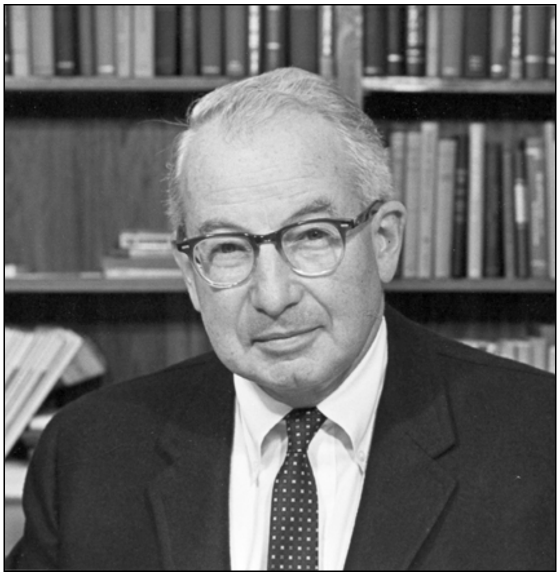

ROBERT F. BACHER
(1905–2004)

INTERVIEWED BY
MARY TERRALL

June–August 1981, February 1983

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Abstract
An interview in ten sessions, 1981 and 1983, with Robert F. Bacher, chairman of the Division of Physics, Mathematics, and Astronomy (1949-1962), Caltech's first provost (1962-1969), and professor of physics, emeritus. He recalls his education at the University of Michigan and graduate work in physics at Harvard (1926-27) and Michigan, where he got to know J. R. Oppenheimer and the European physicists who joined the faculty and/or came for the summer sessions in physics: Goudsmit, Uhlenbeck, Fermi, Bohr, Ehrenfest, Dirac and others. Recalls postdoc year at Caltech (1930-31) working on atomic spectra; Oppenheimer's lectures; Millikan's cosmic-ray work. Spends 1931-1932 at MIT working with John Slater; Chadwick's discovery of the neutron. Spends the next two years as a postdoc at Michigan, working with Goudsmit. Instructorship at Columbia, 1934; association with I. I. Rabi. Moves to Cornell in 1935; recollections of Hans Bethe; cyclotron work on neutron energies.

Early 1941, joins the Radiation Laboratory at MIT, of which Lee DuBridge was director. Recalls start of Manhattan Engineer District; contacts with J. R. Oppenheimer and General Leslie Groves. Joins Los Alamos in June 1943 as head of experimental physics division; recollections of bomb work. Returns to Cornell in January 1946. Postwar development of high-energy physics; Acheson-Lilienthal Report on international

control of atomic energy. Establishment of the Atomic Energy Commission, fall 1946; he becomes a commissioner; moves to Washington, D.C. Recalls weapons testing in the Pacific and the development of nuclear reactors.

In 1949, he becomes chairman of the Division of Physics, Mathematics, and Astronomy at Caltech. Called back to Washington to testify at Hickenlooper hearings; warns the British about Klaus Fuchs. Discusses the postwar buildup of physics at Caltech; comments on the mathematics and astronomy departments. Debate over tactical vs. strategic nuclear weapons. Service on President's Science Advisory Committee; the McCarthy era; comments on his service as Caltech provost. Comments on establishment of Fermilab; participation in the International Union of Pure and Applied Physics. Recalls advent of Harold Brown as Caltech president in 1969; comments on reorganization of NASA contract with the Jet Propulsion Laboratory. Comments on current setup of Caltech's Faculty Board and on his own activities since his retirement.

Administrative information

Access

The interview is unrestricted.

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Contact information

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

CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

INTERVIEW WITH ROBERT F. BACHER

BY MARY TERRALL

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Robert F. Bacher **by Mary Terrall**

Pasadena, California

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TERRALL: You were born in Loudonville, Ohio.

BACHER: I was born in Loudonville, Ohio, on August 31, 1905. Quite a few of my relatives lived in that area at that time, in and around Loudonville, which is quite a small town. East of there is a town called Wooster, where there's a college; I had an uncle who was a professor there. My parents moved to Ann Arbor, Michigan, about 1908, where they continued to reside for many years.

TERRALL: What did your father do?

BACHER: Well, my father was first in the insurance business. He died in 1929, and some years before he died, he had heart trouble. At that time, he worked for the First National Bank in Detroit, in their bond division, so while he worked in Michigan he went into Detroit quite frequently.

I attended the Ann Arbor schools. I went to a primary school named Perry School, later I went to Ann Arbor High School, and then I went to the University of Michigan. So I had all of my education up through my bachelor's degree in Ann Arbor. During the Perry School days, we lived near the Dows, and a son, Philip, who was in the same grade at Perry School with me, was probably my closest friend. His sister, Jean, who was somewhat younger and also went to Perry School at that time, is my wife. We were married on May 30, 1930, which you'll find is just after I took my doctor's degree.

Going back to the grade-school period, I was reading an article of Shirley Hufstedler's in a recent *Engineering and Science*. She was remarking about the seventh grade and how things start going haywire in education at that time. I remember quite vividly that one of the things we did in the seventh grade was probably about as useless as anything I can remember—and that was, we diagrammed sentences, so that you knew what was the subject and the verb. They had a very elaborate way of doing this. One of the things we did was to diagram the Gettysburg Address. This is a rather complicated thing to do; I think it was probably completely useless. Many of the things we did in school at that time—not only in grade school but also in high school—didn't get ahead with the subject matter as much as they should have. That's the point of Hufstedler's article. The situation got much worse, as you'll see later when I talk about graduate work.

TERRALL: You felt it was just rote learning?

BACHER: Well, you were doing things that followed a form that had been built up over the years, and they did not look toward trying to form a foundation for things that would be useful to you in the future.

TERRALL: Do you remember being bored?

BACHER: Well, yes, it went much too slowly, in spite of the fact that I skipped a certain number of grades. Also, they didn't do things that would have been proper to do at that time. For example, a similar amount of time spent in teaching languages would have given me a much better background. My feeling is that from the seventh grade on through high school is just when children pick up languages very well, and they ought to concentrate more on it. I learned some German at that time—I was tutored in German—but I forgot it subsequently. It didn't do me much good when I needed it.

My family liked walks and outings and picnics. This was quite important in our lives. They had many friends who were close to them. They attended church regularly, and they had, particularly in connection with their church group, people they went walking with. This was before we had a car, and cars weren't very prevalent; there weren't very many paved roads at that time. Also, when I was small, they liked to rent a cottage at a nearby lake. This wasn't too easy to do at that time. We spent a number of summers at a little lake called Crooked Lake. The only way you could get there was to go on the interurban car to Jackson, Michigan, which was about thirty miles west of Ann Arbor. About midway along, you got off at one of the crossroads, and you could get the farmer to meet you and take you to the lake. When my father came on weekends, he usually walked over—it was only three miles. We lived there all summer, essentially with zero contact with the town. We didn't have a car, didn't have anything.

I remember the man who lived in the next cottage and with whom I spent a lot of time because he liked to fish. He also spent a certain amount of time writing—he was a composer and wrote band music. His name was Henry Fillmore. This was a great experience, to run into somebody who had as different a point of view as that. My mother was interested in music and studied voice and taught voice for some time. Later, or about that time possibly, she had a goiter operation and this was the end of her singing days, but she was interested in music all her life.

A nearby lake was Cavanaugh Lake. I mention this because we used to walk over there occasionally and in 1920—it was the beginning of my junior year in high school—my family bought a cottage there. That had quite an effect on me, because essentially every weekend the family went out there. Just before then, my family bought their first car, which was a Model T Ford. The road out there wasn't very good then; there wasn't a paved road that went all the way out there or even close to it. So it took about an hour to go out there, even though it was only about twenty-five miles. But we spent summers there. I became very close friends with the families next door. Our cottage was on a hill, and on this hill—sixty feet or so above the lake, with quite a steep slope down to the lake—were three houses. The other two belonged to professors at the University of Michigan. In the one next to us was [Louis C.] Karpinski, who was a mathematician, and the second one over was Professor [Harrison M.] Randall. He was the head of the physics department at the university. I knew his children. His oldest daughter is Mary Emerson, who is the wife of Sterling Emerson, now professor emeritus of biology [at Caltech].

TERRALL: So you knew her from those days?

BACHER: Yes, I was at her wedding. Her husband just recently had his eightieth birthday. They're still close friends of ours. There were four children, and two of them are still alive. There were two girls who were older, and then a boy, Robert, and a younger son, John. Robert was a little older than I and John a bit younger. John must be retired now—he was a professor at Ohio State—in geography, if I remember correctly. Robert was the one who was closest to my age and whom I knew best. We spent most of our time swimming. This lake was about a mile and a half long and three-quarters of a mile wide, and we would swim anywhere in the lake. This was a wonderful free time.

Well, I graduated from the Ann Arbor High School, and I think I ought to say something about that. My introduction to science was a chemistry course, which I took in the junior year and which, in retrospect, didn't really do very much. Some of the things we did in laboratory were just absolutely out of this world—I don't think they really got very deeply into the subject. The instructor was a man named Stitt. I sort of liked it, but I didn't think very much about going into chemistry. The next year, I took physics from a man who was teaching in his forty-ninth or fiftieth year as a member of the staff of the Ann Arbor High School. His name was Chute, Horatio Nelson Chute. He'd taught there forever; in fact, Jean's mother had taken physics from him in high school, and he'd been there as a fixture all those years. I think he probably had taught the same physics for most of that time. He was, however, extremely tough, very much a disciplinarian. He also had built up quite a lot of laboratory work. The laboratory instructor was a man named Bradford Wines, who was the son of the man who taught the more advanced mathematics, which was not very advanced. I can remember making measurements and a lot of laborious calculations. The first time I went in there, he checked them on a slide rule. Before the next laboratory meeting, I went to the bookstore and purchased a slide rule. I appeared at the laboratory, and when it came to make my calculations, I made them on the slide rule. I brought them up to him, and he said, "Well, you're supposed to calculate these things." I said, "If they only need to be done as accurately as they're done on a slide rule and you check them that way, why can't I?" Well, he thought that was a pretty good argument, so he let me do it [laughter]. That's how I learned to use a slide rule. I'd never seen a slide rule before I went to that laboratory, but within a week I had one. It wasn't a complicated one, but that's long, of course, before there were calculating machines that did anything.

TERRALL: Was this something you felt really captivated by? And you hadn't, as a child, been interested in…?

BACHER: Well, I sort of skipped over that, I'm afraid. In answering that question, I ought to go back and say that prior to running into chemistry and physics—which really didn't capture me terribly much—I had in grade school built a telegraph with a fellow who lived across the street. We had a very elementary telegraph, which we operated not on batteries but by a method of tapping off some voltage from the lighting circuit. We used to send messages back and forth in code with a key. The whole thing was pretty elementary.

Then when I was in high school, I did build a radio. This was in the period before you could buy a radio, and I built a radio, which in my opinion was not much good. This was before the vacuum-tube days. I may have learned a bit from it, but I don't think I learned very much. So, even going through these courses in science I didn't get any particular stir from it. What really stirred me was something I was just coming to.

During my senior year, 1921-22, I was doing that kind of stuff, which I thought was really pretty dull. I had a friend in high school who was one year ahead of me—his name was Joseph Grant. He went to the University of Michigan in Ann Arbor. He hadn't been there very long before he said, "My, you should come over and see the books that are in the chemistry library. It's just out of this world." I said, "Well, I can't go in there." And he said, "Oh, sure you can." This was before I wore long trousers; I was sixteen years old, and boys wore short trousers then until approximately that age. So one day when it was raining and I could wear a slicker so they wouldn't see my trousers, I went over there and I got a chance to see the new-book shelf. This just absolutely was out of this world. I'd never seen anything like that. I pulled down a book, which was Aston's first book on isotopes [F.W. Aston, *Isotopes* (London: Edward

Arnold, 1922)]. I took it down and started to read it and found I could understand some of it and some of it I couldn't understand at all.

Now, this was when I was a senior in high school. This book had a lot of things in it that I vaguely knew existed or had seen some reference to, but [it] was essentially his description of the work he had done over many years to measure the mass of the various isotopes of the elements. I found it fascinating. Within half an hour, looking at that book, I knew what I wanted to study. It was just that quick. Of course, I thought it was chemistry, because I'd found the book in a chemistry library. I'd never heard anything about such things in physics; there wasn't a mention of an atom in the physics course I had.

TERRALL: Physics was more mechanics.

BACHER: It was all the classical stuff. The book we used was written by [Henry S.] Carhart and Chute. Carhart had been at the university, and I think he was dead by the time I took this course. I haven't looked through that book in years, but there's essentially very little in it that's illuminating about modern physics.

It was this book on isotopes that really started me off. I thought it was chemistry, [but] what I read there had no relation whatever to what I'd studied in chemistry, or was hearing in my physics course, so I talked to my father about it. He pointed out that our neighbor two doors over at the lake was Dr. Harrison Randall, who was the head of the physics department at Michigan and whose children, as I said, I knew well. He thought I should go and talk with him, which I did. I told him of reading some of Aston's book. He said, "Well, that's physics!" He went on to give me chapter and verse of how that was essentially physics. I said it didn't have anything to do with what I was studying then in high school. But that, more or less, settled it.

TERRALL: Did you follow up on this book? Did you read the rest of it? Could you get it from the library?

BACHER: No, I couldn't take it from the library; it was on the new-book shelf. But I went over and I read quite a bit of it. From then on, I used that library whenever I had time. They didn't seem to object to my going there; nobody threw me out or anything. So I would drop in from time to time. We lived quite close to the university, and I could go there without any trouble and stop in and read in the afternoon—which I did very often. It was a great thing for me to have that chemistry library there. I didn't understand anything much about the chemistry I saw in there. But while there were parts of these books, especially the quantitative things, which involved concepts and things I didn't know anything about, because I had never had any physics that really pertained to this—quite a lot of it I could get. Also I could visualize enough to see that this was really what I wanted to study.

Incidentally, during the summer of '22, after I graduated from high school, and also the following summer after my freshman year in college, as was quite common then, I got a job as a laborer working on a construction job on some buildings the university was building. They were moving some houses and increasing the size of the campus. My first job was as an assistant to a man who was laying up tile for foundations for the houses. They'd move a house from one part of town and make a foundation that would fit under it. The second summer was tougher. I worked on a hospital, the construction of which had been stopped when it was getting close to the end, because of the First World War. It had been abandoned for several years, and then they started to construct it again. I spent a month or six weeks or more swinging a sixteen-pound sledge, breaking up concrete, to make alterations. This was very good exercise for me. I could do that nine hours a day and never notice it. In fact—as sort of a joke—when I went to college, they gave you a physical examination because you had to take physical education. They'd find out how many times you could chin yourself. So I chinned myself twenty-five times, and I'd been doing it with just no effort at all, and all of a sudden I realized I was going to have to improve during the year, so I stopped. The man looked at me and said, "Well, you could chin more." I said, "Gee, I don't think so." [Laughter]

But this laborer's job was a great thing for me, because it really gave me a muscular build that stayed with me for a long time and largely went back to that amount of exercise during those two summers.

When I went to college, in the fall of '22, I didn't think of going anywhere but Michigan. After all, my family lived in Michigan. I think tuition was $50 a year, and if you lived at home it didn't really cost you anything to go to college. Very few students went away to college; some did, whose families had more money, but most of the students who graduated from the local high school and were going to college went to the university. So I went to the university in the fall of

1922. In spite of what I'd learned about where my interests were, I started out taking chemistry and qualitative analysis, because that seemed something to do. I had what I think in retrospect were some peculiar ideas of what one ought to do in college. I had a definite idea that you ought to spend a fair part of college getting a general education, so what I did was concentrate on taking mathematics and chemistry and general subjects. But I didn't take any physics at all until I'd been in college for two years. In retrospect, this was a mistake, because by that time my mathematics training was so much more [advanced], and I had to do the elementary things in physics in parallel with more advanced things in mathematics. This wasn't a good idea.

TERRALL: Were you thinking of majoring in physics?

BACHER: Yes. I was quite clear that I was going to major in physics by that time, but it was a question of what was the best way to do it. This was also, to some degree, influenced by the fact that I joined a fraternity, Kappa Sigma. My fraternity brothers, with only a couple of exceptions, their main interest was how to get through college without doing any work. And let me say that they were very expert at this. They really knew how to get through college without doing any work, and they worked very hard at that subject. But they had a great reluctance to doing what was the natural thing to do, and the idea of getting deeply involved was something they avoided like the plague. One, however, was an engineer and a very interesting fellow. I saw something of him. He was a very good student, and he was also an All-Conference guard on the football team—a great big fellow—and a very unusual guy. He's dead now. My college roommate, McKenzie Shannon, was in the category of doing as little as possible. He ended up as a lawyer—and a good one—and he's still alive, and I still see him from time to time. He lives in Hawaii.

One of the things that affected my college education was that during my sophomore year I became the house manager. I've forgotten exactly how this happened, but somehow or other they needed someone to take on the job. The fraternity had a big house on the edge of town. They'd bought an estate. This was quite an undertaking. At that time I was eighteen, and I took over the management of a house that had something in excess of $20,000 a year in the operating funds, which was then quite a lot of money. We had, I guess, six full-time employees for whom I was responsible, and also I was mainly responsible for making the thing come out even. Well, I learned quite a lot out of that, but it also took a lot of time. Especially during my junior year, it wasn't terribly good for me. I was trying to get caught up in the introduction to physics and running a fraternity house and managing help, trying to do all these things, and living in a fraternity house. Well, I saw that and I gave this job up at the end of the junior year and went home to live. By that time, my family lived four or five blocks from the fraternity, so I went back there regularly for dinner, but I stayed at home and I spent all of my time trying to get caught up and learn some physics during my senior year. I did pretty well at it; I ended up with a straight-A average that year. My undergraduate education had a lot of holes in it and wasn't really very good.

TERRALL: Did you have influential professors who made a particular impression on you?

BACHER: Yes, I had two or three who were really very good; but I think my professors in graduate school, when I went back there later, were much more impressive, because the department at Michigan was in a process of evolution at that time and bringing in a lot of new people. With one or two exceptions, the people I had for undergraduate courses didn't really stir me very much. But I still was convinced that this was what I wanted to do, and I was pretty determined about it. This was a different sort of a decision from the sort that people make today, because there were so few physicists who had jobs in industry, and there were at that time not very many jobs in universities. You couldn't tell at all whether, if you took a doctor's degree in physics, you'd get a job as a physicist or not. It was a very chancy thing.

TERRALL: Did you go directly on to graduate school?

BACHER: I went immediately. As a matter of fact, when I really started catching up, I went to school in the summertime also, so I had enough credits to complete my undergraduate work by the middle of my senior year. The last term I spent almost entirely on physics and mathematics courses. But to give you some idea, we weren't nearly as far along as students are nowadays. For example, physics was generally taken not as a freshman course but as a sophomore course, and the first course in physics didn't really use calculus. I didn't get calculus until I was a sophomore in college. The undergraduate education I had was very much inferior to the education at Caltech at present.

Caltech at that time was probably better than what I had but certainly inferior to the education that's given today. Referring again to that article of Mrs. Hufstedler's, she says that in Russia 3 million students take two years of calculus in high school before graduating, and in the U.S. at present about 100,000 take one year. In 1922, when I graduated from high school, I think it was zero. And in fact, since you didn't really take calculus until you were a sophomore [in college]—in the normal line of things, unless somebody steered you in a different direction—those two years compare to minus one, because you didn't do it until you were a sophomore. That's an enormous difference in background. Indeed, Caltech always had higher standards for entrance than most universities had, and they required more in mathematics. But I took all the mathematics that was available in high school. I took the most advanced course they gave, and it really wasn't very advanced.

Well, when I finished undergraduate work, I continued for the remainder of that year and proceeded to take mostly physics and mathematics courses. But I wanted to go away for at least a year. In part because of Randall's recommendation, I applied to Harvard and was accepted. I spent the year 1926-27 there as a graduate student. The most interesting course I had was a reading course, which I took in the second semester jointly with Gerald Almy, who was later head of the physics department at Illinois. We studied with John C. Slater, who was then a young assistant professor at Harvard. Well, John Slater became one of the outstanding theoretical physicists of that generation in the country. This was a very, very interesting course. This was my first introduction to the new quantum mechanics. I'd taken a course that really should have had some quantum mechanics in it but didn't. My first introduction to wave mechanics came from this reading course with John Slater.

The famous papers of [Erwin] Schrödinger and others were just being published at that time. In fact, Schrödinger came and lectured at MIT, and I can remember going down and listening to some of his lectures, of which I didn't understand very much. But the standard courses had not yet incorporated this material. It was a time when everything was changing like mad in physics. In fact, physics in the United States, one could say, went from being sort of third [rate] through being second [rate] to being as good as anyplace, over a period of about ten or fifteen years. By the mid-thirties, the physics in the United States was very good. That all happened during this particular period, and it's quite interesting, I think.

While I was in Cambridge, my father wasn't at all well. It was clear that I could not afford financially to return to Harvard. But I had rather looked forward to going back to Ann Arbor, and furthermore, when I began to look into it, it would have been wild to go back to Harvard, because that was the year that [George E.] Uhlenbeck, [Samuel A.] Goudsmit, and [David M.] Dennison, who were all very able young theoretical physicists, came to Ann Arbor. Otto Laporte, who was also a very fine theoretical spectroscopist, had come the year before. He was there the year I was away. In fact, the summer I came back, I started working with him and did some calculations for him on a paper that he was working on. Michigan really took a quantum jump during that time and added four very young people in theoretical physics who were really extremely interesting. This made it a fascinating place to go.

Begin Tape 1, Side 2

TERRALL: So at least you didn't have to feel gypped by not being able to go back to Harvard.

BACHER: No. In fact, the opportunities for me were very much greater. I learned a lot of things at Harvard, and I wasn't well prepared for some of the things I studied there. The first semester was fairly traumatic for me, but the second semester I did pretty well. By that time, I was beginning to make up for some of the deficiencies that had come by this too-rapid covering of undergraduate work.

Well, I was saying that during the summer of 1927 I did some work on atomic spectra with Otto Laporte. This was before Uhlenbeck and Goudsmit came. Uhlenbeck and Goudsmit were very well known, because they had discovered the electron's spin. Though they were coming to Michigan essentially just the year after they got their doctor's degrees, this was the way it was done in Holland. I think Goudsmit had probably written a dozen or twenty papers, any one of which would have been satisfactory for a doctor's thesis. By the time he actually got his degree and came over, he was a well-known theorist and one of the leading people in atomic spectra.

I started, as soon as he came over here, to take a course with him. At the risk of getting something into this record that may be too long, I'll report how I really got started working with Goudsmit, because that turned my career around. I took this course with him, which was on spectra. Goudsmit was not highly mathematical, but he knew more about the general ideas of spectra than essentially anybody else at that time. He started giving this course, which he decided he had to pitch at a rather elementary level. It wasn't a large course—he had it in a relatively small room, and I suppose there couldn't have been more than a dozen or fifteen people there. He was then starting out to describe how the Hund theory of atomic spectra originated and the multiplets that exist in the spectral formation that had been worked on for many, many years, without knowing really quite what they were or how they originated. He had started with the simple elements of the periodic table. After he'd gone on for about three weeks or so, he said, "Now, somebody tell me an element, and I'll show you how to figure out what the ground state of it is." And I was sitting in the third row back, and I said, "Gadolinium." Well, gadolinium—and I knew this—was in the middle of the rare earths and was absolutely the hardest element in the whole periodic table to figure out what the ground state was. He looked at me and said, "Well, what's the atomic number?" I told him, "I think it's 64." The previous summer, when I was driving back and forth from the lake, I decided that if I was going to work at spectra, it would be useful to learn the periodic table. So while I drove back and forth, I was learning the periodic table.

So I just asked him this question, and he looked at me. Then he started figuring it out. Well, it was a terrible job; it was an awfully nasty question to ask. When the class was over, he stopped me and said, "How about coming around this afternoon and talking to me in my office." I went around and talked to him. This was the beginning of my working with him. He wanted to know how I knew there was such an element as gadolinium and that it would be tough and so on. From then on, I worked with Goudsmit. We did a number of things while I was a graduate student there, and I, of course, took other courses and so on. But I started working with him. This was really wonderful for me, because while I was there as a graduate student working for my degree, I averaged two or three hours a day with him. When I was working with him, if I didn't get a chance to see him in the daytime because he was busy, I'd go to his house at night. This was the way he worked. He worked by talking. He had an extraordinary knowledge of what went on. I learned an enormous amount from him, and also he knew every physicist in the world, which was great for me.

In addition, after Goudsmit, Uhlenbeck, Dennison, and Laporte came there, they started this famous Michigan summer session. In fact, here on that calendar is the 1931 group. In the row where the speakers are, you'll find Uhlenbeck, whom I got to know very well and subsequently I used to play squash with him.

TERRALL: Sommerfeld?

BACHER: Sommerfeld. I met Sommerfeld, but I never knew him well. He had been Otto Laporte's professor, and he was also [Hans] Bethe's professor. I didn't know Bethe at that time. Next was [J. R.] Oppenheimer. The man to the right of Sommerfeld was [Hendrik A.] Kramers. Well, Oppenheimer and Kramers I got to know very well at a later date; I didn't know Kramers then, I don't believe, but I did know Oppenheimer, because he had been there in the summer of 1929.

TERRALL: That's when they started the summer school?

BACHER: No, they'd started it before but on a less generous scale. This was, I think, Randall's idea—that he could get a little money for this to get it started and, especially with this group, he could really get it going. During this period it was, I guess, *the* outstanding summer session. That's quite a large group of physicists there.

TERRALL: Randall was the one who was responsible for bringing these people from Europe to Michigan?

BACHER: Yes, he and Professor [Walter F.] Colby, whom I don't see in here. Randall was the chairman of the department; Colby was probably the senior theoretical physicist. He worked mainly on the theory related to what was then one of the principal fields of endeavor at Michigan, infrared spectra of polyatomic molecules. Dennison was an expert in this field; Colby was an expert in this field. It was probably the leading place in this particular field of polyatomic molecules. It's not a field I worked at and I never knew much about it, because Goudsmit worked in atomic spectra. Laporte also worked in atomic spectra. Atomic spectra were one of the principal lines of research there, which was also true at Caltech when I came out here subsequently. But people who were there from 1927 to '30 included [Niels] Bohr, [Paul] Ehrenfest, [Enrico] Fermi, Oppenheimer, [Gerhard H.] Dieke, Philip Morse, [John H.] Van

Vleck, [Ernest O.] Lawrence, [P. A. M.] Dirac, and many others. It was a great opportunity and a great privilege for me to hear lectures by these physicists in the summertime. So I just dug in there year-round. In addition to the lectures, everything was conducted in a quite informal way. There were always opportunities for questions and discussion.

I've been talking about this whole period; now I want to go back a little bit into it. My father died early in 1929. I mentioned that he had been ill during the year I was at Harvard, and he wasn't well after that. He'd had a heart attack and he just wasn't well, and he died in 1929. I spent some of the summer of 1929 with my mother at the cottage out at the lake. By that time, there was a paved road out there, and I could take our car and drive back and forth, and some of the time I stayed in town. But very often I'd drive in from the lake, because Mother liked to be out at the cottage in the summertime and I wanted to be with her as much as I could. Every Sunday, many of the staff members and visitors came to swim at our cottage. We'd have quite a large number of people for a simple supper afterward on the porch. This is how I got to know a lot of these people.

As I said, my work with Goudsmit was on atomic spectra, and Goudsmit's background was really fantastic on this. He had worked particularly on what was called the hyperfine structure of atomic spectra, which had just been recognized as being due to the interaction of the magnetic moment of the atomic nucleus with the electrons outside. Goudsmit had worked rather closely in Tübingen with an absolutely outstanding and talented experimentalist named Ernst Back. He had worked particularly on the elements thallium and bismuth, and Goudsmit had a lot of the material he worked on.

Indeed, this guided what I was starting out to work at, which was to work out the quantum-mechanical problem of the magnetic interaction that went from a very weak magnetic field through the intermediate field strength—which could not really be handled except by quantum theory—into the strong field. The weak-field Zeeman effect, as it was called, which was the magnetic splitting of lines when the field splitting was small compared to the rest of the structure, was something that had been known in multiplets; but in the hyperfine structure, due to the nuclear interaction, you could go much further in this, because the splittings were so much smaller. I did work this out for some relatively simple cases. The fit between the theoretical results and the experimental data was really quite remarkable. I first calculated the results in thallium, which were particularly interesting because it was a simple case. The measurements of

Back were really out of this world. These very fine structures he measured to one percent or something like that. The calculations were good to that degree.

TERRALL: So the experiments had already been done?

BACHER: The experiments had been done. As a matter of fact, in thallium we calculated the patterns knowing that the experiment was being done, and he sent the measurements after the theory had been done. We had the predicted values before we got the experimental data. But my thesis was mostly on bismuth, and that was interesting but less satisfactory in some ways, because the patterns were much more complicated. Back's work was beautiful, and again, an opportunity to work on this with Goudsmit was really very good for me. My work in hyperfine structure, incidentally, since that was an interaction with the nucleus, resulted in finding out something about nuclear properties. This was really how I got interested subsequently in nuclear physics.

My doctor's examination was in mid-May of 1930. So you see, I was there as a graduate student in Michigan for three years, after having spent a year at Harvard. Jean and I were married on May 30th, two weeks after I took my doctor's examination. My mother turned over the cottage at Cavanaugh Lake to us for the summer. This was a summer when Fermi and Ehrenfest and Dieke were there as guests, and probably some others, too, whom I've forgotten. But I can still remember the first time that Fermi came out to the cottage. I remember it vividly, because he came out and waded into the water and just started to swim out. Well, at that time, as I explained, I was a reasonably good swimmer. I saw this man swim out, and I always was worried about people getting in over their heads and not being able to get back. I didn't have any idea how well Fermi could swim. I found out subsequently that he always did this. He'd swim half a mile or more out into the ocean. So he swam out and I swam out and caught up to him, and I said, "Are you going to swim a long way?" He said, "I thought I'd swim across the lake and back." I said, "It's three-quarters of a mile across there." "Oh," he said, "that's just fine." I said, "Do you mind if I swim along with you?" because I didn't like the idea of somebody…

TERRALL: You felt responsible.

BACHER: Well, I swam quite long distances at that time, so I swam across the lake with him. We didn't swim too fast and we talked some, and I got to know him fairly well across the lake. We swam all three-quarters of a mile across the lake, three-quarters of a mile back. He didn't think anything about it. This was my introduction to Fermi, and I got to know him fairly well. He was a wonderful man, and I'll come back to him later.

In the spring, I had been awarded a National Research Council fellowship. The question was, Where would I go? For a number of reasons, I chose Caltech. Well, Caltech, interestingly enough, was one of the larger graduate schools in physics at that time. I've forgotten, there must have been forty or fifty graduate students here at that time, and a fair number of postdoctoral fellows came here. It was a place that was changing very rapidly. I didn't know a lot about what went on out here.

TERRALL: So how did you make your decision? Was it on other people's recommendations?

BACHER: Yes, mainly on other people's recommendations, and I think partly on the fact that probably the best experimental spectroscopist in the country was Ira [Sprague] Bowen, and he was here at that time. In fact, his laboratory was in the room in which the division chairman now has his office. Bowen worked in ultraviolet spectra and he was a whiz at it; I think he was at that time the best experimental spectroscopist in the country. He did a lot of classification work; he had done a lot of things on regularities of spectra. There were some other reasons for coming out here. Also, I sort of wanted to get off to a different part of the country for a year.

TERRALL: Were you planning to work with Bowen?

BACHER: No. Bowen was an experimentalist; remember, I'd done my thesis in theoretical work, and I didn't know anything about an ultraviolet spectrograph at that time. So we then started out for the West Coast after the summer session in Ann Arbor. My mother had given us a Ford Model A roadster when we were married.

We started out to drive to the West Coast by way of the Northwest. Actually, we drove up north in Michigan and stopped to see my mother. She was one of the members of the board of the National Music Camp in Interlochen, and knew the director very well. She decided to spend that summer up there, so we drove up to see her before going out. Then we drove out over the Straits of Mackinac and out through Duluth and along the northern part of the country. I don't think we realized quite what we were getting in for, because shortly after we left Duluth, we were on the last bit of paved road that we hit until we got to Spokane, Washington. There was very little pavement at that time; in fact U.S. 2, which was the main road out there, was just being converted from a back-country road into a graveled road. When they made a detour, they just put signs out across the hills with a culvert here and there, and you drove out across the country. Well, we managed to get through this all right, and had a wonderful time, actually.

TERRALL: Were you camping along the way?

BACHER: No, we didn't camp. Partly this was a matter of not having enough equipment to do it, and partly we expected there'd be more places to stay than there turned out to be. I remember going into one place and asking were there any rooms for tourists, and they didn't even know what those were. So we ordinarily tried to find some town with a reasonably good hotel in it. We had a wonderful time. We went to Glacier Park and up into the Canadian Rockies. This was just fine, but the roads were terrible. It was a certain amount of trouble. We took almost a month driving out here.

When we arrived out here, we first, of course, had to find a place to live. And the interesting part of it is that we found a place to live and it still exists. There was an apartment house at 40 South Wilson. We found a little apartment over the garage in back of that main apartment house. It's still there; it's one of the few things that's still around, and I look at it every once in a while. One of the funny parts of it was that when we later came to Caltech to stay, we also lived on Wilson Street, right across from the institute. We lived there for a number of years until we moved where we are now.

Almost immediately after we came out here, there was a meeting of the National Academy of Sciences in Pasadena. At that time, they had a fall meeting, and I guess this may have been the first meeting they'd ever had out here. So they opened up the Mount Wilson Observatory. This, of course, was long before Palomar; Palomar was envisioned by that time but it wasn't very far along. I asked to go to this meeting at the observatory. I was terribly much impressed with visiting the Mount Wilson Observatory. I was interested in astronomy in part because so much of atomic spectra had come through astronomy. In fact, there was as much or more work on atomic spectra at the laboratory of the Mount Wilson Observatory, on Santa Barbara Street, as there was here at the institute at that time. I can still remember that meeting up there. [Walter] Adams was the director, and he was showing us around. It was a very interesting thing for me. It turned out that Ehrenfest, who had been in Ann Arbor during the summer, spent the fall term out here. One of his former students from Holland—a physicist named Dieke—drove him out here, and I think spent a little of the fall here with him. Dieke had been in Ann Arbor that summer, and I'd gotten to know him. I knew Ehrenfest, of course, from the summer in Ann Arbor. Also, Oppenheimer was here some of that year; part of the time he was here when Ehrenfest was here.

It was very much different from Michigan. I worked much more alone than I had there. But I also did some work with hyperfine structures of spectral lines with a student who was just finishing his degree with [William V.] Houston, who was one of the people with whom I worked. The people I knew best on the staff were essentially Houston and Bowen. Then [William F.] Meggers, who was one of the principal spectroscopists at that time and was normally at the Bureau of Standards, came out to work at the Mount Wilson laboratories, up on Santa Barbara Street. I did some work with him. He was working on rhodium, and he showed me some of the pictures he was taking. I said, "Well, it has hyperfine structure in it." And he said, "Yes, but I don't know anything about that." And I said, "Well, this is easy. Here are some patterns and they'll help you very much with the classification of it." So I showed him a bit about what they looked like and how to do it, and I said, "I think we can very quickly find out what the nuclear spin of rhodium is." And in fact, the first afternoon we looked at it, I was able to figure out what the nuclear spin was, and indeed we wrote a little letter to the editor of the *Physical Review* on this.

I need to go back now to explain what I did out here. Toward the end of my year at Michigan, Goudsmit had suggested that the lack of a compendium of energy states of various atoms and ions was a great inhibition to trying to study the regularities of these states and make quantitative calculations of them. Indeed, that was true; you just couldn't find the data. There had been things that had been published fifteen years earlier that had a few of the simpler atoms, but there just wasn't anything of that sort. So during the summer of 1930—this was the last summer I was in Ann Arbor—we took a try at starting such a compendium, and it looked promising. But it looked like an enormous job.

TERRALL: It was a question of searching the literature?

BACHER: Yes, searching the literature and examining the energy states obtained from analyses of atomic spectra carefully. But we did a little bit of it. We concluded that it was possible to do it. So I decided, in consultation with Goudsmit, that when I came out here what I would start doing as a regular job was trying to pull this material together, and this was my main work while I was here. I started to do it over in the Bridge Library. The illumination was so bad in the library—much worse than it is now—that I couldn't work for more than half an hour or an hour without getting a headache there. Also, they allowed the journals to circulate, and it was awfully hard to find anything. Well, I talked to [Earnest] Watson, who ran the building [Norman Bridge Laboratory of Physics], and we got a little better bulbs in there.

But I quickly found out that they had a simply fantastic library up at the Santa Barbara Street laboratory, and with a little bit of asking here and there, I arranged to work up there. That was just fantastic for me, because they had very rigorous rules about the circulation of books. The books were all supposed to be in the library, because everybody was right around there and there wasn't any reason for taking them anyplace. I just worked day after day after day in that library up there, and when I got sick of looking at spectra, I'd go back and look at the early volumes they had from 200 years ago or longer—the *Proceedings of the Royal Society*. You could run into some of the funniest things in there. It was a wonderful opportunity to work up there.

As I started turning out the manuscript of this [compendium] to go to Goudsmit, I did it all by hand; I couldn't really ask the institute [Caltech] to type all of this stuff, but he could get it typed at Michigan and he was looking it over. He was, after all, the expert on all of this stuff. Every one I sent him, I'd put in a postcard. I think I still have some of them. It got to the point where every couple of days I'd have a series of these things, and they were just swamping him in Ann Arbor—which, of course, was my intent. I knew Goudsmit, and I knew if I swamped him with this stuff, he'd really get at it and work at it. I've forgotten just when I got the last of the manuscript done, but it all went off to him while I was out here. And by that time, we had an agreement with McGraw-Hill to publish it, and here it is. It's dated 1932. [R. F. Bacher and S.

Goudsmit, *Atomic Energy States* (New York: McGraw-Hill, 1932)] The manuscript was finished in the summer of 1931, and I spent part of the following year reading galley proofs of this. I was at MIT the second year.

During the spring term, Houston was away, and he asked me to teach his Introduction to Theoretical Physics. I don't think there was a book at that time; he had some notes. I don't remember a whole lot about this, but I think this was the course that was the forerunner of what's now taught for introduction to mathematical physics. I think it's taught for Caltech seniors and graduate students. I don't know how many students take it now, but at that time there weren't very many. And relatively few undergraduates.

TERRALL: Was this your first teaching experience?

BACHER: No, I had had some teaching experience, which I skipped over, while I was a graduate student. I had a teaching fellowship for one year when I was at Michigan, because I felt that I shouldn't be a financial drain on my family at that time. And I then got a fellowship from the General Electric Company, called the Coffin Fellowship. This gave me a small amount of money, which essentially paid my tuition and living expenses and so on. I had that for two years while I was a graduate student. But the only experience I'd had in teaching was essentially that I'd done some teaching when I was a graduate student.

You asked me about making a comparison of the universities I've been at. There were many more graduate students at Caltech at that time than there were at Michigan; it's just the other way around now. There were a lot of seminars [at Caltech], all through the year, including the present [physics] research conference that meets on Thursday afternoon—it ran then and met on Thursday afternoon, too. It used to meet at 4:45, and now it meets at 4:15—people don't stay around as long. And then there was an astrophysics seminar, which was joint with Mount Wilson people; this, too, still continues. And there was another meeting, which I can't remember. I went to a good many of these seminars. They were all held in Bridge lecture hall [201 East Bridge]. I went to them because many of them were on things I thought I ought to learn a little bit about. I gave a talk at a research conference toward the end of the year on some things on spectra that I'd worked on.

As I said before, I knew Houston and Bowen best. Bowen, as I mentioned, was probably the most productive experimental atomic spectroscopist in the United States. I also knew [Linus] Pauling, who was here at that time. He'd written a book with Goudsmit the year before. [L. Pauling and S. Goudsmit, *The Structure of Line Spectra* (New York: McGraw-Hill, 1930)] I had met Pauling before I came out here, because he'd stopped at Ann Arbor. I think Goudsmit was the first one to get Pauling interested in spectra. Pauling was very good and picked up quantum mechanics very quickly. This had a big influence on him. He was more mathematically inclined than Goudsmit was.

I also attended a few of [Richard Chace] Tolman's lectures out here. At that time, he was professor of physical chemistry and also of mathematical physics. His field was relativity, and he gave lectures and also a seminar on that, and I went to hear it. I didn't get to know Tolman at that time. Well, I knew who he was and I knew him to speak to him, but I didn't work with him and I didn't know him then. I got to know him quite well at a later date.

[Paul] Epstein was the senior theorist here. [Robert A.] Millikan [Caltech's head from 1921 to 1946] was not very strong on theoretical physics. He had Epstein, and he had his arrangement with Oppenheimer to come down here [from Berkeley]. And he had Houston, who was part experimentalist and part theorist; he had written papers in both theory and experiment, an unusual man.

ROBERT F. BACHER

SESSION 2

June 18, 1981

Begin Tape 2, Side 1

BACHER: When we last broke off, we were talking about that year I was at Caltech as a National Research Council Fellow in 1930-31. During part of that year, I attended Robert Oppenheimer's lectures; but I must say they were extremely difficult to understand. Robert was not nearly the clear lecturer that he subsequently became. Almost none of the students took these courses, because at that time he was obscure; also, they were in a subject I didn't know very much about and wasn't at that time deeply engaged in. But it was very interesting, and there wasn't any doubt that he was, if not *the* leading young theoretical physicist, certainly one of the leading young theoretical physicists.

TERRALL: Was this a regular graduate course?

BACHER: This was a regular graduate course. At that time, Oppenheimer spent part of his time at the University of California at Berkeley and part of his time here at Caltech; he held a joint appointment for a number of years. This meant that part of the year, usually in the spring term, he came to Caltech, but I think he also sometimes came down in between, because he had a few students down here and he often would come down and spend a week or two in the fall. I knew him in Ann Arbor, but I didn't see a great deal of Oppenheimer here. My contacts on campus, as I think I mentioned, were principally with the people who were working directly in atomic spectra. Then there were people up at the observatories' laboratory who worked at spectra—Arthur S. King, for example. His son Robert was later here as professor of physics, when that work was readjusted and part of it came down here. They dropped the laboratory work at the observatories subsequently, but that was a very lively place then. People would come out to use the equipment in the summertime, and they had very good spectroscopic equipment up there. There were people interested in a wide variety of things associated with spectra. At that time, one of the principal connections of spectra was with the spectral lines found in solar and stellar spectroscopy. Henry Norris Russell, who was perhaps the leading light in unraveling and classifying the spectra of certain complicated atoms, and especially those which showed up in stellar spectra, used to spend part of the year out here. It was a great pleasure to hear him and see him in action. They ran a seminar, which I regularly attended.

