: Eventually it went to sort of a national competition. And eventually, anyway, Berkeley did a big design study and came out with huge reports. Then there was a site competition—where it should be put. That's where they lost. Because by being kind of selfish, they lost support from the general community. In any case, there was a national competition for a site, and there were over 100 proposals. They were all visited. It boiled down to this and that, and finally it went to Chicago and became Fermilab.

COHEN: I see. So you were involved in the very beginning of that, just by happenstance.

WALKER: Yes, that's right.

COHEN: And then, of course, when Fermilab was built you started working there.

WALKER: Then I was in Alvin's group.

COHEN: Did he move to Fermilab? I mean, he left Caltech, I know [1977].

WALKER: Well, eventually he did, yes, and he's been there for many years now. And he's been one of the leaders in the huge colliding detector facility, which found the top quark and things like that.

COHEN: So then you became part of a user group, because you no longer were using the smaller machine at Caltech?

WALKER: Yes.

COHEN: Let me just come back to the general life and citizenship in the Caltech community. Was there anything in particular that you participated in? Any committees that were of interest to you?

WALKER: There was one period of my life when I was practically a professional committee person. [Laughter] At least that's how it felt.

COHEN: Which period was this?

WALKER: It followed the 300-GeV machine study that we did there and the general competition throughout the country of who should get to design it, who should get to build it, and all that stuff. Now, these were national committees. These were advisory committees, sponsored by the AEC. From those days I have quite a stack of letters, minutes, and things that went out. I thought maybe the [Caltech] Archives might want those—I don't, particularly.

COHEN: I'm sure they would.

WALKER: But anyway, it had to do with studies of the United States high-energy physics program for the next some years.

COHEN: So you might say that your work was really on the national scene.

WALKER: Well, I was on more committees of that sort than local committees. Although I was on a number of those. Among those national committees, I seemed to get on ones that were advising the AEC on phasing out accelerators. [Laughter]

COHEN: [Laughter] You mean you were on the death-knell committee?

WALKER: [Laughter] Yes, I was on the death-knell committee. I remember being on a committee that was looking at the work of the Argonne [Argonne National Laboratory] accelerator and what its future life should be. How to shut it down was what it amounted to.

COHEN: All that was very political, wasn't it?

WALKER: Yes, quite political. And then I was on a committee that was to decide where some colliding electron-positron machine should be. And there were proposals from Stanford and from MIT, at least. We were supposed to recommend where the AEC should support colliding electron-positron beams. That was very important, because they became of great importance to high-energy physics.

COHEN: And those things went to Stanford, didn't they, ultimately, and became SLAC [Stanford Linear Accelerator Center]?

WALKER: Yes. And I was on the committee at Stanford, at SLAC, that recommended which experiments should be carried out. I was on the program committee for Fermilab to recommend which experiments should be approved.

COHEN: And during all this time you continued to teach, because you were a professor, of course?

WALKER: Oh yes.

COHEN: So you didn't get too involved in local, Caltech-type committees?

WALKER: No. Not too much.

COHEN: The curriculum was OK as far as you were concerned?

WALKER: Well, I was on a committee for several years. What was that committee? It was not the admissions committee; it was related.

COHEN: Interviewing prospective students, or something like that?

WALKER: That's not what it was. It was interviewing students who were in trouble and deciding what to do with those students.

COHEN: UASH [Undergraduate Academic Standards and Honors committee], or something like that?

WALKER: Yes. Reinstating students who are flunking out or something. Or what to do about them—what conditions to impose in letting them back and all those things. And I was chairman—this is really crazy—I was, for a few years, chairman of the Caltech committee on computers [Computing Facilities Technical Advisory Committee]. Can you believe that?

COHEN: That must have been in the seventies. Or before the seventies?

WALKER: Yes, it was early. There were questions about which computer Caltech should get. It was in the days when those were very important questions for many people, because the big computing facilities were the institute facilities. It was before the days of these powerful workstations and things we have nowadays.

