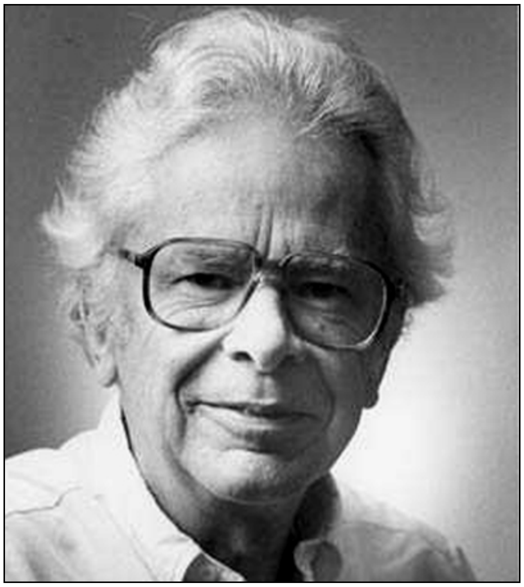

FELIX H. BOEHM
(1924 – present)

INTERVIEWED BY
SHIRLEY K. COHEN

January – February, 1999

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Physics

Abstract
An oral history interview in three sessions in 1999 with Felix Hans Boehm, Caltech Research Fellow, 1953-1958, and Professor of Physics, 1958-1995 (emeritus 1995-). Born in Basel, Switzerland, and educated at the University of Geneva and the ETH (Eidgenössische Technische Hochschule) in Zurich (diploma, 1948; PhD, 1951, in physics), Boehm recounts first coming to the US to Columbia University in 1952 to work in nuclear physics under C. S. Wu. In July 1953 he arrives at Caltech as postdoc; associations with physicists J. DuMond in Bridge Laboratory and C. C. Lauritsen and the Kellogg Lab group. Experimental work in 1950s and 1960s on aspects of nuclear structure and particle behavior, especially parity violation. Interaction with R. Feynman and M. Gell-Mann on parity violation in nonleptonic processes. He takes leave to Europe: Heidelberg 1957-58 and Copenhagen 1965-1966; meets R. Mössbauer and helps bring him to Caltech (1960-1964), where he receives Nobel Prize (1961). Reminiscences of Niels Bohr. At Caltech begins collaboration with P. Vogel (1970); developing interest in neutrino oscillations; neutrino mass and search for dark matter. Visits to Aspen Center for Physics; collaborations with French (Laue Langevin Institute,

Grenoble) and Swiss scientists (Paul Scherrer Institute, Zurich) on neutrino detection; experiment set up in Gotthard Tunnel. Work at Caltech on double beta decay; building of time-projection chamber (TPC); attempts to set up neutrino detector near San Onofre nuclear plant scuttled by environmentalists; lab eventually built in Palo Verde, Arizona. Comments on Caltech presidents and future of Caltech.

Administrative information

Access

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Preferred citation

Boehm, Felix H. Interview by Shirley K. Cohen. Pasadena, California, January-February 1999. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Boehm_F

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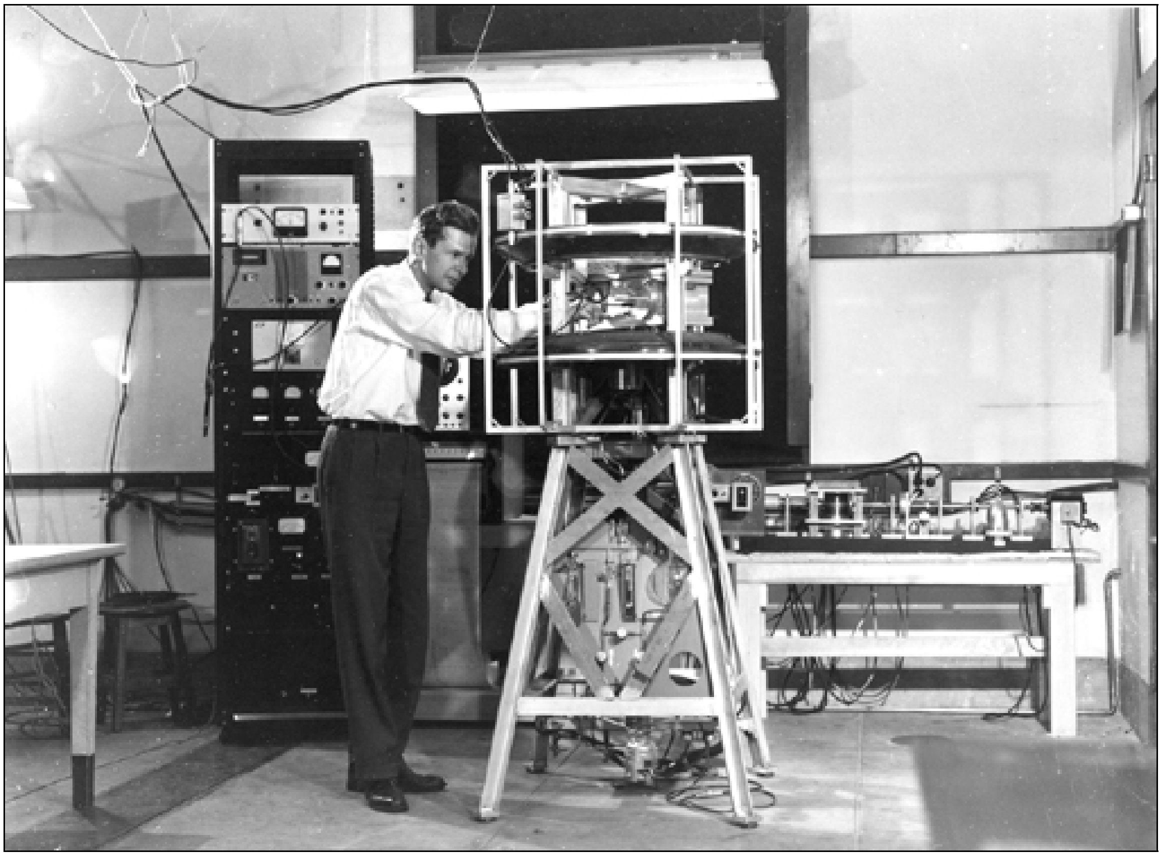

Felix H. Boehm in his Caltech laboratory, ca 1960. Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH FELIX H. BOEHM

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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California Institute of Technology

Oral History Project

Interview with Felix H. Boehm **by Shirley K. Cohen**

Pasadena, California

Session 1	January 19, 1999
Session 2	January 26, 1999
Session 3	February 2, 1999

Begin Tape 1, Side 1

COHEN: Tell us something of your family background—your mother, your father, what they did, your growing up.

BOEHM: OK. I was born in Switzerland—Basel—June 9, 1924. I grew up in Basel. I went to primary school there, and the *Gymnasium,* which is the high school.

COHEN: Now, this is the German-speaking part of Switzerland?

BOEHM: This is the German-speaking part, right. I had very tight contact with my family. I would like to say a few words about my family. My father, Hans Georg Boehm, was a publisher and printer, and so was my grandfather.

COHEN: What did they publish—a newspaper, books?

BOEHM: Well, they published books—scientific things. Basel was the seat of a sizable chemical-pharmaceutical industry, and still is—Hoffman-LaRoche, Sandoz, all these companies. So that is one of the main things I remember. They also produced commercial things for banks.

I had four brothers; I am number two. Two of my brothers joined in my father's company and remained there until they retired. Of the two other brothers, one went into medicine; he's an MD, a psychiatrist, but he's retired now, too, in Switzerland. And my youngest brother is a child psychologist, and he's also retired now.

One very interesting tradition in the family—which I treasure—is music. My family—both on my father's and my mother's side—had lots of musical expertise and exposure to chamber music. They played string quartets.

COHEN: And your instrument is…?

BOEHM: My instrument is the flute. I learned the violin and the piano. But the violin was not successful, because I'm left-handed, so there was a difficulty. After two years of piano, I learned the flute, and I play the flute to this day. I played lots of music here in my first years at Caltech with friends—the Delbrück family and other friends.

COHEN: The schools in Basel must have been very good.

BOEHM: Yes, they were good, traditional schools, with Latin and Greek. I had Latin and Greek for eight years and finished my *Matura* in 1943. Then we had the war, and I was drafted, and I spent about a year in the military as an infantry soldier.

COHEN: This was very curious to me. I've read your autobiographical notes, and the war years didn't seem to be very disruptive.

BOEHM: It was not so disruptive, now in retrospect. You know, Switzerland was not involved directly in the war, but we were surrounded by war. We had food rationing, and there was bombing going on not far from Basel. And everybody was in the military service—my father was, and my brothers.

COHEN: Did that mean you had to leave home and go to a camp?

BOEHM: Yes, I had to leave home and go to basic training. Basic training was about four months, and then immediately thereafter there was active service, and I had to serve on the Rhine, where the war action was.

COHEN: But actually in Switzerland. You never left Switzerland?

BOEHM: No, I didn't leave Switzerland. I never had great enthusiasm for the military, and I refused to go to officer school, so I remained a simple soldier.

COHEN: But there was universal conscription—everybody had to do military sevice?

BOEHM: Yes, everybody had to go.

COHEN: So your schooling just stopped then?

BOEHM: When the active service period started, I could arrange it so I could go for three or four months at a time and then start my university studies.

COHEN: That is, during the time you were actually in the service?

BOEHM: Yes, yes. So I began studying physics in Geneva after finishing high school. My parents wanted me to learn French. My mother's family, incidentally, was from the French-speaking part of Switzerland—my grandmother's family, I should say. So we spoke often French at home.

Anyway, I was sent to Geneva, and I began studying physics—went to the lectures by [E. C. G.] Stueckelberg, a famous theorist at that time, and Jean Weigle, who later came to Caltech [1949] as a biologist. He was a professor of physics in Geneva.

COHEN: Now, was this an undergraduate school?

BOEHM: Yes. That was my first year at university, and I had to learn all the basics.

COHEN: And you lived away from home.

BOEHM: I lived away from home. I lived with a family in Geneva. I enjoyed Geneva very much. Of course there was the hardship of the after-war times, but otherwise it was a nice time.

Then in the fall of 1943 I began at ETH [Eidgenössische Technische Hochschule]—the Swiss Federal Institute of Technology—where I studied for the following years.

COHEN: So you were interested in science from very early on?

BOEHM: That's right. When I was a small kid, I loved to build radios. I was a radio ham. Together with my brother, I built receivers and transmitters. That was, of course, the time of vacuum tubes—a very early time. And my brother and I then had ham licenses. We loved to listen on shortwave to American stations. It was a way of opening a window to the world. During all this time, we were very strongly closed in. Switzerland was surrounded by war action, and there was no way for us to escape. And at this age, you wanted to see the world. So I did it through the radio, and I learned lots of technology that way.

During the time in Zurich, I studied formally physics. I got my diploma in 1948, and my PhD in 1951.

COHEN: And who was your professor?

BOEHM: The experimental professor was Paul Scherrer. He was quite a well-known person; his name is associated with X-ray work: that is, a method called Debye-Scherrer—analysis of crystals. And my theory professor was Wolfgang Pauli, a famous but rather strange person, I must say.

Maybe I should just go back a little. When I was about twelve or thirteen, I was also very intrigued by electrophysiology, after having played with radios and electricity, and finding out that electric currents are everywhere—in our bodies, in our brain. I was really intrigued by that. And I read about the electroencephalograph.

COHEN: Where would you have heard about this?

BOEHM: I guess I heard it from talking to people—from friends who were in medicine.

COHEN: Were your brothers already studying medicine then?

BOEHM: No, my brother was younger—he was not studying medicine then. I picked it up, and I was intrigued. I wanted to learn more about how the human mind works. And knowing that electricity is involved, that was a way for me, as a young physics student, to maybe find out something about it. So I built several types of DC amplifiers, because these electric brain waves are slow-moving waves; you could not use an ordinary AC amplifier.

COHEN: I find this quite amazing—that at this age you had this interest and were actually able to do this.