TERRALL: I got the impression from what you said last time that you were isolated up there.

BACHER: No, no. There were a number of people in spectra. None of them were really interested in the things I was interested in at that time, except that Russell was working on particular spectra and was much interested in the fact that somebody was taking this stuff and trying to get it put together, and I had a number of interesting discussions with him about it.

TERRALL: What about Millikan? Did you have much contact with him?

BACHER: Well, everybody always went to see Millikan when they came. Millikan came faithfully to the seminars, and he came to the laboratory often. I can remember during the summer of 1931, which was a very hot summer here, Jean and I developed the pattern of taking a day off in the middle of the week, especially in the summertime, because the beaches were so crowded on weekends. So I worked on Saturday and Sunday, and we took a day off in the middle of the week. I can remember being down at the laboratory one Saturday or Sunday in the afternoon. It was a terribly hot day, and there wasn't anybody there. I had my door open, and all of a sudden Millikan appeared in the door. Apparently he'd come down on this very hot day to check what the temperature and humidity were around the laboratory. Well, the temperature wasn't bad, but the humidity was nearly 100 percent, because the system by which the Bridge Laboratory was then cooled was simply a water-cooling system. There wasn't a refrigerating system of any sort.

Millikan was around the laboratory a great deal. Two or three years later, when I ran into him at an American Physical Society meeting, he called me by name. He really knew who was here and what was going on. Of course, the group wasn't so large, but the group of graduate students here was probably one of the larger groups in the country at that time. And there were quite a few postdoctoral fellows, though not anything like as many postdoctoral fellows as there are now.

TERRALL: But Millikan wasn't really in touch with what you were working on?

BACHER: No, the things I was working at were not things he was particularly following. Millikan had started, about 1925 or thereabouts, to get interested in cosmic rays. This was a typical move on the part of Millikan: He looked at something and thought there was something there that ought to be looked into. He first became interested in the distribution of cosmic rays on the earth, and he had several people who worked at this. There was a great debate during that year, when Arthur Compton came out here. They had a joint seminar, and they had quite opposing views about the origin of cosmic rays. The fact is that Compton was more right than Millikan was, but the experimental evidence was pretty fragmentary. Millikan contributed greatly to that. He was one of the ones who got cosmic ray work started in the United States, and he got a number of his students into it. For example, he got Carl Anderson into this field. I think Millikan originated the idea of looking into how these very energetic cosmic rays would interact with matter, and that's how he got Carl Anderson started in using a cloud chamber in a magnetic field which was exposed to cosmic rays. Out of that came the discovery by Anderson of the positron in 1932.

Millikan had a very important influence on what things were started at Caltech. The experimental work, in particular, was very good. Millikan was never a great supporter of theoretical physics. He knew that he had to have theoreticians around, and he depended on Epstein as his principal advisor. In addition to Epstein, he had Tolman, of course, in that other special field relating to astrophysics. He also had Houston, who was a knowledgeable theorist who worked both in theory and experiment. And Oppenheimer came down here part of the time. But they did not have as big and strong a group as many other universities did have in theoretical work. This was mainly by design.

TERRALL: This was Millikan's preference, in a way?

BACHER: It was Millikan's preference. Millikan had limited views on theoretical physics. He was always interested in what the theorists thought about, but he had some misgivings about it. This view of his was formed at a time before quantum mechanics made such an enormous impact on the field—before the late twenties and thirties. In the thirties, Caltech had an enormous amount of trouble financially and they couldn't really go off into a whole lot of new things at that time. And I don't know whether Millikan wanted to or not, anyway.

TERRALL: So would you relate his feelings about theoretical physics to the times in which he was educated?

BACHER: Well, that's a very complicated thing; I don't know that there's a simple answer to that question. But it certainly is true that Millikan did not go out and try to get the best theoretical physicists in the country. He did try to get Oppenheimer here, and it ended up that Oppenheimer finally spent less and less of his time here, until subsequently—some time in the early thirties, I think—he detached himself from Caltech and was all the time at Berkeley.

TERRALL: So Millikan was not up on quantum mechanics particularly?

BACHER: Well, I think Millikan naturally had a strong urge to stick close to things that were observed. This was his approach to physics. He had demonstrated all through his career a talent for looking at subjects that were of fundamental importance. He was a great physicist; his greatness as a physicist was that he had some inner guidance or thoughtfulness about the subject, which very often got him working very hard on subjects that either were off in a corner somewhere and people hadn't looked at or else hadn't looked hard enough at them to figure out how to do first-rate experimental work. Like his work on the electron. This was a fantastic job for that time. His idea, for example, of using the cosmic rays as a source of high-energy particles for experiments—which, as far as I know, was something that traces essentially back to Millikan—was a brilliant idea. He first got interested in this business of the distribution of cosmic rays, and he had a lot of people who worked on that. Victor Neher, who studied with him, worked in that field essentially all of his life until he retired about ten years ago. Bill [William H.] Pickering worked in that area, and a number of other people worked in that area. Bud [Eugene W.] Cowan, to this day, succeeded into that area and has worked in it for a long time, though he's changed the nature of his work somewhat. [Robert B.] Leighton also worked in cosmic rays, along the lines that Anderson started way back when he was graduate student.

Leighton took his degree with Carl Anderson just about the time I came back out here, in 1949. His interests were principally in cosmic rays as a source of high-energy particles. He changed his field after a while and now works in astrophysics, and has done brilliant work in it.

TERRALL: Could Millikan's ideas about the origin of cosmic rays be seen as more theoretical then, because the data were so hard to interpret? Maybe that's why he wasn't so successful.

BACHER: Well, I don't know the answer to that question. He had done some work on the intensity of cosmic rays at various places over the earth, and this essentially was a way of getting at the question of the effect of the earth's magnetic field on the cosmic rays that came in. This was a subject he and Neher worked at, and Pickering worked in this area, too. But that was at a time when I was not here, and I don't know very much about it. They did very good work; there wasn't any doubt about that. There were some peculiar dips in the distribution of cosmic rays, and I think part of the difference between Millikan and Compton came over just what the nature of this was and what it was due to, and so on. But really the greatest work in this, outside of getting really good measurements made in this field—he always was very good at suggesting methods of getting measurements—was his idea to use the cosmic rays essentially as an experimental laboratory. That's really what Anderson got started. That was what the cosmic-ray group did here after the war, when Millikan retired, and they were very, very good at it.

When I came to Caltech in 1949 I went up to see Anderson, who then had an office on the top floor of Bridge. Up in the corner of the blackboard was a sign that said, "What have you done about hooks and forks today?" Hooks and forks were tracks found in cloud chambers that essentially were unknown and just appeared. It turned out that this was deeply connected with the production of unstable particles in the cloud chamber. It was used as a method of discovering some of the unusual unstable particles in cosmic rays before nuclear machines existed to do that. Anderson was probably as good as anyone working in this field and contributed enormously to it.

Well, to get back to that time, this was also a period in which—and I didn't know much about this and wasn't really very much connected with it at the time—[Charles C.] Lauritsen was starting to work using the big transformers in the so-called High Voltage Research Laboratory. That used to be just a great open laboratory and had been built and used by the Southern

California Edison Company for testing the transformers that were used on the power line from Hoover Dam over to Southern California. Lauritsen got into nuclear physics by working on those, and that is something that happened after I was here [in 1930], and I don't know really very much about it. But all of the work in the Kellogg Laboratory blossomed very greatly during the thirties. The Kellogg Laboratory was a very strong nuclear laboratory by the beginning of the war, and of course it was out of the Kellogg Laboratory that came the leadership for the rocket program here.

TERRALL: You were going to tell me about the dedication of the Athenaeum [Caltech's faculty club].

BACHER: It must have been in the winter of 1930-31. The Athenaeum was opened in the fall, but they reserved having a dedication dinner for it. Jean and I were present at this dedication dinner. We went with our old friend Phil Fogg, who was here as assistant professor of economics at that time. When I came to Caltech, Phil Fogg was the first person I saw. I saw him quite often—in fact, I used to play tennis with him once or twice a week, regularly through the year. He and I had been in the same Boy Scout troop in Ann Arbor when we were in high school or grade school or whatnot. He moved away from Ann Arbor and went to Stanford. His mother was ill—she lived in Santa Monica—and he turned up out here and became assistant professor of economics and later became the first registrar of the institute. Well, anyway, I've known him all these years; he still lives here in Pasadena, and I've seen a great deal of him since we've been out here.

But I can remember that he took us to that dinner, which was very nice of him, because he knew a lot more people around the institute than we did. There was a reception in the main lounge of the Athenaeum, which was all brand new at that time. And then a large dinner, and I've forgotten what the speeches were. But the important thing about it was that Millikan waited to have this dedication until Einstein was out here, so Einstein could participate in the dedication of the Athenaeum—and indeed he did. He was present in the receiving line at the dedication of the Athenaeum.

TERRALL: So you regarded your year here as a productive one.

BACHER: Yes. It was a very productive year for me. I was looking through some papers: This is all correspondence to and from Goudsmit, who was back at Michigan at that time. Since Goudsmit was inclined to be not so good at keeping track of things, each set of tables I sent him had a postcard in it addressed to me with a list of the things I was sending him. These are all the tables of energy states that I'd sent him.

TERRALL: These were mailed back to you.

BACHER: Oh, yes. This card went with the material I had, which I don't think was typed. But I must have kept some sort of a carbon or something, because I wouldn't have put all that stuff in the mail without it. This was mostly typed in Ann Arbor, because it was an enormous job.

When I was out here, the work that was going on here was interesting to me but wasn't terribly closely associated with my work. I missed both Goudsmit and Uhlenbeck very much the year I was out here. The Michigan group in physics was a much smaller group than the group out here, as a whole. You asked me to make some comments about Caltech as compared to some other places. In the first place, there was no place else that had anyone as world famous as Millikan, and Caltech had a lot of very good people on the faculty. It was a very strong group. But it wasn't as coherent as the group at Michigan. Certainly there were several times as many graduate students at Caltech as there were at Michigan. There wasn't any doubt that overall, at that time, Caltech was much better known, had a much higher reputation, and was overall a stronger laboratory in physics, even though the work in physics had only been going for ten years.

TERRALL: So those are the reasons why you chose to come here?

BACHER: Well, yes. And also one of the reasons I came here was that experimentally, as a source of data, Bowen was probably the outstanding person. I knew ahead of time that there was work, and very good work, on the experimental side at the observatories. So that between here and the observatories, this place was quite a source of experimental information in atomic spectra—there wasn't any other place like it. Also, the work of Houston was quite different from anything that was going on anywhere else. So there was a lot of work going on in this area, especially when one considered the work going on at the laboratories on Santa Barbara Street. That was quite outstanding and was regarded as one of *the* very best places in the country.

In addition—and I shouldn't neglect this at all—Pauling was here. Pauling was then interested in atomic spectra and was instrumental in moving over into this field and getting chemists interested in this kind of approach. A lot of the work subsequently went over into chemistry. Pauling became interested in and subsequently wrote a book on introduction to quantum mechanics, especially as applied to chemistry. And this was one of the things that made the work in chemistry at Caltech really take on an extra special tone, because this was far advanced beyond the work going on in chemistry in many places. So it was an unusual place to be. I saw something of Pauling while I was out here. He knew a lot about spectra, especially from the theoretical end. There were a lot of people interested in this. There wasn't anybody doing just exactly what I was doing, but it was a very good place to be—one of the outstanding places in physics at that time in the country, though places were changing very rapidly.

TERRALL: Well, physics was changing very rapidly.

BACHER: Physics was changing very rapidly, and places were changing very rapidly. The second year of my fellowship, I went to MIT. MIT had not been very strong in the twenties in physics but was changing due to the action of a number of people and especially the fact that Karl Compton went there as president [in 1930]. He took steps immediately to get a number of people there and strengthen the work in physics, and ever since that time it's been a very strong place in physics and they've always had a very large department there.

TERRALL: Was there someone in particular at MIT you wanted to work with?

BACHER: Yes. I wanted to work with [John C.] Slater. I had taken a research course with Slater when he was a young assistant professor at Harvard; that was where I started to learn a little bit about quantum mechanics. At that time, Slater was picked with George Harrison to take the leadership in physics at MIT. He was looking after the theoretical work and George Harrison was looking after the experimental work. Slater was certainly at that time one of the very bright young people in theoretical physics in the United States. It happened that what he was working on at that time were things related to the theory of atomic spectra, and I was very much interested in this. His approach to the problem was a very difficult theoretical one. I spent most of that year, except for reading proofs on that book, working with Slater. I learned a lot from him. He was an extremely able person, and it was the work I first did there, and things that grew out of it, that a couple of years later led me in a direction of making what probably was one of the better contributions I've made to physics. But I'll come to that a little later.

Slater was a very able physicist. It was very good for me, because they had a number of young people on their faculty there—Jay Stratton, who was later the president of MIT, was assistant or associate professor of theoretical physics; Will [William P.] Allis, who must be retired as professor was there; Ned Frank was there in theoretical physics; Phil Morse had come there as a young man in theoretical physics, and I had known him briefly before. John Slater was the head of it. So you see, they had five quite good people in theoretical physics.

So this was a very good year for me. We saw a great deal especially of the Morses, and Jay Stratton, and the lady who is now his wife, because this was before they were married. That was a very good year and we enjoyed it very much. It was quite different from being at Caltech. It was much more expensive in Cambridge than it was here, and we couldn't do nearly as many things as we could here. But it was also very good. They had a brand new laboratory there. It was interesting to see what was happening. I found it a good place to work and had very good contacts. Occasionally, they had meetings jointly with the people at Harvard. But mostly I was with the group down at MIT, though we lived closer to Harvard than to MIT.

TERRALL: Did you have children by this time?

BACHER: No, we had no children yet. On our trip east, we drove, and we stopped to spend a week in Princeton to see [E. U.] Condon particularly, whom I had met. I wanted to talk to him about various things in spectra. I found that very interesting, and I got to know him a little bit better. Then I stopped also for several days at Columbia to talk to [I. I.] Rabi, whom I did not know very well. He'd been in Germany on a postdoctoral fellowship and had come back to Columbia. He worked at nuclear moments by an atomic beam method. So, in other words, we worked in quite different subjects, but the end result came out to be the same thing. He was interested in some things we were doing, and I was very much interested in things he was doing. It seemed to me that with the enormous steps they had made with atomic beams, it was pretty clear that a lot of the really accurate work on nuclear magnetic moments, for example, was going to come out of that field.

Begin Tape 2, Side 2

BACHER: It was clear that the steps Rabi was just starting to make then were very large steps at getting at a new method of measuring nuclear magnetic moments. This was a very interesting subject. As you perhaps know, later a very large fraction of the work that was done in this field was done at Columbia, and some of the best physicists in the United States went through that laboratory.

Well, anyway, we spent the year in Cambridge and enjoyed it very much. It was completely different from being here. It would be extremely hard to compare the two places, because Caltech was then extremely small. I don't know how many graduate students there were, but there was a lot of difference between the two places at that time. A much larger fraction of Caltech was physics, whereas at MIT it had been going for seventy-five years or something like that and was a well-established place. But MIT had not had for a long time, prior to Karl Compton's going there, the distinguished work in physics it subsequently did have.

TERRALL: So Compton got Slater to come from Harvard.

BACHER: I think so. I was not present when this change took place. I saw Compton in Princeton when I stopped there in the spring of 1929-30; I remember talking to him about the possibilities of getting together a collection of energy states. And the next time I saw him, he was at MIT. Some of these people I listed before had been at MIT for some time, and they were good people. But MIT was then much more heavily concentrated in engineering than it subsequently was. All of the branches of engineering were quite good and very strong, and many, many engineers came out of there, but not so many scientists.

As I said, Slater was the real reason I went to MIT, though I was certainly interested in some of these other things. He had written a very good paper on the theory of atomic spectra, which greatly influenced my thinking about the whole subject and got me thinking in an entirely new direction. I also did a little bit of hyperfine structure work. I told you that when I was out here, I worked with a couple of experimentalists and suggested that they look at some hyperfine structure patterns, which they weren't working at at all. Well, I did the same thing with John Wulff. Wulff had worked at hyperfine structure before and had done some of the experimental work I had done theoretical work on for my doctor's thesis. But Wulff was then working in spectra at MIT, and he had worked with Ernst Back, whom I think I mentioned before.

During the summer of 1932, we had a journal club. This was very interesting, because Condon came up for the summer. While this wasn't based around having a lot of lectures and a lot of people coming in for a special session, they had a journal club, which I think Slater and Harrison ran. It was mainly on theoretical things. When I say it was a journal club, this meant that you didn't go in and talk about something you worked at; you were assigned some papers. Slater asked me to report on [James] Chadwick's papers on the discovery of the neutron, which had been published that spring, in the spring of '32, in the *Proceedings of the Royal Society*. [J. Chadwick, "The Existence of a Neutron," *Proc. Roy. Soc.*, A, 136, p. 692-708.] Slater said, "This is probably all nonsense, but I suppose we ought to look through them and have them reported there."

Well, after I had studied these papers for a little bit, I went around to Slater and I said, "You know, this is correct. It's one of the most revolutionary things that's come in physics for a long time. It really is right. I don't know very much about nuclear physics"—this was essentially nuclear physics—"but," I said, "these are simple, straightforward experiments. There doesn't seem to be any other explanation for what he's done." He said, "Well, I'll wait and hear about it when we have the seminar." Essentially, I went in to face a seminar with all these well-known people in physics, and while they had heard about this paper, I don't think any of them had read it or really looked at it carefully. And here I was, just a young postdoctoral fellow trying to espouse this work about neutrons.

TERRALL: So there was a general feeling that it might very well just not be true at all?

BACHER: Oh, yes. I think almost the entire audience was skeptical. Slater was very open-minded about things, but I think even he was skeptical about it. And there were people there who just said, "Well, this is just obvious nonsense."

TERRALL: Did you convince them?

BACHER: Well, that was very interesting. At the seminar, I succeeded in persuading somewhat more than half of the group, including Condon, that the neutron existed. One of the reasons I was interested in it was that this point of view greatly helped two very serious anomalies in nuclear moments—both in spins and statistics of the nucleus, and in magnetic moments, because the magnetic moments of nuclei were about 1,000 times smaller than the magnetic moment of the electron. And there were serious problems about trying to think of nuclei as containing electrons. Something that had the properties of an electron didn't fit very well in being in the nucleus. But one of the things that struck me as the hardest to believe was that even if you could somehow get around that difficulty, you had to say that somehow or another the electron magnetic moment changed its magnitude by a factor of 1,000 in the nucleus. This was a serious problem. Also, there were problems with the statistics. For example, the statistics of the nitrogen-14 nucleus—if it's made up of fourteen protons and seven electrons, that means it has an odd number of particles in it. If it's made up of seven protons and seven neutrons, that means it has an even number of particles in it. And it was very hard to figure out how you got a spin of 1 from an odd number of particles. It didn't obey the proper statistics. So there were troubles. This was very interesting.

TERRALL: When you read this paper of Chadwick's, did you see the implications for what you were doing?

BACHER: Yes, I saw the implications for what I was working at. It really made sense. In fact, I saw that you could get out of the spin and moment problems that way; and I guess the best evidence for that is that the week after this talk, Condon and I wrote a letter to the *Physical Review* about the spin of the neutron. [E. U. Condon and R. F. Bacher, "The Spin of the Neutron," *Phys. Rev.* 41: 683, 1932] We said that the spin of the neutron obviously had to be one-half, as a nuclear constituent. We deduced this from the known spins of hydrogen, which is a proton; deuterium, which would, according to this, be a proton and a neutron—deuterium has a spin of 1; and nitrogen, which also has a spin of 1.

Well, this really stirred me up a great deal, and one of the things I felt good about was that at the end of a two-hour vigorous talk in the seminar with all of these people, I convinced somewhat more than half of them that this was right. They had come in there thinking it was absolute nonsense. And I think everybody went to read these papers after that. Of course, it turned out that these were the papers that were the discovery of the neutron. I didn't go and say I've found these papers or anything. The reason I reported on them in the seminar was I was assigned the job of doing it. It was only after I started to study them that I concluded that this was a real major finding. It's funny; here was a group of people who had looked at these papers and thought, "Oh, this is a lot of nonsense."

TERRALL: I didn't realize that Chadwick's work had been received that way.

BACHER: Well, look, it takes time. You come up with something as radical as that and you're going to find a lot of people who don't believe it. I've had that experience myself. In looking back, it's hard to understand that it happened that way, but I swear it did. I think this wouldn't happen that way today, because people all over the United States would be talking to each other on the telephone about it—somebody somewhere else had doubtless found this out. Today, the way people communicate with each other, it would just take one person really digging through this and finding it to say, "Well, you should go and read that carefully, and when you read it carefully you'll find thus and so."

TERRALL: So it seems that would have had to happen at every different laboratory, almost.

BACHER: Well, it didn't have to happen at every different laboratory, because sooner or later there'd be a meeting and people would discuss the things they'd read and say, "Have you seen this?" But this is the way it happened at MIT, with this group of people. It's quite different from the way things would happen now, and I think very largely because people get around more. At that time, the only person who thought of going to a Physical Society meeting on the other side of the country was Millikan or one or two other people; almost nobody went from here to meetings in the East. The only way you could go was by train, and it was a long and very expensive trip, so people just didn't do it.

Well, by the end of that summer, the Depression had really set in. The National Research Council fellowships had been cut back. All sorts of fellowships had been cut back, and jobs were about zero. The interesting part of it was that those who hadn't received fellowships in those earlier years [1930 or 1931] found jobs, because there were lots of jobs available then. So they had assistant professorships, some of them in quite good universities. But in 1932 there were practically no jobs in the country. I got a postdoctoral fellowship back at Michigan for the next year and went back there. That year was essentially the bottom of the Depression. Many of the universities were going broke, reducing the salaries of professors. That happened here. In other places, there were no increases, and they didn't have money for assistants, and they cut back on fellowships. It was a very difficult time. Universities felt it very, very much. The depth of the 1932 Depression is something I think most people today can't realize, because the fact is that there was no precedent for the government stepping in. It was only when Roosevelt was elected in 1932 and took over as president in 1933 that things began to happen. But it was still very tough; in fact many physicists had great difficulty, up until the war.

TERRALL: Were you thinking of riding it out or did you have to consider alternatives? Did it look like there weren't going to be any jobs in the near future?

BACHER: Well, you just couldn't tell—it was a year-to-year affair. I went back to Michigan then, and I had a good fellowship for the year 1932-33. It paid as much or more than the National Research Council fellowship I had had. But for the next year, there just wasn't anything, and I didn't have any sort of financial support, except from my family, for that next year. We'd been married in 1930, just after I got my doctor's degree. But those two years in Ann Arbor were very good for me; they were two of the best years I ever had, and during one of them I had no job at all. This was partly because I didn't have any responsibilities during those two years and I could spend all of my time working on things. I got some ideas, which grew out of my study of Slater's papers, on developing some work in atomic energy relations—that is, relating the energy states of, for example, oxygen to the states of a higher degree of ionization. In other words, the states of oxygen with one electron removed and two electrons removed. You could show how these were built on the states of the more highly ionized atom. Also, I became interested in establishing it theoretically, and I was able to do this and show how accurate it is. It turned out that it worked to a high degree of accuracy. This I did mostly the second year I was there. I did some of it the first year I was there, and I've forgotten when I got the idea for it, but I wrote the paper the second year and essentially spent all of my time on it. I also did some experimental work with Ralph Sawyer, who was probably the best known experimental atomic physicist at Michigan at that time and invited me to share his office. I had shared his office the last year I was there as a graduate student, and when I came back after two years, he invited me in again. This was very good for me, because although I didn't work with him, we had a lot to talk about and it certainly influenced my work. I tried to get him interested in doing some high-resolution atomic spectroscopy, because I discovered he had some equipment that was capable of doing it and he'd never used it. I tried to stir him up. He said, "Well, why don't you do it?" I said, "Gee, I don't know how to do this kind of thing very well." He said, "Well, there's a way to find out, and that's to do it." With some help from him, I started out and learned how to do experimental work in atomic spectroscopy.

I had a very amusing experience when I was there. We had practically zero money to work with, but here were these wonderful interferometer plates that were really good. I decided we could not do the kind of mounting of these plates that was ordinarily done with the kinds of equipment you bought from the Adam Hilger company or someplace like that. It had been done in the old days by simply setting the interferometer up outside the spectrograph, passing the light through the interferometer, and then focusing these fringes on the slit and letting the spectral lines be split up. Then, for each of the finer wavelengths, it turned out you'd get not a spectral line through the prism spectrograph but a series of interferometer rings, if you used a fairly wide slit. Well, that worked all right as long as it wasn't too complicated a spectrum, which was indeed the situation I wanted. But I had a funny experience. I talked to Sawyer and said, "You know, Ralph, I think that ordinarily one would want to do this in the basement, but I don't know enough about this to say that I can really do anything at it. Let's try it up here first. I have an idea that if we take one of these big laboratories and we put the thing in the middle of the floor, it ought to be pretty good vibrationally if we put it on a big table and bolt everything down. If it bounces around, it will all bounce together." And he said, "Well, it sounds a little wild, but let's try it." It turned out that it worked.

I must say, this was as haywire-looking a piece of equipment as you could imagine, because most of it I'd built myself. The source I used was a mercury arc, because I wanted to test it with a source that had been worked on before so I'd know what I had. So I set up this source with mercury. This was done by running a mercury arc in a coffee can full of water—coffee label on the outside and everything. To my great surprise, since this was a heavy atom and didn't move around too much anyway, it turned out that the fine structure showed up pretty well. I was able to get a resolving power of something well over a million, which is pretty hard in a thing of this sort. Sawyer was surprised. He didn't think it was going to work anything like as well as that. Well, I came in one day and Sawyer had visiting him the man from Hilger, the company that made all the fancy spectroscopic equipment. Ralph said—I knew perfectly well that he was putting this fellow on a little bit—"Why don't you show him the high-resolution stuff you're doing in the next room?" Well, I took him in there, and this fellow looked at this, drew himself up, and said, "Young man, you can't do high-resolution work with anything that looks like that. You've got to have much better equipment than that. You just can't possibly do it." He gave me quite a lecture about this. So I went and got a photographic plate and put it under a microscope and showed him the fringes, and I said, "That's the mercury green line and it shows a resolution of about a million three hundred thousand." He looked at it and he looked at me, and he said, "Well, I'll be damned!" [Laughter]

Well, that was my introduction to experimental physics in that field. I did a few things then in experimental physics in hyperfine structure. Later, when I went to Cornell, I set up a laboratory for doing high-resolution work and got one of the other members of the department to join me in this. But those two years in Michigan were very, very good for me. I had a good rapport with Sawyer as an experimentalist. He was a very good experimentalist. I knew him very well.

I also worked constantly with Goudsmit on things. Goudsmit never paid any attention to the experimental work I was doing; he just wasn't interested—well, he was interested, but not really seriously. He didn't have any urge to do experimental things. I had worked with Goudsmit on the theory of energy states, but the bulk of it was work I'd gotten started and had done. We sent the paper in to the *Physical Review*. It's sort of amusing, because the *Physical Review* turned down the paper. Their statement about it was that it just didn't make any sense. I think whoever read it just didn't understand it. They looked at it superficially, because it was explained in there in great detail how it was tied in, and there was an appendix that essentially proved the thing in terms of the quantum mechanics. I don't know for sure who looked at it.

In any event, the paper was turned down by the *Physical Review*. Well, I was the senior author, and I said, "Sam, you've got to let me handle this with the *Physical Review*. I'll show you what I write, but I've got to say some things about this as the senior author. I just don't think you ought to get tarred with them." I really ripped into them and told them that their reviewer didn't know what he was talking about. I simply told them that you can find this and that and the other thing in there. I got a letter back almost by return mail accepting the paper, and they published it. It was a long paper, twenty-five pages in the *Physical Review*, or something like that. [R. F. Bacher and S. Goudsmit, "Atomic Energy Relations. I," *Phys. Rev.* 46, 948-969 (1934)] This was really pretty funny. But it was good work; as a matter of fact, it has sort of stuck to this day. I found, when I came out here many, many years later, that some of the chemists here were using it in some of their work, and it still gets referred to from time to time. It had to do with essentially for the first time determining the energy states of one atom in terms of other associated ions. And the same sort of relations could be worked up for what were then called isoelectronic sequences—that is, comparing nitrogen with the once-ionized oxygen; it's one after it in the periodic table and has the same number of electrons and so on and ought to be similar. This was an interesting time. I want very much to find this letter.

TERRALL: The letter you wrote to the *Physical Review*?

BACHER: Well, I don't know that I'll find that. What happened was that we didn't get the proof back from the *Physical Review* until I had gone the next year to Columbia as an instructor. I got the proof there, and I sent it out to Sam to look at, too. I have somewhere a letter he wrote me in which he said, "You know, I spent an hour and a half reading this, and I still don't understand as much about it as I did last year when you were out here." [Laughter] It's really a very funny letter. Only Sam Goudsmit would write a letter like this. I hope I can find it somewhere, because it's really so typical of Sam.

He was a wonderful man. I worked with him and saw a great deal of him in the period from the time he came to Ann Arbor in 1927 until 1934. I didn't see so much of him after I left Michigan in '34. Within a couple of years, my interests turned to nuclear physics and I just didn't see him so much, and there were long periods when we didn't see each other at all. But he was a wonderful man. He had more influence on my education than anybody else. There was absolutely no one like him in physics. He was a theoretical physicist, but most theorists, very mathematically inclined and full of quantum mechanics and so on, would turn up their noses at Sam, because if something had to be complicated, that wasn't his approach. This was best shown in this work that we did together; this wasn't his sort of a dish, either, and he just said so.

But he had lots of really brilliant ideas. He and Uhlenbeck were the people who invented—or discovered, or whatever you want to call it—the electron spin. They saw how this had to be brought in as a concept in terms of the doubling of atomic states. This idea of spin, or the double nature of particle states arising out of it in one form or another, is something that persists all the way through atomic, nuclear, and particle physics. It was a major thing. A great deal of this was the enormous intuition that Goudsmit had. Goudsmit and Uhlenbeck were just completely different [from each other]. Uhlenbeck was primarily interested in theoretical physics associated with quantum statistics, and he was extremely good at it; in fact, he worked at it essentially all his life and ended up as professor at Rockefeller University. Sam Goudsmit was at the Radiation Laboratory at MIT during the war, and at the end of the war he did not go back to Michigan. He went to Northwestern and then subsequently to Brookhaven National Laboratory, after that was founded, and he held a major position there for all the rest of the time. He was, for longer than anyone in recent years, the editor of all the Physical Society journals.

ROBERT F. BACHER

SESSION 3

June 23, 1981

Begin Tape 3, Side 1

BACHER: The fall of 1933 started a year in which I didn't have any formal appointment at Michigan, because of the Depression. I got an appointment early in the fall for the following year as an instructor at Columbia University. I went there in the fall of 1934, having spent the summer around Ann Arbor, as I had done the previous summer. The job [at Columbia] was essentially a job as instructor in physics. I helped in the introductory physics, both class and laboratory, and I developed very many associations with Professor [I. I.] Rabi, who was just then getting the very fine atomic and molecular beam laboratory that he had set up there into operation. He had working with him several people who are well-known physicists and whom I got to know quite well—especially Jerome Kellogg, who was later the head of the physics division at Los Alamos, and Jerrold Zacharias, who went to MIT after the war. He was also at the Radiation Laboratory, and I saw a great deal of him at that time. There was also a man by the name of [Sidney] Millman, who subsequently went to the Bell Telephone Laboratories and has been active in a number of things. All of these were very highly qualified physicists. If I remember correctly, Zacharias and Millman taught at City College in New York and did their research with Rabi.

TERRALL: They were all experimentalists?

BACHER: They were all experimentalists and were involved in particular experiments with Rabi. In the second term, in addition to the introductory physics work, I gave an advanced lecture course on the theory of atomic spectra. This was essentially an introduction to that subject, especially as it applied to hyperfine structure problems, which were closely associated with the work of the atomic beam laboratory, and in addition, the recent work I had been doing on the theory of energy states. I had a very interesting group; practically all of these people I've named came to the course, and it was more a seminar than it was a course. I enjoyed it very greatly, and it was something I had a very good time at. We liked the company at Columbia very much. I was very fond of Rabi, and he has ever since been a close friend. But we found New York difficult to live in, and especially at the low salaries that existed then. I've forgotten what my salary was, but if I remember correctly, it was $2,400 a year. Of course, that was a lot more money now than it seems. Heads of departments in some pretty good universities were getting $6,000 or $8,000 a year—I think $6,000 a year was considered a very good salary for a professor.

TERRALL: Was there any question of your staying there?

BACHER: Well, I was welcome to stay on in the appointment I had. What my prospects would have been could hardly have been determined at the end of one year, but certainly they would have liked me to stay on. I had an appointment for the following year. But along in the middle of the year, we were visited by Professor Hans Bethe, who had come to this country and accepted a position as assistant professor at Cornell University. I had never met Bethe before. Of course, I knew his work somewhat—not all of it, because it was in a field I didn't know anything about and was really quite erudite. But he visited Columbia in the spring, and I found that there were lots of things he was interested in that were close to things I was interested in. Especially, I was interested in some of the things they were starting at Cornell University, which sounded as if I might have an opportunity to move over into nuclear physics, which was one of the things I even then had thought about.

In the spring, I was approached about going to Cornell. I, of course, talked to the people at Columbia about it immediately. They were very nice about it. I think they thought I was a little wild to go up to Cornell, which was at that time not nearly as well known and established a laboratory as Columbia was. Nevertheless, I was interested, because of the possibilities. Cornell had always done experimental work in atomic spectra and especially some high-resolution work, which tied into things I had worked at in hyperfine structure. They didn't have anybody who was very active in atomic spectra at that particular time. But they had Bethe, which was a very great attraction to me, and they had work starting in nuclear physics under the general supervision of Stanley Livingston, who was the original coworker with Ernest Lawrence in the development of the cyclotron. I thought it was very interesting and far-sighted of him to move immediately into this field.

Well, finally it was settled that we go to Cornell the following year, and we went to Ann Arbor for the summer and stayed at our place out at Cavanaugh Lake, which I've mentioned. We had a very good time there. We went for a summer meeting to Cornell—there was some sort of a meeting there, primarily on nuclear physics, but [R. Clifton] Gibbs, who was chairman of the department at Cornell, wanted me to come. I drove over from Ann Arbor with Willy [William A.] Fowler and [H. Richard] Crane. I think that was the first time I met Fowler, because I think he was just coming to Caltech; he must have been a beginning graduate student about that time.

Well, Cornell was completely different from Columbia. In the first place, there were many fewer research physicists. There were quite a lot of people who'd drifted out of doing any research and concentrated on teaching work. This was one of their difficulties. Cornell had been, in the late years of the nineteenth century and in the early years of the twentieth, an outstanding place in physics. Then, for one reason or another, it had deteriorated. It had been hit very hard by the Depression. But it was just at this time that Gibbs, who was then chairman of the department, started working very hard to build it up. He was successful at this and very persuasive with the university in backing it. They did an enormous amount, considering the small amount of money they had to work with. For me, the greatest attraction of Cornell was Bethe, right from the beginning. After I'd spent a few afternoons talking with him, I thought that the possibility of being where he was would be wonderful, because he knew so many things. He has this enormous encyclopedic mind—he just knows everything and has worked at everything and has worked very intelligently at it. There were a good many things I worked at that weren't in the area he worked at, but there were quite a number where our interests overlapped. Especially, his thoughts about nuclear physics were very, very attractive to me, and he knew an awful lot about it.

TERRALL: So you were thinking at this time of moving in that direction?

BACHER: Yes, that's one of the reasons I went up there. He also was very good at knowing things about atomic structure and so on. He had been very good at that field, and he was somebody you could go and talk to about subjects of that sort, and I could understand what he said, and I could talk with him. We also liked the town very much. Cornell is in Ithaca, New

York, in the Finger Lakes region; it's very beautiful country around there. The university wasn't so big at that time, much smaller than it is now. We liked the feel of it. It was the sort of a town both of us had grown up in, and we liked the prospect of going there very much.

I mentioned Professor Gibbs, who was head of the department; he was a former atomic spectroscopist. I had known him a little in previous years and had had some correspondence with him when I'd been writing the book on energy states. Gibbs was one of the better known people they had there from the old faculty. And [Floyd Karker] Richtmyer, who had done really quite fine work on X rays, had almost dropped out of that field and gone into administrative work. If I remember correctly, he was dean of the faculty. He was certainly a fine physicist to have around. For reasons I never understood very well, he and Gibbs didn't seem to hit it off, and this was one of the problems in being around there. Then there was Livingston, whom I mentioned, who had worked with Ernest Lawrence in the construction of the first cyclotron and who was a very good experimentalist. He was in the process of building a very small cyclotron, which would accelerate deuterons to about 1.5 million volts. Well, that sounds infinitesimally small now, but it was one of the earliest cyclotrons built in the country. And to have this and to be able to do some nuclear physics with it was a wonderful prospect, and this looked very good to me. It wasn't something that somebody was talking about way down the road; it was something that was actually being built.

Well, this was also important to us for another reason, and that is our daughter, Martha, was born on December 17, 1935, just a few months after we went to Ithaca. The prospect of having a small child in a place like that seemed a lot better to us than it had in New York. Our son, Andrew, was born in 1938 in Ithaca. It turned out that though we didn't know many people very well when we went there—we did know a few—we immediately made a number of friends whom we found congenial, and we found it a very pleasant place to live.

A considerable attraction to me at Cornell was the fact that they had very good spectroscopic equipment for high resolution. I discovered, when I started digging into it, that they had some first-rate equipment that had never been used. As sometimes did happen in those days, somebody had ordered these things. They were expensive by standards of those days, but things changed rapidly, and to use these interferometer plates required quite a lot of work. Well, I discovered they had a wonderful place to set up a spectrometer for high resolution, and I immediately set up some work for studying hyperfine structures.

So I went immediately into doing experimental things. A young man who was then also an instructor in physics, named [D. H.] Tomboulian, who had taken his degree at Cornell and had worked in a totally different field, wanted to learn to work in this field. This was just fine for me, because I thought, "Well, I can start this and he can learn about it." He was a good experimentalist. He didn't know anything about the subject or what was important about the theoretical aspects of it. We had a very good time together. In about a year or a year and a half, we had a very good laboratory set up and were doing hyperfine structure work.

My objective and agreement with Gibbs was that I would get work in the hyperfine structure field set up and then I would transfer over and start to do some work in nuclear physics. In thinking about this, I decided, after looking at Fermi's work that didn't use a cyclotron, that the first thing I'd do would be something that wouldn't interrupt anybody else's work and I could begin to learn a little bit about it and do some experiments, because I had to teach myself most of the time.

So I got the work in hyperfine structure set up. We did several pieces of work that were quite good; the apparatus turned out to be really quite good. And Tomboulian, after I pulled out of it and had only a nominal connection with it, continued to do that sort of work for quite a few years after that.

I had started as an instructor at Cornell at almost the same salary I had had at Columbia, but the dollars in Ithaca meant quite a lot more than the same dollars in New York. In 1937 I was promoted to assistant professor, with a substantial salary increase, which meant quite a bit at that time, because we had a small child; then to associate professor in 1940, with a tenure appointment. So that at the beginning of the war, I had a much better prospect at Cornell.

It turned out that Livingston's interest was mainly in particle reactions. That's what he'd worked at in Berkeley. This machine at Cornell was not a big enough machine for many of the things he worked at, though he did some very nice work on particle reactions in the light elements. I think it was in 1938 that he was offered a chance to go to MIT and build a much bigger cyclotron there. By that time, a good many schools were starting to build much bigger cyclotrons. He was very much tied to the building of machines, and this was a very good opportunity and a very good school. So he left Cornell and went to MIT to build this much bigger cyclotron. Well, I inherited the small machine, and I immediately turned it primarily toward neutron work, because this was what I'd been working at. We'd never really done much neutron work with the machine. There was one circumstance that made it particularly appropriate to move in this direction, and that was that Livingston and Marshall Holloway—who worked with me at Cornell and later at Los Alamos, and who had taken his PhD with Livingston at Cornell and subsequently was a postdoctoral fellow there—had developed an arc source for the little cyclotron. This arc source raised the current that you could get out of the machine very greatly. This was a rather major development, because it put intensity into cyclotrons that didn't exist there before, and it was particularly important in a small machine.

This didn't raise the energy of the machine—we were still limited to the same energy—but it did mean we had a much larger intensity to work with. Also, it turned out that the arc source fitted into neutron work. Just about this time, in '38 or '39, I read a paper that Luis Alvarez had written on work he'd done at Berkeley, in which they had modulated the beam of the 37-inch cyclotron, which was their original machine. They had succeeded in producing bursts of neutrons out of it. This was the first time anybody had ever been able to measure neutron energies by the time of flight it took a neutron to travel a given distance. But his conclusion was that with the method of modulation they had, this could only be done for very, very low energies, and it just seemed impossible to get up as far as thermal energies for neutrons, which was the beginning of the energy region of particular interest in neutron work and was called the neutron resonance region.

Well, it struck me that with our arc source, we had something that could be modulated in much shorter bursts. So I started doing some calculations with it, and to my amazement—and Bethe's initial disbelief—found that we ought to be able to do very much better with our arc source by modulating it, if we could figure out how to modulate it and really do it well. But it demanded a lot of equipment. I figured out the neutron's slowing-down time to one electron volt—this is just above the so-called thermal energy, which is about a fortieth of an electron volt. It was a fraction of a microsecond, and I said that if we could develop fast enough circuits, we could work above the thermal range; whereas Alvarez coming at it from the other end despaired of extending his work up to thermal energies. So we threw all of our effort into this work, when I really convinced Bethe and [George] Placzek. Placzek bet me $10 we'd never do this, and I was very glad to collect that bet from him. It turned out it was relatively easy.

