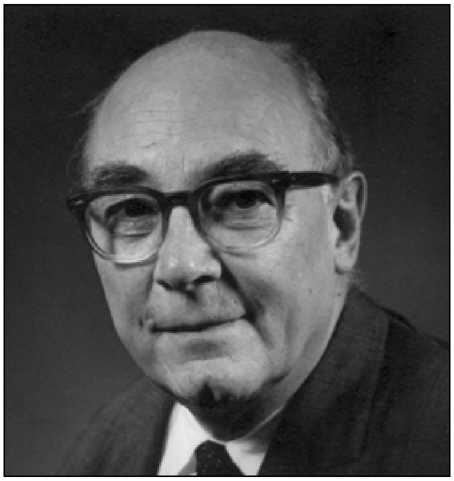

CARL ANDERSON
(1905-1991)

INTERVIEWED BY
HARRIETT LYLE

January 9-February 8, 1979

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
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Abstract
This wide-ranging 1979 interview in eight sessions with Carl D. Anderson, Board of Trustees Professor of Physics, emeritus and Nobel laureate, begins with his recollections of his undergraduate years at Caltech (1923-1927), and the influence of Arthur Amos Noyes and Ira Sprague Bowen. He recalls courses with Earnest Watson, Morgan Ward, Richard Chace Tolman, J. R. Oppenheimer. He offers his early and ongoing impressions of Robert A. Millikan as chairman of physics division and head of Caltech, and of Millikan's work on cosmic rays. He recalls his own postdoctoral work at Caltech on cosmic rays, and his discovery of the positron in 1932 and the mu-meson, or muon, in 1936, and on contemporary developments in nuclear physics. He comments on his Nobel Prize (1936). He discusses his contacts with Enrico Fermi's group at Chicago in the early 1940s and Caltech's rocket projects during World War II at China Lake and Goldstone, including the contributions of Charles Lauritsen, I. S. Bowen, and Seth Neddermeyer. He offers recollections of postwar Caltech, the increase in research funds and undergraduate enrollment, the rise of particle physics and the advent of the large accelerator era. He discusses his stint as chairman of the Division of the Physics, Mathematics, and Astronomy (1962-1970) and concludes by commenting on the current state of physics research.

Administrative information

Access

The interview is unrestricted.

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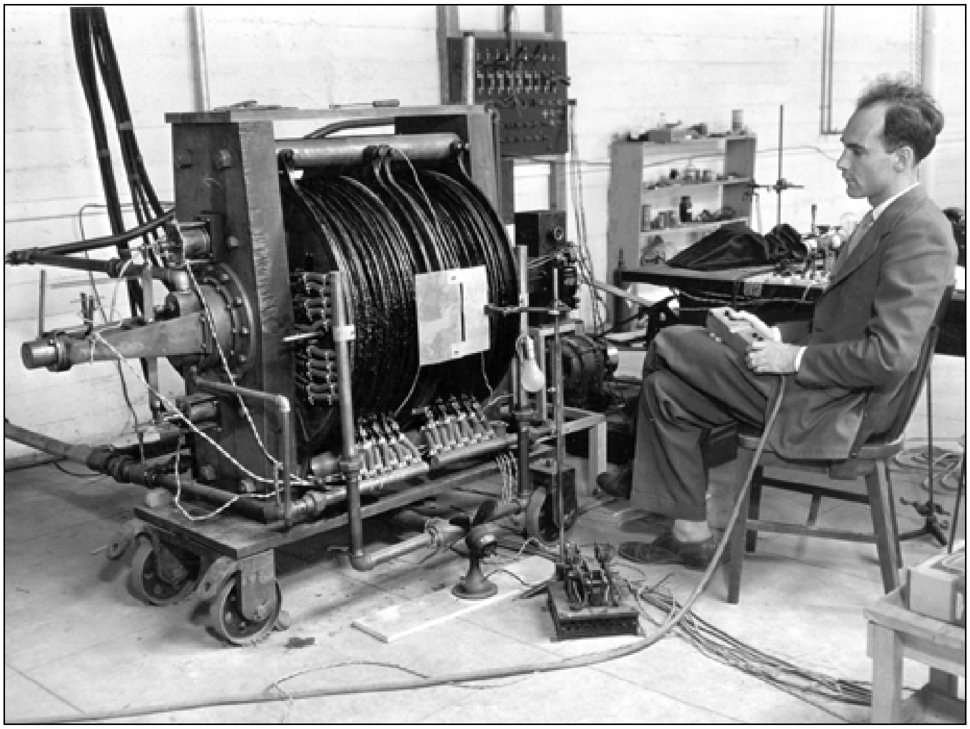

Carl Anderson with the magnet cloud chamber with which he discovered the positive electron, or positron. For this work he won the Nobel Prize in physics in 1936. Caltech Archives.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH CARL ANDERSON

BY HARRIETT LYLE

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY

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LYLE: I know you were born in New York City and that you moved to Los Angeles when you were seven. I wondered if you would tell me a little bit about your family.

ANDERSON: I wish I had discussed things with my family more than I did. You know when you're a kid, you don't think of learning things that you will be interested in in later years. What do you want to know about my family? They both emigrated from Sweden when they were eighteen or nineteen years old, something like that. I don't know; I wish I knew. My father spent most of his life in the restaurant management business. Now, what he did the first five years or so after he reached New York City from Sweden, I don't know. I wish I'd asked him. So I don't know those things and I don't know what my mother did.

LYLE: Do you know anything about her family, for example?

ANDERSON: I have met my father's family. My grandfather was a farmer in Taby, which is a suburb of Stockholm, Sweden. I visited there in 1926 and saw my paternal grandfather.

LYLE: Was this on your Junior Travel Prize trip?

ANDERSON: Yes. On part of it we went to Sweden.

LYLE: Did you get to plan where you went on the trip?

ANDERSON: In those days, it was extremely informal. We didn’t have to have a project. These travel prizes have been revived, and people submit projects—why they want to go to a certain place, to study irrigation in Egypt, for example. In the old days, people didn’t have to have projects; you just went to Europe. But we were coached about all sorts of things by members of the faculty who had interests in the travel prize project. We were given books—Breasted’s book on ancient history and medieval history and books on art. So we did, and enjoyed it very much. We visited many museums.

LYLE: How many went?

ANDERSON: There were two of us. That was the first year there were two. And the following year I think five or six went. They happened to get funds from somewhere.

LYLE: So did you talk to Noyes, then, too? Who set up the itinerary? I read once that Noyes had a distinct plan.

ANDERSON: It was up to us, really, where we wanted to go, although suggestions were made. The people most interested in the travel prize winners the year I won it were Alfred Noyes and John MacArthur, who was Dean of Freshmen at the time and was a great person who took a real personal interest in students. And so was Noyes. John MacArthur was a professor in the humanities division, and he taught French and German, which were required courses of all science majors in those days. And he was also the Dean of Freshmen. Noyes was head of the chemistry department, and a very distinguished chemist, as you know. He also took an interest in undergraduates. I remember he used to ask one, usually two, people to go camping with him.

One of his favorite places was Painted Canyon. You go out toward Palm Springs and then head east. It's at the foot of the mountains along the east side of Coachella Valley, so it's on the opposite side of the valley from Palm Springs. We used to camp there, and this was one of the greatest experiences for a young fellow to be able to chat intimately and informally with a world-renowned chemist. I think I was a freshman, or maybe a sophomore, and we took several trips. He also bought a house on a cliff in Balboa, where he used to go during the summer. He was a bachelor.

LYLE: He had a big car. I remember seeing pictures of it.

ANDERSON: He had a four-cylinder Cadillac, yes. So I spent several weekends with usually one other student, at his house on the cliff there. It was built out of stone—big cast masonry blocks that were used in those days. And then there was a stairway down to the water, which was about twenty feet below the house. I thought that was the same spot where the marine biological lab is, but maybe not. But it's certainly near there.

LYLE: How did you know Noyes, through the chemistry class? How did you meet?

ANDERSON: I guess because I was a pretty good student, and he took an interest in high scholastically rated students—a real personal interest. I remember at the end of the first term at Caltech, I thought I would be lucky if I could get by with all C's, or 2's, I guess they were in those days. But I happened to be the number two man scholastically in the whole freshman class, which was a great surprise. And then I thought, when I looked at the number one man, that I ought to be able to be number one man. The second term, I was number one. So I had a good scholastic record. And I think Noyes probably—he couldn't have done this for the whole freshman class, obviously. So that was a great thing. I remember once I brought a book along. I said I thought I'd do a little studying. And he reprimanded me for doing that, because I wasn't supposed to study when I was out camping with him.

LYLE: What would you do on the camping trips? Would you go hiking?

ANDERSON: Yes, we'd go hiking. He wasn't in too good health, but we did go hiking. We walked, I remember one day, almost all the way from Balboa along the beach to just north of Laguna. It was several miles walking down the beach.

LYLE: Were there many houses there, then, or was it pretty isolated?

ANDERSON: No, it was all open country. The land that's now a hundred thousand an acre was probably a hundred dollars an acre in those days.

LYLE: So you knew him pretty well, then, before you took your trip to Europe on the travel prize.

ANDERSON: I got to know him very well, yes.

LYLE: So you told him that you had your grandparents in Sweden, and so they just arranged it.

ANDERSON: Oh, yes, we put Sweden on our itinerary.

LYLE: You had a six-months trip?

ANDERSON: It was supposed to be six months. We came home shortly before six months, because we wanted to get home. We missed California. We were probably five months or so, actually in Europe. We bought bicycles in Munich with the idea of bicycling up through Germany and into Holland and Belgium and so on.

LYLE: Just the two of you?

ANDERSON: The two of us. We were together all the time. But we never got outside the city limits of Munich. We had made a deal with the fellow we bought the bicycles from, that if we didn't want them, we could return them and get a partial refund. The trouble was, it rained all the time. And we decided that it was not a very practical way to travel.

LYLE: So when did you get to Europe, what month?

ANDERSON: We left on the trip at the end of the second term, which was in March, and sailed from New York to Naples, and arrived in Naples in the last half of March. Spring had already hit Naples by that time, so the whole city was just full of flowers, and it was the biggest contrast between New York City and Naples that you could imagine. We liked Italy and spent about a month in various parts of Italy—traveling most of the time. Living was pretty cheap, and we were on a tight budget.

LYLE: Was the Institute paying for all of the food and lodging?

ANDERSON: Yes. I think the prize was $900, which was just enough to make the trip if you were economical. And then Dr. Noyes slipped us each $50, just before we left, to spend going up to Gornergrat and Jungfrau Peak. He loved the mountains in Switzerland. And he said if it was foggy the first time we went up there, we should go up again. But we had to see Jungfrau Peak and the Gornergrat on clear days. To get to Gornergrat you take a cog-wheel little railroad car from Zermatt up to about 10,000 feet. And then you have a 360-degree view of Alps. That picture right there was taken from Gornergrat. That's Monte Rosa, the second highest peak in Europe, and I just happen to remember it's 15,211 feet high. We decided to climb it. So we did, and that's the only mountain I've ever climbed in my life.

LYLE: Did you take this picture on that trip?

ANDERSON: No. I took some pictures from the same location which show much more snow than that picture shows. That's the Gorner Glacier coming down between where the picture was taken and Monte Rosa. We were the first people to reach the summit that year, because it was a year of heavy snow. We hiked over the Gorner Glacier to a little hut. We had a guide, and he prepared some tea, and we were to start climbing at midnight so we could get up the summit before the snow got too soft. I didn't sleep a wink, of course. We left at midnight, and the snow was hard going for the first mile or two. I sank to my knees almost every step. Fred Ewing, the other student that was with me, was much heavier than I was and had smaller feet, and he sank to his hips. The guide was a little shriveled up fellow with great big feet, so he walked around on the surface, as though he had snowshoes. Well, we finally managed to get to the top, just at dawn, which was great. You could look into, I think he told us, five or six different countries. We had planned to climb the Matterhorn, but it was too early in the year to hope to do that; but we did look down on the Matterhorn.

So our trip was not all business, by any means. The instructions that we had were to go to Europe and travel.

LYLE: Had they made plans for you to meet certain people, or did you just go from city to city on your own?

ANDERSON: No, we had no appointments to meet any certain people. But we did in Munich attend a class given by Sommerfeld, who was, as you know, a very famous physicist at that time. That was great. We read in the paper of American Students' Week in Leiden, Holland. So we made only a very slight revision in our itinerary so we could be in Holland in Leiden during that week. And that was great. There were very few American students—I don't remember now; something less than a dozen. Lorentz, who was a professor at the University of Leiden, spent most of his time during that week with the visitors, the American students. And that's where I first met Oppenheimer. He was one of the people from the United States who was attending this American Students' Week. Lorentz spent quite a bit of time with the students, and there were so few that I got to have many chats with a very famous physicist, which was very interesting and inspiring. We also got to meet Kamerlingh Onnes.

LYLE: Were the students mainly physicists?

ANDERSON: I don't think so. I don't know. Oppenheimer was a physicist.

LYLE: Did you think of yourself as a physicist yet?

ANDERSON: I was majoring in physics when we left to go to Europe, so I was a budding young physicist. All my life, from as early a time as I can remember, I wanted to study electrical engineering. This goes back to long before graduating from grade school. In those days, automobiles, many of them, used ordinary dry batteries—three inches in diameter and eight inches high—and they would have to be replaced when they ran down. I would go to the garages and get these old batteries, and some of them were practically brand new. People had them replaced when they didn't need replacing. So I used them for different kinds of experiments. And all through high school I was hoping to become an electrical engineer. The thing that changed my mind was the third term of my sophomore year at Caltech. There was what they called Section A, which was supposed to be some of the better students put in a special section that did the three-term regular physics course in two terms. And then Ike Bowen took the class for the third term and talked about modern physics. And that was really my first introduction to physics. It was something that was great, interesting, wonderful, and I learned you could even make a living doing it. So then I changed my course to physics.

LYLE: How did you think you made a living doing it? What do you mean?

ANDERSON: Well, in high school, nobody told me what physics really was. And there was no way to find out. Physics, in high school, was what was in the physics books—like steam shovels and pulleys and things. But I learned from this course that Ike Bowen taught, that there was much more to physics than what you learned in high school. He was doing physics and making a living doing it, so I learned that you could do that. And then I changed to physics as a major, but I got a degree in both engineering and physics, because the courses were quite similar.

LYLE: Okay. I want to talk a little bit more about that time. But before I go on, I would like to go back and find out how you got interested in science. Did your father encourage you or your mother or a friend or what?

ANDERSON: No, the interest didn't come from my family. My father was a completely non-technical person. So I didn't know what physics was. I was interested for as long as I can remember in technical things. I built crude radio sets when I was very young.

LYLE: But how did you get interested in that, in building those things?

ANDERSON: I don't know. Nobody told me to do it.

LYLE: Who encouraged you to do it? Anyone?

ANDERSON: I wasn't encouraged or discouraged.

LYLE: Did you have anybody to do it with?

ANDERSON: There were many kids around the neighborhood, but I don't think any of them when I was seven, eight, nine years old, got me interested in technical things. But certainly by that age, my interest was highly developed.

LYLE: Well, what did you think about your childhood education? Was this interest entirely outside of school?

ANDERSON: Well, when I was ready to start high school, or just before, the family moved to a part of Los Angeles that was right at the borderline between Glendale and Los Angeles. So I was planning to go to Glendale High School. But they wouldn't let me in because we lived in Los Angeles, even though it was a hundred yards or so from the boundary of Glendale, because they had an unusually large enrollment or something. So I ended up by going to Poly High School, it was called in those days.

LYLE: Was that a public school or a private?

ANDERSON: Public school. It was a high school, a technically oriented high school, at the corner of Hope and Washington. It later became a trade school, and then disappeared. I don't know what's there now. So I took technical subjects—four years of math in high school, for example.

LYLE: Was it for both girls and boys?

ANDERSON: Oh, yes, it was a regular high school, but technically oriented—L. A. Polytechnic High School.

LYLE: Do you think it was a better education then you would have had, say, at Glendale?

ANDERSON: I don't know, because I never went to Glendale. I was very happy with it, and I was not critical. I didn't try to ask myself whether I was getting a good education or not. I just went to high school. I was interested in electrical engineering; and we had a good laboratory of electrical machinery—motors and generators and alternators and transformers. So I studied quite a bit of electrical, technical things.

LYLE: Were there other students who were also really interested in this?

ANDERSON: There were two of us who were similar in our interests. In fact, we're still good friends. There were four of us in the senior class who wanted to go to Caltech, It was a pretty large school—I don't know how many were in the senior class—but there were four of us who were good friends and we decided we wanted to go to Caltech.

LYLE: How did you know about Caltech?

ANDERSON: I don't know. I had a very good physics teacher in high school, and I may have learned of the existence of Caltech from him. It was not from visiting Caltech on students' day—I doubt if there was anything like that in those days. So we talked to various teachers, and all of them except this physics teacher advised us strongly against going to Caltech. They said, "You probably wouldn't make it. It's a hard place; it's hard to do a good passing job at Caltech. And even if you did, it wouldn't be worthwhile. You'd be so worn out at the end of four years"—and arguments like that. Except our physics teacher. He thought it might be a good place to go. So we all four applied and were accepted and got our B.S. degrees on the same day. We all applied for graduate work at Caltech, and all got our Ph.D.'s on the same day, all four of us, but in different fields.

LYLE: Did you work together, then, as a kind of a team as you went through?

ANDERSON: No. Two of us were close friends and were both majoring in physics. In fact, I still see him [Louis Gazin] once or twice a year. They live in Washington, D. C. He was one of the four finalists in the competition for the travel prize, but while I was in Europe, he, for some reason, decided to become a geologist, a paleontologist. So when I came back from Europe, he was no longer a physicist; he was a geologist and did his graduate work at Caltech and went to the Smithsonian in Washington—and he's still there. He's retired, but he spent all his life as a paleontologist at the Smithsonian. But we were close pals all through high school and through Caltech. When we were undergraduates, the geology department, I think, was first established; Buwalda came. And this friend of mine, while I was in Europe—if I'd been here, I would have told him that he should stay in physics, but I wasn't able to. And maybe he's happier as a geologist. I don't know. But we're still good friends, except we're far apart; but maybe twice a year or so, we see one another.

LYLE: When you started Caltech, you were living at home?

ANDERSON: I lived at home. My mother and father separated, so I really had my mother to support. I couldn't possibly think of going away to college and living away from home. There just wasn't the money there. I don't know now how we managed as well as we did. My mother must have been a marvelous business manager to live on the small income that we had.

LYLE: But was she pretty interested in what you were doing?

ANDERSON: She was interested, just because I was doing it. I think she was interested in her kid. Anything he wanted to do was of interest to her. I was in no way steered—I was not pushed into any direction by either of my parents. I was not given a great deal of advice by either of my parents.

LYLE: Did your friends from high school also live at home or did they live on campus?

ANDERSON: Louis Gazin lived at home. His family was of very moderate means. He couldn't afford to go to Princeton, for example, or to go away to school. The other two—Thomas Gottier and Bernard Moore were their names. Bernard Moore's dad was head of the chemistry department at the high school that we went to.

LYLE: Was it easy to get involved at Caltech, even though you weren't living right on the campus?

ANDERSON: By getting involved, do you mean running for student body president or class president or something like that? I was not involved in college activities. I didn't run for any office such as class president. I worked hard as a freshman. I think I worked harder that year than any other year before or after in my life. I worked very hard in studying. And it wasn't hard to do, because I was interested in what I was doing, but I did study very hard and I found it altogether different. I never had to study in high school. I mean, there you just don't have to study. But at Caltech, you do have to study.

LYLE: And also, you were very worried that your teachers might be right, the ones that said not to go to Caltech.

ANDERSON: All four of us were in Section A, which was supposed to be the top 10 percent of the class.

LYLE: Why did they put you in Section A at the beginning?

ANDERSON: Just because you had good grades. It started certainly in the sophomore year. I can't remember if it started in the second or third terms of the freshman year. But it was because I was put in Section A and had this contact with Ike Bowen, that I switched to physics. I might have learned about physics some other way, but I happened to learn about it from him in this special third-term course.

LYLE: I wanted to ask what instructors you had that you remember as having particularly good classes. His sounds like one of them, certainly. Were there any other classes that you remember that you really enjoyed?

ANDERSON: Well, I admired Watson a good deal—Earnest Watson. He taught the Section A physics classes. For some reason, they used regular professorial staff to teach the Section A boys. I don't know why.

LYLE: But the other sections they didn't?

ANDERSON: Well, they used teaching fellows in those days, just as they do now. And I had some teaching fellows who were very good. In fact, Richard Badger was one teaching fellow that I had in chemistry, who later became a professor and spent his whole life at Caltech as a chemistry professor. There was the usual griping about teaching fellows—they didn't know what they were doing or they were talking over the heads of the students—just like you hear today. But I think less griping in general, because people were not in those days as critical as they are today about whether things are the way they ought to be. I think people accepted things more in those days with a less critical attitude and thought the way it was was the way it was supposed to be and it was all right.

LYLE: What about the math classes? Did you like math?

ANDERSON: Yes, I always liked math. I remember I had Morgan Ward as a teacher, whom I thought was an extraordinarily good teacher. Probably the best teacher of all was Richard Tolman. That was when I was a graduate student that I took a course in relativity from Tolman. I would put him as my number one teacher.

LYLE: What did you like about the way he taught?

ANDERSON: I don't know. He had a very strong and pleasing personality. He was the sort of person that people liked. And if I had to analyze just why, that would be hard. He also had a way of pretending that he didn't know, at times, as he was teaching. And he'd ask the students to help him out. I don't know if the students saw that this was just a technique of his.