BOEHM: Yes. So after a while I tried to find out with whom I could talk. In Zurich, there was a man—another famous man—Walter Rudolph Hess. He later got the Nobel Prize in physiology [1949]. He was accessible, and I was able to go to Zurich and talk to him. I was maybe fourteen or fifteen then. And he said, "Oh, wonderful! Come set up your amplifier and recorder, and I'll let you do some interesting tasks." He was interested in the hypothalamus and the optical center. And we built some electrodes, so we could measure the brain waves. I was still living in Basel then, so I commuted to Zurich. And later, when I was studying in Zurich, I continued that activity at the university with Professor Hess and some of his assistants, particularly a man named Marcel Monnier—French Swiss—a young professor, who was very good to me. We made good progress. We had a shielded room, where we could have this sensitive equipment to avoid the influence from AC-50-cycle. We were able to record this brain-wave potential.

COHEN: You must have been quite outstanding in this, because this is not a usual story for a young boy.

BOEHM: Yes, it fascinated me. Hess pointed out that in the evolution the eye is part of the brain. Thus I should study the eye, a simple organism. So I studied the retinogram.

COHEN: These people must have been quite early for their time, because one hears of these things now as new, you know.

BOEHM: It was really the beginning of that field. We were in touch with a man in England—in Bristol—who also studied brain waves; his name was Grey Walter. And a man in Italy who studied brain waves; I found a paper he had written in Italian, and I scrambled to understand this paper.

COHEN: Yet when you started your serious graduate work, you did not do this.

BOEHM: No. There came a time when I was just so strongly preoccupied with my physics that I cut back on the physiology and I finally gave it up. I was interested in nuclear physics. And then I was beginning to work on my diploma thesis, so I had no more time for that.

COHEN: You worked with Pauli on your dissertation?

BOEHM: Well, I took classes in theory from Pauli. My work was experimental. I did an experimental diploma thesis on the internal conversion of nuclear gamma rays. Pauli was, of course, the professor who examined me. The theoretical course was very formal at that time in Zurich. And at the end of all these courses, there was a rigid exam. Pauli was already feared. [Laughter] And then later, when I defended my thesis, there was my main professor, Scherrer, who was what was called the *Referent*, and there was a co-*Referent*, who was Pauli. So Pauli was also in the thesis exam and asked me questions. I was quite scared but managed OK, with a good grade.

COHEN: Was it much more rigorous than the examination one has here?

BOEHM: Oh, yes, much more rigorous, much more fearful. Here when we have exams, we are sort of kind to the students, and we help them out. But the system there was an authoritarian system. The professors were really very respected and authoritarian people.

COHEN: You did very well, I'm sure.

BOEHM: I did reasonably well, yes.

COHEN: What sort of experiment did you do?

BOEHM: For my thesis, I did an experiment on the cyclotron. Zurich had a cyclotron—one of the early accelerators in Europe, maybe even in the world. Don't forget that Germany was destroyed, and whatever experimental machinery there was before the war no longer existed.

COHEN: You know, that's another thing. Before the war, before all this, there must have been a lot of scientific communication between Germany and Switzerland. And that just then completely stopped?

BOEHM: Yes, that stopped at a point. There was a time during the war, I vaguely remember, when [Werner] Heisenberg gave a colloquium in Zurich—it may have been in '42 or '43. And there were many apprehensions and bad feelings. But then, you know, all the German scientists were drafted. There were some scientists who did war work, which was not known to us—at least to us young people. Many institutes were evacuated. Many left Germany—the best of them left Germany. And when the war was over, then the Allies occupied all these laboratories.

Anyway, getting back to Zurich, the cyclotron was beginning to work in 1940 or so, quite early. It was really a unique, well-working machine to allow nuclear reactions to be studied. We had protons of about 6 or 8 MeV [million electron volts] energy. We could study many reactions throughout the periodic system, and my thesis work was concerned with excitation functions. We studied nuclear excitations as a function of energy. At that time, people didn't have elaborate spectrometers to separate proton energy. Also, a cyclotron could only run at a fixed energy. It had to be tuned so the energy was in resonance with the magnetic field and the radio frequency. So what we did was we introduced thin foils of the material to be studied—say, cobalt. And these foils were stacked, so that the protons which went through the foil lost energy. The first foil saw the full energy—of 6 or 8 MeV protons. The next foil saw a little less, and so on. So by picking the foils and measuring their radioactivity, you could study the nuclear excitation as a function of energy. That was called the stacked-foil method—a rather primitive method, but it was effective.

So this was the method I used to study the excitation of a number of nuclei of intermediate atomic number. And at that time it was possible to compare these excitation functions with calculations from statistical models. And that was when Victor Weisskopf was visiting Zurich. He was one of the first visitors just after the war—it may have been 1949. He gave lectures on his statistical model. And that was a great revelation. There was otherwise very little work going on in nuclear theory. We had very little new information about the old ideas of the liquid-drop model that were known before the war. But these more dynamical models, like the one Weisskopf was explaining to us, were really nice new developments, and we could directly relate all this data we had with Viki Weisskopf's excitation model.

COHEN: Did you have any knowledge of what was going on in the United States on the development of the bomb? Was that anything that was known or talked about?

BOEHM: I guess we had some knowledge, but very restricted—not more than what was public, in the press. We knew about the Los Alamos tests and we knew that bombs could be made and fission and all these things. But no, otherwise we had no detailed knowledge.

So we enjoyed Weisskopf's visit. That was for me a wonderful time, because that was just when the experimental data from our cyclotron experiment came in, and we could compare, and so I could write my thesis; and I handed it in, I believe, in late 1950. I got my PhD in '51, as I said. And after that I remained as an assistant in Zurich, helping in the teaching of Professor Scherrer, and I left Zurich in March 1952.

COHEN: Now, you had an invitation to come to the United States.

BOEHM: Yes. I had corresponded with Madame [C. S.] Wu—the lady physicist at Columbia University who was responsible for interesting work in nuclear beta decay, a subject close to my own research. She got her degree in Berkeley with Emilio Segrè and then became a junior professor at Columbia University.

I wrote her about my interest and experiments, and she replied. And she was able to propose my name for a fellowship, which I finally got. It's a fellowship that is named after a man named Boese. I was never able to find out who Mr. Boese was. But it was the Boese Fellowship which I got, and I spent a year and a half at Columbia University.

COHEN: This was really the first time you left Switzerland?

BOEHM: Not counting short trips to France, Italy, and Germany, it was the first time I left Switzerland. It had enormous impact on me; I looked so much forward! I booked my passage on the *Liberté,* a French liner—there was no airplane then. My parents drove me to Le Havre.

COHEN: Now, you've talked about all these languages, but you haven't mentioned English. Where did you learn English?

BOEHM: In high school we had English; it was an obligatory language. In Switzerland we were much more obliged to learn languages, to communicate with the world. So French was kind of my second native language. Besides the classics—Latin and Greek—I picked up a little Italian, but I learned English in school; this was necessary so I could read the physics papers, and the *Physical Review,* and I could keep in touch with what was going on.

COHEN: You came to New York City, which was quite different?

BOEHM: I came to New York City, and that was a fantastic experience. I came there alone, and I had my heavy baggage with me. From the French Line pier I took a taxicab to Columbia. There was a little hotel, which I believe was called Kings Crown Hotel, on 116th Street. I felt very excited, but I also felt very lonely.

COHEN: There was nobody there that you knew?

BOEHM: Well, I found out that a friend of mine, Hans Noll, whom I'd known in high school and who studied biology later, was in New York. He was living in New Rochelle, and he was passing a couple of years at the New York Institute of Public Health as a biologist. He was very nice to me. He had a family. He rode a motorcycle, and he took me back on the motorcycle to New Rochelle. So that was a nice contact.

COHEN: So you had to find a place to live?

BOEHM: I had to find a place to live. I bought the *New York Times* and found immediately a lovely place on Riverside Drive. The first place I found was only available for the summer period, because the tenant was away during the summer. It was overlooking the Hudson River. I looked more seriously for a place later, and I found a really nice place in an old small mansion between 105th and 106th Streets, on Riverside Drive.

COHEN: Now, did your fellowship cover this, or did your family have to help you?

BOEHM: Well, my family had to help me, yes. My fellowship was $4,000 a year. That was not too bad.

But I should fill in here: when I arrived at Columbia, I was first introduced to the chairman. That was Polykarp Kusch. And later it was Charlie [Charles H.] Townes. And both of these gentlemen were extremely kind and helpful. The fellowship was not effective immediately; I had to wait three months. And Townes said, “OK, we’ll pay you $150 a month to help you out during that time.”

COHEN: Was the department very big then? Were there very many fellows?

BOEHM: The department was very big, yes. And there were many famous people. There was [Hideki] Yukawa, a famous Japanese theorist; he was a professor at Columbia—he returned to Japan later. And there was Robert Serber. There was, of course, Kusch and Townes. And in the basement of the Pupin building there was still classified work going on; there was also a cyclotron, and there was a neutron cross-section program. So there was a guard, and for a time I had a laboratory in the basement, and every time I had to go to my lab, I had to ring the bell, and a guard would come.

COHEN: So you were working by yourself?

BOEHM: I worked with Madame Wu, the Chinese professor. But I also worked by myself; there were several graduate students with whom I talked and worked. It was a profitable time.

And to return to music: Of course, I enjoyed Carnegie Hall. And I enjoyed many other activities in music. My friend Hans Noll—the biologist I mentioned before—played the cello, and he had joined the New Rochelle symphony orchestra and encouraged me to join that orchestra. So we had a regular rehearsal once a week in New Rochelle. One time there was a benefit concert, and Isaac Stern played the *Symphonie Espagnole*, by Lalo. I played the flute part for that—the highlight of my musical career. [Laughter]

COHEN: How long were you there?

BOEHM: I was in New York for about a year and a half. I arrived in March 1952 and I left, I believe, in July 1953.

COHEN: Did you have an opportunity to do any travel?

BOEHM: I went to Washington several times for the American Physical Society meeting. I went to Boston once, to MIT, to visit a friend. I went to Princeton to visit a friend. I went to the Bell Labs, in Murray Hill [New Jersey]. There was enormous activity in condensed-matter physics—as you would say today—at Bell Labs. And several people there were originally Swiss; they graduated from the same institute [ETH] and joined Bell a year or two before I went there. In fact, around the time I left Switzerland a great many young Swiss did the same—emigrated and took up positions in the US. And many of them are still here, and I am in touch with them.

COHEN: But you didn't get out to California?

BOEHM: I did get out to California. I had a friend who was at Caltech, Pierre Marmier; he was a postdoc at Caltech. He probably left Zurich the year before I did. I knew him from there, and he had encouraged me to come and visit. I visited him. And at the same time, I had been in touch with Stanford—I do not remember exactly how this happened. But I was encouraged to visit Stanford, and I was told there was an instructor position. So I went to Stanford and Caltech.

COHEN: So you were sort of looking for a position?

BOEHM: I was certainly debating whether I should return to Switzerland or whether I should stay maybe another year or two in the US. So I remember well the visit to Stanford. I met with a Swiss physicist named Felix Bloch, a famous man, who passed away some fifteen years ago. And another man named [L. I.] Schiff, who was the department chairman—also a well-known theorist at Stanford. We talked and he took me to lunch and interviewed me, in regards to being a candidate for this instructorship.

Then I went to Caltech—after Stanford—and met Professor Jesse DuMond. I met also various people from the [W. K.] Kellogg [Radiation] Lab—Charlie Lauritsen, Tommy Lauritsen, Willy Fowler, Bob Christy, and Ward Whaling were there. And I was very impressed by Caltech. I may have met the division chairman, [Robert] Bacher, at that time also. I was also impressed by him; he was a very strong man and he knew what he was doing. DuMond said to me, "Well, maybe you would like to join Caltech as a postdoctoral research fellow." But no decision was made. I returned to Columbia. Then a few weeks later, I got an offer from Stanford and I got an offer from Caltech, and I had to make a decision. [Laughter]

COHEN: Were you more impressed with Caltech than with Stanford?