Now, meanwhile, I haven't said a word about teaching at Cornell, and what I was doing in this, because I wasn't just thinking about research. One of the jobs I'd undertaken when I went there was that they seriously needed some help in the introductory physics course. They had a man who lectured in this course and had done all of the lecturing in it for years and years, named Professor Harley Howe. I listened to him lecture, and he was an extraordinarily good lecturer. He had one problem, and that was that he'd never done any research since he'd taken his doctor's degree. He didn't know anything about modern physics at all, and we were in a time when students who studied physics ought to know about modern physics from the beginning. This was something that just had to get changed. So they developed what at that time was a very ingenious way to change it. I came in with the general idea that my first teaching obligation would be in connection with this elementary course, and we arranged it so I'd do the lecturing half of the time. I would take the course in February of the first year I went there and do all of the lecturing for one year, around through the first semester of the next year. This was a pretty heavy job; there were about 550 students taking this course. It was the primary Arts College course and Agriculture College course for physics. It wasn't a very thorough course, but the lectures had always been done very carefully, with the idea that good experiments done in the lectures would make people see and remember for a long time what the physical principles were. Howe was excellent at this.

TERRALL: How did he feel about your coming in?

BACHER: Oh, he liked it; he welcomed it very much. We got along very well together. Especially when he found that I genuinely admired his capabilities as a lecturer. Also, he wanted me to teach him some things about modern physics, which I did. We got along very well together.

TERRALL: So he remained associated with the course?

BACHER: Oh, yes. He was in charge of the course, and this was a great help for me, because the year I lectured in the course he ran it, in the sense that he managed all the teaching assistants and saw to all of the details, which are an enormous job if you have 550 students. But you didn't just come in and lecture in the course; you also taught a laboratory section and a recitation section, to keep track of what was going on in the course. I enjoyed this, but it's not a small teaching job, I found. I found it really a very hard job.

It turned out there were too many students for a single lecture, so the lectures were given two days a week, at nine and eleven o'clock. So Tuesday mornings I lectured from nine to ten, and then from eleven to twelve. You inevitably don't give the same lecture the second time, but the point was that you had to end at approximately the same place, because you had to keep the two synchronized. It was hard work. I hoped he learned something about modern physics from me, and I certainly learned a lot from him about teaching, because he was an expert—a real professional, old-style teacher. I liked him. In the off year, when I wasn't giving the lectures, I still taught a recitation and also a laboratory section, and then I got a chance also to teach an advanced graduate course, which varied a little bit from time to time. And it gave me, during that off year, some additional time for research.

The first summer I was there we were pretty hard up, and I agreed to teach in the summer school. I don't remember the figures exactly, but I think I taught two courses for their six-week summer session. I think my pay was about $175 for that. I never taught in a summer session again. [Laughter] That was the end. I learned that we had to get along without doing that. Also, it was impossible to do much research when you taught that much. After this one experience, I kept the summer for research—and of course, I didn't get paid for that. This wasn't in the days when people worked on contracts, where they get paid from their contracts for summer research.

I spoke earlier about Livingston leaving for MIT. Marshall Holloway, who had been Livingston's graduate student and a very competent physicist, stayed on. He had been Livingston's postdoctoral fellow for one year, and he stayed on as postdoctoral fellow with me. His principal job was to keep the cyclotron functioning. When things got tough, we all pitched in on it, but we didn't have a lot of assistants around, the way people do now. Charles Baker, who took his degree after I took over, was a very perceptive fellow. He started to work with me on this neutron work. Later, after we had the work going well, Boyce McDaniel came in as a graduate student and worked at it. He did a wonderful job, and I think advanced the equipment very much. Interestingly enough, he's now the director of the Cornell Laboratory of Nuclear Studies, which is quite a big laboratory now. He has just been elected to the National Academy of Sciences.

It was an interesting and exciting time to set up this modulated neutron source. There were a lot of problems about it. First, there were a lot of questions of how we could collect data without wasting so much machine time, so we eventually had to make a multi-channel device, which I don't think had generally been used at that time. We had to figure out ways, since it was a pretty complicated piece of electronic gear, of running down where errors occurred in the machine and being able to find them. So we built a piece of equipment for doing that. But more particularly, all the circuits had to be very fast, because we were talking about getting down to measuring things in millionths of a second. To give you just a little idea of that, the time of flight for a one-electron-volt neutron for a meter is 72 microseconds—72 millionths of a second. For a thermal neutron—that's at about a fortieth of an electron volt—for that distance, it turns out as 456 microseconds. Well, that's a much more relaxed period. But we wanted to work up above one electron volt eventually, and so we were very much interested in pushing our timing all the way down. There were some big problems in this, because for the machine to work we had to be able to detect neutrons in a time comparable to a microsecond, and that just hadn't been done.

TERRALL: So that was the problem, more than getting higher energy neutrons.

BACHER: We didn't get higher-energy deuterons in the machine; we got higher intensity. We had to get higher intensity because you couldn't think of doing this sort of a thing where you kept the machine on for some microseconds and then you turned it off while you waited for the neutrons to run out, and you had to wait until all the neutrons had been cleared out and then start over again. You could do this hundreds of times each second, but at the same time you lost a very large factor in intensity by so doing. So this was something where the fineness with which you could do the experiment depended in part on being able to get higher intensity. So we needed both of them.

Well, we had the arc source in the machine; that was a very fine development, and it worked pretty well. The problem was, could we make a detector that would work that well? That was a tough job and nobody had done it before. I think partly by just being bull-headed about it, we went ahead and were able to collect electrons out of a gas, boron trifluoride, very quickly. We did it in part by getting the gas a lot purer than it had ever been made before. What we wanted to do was set up a modulated neutron source and investigate the absorption of cadmium, which it turned out had a resonance absorption just above the thermal range. We wanted to really see the resonance absorption of cadmium and of silver and indium and so on, which had been discovered by Fermi by a much more complicated and ingenious process.

The method was very simple, in principle: You measure a distance and you measure the time it takes the neutron to go the distance, so you know what its velocity is. It's the simplest thing you can think of, almost. The problem was that it was a neutron that was doing the going; you had to get it started at the right time, and you had to slow it down. The slowing-down process—as I explained earlier—was quite fast, because the neutrons came out at several million volts energy. So the method was simple in principle but did take some fairly complicated—for those days—electronic equipment. A burst of neutrons from the cyclotron went out from the machine into a paraffin block, where the neutrons bounced around against hydrogen, lost energy, and got down into this epithermal range. As I say, the slowing-down time to just above the thermal range is only about one microsecond, and this was the key to getting this done. Then the neutron pulse was picked up at a distance by this velocity spectrometer. The detector we developed was a boron trifluoride detector. Boron trifluoride is terrible stuff—it's very poisonous, and it's miserable to have around; it's corrosive; it's everything you can think of. Well, by measuring with and without an absorbing target in there, before the detection chamber, we could get a neutron absorption spectrum of various materials by using pieces of cadmium, indium, gold, and so on. This was the first time this had been done, and it was the first time that neutron resonances were fully elucidated. They had been inferred from experiments Fermi had done, but with this you could really see them and plot them out, the way you could in a spectrometer. This never would have been done at that time if it hadn't been for the very fine circuit work that Charles Baker did. He was a very ingenious fellow, who doted on finding trick ways of doing things that seemed impossible to do.

TERRALL: He was a graduate student?

BACHER: He took his degree with me in this work and did a very fine thesis on some of this earliest work in time-of-flight of neutrons. Before the work had finished, I'd been persuaded to leave Ithaca to join the Radiation Laboratory at MIT, which I've mentioned to you before, and of which Lee DuBridge was the director.

Begin Tape 3, Side 2

TERRALL: Before you get into that, I just wanted to ask you, were you leaving theoretical work behind? Were you writing theoretical papers as well at this time?

BACHER: No. I mentioned doing some fairly complicated work with Goudsmit on complex spectra. I wrote with Goudsmit—and I did most of the writing—that rather long paper, "Atomic Energy Relations. I," and I never wrote part II. Some of that work's not been done to this day, but quite a lot of it has been done and I've corresponded with people who've worked at it. I still have some work in this subject, but I don't expect I'll ever get at it.

TERRALL: Once you got into the neutron work, you weren't interested in doing theoretical work there?

BACHER: Well, let's put it this way. Certainly my theoretical background was very helpful to me in getting started in this neutron work. I don't know whether I would have found out about being able to work above the thermal energy if I hadn't had a theoretical background. Alvarez had said this was impossible to do, and Alvarez was a first-rate experimental physicist—I have a very high opinion of him. But the point was that what I did was essentially say, "Well, suppose you could do all of this a lot faster? Is it possible?" So I just went at it as if you could do it, and then found out what the times were and found they were times we could probably attain. That's how we got at it.

I just started to say that before this work was finished, I'd been persuaded to join the Radiation Laboratory at MIT, of which Lee DuBridge was the director.

TERRALL: How did he persuade you?

BACHER: Well, first of all let me say that the title, of course, was a fake. It was named after the radiation laboratory that Ernest Lawrence had built up. The laboratory essentially worked on microwave radar, which had been given a tremendous start by the British.

Well, what persuaded me to do this was that in the fall of 1939 I had certainly done a lot of worrying about the situation in Britain. Through the summer of '39, every day I'd drive downtown and get myself a *New York Times* to try to keep abreast of what was happening in the world. I didn't have very many people to talk to about this. To me this was a very frightening time. Most people took the view that, "Well, we could stay out of such a war." I took the view that I didn't see how we could avoid being involved in such a war, if it went on at the scale that it looked as if it was going.

The Radiation Laboratory at MIT was started by Karl Compton, who was the president of MIT; Alfred Loomis, who was a great supporter of physics and had retired as a wealthy New York industrialist; and Ernest Lawrence, the director of the laboratory in Berkeley. They together had come to the conclusion that something had to be done very vigorously to get research started in the United States in some of the critical areas. In the summer of 1940, the future of Britain looked pretty grim. There was a strong feeling among these people—and I think among most people who knew the inside story—that we had to get much more into war work than we had gotten before.

Furthermore, the British were being very helpful with this. The British invented the key device, the magnetron, that was used in microwave radar. This magnetron was able to produce radio waves that were only ten centimeters long. That meant it operated at a frequency of 3,000 megacycles, which is a very high frequency. Now, it turned out that a young man named [Claude E.] Cleeton had used even one-centimeter waves about ten years earlier, with a Japanese-type magnetron, for physics experiments, using a large aluminum strip diffraction grating for a spectrometer. I knew the work. But what was new about the British magnetrons was that they generated enormous power in pulses. This had never been done before and was a completely new discovery. The whole field of microwave radar wouldn't have been possible at that time if it had not been for this discovery. This was brought over from Britain. It was first taken to the Bell Telephone Laboratories, because they were closer to this than anybody else, and they started into the magnetron business; later, other companies got into it.

But very shortly thereafter came the Radiation Laboratory at MIT. At first, it didn't have any money to run on. MIT, with the backing of Alfred Loomis, provided the wherewithal to keep this going for a while. At the Christmas 1940 meeting of the Physical Society in Philadelphia, I was approached by Lee [DuBridge]. As a matter of fact, Carl Anderson was approached at the same time. They said, "Won't you come up and talk to us?" We rode up to Boston on the same train and were interviewed up there. They gave us the whole story of what was happening and how serious this situation was and how urgent it was to get going. Carl wanted to go to it, but he had an aged mother to look after, and it just didn't seem a feasible thing for him to pick up and go, so he declined. And I went back with the idea that I'd have to talk to my family about it and see what the possibilities were. Also, I'd have to talk to the people at Cornell and see what they thought about this. I had the complication that we were right in the middle of some of this neutron work.

TERRALL: So, as far as the work that they were doing at the Radiation Lab, even if we weren't going to get into the war, it was a question of helping the British develop radar. Is that right?

BACHER: Well, I think almost everybody who went at that time felt that the way things were going in the war, it was inevitable that we'd be in it and we ought to be getting a head start. But if that turned out not to be the case, well, we ought to be developing things that would be helpful to the British. But this was not something that was fully agreed on. There were lots of people around who didn't seem to think about whether there was going to be a war or not. This was something I had worried about for some time.

So, before the neutron work was finished, I'd been persuaded to go to the Radiation Laboratory at MIT, of which Lee was the director. We really had no working equipment in radar when we went there at all. The various parts all had to be developed from scratch. Furthermore, the project did not yet have any government support, due to the American detachment from what was going on in Europe. Without that support, our appointments ran out about the end of March. One evening on the way home, I started to worry about this uncertainty, especially since Jean and the two children were now in Cambridge. Suddenly it hit me that I had a tenure appointment at Cornell and all I had to do was tell them I was coming home if the project ran out of funds. Of course, it did not.

TERRALL: Was there some relation between this radar work and the neutron work you had been doing? Is that why they contacted you?

BACHER: Well, there was, in a peculiar way. The time of a radar echo from one mile is about ten microseconds. In other words, the time scale they were working in was the same time scale we'd been working in with neutrons. Now, getting down to one mile at first seemed a little close to work with. But we were looking at things that were out several miles and that meant measuring times of several tens of microseconds. The same times were involved that were involved in measuring neutrons for a few electron volts.

TERRALL: Is this why they thought you'd be interested?

BACHER: I haven't any idea. Actually, what they did with anyone who came to the Radiation Laboratory was, he was immediately put around in several different places to get educated, because the fact was that none of us knew anything about it. It was all completely new to us. Eventually the laboratory was organized into a number of groups working on various components. Within a few months, I became the head of the so-called indicator group. Well, it turned out that the indicator group had to take signals that came in that were on this fast time scale and display them in some way that you could understand what was going on. I brought into that work from Cornell one man who'd been closely associated with electronics work—William Higinbotham, who was extremely good at this sort of circuit work and had helped us very much in the neutron work.

But I could not just leave and drop all this work in Ithaca. If that were the alternative, I just couldn't see how I could go, because at that particular moment we had done quite a lot of work, and we had reached a situation by Christmas 1940 in which our results did not agree with the results Fermi had used for five years. I didn't know what the discrepancy was. Our results were self-consistent; in other words, if we measured the energy of various resonances and so on—and we were able to measure a number of them by that time—we got a number, but it didn't agree with the number Fermi had. Well, I had some ideas of what to do, and that's why I felt that this thing had to be resolved. Of course I had to get the permission of the people at Cornell to do this, but it was finally arranged that I would go to the Radiation Laboratory at MIT; we'd keep the laboratory at Cornell going; and every three weeks I would come and spend a Thursday, Friday, Saturday, and Sunday there. So I would come every three weeks and spend four days there.

As a matter of fact, I was in the laboratory at Cornell taking data one afternoon very peacefully—we took turns taking data, and we usually had two people around, but I was taking data alone that Sunday afternoon when the Japanese attacked Pearl Harbor. If things were going well, we kept a radio going for amusement, and I was listening to the symphony concert from New York when that was broken into by the announcement of the attack. I called my friends, the Longs, at Cornell, with whom I stayed when I went there. They couldn't believe this was happening. I have to confess that I didn't think that's the way it would start—but I was pretty sure something was going to happen.

I was saying earlier that we had no working equipment in the beginning at MIT at all. Most of the parts had to be improvised from scratch, except the magnetrons, where we got some samples over from Britain. But we were moving at such a rate that when we heard about things, very often we could produce something that was not too far from being satisfactory before we could get a sample over. When you get a lot of people all focused on one thing, it's amazing how well things go. We had very strong backing from some of the best industrial laboratories, and without this we wouldn't have been able to get anywhere. They worked with us very well. For example, the Bell Telephone Laboratories, General Electric, RCA, Raytheon, Sperry, and a host of others I don't remember just at the moment.

TERRALL: This was financial backing, or they were also making equipment?

BACHER: No, they took their best people and assigned them to work on this. We usually put two contractors on a given job and then tried to get them to exchange information, and we guided it. General Electric and RCA worked together on developing special cathode-ray tubes on which the radar signals could be displayed so you could see what was going on. And that took a whole new development. To give you some feel for the job that was involved: If I remember correctly, the number of cathode-ray tubes made in the United States prior to the development of radar was about 1,000 or 1,500 a year, and they were mostly used in oscilloscopes for a few laboratories here and there. By the end of the war, we were making several hundred thousand a year in the United States. This was one of the major jobs we had to do.

Our participation in this was very good, because we could take a different attitude toward the military services from that which could be taken by an industry, where they really had to take orders. Let me give you an example. When I was working at the Radiation Laboratory, a man from the navy came up, and we had a new development we thought was quite interesting—I don't remember the exact details of it at the moment. But he took one look at it and said, "We're not interested in that." But we also found out, in talking to the army representatives, that they were very much interested in it; and we were very much interested in it, because we thought it would be a breakthrough. So he came again a month later. He saw this work going on, and he said, "I thought I told you to stop that work." I said, "Oh, yes, you did, but we're interested in it, and we think it has promise, and the army seems to be interested in it." "Oh," he said, "let's look at it again." Well, he finally saw that he'd missed the boat. The navy was so much better organized that they could miss something like that and they'd still be the first to have it in service. The army always somehow got things mixed up. But to have a laboratory that was not immediately subject to a particular man who looked at something was very valuable. We didn't argue with him; I mean, he could have his own views. But even if the services said they weren't interested, if we thought it was important we could work at it.

TERRALL: Were they funding you?

BACHER: No.

TERRALL: There was no navy money at that point?

BACHER: No, we didn't get any money from them. They funded contractors. Our money came at that time through the National Defense Research Committee. That was just the start of the National Defense Research Committee. And that's the reason it worked so well; the money came through a different source and they couldn't cut it off.

TERRALL: But they [the armed services] were following what you were doing?

BACHER: Oh, they followed it closely. The fact is that you cannot have a system where the judgment of what your policy ought to be depends on one man, I don't care how smart he is. You've got to be able to test it more thoroughly with the ideas of more people. This was a way of doing that.

It was only a few months after the laboratory got started, and I was present in one of our roof laboratories—I don't know the [exact] circumstances of how I was up there, but I think it was because we were testing one of our beginning indicator tubes—that we saw our first radio echoes on ten centimeters. The first thing we saw was the spire of the Christian Science Church over in Boston. We had seen echoes before, when we sent the signal out through one device and then brought it back and picked it up with another device. But for a radar to work—and if you're ever going to fly it in a plane—it's got to come back to the same device. And that means [the device has] got to change from being a transmitter to being a receiver in a few microseconds. And that was a very tricky device; I had nothing to do with the development of that. Lee DuBridge was in England visiting, and he was sent a cable that said, "Mother church on one eye." [Laughter] He knew exactly what that meant, because we always worked off the tower of the Christian Science home church for radar echoes in that particular period.

The speed of development was very fast, but it was barely fast enough. For example, along in the spring of 1941—this was before Pearl Harbor—we were having all sorts of ships sunk off our shores. German submarines were out there, and their pattern at that time was that they'd stay submerged during the daytime and at night they'd come up and recharge their batteries. So they'd sit on the surface and were pretty easy to see by their radar echoes when they were on the surface. They were causing tremendous damage. Before we were in the war, we lost millions of tons of shipping off our coasts, and it was dangerous up and down the Atlantic Coast all during that period. The Radiation Laboratory installed, from equipment it managed to put together around the laboratory, some twenty sets that were built to detect submarines on the surface, especially at night. It turned out that these twenty airplanes essentially drove the submarines off the Atlantic Coast and down into the Caribbean. Then they went down to the Caribbean to hunt them down there, and it became much too dangerous for them to work on the surface there. That didn't mean the submarine problem was solved, because in the later days they had all these so-called wolf packs, great packs of submarines that would attack shipping out in the middle of the ocean. This was a very, very difficult problem. When they were near the coast and could be reached by close planes, why, [we'd] come in and get them. You don't have to sink such a large number of them—if they find they can't come up and surface, they can't stay in that area very long. They had to work an area where they could surface—or that was the case at that time.

The Radiation Laboratory, with its close links with industry and the services, made a real impact on the war—airplanes, ships, ground stations, gunsighting equipment. Essentially, the range it worked in was in wavelengths from ten centimeters on down. The smallest wavelength it worked at finally was about one centimeter—I don't think it ever worked much below that—but most of it was done at ten centimeters and three centimeters. The three-centimeter work was started very early in the game, because we had the idea that once the Germans were hit with ten-centimeter radar, they'd recognize it right away and immediately take steps to put warning devices up. It turned out that it took much longer for them to react and realize what was hitting them. But three-centimeter radar did go in, of course.

I've mentioned that when I went to MIT, I had some neutron work in this critical state that ought to be finished. At that time, my attitude toward the uranium project was that I thought it was barely possible that the work on uranium would be able to come into the war. And I felt that the work we were doing would at any rate verify the correct energy scale and then could be used by Fermi and others at Columbia. When I said that it didn't agree, the disagreement with Fermi's work was not trivial—it was a disagreement of sixty percent. This was traced down to the fact that Fermi had assumed, in the absence of direct information, that all of the neutrons emerging from a paraffin block were in thermal equilibrium. But when the neutron gets down to an energy close to thermal, it makes a lot of collisions without really becoming thermalized. So it can drift quite a long way in that period, and many neutrons would escape at the edges. He used boron as a standard cross section, and he had correctly assumed that boron would have a cross section that varied inversely as the square root of the energy, or as the velocity. You can see this in a very simple way. If you have a neutron going by a nucleus, the interaction with the nucleus is going to depend on the time that it's within the interaction radius. And that time varies inversely with the velocity, or inversely as the square root of the energy. So this was indeed a correct assumption; it seemed unassailable. But it suddenly hit me like a ton of bricks, sometime along in the early spring of 1941, that the trouble was with the boron. We had not measured the cross section of boron. We hadn't measured it because it didn't have any resonances. We weren't particularly interested in it. The only purpose we used boron for was for a shield. When we measured the cross section as a function of the energy of the neutron, we found that the previous measurements were just plain wrong. The cross section that had been measured at around 400 microbarns had assumed that the neutrons that came out were thermalized. It turned out that the cross section for thermal neutrons was about 710 x 10^{-24}

square centimeters—710 "barns," as we called them; a name that was, incidentally, invented by Charlie Baker.

Well, when we looked at it then carefully, we found that our energies, which were consistent among themselves, differed from Fermi's by just the right amount so that if you corrected Fermi's data by using the *correct* thermal neutron boron cross section, everything agreed.

Well, at that point we decided we ought to write this up, and we wrote a couple of papers about it. I can remember very well taking a write-up of this to Fermi at Columbia University where he and his group were then working—first to see if he agreed, because, after all, he had been working in this field for five years and using a fundamentally different number. I knew Fermi pretty well by that time. He read it very carefully, and while he read it I kept absolutely quiet; I think he asked me one or two questions. When he finished it, he said, "You're right." Well, this was quite a step for Fermi, since he had used the other value for almost five years and it was a fundamental part of what they were working on. So I said, "Well, what do we do with it?" And he said, "You ought to publish it; it's good physics." So, not thinking much more deeply about it than that, we prepared two papers and sent them into the *Physical Review*—in one we wanted to report a number of cross sections and in the other we wanted to report this work on boron, because that was the thing that really explained the discrepancy. Later, I thought about all of this, and I thought, "Well, you know, there's just this chance there may be something to this uranium work. If there is something to it, we really should not be publishing things that will help the other side by getting it in the literature." I concluded that there might be something to it, so I decided to write to the editor of the *Physical Review*, and I told Fermi what I was doing. I consulted with Baker and Holloway and the others who had been directly involved in this and explained to them what the situation was, and asked the editor to withhold the papers from publication until after the war. Later, when I got into the nuclear problem itself, I found lots of copies of these papers, and they were all marked "Secret," but nobody bothered to tell us that at Cornell, and the original papers sat in an unlocked filing case in my office all through the war with nothing stamped on them, which was really sort of funny.

We then made a number of other measurements at Cornell, including a measurement on uranium-238 and certain other cross sections they were interested in. Later, the Cornell equipment was taken to Los Alamos, where it was used and improved by Boyce McDaniel. He used that equipment, improved and modified, at Los Alamos, for a period of two or more years and did a number of measurements that were of considerable interest and importance to us. The Cornell laboratory was shut down in 1942 for the duration.

TERRALL: Because everyone had gone to other places?

BACHER: Well, Holloway and Baker had gone off at the instigation of the people in the neutron project. By that time, Oppenheimer was the head of the fast-neutron project working out of the laboratory at Chicago, which was operated by Arthur Compton. About two weeks after Pearl Harbor, they called a meeting in New York, which I went to, and they announced that they were going to try to get all of the laboratories they had in a number of different places and center them around Chicago. All the work immediately related to the work of a nuclear reactor was going to be moved to Chicago. This was precipitated by the greater seriousness with which the problem was taken, and also the fact that the Japanese had attacked Pearl Harbor.

After we shut down the Cornell laboratory, I arranged with the Radiation Laboratory at MIT that McDaniel would go to the Radiation Laboratory, with the idea that—and nobody knew this except Lee DuBridge—he'd go around and be in various groups and learn about the techniques that were developed that involved very-short-time measurements, because the uranium project was going to need these. Generally speaking, the people who were working in nuclear physics when Los Alamos got started worked with relatively slow circuits. My feeling from the beginning was that we just had to get into fast-circuit work. McDaniel went to MIT sometime in late 1942, and he spent some time in various groups, getting knowledge of new developments and of circuits that were in use in radar which would be of use to a nuclear lab. We profited later at Los Alamos by this very greatly.

ROBERT F. BACHER

SESSION 4

July 1, 1981

Begin Tape 4, Side 1

BACHER: Holloway and Baker, who were part of the group at Cornell, took the cyclotron to Purdue to work with two members of the faculty there on some experiments that were preliminary to the Los Alamos project. This was done after Robert Oppenheimer had taken responsibility for the higher-energy work in relation to the Chicago project. This was in the fall of 1942.

Going back to the Radiation Laboratory at MIT, the job I finally ended up in was as head of a division that consisted of all of the work on treating the incoming signals. We had the component work, as we called it, broken up into two parts—one the transmitting section and the other the receiving section. So this involved the work in receivers, which I was no expert on, and the work on indicators, which had been my first responsibility there, and we had to develop a philosophy of this and develop the tubes. The object was to get a display on a cathode-ray tube. We quickly came to the conclusion that our ultimate discrimination of a signal from a reflected image of the radar waves, against just noise that came from the set, should be done finally on the cathode-ray tube. We had a contract with GE and RCA, which was a very interesting relation. We managed the contract, and one of the provisions was that they work together on it. GE and RCA had never worked together on anything before—but it worked very well. I supervised this myself. I would go to GE one time and the next week I'd go to RCA, and the week following that we'd have a joint meeting at the Radiation Laboratory in Cambridge.

TERRALL: So you were really getting into administrative work.

BACHER: I was getting into contract management. I was spending a lot of time on these developments, and it involved an awful lot of travel. Just before I left the Radiation Laboratory, I figured out that with all of the things I had to travel to, there and to Washington, I was spending nearly half of my nights on the Pullmans, and that was pretty rough.

In late 1942, Robert Oppenheimer approached I. I. Rabi and me [for] guidance on starting a new laboratory for the nuclear weapon work. We met several times with General [Leslie R.] Groves and also with Dana Mitchell, who came from Columbia and whom I happened to know, and who was already committed to this project and was in it early to be the one in charge of setting up their procurement facilities. He'd been an expert at that and had done it for years at Columbia. We met with Groves—I guess "surreptitiously" is the right word, because we met, just the five of us, in a room at the Biltmore in New York. Oppenheimer had asked Groves to allow him to consult with Rabi and me, so several meetings were held with Groves present.

TERRALL: Groves had at that point already decided on Oppenheimer as his director?

BACHER: He had already decided on Oppenheimer as his director. I don't know just when that was. There was a big row about this, because there were people, I found out subsequently, who didn't want Oppenheimer as director of this new project. They didn't think he could do it. But what went on in these discussions was extremely interesting. A number of things came out that were very disturbing. The first was that while Groves seemed very knowledgeable, we discovered that Oppenheimer had already agreed to take a commission as a lieutenant colonel in the army and the intention was that it would be turned into a military project, with all the people on it members of the military service. Well, both Rabi and I took an extremely dim view of this. We had had experience with the military laboratories, and almost without exception those laboratories operated directly by the military were in a terrible state. In a laboratory, it's extremely important that the hierarchy be founded on the capability of the people to do the job they have, not on what military rank they happen to have. Here they were essentially ranked for one reason and then put into a laboratory to do a job. We told Oppenheimer that this wouldn't work. We discovered, to our terrific amazement, that he not only had agreed to accept a commission but had ordered his uniforms. We just made it very clear to him that if this was what he was going to do, we weren't going to have anything to do with the project and we were pretty sure that nobody connected with our laboratory would have anything to do with it either.

TERRALL: Do you think he had just not thought about what the implications were of having the lab be a military installation?

BACHER: He just didn't know the military; he hadn't had this experience. This was a terrible shock to him, I think. At first, I think he thought he could certainly handle this in one way or another. He was, after all, an extremely intelligent man and adroit in personal relations. I think he thought he could handle this, but I was convinced he couldn't and so was Rabi, and we told him that if that was the way this was going to be, count us out. Well, this caused an uproar.

TERRALL: Because Groves had obviously been thinking along those lines.

BACHER: Groves had been thinking along the lines of making this a military laboratory, and that this is the way it would be—after they got it assembled, it would turn into a military laboratory, perhaps quite soon. We said we wouldn't have anything to do with it. I don't think he'd ever had anybody talk to him quite that way before, but the fact was that we had dealt with people of equal or higher rank than Groves had, so we were not as intimidated as these other people were in dealing with him. As a matter of fact, I got along very well with Groves, all during the war and afterward, and I think this encounter was one of the reasons.

Well, to make a long story short, Groves saw that if he did this, inevitably the word would get around among the scientists and he wouldn't get many scientists for his laboratory. Some of the people in Washington, some scientists, were very upset about this. At first Dr. [James B.] Conant, who was the number-two man in the NDRC and the one principally connected with being responsible, couldn't see any reason that this wouldn't work. I think he finally came around to realizing that it wasn't going to work. Well, the fact is it wasn't, because we couldn't be drafted into going out there. As a consequence, there was a letter written—and I have a copy of it in my files, and I intend to leave it in my papers with the institute. It's one of the original copies of a letter called the Groves-Conant letter. This letter essentially said that it would start out as a civilian project and would continue as a civilian project until such time as the project had considerable amounts of fissionable materials for the bomb. When that happened, the project would become a military project. Just to jump ahead, one of the reasons I think Groves was an extremely good manager was that to my knowledge he never mentioned that letter subsequently. The project was a civilian project to the end. And I think Groves knew himself that a military project wouldn't work, because he'd had sufficient trouble with the military people and he saw the same kind of trouble we were trying to show him. I've always had a very high opinion of Groves, in part for that reason.

TERRALL: At this point, were they trying to recruit you?

BACHER: Not yet. These discussions I have just been speaking about, with Groves and Robert Oppenheimer, ended essentially with Rabi and me going to a two-week meeting to inaugurate the Los Alamos laboratory. They had gathered some of the people together who'd been working on this subject and started a conference. My memory is that we arrived approximately the 15th of April in 1943 in New Mexico. We had to procure tickets and nobody was supposed to know where we were going. I just disappeared into thin air. Lee DuBridge knew where we were going, because he was the director of the laboratory. But nobody else, except Jean, knew where I was going; we just disappeared for two weeks.

So we went to this meeting. The meeting was called to have the principal people at Los Alamos and several people from the Chicago project, plus the two of us, out there for discussing what the laboratory ought to be. I think we were the only people who were outsiders. Not everybody stayed indefinitely on the project, but almost everybody else had been involved much more in the project than we were. We, after all, came from another project entirely; we had responsibility to the Radiation Laboratory, which was working on radar at Cambridge, to develop a view as to what our attitude in the Radiation Laboratory ought to be toward this work. DuBridge said to us, "There are two reasons that you're going out there: One is to help them get a laboratory organized—that's what they asked you to do—but also it's going to turn out to be something in which they're going to want help. And we've got to form a policy as to what our attitude will be."

It was a very interesting meeting out there. It was mostly devoted to elaborating the unknown physics information that was needed for knowing whether an atomic bomb would work or not. Because there were fundamental things where the information just wasn't known. For example, nobody knew how fast the neutrons came out; some of them were known to be delayed. But even the ones that came quite promptly—you could say, "Well, they probably come right away." But they had to get out of there in a time like 10^{-8} seconds—that's a hundredth of a millionth of a second—and nobody had any idea of whether that was it at all. In fact, any measurements made up to that time only went down to about a hundredth of a second or something of that sort.

So there were lots of things that were discussed. Next was the question, How do you assemble an atomic bomb? Presumably what you want is to get together what's called a supercritical mass, which will then blow up. But you've got to get it together pretty fast, because if you don't get it together pretty fast, it will just go *poof* as you put it together. There were two principal methods considered for putting a bomb together, and I guess those can be talked about now. The first and straightforward one was essentially to shoot part of an assembly into another part. This was the method everybody considered pretty straightforward. This was indeed the method subsequently used to make the Hiroshima bomb. It had a number of difficulties, but it looked straightforward, and with the known nuclear constants of uranium-235 it should be something that could be done without a wholly new art being developed. There seemed to be no reason why plutonium shouldn't work this same way, too, though there was very much less known about its nuclear properties.

Secondly, it was suggested by Seth Neddermeyer, a former student and collaborator of Carl Anderson here at Caltech—and this is one reason I'm telling you about this—that an assembly forcing components together by explosives be used [an implosion]. There was very little information about this, and the technique looked very difficult, even though it might be better if fully successful. It looked as if it had some advantages, but it looked much too difficult for us to develop on anything like the time scale we were talking about. If this project was to be successful, it not only had to develop an atomic bomb, it had to develop an atomic bomb before the end of the war.

So the technique looked very difficult for Seth Neddermeyer's work. It was finally concluded that the gun assembly should be used, but a small group under Neddermeyer was set up to explore the implosion method. That group was kept during the first year of the project as a relatively small group, and they went ahead with the idea of trying to do experiments. It was hard to observe what was going on. Neddermeyer is a very ingenious physicist, but it was a difficult enough program that he didn't get very far at it.

While we were out there at this April meeting, Robert Oppenheimer asked both Rabi and me to join the laboratory. Rabi thought his family problem—schools and so on for his children, who were older than ours—would be impossible. His children were of high school age, and there just wasn't any high school. He wanted to help the project, but he thought it would have to be done some other way. Also, he had charge of an advanced development group at the Radiation Laboratory and was a unique head of that. So Oppenheimer then started to work harder on getting me out there. But his proposal of what to do was fragmented into several problems—he thought I might do this and this and this. I thought this proposal of what was to be done reflected the disorganization of the laboratory generally, and especially the experimental work, so I declined. I wrote him a letter declining and telling him I thought he could get the job better done by getting a few specialists in the areas he was talking to me about, and that that's what would have to be done anyway, and that there wasn't any real need for me there to do what he was outlining. Now, this letter exists some place but can't be found.

TERRALL: You don't have a copy of it?

BACHER: No, I don't have a copy; it seems to have been lost. But after that, Oppenheimer came back with a much more specific letter, and a letter quite different from any that had been written to anybody out there. This letter is published in a collection of letters. [*Robert Oppenheimer: Letters and Recollections*, ed. A. K. Smith and C. Weiner (Cambridge, Mass.: Harvard University Press, 1980)]

TERRALL: Yes, I've just read that recently.

BACHER: Well, that's the letter he finally wrote to me.

TERRALL: So that was prompted by your refusal.

BACHER: That was prompted by my refusal. Quite frankly, it seemed to me that it had to be something I had real confidence in, to warrant my pulling out of the Radiation Laboratory. I had by that time a major responsibility there, and unless I could see I was going to do something really useful [at Los Alamos], I couldn't see any reason for going.

TERRALL: But Oppenheimer, at least from that letter, sounds like he was absolutely convinced that he had to have you.

BACHER: He wrote a pretty strong letter; he really laid it on the line. It had two effects in the [Los Alamos] laboratory. First, I think this was what made him realize he had to have a much tighter organization in the laboratory than he had. Also, I told him he had to get many more engineers into it. The letter responds as if I wanted to do the engineering work, but that wasn't really the point. I said he needed engineers, and I was sure they had to come into the project; it wasn't that I wanted to be involved in that particular engineering work. But I thought they had to permeate the place. The place was full of very good physicists, but they didn't have, at that time, very many good engineers. A lot of the engineering work at the laboratory, even to the end, was done by physicists who converted themselves to engineers. That was sort of the history of the war.

TERRALL: But in that letter, he emphasized the importance of your administrative experience.

BACHER: Well, he knew I was responsible for a fairly large group of people at the Radiation Laboratory and that this had gone reasonably well. I don't know that it had gone any better than any of the other divisions there, but we'd learned some things about how to tackle such a problem in the two years and a half that we'd been at it. And that was true. I have the original of this letter and I'll show it to you. Interestingly, the original is unsigned. Probably what happened was that this was rushed out just as I was leaving. I don't know why it's that way. In fact, I'd forgotten that that was the case.

My answer to this seems not to exist, as far as I know. I think I have a draft of it somewhere. In fact, I found a draft when I was cleaning up, and now I can't lay my hands on it. But one part of it, which shows you part of my attitude, is that it contained a paragraph—as, indeed, my earlier letter had contained a last paragraph—that essentially said that this letter was also my letter of resignation on the day the project became a military project, as projected in the Groves-Conant letter. So it was right in my letter of acceptance that this was also my letter of resignation—I didn't have to write one.

TERRALL: So your response to that letter of his was to accept.

BACHER: I accepted, contingent on getting released by the Radiation Laboratory. So I had to say to him that I'd try to arrange it. But at that point, Rabi and I had agreed between ourselves that the Radiation Laboratory should support the project. Now, that was a very radical conclusion, and we did not go to Los Alamos with anything like the idea that this was a doable thing, to get this in on the war. The question we put for ourselves was not: Could you at some time thirty years hence develop an atomic bomb? The question we put was: Could you develop an atomic bomb and get it in on the war if you had the material? Well, we came away convinced that you ought to be able to do this. We thought it looked tough—we didn't know how hard it was going to be.

TERRALL: You also didn't know how long the war was going to last.

BACHER: That's right, we didn't know how long the war would last, but we were pretty sure from the way things were going then in Europe that it wouldn't be forever. Goodness only knows, that was indefinite. This is not getting down to fine points as to just exactly how many years it [would be], but there are some projects that are twenty- or thirty-year projects, and we thought that if you could get the material and do the experiments that were needed, the probability was that unless there was something that was just a complete block to it, it was possible to get it done—if it was possible at all to do it.

As I thought over going out there, my feeling was not "This is such a horrible weapon to develop that you shouldn't possibly ever work on it." My feeling was quite the opposite. My feeling was that this is such a horrible weapon that if it can be done, the thing to do is to get it out in the open as soon as you can. Because the worst thing that could happen would be a situation where a country—for example, like the Nazis—would develop a nuclear weapon in secret. Though the present situation is certainly an extremely difficult one with the development of nuclear weapons, I think there's a fair amount of truth in this—that bad as this is, if it can be done physically, it's better to know about it than to have it sprung.

TERRALL: What was it about Oppenheimer's reply to you that convinced you to reverse your decision and accept?

BACHER: Well, essentially what he asked me to do was come out there and take charge of the experimental physics division.

TERRALL: So he was asking you something that seemed more specific and made more sense.

BACHER: Yes. He was also making a commitment to press on getting engineers into the project. I also pointed out to him that it was contingent on the Radiation Laboratory release and approval, and that it would take some time to turn over my work, and I said, "I estimate June 1." As a matter of fact, when I left the Radiation Laboratory, they divided the work I was responsible for into two or three parts, because it was a pretty big chunk. But they had very good people, and I could leave my job at the Radiation Laboratory and go out there with full confidence that the people who'd worked with me there could do a good job. The interesting part of it was that there was still enough uncertainty left in this that I didn't resign from the Radiation Laboratory. They put me on leave for the remainder of the war. The understanding was that if it became a military project, I would return to the Radiation Laboratory. But as time went on, it was clear that this wasn't going to be a problem. I never raised the question, because Groves never said anything more about it. I thought the less said about it, the better. I think he was fully convinced.

Well, at any rate, the Radiation Laboratory approved. But some of the senior people who were on the steering committee, when it was put to them that we were going out on a project like this, were very skeptical. Wheeler Loomis, who was the associate director of the laboratory and in charge of all the personnel policies and had been the head of the very good physics department at the University of Illinois, just thought it was plain crazy and a waste of time and effort. But Lee DuBridge took our recommendation seriously. Most of the rest of the people who were consulted didn't have a strong idea about it one way or the other. So the decision was taken at the Radiation Laboratory, with the approval of people in Washington, that the laboratory would give up some senior people to Los Alamos. I would go out there and start this, and we started right away by taking people—about twenty people, in due course—from the Radiation Laboratory. This included one of the other division leaders, [Kenneth T.] Bainbridge of Harvard, who was a very good friend of mine. I also mentioned Willy Higinbotham, whom I had taken to the Radiation Laboratory and who was one of the best circuit experts in the United States. There was also McDaniel, whom I had arranged to have sent to the Radiation Laboratory with the idea that he learn something about their work on circuits, with the prospect of going out to Los Alamos; the way things were set up, he would have gone [in any case]. Later in the project, Norman Ramsey, professor at Harvard, and Dale Corson, now retired, but formerly president of Cornell University, and a number of other experts moved out. These were all people who had various sorts of special training and came in for one reason or another. There were a number of younger people who were experts in specific areas where we needed technical help.

If I recall properly, I made one trip in May from Boston, and came in June 1943 to stay. My family came in late June. It was a serious trip on the train for Jean and two small children. The McDaniels drove our car out. On top of that, our daughter arrived with chicken pox, which she'd picked up somewhere on the train apparently, or perhaps before she came. The worst thing you can do in a community like that is to introduce chicken pox, so she was immediately put in the hospital, which was an as yet unused infirmary. It was terrible for her, but it was the only thing to do. It was a bad introduction to a difficult situation. She was seven at that time. Our housing was an apartment. It was simple but adequate, and some parts of it were really quite good. The laboratory worked nominally six days a week—in other words, it worked Saturdays—and eight hours a day. But most of the equipment, when it was ready, worked seven days a week, and sixteen hours a day; some of it worked all the time.