Begin Tape 1, Side 2

ANDERSON: I don't know what makes a good teacher. He was a very interesting person. He had a way of pretending that he didn't understand something at times in his classes, and would ask the students to help him out. For example, he'd say, "I'm not a mathematician. Is there a mathematician in the class?"

LYLE: Do you think he didn't know sometimes?

ANDERSON: He knew, of course. This was a pretense, a teaching technique. He didn't use it every day; but his classes were bull sessions. It was not a formal lecture where he did all the talking. The whole class participated and it was fun, and I also learned a lot.

LYLE: How big a class did he have?

ANDERSON: Oh, I don't know, fifteen to twenty, which to my point of view, is an ideal size class. I might mention that Millikan, as far as I know, always taught a class when he was, in effect but not in name, president of Caltech. He was much more than president. Well, if there hadn't been a Millikan, there wouldn't have been a Caltech—I'm sure of that. He gave a course called "Electron Theory" to first-year graduate students. In the first three or four minutes, he'd write an equation on the board that had something to do with electron theory. But then he would almost immediately begin to reminisce. He wore these pincer glasses that he put on one finger, and would then tell about what happened in 1906, for example, in connection with his working on the oil drop experiment and the day he happened to think of using oil instead of water. So the whole class was that sort of thing, and it was great. It was, again, much more valuable than if he had talked in a formal way about electron theory. You could learn that by reading in the book, or having somebody else. But here was Millikan reminiscing.

LYLE: Did people understand that, though? It seems like it takes a certain level of interest in history or interest in his work to accept that.

ANDERSON: I think, as a whole, the students liked it. There may have been a few who felt they were being cheated because they didn't learn much about electron theory, which is true. Anyhow, that was one of the great experiences, I thought, of my graduate years at Caltech.

LYLE: What did you do about testing and things like that in a class like this? It seems that students would be a little worried about what they were expected to learn.

ANDERSON: Yes, and as I look back I can't remember, I think he gave examinations. I'm sorry, that's vague. Maybe he didn't; maybe he passed everybody. Now we're going to jump to several years later, when I was in Millikan's office, talking to him one day. I was doing research on cosmic rays, and that was his great interest at the time. We were talking about cosmic rays—and this story is true and it's sort of funny. The registrar came in and said, "Dr. Millikan, you gave A's, B's, and C's to your students in your class." And Dr. Millikan said, "Yes; now take this first man for example. He was a good student; he wasn't top-notch, I gave him a B." and the registrar said, "Oh, I wasn't questioning your assignment of grades to the students. I was really pointing out that Caltech has the 4-3-2-l system and not the A-B-C-D system." So then Millikan said, "Well, I could change these to numbers, or we could change the system at Caltech." There were two solutions to this problem. And it is true that Caltech did change. In those days, grades were given in numbers. MIT, for example, had numbers, and the top grade was 1 down to 4—4 was a D. Caltech had numbers, and the top grade was 4 down to 1, which was a D. This caused some confusion. Well, anyhow, I thought it was interesting that Millikan saw two solutions to this problem—he could change the sheet of paper, or the system could be changed. So this does show that he did after all give grades and must have had some examination or something to base them on. But I have no memory of taking an examination in his course.

LYLE: Is this the only class he taught?

ANDERSON: Yes, I think this is the only class we taught. He was division chairman of Physics, math and—I don't know if astronomy was in there in those days. But he was, certainly president.

His official title was Chairman of the Executive Council, but he ran Caltech to a much greater extent than any succeeding president and that was part of his being. For example he had to approve expenditures in all of the divisions. And he was very tightfisted—as he had to be, because money was very hard to come by and very limited.

You mentioned how did I hear of Caltech. I remember another story. This, again, was years later. I was traveling to an [American] Physical Society meeting on the train, and in the club car, happened to get talking to a fellow and he asked what did I do. I said I was a professor. He said, "Where?", and I said, "At Caltech." "Oh, is that part of UCLA or is it part of USC or what is Caltech?" So I said, "No, it's an independent college; it has nothing to do with SC or UCLA." Then Millikan's name happened to come up, and he said, "Oh, you mean Millikan's school!" And he knew all about it; but he knew it as Millikan's school, not as Caltech. The name Caltech didn't mean anything to him.

LYLE: Was that true, in general?

ANDERSON: Well, Millikan was a great public figure and had a knack for getting publicity. I'm sure it was necessary in order to raise money and develop Caltech, although many people criticized him for this and said he was a publicity seeker. But I do know—I got to know Millikan extremely well—that every move he made was in the interest of Caltech; not his own personal interests as much as Caltech's interests.

LYLE: Did you know Dr. Millikan when you were an undergraduate? Did the students get to know him at all?

ANDERSON: The chief contact that the undergraduates had with Dr. Millikan was—well, at least the freshmen—he had almost every week a freshman tea at his house, at which there might be fifteen or twenty freshmen that were invited. And I guess they went through the class, inviting something like fifteen or twenty each time, so that all of the freshmen had that contact with Millikan. He did not teach any undergraduate courses. Millikan was chairman of the physics, math and astronomy—as I say, I don't remember if astronomy was part of the division in those days—but everything that had to do with undergraduate students was handled by Earnest

Watson. Although Millikan did write a book, along with Mills—Millikan and Mills—that was for many years, I think, more or less a beginning physics undergraduate textbook and very widely used in many colleges in the country. I don't have any statistics on this. He was an extraordinarily busy man, and spent a good deal of his time with research all through his years here.

LYLE: I'd like to talk about that, but I'll wait to do that next time. The other question was this: Did you have any courses in astronomy as an undergraduate?

ANDERSON: Not that I can remember, no. As an undergraduate, I don't remember that I ever went up to Mount Wilson.

LYLE: Okay. The biology division wasn't started here until the late twenties, but did you study biology?

ANDERSON: I did not take a course in biology. I think Thomas Hunt Morgan came probably when I was a senior or maybe after I had graduated. It was in the late twenties.

LYLE: Yes, 1929, I think. Did you meet Linus Pauling in any of the chemistry classes or engineering?

ANDERSON: I can't remember when I first met Linus Pauling. I remember Noyes talking about Linus, what an extremely promising young chemist he was. These were during our camping trips and so on. He would occasionally talk about Linus, as well as a lot of other things. So I heard about Linus through Noyes, and what an extremely brilliant fellow he was, before I ever met Linus.

LYLE: I think he was maybe a little bit ahead of you, so I thought he might have taught one of those classes.

ANDERSON: He was a few years ahead of me, yes. We were not, by any means, classmates. In later years I got to know Linus very well, after I was, I guess, a research fellow or at least on the faculty—after I got my Ph.D. degree.

LYLE: And during this time when you were an undergraduate, did people like Einstein come, or did they come later?

ANDERSON: Einstein visited Caltech, I think, around 1933. I remember hearing him give a series of lectures in 201 Bridge. I had my degree then—I guess I was a research fellow. I never had any close contacts with Einstein. I was introduced to him and talked to him for a brief time, but I never in any way had what you could call close contacts with him. Incidentally, Millikan managed to get maybe even most of the world famous physicists to visit Caltech. Lorentz, for example, and Ehrenfest and Heisenberg and Dirac, and you could just go down the line, and he had them come to stay for a few days. So we at least listened to them give seminars—all in 201 Bridge. So that room has held, at one time or another, just about all of what you might say were the really top-notch physicists.

LYLE: Did that make it a lot more exciting to work here?

ANDERSON: Yes, to me it was very exciting, to see these people, even if you didn't have a chance as a graduate student to meet them or have any real close contacts with them. But that's something that I think was an extremely good idea, and I think in those days, an unusual idea. Nowadays, people travel all the time. I mean, you go to Europe overnight in a few hours. And now physicists are always traveling. But in those days, it was unusual for a European physicist to come to the West Coast.

LYLE: And Millikan must really have wanted them here, to be able to afford to invite them, because money was pretty tight.

ANDERSON: It was probably to help the prestige of Caltech. And whether it did or not, in the public mind, I don't know. But, certainly, he must have had that in mind when he invited Einstein, who was a public figure even in those days. But many of the other top-notch physicists were not known by the general public the way Einstein was. It was a great idea, and just what was in Millikan's mind, I don't know. It certainly helped educate the Caltech students.

LYLE: Of these people who visited, are there any of them that you remember particularly the way they gave the talk?

ANDERSON: Oppenheimer was on the faculty at Caltech and at Berkeley at the same time. So he used to commute and spend one term at Caltech. And Oppenheimer, who later became an extremely eloquent lecturer, was not in those days a good lecturer. He didn't speak loudly enough, and he didn't really face the audience as much as I guess he should have. I remember Ehrenfest, who was a very well known physicist in that day, was visiting Caltech, and he sat in the first row. And he said, "Oppenheimer, is it a secret?" I just happened to remember that incident. But Oppenheimer was not a good teacher. I took a course in quantum mechanics from Oppenheimer when I was a graduate student. There were about forty people or so in the room, in Bridge. The room was packed. I didn't know what Oppenheimer was talking about at all; I had no idea what he was talking about. He, in those days, was not a good lecturer. He paced back and forth, and wherever he happened to be at that instant, he would write some squiggles on the blackboard—part of an equation—and they were scattered all over at random on the blackboard. And I wasn't prepared. I didn't have the background to understand theoretical physics at the level that he was speaking. So I went to his office one day and said that I was afraid I would have to drop his course, because I didn't understand it. I said I didn't have the preparation to take it. And he sort of pleaded with me not to drop it. And then he admitted that I was the last person to come up to him and ask to drop the course.

LYLE: That took a lot of nerve to tell you that.

ANDERSON: And he told me that. He really pleaded with me to stay. He said he wanted to have an official course at Caltech. So I said, okay. He said, "I assure you, everything will be all right at the end of the term. So I stayed registered as a student, and he had an official course. At the end of the term, he asked me what is the highest grade and what is the lowest grade at Caltech. And for an instant there I thought of reversing them, but I didn't. And I told him the highest grade and the lowest grade, which he should have known, and probably did. Anyhow, I got an A in the course.

LYLE: Did he get better? Could you understand it more as you went along?

ANDERSON: No, I didn't. It was over my head, all the way through.

LYLE: So you were taking the course officially. There were other people who were sitting in.

ANDERSON: The forty people stayed on till the end. They were more advanced students and they were theoretical students. And I assume that they were learning something, or they wouldn't have been there. But I was registered for the course. Although I must admit I didn't attend all of the lectures.

Another interesting thing about Oppenheimer: This was in the days when the first papers on the Dirac theory were being published. Tolman got Oppenheimer to agree to give a series of evening lectures two hours long—I think three a week—on the Dirac theory, for anybody who wanted to attend. So I attended the first meeting of that series, and Oppenheimer talked for two hours. We were perhaps a dozen or fifteen people in one of the rooms in Gates. And at the end of the two hours, Tolman got up—he always called Oppenheimer Robert—and he said, "Robert, I didn't understand a damn thing you said tonight, except…" Then he went to the blackboard and wrote an equation. He said, "That's all I understood." And Oppenheimer said, "That equation is wrong." And there was never a second meeting of this attempt on Oppenheimer's part to tell various people, mostly faculty, what the Dirac theory was all about.

LYLE: Was Tolman angry when he said that?

ANDERSON: Not at all, no. That's Tolman for you. Oh, no, they were extremely good friends. If Tolman didn't understand a damn thing, then I'm sure nobody else in the group did. I'm sure I didn't. And it became clear that nobody was getting anywhere.

CARL ANDERSON

SESSION 2

January 11, 1979

Begin Tape 2, Side 1

LYLE: Today I want to talk a little bit about your graduate work at Caltech, which started in 1921.

ANDERSON: Actually it started in ‘26. Well, I took some courses ahead of time, so I had nothing much to do in my senior year. Millikan was away on a trip, so I couldn’t talk to him at that time. But I did talk to Earnest Watson, whom all the students talked to, and told him that I would like to get started on some research because I didn’t have enough to do. So he assigned me to work with Lee DuBridge, who had just come to Caltech as a National Research Council fellow, to work on the photoelectric effect. So Watson told me to look up DuBridge. He needed somebody to help him, so I did. I guess I worked for him for maybe about three years. He assigned me a job to build a monochrometer for his photoelectric experiment. He had just gotten his degree, and he was going to spend some time as a postdoc at Caltech. So that went on for about three weeks. And then Millikan called me into his office and said that I shouldn’t be doing that. I should be doing something else—I should be working with Loughridge and not DuBridge. You see, Millikan really assigned research projects, I guess, at least in my experience. Nowadays, the faculty is very careful to find out what the student’s real interests are, so that he isn’t put on something that isn’t the best thing that he wants to do. Millikan told me that, so I looked up Loughridge, who was working on the photoelectric effect of X rays with a cloud chamber. He was just finishing up his work for his degree, and he left something like a week or two after I started working with him. So I had a room full of his apparatus and carried on and did some modifying and started working with it.

LYLE: Millikan must have known he was going to leave, right?

ANDERSON: Oh, yes, Millikan knew he was going to leave. I guess that’s why he felt he needed somebody to carry on that work. So I did that for four years, I guess, and greatly modified the equipment and did quite a bit more than Loughridge.

LYLE: So that's what you did as your graduate work.

ANDERSON: So that was my thesis work. I guess many months after I was working there, I happened to bump into Millikan and tell him that I didn't have any research adviser, faculty adviser, as you're supposed to have. And he said, "Oh, that's easy, I'll be your research adviser." So I was his student—although not once, during the time that I was a graduate student, did I discuss my work with him or was he in my laboratory. So I had a free hand to do things as I wanted.

LYLE: Who did you discuss it with?

ANDERSON: Oh, everybody, people next door. In fact,—Clark Millikan was next door to me in East Bridge in those days; Maj Klein was in the same room with Clark Millikan, so I got to know them pretty well; and other people. And I had some ideas, too. It was pretty obvious what needed to be done.

LYLE: But Millikan didn't come back to check up on this?

ANDERSON: No, no. I probably talked with him during those years as a graduate student about my research, but I have no memory of doing that. I do remember in the final oral examination—this is sort of interesting, too—it was scheduled for nine o'clock in the little seminar room in East Bridge. In those days, they had much bigger committees of professors—now, for some reason or other, three or four people will conduct the Ph.D. oral exam, but in those days there were eight or nine or sometimes ten. So I reported there at nine o'clock, and no one was present. And then E. T. Bell, the mathematician, came in. (I had a minor in math.) So he said, "Well, I'll start it off." He asked me about Bessell's equation, and I guess for about twenty minutes or so he questioned me on that. I just happened to know Bessell's equation pretty well, and I wrote it on the board, and he asked me various things about it. Then he said, "Well, that's enough; I'm through." So we sat there; nobody else was there and it turned out nobody came in until ten o'clock. I guess they had classes or something. Bell was interested in the history of mathematics. So I had a delightful forty minutes or so listening to him tell me all about Bessell's childhood. So that part of my Ph.D. exam was very simple. Well, then at ten o'clock, several people came in. I can't remember who they all were—I think it was Bowen, and Millikan, and Epstein I know was there. And I made one horrible blunder. Millikan asked me to give a review of the history of the photoelectric effect—that was my thesis topic, except I was using X rays. Of course, the photoelectric effect is involved with visible light, which Millikan became famous for, for showing that the Einstein equation applied—along with measuring the charge on the electron. Those two things were what he got the Nobel Prize for. Well, I forgot all about light and the photoelectric effect of light. So I gave a history of the photoelectric effect of X rays, the experiments and theories and so on, and ignored—or forgot—about visible light. If somebody had reminded me of Millikan's work I would have—I knew something about it, I could have included that. But I just completely forgot about it, and he didn't say anything, so it wasn't mentioned in the examination. I don't know if he ever held that against me or not. But the next morning I met him.

LYLE: Had you remembered by then?

ANDERSON: Well, all he said was that that was a corking good examination. And even by then, I hadn't thought—it wasn't until weeks later that I realized this horrible blunder.

LYLE: If it had bothered him, would he have said anything about it, do you think?

ANDERSON: I don't know. He didn't. It's very interesting, he could have said, "Well, the photoelectric effect is not limited to X rays," you know, or something like that. But he didn't even give me a hint about it.

LYLE: I know that at that time there were a lot of people doing research with X rays at the different laboratories. But what kind of safety precautions did you take?

ANDERSON: Well, the answer to that is, I think, there were no safety precautions. I had an X-ray tube that was enclosed in a quarter-inch-thick lead box, with a little slit in it, so the X rays didn't just go all over the room. So there was a beam of X rays, but they could scatter and in that way there'd be a weak background of X rays. And I don't know, I worked with those things and put my hand in the beam in adjusting things.

LYLE: But was the lead box there to protect people, or was it there so that the experiments would work and you could control the X rays?

ANDERSON: I didn't build it. That was one of the things I inherited from Loughridge. I did put in a crystal diffraction apparatus so I could select different wavelengths of X rays, which he didn't have, and to adjust that crystal, I'd turn on the beam and adjust it with my hands. I was not, I think, conscious of the fact that overexposure might be dangerous. Many of the early workers with X rays, with radioactivity, did get seriously damaged or ill. I've heard that Madame Curie had troubles because of her long exposure.

LYLE: And some of the people who were working with her. And also, she rejected the idea that their illnesses might be caused by radiation.

ANDERSON: I remember meeting someone who was an elderly gentleman—I was just a kid in those days—at one of the Physical Society meetings, whose hands were encased in rubber gloves. You could see that his fingers must have been extremely small—maybe part of the flesh was gone. But anyhow, I was told that that was damage from working with X rays in the much earlier days than I was working with them—the very early days, I suppose. I can't remember this gentleman's name, but he was quite a well known physicist.

LYLE: One of the questions I had wanted to ask was how you started working with Professor Millikan, so that answers that question. Did he have any other students, though?

ANDERSON: Yes. He was interested in cosmic rays. He was doing research on them—and not just as a figurehead. He actually would spend hours and hours taking readings on the intensity of cosmic rays with an electroscope, which essentially measured only the intensity of the radiation and told you nothing about the detailed mechanisms of what was going on. In fact, it didn't tell you what the cosmic rays actually were—whether they were X-ray like or were actually particles of matter. So he was active. He had a knack of sensing very early what were the important fields of research in physics, and he was the first man in the United States, I'm quite sure, who worked with cosmic rays. There were a few Europeans at the time. It turned out in later years that a lot of physics came out of the study of cosmic rays. And this isn't the only instance. There's the far ultraviolet work that Millikan started. He put Ike Bowen on that, when Ike Bowen was his graduate student. And that became a big broad field of physics that brought forth all kinds of important new things in physics. He started Charlie Lauritsen on the cold emission, where you have a cold wire—we all know a hot wire will emit electrons—but for cold emission, you have a wire, and then you put a strong electric field on it to pull out the electrons. Even when the wire is cold, you can do that. That was a very new field at the time. That was Millikan's idea, to put Charlie Lauritsen to work on that.

LYLE: Was he a graduate student then, too?

ANDERSON: He was a graduate student at that time. He was older than most of us, because he had been in business and had manufactured radio sets, I think.

So one day, I asked Millikan the question directly: "How were you able to sense the importance of fields of physics when they were hardly known to people and nobody was thinking about them? How come you got interested in them, even though they didn't seem very important to other people but then later became extremely big, important fields of physics?" The cold emission work that he put Lauritsen on made possible the construction of a million-volt X-ray tube, which Lauritsen built in later years. If he hadn't known about how to handle the cold emission effects, he could never have built the million-volt X-ray tube.

LYLE: Is that what he did his Ph.D. work on?

ANDERSON: Yes. Anyhow, Millikan's answer to that question was, and he said it as though he was completely serious about it, "I read *Science Abstracts*." Well, I told him I read *Science Abstracts*, too, but I don't get these ideas. But that was a property that Millikan had, to sense out in the very earliest times what later were to become very important, large fields of physics. And I think cosmic rays is an example. As far as I know, he was the only, as I said before, I guess, the only person in the United States who was really interested in them, and he had several graduate students working on them with electroscopes. And, as I said, he was not just a figurehead, by any means. He would come in Sundays or nighttime, whenever he had time, and actually take readings and make plots, in spite of his tremendous administrative duties as, in effect, president of Caltech.

LYLE: Was he also head of physics?

ANDERSON: He was head of the physics division, yes, and did much of the assigning of research projects to the graduate students. At least in my case, he didn't say, "Now, what are you interested in; what do you think you would like to do?" He simply said, "You ought to be working with Loughridge and not working with DuBridge."

LYLE: So what did DuBridge say about that?

ANDERSON: I don't remember. I told DuBridge that Millikan had reassigned me.

LYLE: Did he choose the project that DuBridge was going to work on? You said that he generally chose projects. Was that just for students or when a postdoc came in, did Millikan also put him on a project?

ANDERSON: I think not. I think postdocs, in almost every case, came with a research project that they wanted to work on. They had just gotten, in most cases, their Ph.D.'s and wanted in many cases to continue work in the same field, anyhow, as they had done their Ph.D. work in. They were experienced enough so they could pretty much manage their own research projects. I really don't know what the precise interactions between Millikan and postdocs were, but I would guess that the postdocs were almost completely independent in picking their own fields of research.

LYLE: So you were on this project all by yourself. Would you rather have worked with a group?

ANDERSON: No. In those days, most people did their research individually. As an old man speaking, I can say those were the good old days, when you had your own apparatus, and you didn't have to coordinate with large groups of people and plan ahead and schedule things—like in the modern accelerator laboratories, for example. There was no such thing. In those days it was an individual thing. And I like that, maybe mostly because I'm old. I know that when you go to meetings, nowadays, you see young people who are in these large groups, and they're extremely enthusiastic and excited and happy about that kind of physics. I've had no experience with it myself.

LYLE: Well, what about when you were doing your work—would you plan ahead and work out the timing for an experiment and then go do it? And then did you communicate with somebody else about the work, or did you just look at the data yourself and then decide what to do?