BOEHM: At Stanford I was told, "Listen, you have to teach three courses—nine or twelve hours a week. And I can guarantee you that after the end of the contract"—which was maybe three years, or five years—"there will be no more job; that's it." And I thought that was somewhat harsh and discouraging.

At Caltech, I was told, "You won't have any teaching obligation. You can do research. You have a one-year appointment, but it may be renewed. And then we'll see." So then I thought, well, I have no family, I have no obligations, I prefer to do research. I liked Caltech. And that's when I decided to come here. So I came in July 1953.

COHEN: You must have gone back to Switzerland in between?

BOEHM: No, I don't think I went back. I went back a couple of years later. I would have had to take a boat again, and it would have been complicated.

COHEN: So this was really a bold move—and exciting, too.

BOEHM: Well, yes, in a way. But I also felt that, after all, I could always return to my country of origin—that would have been the normal thing—and maybe start my career in Switzerland.

COHEN: So you just thought that you'd do this year [at Caltech] and then you'd see? It didn't occur to you that this was what you were going to be doing [for the rest of your life]?

BOEHM: No, no. Definitely I saw this as a temporary thing. It was a wonderful opportunity. I loved California; I loved the desert, the beaches. And I was very, very impressed by [Caltech's] style and [collegial] atmosphere. I should mention that in Europe there was this very strong authoritarian tradition, and many of the professors were also in conflict with each other, so there was much animosity between colleagues. There was an enormous amount of jealousy.

COHEN: You sensed that even as a young student?

BOEHM: Yes, I sensed that as a student.

COHEN: Well, there was much more competition there.

BOEHM: There was enormous competition, and there were no jobs. Industry was not a source of employment for physicists then. And much too many physicists were trained in Zurich—a very large number, during these years. What was the future for them?

COHEN: There must have been a widespread vision of this country, when you came here, as a place where everything seemed open and everything seemed possible.

BOEHM: Yes, definitely. I could do what I wanted, and I could look up someone and ask him a question, and people would take me seriously and would reply—and we would discuss theory and things, which I wanted to learn more. So that was really very great.

COHEN: So you came to Pasadena after a year and a half in New York.

BOEHM: Yes.

COHEN: And you were still a single man at this time?

BOEHM: I was a single man, yes. I was about twenty-eight years old, or so. I had no attachment.

COHEN: Pasadena would have been beautiful at that time.

BOEHM: Pasadena was a small town. It was also very much more civil, in a way, compared to now. I remember I visited my new friends and they never locked their houses.

COHEN: You must have gotten yourself a car by now.

BOEHM: I had already bought a car in New York. I loved to drive a car. I got myself an old Ford convertible, so that I could commute to New Rochelle.

COHEN: So you drove to Pasadena when you moved here?

BOEHM: That's right. I drove with a friend, George Hermann, who was a schoolmate of my older brother. He was an assistant professor of engineering at Columbia, and he was being interviewed for a job at Stanford. By then I had a big Buick, which I bought secondhand. We drove across the country; we spent only a week or so, visited people on the way.

COHEN: So you came here as a postdoctoral fellow?

BOEHM: Yes.

COHEN: Let's see. Richard Feynman would have been here already.

BOEHM: Yes, Feynman was here. And Murray Gell-Mann came later [1955].

COHEN: Did you work with the Kellogg group?

BOEHM: No, I did mostly experimentation in the West Bridge lab [Norman Bridge Laboratory of Physics] with DuMond. DuMond then built crystal spectrometers. He would be able to measure gamma rays and X-rays with high precision. And he also completed, just at the time I came, the beta spectrometer to measure electrons in nuclear decays, beta decay, and conversion electrons. So that was a wonderful opportunity for me to make use of these instruments; I guess DuMond was happy to have a young guy who would use his creations. But I talked to the Kellogg people a lot. I went to their seminars—they had regular seminars. The group membership depended much on funding here, and it still does. At that time, the Kellogg Lab was funded by the navy, later the NSF [National Science Foundation], and DuMond was always funded by the DOE [Department of Energy]—at that time it was the AEC [Atomic Energy Commission].

COHEN: Had the parties at the Fowlers started yet?

BOEHM: There were seminars every Friday, followed by a party. And the Lauritsens were at the Fowlers, the Whalings also. It was really quite nice.

COHEN: Those were very special years. So then how long did you stay as a postdoctoral fellow?

BOEHM: I was a postdoctoral fellow for two years. And then I became a senior research fellow [1955 to 1958].

COHEN: In ’56 I have a little note that you went and got yourself a wife someplace.

BOEHM: That’s right. I met my wife, Ruth [Sommerhalder], in ’56, at a social occasion at the Swiss consulate in Los Angeles. She is also Swiss. She spent most of her life also in Basel, my hometown; she was not born in Basel, she was born in the canton of Aargau, a little south of Basel, but she went to school in Basel. I did not know her there; we met here in California, and we got married a year later. And again, I told her, “I’m just here as a visitor. I will return to Switzerland eventually.” Well, then I was promoted to assistant professor in 1958.

Nineteen-fifty-six was an interesting year scientifically—that was when parity nonconservation was predicted by [C. N.] Yang and [T. D.] Lee, and an experiment was carried out by Madame Wu at the National Bureau of Standards. And just at that time I was very strongly intrigued by these ideas, and we built an experiment together with a young visitor from

Amsterdam named [Aaldert] Wapstra, and we were able to confirm this parity nonconservation in several beta decays, just a little later, after the famous Wu experiment. [Tape ends]

Begin Tape 1, Side 2

BOEHM: We spoke about Bacher before [chairman of the Division of Physics, Mathematics, and Astronomy, 1948-1962]. I must say that Bacher was very, very helpful to me. He was severe and stern—

COHEN: In some sense, did he hire you, even as a postdoc?

BOEHM: Yes, he was division chairman. He did all the hiring. DuMond was not able to hire me; he could only make a recommendation. And Bacher was very formalistic: "Well, I am the man who makes the decisions."

At that time, that there was a controversy between the Kellogg people—particularly Charlie Lauritsen—and DuMond that had to do with wartime work. I think DuMond felt that he was deprived of recognition during the war, while Kellogg was where one of the vacuum vessels was used for some war-related work. And Charlie had good connections with the navy. DuMond was often a little bit on the defensive—not to say paranoid—about the situation between Kellogg and Bridge. And that had some repercussions on my own career, because I had divided loyalty. I liked the guys in Kellogg. But, of course, I liked DuMond very much and I respected him. And he was my boss.

Anyway, Bacher was above all that, naturally, but he seemed to also not be very helpful to DuMond's requests. He was closer to the Kellogg interests.

COHEN: That was probably because of his Los Alamos experience.

BOEHM: Yes, maybe. Well, DuMond was a difficult man, often. I don't want to go into this more; I just want to say that Bacher was very, very helpful to me for my work I mentioned a moment ago—about parity nonconservation and building the circular polarization magnet. He encouraged me, and he said it's terribly important to do, and he had a good perspective on all these things. And that was a great help to me.

COHEN: So your work was quite separate from the Kellogg group?

BOEHM: Yes, it was quite separate from the Kellogg group.

COHEN: And was that because DuMond's feelings were concerned…

BOEHM: I think so.

COHEN: Now DuMond was what nationality?

BOEHM: He was an American, but he grew up in Paris. His father was an artist.

COHEN: Yes, I remember now; I went back and read your obituary of him. And he was raised on a farm, essentially, by his grandparents.

BOEHM: Yes, in Rochester, New York, but he spent many years in Paris. He was a very cultured man. He had a European background, so to speak.

COHEN: So you got on very well with him?

BOEHM: Yes, quite well.

COHEN: Of course, Lauritsen had a European background, too.

BOEHM: That's true. So there was a little contretemps, so to speak, during that time.

COHEN: Was your group very big, with DuMond?

BOEHM: DuMond had two or three postdoctorals—Marmier, the fellow I mentioned before, was a postdoctoral; I was another one; and there was, I think, one more. And then there were about three graduate students at that time.

COHEN: But no other professors?

BOEHM: There were no other professors, no.

COHEN: It was a traditional group—

BOEHM: It was a small traditional group, dominated by Jesse DuMond.

COHEN: But for you, it was very good?

BOEHM: For me it was excellent! I mean, the fact that I had all these wonderful instruments to play with, to do work. DuMond was also very keen to have guests. He invited many people to come for a time, to provide a new direction. So that was a good situation.

COHEN: Now, by this time you must have done some teaching.

BOEHM: Yes. In fact, I started to teach when I was a research fellow. At that time, apparently, there were not sufficient professors. So I taught mechanics, and I taught electricity and magnetism, four hours a week. And of course when I became assistant professor, I had my regular teaching schedule.

COHEN: Did you enjoy teaching?

BOEHM: Yes, I enjoyed it, but I preferred research. Teaching was never my greatest love, I must say, but I didn't mind; it was part of my duty. But I thought it was lots of work, doing it conscientiously. I spent quite a bit of time preparing the lectures. It was OK altogether, but as I said, I preferred research.

COHEN: You must have been involved in your music all this time also?

BOEHM: Oh, yes. There were guys at Caltech who played instruments; we played music quite regularly.

COHEN: Now, the Max Delbrücks would have been here already by this time, I guess?

BOEHM: Yes. In fact, when I came to Caltech in ’53, a friend of mine introduced me to the Delbrücks, and that was a wonderful contact, the starting point of a wonderful friendship. We went to the desert—the Delbrücks had a traditional desert trip every second or third weekend. They camped out. That was something I enjoyed tremendously. Then, of course, the music. Another man I also enjoyed playing with occasionally was Pol Duwez. He was a professor of engineering. He played the cello, and he really played very well—he played at the professional level.

COHEN: So there was a lot of music?

BOEHM: There was a lot of music. There was a Caltech orchestra, and I played in that. We rehearsed in the old Culbertson Hall—it was a nice building. There was chamber music. There were people like [professor of applied mathematics] Paco Lagerstrom, who was very supportive in getting musicians here and playing at Dabney Hall.

COHEN: You mentioned something else in your notes—that you had always done some painting.

BOEHM: Yes, I have. Since childhood, I loved to paint. I’m not particularly good at it, but I love to do it.

COHEN: So how did it come about that you got your own laboratory?

BOEHM: Well, DuMond retired [1963], and I succeeded him. I took over his AEC grant and went in new directions. For a time, I continued on his instruments. We looked at gamma rays—that was in 1953 or ’54; we measured many gamma-ray decays of nuclei, particularly what we call deformed nuclei. And we found these rotational states; we found that the gamma rays, if combined properly, would indicate that the nuclei had collective states. They had rotational states—zero, 2 plus 4 plus spin. And so that was a very nice activity.

COHEN: And you were doing that while DuMond was still there.

BOEHM: That's right. DuMond was still there, but he did not get so much involved in nuclear physics. He designed another instrument—the Compton spectrometer, which existed until a few years ago, and then we wanted to give it to the Archives. No one wanted it. [Tape ends]

FELIX H. BOEHM

SESSION 2

January 26, 1999

Begin Tape 2, Side 1

COHEN: Why don't we continue our story. You had just built this gamma ray polarimeter. What did you do with it? Whom did you work with on it?

BOEHM: In 1956, as I mentioned last time, there was an interesting announcement by Yang and Lee that parity should not be conserved in beta decay, a weak interaction process. And building on this interesting declaration—and having already [known about] the experiment carried out by Madame Wu, my former supervisor at Columbia University, together with the group at the National Bureau of Standards—we went ahead and built this analyzer which would help us to detect the circular polarization of gamma rays immediately following beta decay. The beta particle is detected, and that determines an axis. And if one measures in the opposite direction to the beta particles the gamma ray, its circular polarization, one would be able to find out how much the beta particle was polarized originally. So we carried out this measurement, with cobalt-60 and several other nuclei, and we found a large circular polarization, in confirmation of this parity theory. That was very exciting. We not only measured this cobalt-60—which is referred to as a Gamow-Teller transition; it's a transition in which the spin changes in the electron decay—but we also measured the circular polarization in other beta decays: Fermi beta decays and mixed beta decays. And that all helped us to build a nice picture of the coupling scheme of the parity nonconservation in weak interactions.