As the work got started out there, and especially after I was in residence, I found that Robert Oppenheimer was deeply concerned about many things and seemed worried about how he was doing as director. I thought he was doing a fine job; in fact, I was convinced that he was the only person out there who could conceivably be the director of the place. They had an extremely strong theoretical group, and the director had to be able to handle the theoretical people. He also sometimes had to tell them that they had to go on experimental information and not on theory on some things, and he did this really extremely well. At any rate, during that summer I developed a very close relation with Robert Oppenheimer—I spent about two hours a day with him discussing things. Sometimes after work at night we'd talk for an hour or more. There was a period during this time when he felt he could not continue as director—I guess I've written something on this subject, and I'll give you a copy of it because it might be helpful to you. [Robert F. Bacher, "Robert Oppenheimer (1904–1967)," *Proceedings of the American Philosophical Society*, 116.] I knew little of his political background then and the troubles it caused him. I grew very fond of Robert Oppenheimer. He was an extremely intelligent man—one of the smartest people I've ever met. He was also extremely good at taking up a new project or a new point of view. Within a relatively short time, he was as different from the professor I had known before the war as you could possibly think of anybody being. He worked himself to death, practically. But he had many things to worry about, and it wasn't until much later, when I was on the Atomic Energy Commission and his clearance case came up, that I began to understand some of the problems he had. I knew vaguely that he'd been associated with Communists, but I didn't know very much about what that connection was. I knew perfectly well that this wasn't a serious connection, in the sense that it involved his loyalty—that was perfectly clear to me.

Begin Tape 4, Side 2

TERRALL: You said that when you first got out there, he was seriously worried about whether it could all be pulled together or whether he was doing an adequate job.

BACHER: Yes. In fact, there was a period through the summer when he really wanted to approach them about resigning from the directorship. He was very much upset about this. I don't know whether he was completely serious about it or not. But in any event, I don't think he ever brought this up and really did anything about it.

TERRALL: Was it just that the problem seemed overwhelming?

BACHER: I think [it was] a combination of things. In the first place, it was extremely difficult to be the director of that laboratory, because he spent a major fraction of his time trying to keep things on track as far as the living conditions of the people were concerned—which wasn't really his direct job at all. In addition to that, he had to see that technically everything was done properly and sensibly and deal with all of the high-power people up above, which he did excellently and maintained a position of great influence with Groves, when most of the other directors didn't get along with Groves. If one knew Robert Oppenheimer and knew Groves, it would be hard to think of two people who were more dissimilar. The directors of the laboratories who were more nearly people who you'd think could deal easily with Groves were precisely the ones who didn't. I don't pretend to understand that, but that's just a plain fact. I think Groves sensed in the beginning that it was going to take somebody with Robert Oppenheimer's broad range of capabilities to be the director of that laboratory, and on that subject he was right.

In addition, which I did not know then, he [Oppenheimer] had very serious problems with clearance. They were bringing up situations of some of his friends who were in trouble with the security people and so on. He had a number of former students who were having trouble with security. It was a messy situation. There had been a fair number of active Communists in Berkeley. Some of the people who came to Los Alamos turned out subsequently to have been, in the past, active members of the Communist Party. In fact, the British brought over one fellow who not only had been an active Communist but was currently an active one and started feeding information back to the Russians from Los Alamos during the war. He was part of the British mission, but this was not found out until well after the end of the war, as far as I know.

TERRALL: Who was that?

BACHER: Klaus Fuchs. And in fact, if you'll remind me later, I'll explain to you that I worked rather closely with him. He helped us on one of the very touchy parts of the bomb and gave us very good help.

Well, there were many jobs that had to get done. We had to find out the nuclear properties of natural uranium and uranium-235. In August of 1943, Robert Oppenheimer asked me if I'd go on a trip with him to Berkeley, stopping on the way at Caltech. He wanted me to spend a couple of days in Berkeley going into detail on the work they were doing on separating isotopes, and see what I thought of the possibility of our ever getting any separated uranium-235 from what I saw of what they did with the best machine they'd been able to make. If it was going to work, it would have to be made in quantities hundreds of times as great as what they had.

At any rate, we did come over here, stopped and recruited a couple of very good people from Pasadena, spent an evening with the [Charles C.] Lauritsens, and then went on under a different set of names. I had to keep a list in my pocket to find out who I was. Then we got up to Berkeley, and I spent two days going over the laboratory. I must say that I was not fully convinced, but I was impressed by the results they were able to show. But what I found even more exciting was that sitting on the shelf up there among things they'd collected over the months, I found some small partially separated samples of uranium-235, which was just exactly what we needed at Los Alamos to find out what some of the nuclear properties were. We didn't know anything about these samples—or at least I didn't, and Robert, even though he had been a member of the laboratory there for some time, didn't know they existed. It wasn't very much, but it was enough to be very useful to us. I made this known to Robert and he arranged to have the material transferred to Los Alamos. I had asked the people up there if this material was needed by them and they indicated that it was not. They had isotopic analyses and these and the samples were sent to Los Alamos by courier right away. This was a lucky find for us.

Well, my feeling about what they were doing on the separation process was that it seemed better than I had thought it might. It looked like a tremendous job, but you got the feeling that with enough effort there wasn't anything that made it impossible. But it was one of the most stupendous jobs that one would ever hear about. The first uranium for the Hiroshima bomb was separated in part by that process and in part by the other process.

TERRALL: Gaseous diffusion?

BACHER: The gaseous diffusion process. And in fact, after the war, as soon as the thing was no longer critical, this magnetic plant was shut down. It was a tour de force.

There were a lot of difficult problems that had to be determined—I started listing them. This separated uranium made it possible for us to get constants, which were absolutely important. It was sometime later when we did get some very small quantities of plutonium. The first plutonium we got came from Berkeley, where they'd made some extremely small amounts in the cyclotron; then we began to get some other plutonium. When a pilot plant at Oak Ridge came into operation, we got some of the first samples out of that. We had decided we had to make measurements on this to find the nature of the work and to check for neutron emission. This was done—very fine physics—under the direction of Emilio Segrè from Berkeley. What was found by Segrè and his group at Los Alamos was that the material produced in this pilot plant at Oak Ridge had different properties from the material we'd had before. It emitted neutrons, indicating spontaneous fission. This created a major problem and was something we wanted to be very sure of. The experiment was repeated and we waited until we got an even larger sampling of it. There was no doubt about it; this new material made in the reactor had a neutron background that the earlier material didn't have, which made it very, very difficult to make a bomb out of it.

We thought for a while that we would just assemble it a little faster. The more we found out about how sensitive this was to the amount of irradiation in the nuclear pile where it was made, the more we became convinced that it just wasn't possible to make a bomb out of it. One source of such neutrons might be impurities in the plutonium sample, and we had to satisfy ourselves that this was not the case. Indeed, we did establish this—that it was not impurities but that the neutrons were real and depended upon the greater irradiation that the material from Oak Ridge received in the reactor. This was a major blow and was not known to the people who were developing the plutonium process—we found it at Los Alamos. We were then struck with the difficult problem that with the way we were making a bomb—using the gun assembly—you couldn't use plutonium. Plutonium was valueless to us.

General Groves was kept informed of this situation and was told when we arrived at this conclusion. Shortly after these results were definite, I visited the laboratory at Chicago where the nuclear piles that produced plutonium were designed. These periodic visits dated back to the summer of 1943, when we discovered we were planning similar experiments without being aware of what the other laboratory was doing. General Groves had expected to keep communications open through Washington, but that clearly was not adequate. I asked Robert Oppenheimer if he would get permission from Groves for visits to Chicago. Robert saw that this was necessary. To my surprise, General Groves gave permission for visits, specifying that I could go. He also specified that there was no limit put on what they could tell us but that we were limited to passing nuclear properties and experimental results and methods. Later he allowed some other Los Alamos staff members to go.

When visiting Chicago, I usually talked first with Arthur Compton, the director, and then reported in a meeting of their group leaders. When I visited Chicago after we had found that the pile-produced plutonium emitted neutrons, probably from spontaneous fission of a higher isotope of plutonium, I thought Compton had already been informed of this situation. When I raised the subject and said I would report the results, he turned white. It was the first he had heard of it. He said he would have to call General Groves immediately to find out what to do, and he did so.

I could not guess why General Groves had not informed Compton, Fermi, and the others, but he had not—perhaps he hoped we would find we were wrong. After Compton reached Groves and told him how upset he was at this new information, Groves asked to speak to me. I told him this fell completely within the communication limits he had set and I thought they should know about the results and how definite they were. As a solution, Groves told me I should report this after the information meeting to about a half dozen senior laboratory members. I did just that, but it was clear to them that unless something close to a miracle happened, plutonium would not be used for bomb material. Fermi, of course, understood the situation more completely than most of the others. We were all glum.

So that caused a great crisis for the whole project. The laboratory was then very strong and was getting results very well. Actually, I think the experimental physics division, and the theoretical physics division too, were fully competent to handle all the problems they had. But it seemed almost hopeless to tackle this other problem. After all, we had previously turned it down as being essentially impossible to do on a two-year scale. Now the problem was to do what we had thought was impossible to do in two years in [what was now] one year. In fact, we didn't know how long it would take to get plutonium produced, but as nearly as we could tell, it was something a little bit more than a year. Well, the net result of this was that there was a complete reorganization of the laboratory.

We gave up on getting a faster gun. It was concluded that if plutonium were to be used successfully, we must use the implosion method. Work on implosion that Neddermeyer had started had gone ahead slowly, but it was far from indicating that it was a usable method. The laboratory was completely reorganized early in July of 1944. The experimental physics division, of which I was the head, was split up and so was the weapons division, under Captain [William] Parsons. Two new divisions were formed to replace the experimental physics division. We had some work on explosives; this was to support implosion work, or other methods where we needed explosives. This was now greatly expanded. George Kistiakowsky had been brought out [to Los Alamos] earlier, and he was a real explosives expert. An explosives division, to utilize the best explosives available and to make them at Los Alamos, was set up under Kistiakowsky, who was a chemist and a very competent scientist. The division was also to do some explosive timing work, finding out how fast various things moved as a result of the explosive, which Neddermeyer's work had shown was essential to do. They had not really tackled this problem, except to take fast pictures. That had been the approach, and some additional work had been done. They did some very good work, but it was difficult.

The other division that was set up was called G division—G stood for “gadget”; “gadget” was what we called the implosion bomb. So this was, one might say, the gadget physics division. Some of the people in this division came from the gun project, but most of them came from the experimental physics division. And it included one part of the work from the experimental physics division that I haven’t mentioned at all, which we’d started during the first year, and that was preparing to make critical assemblies of fissionable material as we collected material. Indeed, we had received enough partially enriched material to make a critical assembly of enriched material—not a bomb assembly, but a critical assembly—which was a relatively small thing that you could perhaps put in this room. I won’t go into the details of it. But we had made such a critical assembly. It was called a water boiler and had been carefully studied by Bob [Robert F.] Christy, who predicted the amount of partially enriched uranium-235 very well. This was the first critical assembly that was made with enriched material. This was all done, since it involved critical assemblies, in a special laboratory we’d built down in one of the canyons, so that if there was a critical accident down there it wouldn’t involve the whole laboratory. This work with enriched materials was transferred over into G division. The entire electronics group, on which we were going to have to depend, was transferred over, and a number of senior people were transferred over to the division, and they started work on a number of different problems. Neddermeyer, who was then working in Captain Parsons’ division, was transferred over. [Donald W.] Kerst, who had developed the betatron at Illinois, was transferred from critical assembly work. So we had very good people; it was a very strong division. There were finally ten or eleven strong groups. I must say that in the fall I wasn’t at all sure that this would work. I ran into one draft letter, when I was cleaning up things, that I guess I never sent—saying that I was just about ready to give up on whether we could get such a thing going. It was very discouraging and very slow. We had very few explosive charges to work with. We had trouble being able to measure things fast enough. But then it began to catch on. We began to get a lot of concern over whether we could make a symmetric enough implosion to make the thing work as a bomb. Of course, this was the real question. We had to do experiments that were good enough to tell this.

Finally, there came the decision that the nature of the explosive charges needed a complete redesign—and I can't describe that, but it meant essentially a whole new development. This put enormous pressure on Kistiakowsky's X division, and it greatly slowed the development work of the G division for which I was responsible. It was a very tough winter and spring. Confirming evidence finally was obtained in the course of that spring. In going back and thinking about it, I concluded that the way it ought to be stated was, three or possibly four methods of quite different nature indicated that our implosion should work. The conclusions were pretty solid. But a wholly new development in the innermost core of the bomb was required to do this. We didn't even know about this when we set up G division. This was tossed in as a last requirement. The development of this was extremely difficult. I'm not going to go into it, because it doesn't make much sense to go into it without giving the details of it. But it was something that was an integral part of the implosion bomb and that hadn't even been imagined as being necessary. When Robert came and asked us to do it in our division, he didn't even ask us to do it. He said, "Will you think about it for a month and say whether you can do it or not?" Before the end of the month, I think we knew how to do it. Actually, some of the original conceptions were thought of by two or three people independently. I won't give any names, but two or three of us thought of some of the different ideas of how to do this. And it worked, as was subsequently proved.

Well, let me say that the problem of getting ready for the test in July was pretty rough. My close friend Bainbridge was responsible for getting together the test site, and he did a wonderful job, as you would expect, thinking of all of the things that had to be done. It was a real tough job. I was, of course, present at the first explosion. In fact, I was responsible, in part, for getting the core down there. I didn't drive it from Los Alamos down to the test site in my car, but I kept the car in sight with the officers and Phil Morrison in it who were taking it down, until we got almost to the site and then I was down there to receive it when it got there. We had a little ranch house where we could work on it. In order to make things go properly, we assembled the bomb core with a duplicate of a set of equipment that had already been sent overseas, so that we weren't using anything that they wouldn't have over there. When we found we needed something, it was immediately sent over. This was just a way of checking up. This had to be done very tightly, because if you go out to some strange island, you can't say, "Well, we forgot to bring the x-y-z." So this was the way we did it. After the core assembly, I drove the car carrying the core to the test site for assembly in the rest of the bomb. We had an anxious moment in the assembly due to temperature differences, but this caused only a short delay. Holloway managed the assembly.

Fig. 1. Inside a ventilated field case, the plutonium core for the Trinity bomb arrived at the MacDonald Ranch for assembly on July 12, 1945. Caltech Archives.

Well, the bomb went off, as you know, and was even more impressive than most everybody thought it was going to be. It worked very well. But that was a very tough operation. Then we went back and then came the job of trying to get successive bombs ready. I might say that when the Hiroshima bomb went off, we had arranged for information to come back, and we started getting information from Groves and from overseas. But we had also arranged that after the Nagasaki bomb, the man who was doing the nuclear assembly, Charlie Baker, would leave the Pacific the following day and come back to give us a personal report of how things were going. We'd set this up with the highest priority, number-one transportation classification. And he turned up, but he was the only person who got out from over there, because General MacArthur shut the area down and wouldn't let anybody out. Baker got out ahead of that shutdown, so we got a report firsthand of how this was done. We sent him because he was thorough and he was very ingenious. He told me some stories that I won't bother you with on this record, but they're hilarious.

TERRALL: The two bombs were two different bombs, right?

BACHER: Two different bombs.

TERRALL: So they were being developed simultaneously.

BACHER: Simultaneously. The Hiroshima bomb was the first one delivered; it was the first one ready. We had measured all the parts of it and made the preliminary assembly in this laboratory down in the canyon. It went over first and was delivered first. I think the time for the second bomb to go was actually only three or four days afterward. We didn't know what was going to happen after the bombs were dropped. Our instructions were that as fast as material could be delivered to us, it was to be fabricated and we were to send it to the Pacific. Actually, almost a week before V-J Day [August 14, 1945], Robert Oppenheimer came running down the hall. This work on checking out the cores of the bomb was done in a room across from my office, because I was directly responsible for it, and I darn well wanted to see that I went over some of these things myself—though most of the measurements were made by experienced people in a small group. But we had just finished the check-out of another core of a bomb, and there was a car waiting out front to take it down to the Albuquerque airport and a plane was ready to fly it over to Tinian, when Robert Oppenheimer came running down the hall and said he had a hold order from Washington. Well, we knew that meant this was the end.

TERRALL: So this was after Nagasaki?

BACHER: This was after Nagasaki. I'd have to look at a calendar as to when it was.

TERRALL: So the fact that the two bombs that were used were different had to do with what material was available?

BACHER: Right. There was not sufficient material to make a second uranium bomb at that time, but more material was coming in to make plutonium bombs. That was being fabricated just as fast as it came in—measurements made on it and so on.

Finally, several days after Oppenheimer had received the hold order, he arranged that the two of us would go to Washington. We learned about V-J Day in western Kansas. I should say that Rabi was with us, too, in Kansas. He was on his way back to Boston. He had been out for the test. Instead of the celebration that went on among other people on the train, we suddenly began to realize how tired we were. I think I slept twelve hours that night. We arrived in Washington on the day that really was V-J Day. General Groves was in his office. Robert Oppenheimer and I stayed there for several days, trying to figure out what to do. I don't think a whole lot had been arranged about this. He very nicely arranged for an airplane to fly us up to Schenectady to see some particle physics experiments we were curious about, and that was very pleasant. But the problem of bringing the project to a close was a very difficult one, because the employers of a large fraction of the people had been promised that they would come home as soon as possible.

TERRALL: So this is what Oppenheimer was worried about at that time; that's why he wanted to go to Washington?

BACHER: Yes. He knew that the place [Los Alamos] couldn't just go on. And furthermore, he had every intention of leaving. This was a very difficult time. On the other hand, we had to see the thing through and see the end of things. It was difficult for me for a number of reasons, too. Groves wanted me to stay there, and I wanted very much to get back to academic work.

TERRALL: He wanted you to stay on at Los Alamos after the war?

BACHER: Yes, and I thought it was better to do something else. I think I'll skip any more things about Los Alamos and postwar problems of the laboratory, which were really very difficult.

We began to think almost immediately about whether we could get some international control of atomic energy. Some of the people from the Chicago laboratory, after the plutonium work more or less got out of their hands and they were through with their job, were already thinking about this. And a fair number of people at Los Alamos started to think about it. General Groves immediately took this up, too, in a different way; he wanted to get information as to whether it was technically feasible to have international control—was it something that fitted in with what you could do? He set up a committee to work on this problem, to see whether international control was technically feasible, or was [the bomb] something that, even if you had full control, could be developed in somebody's backyard or something like that. I was involved in this and spent six weeks or so working on it.

Fig. 2. Robert Bacher receiving the Medal for Merit from Lt. Gen. Leslie Groves in 1946.
Caltech Archives.

This kept us on somewhat later than we otherwise would have stayed at Los Alamos. But Oppenheimer, as I recall, left Los Alamos about the middle of October. We finally decided, when I was involved in this work, that we'd stay on until after Christmas. Then, shortly after Christmas, Jean's father died suddenly in Ann Arbor. She left almost immediately with the children and went back by train. We had then already arranged about moving our possessions out and sending them back to Ithaca. We'd arranged by telephone to buy a house in Ithaca; good friends of ours who were there were helping to search. I don't know just when that was done, but at any rate we finally got back there. I followed, I guess, about two weeks later, driving our car, and stopped in Ann Arbor to visit her mother. Jean and the children stayed there for a while. Then we went back to Ithaca and Cornell.

From Los Alamos, several new faculty members came back to Ithaca—[Richard P.] Feynman, who's here now; Corson, who later became dean of the engineering school and subsequently president of Cornell; Phil Morrison, who's now at MIT; and McDaniel, whom I mentioned before and who is now the director of the laboratory we started at the end of the war. It's a fine laboratory. After I left—which I'll come to in due course—Bob [Robert R.] Wilson came there and did a fine job of building up the work in this area. Later he left and went to the Fermi National Laboratory. In addition, there were Hans Bethe, Lyman Parratt, [Trevor R.] Cuykendall and I—[all of us] had been at Los Alamos but had been at Cornell before. So you see, of people who had some responsibility, there were eight of us that went back to Cornell from out there. There may even have been more whom I don't remember at the moment.

So now we come to Ithaca after the war.

ROBERT F. BACHER

SESSION 5

July 15, 1981

Begin Tape 5, Side 1

BACHER: I came back to Ithaca in early January [1946]. The problem of getting a house was something, which I skipped over previously. One of our close friends, Franklin Long, had gone back to Ithaca earlier. We put an advertisement in the paper for a house, and we called him each night at about six o'clock. By that time there'd been some answers and he'd had a chance to check on them. We actually bought a house without seeing it, but it was across the street from some close friends of ours, so we knew about where it was. One of the funnier things that happened to me was that I had to go up from New York to clear this up on a day in early January. I had to sit up all night on the train to do it, because there weren't any Pullmans. I arrived in the morning and immediately stopped off at the bank where I'd kept an account all during the war. I talked to the vice-president there and he said, "Well, now we're arranging about the mortgage; how do you like the house?" And I said, "Well, as a matter of fact, I haven't seen it yet." He said, "You mean you're here to sign the papers on it without seeing it?" And I said, "Certainly. As far as we're concerned, we don't have any other place to live, so this is going to be it." Well, this practically floored him—that I'd stop on the way up to settle about buying a house. But I was determined that that was where we were going to stay. As a matter of fact, it turned out pretty well. The house wasn't really a very suitable house, not nearly as nice as the house we'd had before the war, but it was what we could get, because houses were short at the end of the war.

Bethe and I had returned to Cornell with the understanding that nuclear physics would be supported both in experimental work and in theoretical work. It was also agreed that Cornell would move from low-energy nuclear physics to high-energy. We settled on a program of use of an electron synchrotron of about 300 million electron volts, with the idea that we'd use the bremsstrahlung, or radiation, which is produced when the electrons collide with the target and thus produce photons, to study nucleon structure. This was a new field, analogous to the field that had previously used very high-energy gamma rays. We set out to build a synchrotron that would have an energy of 300 million volts, which was higher than any other synchrotron that was then being built.

From February on, the plans for a new laboratory were developed. Nat [Nathaniel] Owings of Skidmore, Owings and Merrill was the architect. And we quickly found that our objectives were very close. A good location near the old physics laboratory, Rockefeller Hall, was found and subsequently named Newman Laboratory [for Elementary Particle Physics], which still exists. It's still a good laboratory and the director of the high-energy physics work now is Boyce McDaniel, who took his degree with me during the war and who was at Los Alamos. For many years, Robert Wilson, who was also at Los Alamos, was director of the laboratory. Boyce McDaniel was there with him after I left and succeeded him as director when Wilson went as director to the Fermilab.

During the spring of 1946, Bethe and I had offices in Rockefeller Hall, which was the old hall that had been there since 1900 or thereabouts. There was much activity about the need for international control of atomic energy—its feasibility, and the objectives of controlling atomic weapons. We were very much interested in this subject. Robert Oppenheimer was involved in the production of a report by a group headed by David Lilienthal, which worked under a government committee of which Dean Acheson, who was the secretary of state, was chairman. I saw much of Robert Oppenheimer, who had a major influence on that report, called usually the Acheson-Lilienthal Report. It had a major influence on United States policy. Subsequently, an international atomic energy committee was established under the United Nations [United Nations Atomic Energy Commission] to consider a proposal by the U.S. to have international control of atomic energy, along the lines of but not identical with the Acheson-Lilienthal Report. The head of the U.S. delegation was Bernard Baruch, and several of his close associates were involved. John Hancock was the delegation manager, and Richard Tolman, from Caltech moved there full-time as scientific advisor. He set up an advisory committee; I've forgotten who all the members are. I do remember that Robert Oppenheimer was a member and I was a member. And I'm certain that if we looked hard enough, we could probably find who the other members were.

TERRALL: This was the advisory committee to the international…?

BACHER: Yes, this was the scientific advisory committee to the United States delegation to these international negotiations. I have a feeling that Dr. [Harold] Urey was an advisor, and I don't know, maybe some others.

I was present at the opening meeting of this international atomic energy committee, in June 1946. It was a very impressive meeting. Baruch essentially gave a presentation [on] the United States proposal to have international control of atomic energy. This was a major position to take. It was a very exciting time. What happened after that was that it wasn't quite clear that science and technology were going to play any immediate role in this. In July we went up to the Adirondacks for a vacation—we hadn't had a vacation in quite a while. While I was up there, I read a number of things and thought about this quite a bit, realizing that it was a much more important time than any I'd seen before, because getting international control of atomic energy and weapons was crucial.

Well, as soon as I came down from the Adirondacks and back to Cornell, I immediately called Tolman in New York to find out what was going on, because what I'd found before that was what I could pick up by reading the *New York Times*, which after all is a good paper but didn't have a whole lot in it about this. He told me that things were getting very hot on the technical front. Having kicked the subject around from the heads of the delegations to a political committee to several other committees, they'd fallen on the fact that, after all, nobody knew whether it was technically feasible to have international control of atomic energy. And since they didn't know what to do, the idea was that they'd kick it off into a technical committee and see what could be done. The Soviets were very much interested in this, to see what they could dig out by way of extra information. And of course at that time we didn't know very much about how much information they had secured from espionage. We were aware that through the Canadian project they had achieved some, but the full extent of espionage was not apparent until several years later.

TERRALL: What was the Russian response to the opening moves of establishing an international agency? Was it just wait and see what happens?

BACHER: Principally their response was, "Well, we ought to talk about it. Before we agree on anything, we've got to talk about it and think about it." They quite clearly didn't want to do this, and I think one of the reasons was that this essentially took out of their own hands the construction of weapons. I think, in retrospect, they had already been working at it for two or three years and were well along. They didn't much agree on any of this, [but] they were always very careful not to say "No" categorically.

After calling Tolman in New York, I found that he was just swamped with work. He begged me to come down and spend some time with him, so I picked up and went down there. I found that the situation was much worse than I had expected, and the pressures on him were simply tremendous. The result was that I started going down on a regular basis, because very quickly a technical subgroup took over. The chairman of that subgroup was Kramers, the Dutchman—Hans [Hendrik A.] Kramers. I knew Kramers, but not very well before these negotiations. There was a representative from Canada—and I've forgotten where else the representatives came from. Tolman was the representative on the technical committee, and I served as his backup man. He wanted me to take a major part in the nonformal negotiations. I spent a lot of time with other members of the delegations, especially with Kramers, trying to see what we could do to move ahead. The meetings went on at great length and were recorded—I guess by this time I've destroyed my copies of this.

Well, this was a major undertaking in New York. We had to go through all aspects of the problem of control. A number of papers had been written up, setting forth what had been done in the United States and explaining the fundamentals of the project and so on, and these were very useful. But the Soviets had lots of questions. I don't think we appreciated, when we first started in on this, that this was, from their standpoint, a matter of digging for information, and that they didn't have any intention of making any formal agreement. Actually, we pushed very hard on the question that was thrown to the technical committee: "Is it technically possible and feasible to have an international control of atomic energy?" This was really the question that came before the political committee. So our objective was to demonstrate that while there might be some things we didn't know fully, we thought it was technically feasible. There had been quite a lot of studies made by the Manhattan District and others to obtain this information and come to this conclusion. We had quite a lot of information on this.

The problem came to a crux when we came down at the end to face this question. The Soviets, shortly before a meeting when we were going to consider this question, asked for a postponement to the next day. And if I remember correctly, just before the next day's meeting, they asked for a postponement again, which was granted. But after that postponement, I went to see Kramers. While we didn't want to do anything officially, I told Kramers that it had been decided in our delegation that if the Soviets asked for another postponement, we were going to insist on the meeting whether they came or not. This would, of course, have been quite a blow to them in many ways, because they'd have participated in it and then refused to come to discuss the question. So this really put the bee on them very hard.

TERRALL: This is still just the technical committee?

BACHER: Just the technical question that I stated before. Well, of course, Kramers immediately got the word back and said, "If you don't come to the meeting, they're going to insist on a meeting anyway, so you can't do that again." And everybody else agreed, but it was really essentially between us and the Soviets to figure out. Well, [Dmitrii] Skobeltzyn, who was the Russian representative, came to the meeting. The thing was then written up in the form of a declaration, along the lines I've indicated, and he agreed to sign it. So it was approved. And as far as I know, this was the only substantive thing that was actually agreed on in those negotiations for a long time. There were some minor things that were agreed on, but this was a substantive thing, having to do with what you could do internationally. We thought it was really pretty good. We were a little surprised when, within a few hours, we had a message from Skobeltzyn saying goodbye, that he'd been called back to the Soviet Union. We sent some flowers and things to the boat. We got a very nice thank-you note back from Paris, where he'd stopped off for some time. The Soviet Union withdrew him that day, I don't know whether in pique over what he'd done. I don't think so; I don't think I've ever quite understood.

TERRALL: You never found out what happened to him?

BACHER: Oh, yes. I saw him subsequently on a technical exchange visit to the U.S.S.R. in the early sixties. But he was called back, and he stopped off in Paris, and they put somebody else in as delegation head. But in effect, the scientific subcommittee had answered the question that had been put to it. They then had to face the political problems. They had received a unanimous technical answer from their committee—that it was deemed technically feasible to have international control of atomic energy.

TERRALL: So they could no longer say, “Well, we can’t decide this because we don’t know whether it’s technically feasible.”

BACHER: Correct. That was the sole reason for doing this. It was a lot of work.

TERRALL: I was wondering, in reading this Acheson-Lilienthal Report, what your assessment of it is in retrospect. It seems, from a 1981 standpoint, just terrifically idealistic—to think this could ever happen.

BACHER: Well, I think that’s true; I think it *is* very idealistic. However, the alternatives were rather severe. One alternative was for us to take a belligerent attitude before anybody else had nuclear weapons, and the other was to do what we finally did do, and that was just let it drag. That has certainly not been a very satisfactory solution. My feeling is that, sure, it was idealistic, but it would have avoided a very, very terrible situation that the world now faces and which is particularly acute at the present time.

TERRALL: I was wondering whether in 1946 it just wasn’t that clear what a hard line the Soviets would take on something like this—whether maybe there was still time to get them to agree to something like this.

BACHER: Well, I don’t know quite how to answer that. I don’t think the country would have moved in this direction if it had been thought that there was zero chance that the Soviets would agree. I’ve had some difficulty getting a proper perspective on what the end of the war was like. I’ve found that I’ve been able to get the best perspective as to what it actually was when I talk to some of the military people who were out in the Pacific, getting ready to attack some of the islands and eventually the mainland there. When they heard about the atomic weapons, they just went right through the roof. I think people forget that there was an estimation that we were going to have something like a million casualties going into Japan. And being released from that, the military forces and weapons in the United States immediately went from that to being a postwar subject. Some years later, when I was on the [U.S.] Atomic Energy Commission, we held the first weapons tests that the commission conducted out in the Pacific. I was going to mention them later; but let me say just this little word about it. When they went to the island of Eniwetok, where these tests were done, they came into the U.S. facilities that had been abandoned there at the end of the war. Our forces had left so fast that the tables were still set. People just walked out. When we went to that island, they had so many new trucks piled up there that the first thing we did was send in some cranes and pile them three-high so we could get stuff in on one of the islands to work with—new trucks, taken over there to be shipped off somewhere and simply left. Some of them still had air in the tires.

TERRALL: Let me ask you one more thing about Tolman. You mentioned that you worked very hard with him on this. We hadn't really talked about your getting to know him. You had gotten to know him through the war?

BACHER: I'd gotten to know Tolman through the war, at Los Alamos, because he was the principal liaison between Los Alamos and the NDRC in Washington. While we were directly under Groves, the NDRC kept in very close touch. Conant was the head of the civilian establishment that kept a contact and Tolman was his righthand man. But in point of fact, I really got to know Tolman best during those two months in New York, because we spent every weekday together for a period of two months, working as hard as we could on some of these things. There were quite a lot of very interesting people involved in those negotiations, and it was a very good experience for me, quite different from anything I'd ever done, and I enjoyed it. But it was certainly a very wearing and threatening affair.

While the talks were winding down, the [U.S.] Atomic Energy Commission was being set up in Washington. I don't know just when it was, but toward the end of October 1946, while I was again on a visit with Tolman in New York—I was no longer spending anything like that amount of time down there—I received a call from David Lilienthal, who requested that I come to Washington to discuss my possible membership in the Atomic Energy Commission. Well, I said immediately on the phone that I was extremely dubious about this, that I was deeply tied to Cornell. I wanted very much to get back to my normal career, and I really didn't want to do this. But he was insistent that I at least come down and talk to him about it, and I thought that I couldn't just say no like that. So he sent a government plane up to New York for me, and he wanted me to come down right away, so I did. In Washington I found it very difficult, because what I found was that Clark Clifford, who was the first assistant to President Truman, was taking charge of assembling this commission. David Lilienthal had agreed to be chairman of the commission, and they had three others who had agreed to be members. I think they saw that I was sensitive as to whether there was a scientist on the commission. In fact I was told directly that if I didn't take a position on the commission, they weren't going to have a scientist on the commission. Well, that was a pretty rough way to twist my arm, because I'd practically told them I didn't want to do it. I said I'd have to think about that and consult my family about it. But I said, "There's no doubt in my mind, and you know this, that I think there ought to be a scientist on this Atomic Energy Commission." And of the other four members who'd agreed to serve, none were scientists.

The other people were Sumner Pike, who had been a member of some other commission in Washington—I can't remember just which one it was [the Securities and Exchange Commission—Ed.]—but he had Washington experience and had been in the oil business before then and was quite a wealthy man, from Lubec, Maine. Then there was Lewis Strauss, who later was prominent in the Oppenheimer affair and chairman of the Atomic Energy Commission. And then there was William Waymack, who was the former editor of the *Des Moines Register*.

TERRALL: How did he get on?

BACHER: I think it was probably suggested to get him on because he was a very, very savvy individual. The *Des Moines Register* at that time was one of the really objective papers in the Middle West, and possibly Dave Lilienthal knew him and wanted somebody there who could sort of be the conscience of the commission. Oddly enough, I think he turned out to learn more about atomic energy than anybody else among the other members of the commission. He learned an awful lot about it, with absolutely no background in the field at all. All of the commissioners were very bright; it was a very intelligent group.

Well, this whole proposal hit me pretty hard, and I said I'd have to talk with my family in Cornell. I went home and talked to them about it. I did not ask the people in Washington, as I presume I could have, to call and put pressure on the Cornell people when I accepted. I rather went and told the President I didn't think it was fair to do that, and that I'd just have to take my chances at what I could do afterward. But I did go. This was a very hard thing for me to do, because I'd just come back to Cornell. We were building a new laboratory. We'd just barely gotten started. I hated to leave, I really did. However, if I hadn't left, I never would have been out here. I guess it was Ralph [William R.] Smythe who once asked me, when I first came out here, "Well, if you hadn't gone to Washington, do you think you would have come to Pasadena?" And I said, "Well, I don't know. I guess I've never really faced that. But I think the chances are certainly a little against it."

TERRALL: Didn't you have a job offer from here as well as from Cornell after the war?

BACHER: Well, the situation's a little different from that. In the spring of 1946, I was the Cornell representative on a planning committee for what became the Brookhaven National Laboratory—this is going back a bit from what I've been talking about. Lee DuBridge was the Rochester representative and the chairman of this planning committee, and I was the Cornell representative. There was quite a struggle over whether the group in New York or the group in and around Boston would be closest to the center of where this might be. The universities involved at this time were, I believe, all members of this group, and they still are the members.

TERRALL: So they hadn't decided where it was going to be located?

BACHER: Well, it hadn't really been decided that there was going to *be* a laboratory. This was a committee to look into whether there should be a laboratory in which so many universities in the East were involved, and if there was a laboratory, where it should be. They were given some encouragement in this by the Manhattan District. And as I say, Lee DuBridge was the chairman of the planning committee. He and I were in a somewhat more independent position—Harvard and MIT wanting something up in that area, Columbia and Princeton preferring this area near New York, Yale being a little ambivalent, Johns Hopkins thinking about having it closer to them. And then Cornell and Rochester being sufficiently far away that they knew it wasn't going to be in their backyards. So we were in the position of being the ones who didn't have a major choice between Boston and New York as to where it would go. But there were stronger reasons for having it in the New York area than in the Boston area, except in the minds of the people from Boston.

All this planning was going on that spring, in addition to other things we were getting started in Cornell. During the spring, Lee DuBridge accepted the offer to become the new Caltech president. He immediately resigned as chairman of this planning committee, and I succeeded him and stayed as a member and worked on the site selection, and so on, through the early stages of the planning. This committee functioned to get the thing operating. The Manhattan District was very good in wanting to get something like this started and then providing funds to do the planning.

At that time, Lee DuBridge also approached me about coming to Caltech, and I visited out here in September or October, I'm not quite sure just when it was. I guess I was DuBridge's first visitor in the President's House they had before the present house became the President's House. I pointed out to Lee that it just wasn't possible for me to accept a position out here, because of the Cornell plans and my participation in them—that I couldn't think of going to another university.

TERRALL: This was before you'd been approached by Lilienthal?

BACHER: Oh, yes, this was before I was approached by Lilienthal. It's all around the same time, but I had no idea then of any participation at all on the Atomic Energy Commission. And I think if it hadn't been for the Washington pressure on me, I probably would have stayed permanently in Cornell. It was the Washington pressure that dislodged me from Cornell, even though I was at first very reluctant to go to Washington. And I must say it was very sad for me—sad for both of us and our children, too, with close friends there and colleagues with whom we still have close relations—to leave Cornell. This hit us pretty hard; but we went to Washington.

The Atomic Energy Commission started in early November; and this was a major undertaking. It was also a major personal undertaking, because there was the problem of how did I move my family down there. Jean came down and spent a little time down there. We did some looking around and finally found a place to live, though finding places to live was very tough in Washington. It was extremely difficult for Jean, with children of ages ten and eight, to pick up and go down to Washington, but she did, and came down there after the Christmas holiday. It was also very difficult on the children. I think it was not good for their educational experience, but that's what happened.

Going back to the commission itself, one of the first things we did was take a trip around some of the [nuclear] facilities. We visited Oak Ridge and Los Alamos and Berkeley and Chicago, and did a lot of looking around Washington at the various things connected with atomic energy. And later, Sumner Pike and I took a further trip, in which we revisited some of these places—especially Los Alamos—and went to Hanford for a rather considerable visit. The thing I remember about that trip more than anything else was that we were in an air force plane and we got up in some ice over the Rockies. We got to the point where they were revving the propellers on the plane every thirty seconds to shake the ice off. When we got down on the ground, we all looked at each other and Sumner Pike said, "Well, they almost had to put in a new commission, didn't they?" It was a pretty rough flight.

There were problems everywhere, especially at Los Alamos. They had a dedicated director in Norris Bradbury, but the personnel had been decimated at the end of the war. As soon as it was clear that there was going to be a commission, they found it somewhat easier to recruit people, because the plans had been in chaos for a year or so. But there were problems everywhere. The fact was that the art of making a critical part of the bomb had almost been lost. No successful one of these critical parts had been made in several months, and we found that out when we went out there in early November. This was a real emergency, because the government was going around acting internationally as if we had many atomic bombs, and so on, and here the situation was that we'd almost lost the art of making one of the critical parts of it.

Begin Tape 5, Side 2

TERRALL: So the commission was supposed to be overseeing the actual production of more bombs.

BACHER: The commission was supposed to be overseeing the production of fissionable material, research in nuclear physics—everything that pertained to atomic energy. The [Atomic Energy] Act stated specifically that this included a fair amount of research in this area as well as the responsibility for getting fissionable material and developing nuclear energy in whatever way we thought it ought to develop, and particularly the question of seeing to it that the country was adequately protected with nuclear weapons.

TERRALL: So whose job was it to decide what was adequate protection?

BACHER: Well, let's put it this way. The way that that was initially decided was there was a certain amount of material from which to make weapons, and all of the material was devoted to making weapons, period.

TERRALL: But this was one of your first problems.

BACHER: This was one of our first problems. The Los Alamos lab had a really tough time of this, but they turned this situation around. When they saw what a vital thing the atomic bombs were, the difficulties were overcome. The art wasn't lost, and we soon had an operating production of nuclear weapons from the materials we had available. This was one of the things that turned up in the inventory that I subsequently made out there, as to just where we stood. This was a real emergency.

Also, there'd been trouble at Hanford. The reactors—or piles, as they were called then—that produced plutonium were operating at very reduced power. The [amount of] plutonium that's produced depends upon the power at which the reactors operate, so this meant that production of plutonium was reduced. As for the electromagnetic separation production at Oak Ridge, while it had been critical during the war in producing our first weapon, the fact was that when the diffusion plant was operating, the electromagnetic separation plant just couldn't compete with it. So even before the AEC had taken over, the Manhattan District had tentatively shut it down, though not in a way in which it couldn't be reactivated. But it was perfectly clear that unless something happened and the diffusion plant failed to work, electromagnetic separation, as it had been done during the war, was finished. They tried to get all of the material out of it that they could, but it was shut down.

The diffusion plant was going very well and doing better every day. While they had had difficulty in getting full enrichment out of the diffusion plant during the war—and no final product out of the diffusion plant had been available during the war—the production started to go very well. Those diffusion plants have been absolutely amazing; they've worked all these years and are still working.

To get back to the AEC a moment, my fellow commissioners were very experienced in various ways. They were, most of them, knowledgeable of government, and without exception intelligent and hardworking. We spent hundreds of hours together; problems were discussed very thoroughly. The commission worked through a staff under a general manager who initially—and in fact all the time I was in Washington—was Carroll Wilson. He had worked at the National Defense Research Committee very successfully during the war. For me, it was an entirely new experience and an opportunity to learn a lot. The other side of it was that I was the only one on the commission who was experienced in the scientific and technical side of the atomic project, and it was a real test to explain situations accurately without getting too technical. My colleagues didn't miss anything, and they always had a lot of questions. But I felt as time went on that it was vital that on the level of the commission there be someone with a scientific and technical background—and I'll come back to that later.

Early in the commission's existence came the hearings for Senate confirmation. They were very much interested in Lilienthal; no one cared much about the rest of us. I guess that's not true. The reason they spent so much time on Lilienthal was that Senator [Kenneth D.] McKellar from Tennessee had made a point of attacking Lilienthal at every opportunity while he was at the TVA [Tennessee Valley Authority], and he took [this opportunity] to continue the attack, through a series of hearings that went on for weeks. Probably erroneously, we had the idea that while Lilienthal was down there the entire commission ought to be there, too, if physically possible. We didn't hesitate to send somebody out someplace if we had to, but if the commissioners were in Washington, they went to the hearings. We took it very seriously. This became a real impediment to our work.

In spite of all this, plans were developed for many parts of the atomic project. The first thing was to strengthen the Los Alamos laboratory and get work started on weapons design. New designs were aimed at a series of tests at Eniwetok in the Marshall Islands. We saw that we could not get these immediately; we tried for 1948. The reason for tests was not to find out whether we could make bombs or not—we knew we could make bombs. The problem was to try to make better bombs, to get more explosion out of less material and make different size weapons, and so on.