ANDERSON: Well, it was an individual thing, except you talked to other people all the time. They were there; they'd come in, and you'd talk to them, and maybe you would get ideas from other people.

LYLE: And they were enough aware of what you were doing that they could talk to you about it?

ANDERSON: I don't remember specifically anyone coming in and saying, "Look, why don't you try this." Maybe that happened, but I think it was pretty much—and I certainly wasn't alone in that. I think that was true of most people. I didn't have a research adviser that I talked to everyday, for example, or even once a month or so.

LYLE: I would think that one might get discouraged—it seems that a lot of science is very frustrating, and it goes on and there are things that don't work out.

ANDERSON: Yes, most things don't work out. Well, I was not discouraged to any great degree. I mean, you might be discouraged in the sense that that day you hadn't done anything, or that week, nothing had worked out. But I have no memory of getting depressed about being a graduate student.

LYLE: In our last talk, you mentioned that you had met Professor Lorentz on that trip that you took, and you said Oppenheimer was there. I was wondering why he was there. Was he there to visit Lorentz, or was he there for the meeting?

ANDERSON: This I don't know. My guess is that Oppenheimer was in Europe at the time, probably in Germany, and decided as a lark or for whatever reason to go to Leiden, which wasn't very far. He might have been in Heidelberg at the time. I know he spent, in those days, considerable time in Germany, in Europe. I'm not sure, I think he worked with Max Born for a while—as most American theoretical physicists did in those days. The thing to do if you were studying theoretical physics was to go to Europe, because that was where the prime leading theoretical physicists were, of which Lorentz was one. Although I don't know if Oppenheimer ever had any close association with Lorentz.

LYLE: When you visited with Oppenheimer, did you talk to him about physics?

ANDERSON: I think the answer is no. I was in my junior year, and didn't really know anything. I didn't know much physics. I was a junior and taking introductory courses. My presumption is that Oppenheimer was extremely more sophisticated in physics than I was.

LYLE: What about Lorentz? Your relationship with him was just that you knew he was famous?

ANDERSON: Yes. I was an undergraduate. It is true that Lorentz—how he could have done what he did without discovering relativity, I will never understand. Because he had worked out all of the mathematics. He just didn't take that one little step. For special relativity, there are two basic hypotheses: One is that there is no hitching post in the universe. That is, you cannot answer the question, "Am I really moving or not; or is anything really moving or not?" There was a great deal of interest in those days in the ether, which was supposed to pervade everything, even free space. The question would come up whether you were moving with the ether, through the ether, or did you drag some of it along with you and so on. But anyhow, Lorentz had essentially worked out all the mathematics of special relativity. In fact, they are now even today, called the Lorentz transformation equations, the Einstein equations. He just didn't take that step. Well, the second thing is, the speed of light is constant for all observers, no matter where you are, as long as it's free space. However fast you're going, or with respect to whatever, anybody who makes a measurement of the speed of light in free space gets the same number. That's the second hypothesis of special relativity. I will never understand how Lorentz could have done what he did and be the sort of brilliant person that he was, without discovering relativity. But we're getting off the track maybe, I don't know.

LYLE: No, not necessarily. Did you hear Einstein lecture at all?

ANDERSON: I did only in 1933, I think it was, when he visited Caltech.

LYLE: And it was to a big group?

ANDERSON: A big seminar. I never sat down and had an intimate talk with Einstein about relativity. I wasn't sophisticated enough to talk about it.

LYLE: Well, what I wanted to know was how he presented his ideas. I've heard that he was very clear in the way he would tell things.

ANDERSON: Yes. Lorentz had that virtue—if it's a virtue. While you were listening to him give a lecture, you thought you understood everything he was saying. Epstein had that characteristic in his courses. His lectures were so crystal clear that you thought you understood everything he said. And therefore, in my case, I didn't study because I thought I knew what he had said. That can fool you, because then the next day you try to think about what you thought you understood the day before and find that you don't understand it.

I was president, as a junior, of the Tau Beta Pi chapter. And I went up to Lorentz, even though he was a very elderly gentleman at that time, and asked him if he wouldn't come and give a talk to our Tau Beta Pi group. And he said, "Sure." And he did. I don't know what that means. It shows the sort of kindly kind of person he was to spend an evening with a small group of

Caltech undergraduates. I forget what the topic was.

LYLE: I read also in von Kármán autobiography that you were working in his lab because they had the high-voltage equipment. If you were working in their lab, did you get to know those people pretty well, and the work they were doing? Can you describe that period a little bit?

ANDERSON: I was never a close personal friend of von Kármán. I had many conversations with him, but not any long ones and not technical ones. I certainly admired him, and I heard many technical lectures that he gave. I was at his house one time, I remember. I don’t know why, or who else was there; it’s very vague. But I was never a close associate with von Kármán. He would ask about—I guess he asked everybody about his work. So on a few occasions, I talked to him about what I was doing.

LYLE: Well, he implied in his book that you were a member of this group that used to meet at restaurants in Pasadena to discuss teaching or research. Do you remember that at all?

ANDERSON: No, that’s not true. There were groups. I don’t know too much about how many people were involved or who they were, but there were groups, I think, who were interested in communism and Marxism, and did have meetings at various people’s houses and so on. I was not at all interested in politics to that degree. I was never a member of any such group, and I don’t know where von Kármán got that idea. I don’t know what kind of group you’re talking about.

LYLE: He implied that it was just a group that would meet at different restaurants, and they were talking about teaching methods, for one thing.

ANDERSON: No, I think that’s a mistake. There were many people who were sincerely interested in teaching and trying to develop new methods and techniques—as there are today at Caltech. No, I’m sure I was never a member of such a group.

LYLE: So you were working pretty much alone. But as a graduate student, were there other people that were particularly important to you, that you remember or that you spent time with?

ANDERSON: You mean people who were graduate students at the same time? I saw a good deal of Vic Neher, whose research lab room adjoined mine, and he later became a member of the faculty as I did, and he spent his life at Caltech. Neher spent his life working with cosmic rays, developing the electroscope techniques to a very high, sophisticated degree, and worked much more closely with Millikan than I ever did because this was Millikan's technique that he was using—electroscopes. Neher took many trips all over the world with Millikan—flying balloons, mostly—in Northern Hudson Bay, in Asia, in India. So there were other graduate students—I can't remember if there were others at that time who later became faculty members of Caltech.

LYLE: Now I'd like to go on to the l930s at Caltech. I wanted to ask you why you decided to stay on, after you had your Ph.D. and the circumstances around that.

Begin Tape 2, Side 2

LYLE: Can you tell me why you decided to stay and kind of the circumstances around it?

ANDERSON: Well, that's sort of an interesting story. About a year before I was to get my Ph.D. I went to Millikan and asked him if there was any way I could spend one more year at Caltech. I had two things I wanted to do. One was to learn something about quantum mechanics. I was having a very difficult time, and every physicist had to know something about quantum mechanics. And then I had an idea, which grew out of the work I did for my thesis, of working instead of with X rays, with gamma rays of higher energy than X rays, but with a cloud-chamber technique. In other words, to study the interaction of gamma rays with matter at as high an energy as I could. And the highest energy gamma rays then available were the gamma rays from thorium C'', which were 2.1 million electron volts. I was going to shoot those through the cloud chamber in a magnetic field.

LYLE: You already had this magnetic field?

ANDERSON: No, I didn't have a magnetic field for the photoelectric effect. It would mean building a new apparatus. Another reason I wanted to do that was that Chao-ying Meng, a

Chinaman, was a postdoctoral fellow, and was working with thorium C’’, the most energetic gamma rays available at that time—you could only get them from natural radioactive substances. He was finding anomalous effects of scattering of gamma rays and absorption. He had no way of observing the details of what he was doing—he was using electroscopes, which sort of integrate things, and measuring intensities at various angles in relation to pieces of lead absorber and so on. It wasn’t known at the time, but he was actually observing the annihilation radiation of positive electrons. It’s now known, but it wasn’t known then. I went to Millikan to ask if I could spend another year at Caltech to do that same type of work that Chao was doing, but to use a cloud chamber, where you could see the details of what’s going on. And I’m quite sure that if I had done that, the positive electron would have been discovered before it actually was, because that was the direct way, in hindsight, to attack the problem.

LYLE: Because it was more powerful?

ANDERSON: Well, you had enough energy, at 2.1 million electron volts, to create electron pairs. Not that I thought of doing that, because nobody was thinking seriously of a positive electron. But if one were, it would be the ideal apparatus to use to study pair production. And I’m sure that the positive electron would have appeared very early in that experiment had I ever been able to do it, but I wasn’t. So I went to Millikan and asked him if I could stay on at Caltech for one more year, to study quantum mechanics and to do this experiment. His answer was a very definite no. He said, “You have done your undergraduate work here, you’ve done your graduate work here, you should go somewhere else. You’re getting very provincial. You’ve got to go somewhere else.” And the only way that you could go somewhere else in those days was to apply for a National Research Council fellowship. He said that’s what I should do, and not stay on at Caltech. So I applied for a National Research Council fellowship. I wrote to Compton at Chicago and described the experiment to him in a letter and said that I had applied for a National Research Council fellowship. I hadn’t heard yet whether I had gotten one. Besides, you had to get permission to go where you wanted to go. And he wrote back a very nice letter and said that he would be glad to have me there, and he would do his best in providing facilities, equipment, and some money to build this equipment. That never happened, because Millikan called me into his office one day, at about that time, and said he wanted me to stay on at Caltech for another year. By that time I had sold myself on the idea of going to Chicago to do this experiment. I wanted to do it; I thought it was a very good one and I was very anxious to do it. So I used all of the arguments with Dr. Millikan that he had used on me—namely, that I'd done my undergraduate work at Caltech, I'd done my graduate work, and I should get a broader look at the general field of physics. And he said, "Yes, that's all true; but your chances of getting a National Research Council fellowship would be very much greater if you had another year at Caltech." And it turns out that he was chairman of the selection committee at the time. So I stayed on at Caltech and worked on this experiment that he wanted me to do, which was quite similar to the one that I wanted to do, except I wanted to use gamma rays and he wanted me to use cosmic rays. So there was really nothing else to do but stay on at Caltech.

LYLE: Did you talk to him anymore about the experiment that you wanted to do with gamma rays? Why was he so against it?

ANDERSON: Well, he wanted me to stay on at Caltech, and he knew that I had used and was familiar with cloud-chamber techniques. As a graduate student, I was measuring mostly the space distribution of X-ray photoelectrons, but to some extent the energy distribution. And it was generally believed at that time that the primary cosmic rays from space were like gamma rays, were photons. There was no proof of that; but Millikan had a theory of the creation of cosmic rays, namely the atom-building hypothesis—I won't try to go into it in detail, but that atoms were being built in free space. A bunch of electrons and protons would, in some very mysterious way, arrange themselves in a certain pattern and then coalesce into an atom, and that would give off a calculable amount of energy, presumably in the form of gamma rays. That was his theory of the origin of cosmic rays. I didn't believe the theory, and I think most people did not believe it. But what he wanted me to do was to measure the energy of the gamma rays that were the cosmic rays. Millikan had measured the penetrating power of cosmic rays, and they were much more penetrating than any other radiation known. The most penetrating radiation known to physics at that time was gamma rays. And his hypothesis was that cosmic rays were gamma-ray-like in character, but of much higher energy. And one can measure the energy of the gamma rays by measuring the energy of the electrons—the Compton electrons, in those days. So my job was to build an apparatus to measure the energy of the Compton electrons that were produced by the primary cosmic ray photons. So I started to build a piece of apparatus. Of course it took almost a year to build it, and in the very first experiments, it became clear that the picture was much more complicated than what was then thought to be the absorption mechanism of the primary cosmic rays—namely, by Compton electron collisions—because immediately, as many positive particles appeared as negative particles, which said something new was happening. The mere presence of the positively charged particles showed something different was going on than the Klein-Nishina absorption of gamma rays, which was the process by which gamma rays were absorbed, so far as anybody knew at that time; and I'm sure that was what was in Millikan's mind when he asked me to stay on and do this.

LYLE: How did you feel about staying? Did you just accept that?

ANDERSON: I accepted it willingly. It's what I really wanted to do in the first place, except I sort of talked myself out of doing it. No, I was very happy to stay. Everybody who gets his degree at Caltech is happy to stay on for another year, so far as I know. I've never known of anybody who wouldn't have liked to do that. No, it was great.

LYLE: Okay. Did you think that the experiment that you had designed originally would be better?

ANDERSON: No. I'm not saying it would have been better. I think it would have found the positive electron sooner than it was found, because in the experiment I was going to do, you knew what the incoming radiation was and you knew its energy. In working with cosmic rays, you didn't know what the radiation was that was coming in; in fact, you knew nothing. You didn't know the absorption, whatever they were, how they interacted with matter or what the particles were that you were observing in the cloud chamber, chiefly because the energies were so high that it was impossible with a cloud chamber to learn much more than the momentum of the particle and its electric charge. The cosmic ray particles, most of them, have energies, instead of 2.1 million, of hundreds of millions and higher—billions of electron volts. That's why, as you mentioned before, I was in the aeronautics building, because the magnetic field had to be as strong as one could possibly get. To deflect the cosmic ray particles to a measurable degree. So I designed the magnet to take the full power of the aeronautics departments's generator that provided electricity to run the wind tunnel. And that was, as I remember, a 400-kilowatt generator, which could be overloaded for periods like an hour or so at 600 kilowatts. So I designed the equipment to handle 600 kilowatts.

LYLE: How did you know how to do that? Where did you learn all the skills for building these things?

ANDERSON: To design a magnet is a very complicated thing. But I knew I had to have magnetic fields that were stronger than you can get by using a magnet of orthodox design because of the saturation effect of iron. So that what I built was essentially air-core coils, with iron where you could put it. That magnet was used by other people later on, and they thought it was very poorly designed, but they didn't know the purpose it was designed for. As an orthodox magnet, it would have been a very poor magnet. But we did get twenty-five thousand gauss over a volume with a diameter of six inches and several inches depth. It was water-cooled; we put forty gallons of water through it a minute; and the water came out, not quite but nearly, boiling hot. We were in the aeronautics building because that's where the generator was, and we were on the third floor because that's where the space was available. And the discharge water used to run out of the magnet into Throop Alley and would cross California Street and run down Arden Road. Under certain climatic conditions, it would give off an awful lot of steam, so there were clouds of steam half a block down Arden Road from this forty gallons a minute of almost boiling hot water that was needed to cool the magnet. Some of the neighbors objected to that.

LYLE: In von Kármán book, he mentions that when you found the positron, that you were very excited about it. Did this just happen one day or was this something that took a period of time?

ANDERSON: That's sort of a long and complicated story. Do you know of a paper I wrote called "Early Work on the Positron and Muon"?

LYLE: For the *Physics Teacher*?

ANDERSON: *The American Journal of Physics*. It's supposed to be a non-technical description.

LYLE: Yes, I have the paper.

ANDERSON: The first thing that came immediately out of the pictures was a set of high-energy particles of unit electric charge—roughly half positive and half negative. The conditions of the experiment were such that there was no way of knowing anything about the positive particles except that they were of unit positive charge and had a very high energy. One didn't know what their mass was, for example. But the only known particles of unit positive charge were protons. So the assumption was that, okay, atoms were being broken up by this very high energy radiation into the fundamental building blocks—protons and electrons. The only particles known at that time were the proton and the electron. You can, in a cloud chamber, in a magnetic field, make measurements of mass only on particles that are moving slowly. By slowly, I mean moving with a speed appreciably less than the speed of light. Now, these energies were so high that most of the particles were moving at 90 or 95 percent or more of the speed of light. Then all you could tell was the charge and the momentum; you measured the momentum from the magnetic field and the charge from the density of droplets along the cloud chamber track. Some of these particles, the positive ones, were moving slowly enough so they should have, if they had been protons, exhibited an increase in ionization, which they did not do. Another explanation—not a very good one—was that they were electrons going up. And I had discussions with Dr. Millikan about this and I said, "You wouldn't expect it, but they must be electrons that are going up," because the tracks weren't heavy enough to be interpreted as protons.

LYLE: These are just visual tracks that you see?

ANDERSON: Yes, and you could also count droplets under a microscope. But it was essentially a visual thing. You could just look at it and see that you had electrons.

LYLE: And you could tell that the mass was nowhere near big enough to be a proton?

ANDERSON: Well, they were still, in most cases, high enough energies so the effect wasn't a big effect. You had to worry about instrumental uncertainties and intensity of the light in the film. But you had the electron tracks right there for comparison. So Millikan said that that was ridiculous. They couldn't be moving up—any number of them, anyhow—and they were protons. So then I decided to put a plate of lead in the cloud chamber, which would tell whether they were moving up or down, and you'd expect them to move down, except very rarely one might happen to be scattered backwards—that should be a very rare occurrence. And then one day, a particle of low energy, so it was very clear that it was moving much slower than the speed of light, went through the lead plate. In fact, it *was* moving upward. It was a clear-cut case, and that's when it became clear to me that these positive things were mostly positive electrons and not particles as heavy as protons.

LYLE: So during this time did you also study your quantum mechanics?

ANDERSON: No, not seriously. I was too busy with and excited about our work, that I never did really learn as much about quantum mechanics as I wanted to do. I did in later years learn something about quantum mechanics, but it turned out that was not what I did, spending that one year at Caltech.

LYLE: You had this one experiment where it was clear that this was a positive electron. Did you attempt to repeat this? How difficult was it to repeat that?

ANDERSON: Well, that was by far the best photograph. But there were other things—cosmic ray particles, as was often observed, came in showers, groups of particles. Some of these were usually of lower energy than when they came singly like in most cases. And there it was more striking; if the positive particles were protons, they had low enough energy so they should have clearly given a heavier track than they did. So there was that kind of evidence before this one that I could call the clinching picture. We used to talk about positive electrons before that picture, but never really took it seriously.

LYLE: You said Millikan didn't think it was that. Were there other people who agreed with him?

ANDERSON: Millikan told me to publish. I think he felt there was enough evidence to publish. I was going to write a letter to the editor of the *Physical Review*, but he said, "Send t to *Science*, because you can get it in print quicker than in the *Physical Review*." So I sent it to *Science*. But it turns out, all physicists read the *Physical Review*, but many of them did not read *Science*. But it was met with disbelief on the whole. Ed McMillan, who was a good friend of mine—he was an undergraduates at Caltech in the class behind me; we were good friends as undergraduates, and I remember him telling me, "What sort of nonsense is this that you're writing about in the papers?" And I just read in Kevles's book, *The Physicists*, that Bohr didn't believe it and just passed it off offhand. I heard, too, that Joliot and Madame Curie's daughter—they were working not with cosmic rays but with a most wonderful experiment, namely shooting alpha particles at beryllium. It turns out that in that experiment a bright fellow could have, in one afternoon, had he been lucky and bright and a good experimenter, shot alpha particles at beryllium, and found that neutrons were produced, positive electrons were produced, and artificial radioactivity was produced. Actually, there was no such bright fellow and history did not proceed this way. Now, the history is that Joliot, I heard indirectly—he was doing that experiment with his wife, and they found electrons that seemed to come from the outside and strike the target. Now, they were really positive electrons. Anyhow, I don't know all the details of that, but he was very angry with me—I never met him—for publishing in *Science*, which he didn't read, instead of the *Physical Review*, because my paper might have helped him with his work. This is digressing a bit, but the neutron was discovered in that general experiment—shooting alpha particles at beryllium. Joliot and Irene Curie, the daughter of Madame Curie, did find tracks of high-energy protons, which they interpreted as Compton protons from gamma rays that were produced when alpha particles struck and interacted with beryllium. But if they were gamma rays, they would have to have about fifty million electron volts energy, and there's no way you can get fifty million electron volts in that kind of an experiment. Chadwick, the same year, 1932, discovered the neutron. I'm sure that he knew, before he did a single experiment, that it was neutrons that were producing the proton tracks that Joliot and Curie were observing; and since he was so sure they were neutrons, he did a whole series of experiments to prove that they were neutrons. I don't know how long it took him, but he collected apparatus from different people in Cambridge, where they had a variety of apparatus, and did some experiments to discover the neutron, and then wrote the paper. To me, his paper is a classic in physics, on how to discover a particle and how to write a paper about it. But I'm also sure that he knew that Curie and Joliot had neutrons before he ever did a single experiment to prove they were neutrons—that's a personal opinion.

All this was in '32. Bothe and Becker started off, what I think is one of the great experiments, bombarding beryllium with alpha particles.

LYLE: Why is that such a good experiment?

ANDERSON: Because it produced positive electrons; it produced neutrons; it produced what was then called artificial radioactivity. The artificial radioactivity part was discovered by Joliot and Irene Curie two years later. They took the alpha particles away, and found that their apparatus was still running—they were detecting particles. That could have just as easily been done two years earlier, if they'd thought of the idea of artificial radioactivity. The terminology isn't good; it isn't artificial, it's real. But it was called artificial—induced radioactivity is a better word.

LYLE: Was there a lot of excitement about these discoveries and what was going on in physics right then?

ANDERSON: It was an exciting year, yes. Another thing that happened in '32 was the discovery in England by Cockcroft and Walton, popularly called the first artificial smashing of atoms, the first breaking up of nuclei by solely laboratory means. Cosmic rays were doing it—they had enough energy—and gamma rays from radioactivity had been used. But they built a machine which would accelerate particles fast enough so they could disintegrate beryllium. They had an apparatus for speeding up protons to a high enough energy to break up the beryllium nucleus and give off these things. Some of the pieces were radioactive and gave off positive electrons. It was the first disintegration of an atomic nucleus using completely man-made apparatus, not using cosmic rays or gamma rays from radioactive substances.