COHEN: OK. You are now using the word "we." With whom were you working?

BOEHM: Well, experimentally, it was Aaldert Wapstra at that time. When I say "we," there were also some students. But it was really mostly Wapstra and myself who did the experimental work.

COHEN: Which you built over in the Bridge lab?

BOEHM: We built it in the Bridge lab. We talked to Jesse DuMond a bit. He was interested, but he didn't really participate in that. And we had quite a bit of mechanical help from Herb Henrikson. Henrikson had been hired by DuMond as an engineer to design apparatus. DuMond was very concerned with precision apparatus for his crystal diffraction work, so Henrikson designed the crystal diffraction spectrometers, and designed also our circular polarization analyzer.

COHEN: So he was a valuable person.

BOEHM: Very valuable, yes. And in the following forty years, when I was continuing my work in West Bridge, Henrikson was of great help to us. He designed many pieces of apparatus.

We had lots of interaction with Richard Feynman then. He was very interested in these developments, naturally.

COHEN: Now, did he wait until you gave him some results, or did he come to you with some theoretical idea? How did the interaction work?

BOEHM: In this case, I guess the theoretical idea did not come from him. But a few years later, he and Murray Gell-Mann worked out what is called the current-current coupling scheme of weak interaction—which means that in addition to the parity violation like the one we observed, there should also be parity violations for nonleptonic processes. When gamma rays are emitted from nuclei, there should be a small amount of parity violation. He and Gell-Mann were developing this theory, and we had lots of encouragement from both of these colleagues. I believe it was in 1960 that we first observed the parity violation in nonleptonic processes. And I think we were probably the first to see that. That is, we provided experimental proof for the current-current scheme of Feynman and Gell-Mann.

COHEN: And this was on your relatively small equipment.

BOEHM: It was rather small equipment. Again, what we measured was the circular polarization of gamma rays. At this time—and not in coincidence with the electron, but it was just the gamma ray, by itself—the effects predicted were very, very small, like 10^{-5} or so. So, together with the rather poor efficiency of the circular polarization analyzer, this will result in an observable effect of 10^{-6} or 10^{-7}. These are very very small things, so obviously we needed lots of counts. We needed a very, very strong source to produce sufficient counts so that the statistical error would be small. And we needed to master all the systematic errors. So we did that; we built a device which integrated the counts and actually measured currents, and this allowed us to use sources up to the Curie strengths; we had to consider all the shielding that was necessary to handle these sources. During these years, we didn't have any safety office, and no one was looking over our shoulders. We just used common sense.

COHEN: Was anybody ever hurt?

BOEHM: No, nobody was ever hurt. And in order to master the systematic errors—the drifts in the current-counting process—we devised a very intriguing scheme of varying counting intervals so that the various moments for time variations involved in these counting intervals would cancel.

The person who was quite active during that time—I think he was a postdoc—was Han Vanderleeden, a Dutch name. He was extremely active then in building these electronic schemes.

COHEN: From whom were you getting funding at this time?

BOEHM: The funding came always from the Atomic Energy Commission. And Bacher, who was the division chairman, was a powerful man, and he had also some powerful hands in the AEC and could make a phone call and we would get funding. [Laughter]

In this connection, a little story comes to my mind. This was a couple of years later, when Rudolf Mössbauer came. Mössbauer needed funds to build up his laboratory, and I called my AEC representative, and he said, "Sorry, no." Then Bacher got on the phone and talked to the AEC—he was a former commissioner. And Bacher said, "We need fifty thousand dollars, right away!" And they said, "Yes, of course."

To return to these experiments: We carried out several measurements, and we found an interesting case in hafnium-180. We observed a circular polarization of the order of one percent—a very, very large circular polarization. And we explained it in this way: because of nuclear structural considerations, the regular transition was strongly hindered—as one would say—by selection rules. These were deformed nuclei, and that particular transition between rotational bands was strongly hindered. And the opposite parity component that would admix to result in a circular polarization would not be hindered, so it would be relatively enhanced compared to the main component. The issue in this type of measurement is always that you have a regular and an irregular multipole. Say you have a magnetic dipole transition, which you study in order for the parity violation to manifest itself; you would have to have the electric dipole to admix with the magnetic dipole, because one changes parity and the other does not change parity. And this admixture would then give rise to the observable circular polarization. And the admixture would be due to this nonleptonic weak force in nuclear states.

COHEN: So you really were getting a picture of what was going on in there.

BOEHM: Yes, we tried to understand the nature of these nuclear states, which are not as simple as people thought but have irregular components. They have components mixed in from this weak force.

COHEN: So you were busy doing these experiments. Did you separate at that time the theoretical work from the experimental work?

BOEHM: The theoretical work was not so difficult. We could understand the ideas of Feynman and Gell-Mann with the current-current scheme, and we would include them in our analysis and discussion. In addition to the basic ideas, there were also many nuclear structural considerations. In order to describe a nuclear state, we need to know about the fundamental ideas of this weak force, but you also need to know how you get that mixture of states. Whatever states are nearby, this must be either known from experiment or must be predicted in a nuclear model—a nuclear shell model, or a collective model. And so we would try to understand these things. And throughout the time I've been here, I had always one or two postdoctoral theory fellows, and I appreciated that. I think theory and experiment belong together. It's nice to discuss things, and we experimentalists—my students and postdocs—appreciated being able to just go to the office next door and try to get an explanation of the theory.

COHEN: And sometimes those people must have come to see your experiments.

BOEHM: Exactly. It worked both ways. So that was something I always liked and I liked to encourage. A few years later, Petr Vogel joined me as a young postdoctoral—in 1970. And he could fill that role marvelously and has been with me all this time.

COHEN: Now, when all this was going on and you were in the laboratory many, many hours a day—and night, I'm sure—you still had your responsibilities to the institute. You had to teach.

BOEHM: Yes, I had my class to teach. So I would be in the lab until late, as you say, and then I would prepare my class, and the next morning… [Laughter]

COHEN: Not an eight-hour day.

BOEHM: [Laughter] No. Long days.

COHEN: Was there any sort of specialty that you taught?

BOEHM: Well, I was assigned to teach some of the required courses—electricity and magnetism, classical mechanics, quantum mechanics, various levels. I also taught nuclear physics.

COHEN: Let me go back to something you told me. On one of your first return visits to Europe, you were offered a professorship at ETH, your old school, plus several other places. And you actually came back here to be an associate professor [1959]—not even a full professor. Explain to me a little bit about this. I mean, you must have been quite honored and pleased to be offered these professorships.

BOEHM: Yes, I was quite honored, and I did consider it very seriously—particularly the offer to return to Zurich, to ETH. However, I felt the atmosphere in Zurich was not to be compared [to that here]. I was never too happy about the research and human atmosphere in the Zurich institute. It was very authoritarian; it was very partisan. And I was so pleased to be in the US and to have this extremely positive, easygoing, and congenial atmosphere between colleagues. You could talk to Feynman—no big deal, anytime—and it was wonderful. I also knew that this momentum that I had now here in the Bridge lab, with all these interesting new things we found—parity violation—this was something that I liked to maintain. It gave me a greater feeling of fulfillment, rather than to go and be a professor in the European system.

So I returned as an associate professor. And I was confident that with time I would become a full professor. [Boehm became a full professor in 1961. He is now Valentine Professor of Physics, emeritus—ed.]

COHEN: And your wife, Ruth, had no problem with this?

BOEHM: No. She actually liked the US better. She liked it really quite well here—the informality and the dynamic attitude of people. And she also felt that I was happier here at the institute than I would have been in Europe.

And then in 1960, we had our first child—Marcus, our older boy—and then three years later came Claude, our second boy. We have two children. And the children were here in school and growing up—so all this was settled.

COHEN: Many people are honored when their original institution asks them to come back as a professor. But I have a feeling that with you it's the work that's important.

BOEHM: Yes. The work, as well as the human atmosphere and the peace with colleagues. I must also add that the funding was very nice. The Atomic Energy Commission was really very generous. In addition, I had very good students, and I was just so pleased to see these very, very brilliant young guys. That also helped me make the decision.

COHEN: The structure of the American university is so different from the European. How did you find committee work? I mean, did you do any of these things? Of course, at Caltech there's very little of it, compared with other places.

BOEHM: There's really very little of it. We had meetings about classes and about curricula. There was very little of that, really. Of course, you could accept or reject [the invitation] to be a member of a committee. And if I had a choice, I would not accept. [Laughter] I'd rather spend my time doing something else. I did my duties, I thought; I took my teaching seriously. There were exams, naturally. And there were faculty meetings, but it was not overwhelming.

COHEN: So then you continued with these experiments. Now, tell us about Mössbauer coming. How did that go?

BOEHM: I don't know if I mentioned earlier that I was invited to Heidelberg as a guest professor in 1957-1958—which I thought was quite nice—maybe in recognition of the work we did on parity. I described to you the circular polarization correlation, but there was another experiment we did at the same time where I worked with colleagues in Kellogg, especially [professor of physics emeritus] Charlie [Charles A.] Barnes. And that was to see whether positrons emitted in the decay of nitrogen-13 were fully polarized, and which way they were polarized. So this short-lived isotope—I believe it's about twenty minutes half-life; I've forgotten—could be produced in the Kellogg Van de Graaff generator, and it could be moved to our analyzer in Bridge. So we did that experiment: we measured the circular polarization of the positron-annihilation gamma rays emitted in this nuclear decay. That also gave information on the positron polarization. And we did a couple of other experiments—this was a nice collaboration with the Kellogg people.

COHEN: Now that you were on your own, you had no problems with working with the Kellogg people. There were no undercurrents of the old…?

BOEHM: No. I think that was really, as I mentioned last time, a problem with DuMond and Charlie Lauritsen.

COHEN: And that was in the past.

BOEHM: That was in the past and had nothing to do with me. Anyway, I was mentioning this year in Heidelberg. Hans Jensen, the theorist and later a Nobel Prize winner [1963], invited me to spend a year in Heidelberg in 1957-1958 as a guest professor and kind of help to build up their nuclear physics institute. It was not so long after the war, and things were still not in great shape in Germany. It was an attractive offer. Both Ruth and I decided that we'd go. We had no children then, and it was an interesting year, where we could also do lots of traveling.

COHEN: And you still must have had family in Switzerland.

BOEHM: Yes, I had my parents there, and my brothers. During that time—you asked me about job offers—I had various offers to various universities in Germany: Freiburg and Heidelberg and Berlin and Karlsruhe. Somehow these experiments excited people…

COHEN: Of course!

BOEHM: But these offers I turned down. I didn't think that was something to consider—except later, the offer from Zurich.

COHEN: Well, you had to decide whether you wanted to live in Germany. You would have had to face that problem.

BOEHM: Yes, that was also something, yes. During that time in Germany, I met Rudolf Mössbauer, and he had done just a wonderful experiment, which now carries his name—the Mössbauer effect. I learned about it early on, and I met him; and I was very excited about this very, very interesting idea he had, and the execution of this idea. And I said, "Rudolf, would you like to come to Caltech as a postdoctoral? I can arrange that." And he said, "Yes, that would be nice."

COHEN: Now, he was very young at this time?

BOEHM: He was just a young guy; he was just finishing his thesis. In fact, during that time I had correspondence with DuMond about the Mössbauer effect, and DuMond showed these letters to Feynman, Christy, and other people, and they were all excited that one would be able to get such a recoil-free nuclear excitation. This was not easily anticipated or calculated in the old ideas. So they said, "Oh, by all means, let's get this young man to Caltech." So Rudolf came—maybe in 1960. I returned in '59, and he came as a research fellow.

COHEN: When did he get his Nobel Prize? Was he here when he got the prize?

BOEHM: Yes, he was here when he got the prize [1961], a year later.

COHEN: As a postdoc?

BOEHM: He came here, and then his fame started to grow, and the effect, which he told me in Heidelberg, became known. His paper was published and people got very excited. So his market value went up. And then Caltech immediately promoted him to senior research fellow [laughter] and then to full professor. And then he got his Nobel Prize and we had a wonderful celebration.

COHEN: So he stayed here some years?