The laboratory at Los Alamos gained strength rapidly after the commission was appointed. As I said, quite a few members of the laboratory had stayed on with Norris Bradbury when he succeeded Oppenheimer as director in mid-October 1945. The prospect of the commission being appointed was a great stimulus to Los Alamos and by the time the commission took over, on January 1, ’47, [the lab] was in much better shape than it had been sometime earlier.

At the time of the takeover, the commission decided that an inventory of fissionable material, in weapons form and otherwise, should be made, and they sent me to Los Alamos to do it. This may have been the first real physical inventory—actually checking selected fabricated parts to see if they were plutonium and enriched uranium. I did this myself, with Colonel Gee, who was the head of the place for the Manhattan District; Norris Bradbury, who was the director of the laboratory; one or two of my close associates during the war who’d stayed on at the laboratory and who were nuclear physicists by training. About a half dozen of us went through the great safe—which was as big as several rooms this size—where material was stored. We spent two days doing this. The way it was done was that I’d go in and point at a vault up here and say, “Let’s look at that one.” And they’d dig out the card that said what was in it, and down it would come. And we’d open up and see what it was. I had obtained some particle counters with the help of the electronics laboratory and Darol Froman, who was my successor at Los Alamos. We took it down, and if it was uranium I had a counter there so I could tell the difference between a normal piece of uranium and the uranium-235; the activity was quite different and I could measure this. I could see the things and compare them and know that they were uranium. They had about the same density and so on, so it was a reasonably good check.

Plutonium, of course, is a totally different thing. You could count the alpha particles from plutonium, but plutonium is so rich in alpha-particle emissions that you’d get a terrible burn if you handled it. So plutonium is always coated with something, and we generally coated it at that time with a covering of indium, which was enough to stop the alpha particles. Not only is plutonium extremely dense—it’s denser than uranium, which is one of the densest metals—but it also has the unique property that if you take a good big chunk of it and hold it in your hand, it gets hot. It will get so hot you can’t hold it. So it’s commonly kept in some sort of a container that will conduct the heat away. It’s kept separately, and you don’t pile pieces together, because you’d get a critical mass if you did that. So it’s stored very carefully in containers that can’t allow you to get too close, no matter how anybody would manage it. I simply tested the plutonium by looking at what it looked like, hefting it, compared to a chunk of uranium of about the same size, and just holding it there to see if it got hot in my hand. It’s a perfectly simple thing to do—nothing else in similar quantity does that. We checked a significant fraction of the material in the vaults during those two days. I don't think there'd been a physical check of the fissionable material made before this by the Manhattan District. They had cards with all of this, but we were actually making a check, saying, "Bring this down." And when we finished it, I got everyone in this group to sign, including the colonel and the director and everybody else, and I signed it, too. And we inventoried some of the things where they had materials in solution, but we could only do this on a spot basis.

I inevitably had to be there on New Year's Eve and left the next morning to go back to Washington and got into a terrible snowstorm. We were grounded in Oklahoma City. I missed the first General Advisory Committee meeting, which was being held in Washington on the third and fourth of January of that year [1947]. Speaking of the General Advisory Committee, this was one of the things that the commission had done during November and December. There was a provision in the law for a General Advisory Committee to be appointed by the President on the recommendation of the commission. I had a major part in recommending the members of the first committee, which was a very distinguished committee. They selected their chairman, who was Robert Oppenheimer. He served for a period of six years. Lee DuBridge was a member of that committee; Rabi, from Columbia was a member; Cyril Smith, who was a metallurgist from Chicago; Conant was a member; and so on.

TERRALL: Was there any discussion at all at this time about whether bombs should just be built indefinitely? In other words, you knew what one bomb could do. Was there any question about stockpiling indefinitely?

BACHER: Well, that's a little oversimplified. We knew what one bomb of elementary design could do, but never at any time did someone say, "Well, if you have ten of these, you have enough." The idea was that quite a stock was needed, and the position of the military services at that time was that they needed an arsenal of these things. If we could make weapons either more numerous or more effective, this was something that was absolutely first priority as far as the military people were concerned. Remember, the Atomic Energy Commission had a Military Liaison Committee. This was a committee of people who were knowledgeable; in fact, General Groves was a member of that Military Liaison Committee. So they had people on that committee who had had experience in the Manhattan Project, so they had a very good background. They were convinced that the strength of the military was essentially to have the atomic bomb, and there was no idea that, "Oh, we have five of these, and that's all we'd ever need." That approach never came up.

TERRALL: Not on the commission, either?

BACHER: Well, that wasn't our job. Our job was not to set international policy. We were not a body recommending political positions on this; we were a body responsible for the operation of the Atomic Energy Commission along lines that were determined by President Truman. Remember, we reported to the President; the President had perfectly clear ideas of what he wanted done. It was perfectly clear that if we came in and made recommendations—as we did subsequently about a reactor development program, in which material would be used to make the reactor and would not go into weapons—that was something we could decide. We'd have to explain it to somebody, but if we thought it ought to be done, it could be done. But the idea of just stopping making plutonium was something that would not have conformed with the directives we had from the President as to what we ought to be doing. In fact, we were concerned about getting a larger production out of the facilities we had. Our primary aim at that moment was not to increase the production of fissionable materials, but that came rather rapidly afterward. Within the first five years of the commission, they started an enormous building plan, which stemmed from the time it was discovered that the Russians had exploded a bomb, which I'll come to in due course.

TERRALL: I wanted to ask you about working with nonscientists on a commission like this. You mentioned that you learned a lot from it. Were there problems in explaining things, or feeling that people weren't giving the right balance to questions because they didn't have the technical background?

BACHER: Well, remember that the job of running an Atomic Energy Commission is as much or more a question of national policy as it is of technical policy. The technical problems are only part of it. Indeed, it was the idea at first not to have a scientist on the commission. I felt very strongly from the beginning that it was very important to have scientists on the commission. The commission, as long as it existed, always had at least one scientist; sometimes it had two. For some of the basic problems that the commission had in the earliest days, you needed a scientific background, and you needed to understand about the technical problems, but most of the problems we had to solve were not technical problems.

TERRALL: So as far as the way the commission actually worked—the dynamics of how people worked together—this wasn't a problem, your being the only one with the technical background?

BACHER: No. And indeed, it went further than that, because the commission had also this General Advisory Committee, which was primarily scientific and technical. The bulk of the people on the General Advisory Committee had a technical background and knew quite a lot about this subject. They knew details of the subject that I didn't know anything about, in many cases. The commission was at liberty to pose all sorts of policy and technical questions to the General Advisory Committee, and they could call on anybody in the country—and, of course, the commission could call on anybody in the country, too—to come and offer us advice. But that isn't the problem. The problem is: What do you need in day-to-day working? When the General Advisory Committee is not there, and when you can't say, "Oh, in a minute I have to call up so-and-so to find this out," you do have to have a certain working wherewithal, to know whether something's critical or it isn't critical. The worst thing a commission like that could do would be to get started on a program or a policy that was incompatible with the scientific facts. That would have been a tragedy, and that we didn't do.

But this was something entirely new. At that particular time, the commission had enormous prestige in Washington. When we went to the Joint Congressional Committee to brief them on what we had in our weapons stockpile, at first they didn't want to hear it, so we told them we had the information if they wanted it, and they picked a small delegation to hear it. We briefed the President, too. I was reading something David Lilienthal wrote recently about the first briefing of the President, and I think he [Lilienthal] had some of it wrong. What happened was in our briefing the President for the first time on what we had in stock—and this was sometime in the early spring of our first year [1947]; I have a copy of this somewhere—we didn't even write down on the briefing to the President the numbers that went in there. We took a report over to the President and he sat and read the report. As a matter of fact, the first time we went over, I read the report to the President myself, and when we came to the point where it said how many of such-and-such a thing there were, I put the numbers in from memory—they weren't written down anywhere.

TERRALL: So when you say the Joint [Congressional] Committee didn't want to hear the numbers, you mean because of security?

BACHER: They were scared to death to know the number of bombs we had. Subsequently, there was a rather serious break in security by a member of the Joint Committee who didn't even realize at the time, when he was talking on the radio, that he was saying something he shouldn't. This was what they were originally afraid of.

Through that period of the AEC—and I don't think it's profitable to go into this at great length—we had the problem of getting fissionable material produced more effectively, better bombs developed, tests of new types of weapons. And we had the problem of building up personnel in the various laboratories and getting proper management for the laboratories, where in some cases the contractors that had handled it during the war wanted to be relieved. So it was an enormous job. The budget of the Atomic Energy Commission was several hundred million dollars a year, and that was over thirty-five years ago, and that was a lot of money then. It was a significant fraction of the total budget, and one of the biggest single items.

In fact, one of the first things we had to do that spring was get ready to defend the budget. Of course, we had a budget office, and they were going to put things in and so on. I looked over their outline and said, "But you haven't said anything about scientific work in here." "Oh," they said, "we don't think that really ought to be in there, and anyway we can't defend it very well." I said, "What do you mean you can't defend it very well? We've got to tell them, we are directed in the Act"—and I read it to them—"to do scientific work. We're going to have an increasing budget. We're going to start right from the beginning to defend that budget on the basis of our directive. This is something I'm going to insist on." Well, I never even had to take it to the commission. I just promised, "I'll defend the scientific budget for you." So we wrote down the scientific and technical budget, and I simply went before this committee and told them that we were charged with the responsibility of doing this, that one of the problems at Los Alamos during the war was that we'd come to the limit of our basic scientific knowledge, and that if we were going to get ahead in this subject, we really had to get at it and do it a lot faster in the future than it had been done in the past. I think it took me only about fifteen minutes. They stopped for any questions; no questions; we got our budget. That set a policy, so that it was done that way afterward. One of the great advantages was that it established from the very beginning of the Atomic Energy Commission that it had a budget for scientific and technical work.

In fact, it was in the first budget that we put in several million dollars for basic research. We said, "We think basic research in some of these areas ought to get started, and we cannot, in the time that's available to us, get set up to handle it directly under the commission. We think it's important to start it, so we'd like to ask for money, which we propose to turn over to the Office of Naval Research for them to distribute through their contracts." The navy supported a certain number of contracts for support of basic research. And we actually got several million dollars. One of the things we said to them was, "We think one thing you might do is fix it so that these contracts you have are funded further ahead, because we do not believe that basic science should be funded on a one-year term. It doesn't have any meaning on that period." We turned several million dollars a year over to the navy for providing contracts. They were then supporting work in nuclear physics and in various other things. Our work in nuclear physics here at Caltech was supported by them for years.

I don't know how much to go into the operation of the AEC. We had some very good people. We persuaded James Fisk, who was subsequently the president of the Bell Telephone Laboratories, to be the first director of research. He was extremely good. He understood the problem. He'd known how to direct a big laboratory, he'd played an important part in war research, and so on. The AEC immediately got good people coming to it. The laboratories all were picking up. One of the great credits of the Atomic Energy Commission in its day was that it was able to operate civilian laboratories of very high quality. There are probably no higher-quality laboratories operated anywhere by the government than those laboratories, and this has been very, very important. Part of that traces back to the history of the laboratories during the war, when they had a very strong scientific component, and part of it traces back to the fact that great care was taken to give the laboratories a lot of leeway in terms of projects they could do. If they had ideas, it was a policy to try to give them an opportunity to explore them.

I might talk about the first series of tests in the Pacific. Those were in the spring of 1948.

We'd been working up to this for quite a while, and Los Alamos had been working at it very hard. When it came down to running these tests out there, they suddenly realized, when they talked to the lawyers, that there was no legal precedent for the disposal of fissionable material. These were the first bombs to be set off under the Atomic Energy Act, and the question was, How [would] they do it? Well, the lawyers were very confused on this subject. They finally said, "There's no doubt about it, the commission can decide this." Then we said, "Can't this be delegated to somebody?" And they said, "We're not sure that that authority can be delegated by the commission. Except, it can be delegated to one of the commissioners." And I can still remember the meeting, where they all turned around and looked at me.

What this meant was that I had to go to the Pacific for three weeks. I wanted to go there anyway for at least part of the test, but I had to go and stay for three weeks, through the tests, in order to verify that everything had been done properly. We could OK the material, but it had to be done on the spot. The way the tests had been planned was, first Los Alamos would conduct one test explosion that was thought to be a conservative advance over what we'd done before. And then, depending on how well that went, we'd either do something a considerable step ahead or, if it didn't go, something a little bit back. We had three alternatives for the second one, and one of those had to be picked, so we sent quite a lot of material out there—and then similarly, depending upon how the second one went. Well, things went very well, and we got quite a leg up in these three tests. We made three shots, and each time after the [test] we had three choices. As I remember, we chose the one that made the greatest technical advance in each case, because things went pretty well.

I lived on a navy ship for three weeks or so out there in the Pacific. Actually, I lived on the technical ship by choice, and also because this was a much easier thing for the project. This test group was just full of generals. The general who ran the tests was a four-star general. And the way things stood on the books out there, he reported to me. This was silly, you see. I was perhaps thirty years younger than he was. I don't think I'm exaggerating when I say there must have been twenty generals and flag officers out there. The deputy head of our military division, Captain Russell from the navy, was out there all the time and lived also on this ship. In fact, he outranked the commander of this seaplane tender, which is what this ship was that we lived on. It had fairly decent places to live on it. It was interesting. The captain of the ship was not cleared on anything about the project or anything, so we never talked about anything in front of him. He obviously was cleared for some things, but we didn't discuss technical subjects at the table or anything like that.

I can still remember how funny it was. We arrived after a long flight. The military services go by such protocol that the test commander had to fly seventy-five miles across this atoll to meet me when I landed, because I was the AEC designate. It was absolutely silly. And of course, I climbed out of the ship as about the thirty-sixth passenger or something—we weren't paying any attention to this. But he was very nice. We had figured some of this out. Captain Russell was very good, because the navy is a stickler for what's called small-boat etiquette. Small-boat etiquette means that the highest-ranking officer gets into the boat last and out first.

Begin Tape 6, Side 1

BACHER: I can still remember going over to visit the command ship one time—I guess they asked me to come over for dinner. Well, we piled into this small boat; Russell got in, then he helped me in. They do everything with a great deal of vim, vigor, and vitality. We got over to the other ship, and he says, "Now you've got to get out first and start up those steps." I got up there, and there was the captain of the ship, and right next to him was the four-star general, [John E.] Hull, who was commander of the whole task force. He had to come down to the quarter deck to receive me there. It's absolutely wild. But I kept a straight face through all of it. I was going through my papers the other day, and I found the cable of congratulations I sent him on behalf of the Atomic Energy Commission when the first shot was successful out there.

TERRALL: You sent him a cable from the…

BACHER: I sent him a wireless from the ship I was on. Our ship was the technical headquarters, where all the technical work was done. After the first shot was done, I went to Captain Russell and said, "Well, I see you have a helicopter around here, and I'd like to fly over there and take a look." He said, "Oh, yes, we've taken a look at it already." And I said, "Well, I'd like to go in fairly close and take a look." And he said, "All right, we'll send along a radiation meter and you fly in." So I got a helicopter, which could take off from the ship we were on. We flew in to take a look at the crater that had been produced on this little island. And I said, "Go on in"—he was going to take a great circle, out here—"You fly right in there and I'll look around over this. I've got the radiation meter here, and I'm looking at it. It's nothing." So I finally got the pilot to fly in and take a circle around. We came back and he looked at me as if.… But there was no danger involved. I had a good look at what the first one had been like, and I was glad to do it.

ROBERT F. BACHER

SESSION 6

July 22, 1981

Continuing Tape 6, Side 1

BACHER: Going back to the work at the AEC: The family spent the early part of the summer of 1948 with our friends the Bainbridges. I managed to get away from Washington for two weeks—not in succession, but nevertheless, we had a very good holiday on Martha's Vineyard, of which we became very fond. The Bainbridges were there, and also the Wheeler Loomises. We had a very good time there and we enjoyed it very much, in spite of the fact that I had to go back to Washington a number of times and also had to spend a lot of time on the telephone.

On the first of August, we went to Brookhaven—just as I'd gone to Los Alamos the previous summer—where we stayed for somewhat over a month and I observed the work of the laboratory and tried to get a little closer to the work in particle physics and accelerators, from which I was mostly detached in Washington. Several times I was called back to Washington for commission meetings. If they needed me for a quorum, if they had to take decisions, why, I simply went down. If they weren't taking decisions, I'd arrange some other way to get briefed. Having been associated in the early days with the starting of the Brookhaven Laboratory, it was certainly good to see how it was thriving, because it really was a very good laboratory. Leland Hayworth, with whom I'd worked closely at the Radiation Laboratory—and who, when I left the Radiation Laboratory to go to Los Alamos, took over the principal part of my responsibilities there—was now the director of the Brookhaven Laboratory. And Gerald Tape, who'd been at Cornell and to whom I'd offered his first job after his doctor's degree—he'd also been at the Radiation Laboratory and been my assistant there for part of the time—was the righthand man in the management of the laboratory. So I had many old friends, and there were a good many more to whom I'd been close in various places—Willie Higinbotham; the Kupers [Herbert and Marietta Kuper]; Dick [Richard W.] Dodson, who came from Caltech; and many others. The laboratory was doing very well.

Going on to the fall of 1948, the AEC was involved in many problems. Weapons development had made some real advances. Both the quality of our stockpile of weapons and the numbers available were all in a much better state than they had been. This was a considerable relief to me, because I'd gone through a period when the people at the top of our government in Washington were talking as if they had a stockpile but the fact was they didn't have very much, which I found very alarming. We took steps through AEC to make the policies pursued in Washington more in keeping with what the situation actually was.

There had seemed to be an interminable number of clearance problems at AEC in 1947. These problems were indeed greatly aided by the formation of AEC standards, and particularly the provision of an opportunity for hearings, which were modeled after court procedures by the AEC regulations. As far as I know, these were the first hearings for clearance. They were a big advance in ensuring individual rights—but, as later seen in the Oppenheimer hearings, could be abused very badly.

TERRALL: This was instead of just having it done behind closed doors?

BACHER: Yes. Before the AEC system was set up, in most cases there was no opportunity for an individual to appear. If a question was raised about his clearance, he just didn't hear about it. He didn't get the clearance. And in some cases, clearances were taken away without people knowing exactly why. I think AEC led the way in getting this on a more reasonable basis. But it is an extremely difficult area, and the Oppenheimer case is an example of how nearly impossible this is and how difficult it is to accomplish something that really ought to be done in a court of law, without having a court of law.

Much of my time, when not involved in the commission meetings and hearings, was devoted to working on a program for nuclear reactor development during this year. This program on which AEC's staff—and especially the AEC general manager, Carroll Wilson—worked was a subject of special interest to me. It seemed to be the core around which the future of nuclear energy, and weapons, too, would be built. Many suggestions came from staff members of the commission and of its various laboratories, and some of these suggestions resulted from the reaction of the General Advisory Committee, about which I have spoken so far all too little. There were, of course, some things that were essential and some that came directly from suggestions of the laboratories' members, and some that grew out of military needs.

The program that developed was announced in a talk I gave before the American Academy of Arts and Sciences in Boston on February 25th, 1949. We essentially said [reading from text of 1949 talk]: "After consideration of the designs and proposals which have been made for new reactors, the commission has come to the conclusion that four new type reactors should be built in the near future. These will form the backbone of the United States reactor development program at the present time. They are: 1) a materials-testing reactor, which, as its name indicates, will be used in the studies of materials to be employed in building reactors." Now, it was clear to everyone who thought about reactors that the first thing we had to have was a reactor of sufficiently high flux density to test the materials you were going to use in reactors. The suggestions for this came from the people at Oak Ridge; this went through, and they built a very sensible reactor which was used for a considerable time for the purpose of testing materials for reactors. The second was "a navy reactor designed as a land-based prototype of a reactor for use in propelling naval vessels of appropriate types." Well, this was obviously a land-based prototype for a reactor for a submarine, and it had to be designed in such a way that it would fit in some of the submarines that were being planned, and it would have to work for extended periods under water. And indeed, we thought this was a job that we, as members of the Atomic Energy Commission, could do to help the military program of the United States, in a way that made the most of nuclear energy. Our hope for this has been proved over the years, because it's still, I think, probably one of the most effective defenses the country has. Now the situation is that the Russians probably have more nuclear submarines than we do, but the point is not that you have to have a one-for-one on this business at all. The fact that it exists creates a whole different situation in the nuclear competition. And other nations have now built limited numbers of nuclear submarines, too.

I might digress here for a moment just to say that when we brought this program in to report it to the Military Liaison Committee, the navy, of course, was delighted that we'd come out with this recommendation. This was done at a very early date. If I remember correctly, Admiral [Hyman] Rickover, who's generally credited with being the father, grandfather, and great-grandfather of the atomic submarine, had not yet come on the project. But the idea of a nuclear submarine certainly antedates even the AEC. Captain Conrad and Admiral Mills in the navy had thought of this long before anybody else had, but our moving into it was an indication that we thought it was a practical thing to do, and we had pretty good evidence at the time that we could do it.

Well, when we announced it, General R. C. Wilson, the air force member of the Military

Liaison Committee, immediately was up in arms, because we had not announced that we were putting equal effort into a nuclear-powered airplane. This was even a little embarrassing for me, because I'd been a consultant to them before I went to AEC. I was a reluctant consultant for the company that was doing this; I mainly told them they had a much tougher problem than they thought they had, and that none of the ideas they had had solidity to them. In fact, Hans Bethe and I had a very interesting time. We wouldn't have done this, but it turned out that the head of the project for nuclear-propelled airplanes was one of the principal alumni of Cornell and the president [of Cornell] said, "Oh, please, can't you do something for our outstanding alumnus?" We said we'd help, but we wouldn't go anyplace and we wouldn't agree to furnish any ideas at all. But we did review what they did. Mostly we told them that what they were doing did not have solidity to it.

So that was the state of the nuclear-powered airplane. General Wilson was just absolutely furious with the AEC, and particularly with me, for coming in with this recommendation for the nuclear-powered submarine. He's the same fellow who subsequently raised a question with the air force as to the loyalty of Oppenheimer, and if one reads the testimony on this, he couples my name with Oppenheimer's when he brings this up. [Laughter]

The third kind of reactor proposed was an experimental breeder reactor, designed to operate with high-energy neutrons. "This will be used to explore further the possibilities of breeding; that is, producing more fissionable material than is consumed in the operation of the reactor." And also power production. Now, this was directly to support a program that [Walter] Zinn at the Argonne National Laboratory had already been thinking about; this was simply saying that we were adopting what he was doing as one of the things we thought was important. Our idea, then—and I, particularly, thought this—was that one of the main things the commission ought to do was start up things that were headed far ahead. The idea of putting all the emphasis on what we were going to do tomorrow was something that ought to be left for somebody else, but if you're talking about a development program, you should put your sights a long way ahead. And we thought all the things we were talking about up to this point were a long way ahead. We thought—and I guess perhaps we were wrong on this—that one of the places we would be a lot sooner than we are now was into breeders, and particularly into power breeders. So this was one that was already being thought about.

The fourth part of our program was also a breeder. It reads: "An experimental breeder reactor designed to operate with neutrons of intermediate energy and to explore their possibilities for breeding as well as to produce usable power." This project was one that we dreamed up and persuaded the General Electric Company to undertake in a laboratory built essentially for this purpose near Schenectady. Now, GE could hardly contain themselves in their promises as to what all they would do in helping to get into this subject, because the inside track on the submarine reactor inevitably looked as if it would go in some other direction, to companies that had other interests. The amusing part of this is that they put together a very good laboratory and got one of the very best people they possibly could as one of the young men in the laboratory—namely, Harvey Brooks, who's at Harvard now and is an extremely able person. They put a lot of their very best people on this project. We were delighted, because this meant moving directly into what's called a fast breeder. It's really the direction to go, in order to get the breeding ratios up and so on. Nothing had been done in this direction so far. After I left the AEC, General Electric—I think impelled by Rickover—persuaded the AEC to let them change it into a submarine reactor. I always thought this was a great joke—but a very expensive joke—because their reactor was cooled with sodium, and the idea of sending sodium-cooled reactors undersea just gives you the creeps to even think about it. But, nevertheless, they did. Two things happened: They changed it from being a sodium-cooled reactor, and of course as soon as the other reactor that had been originally designed for driving submarines worked and was done, this was dropped like a hot potato. This was a great pity, because a lot of work that could have been done thirty years ago still hasn't been done.

Well, this is a summary of the reactor program. I spent a good part of that year working on this program. By the time I gave this talk, it had been approved as policy by the commission. In fact, it was approved as policy by the commission just a day or two before I gave [the talk]. It turned out that this got to be the deadline for getting some of the things worked out and writing up the policy. [In] one of the history books AEC sponsored later, this program, which was approved by the AEC and is in their papers, is wrongly stated.[1] The last of the reactors, the intermediate-energy breeder, is forgotten. This shows you how history is sometimes presented in a distorted way, not the way it actually was.

[1] W.H. Zinn, F.K. Pittman, J.F. Hogerton, *Nuclear Power U.S.A.* (New York: McGraw-Hill, 1964). See p. 6. The reactor program is discussed accurately in *Atomic Shield: 1947-1952*, a more detailed history of the AEC by R. Hewlett and F. Duncan (Berkeley: University of California Press, 1990).

My initial appointment at AEC was for one of the short, staggered terms. In 1948, President Truman asked me to take a renewed appointment, which I did, with the understanding that in so doing I was not obligated to stay for its termination, which would have been several years more. In the early spring of ’49, after I’d been working on the reactor development program and a lot of other things, too, I realized how tired I was and how much I needed to get back to a university position if I were ever to do so. Our commission had really worked very well together. Everyone worked hard to get the civilian management functioning and make up for the inevitable letdown that came when borrowed personnel, often in very responsible positions, left the projects to return to their normal jobs. There were many new policies to be discussed and beaten out. There was always more work than could be done, but a lot was done, with the great help of an extraordinary General Advisory Committee, of which Robert Oppenheimer was the chairman, and a number of ad-hoc groups that were brought in to advise us on particular subjects.

My friend Waymack left the commission after two years, and for some time his place was not filled. In the late winter of ’48-’49, I talked with David Lilienthal about leaving. He had hoped I would stay but understood my desire to get back to a university. I talked to him at length about getting a scientific replacement. This was not easy because of the shortage of senior scientists and their reluctance to leave their posts—this was not a part-time job. After many discussions and consultations with the General Advisory Committee—and especially with Robert Oppenheimer—we concluded that Henry Smyth at Princeton was the best choice, and that it would be good for me to take this up with the President when I asked him to accept my resignation. And this I did.

The President saw me alone and first tried to get me to stay on. It was hard to say no, because he had backed us every time and was dedicated to civilian control. But he did understand my situation and did not try to pressure me too hard. I said I hoped there would be a scientist to follow me, and he immediately agreed that that should be done. I told him we’d been thinking about replacements and he’d hear from David Lilienthal about it. He was extremely cordial in saying goodbye and thanks, and it was arranged that I would leave early in May.

Meanwhile, I had to consider whether to return to Cornell, from which I was still on leave as professor. I was very much attached to Cornell—to many friends there, and particularly to Hans Bethe. If I’d not gone to Washington, I would probably still be at Cornell. But returning to be a member of a laboratory that I had had a major hand in starting as its first director did not seem to be a good thing to do. Cornell made a few oblique inquiries about my interests in administrative jobs. But before really seriously considering any job, I resigned from Cornell as professor.

TERRALL: You thought that going back to a lab where someone else was director would be a problem?

BACHER: Well, I think it's difficult, if you've started something and then left it while it was still in the formative stage, and somebody comes, as Bob Wilson did, and does a good job of running the laboratory. It would not be an easy thing to do. Furthermore, it was complicated by the fact that when I was at Los Alamos, Bob Wilson was head of one of the groups in my division. He's an extremely able fellow and I had great admiration for him. I thought this was not something that made any sense. It was just sort of looking for trouble, much as I liked Cornell.

Meanwhile, Lee DuBridge, of course, as a member of the General Advisory Committee, knew about my prospective retirement as AEC commissioner and talked to me about the future. Apparently they had not filled the position of chairman of the Division of Physics, Mathematics, and Astronomy at Caltech, about which he had approached me while I was at Cornell, and he said he would like me to come. At that time, the idea of taking on a new administrative responsibility was not very attractive to me, and I explained that I wanted to get back to research. Lee DuBridge said quite straightforwardly that while they needed a chairman for Physics, Mathematics, and Astronomy and needed some planning, that I could come either as chairman of the division or as professor. This was sometime during the talks before I left AEC, and it was left that way.

Gradually I saw how much some new planning was needed. Charlie Lauritsen had already realized that Caltech had to get into high-energy physics. I don't know just when it was that I decided, but I finally agreed to come here as chairman of the division. Before going very far on this, I also talked at great length with Robert Oppenheimer about the future. When he left Los Alamos, he had gone to Caltech, but for a variety of reasons, some personal, he'd returned to Berkeley. By this time, he had moved from Berkeley to Princeton and was director of the Institute for Advanced Study. From him I learned more about Caltech problems and their seriousness and the need for outside personnel in their solution. The decision I came to was a fateful one, and probably illustrates a major failing in my makeup. I saw what was needed in the division at Caltech and felt some real confidence that I could do a respectable job, so I agreed to take the division chairmanship—at least to get some new fields started and make some additions and some reorganization. I put as a proviso that we would expand the work in physics by starting new work in high-energy physics and that we would strengthen the theoretical work, especially in this field. This Lee DuBridge thought was fine, but we did need support, and there appeared to be some opportunities available.

This was a fateful decision because it turned out that I completely underestimated the demands that would come from Washington. I also found that the job itself had many problems. But I stuck to the plan of getting high-energy physics started—experimentally and theoretically. And Charlie Lauritsen helped very much on the experimental end by providing laboratory space and initial financial help. Robert Langmuir had joined the Kellogg group and had some initial plans for a 600 MeV electron synchrotron. He was a very knowledgeable experimental physicist and had a good background, especially on the radio frequency problems involved in an electron synchrotron. He came here from General Electric, where he participated in the work in that field. Having started an electron synchrotron at Cornell, it came naturally to me to move in that direction, but I thought the proposed energy was too low.

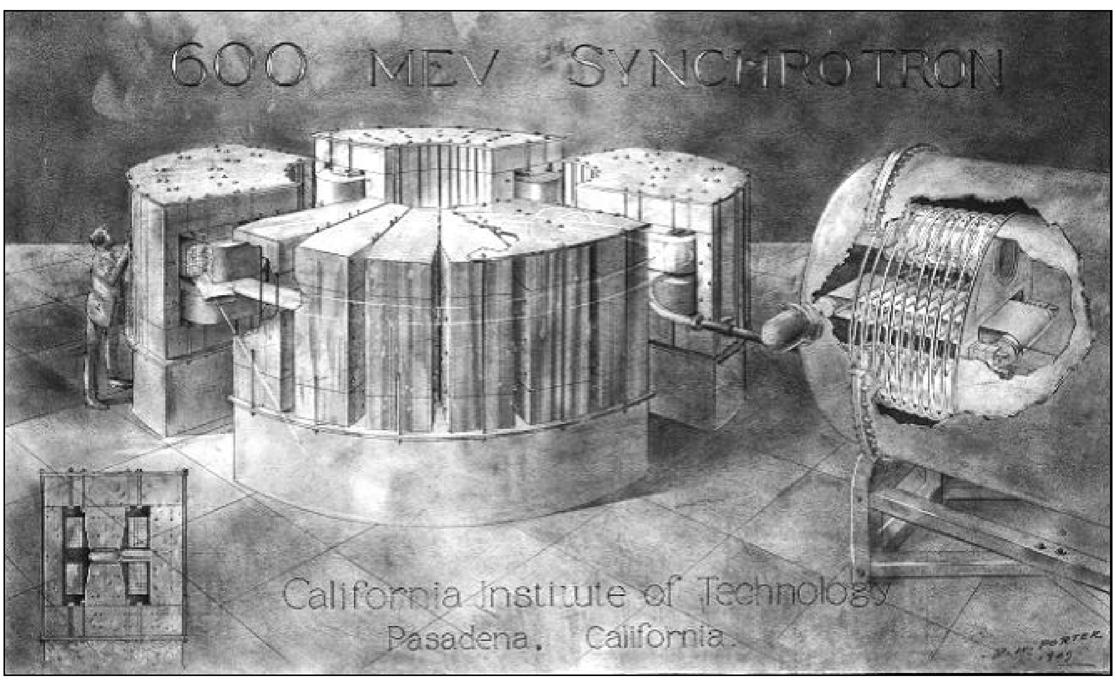

Fig. 3. Russell W. Porter’s 1949 drawing of the 600 MeV synchrotron. Caltech Archives.

Before coming to Pasadena, we took a long and much-needed vacation. But before this there was the problem of finding a place to live. So we visited Pasadena, driving out in our old car, and at Mrs. Tolman's invitation stayed with her in the house in which we now live. Richard Tolman, whom I had come to know and respect, mostly after the war, had died suddenly in Pasadena, in September 1948, after a severe heart attack. This visit started a close friendship with Ruth Tolman for both of us, but especially for Jean. We had hoped to buy a house, but the housing shortage still persisted, and difficulties arose with the one we liked. So we rented a house across Wilson Avenue from Caltech. This location turned out to suit us very well, and we have ever since then lived very close to Caltech. So that was a piece of luck, it turned out.

We returned to Washington by air, where my mother had stayed with our children during our two or three weeks' absence, and prepared to move what furniture we had there and to arrange for some additional possessions from Cornell to be gathered up for our move to California. We had arranged to rent a house at Crystal Lake, in northern Michigan, not far from our friends the Cyril Smiths, and we headed there, stopping briefly in Ann Arbor to visit family. This was a real vacation, and I had time to think over plans for the future. Before then, I had made arrangements for Robert Walker to be offered an assistant professorship at Caltech, and he was to start that fall, the first addition to the staff in high-energy physics.

While at Crystal Lake, I thought about the other staff members and remembered that Feynman, whom Bethe and I had persuaded to go to Cornell from Los Alamos, had separated from his wife and might just consider moving from Ithaca as a consequence. A few calls from the little town of Beulah, nearby, located him at the summer session in Ann Arbor. I persuaded him to visit us at Crystal Lake for a weekend. I had some long talks with him about the future. He did feel very unsettled in Ithaca and had already promised to spend a year in Brazil. After some calls to Pasadena, it was arranged—I don't remember quite how—that we would offer him an appointment underwriting his leave to Brazil. Conversations with Cornell indicated that they felt that Feynman would probably leave, and they regretted this very much but were glad to see him come to Caltech. This took some time to work out, but his coming to Caltech was strongly supported in Pasadena. This brought here two of the very best young theorists from the theoretical group at Los Alamos, since Robert Christy, who strongly supported the move, was already here.

After our family vacation, we drove slowly across the country, stopping in Colorado at the place where the Bethes were staying and then coming on to Pasadena in mid-August. Our reception was an August heat wave and the absence of most faculty, except those from the Kellogg Laboratory, who never seemed to believe in vacations. I could start slowly in my new job, and I had many talks with Charles Lauritsen, Carl Anderson, and also Christy, who was then a part of the nuclear physics group in the Kellogg Laboratory.

I neglected to say that during the spring of 1949 there had been increasing concern on the part of Senator [Bourke B.] Hickenlooper, who was one of the senior members of the Joint Congressional Committee on Atomic Energy and who, for reasons that were never quite clear to me, felt impelled to make a vigorous attack upon the Atomic Energy Commission. This didn't break until after I left Washington, and then they started a whole series of hearings. The first I heard of it was when I got a message to call Washington, while I was in northern Michigan. To my rather considerable surprise, they wanted to send a plane up for me to the nearest town of Traverse City so that I could come down and spend a day testifying before the Joint Congressional Committee.

TERRALL: And this was just when you thought you'd gotten out of that, too.

BACHER: That's right. I thought I'd just gotten out of this and I had to come back and remember a lot of things. I must say that I felt rather badly about this, because on the whole I'd always been reasonably friendly with Hickenlooper. But I what he was doing there was something I couldn't condone at all, and I'm afraid some of my testimony was pretty sharp. It was an outrageous thing for him to do. There was no basis for it, and it was simply disruptive to the program. Hickenlooper, in general, was a very good man and worked hard at things, and I've never been quite sure why he felt compelled to attack the commission as much as he did. Unfortunately, this attack came only a relatively few days after Smyth arrived on the job, and it certainly wasn't fair to put him on there. I felt impelled to go back and testify, which I did. I thought we had a very good record, and I had no hesitation in defending it.

Begin Tape 6, Side 2

TERRALL: What were the things that Hickenlooper was concerned about?

BACHER: His principal charge was mismanagement. Well, this can cover a lot of things. He was pretty vague about what the mismanagement was. I think he was really attacking Lilienthal. But it was a very general charge, and I don't know what he'd expected to have happen. I don't know whether this was something he was put up to by some of the air force people—whether this was part of that decision. I'm not sure that it was. I don't think that ever really surfaced in it. But he was certainly upset. I think most of the things he brought forward were pretty well covered in the defense that was made of the commission. People who were brought in did defend the commission very well. I think somehow or another this was more a personal thing than a general thing. Of course, what one has to remember is that there was always a certain amount of pressure from the military to take over functions that the President had assigned to the Atomic Energy Commission. Their general view was that the commission ought to work for them. But the commission was appointed by the President and reported to him, and he very much wanted it that way and was quite clear in making his defense of it. But I have a feeling that this was involved. I don't remember enough about the details of reading about it in the paper at the time to say more about it.

TERRALL: How long did the investigation go on?

BACHER: I don't really know. I was there only a day or two.

TERRALL: And they asked you specific questions?

BACHER: Oh, they asked a lot of questions, and I gave answers to a lot of questions. I also volunteered a fair amount of information and made no bones about defending the commission as it stood. I defended the commission's accomplishments very strongly. I don't know whether what I said was helpful or not, but at least it was a situation in which they couldn't very well call on Harry Smyth to defend them on the technical [points]. They either had to do it themselves or call me in. I was glad to go down and do it, much as I hated to go back into that atmosphere again.

Well, as it turned out, that wasn't the only time I got called back. I remarked that things started up very slowly in Pasadena when I came out here, and indeed they did. But I can still remember coming down to breakfast one morning not very long after we'd moved into our house. I think it was sometime in the first week in September. Before I'd come downstairs, Jean called me and said, "You have a call from Washington." I said, "Gee, I wonder what's happened now." I wasn't quite fully awake yet, but I came downstairs and got on the telephone. And a man on the other end said, "You don't know me, but I am Dale Northrup, and I work for General Hergenrother." I said, "Who did you say you worked for?" He said, "I work for General Hergenrother." I said, "What is it?" General Hergenrother was the man in charge of the very secret operation set up at the request of the Atomic Energy Commission—and the military, too—to monitor the skies for particles from the explosion of nuclear weapons which did not originate from what the United States was doing. They patrolled the western part of the Pacific very thoroughly, because of course things circulate to the east in the Northern Hemisphere. And I knew this.

He said, "We have some positive information." I said, "Positive information?" He said, "Yes, and we'd like you to come to Washington to participate in a committee that's being set up to evaluate what we've found and make a report to the President on what the situation is." I said, "Well, I guess this is something that takes precedence. I have only one answer, and that is: When do you want me to come?" I said, "It happens that I'm going to be in Washington in about ten days for a meeting that I've been asked by AEC to attend, when we get together with the British to discuss problems of raw materials and so on." There was an organization set up to do this, and the commission had asked me to come—I guess in part because I'd been connected with it in the past and also in part because John Cockcroft was the man principally in charge at that time. "Oh," he said, "that would be just about the right time anyway. I'll get those dates and we'll try to work this in together." So they did.

Let me go back to the first of these things. I'm not sure in what order they occurred, but I think the test evaluation came first. It was carried out in one of those rooms where, when you went out through the door, they locked it up, like a big safe. Yet the meeting had thirty people in it, or something like that. I've forgotten who all was on the committee, but Van [Vannevar] Bush, I think, was the chairman of it, Robert Oppenheimer was on it, Arthur Compton, Admiral

[William S.] Parsons; and I guess I don't remember who else was on it—I think Ernest Lawrence was on it, but I'm not sure. Essentially, they presented the evidence, which by that time had been worked out very thoroughly.

TERRALL: And it was clear from the evidence what had happened?

BACHER: It was absolutely completely clear from the evidence that they had atomic bomb material. There just wasn't any doubt about this whatever; it could only have come, at such an altitude and in such density, from an atomic bomb explosion. So we wrote a report. Well, the President immediately heard this. I think he'd been briefed enough by military people, including Leslie Groves, who thought the Russians wouldn't get this bomb for years and maybe never. Groves had, anyway, argued very vigorously that they wouldn't get it for years, and I never knew why he did that. I think he was so impressed with the difficulties of manufacturing the stuff that went into it, that regardless of how good their scientific background was, they just wouldn't be able to pull it off. I guess he thought the job he'd done was so hard that nobody else could do it. We didn't feel that way about the scientific end of it. And in fact, a number of people were asked for estimates of when the Russians would get it. I had said, in round numbers, five years. It turned out they started two years before the end of the war, so it took them six instead of five. They had very good people, and they had a background such that they could do it. Groves overestimated the problem. The problem of making [an atomic] weapon is very greatly simplified if you know that one has already been made. The greatest part of the work we did at Los Alamos we never would have done if we'd known anybody had exploded a bomb.

But the interesting part of it was that Mr. Truman, when he heard how this hearing was coming out—because of course he was greatly interested in this; it was a major turning point for the government in many ways—he doubted it. In spite of the fact that these were all people he trusted very much, he was apparently so shaken by the fact that the Russians had the bomb that he wanted and got a report written by Van Bush and the rest of us, and signed by all of us individually. This must exist in White House papers somewhere. I'm sure there's only one copy of it, but it must exist somewhere. I learned later that he asked for it because he found it very difficult to believe, from what he'd been told by others, that it would be possible for the Soviets to do this in that period of time. Somehow or other, he had not believed what scientists had told him about this. I'd been thinking that this was about the time for this to happen.

Well, I visited my friends over at the Atomic Energy Commission and reported to them, because they were not a part of this evaluation, which was carried out by a committee set up separately from the commission. It was set up at presidential direction, and I think set up by the military. I'm sure [the AEC] had some staff members there, but they were interested in talking to me, and I went over and talked to them afterward about it and said they needn't worry about the fact that the evidence wasn't sure; it was as clear as the nose on your face.