LYLE: Were you aware here in Pasadena that they had done that experiment?

ANDERSON: I didn't know they were doing it. They published, and of course everybody then became aware of it. It was a very exciting year, '32. I don't think there's been another year like it since.

LYLE: How did you feel about being such a part of that? Did that make you feel really special?

ANDERSON: The feeling was good, yes. Now, Charlie Lauritsen in Kellogg Radiation Lab had a million-volt X-ray tube. He was accelerating electrons, which don't break up nuclei. And Cockcroft and Walton in England only had 600,000 volts. So that when the Walton paper came out, Dick Crane, who was working in Kellogg Radiation Lab, in about an afternoon built a little gadget to put on top of the X-ray tube, a little gadget that would produce hydrogen ions. And then they immediately confirmed the Cockcroft-Walton experiments, and that was the beginning of the Kellogg Radiation Lab. It's that sort of thing that they're still doing today, using not an X-ray tube but using more modern equipment. But I think that the first artificially produced neutrons, so observed and identified, were made in Kellogg Radiation Laboratory. You see, the neutrons that Chadwick found were produced in experiments using natural radioactive sources, not from a man-made accelerating device. Yes, it was a very exciting year.

CARL ANDERSON

SESSION 3

January 16, 1979

Begin Tape 3, Side 1

LYLE: Today I want to continue the discussion we had on the l930s at Caltech. Watson said that Millikan would come to faculty meetings and present a program and would give his reasons for doing something. And then he would say, "All right—thinking men must agree," and then he would leave. Watson's comment was that the faculty was very disturbed at their lack of being involved in the decision making. Did you notice that the faculty was disturbed about this?

ANDERSON: Well, I was young enough, so I did not attend the faculty meetings. What date are we talking about?

LYLE: Well, it's in the middle thirties, I don't know the exact time; I could look that up.

ANDERSON: I would guess the pay cut.

LYLE: The cut was earlier.

ANDERSON: Earlier. Well, I did not attend the faculty meetings at the time that the cut was made. For example, I didn't have my salary cut. It was so small that if you had cut it, there wouldn't have been much left. I probably was a research fellow at that time. And a research fellow, I think, is entitled to attend faculty meetings, but I have no memory of attending a faculty meeting at which Millikan presided. Maybe I didn't go to faculty meetings regularly—I may have been too busy. There was a chairman of the faculty who presided at the faculty meetings. I'm talking now about the time I started to attend faculty meetings. But I have no memory at the moment of hearing Millikan give a report at a faculty meeting. It may be that I was there and forgot it, or it wasn't very important.

LYLE: In general, then, when you did go to faculty meetings, you had the feeling that the faculty had some say about what happened?

ANDERSON: My feeling was that Millikan ran the Institute—not only the physics and mathematics division, of which he was chairman. No, mathematics came later, it was not part of the division. Anyhow, he was chairman of the division which contained physics. But my feeling is that essentially all faculty members in all divisions, if they needed funds for research went to Millikan, not to the division chairman necessarily but to Millikan, and explained their woes and said they needed a little bit of money to do some research. The main thing in those days, I think, was that the research that was done did not need the large sums of money that present-day research does; it was more of an individual effort and people were accustomed to doing with very little money. I remember we used to make regular trips to the Southern California Edison Company junkyard in Alhambra. We knew all the people and they knew us, and we would buy for a song or often they'd give us the transformer or something, a switch, or something that we needed for our research. I guess I was by nature economical, because I remember Watson telling me once one day, "Well, Anderson never asks me for anything." So I thought, "Well, gee, I better get off the dime and ask him for something." But as I remember, Watson had very little to do with the research program at Caltech. That was done by Millikan. Now, I don't know what it was like to be a professor in another division, like chemistry, say. The physics people went to Millikan for funds or help of any kind. They were supposed to because he was chairman of the physics group.

LYLE: And you didn't pick up any feeling that people wanted to have more independence from Millikan?

ANDERSON: No, I don't think I did. My contacts were mostly with the physics people, and everybody then, as today, was short of money. I mean, nobody, no matter what he's doing, ever has enough money to do it the way he thinks it should be done. I remember going to see Frank Capra, who was at the height of his career at that time as a director, and whom I happen to be acquainted with, at Columbia Studios, to get a motor-generator set, and actually did get one that was mounted on a 1911 Mack truck that was used by the movies to run their klieg lights at various locations. It must have been parked n the desert for many years, because it had beautiful purpled headlight lenses—they were acetylene lights in those days. So the truck was towed to Caltech and we used that as a motor-generator set to provide power for our magnets. So this is an example of the fact that research was financed in an entirely different way in those days than it is today.

For example, we spent the summer of 1935 on the summit of Pikes Peak. We bought a used 1932 or so Chevy truck for three or four hundred dollars; we bought a flatbed used trailer out in Vernon at a used trailer lot, and actually built the body of the trailer. A classmate of mine was then, I think, president or vice-president or something of Bekins Moving and Storage Company. So I went to him and said we needed a housing for our trailer. So he gave us a whole bunch of great big packing cases that they used in their moving operation. I don't remember the details—they were great big boxes. So we used those and actually, with our own hands and a hammer and a saw, built the trailer housing that later went to Pikes Peak. And our apparatus was protected from the snow and the rain and the elements by these packing cases. But that's another example—you don't just order, you build what you need. Nowadays, the scientists and the military people know one another extremely well. They got acquainted extremely well during World War II, and if you needed a used trailer, you just went to somebody in the Army or the Navy. Of course, you had a contract, too, usually with the Navy, at Caltech. They would just turn over to you a great big beautiful, very expensive trailer. In fact, we did that in later years.

But during the Millikan years, it was certainly an austerity program, as far as research was concerned. I can't say that I felt deprived of money, because we operated in a very economical way. This I don't know, but we were working in a field that Millikan himself was personally intensely interested in, and we may have gotten a break or two that way. I just don't know. But there was no such thing as a budget. If you needed something, you went to Millikan and he would either say yes or no.

LYLE: You just mentioned the Pikes Peak story. I'd like to ask you a few questions about that, and then I would like to come back to the Caltech campus.

ANDERSON: I'd be happy to talk about the Pikes Peak thing. It was a very good thing to do scientifically. It was a great success, because the cosmic rays are more intense at higher altitudes than lower altitudes, and intensity was one of our major problems. Also, cosmic rays have different components, and the components that do the most interesting things increase very rapidly with altitude. Well, anyhow, we decided to go to Pikes Peak for the summer of '35. And as I just said, we bought this used Chevrolet truck and the used trailer and built, ourselves, the body part of the trailer and built the hitch to hook the truck to the trailer, which was not properly designed and later caused us some embarrassment and trouble. It was a one-and-a-half ton used Chevy truck. Our total load, counting the trailer, was over five tons. I remember in a test run, when it was loaded, just shortly before we were to take off, we ran from California Street up Lake Avenue to Colorado. Now normally you don't think of going up Lake from California to Colorado as much of a hill, but it was a good stiff second-gear operation for our truck. It would just make that grade in second gear. We bought a truck that had an extra low, low gear, because we anticipated having to overload greatly the capacity of the truck.

LYLE: So you decided to go anyway?

ANDERSON: Oh, we had to go, sure.

LYLE: Did a lot of people at Caltech know you were going?

ANDERSON: Well, that I don't know. I guess not. We had planned to leave on a certain day and the truck was parked with its trailer out behind Guggenheim. And the first step was to drive it from that spot to along the curb at the south door of West Bridge, to load on some other stuff. It was eleven o'clock at night, but it was on the day that we were scheduled to leave. So we did that, parked the truck along the curbing by West Bridge and loaded it up. Then I happened to notice that about a foot in front of the trailer, which was a high thing, was a three-inch-diameter branch of a tree, and we were lucky that we didn't hit that. That would have severely damaged the trailer. So we went in and got a saw and sawed off that three-inch branch. It turned out we couldn't back up that trailer very well at all, because of the improperly designed trailer hitch that we made. Well, anyhow, we sawed off that fairly big limb and left it on the parkway and took off and made Hope, Arizona the next day some time. We drove all night, mostly in first and second gear. We had to go over the San Gorgonio Pass, for example, which was a low and extra-low operation. But we did get to Colorado Springs at the foot of Pikes Peak. There was one humorous incident. We stopped about five in the evening at Las Vegas, New Mexico [not Nevada]. It was a little town, and we were to spend the night there. I was driving at that time. Seth Neddermeyer was working with me, and the two of us were on this trip. By that time we'd learned that a truck and a trailer outfit is not like a car. You worry about trees and other things that you can easily run into because the trailer is so high, and we also learned not to park near the curbing. So I parked maybe three or four or five feet, I don't know, from the curb. But we did hit a bakery sign. There was a bakery there and we hit the sign. This sign projected beyond the curbing, and we did hit it and it tore some tar paper that covered the top of the trailer that we had put on. Seth was anxious—it really didn't damage the sign much, but it broke one guy wire that was important to keep the sign from falling. So Seth wanted to go in and apologize to the baker about his sign. We tried, but the bakery was closed. There were living quarters behind the bakery, and there was no one there. There was a garage across the street, and we got a fellow to come over with a ladder. He climbed up and uncoupled us from the sign and fixed that little guy wire on the sign, so there was no damage done to the sign. We spent the night there, and the next morning, Seth Neddermeyer wanted to apologize to the baker. It was early in the morning, maybe five o'clock or something like that—because our cruising speed was very low, so we had to drive long hours. I was driving again, and we drove to the bakery, and believe it or not, I got tangled up in that sign in exactly the same way as the night before. The baker wasn't home. The bakery was closed, and he wasn't in his living quarters behind the bakery. So we went across the street and got the same garage man and he brought the same ladder and untangled us. By then, Seth said, "Let's forget apologizing to the baker." So we took off. We did have other similar experiences along the route.

LYLE: Were you planning to be towed up the mountain, then? What were your plans?

ANDERSON: First we stopped in Colorado Springs—this is an interesting story—at the Chevrolet agency in Colorado Springs to have the valves ground and change the oil and to have a new clutch put in, because that clutch was being over-used. So that was done at the Chevrolet agency, and then we started out for the mountain itself. We knew very well that we had o chance whatever of getting up the peak under our own power, but we went as far as we could, and then got stuck and managed to get stuck in the middle of the road.

LYLE: This is the road up to the top of the peak?

ANDERSON: It was a private toll road up to the top of the peak, yes. We even reached the lower toll gate at the time we got stuck. So we were blocking traffic. The Pikes Peak Company had quite a bit of equipment to keep the road in repair and keep the snow under control and so on, although this was summertime when we went up. Well, they came down with a great big white motor-company truck and tied that on the front end of our truck.

LYLE: They were willing to do that?

ANDERSON: They were willing to get the road open, and also willing to do that. They towed us—both trucks were working as hard as they could—and with both trucks we did get up to the summit.

LYLE: Were they expecting you?

ANDERSON: They were expecting us, yes, but I don't know if they were expecting us to have a truck that would get up the hill itself or not. Oh, yes, we had made plans.

LYLE: So was there a building up at the top that you could work in? What did you do when you got to the top?

ANDERSON: Well, there were some stone buildings. No, we were self-contained. We knew we had to be, because there was no laboratory up there. We borrowed from Jesse DuMond some big tanks—we needed a lot of water to cool the magnet—and we rented a Cadillac engine and a generator that was to supply the power to run the magnet. We parked right next to a tank that was up there. It was a ten-thousand-gallon water tank, so we got about a thousand gallons or so to fill all these tanks that we had borrowed from DuMond and then recirculated our water. Of course, some evaporated, and then we'd just get a little more from this ten-thousand-gallon tank.

This was a continuation of what I had been doing until Seth joined me as a graduate student. It was really a continuation, trying to understand the complicated effects that we were finding in cosmic ray experiments. The idea was to get a much stronger intensity of cosmic rays, and to get as high an altitude as we could.

LYLE: So you had already been doing the work and then you went up there?

ANDERSON: Yes, but we moved the apparatus from the third floor of the aeronautics building into the trailer to get it up to Pikes Peak. Well, after assembling the things and lifting these tanks that we borrowed from DuMond and hooking things up and getting them squared away, we started up the Cadillac engine, and because of the high altitude it had nowhere near enough power to run our equipment. So we uncoupled the trailer and used the truck, which had on it at the time only the Cadillac engine and the generator, and drove down the mountain to the Cadillac agency to get special high-altitude jets to modify the Cadillac engine to work at 14,000 feet instead of sea level. And that cured the difficulty. But we drove up, and as we approached the summit, maybe a mile or a mile and a half away from the summit, suddenly there was a lot of smoke that came out of the Chevrolet engine, and we bogged down and were stuck. It was water vapor and oil smoke. Of course, we didn't quite know what to do, but just by chance, a very bright yellow-painted, brand new Chevrolet truck came down the mountain—it had been up to the summit—not more than five or ten minutes after we broke down. We had lifted the hood, and discovered that when the truck was worked on by the Chevy people in Colorado Springs, they had not properly replaced the valve cover on the engine. It wasn't seated against the gasket, and you could see the oil had been running out, and what happened was we ran out of oil. So then we flagged this bright yellow truck that was coming down the mountain. They stopped, and I immediately recognized that the driver was the service manager that we had dealt with in Colorado Springs. I showed him the engine and I said, "Look, your people didn't put this valve cover on properly and all the oil ran out and damaged our engine." He said, "Yes, I can see that's so, but we can't guarantee our work when the truck is used under conditions like this, highly overloaded and at 14,000 feet," and so on. There was a chap who was driving with him, who was quietly listening to our discussion and looking at our truck. He took me aside and he said, "I think you boys have a case. If they don't fix this up properly, let me know and I'll see that you get it taken care of." He gave me his card. I forget his name, but he was vice-president of the

Chevrolet Truck division of General Motors. The reason he was up there with this bright yellow truck—they were making plans for a speed run for advertising purposes to break the record up the mountain. Apparently there was some kind of a record that they were going to break. So that settled that. The next morning, the Chevy people came up and towed us to the top of the mountain and started work on the motor. All the pistons were scored, and the crankshaft was scored, so they had to take the engine out and bring it down to Colorado Springs and give it a complete overhaul with a new crankshaft and new pistons and so on, which they did, and put it back in place at no charge to us. But we bought, for fifty dollars, a little Chevrolet roadster that was owned by a chap who was an automobile fan. He had adjusted it so that the carburetor and the timing and so on were just right for that altitude. We used that instead of the truck, for our general transportation. And we needed it, because there were some stone huts on the summit, and they wanted to charge us two dollars and a half a night to sleep in those stone huts, with no furniture, no accommodations of any kind. But it wasn't practical for us, and we didn't have that kind of money anyway. But we knew the road gang, and they invited us to stay in their bunkhouse halfway up the mountain, at Glen Cove. So we stayed there with them free of charge. We even got meals for what they paid—very inexpensively. Then we took the Cadillac engine down, again, to the Cadillac place a few days later because it was still not doing its stuff. We drove down to Colorado Springs, and then started up the mountain. I was driving up the mountain in low gear, and felt with my foot that there was no brake pedal. Then I reached down with my hand, and it had fallen flat against the floorboard. So I pulled it up and it fell flat again. The brake pedal had become disconnected from the rest of the truck, because the Chevrolet people had forgotten to put a cotter key in—they were mechanical brakes, not hydraulic. That pin had stayed in all the way down the mountain, but had fallen out as we were near the bottom on the way up the mountain. So we had no brakes. The hand brake, as is often the case, was useless; it wouldn't hold anything. If we stopped, we didn't know how we could keep the truck from rolling backwards, so we decided just to keep going and hope that we wouldn't have to stop and did that, and went up to the summit where there was a level place where we could park it even though we didn't have any brakes. We again called the service manager and told him again of the carelessness of his people, and he sent a car up and put in a new pin and cotter key. Later on, the GM vice-president rang us up and asked us if we wouldn't meet him for dinner at the Broadmoor Hotel in Colorado Springs—this chap that we'd met on the mountain. We said sure.

So we had a fine dinner at the Broadmoor Hotel. His proposition was—we were using a lot of gasoline because we were running that Cadillac engine wide open about eight or ten hours a day. He wanted, on this record-breaking run, to haul gasoline up and make sort of a story out of it, that we suddenly ran shy of gasoline and needed some in a hurry. I knew this was not a good idea because it's not Caltech's policy to get involved with commercial advertising and so on.

LYLE: Were you tempted to do it, though?

ANDERSON: No, we were getting our gasoline—the Union Oil Company gave us the gasoline free, and they even hauled it up. In those days, there was a cog railroad, a little narrow gauge railroad train which ran up to the summit. So we had daily delivery of barrels of gasoline. So I said I answer that question unless I talked to Dr. Millikan, because it involved national publicity and the policy of the Institute and so on. Besides, it was a long distance call. So he said, "Well, call him up." So I did. Dr. Millikan and I talked cosmic rays for a long time at the expense of General Motors Company. And then at the end I brought up this business and he said, "Well, it doesn't look like too good an idea, but I'll let you use your own judgment on that." So we decided not to do it.

LYLE: You mentioned that the Pikes Peak experiment was a good experiment. Why did you think it would be a good experiment?

ANDERSON: There were problems in interpretation of our data. You see, the problems leading up to the positive electron were resolved in '32. So there were positive electrons. But then there were other particles that didn't behave like electrons, positive or negative, or like protons, the known fundamental particles. They had peculiar properties, and that's why I was so interested in that letter that I wrote to Dr. Millikan from the summit, in which I said I thought we had strong evidence for the existence of new particles, intermediate in mass between electrons and protons—particles that were unknown. But there were paradoxes in our data, and this was one way in which we could resolve the paradox, although it was a very radical assumption to have to make. And we got more cases of tracks of that kind up on Pikes Peak than we had here—oh, a hundred times as many as we'd gotten previously in Pasadena. So that was an extremely interesting situation; but we did not feel that we had enough evidence to publish, by any means, at that time, but there was enough to write a letter to Dr. Millikan about.

LYLE: So then you went back to Pasadena and continued that and found enough evidence?

ANDERSON: Yes, in September when the snows threatened, we went back to Pasadena.

LYLE: When you were looking for the meson, had you checked it out theoretically? Had you talked about it with Oppenheimer, for example?

ANDERSON: It was predicted—I guess you could say predicted—by Yukawa. He wrote a paper that was published in a Japanese journal, written in Japanese, saying that maybe there were such particles to explain nuclear forces.

LYLE: Were you aware of this?

ANDERSON: No. I couldn't read Japanese; I didn't know there was such a publication. We used to talk to Oppenheimer more about the positive electron than we did about the early data about the meson. Because the positive electron was predicted by Dirac, and Oppie was expert on the Dirac theory. The real explanation of how the positive electrons came to exist was given in a paper by Blackett and Occhialini in March of 1933. It was published in the *Proceedings of the Royal Society*—namely, pair formation by gamma rays. It is surprising to me that Oppenheimer, during the six months after I first published the paper on the positron—I had no idea, even though I'd searched my mind and gone nuts trying to figure out how these things came to be—it's very surprising to me that Oppie didn't think of that idea. It's the sort of thing you would have expected him to think of. But it was first announced in this publication by Blackett and. Occhialini, who were doing similar experiments with a cloud chamber. They verified the existence of positive electrons and in the same paper came out with the pair production hypothesis, and I can remember saying to myself, "Of course, that's the explanation," as I read their paper. Now, Dirac was at their laboratory at the time they were doing this. So they must have discussed it with Dirac. It may have been his idea; I would guess so, I don't know. They didn't say in their paper.

Begin Tape 3, Side 2

LYLE: After this paper was published, did you talk to Oppenheimer then about theory?

ANDERSON: Well, I talked to Oppenheimer quite a bit. He used to spend something like a third of his time at Caltech and two-thirds of his time at Berkeley. I remember reading about pair production after puzzling my own head for months trying to think of some sensible way to explain how the positive electrons came to be there, without success. I had had some discussions with Oppenheimer on this. I found it hard to talk with Oppenheimer because his answers were usually, at least to me, encased in some sort of a mysticism. I couldn't understand what he was saying, but the idea of pair production, if he had said that, I would have understood. Now, in that respect, Oppenheimer was entirely different than Feynman. You can talk to Feynman and his answers are precise and of the type that an experimental physicist can understand, or at least thinks he understands. But with Oppenheimer I couldn't understand his answers. But I would have understood, had he said pair creation, which he didn't.

Now, I think your question was about the meson. I think I want to say, maybe at this point, that the history of the discovery of the meson is confused in the literature. I think that the credit for the discovery of the meson belongs to Neddermeyer and me, even though Street and Stevenson are at times said to be the discoverers. I don't want to get into this thing; it's highly technical, and it's really not controversial. I think the evidence is clear-cut, to anyone who wants to read the papers. But I do want to say that Bruno Rossi wrote a book called *Cosmic Rays* which discusses this point, and discusses the evidence that Neddermeyer and I published and also that Street and. Stevenson published. I could go into the arguments, but they'd be involved and long-winded and technical. I will dismiss it by just saying that I think the truth is very well spelled out on a few pages of this book of Bruno Rossi, published several years ago. The naming of that particle is extremely interesting.

LYLE: Well, actually I was confused whether the meson was changed to the muon.