BOEHM: He stayed here for a few years [Mössbauer left in 1964—ed.]. And then he went back to Munich [the Technical University], which is his hometown, where he was offered a wonderful position, of course, as a Nobel Prize winner, and wonderful conditions. He could build up his own lab, and they would give him quite a bit of money. And he decided to take it.

COHEN: That must have been a very exciting time, when he was here.

BOEHM: Yes, it was an exciting time. He attracted many students.

COHEN: Now you said he built his own lab. So he was doing his work separately from your work?

BOEHM: Yes. I had two floors in West Bridge. I wanted very much to begin to build my lab. I gave him several rooms where he installed these power drives, these mechanical drives which provide a certain velocity to a source, so we would be able to create this resonance, this recoil-free resonance. Again, Henrikson, the engineer, built the mechanical drive systems. And Mössbauer used them, and he had soon three or four or five graduate students. One of these students was John Poindexter, who later became an admiral and then was involved in a controversy under [President Ronald] Reagan. You may remember this controversy—Iran-Contra. But we had an active lab.

COHEN: So your group was quite big at this time already. You had Mössbauer and then your own group.

BOEHM: Yes, we were already twelve or fifteen people. During Mössbauer's time, we were able to convince the Atomic Energy Commission to build us an isotope handling lab. Because for his work we needed many sources, radioactive sources. And Mössbauer was also interested in organic compounds—hemoglobin. He wanted to see—at that time, this was of great interest to people—where is the iron bound to the hemoglobin? That you could see, from studying the Mössbauer effect; you would be able to see where the iron sits.

So we had this isotope lab built. And we had many facilities that were running there.

COHEN: You say an isotope lab. How big?

BOEHM: This was an underground facility, which is still in existence. It's just south of West Bridge—between our building and California Boulevard. There's a lab consisting of five rooms. One of the rooms has deep wells to store the isotopes. So they are well shielded. All other rooms contained fume hoods. We had chemical hoods and various equipment to work with the sources, to make chemical compounds—electroplate, source material.

COHEN: So this was a big facility?

BOEHM: It was a nice facility, yes.

COHEN: Did anybody outside of Caltech use this facility?

BOEHM: No, but we had other people from campus. Later we had biologists who used it for phosphor-32 research they needed to do. And we had people from the Kellogg lab who used it. Right now it’s still there but not used very much. I use it occasionally to prepare a calibration source.

COHEN: You would think you would need special safeguards for something like that. But that was before they were so strict about that.

BOEHM: Yes, well, the isotope lab had a special lock. You cannot just enter it; you have to have a special key. And when you entered you signed the book, and you had to wear a dosimeter and protective clothing. All these things were slowly introduced and got more and more bureaucratic. But still, Caltech people are very reasonable, mostly; guys know that there are all these regulations, but they try to help us.

COHEN: So you had this going. But then after Mössbauer left, what happened to his laboratory?

BOEHM: That work closed down. We discontinued the Mössbauer work and used the space for other things.

COHEN: OK. You’ve listed here the work with Egbert Kankeleit.

BOEHM: Yes, we talked about that: that was the nonleptonic weak force, and I should have mentioned Egbert’s name.

COHEN: And then you note that in ’65 you went on a sabbatic year as an NSF Fellow to the Niels Bohr Institute.

BOEHM: Yes, in Copenhagen.

COHEN: What happened with your work here? Did somebody else take charge of it?

BOEHM: I do not remember whether Mössbauer was still here during that year, 1965. But I always had postdoctoral fellows and responsible young guys who could carry on and would supervise the students.

It was an exciting year. There was Aage Bohr and Ben Mottelson, too; they formed a fantastic team of nuclear theorists. And we had daily seminars and discussions. In previous visits to Copenhagen—we were there maybe during the time that we were in Heidelberg—we met Niels Bohr, who was still alive then. And he lived in the old Carlsberg brewery building. It was impressive to be connected to the very old times.

Talking of connections to the old times, when I first came in ’53 [to Caltech], I met [Robert A.] Millikan, who was still around. He was in a wheelchair, but we regularly all met at the physics table. There was a physics table in the Athenaeum, and every Tuesday we had lunch—all the physicists. There were not so many. We all sat around one table. And that was really a nice institution and a nice tradition, which unfortunately does not exist now.

COHEN: Well, there are still tables there where certain groups of people sit…

BOEHM: Yes, but this was more of a general physics table, and you could meet the people working in other fields of physics.

COHEN: Maybe when we’re done with this, we can go back and talk about some of those things. So then you had this year in Copenhagen. How do you look back on that, as far as your own work goes? Was it a rejuvenation? Could you just think about things—since you weren’t involved with all this equipment and with raising money—which is the purpose of a sabbatic year, of course.

BOEHM: Yes, yes. I did experiments there. There was an interesting topic on which I began experimenting. It was at a lab a little outside Copenhagen—part of the University of Copenhagen—the Risö Laboratory. I started some experiments which had to do with angular distributions of gamma rays following Coulomb excitation, and how they can be influenced by large magnetic atomic internal fields. And at that time, it was found that there were very large internal magnetic fields of certain materials embedded into iron, fields up to a million gauss. So these nuclei, if embedded into iron, would be exposed to an internal field of a million gauss and would make precessions. And by studying the emission of gamma rays, you could study these precessions and you could measure these internal fields. That was an interesting and novel technique. I spent quite a bit of time going to this research lab on the night shift and doing the experimental work.

COHEN: So in some sense your life continued in very much the same way as here. Although you must have worked more by yourself. You would not have had postdocs and students.

BOEHM: Right, yes. This work I did with one or two local people from the Niels Bohr Institute, who helped me to get going and helped me to operate the machine, and all that.

COHEN: It must have been an interesting atmosphere there—and probably still is—because it must have been so made up of visitors.

BOEHM: Yes, yes. Particularly at that time—you know, the Eastern Bloc was completely sealed off. The Niels Bohr Institute had some agreements on the diplomatic level to have guests from all the Eastern Bloc countries. So there were Russians there; that was the first time I saw Russians. [Laughter] There were East Germans there, Poles, Czechs. It was all rather unusual and difficult to arrange, but it was for all of us a great way to understand the huge schism between West and East.

COHEN: It must have been even more exciting for them; they were so sealed off.

BOEHM: Yes. And in that connection, I was also able to go for the first time to the Soviet Union. There was a conference in Moscow. My wife and I went to this conference, and we were able to visit various laboratories. It was in January 1968. This was an enormous clash in culture, you know, to see this country and how they do physics—in a way which is so different from what we do. There was an enormous discrepancy in cultural attitude, in doing physics, and of course in life in general.

COHEN: Well, they had some brilliant theorists.

BOEHM: Definitely, yes. We have been to the Soviet Union several times thereafter. And things slowly changed a little bit for the better. And I had many good contacts with Soviet colleagues from Moscow and Leningrad.

COHEN: But it was the year when you were at the Bohr Institute that you really got introduced to this?

BOEHM: Yes.

COHEN: So then you came back here....

BOEHM: I came back here. And there was an interesting activity, which then I pushed forward. There was already good collaboration with Petr Vogel. We wanted to study what are called the isotope shifts of K X-rays. These are X-rays associated with the K electrons. We know that the K electron penetrates the nucleus. And so the information on these X-rays will give us information on nuclear charge distributions. The aim was to study the nuclear charge distribution by looking at the K X-rays. So I made use of DuMond's invention of the curved crystal spectrometer. We built a new curved crystal spectrometer, so we could study the isotope shifts of K X-rays.

COHEN: And then that was something that Petr was doing, but theoretical?

BOEHM: Yes, exactly. I tried to get him interested. He was the right man to do that. He was well equipped. He had all the experience to do these calculations. So this was a several years' enterprise. We had in our subbasement special pads to mount the spectrometer, because of vibrations. These are all precision experiments. What we wanted to see were shifts of the energy of these X-rays by one part in a thousand, so everything had to be very, very stable. We drilled a hole in the floor in the subbasement, down into the soil, and built a big foundation. And on top of this, we built the spectrometer.

COHEN: You were still being funded by the Atomic Energy Commission?

BOEHM: Yes, that's correct. And there were several students. There was one student who was very devoted and active. His name is Paul Lee, a Chinese student from Hong Kong. He got his degree and later went to [California State University at] Northridge.

So that was an active period. And then, connected with that, we were trying to explore what you could do using muonic X-rays. I told you about atomic K X-rays. Muons are much more massive than electrons, and their trajectories will penetrate much more into the nucleus. And that field became of interest for some people who looked into this, including Val [Valentine] Telegdi, who was visiting here. I see him occasionally on campus and we exchange old stories, because we once shared an office in Zurich.

So we tried to learn this field. I had no experience in higher-energy physics. My life so far was low-energy—it was nuclear physics, atomic physics, and so on. And so I thought I should go and learn these things. So I went to CERN [the European Organization for Nuclear Research], in Geneva. That was in 1972. And that gave me a chance to learn a completely new type of physics. The deep motivation was to study the nucleus through these X-rays.

COHEN: There must have been a huge cultural difference for someone like you, who worked with a few people and really built your own equipment, in going to a place like CERN, where you're involved with hundreds of people.

BOEHM: Right. It was a little overwhelming. When I got there, I didn't know how things worked. So I joined a small group that was about to engage in this type of work. And that was a formidable collaboration, and I learned all these new aspects of physics, and how to do the experiments, and how to analyze the data. That was also the time when computers were first used. I had to punch my IBM cards—you know, there was a big stack of cards and you'd carry the stack to the CERN computing center. And you put them in the big machine, and *pa-pa-pa-pa-pa-pa-pa*. [Laughter]

COHEN: So again, you're in a world of visitors.

BOEHM: Yes, that's true. Most of the people in that group were visitors, doctoral students from Swiss universities or German universities. These were all people who came and went.

COHEN: Do people come in with their own grants and their own funding to work there? How did that work?

BOEHM: I did have my own funding. I got a senior NSF fellowship. And I wrote to CERN to request permission to be there as a guest—paying my expenses. And CERN said, “Yes, fine. Be our guest. You can have an office. We don’t pay you a salary, but you can have an office. And you can have secretarial help if you need it. And you can use our machines to do your experiment. Of course, we expect you to write a paper and give CERN some credit.” [Tape ends]

Begin Tape 2, Side 2

BOEHM: So in Geneva I was quite successful. It was eye-opening in physics. From a personal point of view, it was in a way easier than Copenhagen, because it was our home—it was close to our home. I had relatives in the French-speaking part of Switzerland, so it was easy for me. The children could go to English schools in Geneva; there were lots of international schools, so it was very convenient. Geneva is a nice place. We rented a lovely little house on the French part of the lake—still on Swiss soil, near Vésenaz.

So then I returned. And the intention was that we should continue that work in the US. And it so happened that Los Alamos built a wonderful apparatus called LAMPF [Los Alamos Meson Physics Facility]—a linear accelerator—and it was ready for use at that time.

COHEN: So you made a jump to high-energy physics?

BOEHM: Yes. Lots of people would not call it high-energy physics today [laughter]. Higher-energy physics, that’s right. So I applied what I learned, and we set up at LAMPF a series of experiments, using muonic atoms. So we did quite a bit of work in muonic atoms. We had several good postdoctorals. Petr Vogel was very interested, and he calculated these things. And we had good students. We brought a trailer, which we set up outside the LAMPF accelerator hall. We had all our own equipment there, our computers.

COHEN: So the Caltech group set up their own existence there? Was that common?

BOEHM: Yes, it was common; many universities had trailers, and in these trailers there was the data-reduction apparatus: the PDP-11 computer, which was the avant-garde of computers at that time.

COHEN: So you really were computerizing your work, too.

BOEHM: Yes, yes, we had to. So that went on for several years.

COHEN: How much time did you spend there, then?

BOEHM: Myself, I probably went there every two weeks or so for several days. I had to arrange it with my classes. Some of my postdocs, my students, were there for extended periods; they would reside there. And, you know, you apply for a block of beam time. There's a program committee and it says "Yes, you have beam from the first of February to the twentieth of February." Then you have to be there, and things have to work, and everybody has to be ready.