COHEN: In your career at Caltech, you went through several presidents. Did you see differences when, say, DuBridge left [1969] and Harold Brown came in?

WALKER: Well, yes. I don't know—I didn't have a great deal to do with any of them. But when we were trying to get support for this 300-GeV machine, DuBridge was president, and we didn't get a lot of support. As I told you, Matt Sands, Alvin Tollestrup, and I were primarily working on that, and we didn't get a big push from the administration.

COHEN: Bob Bacher didn't think it was—

WALKER: Bob Bacher, neither. Maybe they were correct. Maybe it wouldn't have been a good idea for Caltech. But in any case, we didn't. There was one interesting interaction with DuBridge. I was a member of the infamous Caltech Ten. You may not know what that is.

COHEN: No, I don't know what that is.

WALKER: Well, during the 1956 political campaign in which Eisenhower was the Republican candidate and Stevenson was the Democratic candidate, Stevenson gave a speech in which he pushed for the idea that it would be a good idea to have some discussions with the Russians about nuclear bombs and how to control those weapons and things of that nature. A few people who had some connection with bombs thought that that was a good idea. It wasn't a very radical speech. He was only saying that we ought to get together and talk about it and see what could be done to control this thing. Then somebody—I don't know how this got started; it was not any big deal as far as I could see—somebody wrote a letter to the editor or something like that. In any case, they composed a public statement in support of that speech saying, "We think this is a good idea. We should discuss this. We should try to control nuclear bombs, weapons." All I know is that Tommy Lauritsen [professor of physics; d. 1973] was definitely involved in writing this thing. And I was down at the synchrotron; I guess I was operating the synchrotron in those days. On weekends, we would operate it ourselves, in order to continue the experiment and so on. So I was sitting there, and Tommy Lauritsen, or somebody else, came around and showed me this letter. It sounded reasonable to me, so I

signed it. Well, it was a Saturday, and Tommy, or whoever was behind this, didn't make a huge effort to collect a lot of signatures, so they only got ten.

COHEN: Because he didn't try any harder?

WALKER: [Laughter] Probably he didn't. That was enough. He didn't try any harder. So somehow that got published in the paper, probably the *Los Angeles Times* [an advertisement in the *Los Angeles Times*, October 14, 1956.—ed.] And the trustees blew up. Lee DuBridge's phone started ringing with trustees. Now, this was not Lee DuBridge's finest hour, because he sort of folded.

COHEN: Really? He was mostly supportive of these things.

WALKER: No. He also agreed that this was a political matter and it was outside the competence of these professors—and they should stick to things they know about.

COHEN: I see. So he told you guys this?

WALKER: No. Well, he told the paper that. And the trustees, of course. That was the main thing he wanted to do—to tell the trustees.

COHEN: To show that he was in control of his professors?

WALKER: Well, I don't know.

COHEN: I see. But you didn't suffer any personal repercussions from this?

WALKER: No, no.

COHEN: But you felt that Lee DuBridge was not doing so good?

WALKER: Yes. The trustees wanted us fired. Apparently it was a serious matter for them. The person who inserted some calmness and sense into all this ridiculous thing

was Earnest Watson, dean of the faculty. He sort of talked DuBridge into calming down a bit. Nobody ever said anything to me directly as a result of this, but I heard that all these things were going on in the background.

COHEN: Well, you know, you can laugh at it now, but the political climate then was very different.

WALKER: Yes. And then the important thing—it's maybe the important thing—is that it turns out that the situation was more or less saved by Carl Anderson and me. And that was when I got a call from the *LA Times*. They were following up on this letter, and they wanted to know what my... **[Tape ends]**

Begin Tape 3, Side 1

COHEN: OK. So the *LA Times* called you.

WALKER: The *LA Times* phoned me and wanted to know what my political affiliation was. And quite by accident, and not my normal situation, I was registered as a Republican.

COHEN: How was that?