ANDERSON: That's a long story, but let me just say one thing. Seth and I discussed the name of it. It was called the Yukon for Yukawa; it was called the X-particle; it was called a heavy electron, which turns out was not a bad name, because that's what it really is; it was called a baryon, and so on. So Seth and I—Millikan was away—wrote a little note, one paragraph, to *Nature* suggesting that the name of it be mesoton—"meso" meaning intermediate, like mezzanine in a building. We sent a letter to the editor of *Nature,* which is a British publication. When Millikan came back, I told him about this and showed him the letter, and he hit the ceiling. He said, "That's not a good word. It should be mesotron. There should be an "r" in there." And he said, "Look, there's electron, there's a neutron. And I said, "There's proton." Well, the end and issue of it was that Seth and I cabled that "r" to *Nature*, and it came out mesotron, a word which I didn't like—nobody liked it. I think if that "r" had not been in there, the word would have been accepted—mesoton. But people objected to mesotron, because "tron" like audiotron or cyclotron or synchrotron, are instruments. Well, anyhow, nobody liked the word mesotron, including Seth and me, and it later became meson. It did not have any of the properties that it should have, according to Yukawa's prediction. His particle was to explain nuclear forces. The meson, in cosmic rays, ignored nuclear forces, so it was clear even in the very early days that they had similar masses and other properties, but the meson we found, which is now called the mu meson, or muon, could not be the Yukawa particle because the experimental particle ignored completely nuclear forces. The predicted particle was predicted to interact strongly with nuclei, to explain the intense strong nuclear forces. That was not resolved until after the war by this fellow in England who used photographic emulsion—Powell. He found the pi meson. And he used, I guess for his own bookkeeping, Greek letters—pi and mu—and the pi meson disintegrated spontaneously into a mu meson and a neutrino. The pi meson *is* the Yukawa particle, because it has a strong interaction with nuclei, except now the situation is much more complicated because of all the strange particles that have since been discovered.

[Tape recorder turned off]

LYLE: Last week you mentioned that you had worked in von Kármán's lab because the equipment was there. I read in Watson's papers that one of the people in von Kármán's group, Tsien, was a member in the 1930s of a musical organization that apparently talked a lot about politics.

ANDERSON: You're talking about the aerodynamicist Tsien?

LYLE: Yes. And in the 1950s he got in a lot of trouble with the FBI and eventually left.

ANDERSON: Yes. Horace Gilbert was chairman of a committee on foreign students. He went away, and I acted for him one year as chairman of that committee and had some contact with Tsien.

LYLE: Now which year is this you're talking about?

ANDERSON: The early fifties, I guess. I don't know what year. But I understand that Tsien wanted to go back to China and was down at the dock to get on the boat. They found—this is secondhand information—they found some logarithm tables among his papers that looked mysterious. Anyhow, he was prevented from getting on the boat to go to China.

LYLE: Did you know him in the 1930s?

ANDERSON: No, I didn't know him. I knew of him as an aerodynamicist. I understand there were many Caltech people in the l930s who attended groups that presumably were Communist groups, including Oppenheimer and Tsien.

LYLE: Was it a common thing, though? Did people know about these groups or did they think that this was a bad thing?

ANDERSON: Well, I'm a poor person to ask because I don't know about them. I'd heard about them, but I wasn't interested in them and didn't get education at all about what was going on. My feeling is that they were not secret groups; that people were not trying to keep the fact at the time secret, that they did attend these groups. In those days, Communism was thought of by most people in a way that was entirely different than it was thought of in the fifties.

I could tell about a graduate student of mine who was charged with being a Communist while he was working with me and while we had a B-29 and while he was flying in that B-29. I certainly won't name the student. I just don't know. I did ask him the question and told him that I didn't want him to tell me anything that he wouldn't say publicly. I asked him the question, under those conditions, whether he was a Communist. He said, "I'll skip that one." The Navy refused to let him fly in the B-29s; they refused to let him go to Inyokern or China Lake, or to have anything to do with Navy Property; and they asked me to take him off cosmic rays as a thesis topic. I said, "Of course, you can refuse his flying in a government-owned B-29 or going on a naval base and so on, but the thesis topic that he's working on is really Caltech's business and my business, and it's unclassified information, cosmic rays." I said that I would not take him off that thesis topic, and that he would work with analyzing at Caltech data obtained from photographs made by that B-29. He would get the photographs and analyze them. Then one of the people in this meeting said, "Suppose that information should get to the Russians?" Well, I said I hoped it would, because we intended to publish that information and send reprints to the Russians. Later, I checked with DuBridge about what I had done, and he said that I did exactly right. Of course, we couldn't expect the Navy to permit him to go on a Navy installation; but on the other hand, we didn't ask people, when they applied for graduate work at Caltech, what their political beliefs were, and it was perfectly right that he continue with his thesis work, even though the data were collected in Navy-owned equipment.

CARL ANDERSON

SESSION 4

January 18, 1979

Begin Tape 4, Side 1

LYLE: I just wonder if we have overlooked something so far, or if there's something you'd like to mention.

ANDERSON: Well, I just happened to think of one thing that may be of interest that will fit in somewhere. Dr. Millikan was my supervisor when I was a graduate student. I had a graduate student, Don Glaser, who won a Nobel Prize. Millikan did, and I did, and so did the graduate student. So there were three generations. I don't know how often that happens. I was talking to Don Glaser one day—he's at Berkeley—and pointed this out to him and said that he ought to keep up the good work and see that he had a student that does some good. So he said, "Wait a second and I'll get my roll book." And he looked over the list of students, and he said, "Well, I don't think it will happen this year." I thought that was sort of interesting. It would be nice to have a fourth generation.

LYLE: Yes, all of his students will have to get to work. I wondered how the Nobel Prize affected your life. I know that finances had always been a problem, that there'd never been enough money. Did that help you financially?

ANDERSON: Of course. I happened to get it when I was young—I was an assistant professor, I think—and Caltech's salary scale is now very much better relative to other universities than it was at that time. Caltech became, you might say, competitive in salaries shortly after the war. But during the Millikan days, it was not I, of course, didn't have much money, and I had a mother to support, who was not well and had to make several trips to the hospital. The insurance that I had didn't cover dependents of that kind—it covered children, but not parents. So it was a great help to me financially, even though the dollar value of the prize itself in those days was much smaller than it is now. But then so were prices. I got my Ph.D. just about in the depths of the depression, 1930, just before the depression got really bad. I was a research fellow and had a small salary, but the depression in the thirties was not like modern depressions, in that it was not a high-priced depression. Businesses failed and prices dropped. So that I had my salary, which was small, but at least it didn't change. So the depression to me, financially, was useful. I could buy a pair of shoes for half the price of what they were before the depression. But the Nobel Prize money, of course, was of great help. Incidentally, I didn't have enough money to get to Stockholm. So Millikan loaned me $500 for a one-way ticket, which I paid back when I came back from Stockholm.

LYLE: Was Dr. Millikan aware that you needed more money, that your mother was ill, and that that was a problem?

ANDERSON: I would guess so, although I can't ever remember going to Millikan and saying I was having a hard time financially. I probably did, but I have no memory.

LYLE: Did you ever consider leaving Caltech and going to a place where you would make more money?

ANDERSON: No. My mother was a genius apparently, now that I look back on those days, at managing money. I mean, we had a decent house to live in; we had plenty of food; we had a car. And the depression helped. I forget what my salary was in those days. Even today, I think most young research fellows and assistant professors are hard up—who isn't?

LYLE: Today, do all of the assistant professors make the same salary? Is there a scale?

ANDERSON: There is a scale, although the salaries at Caltech are not based primarily on seniority. There can, for example, be associate professors whose salary is higher than some full professors. There is, as I guess there is in most of the really good universities, a wide variation in salaries, even within a given rank—as there should be, because some people are not worth 10 percent more than others, they're worth a thousand times as much as others are in research capabilities, for example. In its organization, Caltech has a simpler administrative setup than any other institution that I happen to know of. There are no deans; there are no department heads either, officially, at Caltech. There are division chairmen and the provost and the president. And for many years, there was no provost.

LYLE: Doesn't the chairman, though, act as a dean?

ANDERSON: The division chairman acts in part as a dean and in part as a department head. The question he has to ask himself is, "Am I a member of the administration, or am I a member of the faculty?" I always decided that I represented the faculty and not the administration.

LYLE: So what implications did that have for you? Can you give me an example of how that was expressed?

ANDERSON: I don't know that in practice there was very much difference, but we had division chairmen's meetings at which the president, of course, was present, and the provost and the vice-president for financial affairs. I made it clear at those meetings that I was to be considered a member of the faculty and not a member of the administration. I don't think in practice it made any difference. I have not been associated closely with any other institution except Caltech, but from what I've heard, there is in colleges, big or little, a lot of politics and back-stabbing or whatever you want to call it. I have found that Caltech has been in that sense a very friendly place, where people have cooperated, and give-and-take has been an important part of the relationship between the faculty and the administration. Now, since I retired, times are different for Caltech; money is harder to come by. Maybe it's different now; I don't know. But I've always thought of Caltech as a friendly place and have thought of that as one of its greatest attributes.

LYLE: Another thing I was wondering about the effect of the Nobel Prize is how it affected your life socially. That is, did you find that all of a sudden that you were on a social circuit that you had not been on before?

ANDERSON: Not really. My friends were, as I recall, the same ones. One thing that happens is you get requests to make speeches here and there at various universities and places like city service clubs and churches and so on. I didn't like to give speeches, so I just turned down most of those. There wasn't any great sudden difference in my life socially—somewhat financially, yes. I think I bought a new car.

LYLE: You were just saying you didn't like to give speeches. That brings up another subject. How did you like teaching?

ANDERSON: I enjoyed teaching, probably for the reason that the teaching load at Caltech, at least in the science departments—probably not in the humanities division—is very light. The normal teaching load is three hours a week. Sometimes a person, in addition, takes a laboratory if there is one connected with the course, or often a teaching assistant will do that part. I enjoyed teaching, and probably one of the chief reasons is, there was so little of it to do. If you do something three hours a week, you can enjoy it. If you have to do it five hours a day, I think your attitude toward it could be different.

LYLE: You mentioned that when you were a freshman Noyes in a sense noticed which students were the better students and tried to include them in some activities at Caltech. Did you then try to do that?

ANDERSON: No.

LYLE: Did you think about it at all?

ANDERSON: Yes, I did. I probably should have done more of that sort of thing than I did. For example, like inviting freshmen over. I did have my own graduate students over quite often to the house; we'd barbecue something. Later on, when we moved into this house where there was a swimming pool, we'd have people over—mostly the graduate students and then several of my friends on the faculty. But I did not do as Millikan did, though he was the chief administrative officer. I'm thinking now of Millikan's freshman teas. I don't know of anyone else, though there may have been other people, who was like Noyes. He was a bachelor; these were not social affairs. He just on weekends would take usually two students and go on a camping trip. I was a freshman and sophomore; it was great, and I was very surprised that he would take the time and have the interest to do that. But I think that was Noyes; that was not really customary.

LYLE: Do you also think it had to do with the times; that is, the 1920s were different than in 1930, and people did things differently?

ANDERSON: I guess so. That's a terrifically big subject. My own feeling is that in the laboratories, the lights were on at midnight much more often in the thirties—let's say in Bridge Lab or the biology labs—than they are today. These lights were graduate students working at midnight, and one difference is, of course, a much higher percentage of graduate students are now married and can't very well leave the family every evening. As a graduate student—I was a bachelor in those days—and also during my first several years on the faculty, counting research fellow as a faculty member, I worked long hours, and it was not unusual to work until midnight and all day Saturday. I wasn't alone. I mean, the place was full; other graduate students were doing the same thing. I have no statistics, but I have a feeling that in those days the graduate students and the young faculty members spent more time doing their research than they do now.

This isn't to say that there was a complete lack of social life at Caltech. The undergraduate students had regular dances, which I attended and enjoyed. Also, after the Athenaeum was built, the graduate students had regular dances and other social affairs that were not connected with the faculty. It was graduate students and their friends. I'm not aware that they are doing that, or have done it in recent years. The graduate students don't make use of the Athenaeum to the extent that they did in those days. Of course, now there are so many more members of the faculty that the Athenaeum is more crowded.

LYLE: Did the Athenaeum seem to belong more to the students at that time than it does now?

ANDERSON: One thing that happened was during the depression—and as I've said it was not a high-priced depression like we have had recently—money was scarce and prices were low; goods were plentiful. It was during that period that Millikan—I guess it was Millikan—got the idea of giving fellowships and assistantships in the form of board and room at the Athenaeum rather than cash. So the Athenaeum was filled with graduate students living there. They had what they called the loggia. It was a great big open porch on the second floor that was just filled with cots; it probably housed, I don't know, thirty or forty graduate students. Then they were given their meals at the Athenaeum. There was a lot of griping; they would have much rather have had money, so they could eat more cheaply and have some money to spend rather than just board and room provided.

LYLE: When did they stop that, do you know?

ANDERSON: I suppose as the depression ended, and money became more available. I lived at home; I lived at my mother's, so I didn't experience this. But I would eat quite regularly at the Athenaeum as a graduate student.

LYLE: Rather than go home?

ANDERSON: Yes, I also went home, but it was cheaper. The Athenaeum was used more by graduate students in those days. I have a feeling that nowadays, many of the students think, well, this is the faculty club, and we're really not welcome here. And besides, it costs a lot more to eat at the Athenaeum than it does down in some little restaurant down on California Street.

LYLE: Were there any favorite restaurants off campus that you remember?

ANDERSON: The greasy spoon was there. Now, it depends on what year we're talking about. Because the Athenaeum, I think, was completed about 1930 or '31, which is when I just happened to get my Ph.D. degree. So I couldn't have been there as a graduate student, because it wasn't there. There was a greasy spoon on the campus, where many people—faculty, undergraduates, graduate students—ate. There were also restaurants around the neighborhood.

LYLE: Where did you tend to, for example, eat lunch?

ANDERSON: Many graduate students would bring their lunch. I guess those that were bachelors would—I don't know. My mother would fix me some sandwiches and things, and I guess that's how I usually ate lunch, and then at the greasy spoon. In fact, there was no other place on campus to eat lunch. There was a drugstore on the corner of Lake and. California, where many of the students and faculty would go and get a sandwich. This is all very vague, what I am saying.

LYLE: The other question I had a bout the Nobel Prize was how it affected your work. Did it change the nature of your work at all?

ANDERSON: I would say it had absolutely no effect whatever on my work. At the time I happened to get it, Neddermeyer and. I were deeply engrossed in following the clues and trying to resolve the paradoxes that were present in the cosmic rays and in the data we were getting. I didn't work any harder after I got it, and. I didn't work any less hard. We were both extremely interested in what we were doing. We thought it was important, and these paradoxes led to the discovery of the first meson, and that's a long story in itself, but I won't try to go into that. I did mention, I think, last time that it's pretty well written up in this book by Bruno Rossi.

The positron came completely accidentally. I, I guess I should say in those days, was not looking for one, not even thinking about it until the data came. But the meson work was different. There were paradoxes, and one could not explain the effects in terms of known particles. It was as early as 1934, in a paper that Neddermeyer and I wrote to the *Proceedings of the Royal Society* [*of London*], that we outlined some of these paradoxes and we pointed out that there was some unknown feature that was not recognized, that was needed to resolve these paradoxes. We knew that there was something there, but we didn't know what it was, so in that sense the discovery of the meson was entirely different. It was not an accidental thing that happened in a short period of time. It was a long intense devoted effort to solve certain puzzles or paradoxes.

LYLE: Did you become absorbed or fascinated by the puzzles?

ANDERSON: Yes, yes. And it was not unusual for us to work until about midnight and then take a walk, a pretty long walk, up to Colorado Street and down to Orange Grove, for example, and stop and have a glass of beer on the way. Or we might head east and walk out to San Gabriel Boulevard or something like that, arguing, chewing the rag—discussing the work and what we should be doing next, and so on. So in those days, we did put in long hours, not to the exclusion of a social life entirely, of course. But it was extremely interesting and intriguing, and it was the path of least resistance, I guess, to do this. It was a lot of fun.

LYLE: As a professor, do you think you have something particular that you taught your students that you feel was the most important thing? Is there some way at looking at things or some idea or some way of doing things?

ANDERSON: Well, that's a big order. I guess I left a lot of responsibility to the students and didn't try in any way to tell them on a day-to-day basis what they ought to be doing. I tried to be very careful in picking good graduate students and then would let them pretty much decide what they wanted to do. Now, we had essentially—this is not unusual in physics—one piece of apparatus that was running on a twenty-four-hour-a-day basis and I would talk over with the students various sub-problems in there. Now I'm talking of later years, when strange particles were our chief interest. In some cases, it was convenient from an administrative point of view to give one student one particle. And then whenever an example of that particle came from the apparatus, he would analyze that film. That didn't work in every case. But to answer your original question, I think it was my tendency, maybe by nature, not to try to direct on a day-to-day basis what they should be doing but to give them a problem and let them go away and worry about it. We did have many bull sessions, which I think were valuable—not that I originated them, but I did all I could together with Bob Leighton, to encourage a group of graduate students. I worked for many years with Bob Leighton, to encourage bull sessions, where each person would present his data and argue his point of view as to the interpretation of the data. I think these so-called bull sessions were very valuable things.

LYLE: It's been my observation that bull sessions are kind of difficult to get going. Did you have any problems?

ANDERSON: No, as I say, I didn't originate the idea or schedule them and say at four o'clock Friday afternoon we'll have a bull session. We might say that Friday at four o'clock some certain person would prepare a report on what he was doing and the others would come in and listen to it. But these would occur spontaneously, and often as a result of an argument. The fact is that there were many bull sessions and people would hear them starting and would drift over, attend them, and other topics would come up. No, I agree with you, there are certain things that you can plan and try to organize and they don't happen. I guess I was not what you would call a highly organized administrator, even though teaching or managing graduate students is not really an administrative job.

LYLE: Let me ask another question. I want to jump forward now. I read that you were asked in 1942 by Arthur Compton to be the director of the bomb laboratory. Could you just describe the whole situation and what you thought about it?

ANDERSON: We've discussed this somewhat before. I turned it down for economic reasons. According to the rules, I could not afford to do it, because the rules were that you could get a 20 percent increase in salary if you joined the war project and went to a different city. My mother was not well enough to move to, in this case, Chicago, and I could not move there and support myself and her in two different cities. Now, maybe that was more of an excuse than a reason. I don't know. I did not give that as an official reason.

LYLE: Do you remember what reason you gave?

ANDERSON: I may still have the handwritten letter that I wrote to Compton. I did go, very shortly after receiving the telegram, to visit Chicago and saw the small group—Fermi and others—whose chief activity was building under Fermi's leadership—he, incidentally, was an extraordinarily great guy—the first [atomic] pile, or the first reactor. This was in January or February of '42, and it first worked, it worked for the first time in December '42.

LYLE: And you were there in January.

ANDERSON: And. I was there in January, in the very early stages of this. Forgetting economic things, I think I would have turned it down on the grounds that I was not the right person. I had very little administrative experience. One could not foresee, in those days, that it was to be as big an operation as it was. In fact, it wasn't even known whether it was possible to make an A-bomb in those days.

LYLE: But theoretically you knew it was?

ANDERSON: Well, I think you could say theoretically you knew you could; but whether it would be practical or not, one didn't know. One had no idea, at least I didn't—maybe some people did—as to what the size of the so-called critical mass was, whether it was the size of a pinhead or a baseball or house. If the critical mass had been as large as a house, then it wouldn't have been a very practical kind of bomb. But I had not been thinking about these matters, I was not up to date on what was known in detail, particularly in the numbers. And. I think I would have turned it down on the grounds that I wasn't the right person.

In fact, I remember, in replying to Compton, suggesting several names of people whom I thought would be good people to ask. And I don't know either what the job I was offered at that time would have meant when things really got going. That would have depended on the person doing the job, whether I would have become director of the whole thing or not. Because there wasn't any whole thing; there was just a little group of people at Chicago. Although there were people doing other things, like separating isotopes and so on, in other places. Whether that came later or at this time, I'm not really sure. For example, Lawrence took over the job of trying to separate the uranium isotopes in a magnetic field. I don't know just when that work was begun, whether he was doing it at that time or not. He would have been a good person, I guess, to direct the project. I'm giving you vague answers, but I don't know what else to say.

LYLE: Speaking of Lawrence, how did you feel about the accelerators? Did you ever want to work with an accelerator rather than working with the cosmic rays?

ANDERSON: I knew Lawrence, and attended Physical Society meetings at Berkeley during the time he was building his very first tiny cyclotron. I remember seeing it, a little thing that you could hold in your hand, and it was made of glass. I knew Ernie very well, and. I certainly admired him as an extremely ingenious physicist. This was before the days when he became a first-class promoter in trying to get large amounts of money to build large accelerators. It turns out that was the thing to do. He was unusual. He promoted the building of large accelerators much better and much faster than I think 99 percent of physicists would have done, had they been in his place. That was part of his nature. I'm not saying this 1n any way as a criticism, because we all know what good physics has come out of big accelerators. They were absolutely necessary to build if you were going to study high-energy physics.

LYLE: At the time, were you interested in working with the accelerators at all?

ANDERSON: No. I was interested in working with cosmic rays. I guess it was in the middle or early fifties that the accelerators had been developed to such a degree that you could not hope to compete, using cosmic rays, in studying particles.

Begin Tape 4, Side 2

ANDERSON: No, I never had any desire to go to one of those large accelerator sites and work there. Life would have been entirely different. The young people like it. They don't know the life of simple physics. They don't have that choice. They're interested in high-energy physics these days.

LYLE: Going back to the proposal about working as the director of a bomb laboratory; what did you think about this whole thing?

ANDERSON: I had no feelings at all that this was a terrible thing and it shouldn't be done, and that life would be better for everybody if there were no atomic bomb. Nature is so constructed that it is possible to build atomic bombs, and I certainly felt very strongly that if there is to be one, it would be much better to have the first one built in the United. States rather than in Germany, for example.

LYLE: Can you describe when you got the telegram from Compton about the possibilities of working on the bomb?