COHEN: Did this entail as large a group as it took in CERN to do this kind of thing?

BOEHM: We had a group of about eight—two or three postdoctorals, maybe three or four students.

COHEN: And the students were getting theses out of this?

BOEHM: Yes, yes. And there was Petr Vogel, who helped us in the theory part. So we had this team of eight to ten people.

At the end of that time, we decided it would be really nice to make a fundamental check of what is called the Klein-Gordon equation. This is an equation which determines the behavior of particles with spin 0 or 1—as opposed to the Dirac equation. The Dirac equation determines the behavioral spin 1/2 particles. The Klein-Gordon equation had not really been tested in that sense. So we used pions, which are what is called bosons. And we made a test of transitions in a pionic atom. This is a negative pion which moves in the field—the Coulomb field of the nucleus—and makes transitions, just like X-ray transitions, and by studying these transitions we could get various pieces of information: the mass of the pion, the coupling strengths with the Klein-Gordon equation parameters. And that we had to do with great precision. So naturally we had experience in precision before, with bent crystal. We built a special bent-crystal spectrometer for that; we built a DuMond type of crystal spectrometer, brought it to Los Alamos, and set it up, and measured these pionic conditions with great precision.

COHEN: So there is continuity?

BOEHM: There is some continuity, yes—in the technology which I had the fortune to inherit from DuMond, the crystal spectroscopy. Also in physics, this distribution of charge in the nucleus. So this went on until probably 1978 or 1979 or so.

COHEN: You used what I think is jargon. You said, "We had the beam." What does that mean?

BOEHM: The Los Alamos machine would use a proton beam. The proton beam hits targets and produces pions and muons. And these are the particles we want. So the machine is run and organized so experiments are ready, and then you get the beam. [Laughter]

COHEN: OK. So your main experimental work was going on there, and not here in Pasadena?

BOEHM: That's correct, yes. We continued some X-ray work here—the work I described a moment ago was still going on. We did some other X-ray work: it was a smaller experiment I did with a postdoctoral to look for forbidden X-ray transition—again, that had to do with parity. Parity violation would occur in certain transitions which are forbidden by the selection rules and may be visible. And so I did a measurement with a postdoctoral Swiss fellow, with the crystal spectrometer here. We did a careful measurement and published a limit.

In terms of postdoctorals, we had many guests from Europe all that time. We had Danish fellows—because I had some contact with Denmark. We had lots of Swiss fellows, and they came with their own money. They came with the Swiss NSF fellowships, and I had a half dozen of those. We had some German guests.

COHEN: Did you have to supplement them a little bit?

BOEHM: Yes. Originally they would be paid for a year, and you would pay them for the second year so that they could stay on.

COHEN: And most of your people were coming from Europe? Did you have any people from Japan or China coming?

BOEHM: Yes, yes. We had various graduate students who were Chinese. We had this fellow Paul Lee, whom I mentioned before. We had later many other people from Hong Kong and Taiwan, who were graduate students at Caltech and then got their degrees and maybe would remain for a year.

I mentioned the European postdoctoral guests, who were very, very good, and it was nice to have these guests fully paid. We had several Humboldt Fellows.

COHEN: Well, it's certainly a tribute to your lab that they wanted to come here, because they could have gone anywhere.

BOEHM: Maybe so. I was a Humboldt Prize winner [1980], and then that helped me to have more contact with Humboldt Fellows, younger ones, several of whom came here.

At Los Alamos I was also involved in committee work; you asked about that before. I was chairman of the Users Organization. Los Alamos had a Users Organization. Everybody who worked there was a user of the machine, and they all had to synchronize their wishes as to what kind of beams they would use, the time, the physics program. So I was for two years chairman of that organization. That took some time. I went quite frequently, and I maintained a nice relationship with Los Alamos. And later I was an advisor to the physics division, in later years, for a long period. During that time, I received clearance for classified work, so I could get involved of all aspects of the laboratory's activities and help make recommendations.

COHEN: Did you enjoy going there? Did you ever move your family there at all?

BOEHM: I went by myself. It was just a short flight from Los Angeles to Albuquerque, followed by a ride in a GSA car. [Tape ends]

FELIX H. BOEHM

SESSION 3

February 2, 1999

Begin Tape 3, Side 1

COHEN: Last time you were going to start telling me about the experiments and the problem with time reversal and how that got your attention.

BOEHM: Yes. The simple symmetry laws of physics have always fascinated me. I was involved in this parity work I described. The problem was whether left- and right-handed systems are the same or not the same. And it turned out in weak interaction, in beta decay, they were not: there was a maximum of parity violations; left-handed systems are preferred from right-handed ones. You know, if you look at nature, it's hard to say. Humans, for instance—we know that our body is not symmetric, our brain is not symmetric. Another big and important symmetry is time symmetry: whether the arrow of time, which we know exists in the biological world—we are getting older, the universe expands, and all that—but is that also manifested in physics, in the laws of physics? The only experiment that was done and which showed some interesting results was carried out by two researchers in 1964—[Val] Fitch and [James] Cronin. And they found a time-reversal violation of a fraction of a percent, in the decay of a neutral K particle. And of course, that prompted many people, including myself, to look for similar time-reversal violating effects in other manifestations in physics. They should be embedded in all of physics at some level.

In my case, I was interested to see whether in nuclear physics such violations occur. So one had to design experiments which would reveal time-reversal violation upon a symmetry operation where T goes into minus-T. This will be the same as if you play a movie backward.

COHEN: If you play a movie backward, it should be the same…

BOEHM: It should be the same as when you play it forward, if time-reversal is conserved. So one can devise things that are odd under T, involving typically three vectors—such as nuclear polarization, the momentum of a gamma ray, and its linear polarization. That was the particular combination we began to investigate. We built—at an early time, when these things were not commercially available—a dilution refrigerator, which allowed us to cool nuclei to a very low temperature of a few thousandths of a degree Kelvin.

COHEN: What is a dilution refrigerator?

BOEHM: It has to do with a helium-3, helium-4 mixture. You pump in that mixture, and then you can go much below four degrees, which is the point where helium-4 liquefies. You can go down to a few millidegrees—very, very low temperatures—with this combination. At these temperatures, then, atomic nuclei become polarized.

So we built such a system, with the help of graduate students and postdoctorals. And we were able to make it work and polarize nuclei. And again, we used the trick I mentioned last time. I mentioned that when I was on my sabbatical leave in Copenhagen, I implanted nuclei into iron and used the internal fields, which are very large, to have a mega-gauss, or even larger sometimes, to produce nuclear polarization. So by using this very large field from internal implantation of nuclei into iron and applying low temperatures, one can polarize nuclei almost completely—so their spins point in one direction. And then the gamma rays which are emitted, are emitted from these completely polarized nuclei. We studied the correlation between these gamma rays and their linear polarization, which can be measured through Compton scattering—it's another well-known technique. So we obtained rather stringent results. We did not see a time-reversal violation. And we obtained results on the order of 10^{-3} in the amplitude of this time-reversal force.

COHEN: So this was not what you were expecting.

BOEHM: Well, we didn't know—no one knew. We just knew that in this K^0 decay there was a small effect seen. But where else in physics it would manifest itself was not known.

So we did not see that. And we then explored various nuclei. The first one was cobalt-57, which we implanted. Other nuclei that we implanted were heavier. There was one called iridium-191 that did show an effect, but we attributed that to what is called a final state effect. This is a heavy nucleus with a large Coulomb force. One could calculate these final states effect—kind of fake effects which simulate time reversal—but they are not really time-reversal effects. But at least it showed that the apparatus worked, and we could relate it to calculation, which was done by us, including by Petr Vogel.

So this was an apparatus which was in operation for several years, and several nuclei were studied. Because if you study one, you don't know whether you have the best case. We have seen in the parity experiments that certain nuclei are strongly favored—show a much larger effect than others—because of the complications in nuclear structure. Anyway, we did that, and we established, over three or four nuclear transitions where we had sensitive measurements, a limit—10^{-3} of this amplitude. And at that point we stopped.

COHEN: Now, would you have graduate students writing theses on this?

BOEHM: Yes, that's correct. Maybe three students got theses out of that. I myself tried to understand related experiments and also the underlying theory. We had a little meeting in Aspen [Aspen Center for Physics]—just four of us—to try to discuss these issues. That was in 1973, and from then on I was much involved in Aspen. I went there every summer, and we had discussions on issues—time reversal and other issues. I was very interested, of course, in the other activities, like music, in Aspen; I went to the music events very often. Even the first year, we liked it so much that my wife and I decided to buy a piece of property on Red Mountain and build a house. So we did that, and the next year we could already move into our house.

COHEN: That was in the mid-seventies. And did you spend the whole summer in Aspen?

BOEHM: Yes. My wife and the children spent the whole summer there. The children were still small. I was there for maybe four or five weeks each summer, and the rest of the summer I had to be back here, since in an experimental lab one needs to be present.

So that was a nice thing. I maybe mentioned this before to you: A year or so ago, when we thought we were not really using Aspen much longer, we gave this house to the institute. So that's the end of that.

COHEN: Well, that's a nice gift to the institute, because I'm sure everything has gone up much in value in Aspen.

BOEHM: [Laughter] Yes, the appreciation has been enormous.

COHEN: With whom did you discuss these things? With other physicists coming from other places?

BOEHM: Yes, in Aspen there were many people who were very interested in these things. We talked a lot to Lincoln Wolfenstein. He is at Carnegie-Mellon. And then we talked to Ernest Henley at Seattle [the University of Washington].

COHEN: I wonder how these groups happen sometimes. Did you know that there would be people you would want to talk to? Or would you try to get an invitation extended to them to come?

BOEHM: Yes, I knew that these people were there. We were just in touch, and we agreed that we would overlap.

At the same time, the subject of neutrinos was becoming more and more intriguing. Particularly Lincoln Wolfenstein was much interested in neutrino physics and the workshops in Aspen in neutrino physics. And I got also very intrigued by that.

COHEN: The neutrino. Which you continue with now?

BOEHM: Yes, which we continue up to this day.

COHEN: Now, neutrinos are, of course, very popular here at Caltech. There's many people that look at neutrinos here.

BOEHM: Yes, yes. We had, in high-energy physics, colleagues who were studying neutrinos.

COHEN: Now, are neutrinos really high-energy physics?

BOEHM: It's both. It's high-energy and low-energy. There are neutrinos in the universe that have all kinds of energies. Actually, the trigger for my interest was a visit by two young European fellows who worked with Murray Gell-Mann as postdoctoral research fellows—one is [Harald] Fritzsch and the other one is [Peter] Minkowski. They worked on these problems and suggested that there could be mixing between neutrinos—that what we call weak-interaction neutrinos are not the original states but the original states are what are called mass eigenstates. And our neutrinos, the weak neutrinos we see, are a mixture of mass eigenstates. There is a mixing amplitude. And all this would require that neutrinos have mass.

COHEN: Now, these young men, were they experimentalists?

BOEHM: No, they were theorists; they built up a little theoretical framework. And if neutrinos have mass and are mixed, then there are neutrino oscillations. They were not the first to talk about this subject. There was this Russian-Italian named [Bruno] Pontecorvo, who was a pioneer in this field—and other people. But the Caltech postdocs worked here on this subject matter and interacted with me. They came often over to West Bridge and we talked about it, and they encouraged me to think about an experiment we could do in our field of physics—not in high-energy physics but in nuclear physics. And in fact we knew well that power reactors emit neutrinos. And these neutrinos are what are called electron neutrinos—electron antineutrinos, in fact. And one would be able to observe these oscillations, if they are present, doing an experiment at a reactor. So that's what we designed. And then I thought, Where could we go? What reactor could be used? And my friend Mössbauer—who, as I mentioned before, had returned to Europe—was then the director of the Laue Langevin Institute in Grenoble, France.

COHEN: Just curious: Is that the Langevin who was the friend of Madame Curie?

BOEHM: Yes, yes, a famous French physicist. I think he was also involved in magnetism.