WALKER: Well, that was because there were some people running in the primaries, and I was going to influence the choice of the Republican in the primary [election] and thereby influence who might get elected. Anyway, I was a registered Republican and so was Carl Anderson. So two out of the ten were registered Republicans. And so it was not a purely [laughter]—

COHEN: You mean they called all ten of these people?

WALKER: Probably. It was not a purely partisan statement in this letter. It was really kind of ridiculous, because this wasn't anything partisan. So anyway, aside from that, I

think Lee DuBridge later on admitted to somebody, like my wife, that he was sorry that that had happened. But in general I got along fine with him. Then came Harold Brown [Caltech president 1969-1977]. I think that the faculty started out by not being fully enthusiastic about Harold Brown because of his [Department of Defense] background. And I think he realized that, and as a result he was very sensitive to faculty opinions on things he might do. My impression was that he was pretty careful to keep in close touch with how the faculty felt about various things, and that he was quite a good president.

COHEN: And then we had [Murph] Goldberger [Caltech president 1978-1987].

WALKER: And then we had Goldberger. Now, Goldberger was a physicist, of course—a theorist. So we started out by being enthusiastic about him, but I think he was not a very powerful president.

COHEN: Were you in any way involved with the Arroyo Center business?

WALKER: No.

COHEN: Because that was kind of his big problem.⁷ How did you feel about LIGO [Laser Interferometer Gravitational-wave Observatory]—since we're talking about controversial things?

WALKER: Oh, LIGO. Yes. Well, most of the big controversies with LIGO happened after I left.

COHEN: Right. But then there was the decision to go ahead and do this kind of work at Caltech. That would have involved the faculty, I think.

⁷ A U.S. Army analysis center was proposed for the Jet Propulsion Laboratory in 1982. Major concerns among the Caltech faculty about Caltech's reputation if connected to classified military work surfaced in Faculty Board meetings in November and December 1983. President Goldberger reported to the faculty that he had failed to adequately keep them informed about the center's development. In January 1984, the faculty voted for Caltech to divest itself of the Arroyo Center, and in September 1984, the U.S. Army Chief of Staff decided to transfer it to the RAND Corporation.—ed.

WALKER: Yes. Well, I guess I would have felt that it was a good thing to do. I know there is a peculiar problem with that, having to do with the source of the money. I'm not sure about this, but I gather that the money comes largely out of the astronomy budget.

COHEN: Well, that's what some people think.

WALKER: At least, that's what the astronomers think. And they do not view it as an astronomy project. I have met that same problem previously, when it came to—let's see—the linear accelerator at Los Alamos, LAMPF [Los Alamos Meson Physics Facility], which cost a lot of money. It has that in common with LIGO. They both cost a lot of money, and they came out of the budget of a group whose total budget wasn't terribly big. LIGO, that was astronomy, which is not one of the biggest budgets. And in the case of LAMPF, it was from the nuclear physics budget, which is also not terribly big. The nuclear physicists thought, while it was being proposed and so on, that it was a high-energy physics project, so they weren't terribly concerned about this. But the high-energy physics community is organized. They have had, for years, committees like the ones I was on.

COHEN: To direct the money?

WALKER: Yes. To very carefully make recommendations about priorities in high-energy physics. And the LAMPF project, I am quite sure, would never have been approved out of the high-energy physics budget—at least not through the high-energy physics committee. And the nuclear physicists didn't have that, and they didn't know what was going on. So all of a sudden there was a big surprise—“Here's this big, expensive project which comes out of our budget!” Now, the same thing happened, I think, with LIGO.

COHEN: The astronomers weren't organized enough, or they were convinced that wasn't going to happen.

WALKER: I don't know about that. They probably thought, “This physics program doesn't concern us.”

COHEN: I'm trying to think of whether you retired before [Thomas E.] Everhart [Caltech president 1987-1997] came or not. Were you still there when Everhart [was president]?

WALKER: I retired in 1981. I left when Goldberger was still there.

COHEN: Why did you feel you wanted to leave early?

WALKER: Why did I want to leave? I think it was a combination of a number of things. For one, I felt that I was no longer very effective in my research field.