ANDERSON: I received two telegrams. One of them was to me, asking me, I think, although it was worded because of the secrecy of the whole deal in a way so as not to give away any secrets. But I also received a copy at that same time of a telegram that Compton had sent to Oppenheimer, asking him to work on the bomb project and stating in that telegram that he was to be my assistant. Also, in my telegram it said that there was an accompanying telegram and that Compton wanted Oppenheimer to be my assistant. Oppenheimer happened to be at Caltech at the time we received these telegrams. I remember sitting on a cement bench shortly after we got the telegrams in the sophomore physics lab in East Bridge, discussing these telegrams and what we should be doing about it and so on. He didn't indicate to me how he felt about accepting or not accepting; this question didn't come up. He did make the statement that he felt the whole thing, so far as he was concerned, was academic because he felt so sure that he could not receive the required clearance to work on a project that was as secret and as highly classified as this one.

LYLE: I wonder if it was common for them to ask somebody if they'd do a job if there was any question whether they would get the clearance?

ANDERSON: I'm sure that before they sent the telegram, they had considered this matter, of course. I don't know just what he did about it. But I have no memory of hearing that there were any clearance difficulties in connection with Oppenheimer when he joined the project. But then I don't know the details. We didn't follow up with a second discussion of this. But I would feel, as you say, that they must have considered that he was capable of getting full clearance for asking him to do this.

Incidentally, Seth Neddermeyer and I shared an office. When Charlie Lauritsen decided to go to Washington to build the proximity fuse, he took with him Willie Fowler, Tommy Lauritsen and. Seth Neddermeyer. Seth and I shared an office, and we were both in the office one day when an FBI man came in and looked at both of us and said, "Is Seth Neddermeyer here?" Seth said yes, and he said, "I'd like to talk to you a little bit." Anyhow, I knew they were going to discuss security clearance, so I excused myself and said I'd leave the room during this discussion. And both Seth and the FBI man said that wasn't necessary. I could stay right at my desk, so I did. I did leave the room during the discussion; but I did hear Seth volunteering the fact to the FBI man that he was registered as a member of the Communist Party—not a card—carrying Communist, but you could be a Republican or Democrat or a Communist in those days.

It was an officially recognized political party in this country. And Seth volunteered that fact early in the discussion, and the FBI man said something to the effect, “Oh, well, that doesn’t mean anything.” So maybe they were not giving much weight to such things.

LYLE: Maybe they weren’t in the forties. The real problems came up in the fifties.

ANDERSON: I don’t know too much about this, but the problem certainly was very active in the fifties with McCarthy. He was a crazy man, McCarthy. I watched the televised sessions of the McCarthy hearings, and I just couldn’t understand how a thing like that could happen in the United States.

LYLE: Was Pauling on television at all, during those hearings?

ANDERSON: No, I’ve never seen Pauling on television. No, this was McCarthy’s investigation of the Communists in the Army laboratories and various Army facilities. I couldn’t understand how this sort of thing could go on in the United States. I thought McCarthy was a crazy man, and I didn’t see how he could get away with the kind of accusations he was making without more opposition than there was.

LYLE: Was Neddermeyer a political kind of person? Was he interested in politics, or were you completely surprised at this statement?

ANDERSON: No, no, Seth was not what you would call a political activist. I’m not surprised that he was—I didn’t know it until then—registered as a Communist. In fact, we didn’t discuss politics much. I was working with Seth for many years, and at least now, I can’t recall discussing politics with him. I don’t know, really, what his real political feelings are, even to this day. I’m sure his registration as a Communist was a mild protest against things that he disliked in the federal government. It didn’t at all mean that he was a real Communist in his beliefs.

LYLE: Now, if he had been Linus Pauling, for example, you would know what his feelings were, right?

ANDERSON: I've never discussed politics with Linus. Linus has asked me on many occasions to sign these numerous petitions that he was always coming up with about this and that. And I just refused, in as friendly a way as I could, to sign them, because I didn't have any of the intense feeling that he did about the importance of these things. I did sign a petition once, which I probably shouldn't have signed, that did get a lot of publicity. Tommy Lauritsen one day asked me to sign a petition, in my office, against the testing of very large nuclear weapons. This was at about the time that Stevenson was running for president. I liked. Tommy, and I foolishly signed it. I didn't read it. It was several pages. But the gist of it was to stop, I suppose unilaterally, the testing of very large weapons, like large hydrogen bombs as against ordinary fission uranium A-bombs. I don't know. Several people did sign it—Christy signed it, and Harrison Brown I think, and Tommy probably wrote it. So they signed it. There were ten people who signed it—Bob Walker signed it. Well, there were ten faculty members. And that had a tremendous reaction. If I had known that the reaction that followed was to be anything like it was, I certainly would not have signed it.

LYLE: What kind of a reaction was it?

ANDERSON: Well, the *L.A. Times* took it up and ran a story on it. DuBridge had to make a public statement that this was not the official position of the California Institute. I know that Tommy carefully put into that petition that the signers were not doing it as members of the Caltech faculty but only as individuals. Ruddock, whom I liked very much and who was chairman of the board at the time, also had to make a statement that it was not the official position of Caltech, and that Caltech supported the policies of the United. States government. It was the policy at that time to test big weapons. There was no restriction, officially. This made national news, and the American Federation of Scientists came out strongly in favor of our side. (They're an Eastern group, primarily) It caused editorials to be written, I guess, in most of the newspapers in the country. I went to DuBridge to discuss it with him. I told him that I hadn't read the petition.

LYLE: But you did know what it was about?

ANDERSON: I did know the gist of it. He asked me if I wanted to retract, and I said under no circumstances.

LYLE: Was he doing this because he was the president of the Institute?

ANDERSON: I don't know. He so strongly was against it, he couldn't see how anybody could be in favor of it. He showed me the mail—he had two stacks of mail on his desk, a little stack and a big stack. He pointed out to me that this big stack was mail that he had received in favor of the "dirty ten," as somebody called us. Matt Sands was also a signer to that. I didn't have any strong feelings, but it later became the policy of the United States government not to test weapons except underground. It was also charged by many people, and maybe it was, that it was a political statement, because it was just before the presidential elections when Stevenson was one of the candidates. I admired Stevenson, but it didn't occur to me that this thing had anything whatever to do with the presidential election. And it turned out I didn't vote for Stevenson, but I admired him greatly.

LYLE: So you wish you hadn't signed it, but you didn't want them to do the testing.

ANDERSON: Well, I hadn't thought it through. It was an impractical thing, because I don't know if the definition of big bomb was properly made in there. I mean, how do you know whether what you're testing is a big bomb or a little bomb; so that it wasn't a practical thing. If I had read it I probably would not have signed it because by nature I'm not a political activist; I have political feelings, but by nature I'm not one to get up and start petitions and attend meetings and do things like that.

LYLE: Did Linus Pauling give you a call and ask you how things were?

ANDERSON: No. I think Linus was not one of the people—I'm sure he would have loved to have signed this thing.

LYLE: How about Charlie Lauritsen?

ANDERSON: Charlie was wise enough not to sign it; he didn't sign it. The reason he didn't, I think he told me, was that he was receiving substantial sums of money from the government to carry on research, and it was probably not a diplomatic thing to do. I don't know what his real feelings were, but I would say all ten of the people who did sign it were getting funds from the government to finance their research. To me, it's something I did on the spur of the moment that I shouldn't have done. I had no business signing it without reading it thoroughly. But I did that.

LYLE: Are you aware that any funds were stopped because of your all having signed it?

ANDERSON: As far as I know, none of the ten suffered in any way financially by lack of government funds or reduction in government funds or by promotions at Caltech and so on. Although I'm sure the administration would rather that this had never existed.

CARL ANDERSON

SESSION 5

January 23, 1979

Begin Tape 5, Side 1

LYLE: When we left off at the last interview, we were just beginning the Second World War period. You mentioned that Charlie Lauritsen was going to Washington, D. C., to work on the proximity fuse. Did you think about doing that, or did you have work you wanted to do here?

ANDERSON: You're asking why I didn't go along?

LYLE: Yes.

ANDERSON: Well, a) he didn't ask me and b) this was early in the spring of 1940, just at the time that Hitler was splitting France in two with his tank columns, and had just reached or was about to reach the English Channel; so it was actually a year and a half before Pearl Harbor. This shows, I think, that Charlie Lauritsen could see what was coming. I was not thinking war at the time, and had nothing whatever to do with what was at that time a very small project, consisting of four people or something like that, who went to Washington. I think we said last time that—I can't remember the timing exactly—but they did get the proximity fuse into a state where it could be taken over by engineers and manufacturing companies and put into production. I think it was before Pearl Harbor. Charlie gave up the proximity fuse project because it was in good hands, given over to the engineers and companies that could actually build them, and came back to Caltech with the idea in mind, as he put it, of finding something good to put the proximity fuses on. In other words, to build a form of ammunition which would be superior to anything that the armed forces were using, and he started building rockets. This was not an outgrowth of the Goddard experiments; he used liquid fuel for his rockets. It was not related, in any way that I know of, to the Aerojet group, who also used liquid fuel, and whose primary purpose, at least in the early days, as far as I know, was to make rockets for take-off assistance for airplanes, so they could take off from smaller fields. So far as I know, there was very little interchange or communication between the two rocket projects, even though they both had to do with rockets.

The Lauritsen idea was to build artillery and to use solid propellant rather than liquid propellant; they were two quite different projects. Even the basic engineering was quite different because of the liquid versus solid fuel.

LYLE: Well, you referred to the Caltech Rocket Program, the one that Charlie Lauritsen was proposing. How did Caltech get involved in it?

ANDERSON: Caltech was not asked by anybody to work on these artillery rockets. It was Charlie Lauritsen's idea, solely. He thought it was a good thing to do, that rockets would in many instances be much superior to ordinary artillery, which from the smallest size to the largest size were bullets, where you have a projectile that's blown by a high-explosive, or maybe a low-explosive gunpowder, out of a shell; which means that the cannon or gun took a big recoil. The great advantage of a rocket over a cannon shell is that there is no recoil from a rocket. And it can be fired from a much simpler, much lighter weight mechanism than a cannon. In fact, the bazookas GIs would carry on their shoulders—I don't know the exact dimensions of a modern bazooka, but they're equivalent to something like a three to five inch cannon shell, which if it were an ordinary bullet fired out of a shell, it would require a five-inch cannon, which is a very heavy piece of mechanism, just to stand the recoil that accompanies the firing of the shell.

LYLE: Is a bazooka a rocket?

ANDERSON: It's essentially a tube, and then you put a rocket in it. You stay away from the rear end of the tube, because the rocket's blast shoots out there, and the rocket takes off. But the tube experiences no great force, as it would have to if it were the conventional bullet type of firing equipment.

LYLE: Well, when war was declared what happened at Caltech? What did people think would happen to the school? What was the general feeling then?

ANDERSON: Well, I think before Pearl Harbor, Caltech was pretty much involved in the war effort, in the sense that the teaching staff left their teaching jobs at Caltech and went off to other places. For example, many Caltech people joined DuBridge in Cambridge in the radar program. Also, the A-bomb project, as it became larger and larger, took away many Caltech people. But the rocket project of Charlie's was headquartered on the Caltech campus. But other space was needed and used. For example, the powder at first, in the very early days, was processed on the campus of Caltech, and that was a dangerous operation. There was an explosion in Kellogg which killed one person, I think—at least one—and injured some others.

LYLE: Were you around when that happened?

ANDERSON: I had not joined the project at that time, but I heard the noise from the explosion. Also, other isolated land was needed, and they went to what was called Goldstone which was just a dry lake out in the desert, with no facilities whatever, simply a dry lake. That was used for the early firings of rockets, where you needed space and isolation, not only for safety reasons but also for secrecy reasons. The project was highly classified. I didn't join it in the very early days. In fact, I can't remember when I actually joined the Lauritsen project.

LYLE: Did you go out there to live, or did you live in Pasadena?

ANDERSON: Well, there was no place to live out there. We used to drive out to Goldstone, carrying rockets whose metal parts were manufactured in local machine shops—that is, machine shops in the southern California area. I think at one time Caltech kept about 400 machine shops busy, machining the metal parts of the rockets. The fuel, the ballastite, the solid explosive material that was used as a propellant, was processed up in Eaton Canyon. As the project grew, it became clear that more space was needed, and space, instead of being scattered all over, that was more localized. I should not be telling you this history, because I don't know it as well as Willie Fowler, for example. It's too bad we can't ask Charlie himself, but we can't. I know that, as I just said, large areas were needed for testing rockets. As the rockets became larger and had longer ranges, larger pieces of ground were needed, and China Lake was selected after a very extensive search of various desert areas in southern California. It's a pretty large piece of land.; it's bigger than the state of Rhode Island, for example. It was set up as a Naval Ordnance Testing Station—it was called NOTS. There was already an airfield there, so it was quite suitable for the testing by firing rockets from aircraft, although all of the early work was done at Goldstone, where the dry lake was used as an airfield. At first, people just went out there and spent the day. Then later on, some sleeping quarters and a cooking mess and so on were set up. Goldstone was used for the airplane-type work until China Lake was acquired by the Navy for this purpose.

LYLE: Before you started working on this project, were you continuing you work on the cosmic rays?

ANDERSON: Yes. Not terribly actively, but I guess the answer's yes. I can't remember exactly what I was doing during those weeks or months before I joined the rocket project.

LYLE: What periods during these war years do you remember as being significant and important to you?

ANDERSON: Well, DuBridge, of course, was trying to staff his radar laboratory. I remember in the very early days going to Boston and visiting there and talking to DuBridge, who tried to get me, as well as everybody else he could find, to join the radar project. Probably the reason I didn't go away was that I had a mother to support, who was not very well and could not travel. I was not a free agent at that time. So it was natural to do work at Caltech, with occasional weekend trips or trips for a few days to Goldstone and China Lake. I never did spend any extended period of time away from the Caltech campus.

LYLE: But then you did become more and more involved in that project?

ANDERSON: My job, theoretically, was to head the work on the firing of Caltech rockets from aircraft. They were originally used for antisubmarine purposes, and also barrage rockets where probably they had their biggest payoff. A PT landing boat is a small boat and could be equipped, as they were, with machine guns, but they weren't large enough to carry the weight of even a five-inch cannon. But a single PT boat could carry hundreds of barrage-type rockets, where accuracy was not terribly important. They were of great value in island warfare, because they could just flood the beach with much more than 50-caliber machine gun bullets or even two-inch cannons in rarer cases. They could flood the beach with the equivalent of three-and-a-half inch, or something like that, shells, which were much more powerful than machine gun shells. They could fire thousands of them in a very few minutes during their critical period of approaching the beach for the landing operations. I'm not an expert, but it's my guess that that is where they had their greatest effect in the whole war effort—and I think a very substantial effect, in the sense that these landings were extremely dangerous operations in the early days, with very high casualty rates. They made the landing operations relatively safe operations.

LYLE: Were they ever able to make them more accurate?

ANDERSON: Well, this is a very complicated question. For the landing operations, you tried to cover the whole beach area with explosive devices. You were not pinpointing a single target.

The early use of Caltech rockets from aircraft was the retro-firing, in which they were fired toward the rear of the airplane. There were, at that time, magnetic devices being developed so that you could by flying low, detect a submarine. But you didn't detect it until you were right over it. So there was no point in dropping a bomb, because a bomb would have a speed of about a hundred miles an hour, and would land nowhere near where you were at the time the bomb was dropped. So that the retro-firing idea was to cancel the speed of the plane so that the rocket which carried the explosive device could be dropped essentially vertically from an airplane flying at airplane speeds. Or you could even shoot them backwards a little faster, so you could even more than cancel the speed of the airplane. It made possible the use of a signal, indicating you were over a submarine, it made possible the attacking of that submarine.

LYLE: Were they able to do that, then?

ANDERSON: Well, again, I really don't know. For a while, the aircraft were flying back and forth with these retro devices across the Straits of Gibraltar, which is not a long distance, so a plane could make many passes in one day. There were also the forward-firing rockets for submarine use, which the British were working on; Caltech did quite a bit of work with them. They were non-explosive and they were essentially rods of metal which would penetrate and go right through a submarine and leave two holes—one where it entered and one where it left—and at least it could damage a submarine. Again, I don't know the exact record, but I've heard that some submarines were sunk by those rockets.

I remember one contribution that Ike Bowen made—he was on the rocket project all through the war. I'm talking of the Bowen who later became the director of the Hale Observatories. The rockets had a tendency, if you fired them into the water, to skip, like a flat stone would skip over the water. He devised a very clever but very simple shape for the nose of the rocket, which would reduce that tendency and the rocket would penetrate the water and therefore would have a chance of hitting a submerged submarine.

LYLE: Did he do work like that at Caltech or had he gone someplace else to do that?

ANDERSON: He stayed at Caltech during the whole war. That was just one little item that he happened to do. His main contribution to the rocket project was in photography. He designed and had built a very high-speed camera so you could photograph the rocket as it took off and measure and study in detail the oscillations of it, which were very important in trying to get as accurate rockets as possible. It's interesting that in firing rockets forward from an airplane, the direction in which they took off from the plane was determined more by the direction the plane was actually flying, rather than—as in the case of gunfire—the direction in which the airplane was pointed. This is related to the angle of attack, so that the process of building rocket sights was quite different from building sights for firing ordinary cannon shells, for example. And the person who had to do with the development of sights for rockets, again stationed at Caltech, is Horace Babcock, who succeeded Ike Bowen as director of the Hale Observatories. Not that that had anything to do with his work during the war.

LYLE: In what sense was Millikan involved in these projects?

ANDERSON: Watson was the administrative head of the rocket project. He attended to personnel matters, to contractual matters, which were one agency of the government or other; at first it was through OSRD, the Office of Scientific Research and Development, and later with the services themselves. So he was the person who attended to all of those relations, which were very complicated and unusual, because during World War II, the military and the colleges first got to know one another. I mean, the scientists were not working with the military to any extent at all before World War II. So it was a new effort and there were complications in administrative dealings. So Earnest Watson was the man who was in charge of that end of affairs. Millikan was not a very young man. Let's see, the war started in '41 and he was born in 1868, so that would make him seventy-three years old. He did not actually play an active role in either the administration or the technical operations. So Lauritsen did not have to worry about all the paperwork. He could go ahead and build rockets, and if there were any problems, he would just tell Watson to get the paperwork problems straightened out.

LYLE: Where were the headquarters, on the campus? Was Watson on the campus?

ANDERSON: Yes.

LYLE: And was Charles Lauritsen also on the campus?

ANDERSON: Yes, but he traveled a great deal during that time, and made many, many trips to Washington, I guess primarily to get the services interested in rockets. That was a selling job. It's interesting that the Navy became involved in the Caltech rocket project. The Navy thought it was a worthwhile thing and supported it to a very large degree in all sorts of ways, turning over airplanes for experiments and providing boats to use for testing for the landing operations, and so on, which is quite different from the way the Army reacted. They really never got into using Caltech rockets. Whereas, at the end of the war, the Navy was spending something like $200 million a month procuring Caltech rockets when the war ended. That was a figure that was near, or it maybe exceeded, what they were spending for all other forms of ammunition put together. So the Caltech rocket project was extremely successful in getting into building devices that were actually used successfully and effectively in warfare. Now, when I quote these numbers, that should exclude the A-bomb project entirely, which was separate. It was an entity in itself. Although there were people who worked on both. In fact, Caltech did some work for the A-bomb project later in the war.

LYLE: According to this book, *Lawrence and Oppenheimer* [by Nuel Pharr Davis] Seth

Neddermeyer was one of the major people working on the A-bomb. Did you ever talk to him about it?

ANDERSON: I think when Charlie gave up the proximity fuse business that Seth worked on, Seth went to Los Alamos. It was Seth who thought of the idea of setting off an A-bomb by encasing the uranium core in a shell of high explosive TNT. That was Seth's idea, which was a major contribution to the whole business. You see, it's a question of critical mass—if you get enough uranium 235 or enriched uranium of a certain size, it just spontaneously goes off. A cosmic ray neutron, of which there are millions and millions, can set it off. I can speak freely because I don't know what I'm talking about here. But I think the first A-bomb that was dropped on Hiroshima had two pieces of enriched uranium, one at each end of a tube, and one of them was blown by a small amount of gunpowder over to the other one. When they came together, the two of them together had a mass that was greater than the critical mass, so it went off. I think that the Seth Neddermeyer idea is the standard technique, even today, for setting off A-bombs. I don't know whether I should be talking about all these things that I know so little about.

LYLE: While we're on this period, is there anything that I have not brought up that you would like to talk about?

ANDERSON: Maybe in a more lighthearted vein. Caltech professors showed their independence by leaving. I suppose Charlie told Millikan that he was leaving to go to Washington, and Millikan would have to find somebody to take over his classes, or Watson would have to find somebody to take over his classes when he went in 1940. So that Caltech as a teaching college was totally disrupted. The faculty just quit teaching and went off some place, or stayed on the campus but forgot their students, didn't pay any attention to them.

LYLE: What happened to their students? Were there some around.?

ANDERSON: Well, most of them, I think, disappeared, too. I don't know too much about the statistics. But Caltech had non-professional staff that carried on a teaching program. I remember the Navy V-12 program was centered at Caltech. Most of the Stanford football team came along with this Navy V-l2 program. The Caltech faculty shortly before that, had voted that intercollegiate football isn't a very useful thing. Then they reconsidered this idea. I know with the Stanford football team Caltech had at least a couple of years of a winning football team.

Begin Tape 5, Side 2

LYLE: Did you have any graduate students during this time? What happened to all the graduate programs during the war?

ANDERSON: During the war? Well, all of the peacetime research at Caltech, so far as I know, stopped. In some institutions, some of the professors, probably mostly non-citizens, carried on their own peacetime research during the war. I don't know of a single such case at Caltech, though there may have been some. I suppose graduate students took jobs on various projects, like the Caltech rocket program. I think Bob Leighton was a student at that time; I'm not sure. But, anyhow, many of the graduate students did join in and work hard and were very able to do highly technical, sometimes sophisticated work.