So that was a wonderful thought. I traveled to Grenoble, met with Rudolf, and we discussed that. And finally we came up with a team, from people in my group and people from the Laue Langevin Institute, and some people from a French institute nearby. We designed a detector, we built it. The detector was built at Caltech in our shops here and shipped to Europe, and we installed it.

COHEN: What is the size of something like that?

BOEHM: That detector was about a cubic yard in size—it was not so big. But surrounding it we had veto counters. We had detectors that would signal when a cosmic ray particle hit it, because that particle would give a false signal. So the whole thing was a little bit bigger than that.

So we mounted this. These [neutrino] experiments are difficult, because the data rate is very, very low. You know, we have a few counts per day, that's all. Maybe the first one, where the detector was close to the reactor, we had a little more; we had a few counts per hour. But still, it was a difficult experiment.

We had some early results which indicated that there were no oscillations—that the neutrinos we measured were exactly those we expected, using our knowledge of how a reactor works—fission products, the beta decay, and the emission of electrons and neutrinos. Around that time—that was, I believe, 1980, or maybe a year or two later—there was an announcement by [Frederick] Reines, a man who was a pioneer in neutrino physics and got a Nobel Prize a few years ago [1995], that he did see oscillations. Reines was down in Irvine. He claimed he had seen oscillations, and he went to the *New York Times* and made a big story. But it turned out that he was wrong.

COHEN: But that's not what he won the Nobel Prize for?

BOEHM: Well, he won it anyway; he had done some early pioneering work. But we remember well a meeting in Erice, in Sicily, where he presented his evidence, and we had evidence that there were no oscillations. So we had some heated debates. [Laughter] Well, we were right—there were no oscillations—and he was wrong!

We then tried to improve the sensitivity by going to a more powerful reactor. The Grenoble reactor was a research reactor—57 megawatt. But we could have much larger data range and could go farther away to measure different parameters by going to a power reactor. So, again, I contacted a friend. His name is Jean-Pierre Blaser; he was a colleague of mine in

Switzerland, at ETH, and he was now director of this institute, which is now called the Paul Scherrer Institute. He was able to negotiate an agreement with a commercial power reactor in a little town called Gosgen, in Switzerland, not far from the Paul Scherrer Institute. He was extremely helpful to arrange all this. We moved the detector from Grenoble to Switzerland and built a big concrete bunker around it and set it up a few meters from the reactor core.

COHEN: Now these arrangements you made with all these people who were friends—they were just informal?

BOEHM: Yes.

COHEN: You called on the telephone and said, “How about let’s do this?”

BOEHM: That’s right.

COHEN: You didn’t do any sort of formal going through organizations?

BOEHM: No, it was very informal. And my sponsor, the DOE, would go along with this. They would say, “Fine, go ahead. Do what you think is good physics. As long as you produce good results, we don’t mind.”

COHEN: It’s a bit unusual.

BOEHM: Yes, that’s really very nice. And so for the next four years, this went on at three different distances from the reactor at Gosgen. It was of interest to explore these oscillations at various distances, because the oscillations involved various oscillation lengths. And the farther you go away, the more sensitive you are to the mass parameter which enters in these oscillations.

Well, the work was carried out successfully, but no oscillations were seen. We had the best limit, but the subject was still very controversial. There was a similar experiment carried out in France at the Bugey reactor by a French group. And they did see oscillations and published it. Well, they were wrong, too. [Laughter] It’s a difficult experiment, and some people were just not taking into account all the background subtractions that had to be done.

So the Gosgen experiment was concluded, I believe, in ’84 or thereabouts. And then we took this experiment apart.

Then my attention was spent on another issue, which also had to do with neutrino mass, and that was double beta decay. That is a rare process where one nucleus does not decay into its neighbor but into the next-door neighbor, by changing its atomic number by two units. And that process can occur without emission of neutrinos, so it’s called neutrinoless double beta decay. And that process violates selection rules which we know hold in physics, and it can proceed only if neutrinos have a small mass. So it turns out that the study of this double beta decay is a very sensitive tool to see neutrino mass. So, in a way, it’s related to the oscillation search, which also can occur only in neutrinos of mass.

So we engaged in these double-beta-decay experiments we set up here, in our Bridge lab. We set up a germanium detector and looked at germanium—whether double beta decay occurs. And this would give rise to a definite peak at a value of 2.04 MeV. One could directly search for this peak. These experiments went on, then, with improved sensitivity, as beta detectors were built. And that was right here on campus. But there came a time when we could not make so much more progress here, because of background—because here we are bombarded with cosmic rays, with muons. And the only way to make progress is to go underground.

So again, through my very same colleague in Switzerland, I arranged that we could go into a mountain tunnel. It’s called the Gotthard Tunnel; it’s a road tunnel now and a rail tunnel. The road tunnel has places where we were allowed to set up.

COHEN: These would be rooms off to the side?

BOEHM: Actually, parallel to the main road tunnel there is a small tunnel which was built to service the main tunnel and also as a way of expanding the capacity of the main tunnel in later years. And this service tunnel and the main tunnel are connected by transverse wings, transverse tunnels. And we could use one of these transverse tunnels, which were maybe fifty meters long or so. They were shut off by doors.

COHEN: And this was not a problem with the government, or…

BOEHM: It was really easy. We had to negotiate with the government of the canton of Uri and the canton of Ticino—they share this tunnel. And it was really a very amicable discussion; they said, “Go ahead. We have no problem.” It was quite easy. We had to obey the rules. We could not make a U-turn in the tunnel; we had to obey the traffic rules so that we would not impede the traffic flow of the tunnel. And we could build the whole experiment. We built a little house inside this connecting tunnel. Anyway, that worked out just fine.

COHEN: And did you have people there who did the work, or did you have all your own people?

BOEHM: No, we had people from the Paul Scherrer Institute. It was a fairly large institute and it had technicians. So they came and built the arrangements.

COHEN: The way you tell it, it sounds very easy, but I can’t imagine that it was that easy.

BOEHM: Well, there are small things to surmount all the time. If I think back, I don’t recall any serious problems, or anything that I had to fight for. It was relatively easy, and people were genuinely interested and helpful.

COHEN: Do you think it’s because you were Swiss that it went so easily?

BOEHM: Maybe that helped. So we carried this experiment through. Again, the people who did most of the work were students. There were Caltech students who were delegated to Switzerland to stay there and to work in this tunnel, and got their degrees. There were a couple of them who did very well and got degrees from that. And as work went on, a time came when the returns no longer justified continuing. If you measure for one year or two years, an additional year will not really justify the efforts.

COHEN: Did you get much better results?

BOEHM: Yes, we got much better results there. We got really very good limits. So we decided to stop that experiment.

In addition to the neutrino mass search, we also looked for what are called dark-matter particles. We know, from our balance of the matter in the universe, that there is some matter that is missing, and we call it dark matter. And this dark matter is not charged, and we don't know what it is—it could be supersymmetrical particles, or something else. But it may leave recoil: it may interact with nuclei and deposit a recoil energy. And we would see this recoil process [with our detector]. So we looked for this recoil process, and we were able to establish good limits. We could then calculate the energy of the particles which would make the recoil, and we would get a rather good limit on these energies of the dark-matter particles. That was a little sideline we could pursue.

But then it had already been decided that to make more progress for these investigations, we needed to have a much larger apparatus. We needed to have a larger number of target nuclei which could decay. And we designed, here, a xenon-136 time projection chamber. Xenon-136 also double beta decays; it will, again, leave a track from that double-beta-decay event—two electrons which, combined, are full energy. We then thought we could measure this track with a time projection chamber. It's a gas chamber where the charged particle would leave a track. It makes ionization, and the ions and electrons are projected on electrodes and leave a track. So we built this TPC here in the basement of Bridge; and, again, it was a fairly large instrument—about a meter by a meter, or so—and it involved rather complicated and demanding electronics.

COHEN: But you did not take this one to Switzerland?

BOEHM: Yes, yes. We completed it and then we moved it to Switzerland, and we put it in the Gotthard tunnel, in the same place where the germanium detector was.

COHEN: How long did that take?

BOEHM: The construction of this TPC took two years or so. It's a rather complicated instrument. We had to master all kinds of technologies. We had to produce printed circuits which represent the electrodes. We had to have many channels of electronics—all these elements of the TPC. But this was all working and ready to be shipped. And in 1990, I spent a fraction of a year in Airolo, this little town at the southern end of the Gotthard Tunnel, with my students. My wife was the housekeeper for the students. [Laughter] She cooked meals. We took care of them and made them comfortable. And every morning we drove into the tunnel and spent the day there, in this dark cave, and got this TPC to work. And eventually it worked, and we have now been able to press the neutrino mass limit considerably.

COHEN: And is it still working?

BOEHM: This is still working now, yes.

COHEN: And who is looking after it?

BOEHM: I had a postdoctoral who came with a Swiss fellowship. His name is [Jean-Luc] Vuilleumier—he's a French-Swiss fellow. He then returned to Switzerland and was named professor in Neuchâtel, a small Swiss university, and from there he could operate this experiment.

COHEN: So it's essentially his project going in there.

BOEHM: It was his project. We still are associated. We helped in data analysis, we supplied equipment.

COHEN: So he has funded himself?

BOEHM: Yes, he funded himself. He got some money and he got two or three graduate students and technicians from Neuchâtel.

COHEN: So now we have you coming back and doing battle at San Onofre.

BOEHM: That's right, yes. The interesting trigger—around 1990, it was observed that the atmospheric neutrinos show an anomaly. One can predict how many nu mu's there are and how many nu e's there are in the atmosphere, because pions decay. Pions are made by impacts of nucleons that impinge upon our atmosphere, and these pions decay. And one can easily predict that there should be a ratio of 2 to 1 between muon neutrinos and electron neutrinos. Well, what was observed in several independent experiments underground was that this ratio was not 2 but much less—a little bit more than 1. There was a shortage of muon neutrinos. What does that mean? Well, one could, for example, postulate that the muon neutrinos oscillate with a very long oscillation length—we are talking about thousands of kilometers. And that prompted us to return to the oscillation experiments. What we'd done at reactors before should be redone at much larger distances. Then we could catch a glimpse at very, very small masses—large oscillation rates, very, very small masses.

So we began thinking about building a much larger detector. The previous detector was about a cubic meter. Now we are talking about a detector which would have a sensitive volume of maybe twelve tons, and additional volumes of water shielding and veto counters. So it would be really a very large instrument—maybe half the size of this room, or even more.

While we were designing that, we negotiated with a local power company in Southern California—the Edison company—about being able to set up such a detector near San Onofre. And that progressed nicely. We had talked to engineers; we were even ready to work on an underground laboratory. We needed to be somewhat underground, to shield [the detector] from cosmic-ray pion rays.

COHEN: And your energy source would be the nuclear reactor?

BOEHM: That's correct, the nuclear reactor. At San Onofre there are two reactors, and they would be supplying the neutrinos. And we would be about a kilometer away. And that distance would give us the sensitivity so we would be able to observe these atmospheric phenomena, these atmospheric anomalies.

We acquired again funding from the DOE, which came after proposal writing and visits to Washington and the usual things. We had many students at the time when we planned this detector and the preliminary test. I probably had six graduate students and six postdoctorals. So we had a good team.

But finally it failed, for a silly reason. And the silly reason was that it was discovered that where we were, there was an endangered bird.

COHEN: Where you wanted to put your detector.

BOEHM: Yes, that's right: where we wanted to build an underground laboratory there was the California gnatcatcher, and it had just been declared an endangered species—just a little bit before. [Laughter] And so that killed the project.

COHEN: Just that?

BOEHM: Just that. We went to negotiate. I took the Caltech lawyer with me, and we went to the Edison headquarters, and we negotiated with environmentalists. And we were just told, "No, you cannot do it." We could do it for a very short period during the year, when the bird is not breeding. But then there is a long period of courtship and nest-building and breeding. [Laughter]

COHEN: Did you ever see one of these birds?

BOEHM: It's a very small bird. Yes, I've seen it. There are plenty of these birds there. The irony of it is that we were on federal land—Camp Pendleton, a huge Marine Corps base, an enormous piece of land. And the Edison company leases its land from the Marine Corps people. And, of course, the Marine Corps people are not quiet. They train. They have jet aircraft that fly overhead; they have tanks that are rumbling; and they shoot. And that apparently does no harm to the birds' survival. [Laughter] But our laboratory would do harm.