Following that came the meeting in which I was to join with the AEC delegation and meet with the British. One of the people associated with security at the commission, who'd also been associated with our other security people during the war, came to me and said, "You're going to be talking with Cockcroft. We've wondered if you'd be willing to ask him if he has any misgivings about Klaus Fuchs?" And I said, "Of course. I have no reason not to do that at all." They said, "We'd only ask you to do it in private and see what he says." Well, I did. At this meeting, I said to Cockcroft that I had something I wanted to ask him in private, and I simply told him that the question had been raised in the United States as to whether they'd had any suspicions that Klaus Fuchs might really be working for somebody else. And he said, "No. We haven't had any indication of that at all. I haven't heard anything." Well, he was a very important man in their atomic energy program at that time, Klaus Fuchs was in Britain, and this, of course, was a very delicate situation. "Well," I said, "I've been asked to ask you this question." And he said, "Well, I shall look into it as soon as I go home."

TERRALL: But you at that point didn't know what the AEC security man's question was based on?

BACHER: No, I didn't know what it was based on. I knew the security people well enough to know that they wouldn't ask me this—and in fact, I put it to them: "Unless you have some really solid evidence to go on, I don't want to ask him this question." And they said, "It's pretty solid."

TERRALL: So this was a surprise to you at that point, too, then?

BACHER: That was the first I'd heard of it, and it was the first that Cockcroft had heard of it. But you know afterward what happened when the British investigated it. They found out, when they confronted him with the evidence they dug up, that he had apparently constantly been supplying information to the Russians up to the end of the war. He no longer had an association with the project, as far as I know, after the war. He may have been back at Los Alamos once, but I don't think so.

TERRALL: Was he working in your division?

BACHER: No, he was a theoretical physicist and worked in Bethe's division, but he had been our theoretical advisor on one of the most delicate parts of the weapon. So this was quite a shock.

TERRALL: Especially finding out about it two weeks after the Russian bomb had exploded.

BACHER: Apparently all that information had been transmitted to them. I don't think there's any doubt that they had a pretty good insight into things we'd tried and things that worked and didn't work. Some of the most ticklish parts of this, and things we really worried a great deal about—I'm sure they had detailed information about how to do them. Well, it was quite a blow.

Now, coming back to Caltech, I think my many years at Caltech can probably be broken down into three periods, but they don't easily fit into a real chronological account on a year-by-year basis. The first of these was while I was division chairman [1949-1962] and head of the high-energy physics project. The second is the subsequent period, to August 31, 1970, which was the day I retired as provost and vice-president, which I remember very well because it was my sixty-fifth birthday. By the rules that I was trying to get the institute to adopt—that people retire from major administrative jobs at the age of sixty-five and not the normal retirement age of the institute—I decided it was appropriate that I retire at age sixty-five, and that was the day. It was also an appropriate time to retire because I had helped Harold Brown get started as president, and I thought he ought to have his own provost. The third is the period after this, first as professor and then as professor emeritus, during which I've had a variety of responsibilities here and elsewhere, which have gradually been diminishing over the years.

One of the first things I found at Caltech was that the physics budget was very small, and almost all of the research was supported by government contracts. Five-eighths of the salaries of the professional faculty members who participated in such contracts was charged to the contract. I felt that regardless of time spent, Caltech should pay at least half of all professorial salaries, and I quickly moved to get approval from Earnest Watson—who was the dean of the faculty and had been acting as the chairman of the division as well and with whom I generally talked about things—and from Lee DuBridge. When DuBridge first came here in 1946, he immediately took steps to increase faculty salaries substantially, because they were very low compared to other universities. Even when I came, I recall that one of our full professors who was very highly regarded was paid $8,500 for a full year. This sounds shocking now, and is a measure principally of inflation, but partly also of the low faculty salaries at Caltech. Their salaries in the period before the war were probably less than half that. This was a serious situation. I don't recall exactly what the numbers were, but my memory is that our total budget for physics—or maybe for the division—was just a few hundred thousand dollars a year. It was very small.

Well, from the first, I moved to get us started in high-energy, or particle, physics. And by the following spring we had a contract to build an electron synchrotron and support experimental and theoretical work in this area. That we negotiated and had in operation by March. While speaking of the contract—which, with some modifications, still exists in the form it was written thirty-two years ago—I asked our business people to let me write the section on the purpose of the contract. I thought this statement ought to be broad enough that it could stand for a good many years and wouldn't have to be renegotiated as we went on. I don't remember all the details about how this was written, but the purpose of the contract was, as we saw our way then, first to construct a laboratory for high-energy physics in which there would be a high-energy accelerator—an electron synchrotron—and to support work with such a machine, and general experimental high-energy physics, and also theoretical particle physics. Then it went on to discuss various other things we might do. The nature of the research I tried to keep in general terms, but, realizing that it would be very difficult to do that, I put in a final point—to support any other work that the institute and the Atomic Energy Commission might agree upon. And several times we used that last clause to cover some little thing we agreed we would do. I think it's been modified, because subsequently we did most of our work elsewhere; we shut down our electron synchrotron in 1969. But many of the terms of that contract still stand—and this is now thirty-two years that it's been in operation.

TERRALL: And the agency has changed names three times.

BACHER: The agency has changed names three times, I guess. [Laughter]

TERRALL: Was this the first Caltech contract with the AEC?

BACHER: I don't know the answer to that. I think not; there probably were small contracts in other fields. It was certainly the first contract of any size in physics, and it was started off as what was then quite a large contract. I've forgotten exactly what it was, but it started off with an amount for a capital expenditure for an electron synchrotron, which was at first just put in in terms of investigating it and then put in as we made an estimate of what it would cost us; and secondly, to support the work. The part to support the work started out at something like $250,000 or $300,000 a year. It was the largest contract the institute had for the support of research. One of the things I was particularly interested in was getting theoretical work started, and I moved in this direction as fast as we could. The principal start in this direction was getting Feynman out here.

TERRALL: You mentioned a few minutes ago about getting Caltech to pay a higher percentage of salaries. What was the reasoning behind that?

BACHER: Well, I didn't think it was right for the institute to be having a fraction larger than half of its faculty salaries paid by an outside agency. It seemed to me they were regular faculty members and at least half of the salary ought to be paid by the institute. It ought to be clear that they worked primarily for the institute and not for the Atomic Energy Commission. That's not so clear if a fraction larger than half of their salary is being paid by an outside agency.

TERRALL: It wasn't a question of freeing more contract money?

BACHER: No, no, it wasn't that at all. In fact, this hit us where we were very seriously pinched, because it was institute money that was very short. We could have gotten more money on the contract at that time without any trouble.

TERRALL: So did DuBridge agree with you on this?

BACHER: Yes, and he thought the reasoning was sensible. Let me just digress now a bit to say something about DuBridge. I first met DuBridge in 1929, which is a long way back. I hadn't yet taken my degree. He came to visit the summer session at the University of Michigan and had a cottage at Cavanaugh Lake just next door to where we were. I met him for the first time that summer. I didn't see much of him subsequently, because we didn't work in the same field, until I went to Ithaca. When I went to Ithaca, he was the head of the department at the University of Rochester, and I saw quite a bit of him. Then of course he became the director of the Radiation Laboratory at MIT, and I went there rather early—before the first of January, 1941. I saw a lot of Lee during that period, on a whole variety of subjects. I've mentioned a number of our contacts when I was at AEC and subsequently. He is an absolutely wonderful person. This institute has been extremely lucky to have had him as president for twenty-three years.

The institute was an extremely good place, but it had fallen into a rather desperate financial situation as a result of the Depression. Caltech was really hit hard by the Depression. It's one of the places where things really didn't move, in the period right before the war. That was a very slack period everywhere in physics, but Caltech was hurt more than many places were. That it did as well as it did in that period is a great compliment to the faculty members who were here and a great compliment to [Robert A.] Millikan—but especially to the faculty members who learned how to do first-rate work with almost nothing to support it. One of the leaders of this was Charlie Lauritsen, who built up the work in nuclear physics with very little funds to support it during that period.

Lee DuBridge came out here at the end of the war, and his first job was to take a situation where there was desperate need for financial help and get it going. He was very successful in doing that. He is to a very considerable degree responsible for a situation that exists at Caltech that is very different from the situation that exists in many educational institutions. Often you will find an educational institution where the members of a department feel close, or the members of some small group, but you get a faculty group from one field talking to a faculty group from another field and you may get opinions that are miles apart. At Caltech, there is a much higher degree of identification with the institute as a whole than in most institutions. I don't think that's quite as much as it used to be fifteen years ago, but to a considerable degree it's a very much greater unity than exists in most places, and you find it comes out in a lot of different ways. There are many things that illustrate it. One of the things that I think wouldn't happen in any other place is the effort to get senior faculty into teaching undergraduates; one of the things that has been done is to bring in professors from other fields to teach in that area. I don't know of another place where that happens in the United States. It's done and done very successfully. If you talk to people who are doing that, they feel they're doing one of the most important things they can do in their teaching experience. That's a very unusual situation. In many respects, Caltech is different.

TERRALL: So you would attribute a lot of that to DuBridge?

BACHER: Well, I think that's something that grew up at the institute; there was a background for it. I think it existed before and was enhanced by the tough times that the institute had, but Lee DuBridge certainly fostered this; he was very successful in preserving that view and developing it and making a great continuity of views in the institute. Now, that doesn't mean that everybody in one field agrees with everybody in another field. That doesn't mean there isn't a certain amount of competition between different parts of the institute. But when you get an overall institute policy—and everybody agrees that it is—it's looked at a little differently at Caltech than in many institutions.

ROBERT F. BACHER

SESSION 7

August 6, 1981

Begin Tape 7, Side 1

BACHER: When I came out to Caltech in the late summer of 1949, I found a complicated situation, especially in physics, which grew out of the fact that the physics work had been really depressed in the years immediately before the war. Some of the work was outstanding, but some of it certainly needed building up. One of the principal things lacking was that all new work in physics nationally after the war started to head off into the subject of high-energy physics or particle physics. There was practically nothing [at Caltech] in that field, except for the fine work Carl Anderson and his colleagues were doing in cosmic rays. The problem there was that it was perfectly clear, even at that time, that one could not forever continue to do the particle work using cosmic rays as a source. Carl Anderson was one of the stalwarts of physics, and he and Victor Neher both worked in cosmic rays. They worked on rather different things. Carl Anderson's work with some younger members of the department, some of his students, used cosmic rays as a source of high-energy particles to do particle physics with cosmic rays. There was no question that they were either the leading or almost the leading group in the world. Interesting enough, this was work that Millikan had started. One of Millikan's great characteristics was that he had a fine nose for where to go in experimental physics, and he had started Carl Anderson, as a postdoctoral fellow, using a cloud chamber to do the work that subsequently led to the discovery of the positron and also to the meson discoveries and a number of other things their group did subsequently. Anderson, of course, was a phenomenal experimentalist, and this was very productive work, probably the most outstanding work being carried on at that time.

Neher worked on a subject that Millikan also started and was very much interested in. This was essentially trying to understand the way cosmic rays come into the earth's magnetic field; indeed, it was through work of that sort that a good deal of the nature of cosmic rays was subsequently worked out. Neher was a first-rate experimenter and did very good work. There had been in the department a very able and strong man, [William V.] Houston, who served as the division chairman up to the end of the war, but at about the time that Lee DuBridge came here he left to become the president of Rice University. He was a very unusual physicist; he did both experimental and theoretical work. I worked a bit with him when I was out here as a postdoctoral fellow. Earnest Watson, who had been the dean of the faculty and had also done a fair amount of the managerial work for the division, served as acting division chairman after Houston left and was still in that position when I came here.

TERRALL: Was that understood as a kind of interim thing. Watson knew they were looking around for someone?

BACHER: Yes.

TERRALL: Was there any tension there when you came?

BACHER: None. No tension at all. It was not a job he particularly felt involved in. There was no tension at all with Earnest Watson, and I became close friends with him. In spite of the fact that we had quite different backgrounds, our views of what sorts of things we ought to do were similar. He was enormously helpful to me and someone to whom I could go whose view was not [limited to] some particular project. He hadn't done any research in physics for a good many years, but he was an expert at the teaching of elementary physics. He also was a very good manager. He was having a lot of fun writing a lot of things about history of physics—he wrote a series of forty or more articles in the *American Physics Teacher* on the history of physics, which are really very interesting articles.

Paul Epstein was here. Epstein had been one of the leading theorists in the days when he came to Pasadena, and he had contact with and knew almost all of the famous European physicists. He was somewhat older, I believe, than some of the younger group like [Werner] Heisenberg and [Wofgang] Pauli, and so on, but he was well known and respected as a theorist. His interests, however, never really got into doing research in quantum physics. He essentially taught courses that formed the backbone of the theoretical courses. As far as I know, he was a very good teacher. He also ran the physics colloquium—the physics research conference, as it's called—which meets on Thursday and in fact, as far as I know, has met at essentially the same time for more than fifty years.

Millikan was still alive when I came. He had moved into the office that's now the division chairman's office; they'd fixed it up nicely for him. I found Millikan extremely helpful. I was a bit concerned when I came out that, after all, if you come to an institution that has been put together by somebody and he's now retired and is down the hall, and he's the only real permanent head that physics has ever had—he had essentially run the division ever since things had started out here—that nothing that one did would look quite right in his eyes. Millikan was remarkable in this respect. I thought about it for a long time. I had good relations with him—I'd known Millikan before I came. He had written some on the atomic energy problem, and I very carefully stayed off this subject in talking with him, because he'd written some things and given some speeches, and essentially he was under the impression that there just wasn't enough uranium in the world to make any impact whatsoever. I thought it was just better not to discuss this subject with him, and I almost never did. There had not been much uranium known in the world, but the point was that nobody had ever looked for it very hard. And this rapidly became a quite different thing.

TERRALL: So did he come in every day? Was he around?

BACHER: He was around essentially every day. He had a secretary and he came into the office, and he was alive for four or five years after I came—he died in '53. I didn't go to see him very often, but whenever I had a problem that seemed to tie back into things he had an interest in, I went down to talk to him about it. Or if I were going to start something really new, I went down to talk to him about it. Invariably, if I proposed something new to him that was quite different from anything that had been done, almost without exception he said, "That's a fine idea. Is there anything I can do to help you?" It was inconceivable to me that a man who had had such an important part in starting this institution could have been so supportive to someone coming in to take over from that point, at least as far as the division was concerned. He was, I think, very supportive of DuBridge, too, but I know less about that.

Also around was [Fritz] Zwicky, who had been in physics and worked in astronomy and used to teach some of the graduate courses in physics. By this time, he was thoroughly tied in to the work in astronomy and was off working on certain kinds of novae and so on, and this was his principal operation. Zwicky was a pretty tough character, but I never had any particular difficulty with him. He was not much tied in to physics and had no interest by that time in what happened either in teaching or research. His interests then were in astronomy.

Alexander Goetz was here, and he had been one of Millikan's favorites. He brought Goetz in to work in low-temperature physics, and it had never gone very well. Goetz didn't have much standing. I think Millikan always thought he was extremely good, but Goetz really didn't have a very high reputation. While he seemed to be a clever experimentalist, the low-temperature work never really caught on. They had a liquefier they'd made, and the liquefier didn't work. I think it may have produced hydrogen in the past. But as you will see, when we came to needing some liquid hydrogen, we took a different course.

TERRALL: What about Jesse DuMond?

BACHER: Jesse DuMond was here. The experimental physics divided into three groups—probably the largest was the group that worked in nuclear physics, of which Charlie Lauritsen was the head and the person who'd started it; Carl Anderson, whom I've discussed; and Jesse DuMond was the third. Jesse had started off in nuclear physics, doing high-precision experimental measurements, and he was a really remarkable man at that sort of thing. And then he got off into the subject of physical constants and wrote a number of papers on that subject and was the acknowledged leader in that field. He then went into gamma rays, and he built some beautiful gamma-ray spectrometers—gamma rays are the photons emitted by nuclei. He was very good at studying these gamma rays; he produced ingenious equipment of the highest quality and did very well. His whole program before the war was managed on an extremely small budget. Even after the war it had very modest funding.

TERRALL: He was still doing that?

BACHER: He did this continuously and expanded the work considerably, and it expanded still more when he brought Felix Boehm out here. Felix essentially became his successor. Felix has worked in quite a lot of other fields, but he came here first as a postdoctoral fellow. And Mössbauer was here for a time. That's an interesting story. Sometime during the fall of 1958, Felix Boehm, who was then an assistant professor of physics, sent a letter to Jesse DuMond saying that in Heidelberg he had met a young man, Rudolf Mössbauer, who had recently done some very interesting experiments on the recoilless emission of gamma rays. These gamma rays came from an excited state of iridium—iridium-191—and were obtained by exciting an osmium-191 source. At almost the exact excitation energy, the emission showed a narrow peak, due to the iridium-191 at exactly the right energy being coupled to neighboring atoms so that the crystal as a whole took the recoil. This was a new phenomenon and was treated in Germany with some skepticism. Mössbauer had published some of these results, and Felix Boehm thought the work was interesting and we should get Mössbauer to come to Caltech, because he seemed to be available. Jesse DuMond said the report of the work looked good, and I called Christy and Feynman to come over and see the letter and asked them to investigate the references carefully. They did that independently and quickly. Both of them were convinced this work was correct and was an important discovery. An offer of a postdoctoral fellowship at Caltech went off very quickly, and in due course, after completing his work in progress, Mössbauer came to Caltech to work with Felix Boehm and Jesse DuMond. This happened before his work was fully recognized in Germany. It was not long before that work was recognized, and he was awarded the Nobel Prize [1961]. He continued in Pasadena as a professor for several years, and some very interesting work came out of his collaboration with Boehm and DuMond. Eventually the Germans made fantastic offers and put considerable pressure on Mössbauer to come home. His work started a whole new field of physics, with important applications to other sciences. Had it not been for the astuteness of Felix Boehm, the judgment of Feynman, Christy, and DuMond, and some fast and definite action by Caltech, we never would have had him here.

Then the other group I found here in experimental work was the Kellogg Laboratory, which was probably one of the best of the nuclear physics groups. Lauritsen had started it and had rapidly moved over to using electrostatic generators and essentially worked more in the low-energy range. These went, at that time, only up to a million-and-a-half or so volts; they have built higher-energy machines subsequently. But they kept to an energy that was primarily aimed at studying nuclear excited states. It was their idea to work principally on the light elements, though they have done a little bit of work subsequently in some of the heavier elements. The reason for concentrating on the light elements was interesting, and a very wise decision on Lauritsen's part in getting the work started. He worked on the light elements because they were the ones of interest in finding out the source of solar and stellar energy, so they concentrated on those problems tied into our exceedingly strong work in astronomy.

But there was not much experimental work going on outside of those three fields, and I came to the conclusion—and I talked at length with Lauritsen and Anderson about this, and both of them felt that we needed to get into high-energy physics. They both agreed, too, that in doing that, we ought to try to build up the strength of theoretical work here.

TERRALL: Just let me ask you one more thing. Right around this period, electrical engineering had been part of the physics division.

BACHER: I should have mentioned that. Electrical engineering had traditionally been a part of the physics division. The spring that I was deciding to come out here, they apparently studied this and came to the conclusion that it was wise to move it over into the engineering division, because it was one of the principal parts of engineering and inevitably would be even more so in the future. Lee talked to me about this before that move was made and before I came out here, and asked how I felt about it, and told me what the problems were, and so on. My reaction was that this was a wise thing for the institute to do. Of course I asked him what other people thought. I think everybody in physics—which was the part of the division that was mainly concerned with this—felt that moving it over into the engineering division was the proper thing to do, and it was done. I think it's been good for electrical engineering. Electrical engineering's ties with physics have always been close. In fact, the chairman of the engineering division [Roy W. Gould] is a PhD in physics and a former member of the faculty in [applied] physics. But that's the way it should be.

Well, this is more or less a summary of what we found here. I did not enumerate the people who were here in Lauritsen's laboratory, and I should. Lauritsen was clearly the leader. Willy Fowler had taken his doctor's degree four or five years before the war with Lauritsen at Caltech and had played a major role in the operation of the rocket project they'd been responsible for during the war. He was an extremely able experimenter and also had an interest in the sorts of theory that apply immediately to nuclear structure, and he was very knowledgeable about it. Tom Lauritsen was also here but, as I recall, he had an assistant professor's appointment for a limited term, and the question was, Would he stay on or not? Well, you know, that's a difficult question, because the idea of having a father and son working in the same area is, generally speaking, not highly thought of in academic circles. My feeling on this subject was principally that you didn't have absolute rules on things of that sort. You might say, well, you don't do it unless there is some really good reason for it. The fact was that Charlie and Tommy Lauritsen worked together extremely well. They were supplementary in many ways and both very ingenious experimenters. Tommy had worked for a number of years in the Bohr laboratory in Copenhagen. As you know, Charlie was Danish by origin, and both of them spoke Danish fluently, so it was natural that he'd have a close association; indeed, their relations with Bohr's laboratory were always very close. We made a decision that we would renew Tommy's appointment here, and if he continued along the lines he was going on, that he'd be eligible for tenure, which happened in due course. So the three of them were really the principal force in the work in nuclear physics. Rather soon, Ward Whaling came out here. He came as a research fellow, about the same time I did [1949].

Now, one other person who was here when I came, and a very important member of the department, was Bob Christy. Essentially, the tie to modern theoretical physics was through Bob Christy. At that time, the field he was working in was nuclear theory. He and Feynman had been our two most outstanding young theorists at Los Alamos. One should be careful about that, because we had so many outstanding people that it's hard to make a statement like that. But they certainly were people immediately called upon if something new and different had to be worked on by some of the younger people. I had seen a great deal of both Feynman and Christy during the war, but probably more of Christy than of Feynman. I had a very thorough respect for him. At the end of the war, I think he went to Chicago. He didn't like it there and he moved out here [1946]. Robert Oppenheimer came here from Los Alamos at the end of the war and had left by the time I came, but I think he had been instrumental in getting Christy to come here. Christy was a very good theorist. He was thoroughly versed and working actively in theoretical problems associated with nuclear physics. In fact, I never considered thinking about anything we should initiate in appointments that had anything to do with new fields or theoretical people without first talking to Christy about it. He had very good judgment and he knew people around the country.

Parenthetically, I might remark that it's very interesting, as someone who is not an expert in the field, to get recommendations—or was at that time—from some of the people who were senior in the field. They sometimes recommended people to me who I didn't think were all that wonderful, and sometimes when I asked them about people who seemed absolutely outstanding, I got contradictory recommendations. I never quite understood this; I guess I don't yet.

Well, this was more or less the situation in the faculty when I came out here, and I think now I've covered almost everybody who was here. I may have left someone out, but not intentionally.

TERRALL: So, from what you said, the response to the idea of expanding high-energy physics was favorable. You didn't have any opposition to that within the division?

BACHER: No, none whatever. I don't know to what extent, for example, DuMond participated in this, but certainly the people who were most intimately involved were supporters of it. In fact, as we got going in this field, the Kellogg Laboratory said, "You have no laboratory? We'll give you some space. We need to get started building some detecting equipment of one sort or another. We'll provide a little bit of support for this." Before we had a contract even, they were helping us. So there was thorough support for this. Charlie Lauritsen had come to the conclusion the year before that they ought to move in this direction. He probably knew that DuBridge had been thinking about getting me out here and that I might be interested in the sort of thing I had started at Cornell. But I think he independently had come to the conclusion that this was an interesting field to go into, and he had hired Robert Langmuir—who's just retired in electrical engineering and who prior to that time was a part of the division and worked with Lauritsen on drawing up some designs. They'd done some work on drawing up a machine that would have a maximum energy of 600 million volts. My conclusion was that that might be about right for a first step but one had immediately to go up in energy, for a number of reasons. The indications were that if you were going to do interesting things, you'd have to get up to higher energy.

TERRALL: When you started trying to hire people, was there any problem in attracting people here? Did you have people turn you down? Was that ever a problem?

BACHER: Actually, we were extremely lucky. We were rather careful to put our emphasis on getting younger people. For example, with Bob Walker, after correspondence and recommendations and thinking about it, I approached him before I came out here. In fact, I think we offered him an appointment, and this was done formally. However, there were people here at Caltech who knew him very well at Los Alamos. Christy knew him, I'm sure, quite well, and knew just where he fitted in. He'd done a very nice piece of work at Cornell. He had gone to Cornell from Los Alamos to take a PhD. Most of the people we did get, we did not approach until after I came out here; however, Walker was approached, and I made a tentative approach to Feynman before I came out here. If I remember correctly, Feynman was appointed to the Caltech faculty starting for the 1950 academic year, but he had agreed to go to Brazil and actually came to Pasadena in the summer of 1951.

After I came here, I soon developed two policies. There never had been a way of getting the faculty in physics together. We started doing this, and I used it as a method of discussing major problems. We would get all of the full professors together for a meeting about once a month. Now, that didn't mean I did all of the discussion or left problems to be taken up there entirely, because we always tried to arrange things to move much faster than that. I also set up another scheme, which was very helpful to me. Every two weeks or so, I'd have a fairly long informal session with Charlie Lauritsen and Carl Anderson, as the principal people in the division with whom I might talk. And I usually checked and talked with Watson about plans, too, but I did that usually separately, because Watson wasn't immediately involved in the research in these areas and it was just more natural to do it this way. While Christy didn't participate in these meetings, I consulted him a great deal about moves we were making, and especially relied on his views, which I had a great respect for, in approaching theoretical people. Right from the beginning, one of our objectives was to build up work in theoretical physics.

In 1950, Matthew Sands came here on the high-energy physics project. He also had been at Los Alamos, and I knew him well there. He was not only a good physicist but probably one of the most ingenious circuit experts we'd had at Los Alamos; he was responsible for a great deal of the work there. This meant we had somebody who was not only broad but could do the technical things we needed. Another person who joined our group that first year in high-energy physics was Alvin Tollestrup. Sands and Tollestrup have left—Sands some years ago and Tollestrup several years ago. We also consulted Ernest Lawrence at Berkeley, and especially Ed [Edwin M.] McMillan and Luis Alvarez, both of whom had been at Los Alamos.

Ernest Lawrence was extremely helpful to us. The minute he found we were going to move into high-energy work, he supported this and thought it was a great thing we were doing. He was very happy about it. I don't know whether in the past, when Millikan was active, there was some feeling between the institute and Berkeley or not. But I can say that all the time I've been here, starting immediately, Ernest Lawrence was extremely helpful to us. His principal contribution in getting our work going in high-energy physics was immediately to say, "Well, if you're going to build an electron synchrotron, why don't you take this magnet we've used as a model for our bevatron up here and work around that? At least the iron will be useful to you." I'm not sure this was in the long run the best thing we might have done, because we ran into some serious troubles because of the nature of that iron. But the fact was that it was an enormous start, and they not only sent that material but they sent down lots of other material they were through using and that had been used in connection with that job. We had to rebuild the machine, especially things related to the coils of the magnet, the power supply, and everything associated with the problem of injection into the machine, and so on. But it was a big help.

When I say this happened fast, by the spring of 1950 those things were coming in; we'd taken over the laboratory that still exists over here; we were starting to remodel it and get things in and get things going. We negotiated a contract, as I mentioned before, with the Atomic Energy Commission, which I think started in March that year. It was a very busy time. I spent a fair amount of time on this work in high-energy physics, especially when we were just getting it started. But let me say that three people—Walker, Sands, and Tollestrup—were really excellent in this and carried a very large measure of the responsibility for parts of it. If it hadn't been that they were so good in this and so very ingenious and sensible, too, we wouldn't have had any work in experimental high-energy physics. They are the people who deserve the credit.

Meanwhile, there was an awful lot more to do in the division. For example, the Caltech pattern of undergraduate education, then as now, depended heavily on introductory physics and mathematics for all of the students. This is something that existed for many years before then, and even thirty years later is still a major part of it. What I found here when I came had been in place for a good many years. The basic education of students coming to Caltech started with two years of physics for everybody and two years of mathematics for everybody—and as far as I know, that's still the case. It's good. There have been worries about this from time to time, but this was a pattern that had been set and was a very good pattern. The content of what was taught in this [curriculum] has changed enormously over the years, and that certainly happened during the first decade that I was here—it changed a lot.

But the problem for physics majors, of which there were many, was that the program of education was an almost fixed program. Almost every course was laid out for the student who started out to get a bachelor's degree in physics; there were almost no electives. I thought this was just too much. I'd had a quite different undergraduate [experience], and I just could not believe that students shouldn't have some latitude in what they did. I didn't think it was necessary, either. I immediately started to approach people about this. The person who was most concerned was Ralph [William R.] Smythe—I'm afraid I didn't mention Ralph Smythe when I was going over people. Well, Ralph Smythe was a very active person, especially in the education of students. He, more than anyone else, was influential in setting the tone of the graduate and undergraduate physics. There are now people in physics who are retiring from their positions in various places who took electricity and magnetism from Smythe when they were here as undergraduate or graduate students.

TERRALL: He taught that required graduate course for years.

BACHER: He taught that required graduate course, and essentially it was a stumbling block for graduate students. It was a terrific course. I don't know whether I could have passed the course or not. I don't know how many of our faculty members could have passed the course. We had one faculty member—and I'll leave him nameless—who had to be excused from the course. He was a graduate student at the time.

Begin Tape 7, Side 2

BACHER: This was not a question of knowledge; it was a psychological block. But Smythe was very influential in this problem approach. I think Smythe thought I was crazy, because he was the one who had set up this rigid system of what the students went through. I spent a lot of time talking to Smythe about this. I don't know that I ever really persuaded him that it was the right thing to do, but at least I persuaded him to do it. As a matter of fact, our undergraduate course is still, by most standards, more rigid than most undergraduate courses. I think a certain amount of rigidity is probably all right. There are more alternatives now. At that time, essentially all the physics courses were specified. I concentrated on trying to have the specification of physics and mathematics courses so that at each point past the freshman year there would be an opportunity for a student to take something completely out of line without taking an overload, because it seemed to me that they needed an opportunity to get out and find out what other things were like. How did they know how they would like to tie their knowledge in physics to other subjects? But I must say there was never any great hullabaloo about this. The first year I was here, Smythe was probably convinced I was crazy, but he was good-natured about it.

TERRALL: Well, I think he instituted that strict course because he was shocked at how little people knew at their qualifying exams.

BACHER: Oh, that's perfectly right. In the early days, Smythe's course was used as a hurdle to ensure that people had a certain degree of knowledge and background and capability to go on. It wasn't so much that they used that work subsequently; it was essentially set there as something they had to accomplish in order to go farther. And you'll find that people who have taken that course, thirty or forty years ago, refer to it as the thing that really sorted out the men from the boys.

There were other courses that played the same role. At the junior level, there was a course in classical physics, if I remember correctly. Then there was also a course in modern physics that played a major role in the senior year. The content of all of these courses changed over the years. In fact, if one looks at the numbers of the courses in the catalog, it doesn't really show what happened. For example, Feynman spent two years revising the entire content of the elementary course. That's the source of these books, which you may have seen [*The Feynman Lectures on Physics*, Addison-Wesley, originally published in 1964]—and gave these fantastic lectures. At that time, he introduced some of the elements of quantum mechanics into the first-year course. Essentially one quarter was devoted to that.

When that elementary course was first put in, students had a rather poor training to go into it. In general, many students practically had no work in physics when they came; some had had rather good courses. But the course from the beginning was set in such a way that some of the elementary things of calculus were taught, so that calculus could be used in the elementary physics course from the beginning. The undergraduate course was certainly a much more thorough course and presented at an earlier level than in most institutions around the country. The introduction of quantum mechanics then came somewhere in the junior or senior year and now comes in the sophomore year. It's relatively thorough, and most students take another course in quantum mechanics before they take their bachelor's degree. So things have moved down enormously, over the years, as to when they're taught. This is, I suppose, in part because there's so much more for students to know in physics than there used to be, and in part because you can't get very far into the subject without having some of these fundamentals, which at that time were out on the fringes of knowledge.

TERRALL: The number of physics undergraduates expanded a lot in the mid-fifties, didn't it?

BACHER: The number of undergraduates majoring in physics did increase. There was a period through there when, with the increase in mathematics and the increase in astronomy and an enormous increase in physics, something approaching half the undergraduate students in the institute were majoring in one or another field [of the division]. Even today, physics is one of the larger majors in the institute.

TERRALL: Was that a problem in terms of teaching staff, to teach the expanding numbers of students?

BACHER: Yes, that was a big problem. One of the points I made when I came was that we had to face the fact that our staff was inadequate for covering the amount of research the institute ought to be doing in physics and we could not have the teaching load as high as it was. The teaching of undergraduate physics at that time was done principally by one man, in each of the freshman and sophomore years, who did the lecturing and ran the course. I don't know how many there are today, but I think there may be four or five in each of the freshman and sophomore courses. There's a much greater concentration of professorial help. Physics has done something which is very good, and that is it has encouraged faculty members to come in from other fields and teach there. I think that's a fine idea. I don't know of any other subject that does that, but it's a very good thing.

TERRALL: But in the fifties, when you were expanding the staff in general, this was for the research end of it, but it was also to be able to handle the teaching load?

BACHER: We needed more teaching at the undergraduate level. One of the first things we did was to break up some of the junior and senior courses with as many as a hundred students into sections, so that they were taught in much smaller groups. Some of these courses had become very popular; everybody in engineering would take a course over in physics at the junior year, and it made an enormous teaching load. This is something that, while it got started earlier, is in much better shape today than at any other time, and it's because the members of the faculty have worked hard at trying to get more senior staff to participate in these courses.

I want to talk a little bit about the other subjects in the division and some of the problems we had. The work in mathematics, when I came out here at the end of the forties, was dominated by a few people, and I'm not sure I can put my finger at this late date on all of the names. But [E. T.] Bell was certainly the senior man. He had definite ideas about what ought to be done. Morgan Ward was one of the senior people. [H. Frederic] Bohnenblust was here when I came and has, through the thirty years I've been here, had an enormous influence on the work in mathematics. There was [Aristotle D.] Michal; he died in 1953. These four were at that time the senior people. There were others who were quite senior. [Harry] Bateman was no longer here when I came, but Bateman had had a great influence on what happened in mathematics and was a tremendous man who worked in a number of fields of mathematics and was also professor of aeronautics, which at that time required a great deal of mathematics. This seems like an unusual thing, but with [Theodore] von Kármán at the head of aeronautics this was natural. Millikan had never really pushed mathematics as a research subject; in other words, he felt that mathematics was something you had and you used it. His approach to mathematics was quite conscious in this respect, and he did not try to develop one of the leading mathematics departments in the country.

TERRALL: So the fact that it was part of the physics division meant it was sort of a subsidiary part, in Millikan's estimation.

BACHER: That's right. The chairman of the division almost always has been a physicist, as far as

I know, never a mathematician.

TERRALL: Was this a problem? Was this an area of conflict—that the mathematicians felt that they were not respected?

BACHER: I don't think there's any question that the mathematicians felt somewhat put upon by the institute. And I spent a lot of time talking to the mathematicians, who incidentally did not agree fully among themselves as to what ought to be done. I spent quite a lot of effort during the fifties—and I talked to Robert Oppenheimer at great length about this, because he was after all at that time the head of the Institute for Advanced Study, which was one of the principal mathematics centers in the country. But it's a difficult problem. It was even a difficult problem for the Institute for Advanced Study. The mathematics people were very independent. The problem at Caltech was that there were certain obligations that mathematics had to take, in terms of the education of students. And it was true that research in mathematics—it hadn't been discouraged, but they never really moved with enormous support. Bell was a very distinguished mathematician.

TERRALL: Was there ever any talk of splitting off mathematics from physics?

BACHER: From time to time a variety of people did talk about making a separate division out of mathematics. I think the mathematicians would have liked this. I've heard this from time to time. The fact is, the institute isn't set up to do that in a sensible way, because the divisions are strong entities. If mathematics had been a division by itself, I think it would have suffered enormously, because it would have had to compete with divisions that were several times as strong. I had tried very hard to offer them a certain degree of protection against the problems they ran into in competing with other parts of the institute. I spent a lot of time trying to find good people here. I just about persuaded a mathematician—Mark Kac, who subsequently went to Rockefeller and was certainly one of the leading people in the country—I almost persuaded him to come out here. Subsequently he said, "You know, I came within an ace of doing that." It would have made a great difference if he had come.

TERRALL: So you were concerned to expand the research in mathematics.

BACHER: I wanted to, very much, and I worked hard at trying to do it.

TERRALL: The problem was attracting people here?

BACHER: It was the problem of attracting people here and getting a critical-size group with interest. The interests of the people here have concentrated on algebra. One of the things I found was that though we got people in theoretical physics who were very accomplished in mathematics, their contacts with our mathematicians were not very great. When they wanted to find out something about mathematics, they went and found it out. This is partly due to the subjects they work in; you can almost never find a mathematician in a physics seminar. The mixing of physics and mathematics has not happened. This is partly historical and partly due to the nature of the subjects, and that doesn't make it easy. If we'd been able to get Mark Kac out here, that would have changed, because Mark Kac talked as easily to the physicists as he did to the mathematicians. That's why I worked so hard to try to get him here, and it's been one of my greatest regrets that I didn't make it go. We also tried hard to get some other people out here, and we drew some people who were very good. But when they came to a point where they were really very good, someone stole them away from us. That's happened in two or three cases. There are people at Stanford and at California who are highly regarded and who came from here.

Now, the interesting part of it is that mathematics [people have] always been very successful in their teaching work. A great deal of this is due to Bohnenblust. They were successful before this, but Bohnenblust was a leader in that, and he saw to it that they did. In recent years, since they started this mathematics competition, Caltech has come out very, very well. Their undergraduate students, a number of times, have won the national competition in mathematics. Our undergraduate majors in mathematics are very highly regarded, and our graduate students have been good. But it's a complicated thing. As I look back over those years of the fifties in the division, I feel almost worse about not having been able to accomplish more in building up our work in mathematics than anything else. It centers on not being able to get Mark Kac out here. If I'd gotten Mark Kac out here, it would have worked much better.

The situation also had another aspect to it. That was that engineering had always been quite strong in applied mathematics. It used to be that there was almost no contact between the pure mathematicians and the applied mathematicians. I had a strong feeling, and was quite outspoken about it, that we ought to have some applied mathematics appointments that were joint between the engineering division and our division. We have some very good people in that area. One of the principal people, who's been here for a number of years, is [Gerald] Whitham. He's a very good applied mathematician, and he holds a joint appointment. Now, having joint appointments doesn't always mean that there's an enormous rapport. But at least it's a step, and we don't have a situation in which the applied mathematicians are all over somewhere else and the pure mathematicians are here. I liked the idea of joint appointments between the two divisions. In fact, Roy Gould, who's now the chairman of the engineering division, for many years held a joint appointment in the division here as professor of physics. I think it's a good way of making certain there are ties between the divisions. Our division perhaps did this much more than any other division.

Now I'd like to talk about astronomy for a bit. Mount Wilson Observatory essentially antedates the institute; it doesn't antedate the institute as Throop [College of Technology], but it does antedate the California Institute of Technology and the modern period of Caltech. In fact, it was [George Ellery] Hale who had the idea that there ought to be a first-rate institution here. What he first did was to persuade [Arthur Amos] Noyes to come out here. After he persuaded Noyes to come out here, together he and Noyes persuaded Millikan to come out here. Once that was done, Caltech was started. But in a sense, the Mount Wilson Observatory antedates the institute. In getting Caltech going, Hale probably deserves the credit. One can't go into who did what and so on—that's not the point. But he really had an enormous influence, and it's probably fair to say there wouldn't be any California Institute of Technology if it had not been for the Mount Wilson Observatory, because that was really where it started.

Now, inevitably, when Millikan got into things and got working at it, he began to think—along with everybody else, including Hale—about what they'd do in astronomy in the future. The Carnegie Institution in Washington had built that observatory up on Mount Wilson and operated it as the largest telescope in the world essentially on its own; they didn't take money from other sources. But they came to a point where, if they were going to go beyond that, something had to be done in a different way, and they approached the Rockefeller Foundation to grant some money to build Palomar. What happened was that they [the Rockefeller Foundation] were prepared to grant that money to the institute, but they didn't feel they could grant it to an organization that was supported by another foundation. So at least Palomar was set up and from the beginning was owned and built and operated by Caltech. This meant that astronomy began to play a much greater role in the institute, because the education was set up down here. Although the staff at first wasn't very large, it always had people who were interested in astronomy. Tolman was interested in parts of astronomy, and Zwicky was interested in parts of astronomy, and there were many physicists who were interested in problems related to astrophysics. Palomar was started in the thirties, before the war. It was stopped during the war, shut down completely, and it was started up again immediately after the war. The dedication of the observatory was in 1948. Here I have failed completely to point out one of the principal people in physics again, and that's [Ira Sprague] Bowen.

He was not at Caltech when I came in 1949. But Bowen had been one of the strongest physicists at Caltech. I think you would say that Bowen was one of the leading experimental atomic spectroscopists in the country. He worked at a lot of the light elements, just as the physicists in Kellogg Laboratory did from the nuclear end. Bowen particularly concentrated on atomic spectra that were of interest to the astronomers. Very likely, Bowen's moving in that direction may have set a pattern for the work in nuclear physics; I hadn't even thought of that until this moment. But Bowen was very good. He was picked to head the Mount Wilson and Palomar Observatories [1946-1964].

Well, Bowen was an extraordinary man. When I came out here, I found the principal thing he was doing was seeing to it—by his own tests, which he did himself and with a helper—that they were improving the quality of the final finish on the mirror on Palomar. I don't know whether it's true today, but certainly before some of the modern telescopes were built astronomers have told me that not only was Palomar the biggest telescope but the best telescope per square inch. This is largely traceable to Bowen; he made it really a high-quality instrument. Credit must also go to the design by Bruce Rule, who was one of their chief engineers, and Mike Karrelitz, who was here during design and construction but gone by the time I came. It's a marvelous job. The people who worked on that telescope deserve great credit. In many ways, that set a pattern for work in the institute, because from that day forward the work at Caltech in astronomy has ranked among the top work in the field in the country. It's partly by having good people and partly by having good equipment. But this certainly was an important step.

Oh, I missed one man in talking about the design of that telescope up there, and that was the man who was really responsible for the building of it in the early days and who was no longer here when I came in 1949. His name was John Anderson; he was already working on this back in the thirties.