LYLE: It seems that after the war, people had a different way of thinking about running the Institute. Did you notice that?

ANDERSON: Well, yes, of course. I think, however, Millikan was doing a job that was much more difficult than that of his successors. They were presidents of a going institution. By the time DuBridge took over as president, Caltech was a famous institution. The day Millikan came there was nothing to speak of, except a few people like Noyes, and. I suppose that was it. Their job was to build an institution from scratch. I think that Millikan was an extraordinarily great person in so many different ways: As a physicist, doing physics; as a administrator, doing more than ordinary administration—actually building a new institution from essentially nothing, which meant getting money, for one thing, and getting people; and being a good enough promoter, or advertising agent if you like, to put Caltech on the map. I guess it took that sort of a person to do that entirely different job than to run a going institution, which had a good faculty at the time DuBridge came. This is not to say that DuBridge did not do a great deal for Caltech; he did. He was a very good president. He had some of the problems, of course, of getting money and staff, that Millikan had, but not to the degree of having to do it from scratch. I think you had to be a determined person and dictate certain things in order to do this.

LYLE: And with Morgan, in a sense, even though it was just a department, it was the same thing.

ANDERSON: That was a smaller example of a similar thing; it was a department.

LYLE: What about in chemistry? Noyes would have been the first person there.

ANDERSON: Noyes was a chemist, and he built up the chemistry department. He had a way of getting acquainted with and liked young people. I don't know the details of it, but my guess is that Noyes had a great deal to do with getting Pauling to come to Caltech. I think he had a way of sizing up very young people, by some means or other—people who showed promise in some way that Noyes could recognize. I think you could rightly say that Noyes was certainly the principal person in building up the chemistry department.

LYLE: During the war, was Millikan still pretty much the chairman of the physics department?

ANDERSON: The physics department essentially disappeared, in the sense that the staff was not doing their research; they were not doing their teaching. It was actually a war plant, probably to an extent much greater than most other campuses in the country. I remember Jim Page came through Kellogg with a visitor—I don't know who the visitor was. Jim Page was at that time chairman of the board. I remember I heard him say, "Isn't this a hell of an educational institution?" It turned into a factory.

LYLE: And could you tell that just from walking through it?

ANDERSON: Yes, of course. You saw what you would see if you went to the Douglas Aircraft Company. It was a manufacturing plant. As far as the Caltech rockets were concerned, I think the number of rockets actually fired, as they say, in anger at the enemy, exceeded a million rounds, even though the contract was for the purpose of research and development and not procurement.

But the Navy went along with Caltech, and Caltech actually provided—the figure I've heard, though again I can't guarantee it, is something like a million rounds that were used in warfare. Therefore, shouldn't Caltech look like a factory rather than an academic institution? They were being made, as I said, in something like 400 machine shops in the southern California area. The shops were given detailed drawings and made the metal parts of the rockets. On some of them, the explosive part was just a standard cannon shell, which the Navy had been using for years. So in that sense, they were sort of assembled out of, in part, existing devices.

LYLE: But some of them were assembled at Caltech?

ANDERSON: I suppose it was at China Lake. There was a big powder processing plant set up as part of China Lake, where they were actually manufacturing the propulsive explosive, made out of explosive material. I don't know where the actual assembly was done—when we're talking on the order of a million units, I don't know. But certainly hundreds and thousands were assembled in Eaton Canyon.

LYLE: I just wanted to get this picture of Mr. Page walking through, and what it was he saw. Did he see a lot of people working, or was it an empty campus?

ANDERSON: Well, I happened to be working in Kellogg, which was Charlie Lauritsen's laboratory at the time, and he happened to come walking through there. What you saw were people sitting at desks, like you see at an aircraft factory, except in the place where they actually assembled the planes. In the extremely early days, before the fire in Kellogg that was caused by an explosion, the rockets were assembled there. But that was soon taken off the campus. Talk to Willie Fowler. He knows the answers to all these questions.

CARL ANDERSON

SESSION 6

January 25, 1979

Begin Tape 6, Side 1

LYLE: I wanted to ask you if there had been any committees that you had served on over the years that you thought were particularly important.

ANDERSON: You mean Caltech committees or national?

LYLE: Actually I meant Caltech committees, but we could discuss both.

ANDERSON: Sure. Well, let's talk about Caltech committees. For many years I was on the Freshman Admissions Committee. This was way back in the early thirties. As you know, Caltech goes to a lot of trouble and expense in trying to find the best students it can for its freshman class, realizing that this is an extremely important thing for the Institute. One of the processes in the procedure of picking the students is to send out faculty members—not clerical help or PR people, but actual faculty members—to interview as many of the applicants for freshman admission as feasible; not only to talk to the students, but to their high school teachers and the principal and sometimes even visit with the parents. In my own mind one of the chief reasons for this is to let the student know what he's getting into and to let him ask questions and find out as much as he can about Caltech, to make sure that he really wants to do this, and to make sure that he's willing to work probably harder than he's ever worked before. Also, to size up the student and try to find out what his potential is, which is an extremely difficult thing to do. Well, anyhow, I did that for several years and enjoyed it. That's still going on.

Incidentally, I don't know if you happen to know one or two interesting statistics that say something about the quality of the freshman class. They are now required to take the college board exams, which are given nationally and are a measure, in a sense, depending on how good the examination is, of the student's aptitude or potential to do good work in his chosen field. It has been true for many years—I guess it's still true—that the average freshman has rated either in the 98 or 99 percentile in mathematics and in physics. That is an astounding statistic to me, that we are getting the top 1 or 2 percent of the students each year who are applying to go to college. Even in English, although that's not stressed in picking the students, I think their average rating—I may be quoting from some year a few years back—is the 92 percentile or something like that. It's very interesting to talk to these people and try to tell them what Caltech is and listen to their questions.

LYLE: Do you ever see these students again, after they have come, and talk to them?

ANDERSON: Yes. And in the interviewing, you always have a few students that you're particularly interested in. You think they may be extremely bright, but they may not be. And then it's a question of how to do this whole thing. A straight-A high school record alone does not mean that he's a suitable candidate for Caltech; although I would think most of our freshmen do have essentially a straight-A record in high school. They're usually the top man or second in the high school.

This is a long, long story, and we can talk about the morale of the student when he comes to Caltech and finds that he's not the top man in a class—they all can't be. All I want to say about this now, I think, is that it has surprised me that the morale of the freshman has been as good as it is, considering the fact that he's been accustomed to being the top man in a high school and now finds himself maybe average or below average. Still, most of them keep up their morale and like even the hard work that they find at Caltech. I don't know too much about those students who leave—the drop-out rate I know is higher than most people would like it to be. I've heard it said that it's not because of the inability of the student to do the work, but because of his loss of interest in studies or because he doesn't like Caltech or doesn't like the student houses, or something or other. Well, this is a big subject; I'm not an expert on it.

LYLE: It seems like a student who is always at the top in their high school class must by nature be a competitive type. Was there ever a conscious effort to look for students that seemed to be more cooperative than competitive people?

ANDERSON: Yes, that's an interesting point. I suppose if you could rate them in competitiveness, you would find that they were motivated to be number one for reasons of vanity; I don't know. I do know that many of our very good students have not studied hard in high school, and many of them have had interests in student activities and other work. The ideal student would be one who was not working for grades but was working because he was interested in the work and not trying to compete with fellow students.

LYLE: So did you look for that when you interviewed?

ANDERSON: Well, I don't know. I didn't think too much of that. I haven't done this for about forty years, so my information is not up to date.

LYLE: Any other committees that you'd like to talk about at all?

ANDERSON: Well, as I told you before, I was chairman of a committee of three, to admit graduate students. The idea is that the whole faculty should have a hand in picking graduate students, because they know the field they want to work in and the faculty members that they'll be associated with. Our little committee tried hard to get input from other members of the faculty, but it was very hard to do. They were busy. So it was largely done, in those days, by a committee of three. Now it's done by a much larger committee, which has much wider responsibilities having to do with assisting the graduate students after they become students of Caltech, and following their whole career and helping them in whatever ways they can. It's not now just an admissions committee as it was in those days.

But again, it's extremely interesting. What sort of philosophy should one have? One could pick students who would not flunk out and would do well by choosing, say, A- students from MIT or Princeton or Harvard or well-known universities. But we always had a category of people whom we called long shots. We were very uncertain about them. They came from small institutions that no one on the committee really knew much about. It was much harder to evaluate them. But our policy was to take chances on some of these so-called long shots that might be extremely good or might not even be students that could pass the work. I think on the whole we had pretty good luck with the long-shot category.

LYLE: Now, were these graduate students in all divisions?

ANDERSON: No, this is strictly physics. In picking graduate students, each option does the work. In picking freshmen, that's an Institute-wide operation that involves all the divisions, because most of the students don't really know what their major interests are. But in graduate work, they presumably have decided the field they want to go into. It's done by the departments, if you want to call them that—by the options.

LYLE: Did you find there was any big difference in the way the graduate students were selected compared to the undergraduates?

ANDERSON: One knows more about a graduate student because the student will have a record of four years in college. And it depends to a large extent on how well you know the college and how well you know the individual professors at that college who write recommendations. Through the years, you get to know that a certain professor in a certain college, if he says his student is really good, you can trust him. But then there are many who come from small, little-known colleges where no one on the committee knew the professor. We often talked about interviewing, but the practice of interviewing graduate student applicants never materialized. Maybe it would be a good thing or maybe not, I don't know.

We would canvass various groups in the physics department and try to find out how many graduate students they thought they could handle. We also knew that certain research groups in physics wouldn't take a student at all, wouldn't consider him as a graduate student in their group, unless he were a quite superior student. But we were familiar more or less with the demand. We had to guess at the percentage of those who would choose Caltech, because the top-notch students would apply at Princeton and Harvard and the top-notch institutions. There were such things as fellowships, and we tried to be competitive in the matter of awarding fellowships. For a period of years, for graduate students there were the National Science Foundation fellowships, and it's a fact that a very high percentage of the Caltech graduate student body were National Science Foundation fellows. They had received those fellowships previously, so we had no control whatever. We did know whether or not they had received a National Science Foundation fellowship, and I suppose that had some influence on the committee and probably gave that man a preference.

LYLE: Because financially they had some support or because they already had been selected?

ANDERSON: There was always a question of supporting graduate students. The post-Sputnik years, the early and middle sixties, brought forth substantial sums of government money because it was believed by Congress and other government officials that it was extremely important to compete with Russia in the matter of sending up satellites. Before that, after World War II until the Sputnik era, there were substantial government funds available for graduate student support, some with money that was given directly, like the National Science Foundation fellows, and then other money that came from contracts that Caltech had with various agencies of the government. We were able to employ graduate students on research projects, and money was available, I think, largely because Congress believed that it was in the interests of national defense to have a strong graduate research collegiate program in the country.

LYLE: At Caltech, has there been any serious consideration of making it only a graduate school?

ANDERSON: It is something that has been discussed and under discussion for many, many years. I know there are many people on the faculty who feel that undergraduate students are a nuisance, and you have to waste time teaching them and talking to them and so on, and if they weren't around, you'd have more time to devote to what you really want to do—your research. This movement of making Caltech solely a graduate school I don't think ever got very far. I don't know of any poll that was taken of the faculty, but I believe that if the faculty were polled, the result would be strongly in favor of keeping the undergraduate body, but having it of limited size, as it is. For many years—and even now, I'm pretty sure—there are more graduate students enrolled at Caltech than undergraduates. My own feeling is that Caltech would suffer greatly if it eliminated its undergraduate student body.

LYLE: Why do you think that?

ANDERSON: That's too hard a question to answer. I guess it's partly personal. It's a terrifically large subject to discuss. I just have a feeling that the spirit of the whole place would be different, and that the undergraduates contribute to that spirit, which is very good at Caltech. I've never been anywhere else, but I've found that Caltech has many, many very good features that are not usually found, as far as I can tell, in other institutions. And it would become, then, more like a strictly limited research laboratory. I think that would be an entirely different kind of place, and I think nothing like as good as it is. Also, the teaching demanded of the faculty is not very great. Three hours—sometimes plus a laboratory of another three hours a week—but three hours a week is not an unusual teaching load for a faculty member at Caltech. That shouldn't divert them from their research by any great amount.

LYLE: Are there any other committees that you'd like to discuss?

ANDERSON: Not really. At least those are the two chief committees. I was on both of them for quite a number of years. I believe now there's a more orderly process by which people are removed and new ones appointed.

LYLE: It sounds like it would be more interesting to be on the undergraduate one, though, because you could go and see the schools.

ANDERSON: Yes, it's a lot of fun. But that is also a time-consuming process. It depends on whether you interview students locally. I used to see them in New England; I think one year I went up to Maine. This takes time. Caltech, of course, paid travel expenses. But in those days automobiles were very much cheaper in Detroit than they were in Los Angeles, because they were in those days all made in Detroit. There were not assembly plants all over the country. So I used to buy a car, either a new one or a used one, at a big discount, if I happened to be in the Detroit area, and then travel by car and make a little money by selling it the following year out here, at a substantially higher price than I paid for it in Detroit, after a year's use.

I've also been a member of the Committee on Sponsored Research, which is a committee that has to approve all applications submitted by the faculty to any outside agency, government or otherwise, for support for their research projects. By far the majority of the Caltech research programs are supported by funds that are not the general budget funds of Caltech. Since the war, the support has come largely from one of the government agencies—might be the DOD

(Department of Defense) or the Office of Naval Research, or the Air Force, or in later years, the National Science Foundation; or it might come from an industrial company like IBM, for example. But most of the money was government funds. There is a committee that was set up right after the war, when government funds first became available, that scrutinized all applications for funds from agencies outside the Institute. And the system has been that most of this money was not money that Caltech obtained as an institution, but that individual faculty members, through their own prestige or reputation or knowing the proper person in Washington—anyhow, the application was done by an individual faculty member or by a group of people who would work on a project as a group. The main function of that committee was to make as sure as possible that the people who were asking for this money were not asking for it in order simply to maintain a research project, but to get funding for doing what they wanted to do. For example, in a questionable case we might ask somebody, “If you had a million dollars on deposit for your use in the Security Pacific Bank, is this what you would want to be doing?” I think that on the whole, the faculty at Caltech was doing, in the matter of research, what the individuals really wanted to do. They were not doing something simply because funds might be available for that particular project. The Institute received letters, many a day, from various government agencies, asking that they accept research projects to do specific jobs. And these were invariably turned down.

LYLE: Did somebody look at them, though? I mean, were they seriously considered?

ANDERSON: Yes, they were looked at by this committee. And there was some other screening process, because most of them were obviously research projects that should not be done in a college at all, but at the Hughes Aircraft Company or one of the myriad of companies in the area who live on government contracts doing work of a more applied character. And one function of this committee was to see that the professors could do what they were really interested in and wanted to do, and not what they were able to do because of the availability of funds.

LYLE: Were there any problems with it?

ANDERSON: No, no problems at all. Well, maybe one or two every three or four years. But

Caltech has been in a position through its reputation, through the excellence of its staff, to get funding from the government so the people could do the research that their heart was in, and not in any way act as a service group for the government. Now, there were exceptions to that. If the Institute had unique facilities, and if the Institute felt the research was of great importance to the national defense effort, for example, contracts might be accepted of this type, where one was doing what a government agency wanted done. But there was very little of that, except during the war, and then Caltech was not a college at all, but a research and development group and to some extent a factory for building war equipment.

But during peacetime, many of the Caltech faculty served on national advisory committees, who would meet usually in Washington, to advise the government on classified work. Caltech has had essentially no classified research projects carried out on campus. I say essentially, because there may have been one or two, but they were closed down. Some were left over from the war, but they were soon closed down, so that Caltech was doing what it wanted to do, which was pure science or engineering or chemical engineering—whatever the interests of that faculty member might be. We were not a service organization doing research for anybody else except the faculty at Caltech, doing what it wanted to do to a very large extent, like 99 percent. Except in time of emergency, like during the war.

LYLE: Do you think that's true at other schools?

ANDERSON: I don't have any real detailed information. I think it's true of what we would call the really good colleges in the country. There is classified work—you might say war work—supervised by colleges, where the work is carried on off campus. For example, Caltech is managing JPL. And there was a period when everything, practically, that was done at JPL was of a classified nature.

LYLE: But your committee would have nothing to do with making those decisions?

ANDERSON: No, our committee had nothing to do with what projects JPL decided to accept or reject. But I do want to make the statement that even at JPL—at least this was true several years ago—there was no classified work going on at JPL. And I think this is an extremely important thing for a college, not to have classified work going on, on campus.

LYLE: Do you think they didn't because of their connection with Caltech?

ANDERSON: JPL? Well, again I really don't know enough to answer that question, but I think that Caltech would be an influence in that direction. How strong it's hard to say. I think it would depend to a large extent on the administration—the president of Caltech and the director of JPL, who reports to the president of Caltech, although he has to keep in touch with and keep on good terms with the NASA officials who are supporting the work.

There have been many people who have said Caltech should get rid of JPL, that it's an educational institution and has nothing to do with this service work for the government. And there are many good arguments on both sides. But I think on the whole, the quality of the work done at JPL is of much higher quality because it is under this kind of supervision by Caltech than it would be if it were a strictly government laboratory reporting to the brass in Washington. There was a committee, chaired by Robbie Vogt, and I think they did an extremely good job and came to the conclusion that Caltech should not give up JPL. I have never detected any adverse influence on Caltech because of the presence of JPL, in any way, even through my work on the contracts committee. It has not perturbed the campus in any detrimental way whatever. There are people who believe there should be a much stronger interrelationship between JPL and Caltech, and that the students even should go up there and take advantage of the very high quality equipment of all kinds that they have up there at JPL. I don't know. I think too close a connection would be bad, but this is a big subject, and I'm not an expert on it. I have not thought about it a great deal.

LYLE: Why don't we change to the subject of the changes that took place in Caltech right after the war, when DuBridge came in as the new president and Professor Bacher came in as the chairman of physics.

ANDERSON: Of course, as we've said before, during the war Caltech was not really an educational institution. It returned very quickly, after the war, to the peacetime Caltech. People came back from their war projects very quickly after the end of the war and started on their work. The war, for the first time, introduced the military to the academic people in the country. They got to know one another, and they worked very closely with one another all through the war. I can speak only about the physicists, I guess, but physicists helped tremendously to do things that helped win the war that would have been impossible for the military to do. There were farsighted people who started getting the academic people to aid and to bring a new point of view and a new expertise to military problems, even before we were formally at war.

At the end of the war, government funds became available to support research in very substantial amounts. So life at any technical college after the war was entirely different than before the war, simply because funds were available to carry out research. And that made a big difference—maybe it's good, maybe it's bad, I don't know. Equipment could be bought. I know at Caltech there was quite an increase in faculty salaries, immediately after the war. Part of this may be due to the fact that it was perfectly legal for the Institute to charge a certain percentage of a professor's salary to the government, corresponding to the amount of his effort and time that he put into research. So that, very quickly after the war, Caltech got back to its business as an educational institution.

Begin Tape 6, Side 2

ANDERSON: We were talking about students. After the war, there was an increase in the number of students—undergraduate in particular, as I remember. My normal teaching load before the war was about three hours a week, and I remember teaching the year after the war for eight hours a week, simply because there were a lot of students and there was a need for people to teach them. There was the GI Bill, which helped students finance their college education, and this may have played some role. The whole research effort—I'm talking now about pure research that the Caltech faculty was really interested in doing—was for the first time, perhaps not adequately financed because there is no such thing apparently as an adequate amount of money for anything, but did have funds to carry on research projects which under the conditions prevailing before the war would have been impossible to do, because there was essentially no government money available for research in the colleges before the war. The professors didn't know the congressmen and the military people who had substantial funds. Of course there were ups and downs. About 1965, the amount of money available from the various government agencies became harder and harder to get, I guess today the funding by the government is limited more so than it was in the sixties. But even so, I think it is true that the annual government funding for research at Caltech has continued to increase. I'm not sure on this; I think those statistics are available. And this is for—I want to emphasize this—research that Caltech wants to do, not what the government wants done. That's done primarily in commercial laboratories and to a smaller extent in governmental laboratories like the Naval Research Laboratory and so on. Even though colleges are managing research projects, like Los Alamos, which is managed by the University of California at Berkeley, and Livermore, which are large places where a substantial amount of the effort is directed toward military classified problems—though Los Alamos does a great deal of unclassified work of a character that you could call pure science, unrelated to military matters.

LYLE: So, after the war, DuBridge came in as the president. Was there much discussion in the faculty about what programs were going to be pushed, or what departments and so forth?

ANDERSON: Well, yes, and I guess that type of discussion went on before the war and after the war and before DuBridge came and while DuBridge was here, and it's going on now. The various departments or groups want to expand; they want to hire new people, and they want to get the best people in the world if they can. One of the jobs that the division chairman has is to listen to all these people and try to determine, in what area expansion should take place. And it's a difficult thing. The faculty, if left to itself, would grow at an exponential rate with a pretty high exponent. In fact, that's what Caltech has been doing. It has been getting bigger, new buildings have been going up, and the staff has been increased, not by a very great percent. And all this is mulled over and argued about. The power is really in the hands, theoretically, of the Board of Trustees. But they will listen to the division chairmen, and they will listen to various professors in their group.

When I was division chairman, we had to come up with five-year plans—by what percentage the number of faculty should grow and by what percentage the budget should increase. All of these plots were exponentials, and exponentials have a way of getting bigger so fast that they become impractical. I guess it was about the year 1965 or so that some of these exponential curves of growth had to be pushed back. It really doesn't matter what you plot—whether it has to do with Caltech or government spending, you find exponentials, and you know that they can't carry on indefinitely. An example might be the federal debt; it's extremely large, and it can't go on forever. So someday there has to be a day of reckoning, but it can go on for a long time.