So we thought this was all very hypocritical. I even wrote a letter to people in Washington—our congressman [Carlos] Moorhead. I got sympathetic responses, but nothing happened. I went many times to talk to the colonel who was second in command at the Marine Corps, just to try to find ways that we could build this lab. And there was just no way.

COHEN: That must have been really a very trying time.

BOEHM: Yes, that was frustrating, and we lost about two years in these negotiations. It took a year for an environmental impact statement to be written, and during that time everything was blocked.

COHEN: Who did that for you? Did the people here at Caltech do that writing of the proposal?

BOEHM: Well, most of the writing was done by the Edison company. They had biologists, and they had to do it, by law, because we were on federal land. People here helped me—particularly Sandy Poole, the lawyer, was of help when we needed him. But the bottom line was that we were not allowed to build.

COHEN: Now, your detector was built already?

BOEHM: Portions were built as prototype and were in our subbasement and we operated them. We tested them. But that was about as far as we went.

Then we searched for other reactor companies. We went north to the former Hanford lab [in Washington]. There was a reactor or two still running. And finally we found this place where we are now—namely, Palo Verde, in Arizona, which has three reactors. And these people were quite nice and helpful from the beginning and welcomed us. There were no birds and no mice—I mean, it's in the desert. [Laughter] There was nothing to worry about environmentally, because it was just desert land. We then agreed with them that we would build an underground lab. Caltech was extremely helpful. The former provost [Paul Jennings] and the present provost [Steven Koonin] gave me funds so we could build the lab, and we got funding to the tune of $4 million.

COHEN: From the institute?

BOEHM: Yes, yes. I pointed out to the provost that over the last thirty years I had brought in $30 million of overhead. [Laughter] And then we also got some loans. And finally, we got all this money together. In the meantime, some people joined me from Stanford. A young fellow named Giorgio Gratta, who has been a student at Caltech and a postdoc with Harvey Newman and is now an associate professor at Stanford. And he joined with great enthusiasm. He's a young guy and very vigorous, and he helped in these negotiations with Palo Verde. And Stanford gave some money, a portion, for the lab. And there was another guy, who also formerly was at CERN, like Gratta. His name is [Jerome] Busenitz, and he now is at [the University of]

Alabama. And he also came up with a little bit of money and a great deal of enthusiasm. So we were able to get it started. We built the lab, and that was in 1996 when we wrote the contract—not quite three years ago.

COHEN: Now, this was before you retired formally?

BOEHM: Formally I retired in ’95. It was after my retirement. It just dragged out, and I was still interested, and even enthusiastic, and wanted to do this experiment.

So now the lab is built. We are taking data. And we have already a little set of data, and in two weeks I go to Venice to a conference to present that. So we have already some results. Of course, what we have is not the final result; that will take a long time. The data rate, after all, is, as I mentioned before, a few per day. So we have to measure for a long time. And we also need periods when the reactor is shut off, which will give us background. You know, when one reactor is out, we have fewer neutrinos. And then we can derive what the background is.

COHEN: Now, these experiments are different from the one going on in Italy that [professor of physics] Charlie Peck is involved in?

BOEHM: Yes, that’s right.

COHEN: They’re looking for neutrinos, too, aren’t they?

BOEHM: Yes, that’s right. They also looked for high-energy neutrinos. Well, their aim, of course—[Linde Professor of Physics Barry] Barish and Peck wanted to find magnetic monopoles, that’s the main aim.

COHEN: And then there’s something going on in Japan also.

BOEHM: Right. Kamiokande. That is a very large underground lab, and they have done wonderful work looking for solar neutrinos and looking for atmospheric neutrinos.

COHEN: So there’s enough neutrinos to go around for everybody.

BOEHM: Right. In fact, the anomaly I mentioned before—that people have seen fewer nu mu's than nu e's—comes from Kamiokande and was confirmed by the Super Kamiokande, which is a new, much bigger detector. [Tape ends]

Begin Tape 3, Side 2

BOEHM: It has always been my taste not to be a member of a very large collaboration, like what my high-energy colleagues are doing. I prefer to do it in a more traditional way—a small group. And that's what I have been doing most of the time. Now, in this Palo Verde experiment, we are four universities now. In addition to those I mentioned, Arizona State University has also joined; it's nice to have the local university. They have students locally available.

COHEN: How often do you go there?

BOEHM: When things were being built, I went there quite often—at least once a month for a few days. Right now the experiment is running, everything is fully automatic, and there's just one guy, one of my postdoctorals—a fellow named K. B. Lee. He's a Korean postdoctoral; he's there now. We just had a telephone meeting, and that's why I couldn't come earlier. Every week we are on the phone, all of us. The group consists now of roughly fifteen or so members; it's still a small collaboration.

COHEN: So who works with you here, besides your students and your postdocs?

BOEHM: Right now my group is also much smaller. There is a senior research fellow by the name of Andreas Piepke.

COHEN: Do you still support him with your grant money?

BOEHM: Yes, he's on my grant. And there is this postdoc I mentioned, K. B. Lee. And Petr Vogel is always very strongly involved, even in experimental issues. And Herb Henrikson is still part-time consultant and does mechanical things.

COHEN: He's not retired?

BOEHM: He's retired. He's older than I am. So the group is very small now.

COHEN: But you're doing very ground-breaking work.

BOEHM: Well, I'm happy that we finally have data. It has been a long, long build-up process, and a struggle. And fortunately we have these younger collaborators. There are students from Stanford, and students from Alabama, and one or two students from Arizona. So this helps, of course. These are young guys. They are very gifted in computer-related things.

COHEN: Just looking back a little bit, you've not done a lot of committee things here at Caltech, because your interest has been elsewhere. Now, can I take you back a few years and ask you if you were at all involved in LIGO [Laser Interferometer Gravitational-Wave Observatory], being a physicist, and understanding the equipment?

BOEHM: Yes, I have always taken an interest in LIGO. I think it's a fabulous issue.

COHEN: Were you enthusiastic about Caltech picking this up?

BOEHM: Yes, I was. I think this is a wonderful experimental issue, difficult issue. I have talked a lot to a colleague in my building—Ron [professor of physics Ronald W. P.] Drever. He was just upstairs, and he's a very ingenious man, so from him I have learned many things related to LIGO. But I have not gone to their seminars, so I have not actively participated in the LIGO work.

COHEN: Did you have any feeling about the controversy, when it was really so disruptive on the campus?

BOEHM: Yes, I felt really very bad about that. I was kind of on the side of Drever in the controversy. But, you know, I don't know all the aspects. But I thought it was deplorable that this had to happen. The National Science Foundation—the sponsor—was about ready to give up. My personal view, and the little I know, is that [then LIGO director] Robbie [Rochus] Vogt was rather unreasonable, the way he treated Ron Drever. But, again, I really do not know all the angles.

COHEN: But you did not get yourself involved?

BOEHM: I did not get involved, no, no, no.

COHEN: So you have actually been here long enough so that you've been here with almost all the presidents.

BOEHM: Yes, since [Lee] DuBridge. And I saw [Robert A.] Millikan just a few weeks before he died.

COHEN: So how did you feel about the direction of the institute? It's wonderful when we can talk about all the presidents. You obviously got on fine with all of them. I mean, you continued the work you wanted to do.

BOEHM: Yes, yes. I was very impressed by DuBridge. He was president when I came. He was a physicist, and I respected him greatly. He was also a very good man to stand for the institute. His speeches were very good. I liked him very much. After him came Harold Brown. I had very little interaction with Harold Brown, I must say. He was a physicist, too, but he was much less visible.

COHEN: Well, he wasn't here very long, actually [1969-1976]. But I think people didn't expect him to be here very long. And then we have another physicist.

BOEHM: Then we had [Marvin L.] Goldberger, who was also a good president, I think.

COHEN: So none of this has really affected you?

BOEHM: No, I don’t think it has affected me. I mean, the fact that these presidents we mentioned were physicists helped the physics division, which, relatively speaking, was a much stronger division than it is now. But times change, and I have no problem to see biology evolve. It’s fantastically important. And I think our current president [David Baltimore] has a great chance to make a big impact in biological research. But on the other hand, I wish that he would not let down physics. I think there is still a wonderful opportunity for physics, particularly for astrophysics, astronomy, where Caltech has been so strong. And we have the most advanced equipment. And that should be supported, and appointments should be made. And we should get a new building finally. All these things I feel strongly about. I have expressed that to the various division chairmen, that I would like to see our division strengthened.

COHEN: Well, I think the push is to build biology buildings.

BOEHM: Yes.

COHEN: The fact that you’re retired doesn’t seem to mean anything as far as your work goes. But maybe you spend a little bit less time [on campus].

BOEHM: Yes, I certainly spend much less time here. I feel that I do not have the strength I used to have. I cannot work sixteen-hour days. I have also been plagued all my life with migraine headaches, which actually are getting worse now as I get old, for some reason. So often I’m down with these terrible headaches, which I don’t know the reason for, and that prevents me from doing productive work.

COHEN: Is there anything else that we should talk about?

BOEHM: Well, I have also in my notes my various activities. Besides the institute, I have been on various committees.

COHEN: You spoke of the Los Alamos committee.

BOEHM: Yes, the Los Alamos committee.

COHEN: Being a trustee at Aspen, let's go back to that a bit. What did that mean?

BOEHM: Oh, that meant I had to attend meetings and we had to make some policy decisions.

COHEN: And this was for the physics center?

BOEHM: Yes. It's not a big center, and the financial budgets were rather minimal.

COHEN: People came with their funds.

BOEHM: People came with their own funds, mainly. But there was also the question of how many people do we want, ultimately? Do we want to have another building? I always felt it was important not to grow too much—leave it about the size it is. Have people come for shorter periods, but more people, rather than have certain guys spending three months in Aspen. I was much in favor of inviting foreigners. At one time, young Soviet physicists were invited. That was a big thing; it was difficult to do; but I thought it was a good move.

COHEN: You've gotten many honors. I see you were elected to the National Academy [1983]. Do you go to the meetings?

BOEHM: I go occasionally, but not often. I have not been since 1995, I guess. I go every five years, not very often.

I was given the Humboldt Prize some years ago, and that was nice. That allowed me also to spend the time in Grenoble. I think I mentioned it before; I have also supported many young Humboldt postdoctorals who came here from Germany, and I was their host.

I got the Bonner Prize some years ago [1995] in Washington—that's a nuclear physics prize.

COHEN: And I see that you continued to serve on the boards of some journals.

BOEHM: Yes, I'm still on the board of some journals. I'm spending quite a bit of time in reviewing for journals; everybody does that.

COHEN: To sum up a little bit, you would say that your life at Caltech has been very good?

BOEHM: Yes. I think Caltech has been good to me. I wouldn't have remained here otherwise; I had other possibilities, but I thought Caltech is a wonderful place. It was good to me. Bacher, as I mentioned before, was very energetic and a good man. And other division chairmen as well. And the general atmosphere—because it's a small campus and a small physics division, with excellent possibilities of interaction in all kinds of fields that are adjacent to my own field.

COHEN: And you're hoping that physics will build up again.

BOEHM: And I really hope that physics will build up. And I hope that people agree on new focus points. You know, nuclear physics is no longer terribly relevant; academically, it may be not so important. The Kellogg lab has also decreased in size and scope. High-energy physics has also decreased. There should be other fields that will fill in, which would give Caltech physics some visibility and strengths.

I personally feel that astrophysics and cosmology—including LIGO—are excellent fields to go into. These are hard things, but that's what we need—hard things to do, so we can really put our teeth in. I still think that neutrino physics is not exhausted; there are still many more problems with neutrinos. You know, we have a hunch that our world is full of what we call Fermi neutrinos—neutrinos of low energy that are everywhere and that contribute to the mass of the universe, if neutrinos are massive. We know very little about those. One could think of doing experiments to detect these Fermi neutrinos. There are many issues that are worthwhile thinking about and doing.