Another person who made a contribution which was of great importance to the Palomar telescope was John Strong, a very ingenious experimental physicist. In 1930-31, when I was at Caltech as an NRC Fellow, he was here, too, as an NRC Fellow—also from the University of Michigan. During that year, he became interested in evaporated films and was able to make excellent thin films in high vacuum by that method. A friend of mine, Philip Fogg, was an assistant professor of economics at Caltech then. He had become interested in astronomy and telescope making and had made a telescope with about a six-inch mirror, which he showed me. The reflecting surface was silver, deposited from solution as had been done for many years. These mirrors always had imperfections and soon lost their reflecting power. I suggested we consult John Strong; it might just be possible to make an improvement by putting the reflecting surface on by evaporation. Strong had a fine solution. He evaporated silver onto the surface and then covered it with an evaporated layer of quartz, to my astonishment. The quartz protected the silver and this made a high-reflecting, long-lasting mirror. As far as I know, this was the first telescope mirror made by evaporating the reflecting material onto the mirror surface. Later this method was used on all reflecting telescopes and was used on the Palomar mirror from the start. Aluminum was used instead of silver with a quartz protecting layer, in part for simplicity and in part because aluminum is better in the near ultraviolet and holds its reflectivity quite well.

TERRALL: Over the years that you were head of the division, were there problems with administering Palomar?

BACHER: You know, the two observatories have now separated. I have a certain sadness over this, because there were forces in this direction even during the period in which I was provost [1962-1969], and I tried very hard to put the thing together in a way that would work better. But the forces toward separation became very large. When I came out here, one of the ways the operation was carried out was that there was an observatory committee and two *ex-officio* members—Bowen as director of the observatories, and myself as chairman of the division. At that time, I think, there were two additional members from the observatory and two from Caltech. I used to talk to Bowen a great deal about the fact that we should talk about the research planning in the observatory committee, but Bowen never liked to do it that way. He was glad to talk to me about it, but he didn't really like to get into it in a meeting of that sort. And the observatory committee became a committee that sort of put the rubber stamp on things to be done and particularly supervised the allocation of observatory time.

TERRALL: So that was a division committee?

BACHER: It wasn't a division committee, it was a committee between the observatory and Caltech. When Bowen became director [of the observatories], he left the institute and went full-time as an employee of the Carnegie Institution. I don't know exactly why it was done that way, but it was probably out of the question, since the observatory had been here since the year one, to have the director from one place or the other entirely. In fact, in the charter between the two institutions, it was agreed that the director of the observatories could come from either institution. But while Bowen came from Caltech, as a Caltech professor, when he became director of the observatories he became not a joint employee or a Caltech employee but an employee of the Carnegie Institution, if my memory serves me correctly.

You asked how did I get along with Bowen. Bowen and I got along very well together. The only problem I ever had with Bowen was that he hated to act on any appointments at Caltech in astronomy. He was responsible, not I, for the research carried on at Caltech in astronomy. Things having to do with teaching reported through the division, and things that had to do with research reported through the observatory. But if somebody had to be appointed, connected with research and so on, he'd always say, "Well, you do it, you do it." Overall we got along just fine. I'd known him for a long time.

But it wasn't an easy thing. There were always some observatory problems between Caltech and the Carnegie Institution. These became worse as the years went on. To go into details about that is not really an appropriate thing to take up in this [interview]. But it wasn't easy. But I never had any feeling that Bowen and I didn't see completely eye to eye on what to do. Bowen was very glad to come and talk indefinitely about research plans between the division and the observatories. He'd talk about anything; he was very free about it. But to use the observatory committee as a way of discussing plans for the future just didn't work. I don't know why.

One of the things that happened that also affected astronomy was that along in the middle of the fifties, [Edward] Purcell at Harvard observed the hydrogen line in radio astronomy. The minute I heard about this I thought, "Gee, that's where we've got to be." I talked to DuBridge about it, and he agreed completely. To my great amazement, this was not something the astronomers thought was essential. I talked to Bowen about it, and essentially Bowen's view on it was, "We don't know anything about those techniques." Well, that was true. He said, "We feel it's just out of the question to move into that field." I said, "Gee, this opens up a whole new field. Astronomy is limited at the present time by the furthest thing you can get on your telescopes down into the ultraviolet and on up into the infrared as far as you can photograph it." And I said, "Here immediately you've got many, many octaves of spectra added on on the long-wavelength end." Of course, it didn't continue on directly. It now almost overlaps. There had been work done, essentially using infrared methods with radio waves, which went back to the early thirties. The doubling of the lowest state of the ammonia molecule had generated wavelengths down in the centimeter range, and they had been detected essentially by radio methods. So the idea of a subject related to this wasn't new, but the finding of a hydrogen line at fourteen centimeters, or whatever it is, was something entirely new. This struck me as something we really ought to pursue.

I talked to DuBridge about it and to the people in astronomy. There were various reactions. Bowen said he thought it was a good idea to do work in this field. He certainly had no objection to its being done. But as far as he was concerned, not at the observatory, because he felt they didn't know enough to take up this field. It just looked too hard and complicated for him. So we decided that we ought to talk to Van [Vannevar] Bush and Merle Tuve—Bush being the head of the Carnegie Institution of Washington and Tuve being head of the part of the Carnegie Institution that worked in radio astronomy even then, along with a number of other subjects. Well, they thought it was a good thing to do. They didn't particularly want to get the observatory into it directly. Their advice to us was to set up something across the street, in the park over here, that was a dish about ten feet across and start at it this way. Quite frankly, that just didn't seem to me to be enough of a start. Well, the upshot was that we then talked to Taffy [Edward G.] Bowen—another Bowen—who had been at the Radiation Laboratory at MIT during the war and was a friend of mine and an even closer friend of Lee DuBridge. After the war he had gone to Australia and set up a radio astronomy observatory. So we got him to come to Pasadena to advise us. His advice was clear: “You *must* go into this field.”

ROBERT F. BACHER

SESSION 8

August 11, 1981

Begin Tape 8, Side 1

BACHER: In starting to get into radio astronomy, our main problem was that while we had people who were somewhat versed in the field of centimeter radiation, they were not in astronomy and hadn't been working in this field for a long time. So we decided it would be essential for us to get somebody to lead the work from outside. Taffy Bowen, who was head of the program in Australia that had been so successful in working with microwaves, recommended John Bolton, who was his right-hand man over there. This seemed very good to us, and after talking with Bolton, we engaged him to come here and start a project, contingent upon our being able to raise some funds. One of the problems was how big a scale should we go into? Our leanings here were to do something larger than had been recommended by the Carnegie people—Bush and Tuve—when we talked to them in Washington. When Bolton came here, he had definite ideas of what we ought to be doing and agreed that we ought to start something on a larger scale. His recommendation was for something new, and that was to try to get two reflectors that would work together as an interferometer. A whole new development in the circuitry of the receivers had to be worked out here, in order to do this. Fortunately, Gordon Stanley, who was expert in this area and anxious to do something in this direction, came along with Bolton, or shortly thereafter, and played a major role in developing the Caltech program in radio astronomy.

The first problem was to look for a site. Bolton did a very thorough job of looking this over and fortunately found out that if one located up in the Owens Valley, one could get onto land owned by the City of Los Angeles and they were happy to have somebody in there. They didn't want to have farming in there, because they used it as a water reservoir. We were able to work out an arrangement—which I think must be close to expiring now, but I'm sure will be renewed—that for a dollar a year a sizable site could be rented up there. And indeed that's where the observatory [Owens Valley Radio Observatory] was put and it's still there to this day. The limitations that came from the amount of water locally available meant there wouldn't be enormous industrial or manufacturing growth in that area. So the two things fitted together. It has proved to be a very good site.

To get this started, we obtained funds from ONR [Office of Naval Research] and put some trial reflectors up at Palomar. The people at the Palomar Observatory, and Bowen in particular, were very helpful in providing facilities up there to help us get started. But it was fully realized from the beginning that that wasn't a good place to have a radio observatory, because there were lots of planes flying around all the time using radio sets and radar and what not.

The radio observatory at Owens Valley has turned out to be a very good one and some fine work has been done there. It's going strong today with work that [Robert B.] Leighton is now doing, down into the millimeter range. With other work that's going on in Caltech at the present time, almost the entire spectrum is covered; Gerry Neugebauer has been carrying on work in the very far infrared. What once was a field in astronomy where the light waves that were studied were mainly of wavelength in the visible range has now been extended from the infrared up into the sub-radio range. This has turned out to be a very fruitful field.

One other thing we tried to get started when I was chairman of the division was some new work in low-temperature physics. This had some help from the synchrotron laboratory, because the problems we wanted to study in high-energy physics essentially involved using a liquid-hydrogen target. It turned out that at that time there wasn't any liquid hydrogen in Southern California at all. If we were going to get liquid hydrogen, we were going to have to make it ourselves. So that's exactly what we set about doing—getting a liquefier and making liquid hydrogen. But the same Collins liquefier that would make liquid hydrogen could also make liquid helium, which was the primary material needed to start a program in low-temperature physics. It seemed to me that this was something we ought to look into, to see whether we ought to start work in that field.

TERRALL: Was there anyone here at that time who was working in that area?

BACHER: There had been work in low-temperature physics under [Alexander] Goetz, and he had a liquefier. I don't know whether this ever turned out to function on any sizable scale, but he had done work in low temperature. That work had been inactive for quite a few years, so when we took it up, we essentially made a fresh start at it. Goetz was still here but working mostly in other areas.

The decision to go into this field was not taken until after we'd consulted a number of people in various locations, including a man from Holland, [Cornelius] Gorter, who was one of the leaders of the field. It was finally recommended that we get John Pellam, who was at that time one of the leading people in low-temperature physics and was at the National Bureau of Standards in Washington. So Pellam came out here [1954]. This was not a complete success. Pellam was always a little unhappy with the fact that the liquefier belonged to the high-energy people. We ended by getting the government to give the liquefier to the other contract under Pellam. By that time, we were able to buy liquid hydrogen out here and it didn't make much difference anyway. Pellam did some very nice work. But more serious was the fact that Pellam became interested in other fields after a while and left Caltech to go off into industry. Subsequently he wanted to return, but this was not something that seemed feasible, because Pellam's interests and the department's interests had deviated quite a bit. But the work has continued. It continued with [James E.] Mercereau, who was one of Pellam's students and who is here now as professor in that area, and also has been expanded with the addition of [David L.] Goodstein, who came here in 1966. Now we have very strong work in that field, but it's not an enormously large undertaking.

One subject I wanted to mention something about was visiting lecturers. When I came out here, I asked Dr. DuBridge if we could have a fund to get visiting lecturers out here, because it seemed to me that we suffered a bit from being so far away from other sources of high-energy physics, and in fact all fields of physics. Caltech had had visiting lecturers in the past, but this had been interrupted by the war and hadn't been started up afterward. We restarted it with a vengeance, right away. The list of visiting lecturers, just from what I can remember without looking anything up, included Niels Bohr, Fermi more than once, Oppenheimer more than once, Bethe, Feynman, Purcell, Rabi, [Victor] Weisskopf, Alvarez, McMillan and [Felix] Bloch for shorter periods, Gorter in low-temperature physics, and [Nicholas] Kurti, also in low-temperature physics, from Oxford. During the early part of this period, [Murray] Gell-Mann came sort of out of the blue one year, before Christmas. We heard him give two lectures—this wasn't something that had been arranged—and we were all so impressed that within a few weeks we had engaged him to come out here, and that's how he came to Caltech. That was in 1955.

TERRALL: It was his visit here that got everyone interested.

BACHER: Oh, yes, there wasn't any doubt about it. We had heard of Gell-Mann, of course. But he visited here, gave one or two very interesting talks, and we immediately broached the subject among ourselves of doing something about it. At that time, when we decided to do something we usually tried to move fairly fast, and this was all done and settled in a matter of a few weeks.

TERRALL: And he accepted right away?

BACHER: Well, it took a little time, but the whole thing was settled in a few weeks.

TERRALL: He was eager to come out here?

BACHER: Yes. He came out as an associate professor in 1955. In addition to Christy, we now had Feynman and Gell-Mann here, with some younger people. So we had a very good start on a vigorous theoretical group.

I want to say a few things about what was going on in the meantime in the experimental high-energy physics work. I mentioned our getting the electron synchrotron started. What we did in terms of research had been imagined from the beginning. Our plan was to try to observe some of the simplest nuclear reactions with a photon on the proton to see what would come out. And indeed, it was with the synchrotron here that it was fully established that what appeared to be a cross section which increased with energy for the interaction between a photon and a proton, producing a pi-meson and a neutron, turned out to not only go up but also come down, so that it really was an elucidation of the first nucleon resonance. This is work in which Bob Walker played a major role, but also Matt Sands and Alvin Tollestrup were very much involved. A number of further experiments were carried out, and, indeed, for about fifteen years the synchrotron was a very productive device. At one time, interestingly enough, it was the highest-energy accelerator working.

TERRALL: How long did that last?

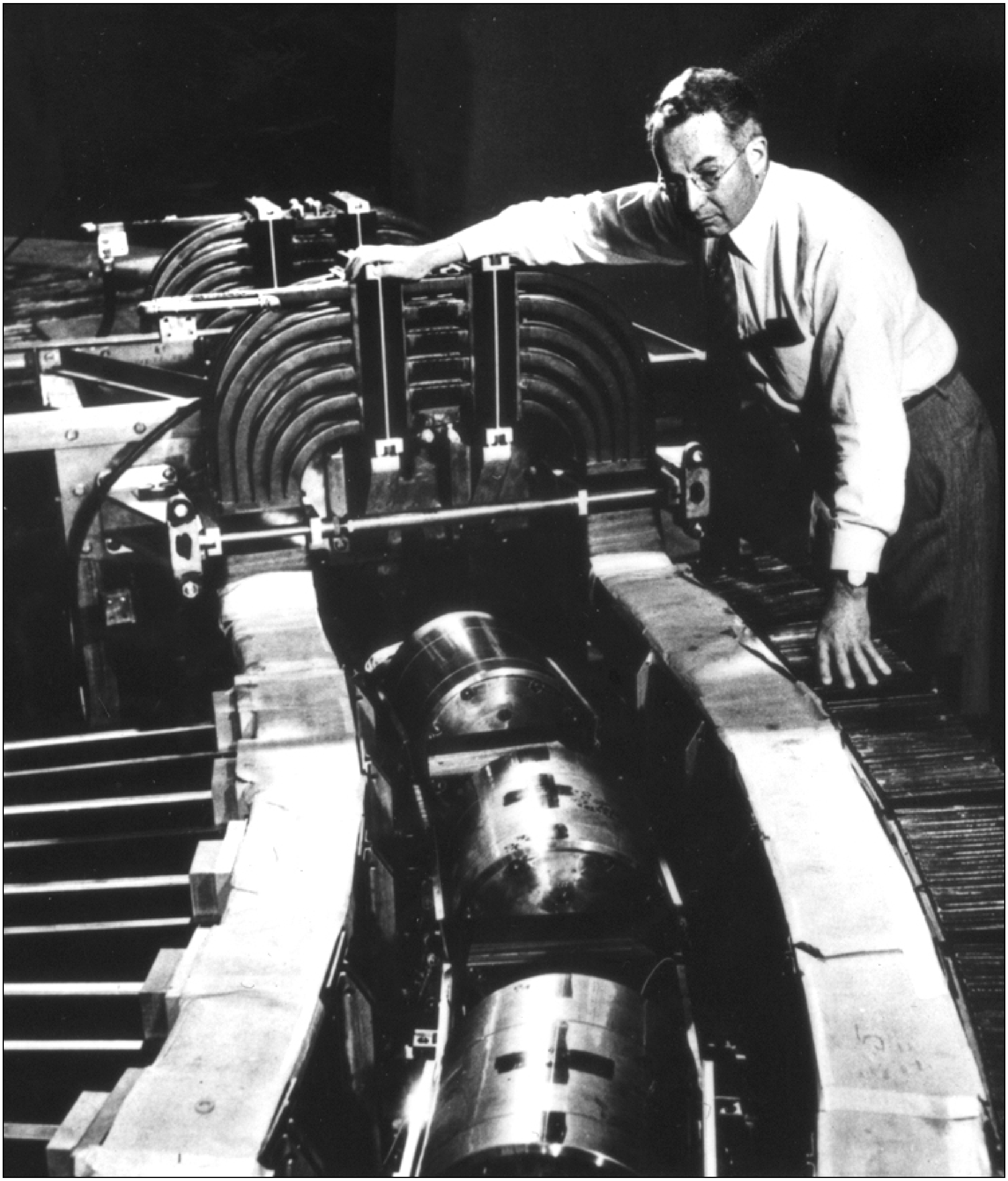

Fig. 4. Bacher examines the Caltech Synchrotron, ca 1950-55. Caltech Archives.

BACHER: That didn't last very long. [Laughter] We finally got it up to the top energy it could attain. If we were going to continue in the field, we had to build a completely new machine. Indeed, we were given some inducement to do that and could have, I believe, obtained financing to build a machine of about twenty BeV. That's not certain, but it looked very promising. But when we got down to considering this with all the people involved, it turned out that the problems they were interested in by that time almost certainly required higher energy, and we couldn't see any good reason for trying to work for years on a machine that, before you started, was not going to be the energy you wanted to work with. So we put that aside.

TERRALL: This was the early sixties, when you reached full energy on the synchrotron?

BACHER: I don't know just exactly when it was that we reached full energy. We got the machine going well in the early to mid-1950s. I think it may have been in the late fifties when we got up to full energy. The machine was not shut down until about '69, but by the early sixties we had come to the conclusion that if we were going to continue in high-energy physics for the long run, we'd better get started sending people away to work on some of the really big machines. I think that was a wise move at the time, and in due course all our work turned in that direction. We now have people who work at most of the laboratories around the country, and some work has been carried on in Europe.

TERRALL: So during the time when the synchrotron was doing its best work, you were director of the synchrotron at the same time as being head of the division.

BACHER: Well, I don't think we had a formal director of the synchrotron laboratory. What we had was a head of the synchrotron project, and I was the head of the high-energy physics project and essentially responsible for the contract we had. We kept both the experimental and the theoretical work together. Every once in a while, people thought it might be a good idea to break it up into a number of smaller fields.

TERRALL: On the same contract, you mean?

BACHER: Yes. It still is all under the same contract and involves much less red tape in dealing with the government. Our report became more or less of a form, which Walker continued in subsequent years when he took over the project. Formally, I continued as project director for a good many years, but he took over the actual management of the project. There were some reasons for this—not having to do with his ability to run the thing but rather with the question of salary charges to the contract.

The work was very good work. And our method of dealing with the government was exemplary as far as our end of it went. The report for both theoretical and experimental work, essentially the principal report, was less than ten pages long. But when it went out of here, it went out with a complete list of everybody who'd worked on the project and details about when they'd been here and all of the preprints that had been done under the program, and the thing would stand about three feet high though the report itself was only about ten pages long. They were quite satisfied, because they had a full record of what happened, but we weren't involved in writing a long report; it consisted in the main of the actual material that had been produced under the contract.

TERRALL: So during those years, there was never any questioning of the money you were asking for?

BACHER: Well, that would be an overstatement; there's always questioning. But quite frankly, when money seemed to get short, I would say to the people from Washington, "Well, now, we think we're doing reasonably well; we don't spend very much money. Can you tell me anyplace in the country where you're getting more per dollar spent?" And they never could. So we usually had a pretty good argument for them. We were a relatively small project but a lot of work came out of it. We tried rather hard not to do things in an overly elaborate way. On the whole, it was a good thing for us, as a relatively small laboratory, to do it that way. Our relations were always very good with the people from the Atomic Energy Commission, and they were very supportive of us. There's no doubt that that contract would not have continued if it had not been for the fact that it did produce very well, and this is a credit to the people who were actually doing the research.

TERRALL: I wanted to ask you about the Institute Academic Council, the group of division chairmen that met with DuBridge.

BACHER: I don’t know just exactly how it got started. I was under the impression that DuBridge started it as an informal get-together, and then it became a much more formal organization.

TERRALL: So they met, what, every month or so?

BACHER: Yes, about once a month.

TERRALL: And how did you think that worked? Was it productive?

BACHER: Yes, it was very productive.

TERRALL: We haven’t said too much about DuBridge and his administration.

BACHER: Let me say a few words on that subject. DuBridge was a fine administrator. One of his greatest points was that he kept to general policy and turned to the people who were responsible to carry it out in a way that was left very largely to them. He was quick to back new ideas if he thought they were productive; in general, I don’t recall going to him with an idea to move off in some new direction, or strengthen some existing work, when he didn’t give it full backing.

Some of the divisions traditionally stayed off by themselves; physics had always interacted more with some of the other divisions, because there had been people who had come through physics who’d gone into most of the other divisions, so there always was a fair amount of contact with people who by training or experience were physicists. However it was started, the idea of getting the division chairmen together was a very good idea, and this was started originally with the idea of exchanging views on plans for the future. As it continued, it became a much more formal organization, dealing in a much more formal way with institute policy. The early way of its working meant that it seldom became a bottleneck in getting something done very fast, because you were focused on long-range plans, and what you were going to do in terms of a particular new appointment wasn't immediately covered by that.

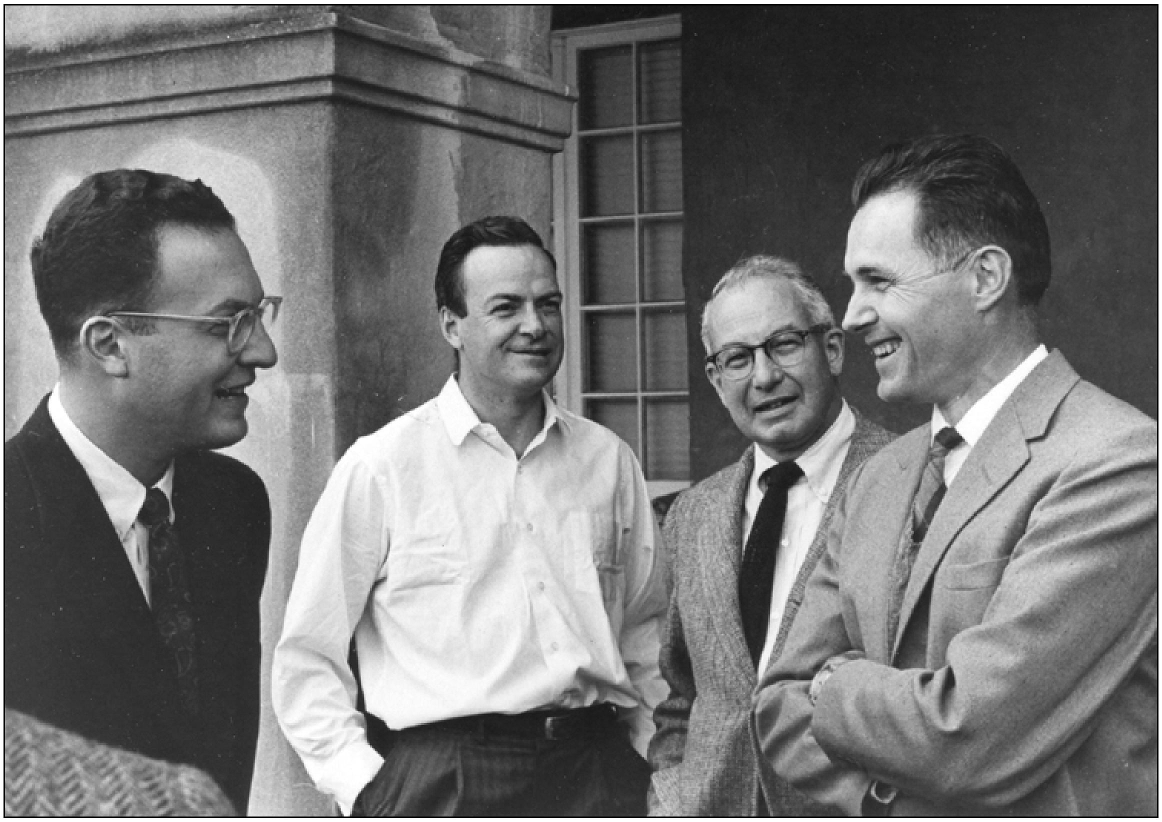

Fig. 5 Robert Bacher enjoys an informal conversation with three eminent physicists. L-R: Donald Glaser, Richard Feynman, Robert Bacher and Robert Leighton. Caltech Archives.

We had a policy in the Division of Physics, Mathematics, and Astronomy during the time I was chairman that whenever there was any appointment up, if we knew of senior faculty members in other divisions who had an interest in the subject in which we were making an appointment, we consulted them. We didn't necessarily go directly to the chairman of that division for the consultation—we went to the people who were directly interested in it but told the division chairman what we were doing. The point was that we felt if we were going to have collaboration between the divisions we ought to have an idea of what the views of other faculty members were of the people we were going to appoint. Now, that didn't apply to most of the subjects we worked in; there was practically no one else interested in astronomy, for example, elsewhere in the institute. There were some people interested in mathematics, especially in applied mathematics, and we tried to cultivate that link. And in physics, in some fields there were ties and in other fields there weren't. Inevitably, as geology became more and more associated with the planetary work, there was more of a tie to the astronomical group than there was earlier.

TERRALL: The faculty Committee on Academic Freedom and Tenure you were on—I wanted to ask about any particular problems that came before that committee.

BACHER: Well, I hadn't put that down in my notes, and I might say a few words about it. When the crisis occurred at Berkeley—the loyalty oath—the Caltech faculty took it up and decided that they wanted to have a Committee on Academic Freedom and Tenure. This was in the early days of the McCarthy era. Through some quirk of fate, I was the first chairman of that committee; in fact, I served two terms as chairman. I felt this was both a difficult and, as far as I was concerned, an unknown job to tackle, so I took some time to read what had been written about the Berkeley situation and to go up and see people at Berkeley and find out why it had gotten into the situation it ended up in. I found it was absolutely essential that one have a way of dealing directly with this, because there was evidence from the work at Berkeley that the positions of the opposite sides had actually switched back and forth at various times but nobody agreed about anything. So we tried to proceed in a way in which in an emergency we would have some direct consultation. And indeed, that did happen; there was a case that came before the committee, and I think it's better not to go into the details, but the committee acted in a way and met with a committee from the trustees. And the views expressed and advocated by the Committee on Academic Freedom and Tenure were supported by quite a few influential trustees and finally became institute views. But there was a bit of a touchy time. Two or more trustees resigned as a result of this episode.

TERRALL: So the committee was really set up to forestall the kind of situation that got out of hand at Berkeley?

BACHER: That's right. It was essentially set up to avoid getting into that situation, and we studied what went on at Berkeley to see what had happened to make the situation so bad.

TERRALL: But then it was a question of waiting for cases to come before the committee.

BACHER: Oh, yes. The committee was not set up to take the initiative on subjects of this sort. The committee was set up to handle a situation that came up and think ahead about what it would do in a given situation. And indeed, the trustees in coping with this, had a great deal of latitude—they didn't have to come to us. We didn't organize the trustees. The trustees, when the situation arose, set up a committee and asked that committee to sit down with our committee and discuss the situation. It was an almost unheard-of situation to have a committee of a half a dozen trustees or so sitting down under the chairmanship of one of the members of the board and calling in a whole committee elected by the faculty. A great deal of understanding of different points of view came out of that interaction, because in general trustees have some difficulty understanding faculty views. Unless these are stated clearly and there's complete rapport and full discussion of it, it's not easy.

During this period, I was also involved in various sorts of work for the government. First of all, I continued as an advisor to the Atomic Energy Commission, but this really came up only on special occasions, when something arose that they wanted to talk to me about. I did not ever belong to the General Advisory Committee, though I was invited once to join it. But some of the time I had rather close ties with them. I was chairman of a committee in the Department of Defense—an advisory committee on nuclear problems—for several years, and I was a member of this committee for some time. Robert Oppenheimer was a member; I think Hans Bethe was a member. This committee considered specific problems that were brought to us by the military for trying to move ahead in various directions. There was always a problem with the military. All of the branches of the military had some feeling that they wanted to be preeminent in the field of nuclear weapons. This immediately biased their approach to problems, because they wanted to compete and be able to do exactly what another service would do. This created more than one awkward situation and got us off sometimes on things we really shouldn't have been doing. But it was the nature of the Defense Department at that time. There was also a tendency to take up subjects that were so expensive that by the time they came around they just couldn't be done as originally expected. Probably the most important of these was the nuclear airplane, on which I don't know how much money was spent, but something like $2 billion, or perhaps even more. We steadily recommended against this. Over the years, there must have been a dozen or more surveys of this subject, and I don't know of one of them, made by technical people, that came up with an affirmative recommendation. This is not because people were against it; it was just that it was an extremely difficult problem, and there still are not any nuclear planes. The nuclear submarine was doable, and also militarily was a very good move.

Caltech in 1951, during the Korean War, was asked to start a project that was primarily concerned with tactical military problems.

TERRALL: Was this the Vista Project?

BACHER: Vista Project. The institute was reluctant to start a classified project on the campus, so everything associated with it would be moved off and done at the old Vista del Arroyo Hotel, which was then vacant; that's why it was called the Vista Project. I was the chairman of a small committee that wrote the nuclear part of this. We made some rather strong recommendations along the lines of getting into tactical weapons. This was not a popular subject with any branch of the military, so they would come out here, one after another, to try to talk us out of this.

TERRALL: What were your recommendations?

BACHER: Our recommendations were mostly that there were tactical applications of nuclear weapons, and that we ought to think about those and be prepared for them and not just depend on strategic use. There was a belief at that time—and I think, as a matter of fact, it's still true—that nuclear weapons used in some tactical situation do not necessarily involve the complete release of all strategic weapons in one great boom. However, the view has persisted that the use of one weapon somewhere is going to set off all the weapons in the world. I think that's a view that, one way or another, we've got to get past. This was certainly one thing we worked at in trying to work this up. There were opportunities also for kinds of missions using small weapons, which are perfectly feasible with nuclear weapons—relatively small weapons that are tactically very good. This was a rather important project. As far as I know, that report was entirely suppressed. Caltech does not have a copy of it anywhere, that I know of, and I don't know of anybody that's ever seen copies of it around the government. Sort of an interesting situation. They were all gathered up, very clearly.

TERRALL: You mean the full report at the end of the project?

BACHER: The full report at the end of the project. Caltech does not have a copy of it.

TERRALL: Wouldn't you have had a copy yourself?

BACHER: No. All of our copies were turned in. And all the things that I had connected with it were destroyed; all the draft papers and everything[2]. To the best of my knowledge, there is not a full copy of the report at Caltech, and it was never circulated by the Department of Defense.

TERRALL: Was this because the recommendations you came up with were just not what they wanted to hear?

BACHER: Well, I don't know exactly why. I think they didn't want to hear some of the things that were said. They didn't want that sort of thing out.

Another thing I was involved in during this period was the President's Science Advisory Committee. When that committee was set up in the early fifties, it reported to the President and was referred to as the President's Science Advisory Committee—PSAC—but it didn't really report directly to the President, it reported to Arthur Flemming, who was one of the assistants in the White House and reported to the President. He was head of a group called the Office of Defense Mobilization. I served from something like 1953 to '55. Subsequently, when the Russians put up *Sputnik* [1957], there was an enormous upheaval in Washington. They said they hadn't been paying enough attention to getting new things done and so on. On this occasion, the committee was requested to report directly to President Eisenhower; in fact, at that time they did some revising of the committee and I was asked to go back on the committee for a term, just at the time that it was set up this way.

TERRALL: What was the role of the committee, let's say, before *Sputnik*? What kinds of things were you advising on?

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[2] Or at least so I thought. It turned out however, that a hand-written draft of the section I worked at persisted and came back from Los Alamos declassified recently. - R.F.B.

Fig. 6. The President's Science Advisory Committee (PSAC), 1953. Seated next to President Eisenhower is Lee DuBridge (on the President's left) while Robert Bacher stands behind DuBridge. Photo by Abbie Rowe/National Park Service. Caltech Archives.

BACHER: A large fraction of the things the committee worked on were problems having to do with defense. In fact, the committee would go in and see the President from time to time, and they put on a number of studies of one sort or another. We tried to be guided by questions he asked. Once, I came out of a meeting and realized that the President had asked a question we hadn't been able to answer. When we got downstairs, I pointed [this out]. It seemed to me that we ought to try to answer him, so that was the beginning of a big study. This was the kind of thing that happened, mainly having to do with defense problems. The PSAC was not confined to that, but these were the problems that people thought ought to be worked on, and that's what got worked on.

When the committee was set up directly under the President, it operated in a somewhat different form. Almost all the members spent a significant amount of time working on various subjects for the committee. I worked on a number of problems. I mainly was involved in the nuclear weapons problem. I spent almost half my time in Washington during that time—

certainly more than a third. Very often, I'd be there for a period of two or three weeks. Then later, we went off to Europe for some negotiations, and I was in Europe for two months and then went back for another month in the fall. So that's three months out of one year that I was involved, and we spent a couple of months almost full-time getting ready for these talks. These were the first talks.

Begin Tape 8, Side 2

TERRALL: You wanted to talk about going to Geneva. That was '58, I think, wasn't it?

BACHER: Yes, it was the summer of '58 that we went to Geneva. This turned out to be a major undertaking, having to do principally with nuclear weapons tests. It was very difficult and didn't really get anywhere, but if it hadn't been for those negotiations it would not have been possible subsequently to get ahead in this area.

TERRALL: That was really a large part of the summer, wasn't it?

BACHER: Well, we were there all summer. This was decided, I think, sometime in April or thereabouts, and we spent most of May and part of June getting ready for it and trying to get a position worked up and getting information. We quite frankly were not very well prepared for how the Russians would react. They came in with lots of definite proposals and they didn't have much technical information to back them up. We had an extremely good staff built up over there, and we dealt with problems on an almost day-to-day basis. We had some really extremely good people there, both from Britain and from this country. The British and ourselves and a few Canadians led the main part of our side; the French helped some, but not as much. It was a very interesting negotiation and was an essential step in getting ahead, although it really didn't accomplish anything.

TERRALL: Was this because the Russians were just not talking in the same terms? Was it that kind of a problem—they weren't responding to the kinds of things that you were suggesting?

BACHER: It's awfully hard to answer that question. They were essentially, one after another, raising questions as to whether you could really monitor [nuclear] testing or not. If you agreed not to test, how were you going to find out that it didn't happen? One of the principal problems was that it could work if you had inspectors that could go in, but the Russians didn't want any inspectors in the Soviet Union. That's a very difficult situation, and I think has been one of the principal reasons why there hasn't been an agreement over the years on this subject. Now the testing is underground, and the detection methods are sufficiently good that it's very hard to set off much of a bomb anywhere without somebody knowing something about it. The monitoring of testing has been greatly aided by the satellite program in the meantime, so the situation is different from the situation that existed then.

Fig. 7. A trio of American nuclear scientists, including Robert Bacher, was chosen by President Eisenhower to discuss ways of policing a nuclear test ban. They met with Russian scientists and others at the Palais des Nations in Geneva, Switzerland in July 1958. Photo by the U.S. Information Service. Caltech Archives.

TERRALL: When you were spending these huge chunks of time away from here, how was that working out?

BACHER: Well, it wasn't good. The division was by that time quite well organized and operated in sections that were coherent, and they got ahead very well, but it wasn't a good idea for me to be away so much. When I went away, Earnest Watson usually served as acting division chairman, and he of course knew everybody very well. I had always talked with him thoroughly about what we were trying to do, and I wouldn't have been able to do this advisory work if it had not been for Earnest, because he was completely objective about all the problems and could take a detached view. This was essential if someone were going to fill in, and he was very good.

TERRALL: You got involved in all this from your own feeling of commitment that it was an important thing for you to be doing, is that right?

BACHER: I wouldn't have done it if I hadn't been convinced that this was an important thing to do. I was thoroughly convinced that the country ought to have a PSAC. We met fairly frequently with President Eisenhower and he was extraordinarily frank with us. The degree of his frankness with us at the time of the McCarthy hearings almost made one gasp. I heard him speak for nearly half an hour on this subject one time. If anybody had ever told me I'd hear that in the Oval Room of the White House…

TERRALL: What kinds of things was he saying?

BACHER: Well, he was very frank about how dreadful he thought McCarthy was and the dangers of what McCarthy was doing. But he felt equally strongly that it was very important for him not to take an anti-McCarthy attitude in public. His reasons for this were complicated, but he didn't want to give McCarthy the chance to oppose the government. He could oppose people, but he didn't want McCarthy to have the opportunity to strike at the heart of the government. He succeeded finally in creating a situation in which McCarthy completely discredited himself. Now, whether this was the right way to do it or not, I don't know, but that's what he did. I didn't realize, until I heard him speaking, how strongly he felt on this situation. He must have had to hang on to himself completely in order not to have his feelings come out, because I'm sure we were hearing what he really thought.

TERRALL: During that period of the McCarthy hearings, when things were really heating up in Washington, I'm wondering what your recollections are of what it was like around here?

BACHER: Well, people were perturbed. There were a great many people who were hurt very hard by this—not the least of these was Robert Oppenheimer. Several of us here at the institute testified for Robert Oppenheimer in the hearings in Washington [1954], and this was a dreadful affair. The case is a discredit to our government, because as far as I know there was essentially nothing new brought out in those hearings that had not been before the Atomic Energy Commission at the time of his original clearance. During the war, clearance was handled in a very easy way, and there weren't any chances for hearings, and so on. One of the things the Atomic Energy Commission did was to set up a hearing system, but it was very difficult to carry out. It sounds easy, but it was difficult to get a complete picture on something of this nature.

But I don't believe there were things brought out in the Oppenheimer hearings that weren't known when he was cleared unanimously by the Atomic Energy Commission. I participated in that clearance, and I had very strong feelings about it. The attack that was made on him subsequently did not give as much credit to the hearing system of the Atomic Energy Commission as one would like, because it was very difficult to know what was going on. The lawyers who were acting for Robert Oppenheimer had very little access to the material, and if there were questions about highly classified material, most of them had to be excused from the room. I think there was at least one who was cleared, but it was not easy. It was a very difficult thing, and it's certainly something in history that our country cannot be proud of.

A wide range of people testified in favor of Robert Oppenheimer. These included [Dean] Acheson, [George] Kennan, Groves, Conant, Bush, Lilienthal, DuBridge, Fermi, Bethe, Rabi, Lauritsen, and many more. I testified for him. I'd been off the commission then for a few years, but I had worked with him very closely during the war. There were some physicists who testified against him. That's something I don't want to go into here; it's not a part of Caltech.

TERRALL: But when you and the other people you mentioned were testifying on his behalf, were you basically ignored by the committee?

BACHER: Well, I wouldn't say that. For example, when I testified, they tried very hard to keep on extraneous subjects. The most extensive subject they asked me about was Bob Serber, who is a well-known theoretical physicist who was a very close associate of Robert Oppenheimer's, and either one or both of the Serbers may at some time have been members of the Communist Party. But what that had to do with Robert Oppenheimer was not entirely clear. Serber was at Los Alamos, his wife was at Los Alamos during the war, but their clearance was not a question for Robert to decide; the clearance was decided by the military people responsible to Groves and if they didn't want to clear them, that was something else again. That sort of approach to a problem becomes an innuendo situation. One of the people who instigated some of these hearings was one of the generals in the air force who objected to the views that Robert Oppenheimer had taken. In fact, he coupled me with Robert Oppenheimer as espousing the same views, and indeed I did. But my background of association was not the same sort of a background that Robert Oppenheimer had, fortunately. This was not a pleasant situation. There's a great volume of hearings, which cannot be covered in a few minutes of this interview.

TERRALL: Were there other people around Caltech who felt the potential for being threatened by McCarthy?

BACHER: Well, I don't know the answer to that question. In general, people who'd been around Caltech weren't at that time much involved in this. A little earlier, there had been someone around—[Sidney] Weinbaum—who got into difficulty. He had been at Caltech, and I think there were some others. But mostly, those people weren't here at that time. I can't remember at the moment anyone who was seriously concerned, though this was something that concerned everybody, in the general terms that you couldn't tell where it would go. And the record shows that some of us were pretty outspoken on this subject.

One episode that does relate to this concerned Linus Pauling. As you know, the use of innuendo in the McCarthy hearings became highly developed in this period. McCarthy's threats, some made publicly and others not, threw special fear into branches of government not dealing with classified material. They had neither experience nor regulations in these areas. These attacks and the publicity they received encouraged other state and national committees to conduct hearings to seek out and expose scientists, and many others, too, who had associated in the past with Communists or who had other left-wing attachments.

One such committee had been active in California and had attracted a great deal of attention, especially in university centers, calling in faculty members and others and asking them to testify under oath whether or not they were Communist Party members. They held some hearings in Pasadena and called Linus Pauling to appear. He avoided their direct question about whether he had been a member of the Communist Party, on the grounds that it was improper. The committee dismissed him, but it seemed likely that they would cite him for contempt the next day. This all appeared in the evening paper and looked like a real impasse. As I read the story, it struck me that some considerable time earlier Lee DuBridge had mentioned to me that Linus had voluntarily written him a note saying quite definitely that he had never been a member of the Communist Party. This made it clear that Linus was refusing to answer because he believed they had no right to demand that.

Early the next morning, I checked with Lee, who confirmed my memory, and then went to talk to Charles Lauritsen, who had also known Pauling for many years. He agreed both as to the emergency and to the proposal that Linus voluntarily take a copy of his earlier statement to the committee. We hurried to see Pauling, who realized somewhat, but not fully, the precarious position he was in, though he did not regret his action. He had apparently not remembered the earlier statement but had no objection to taking it to the committee. He called them and asked to come in to see them at noon. Word of his coming citation by the committee had already leaked to the press representatives outside, and they asked what he would do. He took in his earlier statement and that stopped the citation action. It was a close call.

In 1961, I asked for a leave, and I was informally granted a leave of six months. I was tired after all this work in Washington, and I needed to think about what was to be done. Prior to this, Lee DuBridge began to feel that the top administration needed some help. We have quite a group of top administrators in the institute now, but at that time it went directly from DuBridge to the division chairmen. Ernest Swift, who was then the chairman of the chemistry division, felt that something ought to be done in this direction, and he talked with me about it. I thought we had reached a point where the institute had grown beyond its rather limited administration capabilities. Also, there were things that Lee DuBridge had to do outside; he was the principal outside contact for the institute directly, and this put a crimp in things. So Lee, Ernest, and others thought about it and concluded it might be a good idea to get somebody here to be responsible for academic coordination and planning. It wasn’t clear what to call this person, but it turned out that it became the provost position. I recommended Harvey Brooks at Harvard, who was subsequently the dean of applied science there—Harvard has no engineering school. Incidentally, I'd known Harvey Brooks and had a great admiration for him, because while I was on the Atomic Energy Commission he had been a major part of the nuclear reactor program that had gone on at the General Electric Company. As a matter of fact, some of the subsequent moves that were made for that work, changing it from a breeder as it had been originally set up to be, were what impelled him to leave and go to Harvard. But anyway, he turned it down without even investigating it. I think he had already determined that he wanted to spend his life at Harvard. He just said he wasn't interested in getting into a post of that sort, and I think he was only interested in staying in the Boston area. He was deeply embedded there by that time.