COHEN: Do you mean that the machines had gotten too big, and you didn't have any—

WALKER: Because the groups, the experimental groups, were getting enormously bigger. And I had always worked with small groups—most of the time with one or two graduate students. Even when I went to user experiments at the Fermilab, the big experiment we did there had eight people, four from Caltech and four from Berkeley, and that was a good number. It was a big experiment and we had eight people. That was fine; that was something you could manage. And I felt I could contribute and did contribute to that. But then when it got to be hundreds, that wasn't very attractive. In addition, it became heavily dependent on computing. I was not an expert. I had done computing way back during the days when you put your input in with IBM cards and got it out on printed paper. But I used the big computer only as a fancy calculating machine. Somehow or other, I just didn't feel I'd be useful for the facility if I used the computer, compared with everybody else. So I wasn't in a good position to compete. For part of the time when I was working, and computing was very important, I had graduate students or a research fellow I depended on for that. But that depends very much on a proper relationship with your graduate student, and it really isn't a very satisfactory thing. I spent a lot of my later years teaching freshman physics.

COHEN: That's a big commitment.

WALKER: I worked pretty hard at that, but I don't think I was terribly good at it, for some reason. At least I had the feeling that the students didn't appreciate it.

COHEN: So you weren't getting satisfaction out of all these things.

WALKER: If I don't think the students are appreciating it, and I'm working hard at it, that's not satisfying. And I was doing less research. I always liked to do research, but I was somehow getting more and more into administrative things. I was the executive officer for physics for a while [1976-1981]. That's a position which is not very satisfying, not rewarding. I mean, whenever anybody in the institute has some trivial little problem—trivial or big, it doesn't matter—any problem having to do with physics, you're the one they call up. They want you to solve that problem for them, and it may be some stupid little thing.

COHEN: So it wasn't challenging or fun.

WALKER: Right. So I had all those things going. And we didn't particularly like living in Southern California, which is getting more and more crowded and trafficked.

COHEN: And you already had this place [in Tesuque, Santa Fe, New Mexico].

WALKER: And we had this place already.

COHEN: So it was a good decision, as far as you were concerned.

WALKER: Yes, I think so.

COHEN: You've really done a lot. You look back at a rewarding career, I would guess.

WALKER: Yes. I appreciated Caltech very much. And I still appreciate it, because now I spend more time than I ever did reading *Science* magazine and books on fields that are not high-energy physics but something like biology or astronomy. Anyway, I notice

when I'm reading *Science* that quite often some of the people involved in a specific article I'm reading are from Caltech. That leads me to think that it's still a good place.

COHEN: I think so. Well, are there any other observations you want to make about Caltech life in general, or any students you particularly remember, or any organizations—well, you belonged to the American Physical Society—and things of that sort that you belonged to?

WALKER: I've been a fellow of the American Physical Society for years and years. And I notice that when people write a résumé or something, they include things like that, but I can't see that it's a big— It certainly isn't an honor. [Laughter] All you have to do is be a physicist and join the society at some early age. When you get a little bit older, they feel they can make you a fellow, and you pay more dues and so on.

COHEN: You're being modest. It's not quite so simple. So, anyway, you might say it was a good run and you're still enjoying yourself?

WALKER: Yes. And when I retired, I was somewhat different from many people who retire. I'd say the common mode is that they don't give up.

COHEN: They keep up with their physics?

WALKER: Well, they keep up. They go to their office and they keep trying to do things sort of like they used to do. And I stopped doing any physics. Well, after I retired I spent a few years on the program committee for LAMPF, after I moved over here, because I was close by. That was the only connection to physics I've had, other than reading something. But then I took up other things. I took up making harpsichords, at Dorothy's instigation at the beginning. And that was sort of fun. And I started reading more about different fields, like biology and astronomy and other things.

COHEN: And here you're living in this beautiful place and really enjoying yourself.

WALKER: I go hiking and on canoe trips in the Arctic and things like that.

COHEN: Well, I think you're a lucky man. Thank you.