Next they started working on me. I think Lee had sensed that though I had no thought of leaving Caltech, just about that time I had been offered a number of academic positions outside, in administration, and most of them I'd turned down without even looking into them. But I did have some thoughts of getting out of the division work, because I'd been in it for over ten years.

TERRALL: This was during the six-month-leave period that you're talking about?

BACHER: Well, I'd thought of this before then. I'd come to the conclusion when I came to the end of my period on the President's Science Advisory Committee. Along in'59 or so, I began to wonder whether that was something we should get somebody else to do. After all, I'd started a number of things [in the division] that were going well, and it seemed to me that this was an appropriate time to do something else. I'd mainly thought of going back and getting closer to the research in physics, though I'd been out of direct work of that sort for a long time. But at any rate, I was not enthusiastic about continuing ad infinitum as chairman of the division. There were a lot of responsibilities, and I'd been out of some of these for a little while due to this President's Science Advisory Committee. Well, it took some persuading, but having been convinced that we needed a provost to head the academic side of administration, I finally agreed to do it. But I first asked that my leave go on, and I received that leave. After Watson retired, Will Lacey became dean of the faculty; I think it was at that time that Carl Anderson was made chairman of the division [1962]. [Tape recorder turned off]

I went on leave in the late spring of 1961, and we went to Europe for a while, and about the first of November we came back here. Although I didn't take over as provost until the first of the year, I got two months of trying to find out informally what the problems were. This was good for me; I went to work at it.

So I talked with Lee DuBridge about what he expected me to do [as provost]. The relation with Lee DuBridge was just a wonderful one. I'd known him since 1929, which was before I took my doctor's degree; of course, my closest association was while I was chairman of the Division of Physics, Mathematics, and Astronomy for eleven years. I used to see a great deal of him. It was now a relation of mutual confidence, long established. I enjoyed working with him very much. This, of course, gave me a good start in the job of provost.

Well, what was the provost supposed to do? In the first place, he was supposed to act for the president in academic management and appointments. This included promotions, salaries, budgets, and so on. The provost attended all trustee meetings, and essentially presented appointments to the trustees for approval—because all faculty appointments, research fellow and otherwise, were approved by the trustees. Also, it was the provost's job to speak to academic questions. This didn't mean that only the provost spoke to academic questions; the president could or not, as he saw fit. This required a very close relation with DuBridge, and in fact we worked very closely together.

After coming back to Pasadena from Europe, I had a little time to get my feet under me before I took over the responsibilities of becoming the provost. There were a number of things I needed to do. One was to figure out how some of the divisions planned their long-range activities. At that time, the Biology Division was having a series of meetings of their senior professors to discuss just this problem; they were making some changes in the fields in which they had traditionally worked. Biology, of course, was one of the subjects I knew least about. I was delighted by the opportunity they gave me to sit in on these discussions. Essentially, I just listened to what they were doing and tried to get some understanding of the sorts of problems you ran into. It was very useful, because later it became clear that the organization in biology had some weaknesses in it. The question as to whether some of the work shouldn't be separated from some of the other work came up, and I had finally to take a rather firm position on it. But that's another story, and not worth going over in detail.

I did learn quite a little bit about how the Biology Division worked, how they judged subjects. Biology had one point in common with physics—that is, the Biology Division had always worked in a rather narrow range of the subjects that constitute biology. Long before it was recognized in physics, they'd found that this was the way to do it in a subject as complicated as biology. George Beadle was then chairman of biology. I had known Beadle since 1930, through mutual friends. He was a very perceptive person and quick to understand what should be done. I don't remember just when it was that Beadle left to go to [the University of] Chicago as president [1961], but I think he had decided along about this time that he wanted to get into administrative work. He was a logical person to be the provost; in fact, I think he had the job of dean of the faculty for a while when Watson was away. But I don't think he felt that this was what he wanted to do. When he left biology and went off to the University of Chicago it was a great loss to the institute. Beadle was a first-rate scientist and a very perceptive person. I found him absolutely wonderful to work with.

But biology, of course, has always been an extraordinarily strong subject here at Caltech, and I was very glad to see how some of the people worked there, especially when they were going through what every subject at Caltech has to go through, and that is from time to time to say, "Well, this subject is not where the new things are going to be. We have been strong in this, but we've got to do all of our building for the future in a subject that's defined in the following way." This was their problem, and I sat in and listened to them beat this around. They did a very intelligent job.

This convinced me that by and large, except for some ties between divisions, the division level was where the principal planning for the future ought to be done. Trying to do this as it's done in some places, by masterminding it somehow from the top, doesn't work very well. Occasionally guidance has to be put in from the top, but largely because subjects overlap. If an institution is going to be an integral unit, you simply must have ties and overlaps between the divisions, and these must be sensible. That has been done very well at Caltech traditionally, and in part because the institute acts more as a unit than most of the larger schools do. That's one thing that's important about our small size and the friendly relations that exist around the campus. Caltech is unusual in that respect.

TERRALL: Did you go around to other divisions, then, at the same time?

BACHER: No, not in the same way that I went to the Biology Division, because the Biology

Division was involved in this planning study at that time. I did talk to people in the other divisions to find out what they thought their problems were, but unless they were in some state of ferment, there wasn't any point in trying to do more than learn what they were doing. There were some problems that existed throughout the institute, and some that were coming to a head at that time.

One of the questions was whether there was any graduate work whatsoever in any of the humanities. This was a subject under considerable debate at that time. I'm not entirely sure that we've solved that problem, but we found some subjects we thought made some sense for us to offer master's degrees in, and now the institute offers a doctor's degree in one or two subjects associated with the humanities. It's perfectly clear that we can't begin to go into many of the subjects that a humanities division often goes into, especially because in the last twenty years the overproduction of PhDs in all of the humanities has thrown the whole subject into chaos. It has helped us enormously, in the sense that the availability of better people has meant that we have a much better and stronger group in the humanities than we otherwise would have. But that's a matter of circumstances. I favored giving Caltech students the opportunity to take subjects in the humanities without having to worry about whether they were going to ruin their grade-point average or get into something that they really couldn't handle. Caltech, perhaps even more than some schools, has a fair number of students that shy away from things they can't feel confident they'll get a good grade in. I think that's a bad situation, but I think some of it has been taken care of.

TERRALL: When you became provost, was there tension between the Humanities Division and other divisions because of this problem?

BACHER: Well, there was some tension. There were some people in the Humanities Division who, from the beginning, felt that Caltech ought to be a full-fledged university. And there were people in other divisions—I could name some people in physics who felt the same way. My feeling is they were demonstrably wrong. If Caltech had tried to become a full-fledged university, it would have been inevitably unable to support this on a level in which it would achieve the esteem and standing it has in the major fields it does work in. It would have ended up being a mediocrity. I think it's much more important to have a school that has people with broad interests and a limited number of subjects in which it tries to specialize but is very good in those subjects. And indeed, that's more or less the Caltech philosophy.

All during the 1950s, one of the major Caltech problems had been to get salaries competitive with other institutions. Lee DuBridge had made a major step to increase faculty salaries soon after he came to Caltech, but we were still behind. When I became provost, I spoke in one of the trustees' meetings on our needs and how important competitive salaries were to Caltech. The trustees were clearly in favor of this, but funds were needed for other purposes, too. Some days later, Lindley Morton, a trustee who spent much effort helping Caltech, came to see me to ask a number of questions about faculty salaries. When he learned that the American Association of University Professors had a salary rating for universities and colleges, he wanted to know more. This led to his taking up this subject and trying to find new sources of income. The idea of getting Caltech into the top category immediately appealed to him. At that time, there were only a few specially endowed and named professorships at Caltech. In fact, in one case, the Tolman Professorship, there had been some difficulty keeping the endowment separate. Gradually, the need for endowment for special professorships and higher faculty salaries became increasingly important. Caltech now has a large number of specially endowed professorships, and this traces back to the efforts expended at that time and which Lindley Morton followed as a special project.

There was always a problem at Caltech because it is a small school and the emphasis has always been on limited growth. I used to say that Caltech must change a great deal and must change much faster than it grows. Most big universities just add something on, and when something atrophies they throw it out. But you can't do that at Caltech, because of the size of the place. Caltech is much too small to carry out work in all or even in most of the parts of its principal fields. So as any subject advances, some parts of each division must be given up; this is more or less the problem. There are whole fields, in biology for example, that they used to work in where they do practically no work now. There are a good many fields in physics we don't work in anymore, and so on. It's not easy for Caltech, which is a small school, to compete with the large, old, rich, private universities and with the enormous state universities, one of the largest, and certainly the strongest, of these being the University of California, which is right here. So there's a problem of how does Caltech fit into a situation like that, and how does it keep on the forefront of knowledge? This is not easy; it is very complicated.

TERRALL: You mean competition in the sense of attracting people and also in accomplishments?

BACHER: I would say it's competition in both of those areas. But I would say particularly in trying to see to it that both the quality and to some extent the volume of work that comes out of Caltech is significant, so that if we are assessed in some field, we're not number thirty on the list. By and large, over the years Caltech has done pretty well in comparison. In the survey that's conducted by a committee of the American Association of University Professors from time to time, the principal fields of the institute have placed it up toward the top of the ratings. In some of them, like astronomy, the institute's been right at the top for years and years, and it's been close to the top in a number of other fields. I'm sure these ratings aren't all that good in some ways, but it gives some feeling and it's important, because you're being compared with places—well, MIT, for example. Now, MIT in almost all respects is five times as big as Caltech; they have five times as many students. They're in a lot of fields we're not in at all, and in fields we work in, they may have three times as many people, like physics. I don't know how many they have now in physics, but it's several times as many as we have. Interestingly enough, the most nearly comparable place in terms of restricted fields and general size is Princeton. Of course, it's completely different in the sort of a place it is and the fields they work at. But it's been important for Caltech to keep its growth in students small, and its expansion in fields likewise not too great. It strives for excellence in faculty and students and staff and academic accomplishment. One index of this that people quote to me is how many members of the National Academy of Sciences there are, and the percentage of the faculty here who are members of the National Academy of Sciences is probably larger than at any other school. Now, that's not a fair comparison, because people work in other fields, but the percentage is really quite large.

Incidentally, the professorial staff [at Caltech] is about 250. And I looked up some numbers on this. It was about 250 when I was provost, it was 250 in 1974-1975, and during 1979-1980, it was 264. So we have not grown much in the last fifteen years in faculty size, and principally this is a reflection of the difficult financial situation, in which the institute has to increase resources all the time in order to keep a faculty that large. But the institute has made strong efforts not to get too big. It has varied somewhat in size, but Caltech students are about equally divided between graduate and undergraduate work and that seems to have been roughly true for a long time. Once in a while views have been expressed that the institute ought to grow to be a big place, but on the whole that's not something that has met with wide acceptance here.

Fig. 8. Caltech colloquium audience, 201 East Bridge Laboratory, June 29, 1959. Front row L-R: William ("Willy") Fowler, William Houston, Lee DuBridge, Robert Bacher, Niels Bohr, Robert Christy, Richard Feynman and Max Delbruck. Caltech Archives.

TERRALL: So this was the kind of thing that was your concern during those years?

BACHER: During the years I was provost, I was concerned with decisions on appointments and questions having to do with planning of new fields that we get into—that we keep dynamic and try to keep in the forefront and that we don't grow too much. In other words, try to keep our excellence in both faculty and students. This is not something that is entirely definitive but it's interesting: In terms of the college board examinations, Caltech has probably had the highest standards for twenty-five years or something of that sort. I don't think this means that Caltech has the best students necessarily, but it does mean that the students must be pretty good. It's important to keep those standings high. We're the only school I know of that requires the advanced mathematics test for entrance. So it means that our students are at a starting point that is somewhat further along.

This sounds as if I think everything at Caltech in this respect is perfect. I don't at all. There are lots of things we could do better, but it's awfully hard to keep the standards up and keep getting into new fields and keep out on the edge of the fields we're in. There are areas in which this could have been done better than we've done it, but on the other hand the institute hasn't done too badly.

TERRALL: Were you concerned with problems of curriculum and that sort of thing?

BACHER: Yes, I was concerned with this, but it's very largely delegated to the faculty. If we were to go into a whole new set of subjects, or if, for example, some part of the institute suddenly wanted to generate a number of courses that was too large for the number of faculty they had, this would be a problem the provost might have to get into. The teaching load varies considerably from one division to another. I was looking over some figures just the other day, and about twenty percent of all of the students at the institute, graduate and undergraduate, major in the subjects of the Division of Physics, Mathematics, and Astronomy, and it used to be even higher than that. Fortunately, in recent years a much larger number of students have majored in engineering, and that's a good thing on the whole. Some of the divisions have relatively small teaching obligations. Geology has a much smaller teaching obligation, and biology has really a very small teaching obligation, because their concern is principally with teaching graduate students. They have a fair number of graduate students, but that's not an enormous teaching problem. They don't have many people taking biology as a requirement for something else, whereas that's true in physics and mathematics, especially. So that situation varies over the institute quite a lot.

In connection with undergraduate education, I should say something about the association of the psychologist Carl Rogers with the institute. I met Carl Rogers and Mrs. Rogers through Ruth Tolman sometime in the early fifties. He was interested in Caltech, and the Caltech Y invited him here to visit and lecture. Somewhat later we talked about this, and his comments and ideas as an outside observer seemed to me a valuable new way to see the institute. There's a danger at a place like Caltech of getting into ruts; I felt Carl Rogers might be able to shake things up a little bit. Sometime in the winter of 1963-'64, I invited him to come up to visit and to talk to me and also to Lee DuBridge.

It was clear after the first meeting that he was interested in having group discussions on a continuing basis. I liked this idea, and Lee was at least tolerant. Chuck Newton, Lee DuBridge's assistant, who was on the board of the Western Behavioral Sciences Institute in La Jolla where Carl was the senior participant, thought it would be a fine thing to do. So we—and this included Wes Hershey, Chuck Newton, Hallett Smith, and John Weir, in addition to Carl and myself—dreamed up a group that would have a varied institute flavor. The first meeting was a dinner at the Honker restaurant—later the Chronicle—so Carl called it the Honker Group. We had discussions about Caltech, faculty and students, stimulated by Carl, who was expert at introducing a subject without expressing his views. Furthermore, he was expert at pulling out views, even quite differing views, without directing the discussion. Some members always attended; others were frequently absent. Except for Carl, most were faculty members or associated with the Caltech administration. DuBridge was a member. Another, Henry Dreyfuss, was a trustee of Caltech and a friend of Carl's and also of mine. Another, Stanley Johnson, was and is an active Caltech alumnus. Wesley Hershey was then the head of the Caltech YMCA. The others were faculty members invited by reason of their interest and to get a wide faculty representation.

The first meeting of the Honker Group was held on April 13, 1964. Carl Rogers kept notes of the discussions and prepared summaries of the meetings in some detail, which he sent to all the members.[3] Rather than attributing opinions to individuals, he indicated the extent to which there was an agreement on a given issue. At the first meeting, someone raised the subject of the negative impact of grades for freshmen. This was raised again in the May and June meetings. Although the record does not usually give names, it was probably I who raised this subject, since I can remember talking to Carl about it earlier. From the tone of Carl's notes of these meetings, which I have reviewed, and from my memory, there was support for the idea of eliminating grades except pass and fail for freshmen, but the majority at that time was not strong.

[3] These summaries have been deposited in the Archives.

The subject of admitting women undergraduate students was also discussed, and the general reaction to such a move was favorable, although there was realization that some faculty members would be vigorously opposed.

Sometime not long after the mid-June meeting of the Rogers group, I came to the conclusion that I should ask the chairman of the faculty, Ernest Swift, to set up a committee to study the grades-for-freshmen idea during the summer. Whether the question of admission of undergraduate women was included in the charge to this committee, I do not recall. Foster Strong, the dean of freshmen, remembers that I talked to him about the pass/fail idea, and he favored it. Ernest Swift set up a committee, with Ray Owen as chairman, and asked them to meet over the summer and to make recommendations by September. I had forgotten that I attended their first meeting, but Ray Owen's minutes, which I have seen recently, indicate that I made a definite recommendation to them to consider the change to pass/fail grades for freshmen. I also urged them, if they approved of the idea, to bring in a recommendation for faculty action, so that if it were to be considered favorably by the faculty it could go into action in the fall. I felt this should be done as quickly as possible.

Without the sympathetic support of Ernest Swift and Ray Owen and his committee, nothing could have happened. They did in fact bring in a report to the Faculty Board, dated September 29, 1964, recommending that grades for freshmen be limited to pass or fail at the end of the freshman year. The Faculty Board approved this policy for a period of two years, at which time it would be reconsidered. Their recommendation was brought before the faculty sometime in late October and was passed by an unbelievably large majority. I was surprised by the extent of the support. I recall that after the faculty meeting in the Athenaeum, students hung out of their windows to ask what the result was.

Caltech was, I believe, the first of the high-standing schools to adopt such a policy, and our colleagues elsewhere were aghast. The plan was renewed in two years and either then or somewhat later was extended to include one course per term in the sophomore through senior years. The plan is followed today, and I think it has been a success.

I don't recall when the action on admitting undergraduate women was taken. I think it was later [1970]. The Honker—or perhaps it should be called the Rogers group—continued for about two years and, I believe, had a beneficial influence in getting touchy academic policies discussed generally. Later, Carl Rogers had a group of students for discussion. His discussion groups had a real effect on faculty opinions and views. He thought that the changes were much too small, but he may not have realized what a lasting impact those discussions had.

ROBERT F. BACHER

SESSION 9

February 16, 1983

Begin Tape 9, Side 1

BACHER: During the early sixties at Caltech, we had done a fair amount of thinking about where high-energy physics was headed. It was perfectly clear, with excited states of the nucleon, that it was going to take more and more energy until much of this could be straightened out. Nobody at that time had the idea that the complexity of nucleon states would be anything like as great as it has turned out to be. But we didn't know where we wanted to go. The AEC was very supportive of us. One summer we ran a study of what a really high-energy machine would be like, and we made some efforts at trying to get other schools on the West Coast interested in this, without very much success. But the studies, interestingly enough, came out with a lot of ideas involving a low-energy pre-accelerator, then into a Lineac, then into a circular machine, and then into a big circular machine—it was almost a plan for what turned out subsequently to be the accelerator built by the Universities Research Association in Batavia, Illinois.

TERRALL: Was this before SLAC [Stanford Linear Accelerator Center] was built?

BACHER: I don't know the answer to that. I think SLAC had been started, but it was built in several stages, and I think was under construction at the time. Either that, or it was in the middle of planning. [Construction of SLAC began in 1962—Ed.] The position of the people in various places was complicated, because the Berkeley people were supporting the SLAC move. And I saw, a little more than some people did, that we were running up against a situation in which there were going to be only a very limited number of accelerators. AEC did not like our making studies in this direction, and in fact ordered us to stop.

TERRALL: Why was that?

BACHER: They felt it complicated their political problems with the Northern California universities, as indeed it did. But there is a report out that describes our study. It was very good work. Some very interesting work was done by Matthew Sands, Alvin Tollestrup, and Bob Walker. In addition, some people came out from Brookhaven and worked with us. It had a lot of very good ideas, which have persisted. It wasn't clear at that time that some of the things that were postulated could really be done, but it turned out that they could be, and things went very much along those lines eventually.

In early 1965—this is somewhat later—it became clear that some sort of a national accelerator group needed to be set up. There was very strong pressure from the people at Brookhaven that this ought really to be formed around Brookhaven simply by expanding it. But that was not an idea that appealed very strongly to people in other parts of the country, mostly because that was entirely an eastern group. There was a fragmentation, and you can't get rid of a fragmentation by trying to put the fragments back together again. Our conclusion on this was that a new group should be started.

Fred Seitz, who was then the president of the National Academy of Sciences, took a strong initiative on this as a national issue. If it hadn't been for Fred, this thing would have dragged on for a long time, but he pushed the setting up of such a group. Early in '65, thirty-four universities formed the Universities Research Association. At about the same time, the AEC set up a site-selection committee for a national laboratory. The ticklish part of the whole thing was, Where would it be? I had the misfortune—or fortune, depending upon how you look at it—of being a member of that site-selection committee. Our contact on the Atomic Energy Commission was Gerald Tape, whom I'd known ever since he took his doctor's degree; in fact, he had his first job at Cornell before the war and had been my administrative assistant for some period at the Radiation Laboratory during the war. He was an AEC commissioner at that time, and he formed this committee, which was then set up under the academy. The academy played the promotional role, and he came in to advise on what was needed by AEC.

We were given the job of finding the places that wanted to be considered. We did it in a public way and sent groups around to look at all of these prospective locations. Well, everybody and his brother wanted to have this, and I think some hundred sites were proposed. They were weeded out in part by sending groups of people around to look at them. Some of the places were utterly impossible, and others weren't centrally located enough so that you could get to them. We ended up with about four sites. In order to get some variety into it, we had put in a site near Denver, which looked very good. There were people who felt that that was a good place to go.

Other sites were in the East. One site was in Michigan, as I recall, and one was the site that was finally chosen, west of Chicago. We felt that the AEC itself had to come down and do the final selection; they finally chose the site at Chicago.

So these things were going along in parallel. It's quite a job to select a site for a big undertaking like this. The state of Illinois was very much interested in this. As a matter of fact, the site for this laboratory, which consists of several square miles of prime cornland west of Chicago, was bought up, condemned by the state of Illinois, and given to the United States government. So the site for this accelerator was paid for, essentially, by the state of Illinois, which was a very nice thing.

In the meantime, the URA started going. Lee DuBridge, as president of Caltech, played a role in that organization right from the beginning. We took it as our policy at Caltech that we were going to continue to play a role in this organization, because we were then deciding that we weren't going to build a machine here at Caltech and we'd do our work elsewhere. This was a very serious step. It has made some difficulties for our program, but if we'd built a machine here at that time, we would simply have postponed the day when we would have had to do this, because there are only a few places in the world now where really high-energy experiments can be done. That's one of the difficulties of high-energy physics.

Lee DuBridge served on the council of presidents of this group. After the first few meetings, I was his alternate, and I almost always went to it. At the same time, I usually met with the group because I was by that time one of the trustees of the Universities Research Association, which began to be set up in groups around the country. We had a group in Southern California, and I was the representative on the board of trustees for that group for almost ten years.

Each of the universities that joined was asked to make a commitment to come forth if necessary with $100,000. Actually, only $10,000 was called on from each university. As far as I know, because the overhead of the operations of the central organization was charged against the contract, that was only used for certain things the universities felt ought to be at the site that the AEC didn't think were proper to charge against the contract.

TERRALL: But there was an AEC contract from the beginning?

BACHER: There was an AEC contract essentially right from the beginning. Until that could get into operation, the big problem, of course, was to get the laboratory going. The executive committee of the board of trustees, of which I was a member, picked a director and cleared it with people around, and we approached Bob Wilson, who was then at Cornell, to come as the director of the laboratory. If my memory serves me correctly, he was one of the members of the group itself, and I was given the job of taking him outside and putting it to him that we wanted him to be the director of the laboratory.

He did a remarkable job as director [1967-1978]. One of the reasons that the laboratory [Fermilab, or the Fermi National Accelerator Laboratory] is a great success in its present form, and has such interesting buildings, and was able to accomplish as much as it has, is because of the ingenuity he put into it. There were some problems—he promised a little more than could possibly be delivered—but he came awfully close to delivering even what his accelerated schedule was. I thought it was too bad that in pushing so hard for a still higher-energy machine, he laid it on the line that either he got it or he resigned. He didn't get it, so he resigned. I always thought that was a pity, for him to end his connection that way. But he was a great help for the laboratory, and a great deal of credit for its success goes to him—and also to the man who was his administrative aide, Ned Goldwasser, who is now back at the University of Illinois.

Well, it's been a successful laboratory. We've done a lot of work there. Walker, [Barry C.] Barish, [Jerome] Pine, [Frank J.] Sciulli, Tollestrup, and others did a lot of work at that laboratory. Today we're doing much less work there than we used to do.

I was present at the ground-breaking for the laboratory, on December 1, 1968. Wilson really dove right into it, and things started to happen right away. I served on the committee for almost ten years. I was vice-chairman of the board of trustees for a fair part of that time. Harry [Henry DeWolf] Smyth, who's a very close friend of mine and longtime associate, was the chairman. After he retired, I was chairman of the board for a while and president of the organization for one year, so that Norman Ramsey could accept a fellowship at Oxford.

One other thing I wanted to talk about that came in these years was my participation in the International Union of Pure and Applied Physics [IUPAP]. I became involved in this as a consequence of the death of Bob [H. P.] Robertson. When Bob Robertson died [August 1961], he was the foreign secretary of the National Academy of Sciences. He was also the American representative on the council, which was a five-man group, of the International Council of

Scientific Unions [ICSU]. Bob died suddenly, as a result of [injuries from] an automobile accident, and this threw things into a great hubbub. The National Academy looked to us here at Caltech to help them in solving the problem of how this was to be taken over. Well, it was agreed that Harrison Brown would become foreign secretary of the Academy, and he was foreign secretary for many years and made enormous advances at it. They asked me if I would take Robertson's job as a member of this five-man ICSU board, and I said I would. Actually, ICSU was very slow at acting on this recommendation from the academy. Almost a year passed when we had no representation on the council.

I found soon after I went on to ICSU that it was as Bob had told me—that the United States, especially in physics, didn't play much of a role in this international organization. The more I found out about it, the more it became clear that this was a serious mistake. So I became interested especially in the International Union of Pure and Applied Physics, and the academy asked me to serve on the council of the International Union of Pure and Applied Physics.

TERRALL: Is that a subgroup of ICSU?

BACHER: The International Union of Pure and Applied Physics was one of the unions that belonged to the International Council of Scientific Unions. It wasn't an enormously old organization; it had been set up in Europe around the First World War or thereabouts. Some of the unions were older than others, and some were much stronger than others. It had traditionally been very strong in astronomy, because there's so much international work going on. It had not been strong in physics, and one of the reasons was that it was almost a purely European organization. This was—in part, at least—due to the considerable lack of cooperation of the American Physical Society. More or less, the feeling at the American Physical Society was, "Why should we pay any attention to that? If we want foreigners to come over here, why, we'll just do it through the Physical Society." I thought this was not a good way of fostering cooperation and getting it set up in a way in which other groups would participate in a friendly way. But I was president of the Physical Society in 1964, [which means] I had been involved in it for a number of years, and this was about the same time that I became involved in ICSU and in the International Union of Pure and Applied Physics.

Well, I went to Bill [William W.] Havens, the executive secretary of the American

Physical Society, and simply said that I thought we ought to do something to get them more involved. I said, “What I’d like to suggest is that both you and the president of the American Institute of Physics come on the steering committee at the academy, and I’ll recommend that this be done.” And he said, “Well, all right, but I don’t see much point in our doing it.” I said, “We just can’t go forward without bringing these groups together. The way for this to happen is for the Physical Society to have its own people there so they know exactly what’s going on, and vice versa.” So Bill Havens did this, and he became a very good member of the committee. He drew his own conclusion that it was important for the American Physical Society to cooperate. And he was on this steering committee for years. I was chairman of it for a long time.

The group became much stronger especially with United States participation in it. We found when we first went into it that it was essentially operated by a rather small, ingrown group of a few small Western nations. The Russians weren’t in it very much, and we weren’t in it very much, and a lot of the larger European nations weren’t in it very much. That changed.

TERRALL: What about Japan?

BACHER: Japan at first wasn’t involved at all, but over the years they became very much involved, and there are quite a lot of meetings in Japan now. Even the Chinese have become involved in it now. And the Russians took this up. Well, actually, there had been some precedent for this in high-energy physics, because Robert Marshak, who was at Rochester, had established, independent of all of these organizations, a high-energy physics conference that met in Rochester yearly. He soon found that it was advantageous to try to get government support for some of these meetings and, especially to avoid visa problems of one sort or another, he found himself dealing with the government. Of course, in such a situation, it’s very good to have the academy, or some group in the academy, working at this. Those meetings subsequently became part of the meetings sponsored under the International Union of Pure and Applied Physics.

But I think it’s done something, and some of us worked pretty hard at it in the early days to get it going. It has support from the American Physical Society; it has the support of physicists; it has groups supporting conferences in all sorts of subjects related to physics. Almost all of the international meetings are held under the supervision of IUPAP. They get very little money, but some of the money does come from the State Department, essentially through the academy, to help support some of the meetings. It's customary for just a few thousand dollars to be given for quite a major conference, and they have to raise their own money outside.

TERRALL: So the main function of the organization is to sponsor meetings?

BACHER: I would say there are two main functions. One function is to sponsor conferences. But in the process of sponsoring conferences, this provides an area in which ticklish problems can be discussed that involve the East and the West. And it's been used for that on a number of occasions. Very touchy situations come up. Africans will exclude some group from coming in, and then they have to be told that nobody will come to the meeting. Some of them try to exclude Israel from some of the meetings, and we were always fighting that. Or the Russians won't ask certain people. Or we will ask people to come from Russia, and the Russians won't let them out; we've had that happen any number of times and it's been very embarrassing. That's by no means solved. The situation is so bad at the present time that there are some organizations that won't hold meetings in Russia. This is a very touchy business. On the other hand, there is a framework for discussion, and that's a very good thing.

Along in the late sixties, Lee DuBridge decided he wanted to retire. When Nixon was elected, Lee was offered the position of science advisor. I didn't have any idea myself what that was going to be like, but I think Lee was anxious to find some reason to separate from the institute. He was then in his late sixties, past sixty-five, and he felt it was high time he retired as president. In the fall of 1968, when he was offered the position of presidential science advisor, he decided he would accept and use it as a reason for leaving the institute. Well, this meant that the institute was left needing a president right away. Some digging around was done rather fast. Actually, I was acting president for a rather short time. But almost with Lee's departure, a new president was appointed. I can remember vividly getting out the announcement of the new president, who was Harold Brown.

Harold had been in the Defense Department; he had been secretary of the air force and was essentially leaving because of the change in administration. I had known him well. He had been very active with the President's Science Advisory Committee. He had played a major role in our negotiations with the British on the test ban and was a very able person. Lee knew him well, too. In fact, I can remember vividly calling on him with Lee, sort of sounding him out to see whether he'd be interested in coming here.

Well, all of this happened just when we were having negotiations with a new head of NASA about problems we never had been able to get settled at JPL [the Jet Propulsion Laboratory]. In fact, the new chairman of NASA—Tom [Thomas O.] Paine, a really very fine fellow—came out here with all of his chief advisors and said, "I hear there are a lot of problems that don't seem to be able to get settled. I've brought all of my chief staff out here and I'm going to stay here for two days and see if we can't get them settled." Arnold Beckman came up and Lee came, and I was there. Lee asked me to come because, he said, "I have to go away before this meeting is apt to be over." Well, I must say, it was really a breath of fresh air, because there were things that had bothered us for years, and he just couldn't understand why there was a problem. These things all got settled. DuBridge had to leave before much of this was in form. We wrote a new charter that dealt with all of these things. It ended up that I signed it for the institute, and I think it's still the charter under which JPL operates, with very few modifications. But it was a godsend to us, because it really cleared up a lot of things.

TERRALL: What kind of problems did you have?

BACHER: Oh, all sorts of problems having to do with to what extent we could proceed independently in taking up things and what permission we had to get repeatedly from Washington and so on. These were very sticky. A laboratory like JPL has got to have a great amount of initiative to be able to go ahead and do things on its own. Somebody has an idea and you've got to explore it a little bit. You can't just say, "Well, we'll make a great plan for the future," until you've tried out a few things, and they weren't set up to do that very well. There was far too much permission needed, in terms of the operation. The laboratory didn't work that well, and we weren't accustomed to being treated that way, either, and it was difficult. I don't remember a lot of the specific problems anymore. I'd have to go back and refresh my memory. It was our view, and JPL's view too, that it really couldn't function if it was just assigned jobs. It would have to have a certain amount of initiative and would have to participate in some of the planning. JPL has become a very strong laboratory. It's unique among the NASA laboratories. It's the only one run by a private contractor; all the others are government laboratories and operated as such. And I think it has done a magnificent job. It really is an extremely fine laboratory.

Well, I was talking about Harold Brown coming out here. Harold, of course, was very much an administrator. One of the things that was awkward for me was that just when he was coming out here, this Catholic girls' school over in Los Angeles [Immaculate Heart] was having severe financial problems and had approached the institute about whether they could set up over here. Well, I saw grave difficulties in this, and especially financial difficulties.

TERRALL: They were proposing a merger of some sort?

BACHER: They were proposing a merger, so that we would have associated with us, nearby, a girls' school; the students would come from this Catholic school. Well, the woman who ran it was a very competent person. We appointed some committees to look into it, and Harold Brown didn't want us to make any definite decisions on this until he came out here. It became perfectly clear to me that this wasn't going to work, but on the other hand I had the job of trying to keep it so that the decision was not taken before Harold came to Pasadena.

Harold wanted me to continue as provost, and I said I would, because I felt there ought to be continuity. It was an easy relation for me, because I knew and respected Harold. We saw eye-to-eye. He took seriously any thoughts I had on academic problems. If I wanted to see him, why, I could see him in five minutes—essentially anytime. He'd put me in between a couple of appointments if I needed something urgent, and so on. He was an extremely fine person to work with and very supportive. But I explained to him that I thought he really ought to have his own provost, and that I would be coming before long to the age of sixty-five, which I thought was the right retirement age. I had come to the conclusion that people shouldn't hold administrative positions in the institute after sixty-five. That was not a view I held originally, but when it came down to my own case, I began to realize that I was slowing up somewhat—and I don't think you want people in positions like that who are slowing up.

Actually, I retired a little before I reached that date, which is usually the June 30th after the sixty-fifth birthday. June is not a good time for a provost to retire. The best time for a provost to retire is when he has everything all set for the coming year. It happens that my sixty-fifth birthday was on August 31st, and I said I thought that was the day to retire, so I did. Harold sort of argued with this for a while. But after some looking around, Bob Christy was chosen to become the new provost, and he took over and then served as acting president after Harold Brown left, until [Marvin L. "Murph"] Goldberger came [1978].

TERRALL: So you overlapped with Harold Brown for about a year?

BACHER: A little more than a year, about a year and a half. He urged me to stay on longer, but it seemed to me that a year and half was enough time to provide an overlap and get things going and that the summer was a good time to go.

When I left that and moved over here [to George W. Downs Laboratory of Physics], it was the first time I'd been relieved of fairly major administrative responsibility. I was still associated with the high-energy physics contract; if I remember correctly, I still was nominally the principal investigator. But actually Bob Walker was then doing essentially all of the planning for it, and I proceeded to get out of that and let him take it over as soon as I possibly could.

ROBERT F. BACHER

SESSION 10

February 23, 1983

Begin Tape 9, Side 2

BACHER: I left the provost position on my sixty-fifth birthday, August 31, 1970. Christy had spent part-time in the provost's office the previous spring and summer, after Harold Brown had selected him to be the next provost. He knew the main jobs of the office really quite well.

At that time, August of 1970, I was heavily involved in Fermilab and in the International Union of Pure and Applied Physics, both of which took considerable time and effort. When I retired as provost, the trustees passed some statement of thanks. As a matter of fact, they didn't even bother to send it to me. It struck me that that was in sharp contrast to the various boards from which I retired, which created quite a fuss. That's just not the way it goes here. I was even a little bit more surprised, in looking through my files, to find that President Eisenhower had sent me two letters thanking me for serving [on PSAC].

My retirement as provost was, however, recognized by the faculty in two actions that really touched me. First, as they had done a few times before, they put on a dinner followed by a musical show with Caltech talent, with songs and dances arranged by Kent Clark, and gave us a really magnificent album of pictures of our home, the Tolman home. These pictures were really grand. They were taken by Hal Smith, Hallett Smith's son, and really caught the spirit of the place. The second thing touched me even more. When I returned to the faculty as a regular faculty member, they elected me to the Academic Policies Committee. I could not believe that my colleagues, after suffering for nine years under my guidance as chief academic officer, would elect me to the Academic Policies Committee. That really touched me.

It also made me go to work and try to do something on that committee. Nothing done by a committee is done by an individual; the work evolves through discussion. Various means were discussed in the committee meetings to improve the workings of the Faculty Board, which guides faculty action. As provost, and earlier as division chairman, I had been an *ex-officio* member of that board for more than twenty years. Problems had inevitably become more complex. Faculty Board meetings lasted longer, and on occasion for several hours, without real progress.

It struck me, although it was perhaps a bit off our normal track, that we might be able to suggest a change that would make the faculty action proceed more effectively and smoothly. My memory, which may be wrong, is that I introduced the suggestion that something was lacking to prepare subjects for Faculty Board action. Often, vital background information was needed, as well as preliminary consideration by the interested and informed faculty members. At first, this did not meet with much favor, but after several discussions in those committee meetings, and much thought, the committee forwarded a suggestion that a steering committee of the Faculty Board be established, with the chairman of the faculty as chairman, the vice-chairman of the faculty, the provost, the secretary of the faculty, and several elected Faculty Board members, to consider items before they appeared on the agenda, to ensure that background information needed for Faculty Board action was available.

TERRALL: Let me ask you one thing. The Academic Policies Committee was a committee of the Faculty Board, is that right?

BACHER: It was a committee of the faculty.

TERRALL: And the Faculty Board?

BACHER: The Faculty Board is the executive body of the faculty. The suggestion that was brought in by this committee was that a steering committee for the Faculty Board be set up.

TERRALL: So the suggestion came out of that Academic Policies Committee.

BACHER: That suggestion came from the Academics Policies Committee. This steering committee was to consider items before they appeared on the agenda to ensure the background information needed for Faculty Board action was available. Somebody else may remember this quite differently, but the Faculty Board was willing to try it, and its success has, I believe, made faculty action much more effective. It exists today.

TERRALL: So that suggestion from the Academic Policies Committee was passed by the Faculty Board.

BACHER: It was passed by the Faculty Board, and it was probably approved by the faculty. I don't know for sure. But it was certainly set up by the Faculty Board, and it is followed almost unchanged, I think, today. Everything that comes up in the Faculty Board first goes through this steering committee—not with the idea of trying to formulate the ideas ahead of time but to see whether the information that's needed for the Faculty Board to decide on something in an intelligent way is all there. If the administration is going to be asked some questions, these are at least thought about ahead of time, so that the administration, as represented by the provost, can dig up information ahead of time and be prepared.

TERRALL: So it's mostly just to make everything run more efficiently with less waste of time?

BACHER: Well, I would say it also has an additional point, which is even more important than that. It sometimes means that the faculty gets action on something without just discussing it and nothing happening. Very often if something in the old days was discussed in the faculty, it would be discussed *ad nauseam* and nothing concluded. After a session of that, it was always extremely difficult to get the subject up again and discussed under circumstances in which something could be done. Though the faculty is much more complicated than it was before, and the problems of the institute are very much more complicated than they were before, the Faculty Board meetings, I believe, are shorter than they used to be and they do more.

The Faculty Board is a very interesting idea. It's composed of *ex-officio* members who have administrative responsibilities, but a majority of the board consists of elected members. A good deal of effort is put into having representation from around the institute generally. And there is also care to get younger faculty members involved. That doesn't mean that senior faculty members are excused from it, but there's a real effort made to get wide faculty representation. It's probably true that the way the Faculty Board has been run is in part responsible for the general air of faculty interest in problems around here that may not be someone's own problem. Now, sometimes you get someone who's only interested in his own problems. Once in a while you get a division chairman, for example, who never comes to the

Faculty Board meetings. That's too bad. But on the whole, it's an integral part of the institute and has helped make it the kind of a place it is.

Before I reached the usual date, June 30th after my sixty-eighth birthday, for the customary transfer to half-time, I concluded that I would reduce on my own. After a year at half-time while serving as president of the URA, I did not return to full-time. It made me feel better, because with outside obligations I was not teaching.

The transition into retirement seemed less of a change than I thought it would be. I had wanted for a long time to spend more time learning about our sources of energy, especially looking to the future. I spent quite a lot of time catching up in this field, and I have continued this interest to the present time. Energy costs have skyrocketed, nuclear reactor costs have increased greatly, and probably most important, the world, and especially the United States, has come to realize that our energy waste was a disaster and was unnecessary. Our energy use has leveled off and even decreased a bit. Our oil imports are less than half what they were, and we have increased our use of fuels we have, such as coal. We still have far to go, but now world oil prices have started to decrease. Hopefully, we shall still conserve.

In the summer of 1978, which was roughly two years after my retirement, Caltech found it necessary, with changing national and California laws on retirement, to review and modify its retirement policy. President Goldberger appointed a committee of faculty members—Bacher, Christy, [Leverett] Davis, Epstein, [David] Grether, [Paul] Jennings, and [Felix] Strumwasser. Christy was then provost. I was emeritus and chairman; Davis was vice-chairman and the one who kept careful records, prepared the minutes, searched out information, and prepared agendas. He must have spent roughly half his time on this work from the time the committee was appointed in August 1978 until it was discharged in January 1980. The committee could not have functioned without him.

The committee members took their work seriously. There were four absences in fourteen meetings. Recommendations were set forth in an eleven-page report with appendices. To a considerable extent, these recommendations were accepted, either on a permanent or a trial basis. The Caltech administration converted general recommendations into specific policies, providing for early retirement of professors, starting at age sixty-two as recommended, as well as term appointments for those holding research positions. It was a complicated situation, and Leverett Davis has prepared a file on this subject, which is a part of the Archives.

For several years I served as a member of an advisory committee of the Jet Propulsion Laboratory on their solar [energy] work. This work was at first done largely by contract, and there was a wide range of competence among the contractors. The problem is basically difficult, being severely hampered by low efficiency of the available photovoltaic materials. For a time I was doubtful that my participation was of any use at all. It arose from a long letter I wrote to Bruce Murray, JPL's director, which questioned some of the optimistic comments he had made in a speech on the efficient use of solar energy. It was an interesting and an attractive subject, but the initial photovoltaic losses are very large and have led to major cutbacks in this work.

Most recently, I have been asked to serve on another JPL advisory committee—this time on nuclear-reactor propulsion for satellites. This is so new that I know little about their plans. But I have always followed the policy of trying to help if I could, as an unpaid advisor. I continue firmly in the belief that the faculty can be helpful to JPL and introduce a different viewpoint from time to time.