Since I've ceased being division chairman, I think funding for research has been more and more difficult to get. I do know that the increase in the faculty, percentage—wise annually, has been cut down, and the difficulty of obtaining tenure has been increased. In order for a division chairman to propose tenure for one of his professors, much more in the way of meetings and paperwork and letters from outside and inside the Institute is required than it used to be. This all boils down, largely, to funding. Although I was a member of a committee once, many years ago, whose charge was to come up with recommendations as to how big an institution Caltech should be, regardless of funding. In an ideal world, how big should it be, on the assumption that if it wanted to grow, there would be any amount of money available. This was certainly not an exercise in logic. It couldn't be done on any logical basis. I think the result was that the student body should be something like 2500—why, I don't know. But Caltech I think has been fortunate in being a "small" place—small compared with UCLA or USC or Berkeley. It has been in its history a unique place, I think. And it's probably losing some of its uniqueness. I think Caltech should try to continue to be unique, whatever that means—to be different from other places.

LYLE: Did you try in the physics department to think about what makes it unique?

ANDERSON: You're asking a question that certainly I don't know the answer to. One thing that to me was important was the availability of extremely good people. I could almost say that if a young, extremely brilliant, man were available, the Institute should hire him, and let him do what he wants to do. And it doesn't matter whether it's physics or chemistry or a joint effort between astronomy and something else. In other words, the quality of the faculty is much more important than the size of the institution. In an ideal world, to my thinking, if you had a university, you would do your best to get the best person in the world, talent-wise, and young enough to have a future, and not even ask the question whether he wants to work in physics or in chemistry or biology. But let him do what he wants to do. That is a program that's extremely difficult to administer and is in a way, I guess, completely impractical.

LYLE: Has that ever happened or even remotely happened that you can think of?

ANDERSON: Well, I think Caltech is searching for the best faculty it can find in all the departments. When I was division chairman, as I said, money was more available; expansion was easier than it is today. I quit when I became sixty-five, which was in 1970. And I have not been in touch to any great degree at all with the administration and this type of problem. But, in general, money seems to be harder and harder to get. The economy is in a very unstable mixed-up state; it's hard to make predictions.

LYLE: How did you like being a chairman of the department? You said that you put off doing that job for a long time.

ANDERSON: I put off doing administrative work, except committee work, for a long time. Let's see, I was division chairman from January '62—acting until the fall, when I became regular division chairman—from '62 until I became sixty-five, which was in the fall of 1970. So it was almost 9 years. I enjoyed it; it was different. I really enjoyed it. I have no idea whatever whether I did a good job or a very poor job. There was no reaction; nobody would say one way or the other. I don't think I was an extremely aggressive person in trying to make the physics department the biggest one in the Institute—although maybe it is. But if so, it was that way when I took over. One of the things that I gave great weight to was to find young faculty, to find brilliant people of great potential—hopefully youngsters—to become faculty members. I think that's an extremely important thing. We did hire some people who have proved to be top-notch scientists. I found that the faculty was full of ideas and wanting to do things. You didn't have to sit down and try to think up things that you should tell somebody that he ought to be doing. It wasn't necessary—the faculty was full of ideas. To me, they seemed, most of them, to be very good. I guess I was trying to support these ideas to the best extent that I could in trying to help get funding, to argue at the division chairmen's meetings about the need for hiring a particular person. But I think the problem now is more difficult. It was easier to expand in those days, to get the funding which is necessary, of course, for expansion, than it is today. But the division didn't in any way—well, maybe with a few exceptions—need prodding or need a division chairman to sit there with bright ideas and plan in particular what the Institute should be doing. That came spontaneously from the faculty.

LYLE: Were there ever problems with programs that weren't working?

ANDERSON: I'm trying to think now. Of course, one of the things about any institution is it tends to grow, and even programs that are not really topnotch can also tend to grow, especially if funding is available through a professor with direct contact with the government. I'm not aware of anything like that during the time I happened to be division chairman—having to close down some project because it wasn't any good. Some work, including my own work, died a natural death. That is, the study of fundamental particles by using cosmic rays to produce them was a very active field, but it died because of the accelerators that were being built. Bigger ones operating at higher and higher energies were developed, with much greater beam intensity than one could get with cosmic rays, so that that program just died a natural death. I'm not aware of any programs that presented a problem in the sense that, "Here, we've got to get rid of this thing."

LYLE: I did want to ask a question about the accelerator. There was one here at Caltech, and apparently they decided not to go ahead and build an even bigger one.

ANDERSON: Yes; I think this was largely before I was division chairman. There was a group of people, primarily Caltech people but not solely Caltech people, who were interested in the next generation of accelerators—a great big thing and of course very expensive, hundreds of millions of dollars and so on. There was a strong push on the part of some people for Caltech to be the chief sponsor to build such an accelerator. It was clear that this thing could not be a single-university thing; it would have to be designed in such a way as to accommodate people from a number of universities. And that's the way the large accelerators are being used today. In fact, Caltech is very active in accelerator programs and is doing first-rate work in accelerator programs without having an accelerator. They're using chiefly the one in Batavia, Illinois, and to a lesser extent the accelerator at Stanford.

Well, this came to a head before I was division chairman. When the decision was made, officially, that Caltech was not to undertake the building of a super-accelerator, I was not a member of this group pushing for the accelerator. My feeling was Caltech should stay out of it. It would be bigger by far than all the rest of Caltech was, so far as budget is concerned and perhaps people. That's true of JPL now; but that's a little different, because JPL is not really an integral part of Caltech the way this accelerator would have to be. Well, anyhow, it's a long story; I was not in on most of these arguments, pro and con, but the decision was made by the trustees through the advice of the president, who listened to this committee and so on, not to get into this business. My own feeling, although it'd be hard to justify it, is that it's a good thing that Caltech did not get into that business. It of course couldn't be on campus, but I heard that the Irvine Company in Newport Beach would have been glad to donate a square mile or so of their land as a site for such an accelerator. But I did not play an active role, one way or the other, although there was considerable time spent in studying and making plans for such an accelerator by Caltech people.

I am sure that this effort on the part of Caltech people had a strong influence on the design of the machine that's now running in Batavia, Illinois. In fact, we've lost some of our faculty people who like accelerators so much, they like them better than Caltech. Although some Caltech faculty, with no idea of leaving Caltech as a professor, are carrying out work at Batavia, which is now the largest accelerator perhaps in the world—at least in this country—and doing successful work.

CARL ANDERSON

SESSION 7

January 30, 1979

Begin Tape 7, Side 1

LYLE: I wanted to talk a little bit about your marriage and your family, and I thought I'd ask some questions about your sons. First of all, I think both of your sons are physicists; is that right?

ANDERSON: No, one is.

LYLE: One is a physicist, okay. I was just wondering if you had encouraged him or discouraged him to be a physicist?

ANDERSON: I tried not to encourage or discourage either one of my sons about what they should do in later life. I learned by that time that giving advice is a very poor thing because it's usually wrong. The older boy had no idea whatever what he wanted to major in when it was time to go to college, and I did give him some advice. He took, along with all the other students, the Iowa tests, I think they're called, in the tenth grade. He had a peak in mathematical aptitude, up to the 96 percentile, and an equally large dip in verbal aptitude, down to the fourth or fifth percentile. For this reason only, I told him I thought maybe a good thing to do would be to major in mathematics—not that he would ever become a professional mathematician, but in this world that's getting more and more technical and complicated and computerized and so on, it wouldn't be too bad an idea to have a certain background in mathematics. So he majored in mathematics. He went to the University of Colorado. In his junior or senior year, on his own, he got interested in computers—they had a computer of some kind there—and did actually become a professional mathematician. His title is something like mathematician and computer analyst. This surprised me no end, because he as a youngster showed no interest whatever in mathematics. He stayed out two years between high school and college, trying to figure out what he should be majoring in. Re worked for IBM as one of these fellows that goes around and services typewriters and has to wear a neat coat and a tie.

LYLE: Did you advise him to stay out for those two years?

ANDERSON: I can't remember. I certainly did not oppose his wanting to stay out. I don't remember whether I took any positive part in that at all. And the younger boy is a physicist. It's not, I think, my fault, because I didn't encourage him, at least directly, to go into physics. I may have, indirectly, because he was curious about various things, even when he was extremely young. He would point to something on the sidewalk that I couldn't see and ask, "What's that?" And then I'd look very, very closely—I didn't know there were such things on sidewalks. It was a tiny little black speck about the size of a period on a printed page, and it moved. So it must have been some kind of a little bug. Then we happened to buy a house that came with a swimming pool, and he liked that and spent a lot of time in it. And he asked why the steps looked so shallow from one end of the pool; actually they were high steps and they looked like very low steps. I tried to explain it to him in terms of refraction of light. We didn't get very far, and then I told him, "Well, you've got to study physics to understand that." And that then became sort of a phrase—when he asked questions, he himself would use that as the answer. And maybe that had something to do with his decision to major in physics. Neither boy did any graduate work; neither one was at all interested in carrying on with any kind of graduate work. Although the older boy, Marshall, did get a master's degree, but only after he got a job, and they would give him time off on pay, so he could study. And he did in that way get a master's degree. But neither boy was interested in graduate work.

LYLE: You were quite a bit older than most people to start having a family. Did that make a difference in how you raised your children?

ANDERSON: It probably did. I was never a young father, so I can't say what that experience is like. It's true that I didn't get married until I was forty years old. My wife had been previously married, and the older boy is from her first marriage. The younger boy is our boy. I think I have been very successful in treating them alike. In fact, I've never discussed with the older boy the fact that he was not my biological son. I treated him in every way equally, and I think I've been successful.

LYLE: How old was he when you became his father?

ANDERSON: He was born in ’43, April 30th, ’43, and we were married in June ’46; so he was three. The first thing I did was to adopt him, and I tried my best to treat him as my own son. And I think I’ve been pretty successful in doing it.

LYLE: Is there anything about being a scientist, do you think, that has any bearing on how you raise your children?

ANDERSON: Well, that’s pretty hard to generalize. I think the way people raise their children probably depends more on their genes and on their personality and makeup, rather than their profession. I guess there’s a big difference between the way professors raise their kids and the way, say, movie actors in Hollywood raise their kids. I don’t know.

LYLE: What about time; did you feel that you had more time since you were older or not?

ANDERSON: I feel I had plenty of time to be with the kids. I wasn’t working as many hours a day in those days as I did in my earlier days, although if I’d been married earlier, I probably wouldn’t have worked as many hours a day in the early days; I don’t know. No, I feel I had plenty of time to devote to the kids. Both of them liked to go fishing, and I tried to get as much of that in as I could.

LYLE: In the San Gabriel Mountains?

ANDERSON: Yes, up there, and summer vacations up in the Sierras, and off the pier at the beach. Although I used to fish there as a kid, and really catch fish, but now it’s altogether different in the kind of fish you catch. Well, and also, we were interested in sports I guess, spectator sports mostly; we’d go to football games. I made an effort to spend as much time as I could with the kids. I enjoyed it.

LYLE: Dr. Millikan had a son who became a physicist.

ANDERSON: Well, the Millikans had three sons, Clark and Glen and Max; that's in the order of their ages. Clark was an aerodynamicist. I don't know what Glen was. I know Max came to Caltech and spent one year at Caltech and didn't like it and left, and I think went to MIT. His career has been in economics. As far as I know, he's still professor of economics at MIT.

LYLE: Then also there's Charlie Lauritsen's son, Tommy. Did you watch these as father-son situations at all?

ANDERSON: That relationship was most unusual. Charlie used to take Tommy down to the laboratory when he was maybe, I don't know, twelve years old, thirteen, certainly when he was fourteen. Tommy would work at times—of course he was going to school, but he would spend a great deal of time at the laboratory working with Charlie as a very young boy, in his very early teens. I suppose that had a big influence on what he did later on in his life.

LYLE: You mentioned that you went fishing with the boys and that you went hiking. Were there any other activities that you did as a family that you did because it somehow was a good thing to do together?

ANDERSON: As a family. You mean including the whole family? I can't think of anything special. In 1954 we bought the house that we're in right now, and it has a swimming pool, which both of the boys loved and used a great deal, and many of our friends used it. In '54 when we moved here, David was four and a half years old. Anyhow, not long after that, he invented a game, completely on his own initiative—I didn't suggest that maybe this would be a good thing to occupy an afternoon with—but he got a pail, an ordinary household pail, and inverted that over his head so it was full of air. Then he weighed it down with some concrete blocks that were on the property because we had had a concrete fence built, and sat at the bottom of the deep end of the pool. And he'd sit there for, I don't know how long—it seemed forever to me; but it was at least five minutes or maybe ten minutes. Why he wanted to do this, I don't know. He did become interested in scuba diving at quite a young age and we got him some stuff, and he took lessons. He still likes to scuba dive. He's now living in Newport Beach and has a motor boat and goes over to Catalina quite often and still goes scuba diving. I understand that Catalina is a good place for that. But that's all his own doing. I mean it's not a part of the family activities.

I have been interested in watching auto racing from my very early days, and I did take both boys on occasion to see an auto race. My wife would once in a while go along, but she didn't care for it much. My younger boy still has an interest in auto racing. I like it, in part, because there's a lot of physics that you can think of in connection with auto racing. They're just on the bare edge of sliding off the track, and there's a great difference in the skill of auto race drivers. Some of them could do things that to me seemed almost to violate the laws of physics, which of course you can't do. I'm thinking of one person in particular, Parnelli Jones, whom I saw drive when he was first starting in. He was seventeen or eighteen years old, something like that—just a kid. But he could do things then with a car that very few people could do. I'd try to analyze the physics of just how it worked; how you could make a car not slide off a turn at a mile or two an hour faster than anybody else. I prophesied at the time that he would have a good career in auto racing, and he did have a spectacular career. He joined the national circuit and he set records at about every track across the country that he drove on. I used to keep track of how long some of those records were that he set.

LYLE: I wondered if you might talk a little bit about your work at White Mountain. What kind of things did you do there?

ANDERSON: Let me start by saying that we did have this B-29 airplane assigned to us very shortly after the war. I tried to get a Navy plane, because I knew some of the high brass in the Navy quite well from my war work. But the Navy planes were not suitable. Anyhow, the Navy got us a B-29. In fact, I got a notice that three B-29s were up at China Lake for our use. We only needed one, but we did get other scientists from other universities to use the other two, so they were all being used. I'm talking about the B-29s now rather than White Mountain that you asked about. We could have, had we thought more about physics and less about the engineering problems of installing a two-ton magnet in a B-29 and observing all of the safety precautions—we ourselves realized that safety was important. First we were told the magnet had to be put in the bomb bay, so that in case it caught fire or something happened to it, the pilot could push a button and jettison it like a bomb. We worked on that idea, which was a terrific handicap, because a) you couldn't go and look at the apparatus; the adjustments would have to be made automatically in the bomb bay; and b) the temperature, since we wanted to fly at as high altitude as we possibly could, would be many degrees below zero centigrade. Finally, we managed to talk the Navy into letting us put it inside the airplane. If we had spent some time thinking about physics rather than the engineering problems of how to cool it and how to handle 1600 amperes, I think it was—we had special generators which were standard Navy equipment that they put on some of their airplanes. So we had all kinds of engineering problems. What I'm saying is, had we spent an afternoon in thinking about physics, we could have discovered, by making very minor changes in our equipment, that could have been done in half an hour, we could have discovered strange particles. We could have gotten many, many examples per flight—the flight was usually pretty long, eight hours or something like that—and we could have gotten just loads and loads of very good data on strange particles. But we didn't, simply because nobody had the right idea. It's true we were not thinking about physics as we should have been.

Now, coming back to White Mountain, strange particles were discovered—I can't remember the year, but two cases were discovered [in 1947] by Rochester and Butler in England, the first two that were ever observed. This was several years after our B-29 experiments. And then nobody found any more of them for a few years.

In the White Mountain work that you mentioned that I did with Bob Leighton and some number of graduate students, we then were thinking about these particles and how to get more of them, since only two had been observed in the world. And we got fifty-some cases on White Mountain in one summer. We could have gotten thousands of cases in the B-29 several years before, if we'd had any sense and thought right about what we were doing instead of worrying about all these engineering problems.

LYLE: At White Mountain, you set up a camp or was it an institute? What was the situation?

ANDERSON: When I first heard about White Mountain, there was a Quonset hut that the Navy had put up for some purpose at just over 10,000 feet elevation. And that's where we set up our trailer and our cloud chamber to study cosmic rays. There was also a building at about 12,500 feet, that I happened to go up to for another reason several years later, but there was no road and we couldn't get our rather heavy magnet up there, so we decided on the 10,000-foot elevation site. There was a Quonset hut there that was very useful as a laboratory and to sleep in and so on.

There's a funny story about that Quonset hut. As was everybody else after the war, we were spending government money, and you were accountable for the expenditures in what I think was an unorganized way. The whole idea was new to the Navy, but it was government money and you have to account for government money. One day—this was several years after we were up on White Mountain—a young lady came into my office at Caltech and said she had a list of equipment that we had bought with Navy funds. She wanted to see them, so she could put a sticker on each piece of equipment. One was a developing tray, to hold film which you dumped into a solution of developer. I showed her that and said she could put a sticker on it, but it would come off the first time we developed pictures. The second item she had was a frequency meter that was built into a rather elaborate complex of meters and electronic stuff that was used in the B-29 that was sort of in the center, covered up. We could see it by looking through little cracks between other pieces. I said, "You can put a sticker on that if you can reach it." Well, one of the items that she had on the list was a Quonset hut, and she wanted to put a sticker on the Quonset hut. That really surprised me, and I said. "We don't have any Quonset hut." Then after a while, I remember that the Navy, several years before, when we were up on White Mountain, asked us if we would take, in a formal way, responsibility for that Quonset hut. I said yes; so in some way or other it got attached to my name and it got on this list. This was in my office in East Bridge, and the young lady wanted to see the Quonset hut. I finally remembered that there was a Quonset hut on White Mountain, which undoubtedly was the one that she wanted to see. In the meantime, it had burned 'down; otherwise I could have told her she could have gone up to White Mountain to 10,000 feet. [Laughter]

LYLE: Okay. I wanted to discuss the problem of having mathematics and physics and astronomy all together in one division. Do you think that's a good idea?

ANDERSON: I think the division structure that Caltech has is good from the point of view of minimizing the bureaucracy of running a college. As we mentioned earlier, Caltech has no deans that have administrative responsibilities with respect to various departments and so on. They do have the divisional structure and I guess there are six divisions at Caltech. I remember DuBridge saying this was a very nice setup because he had six people to talk to about administrative problems.

I'm not a mathematician, but when I became division chairman, I thought we could have divisional meetings where the physics and astronomy and mathematics professors all met together to discuss various things, like new appointments and promotions—anything they were interested in discussing. But I also found that mathematicians didn't attend such meetings. The astronomers and the physicists would attend meetings together, and they interacted. They could speak one another's language and so on, whereas the physicists had a lot of trouble talking to the mathematicians. So we'd have separate meetings of the math department, so-called—they're not officially a department. Many of them wanted their own division. I think they still do. DuBridge was very much against it because it would increase the administrative structure and the number of people he would have to talk to. It might make sense, I don't know. Because there grew up a group of applied mathematicians—a very good group of people—in the engineering division. Now, they should have been more closely associated with the pure mathematicians. I don't know if an applied mathematician can talk to a pure mathematician.

But anyhow, there were difficulties. I did my very best to support the mathematicians as much as the physicists. At one of the meetings of the math professorial group, I even got both DuBridge and Bacher to attend the meeting and tell them that Caltech officially recognizes that mathematics is as important a discipline as any other discipline. But there were difficulties. I suppose they, to this day, may still want their own division and want to run their own affairs.

LYLE: If you wanted to strengthen the mathematics department, what would you suggest they do?

ANDERSON: Well, the thing I tried to do was to get permission from the administration—which I did get—to hire a very distinguished, preferably young, mathematician at a completely competitive salary, a salary that was probably as good or maybe even better than he could get anywhere else. We tried to get a top-notch mathematician. But the mathematics group couldn't agree on who that person should be, nor what field he should be in—there are analysts, there are number theory people, there are topographists, there are statisticians, algebraists, and so on. We did discuss many, many names. We did get people to visit the Institute. These are people where you could get more or less of an agreement among the mathematicians that this was the right person. This was very difficult to do; and I don’t think we ever got a unanimous agreement, as we often did in physics and astronomy. Even though there were many different fields, you could get agreement as to the top-notch person to try to get. We did succeed in getting some very good, I would say top-notch, mathematicians, but they would leave Caltech. I would try my best to find out why, and I don’t think I succeeded in finding out the real reason in all cases.

I don’t know that I should mention names, but one person who was extraordinarily good and young and had a world-wide reputation—he was known as one of the top-notch mathematicians in the world. He did accept the job, but he didn’t stay more than three or four years. He said he loved Caltech, but he wanted to build up a little group. He didn’t want to be all by himself in his field, and it was just something that Caltech couldn’t afford to do. So he went to Stanford; they were able to promise him, I think, a half a dozen or so, assistant professors of his choosing in his field. As far as I know, he ‘s still there and still happy and still very active in research.

Take this rating by the American Council on Education of the various departments in all the colleges in the country. That was published, and Caltech did very well. We were always rated number one in astronomy, and I think that’s understandable. We had equipment that no other institution has, plus top-notch people. We were number one or number two in physics. But in mathematics, the rating was more like ten to fifteen. I guess that’s enough about the math department.

LYLE: Why don’t we talk about the astronomy? Had they already started putting the telescopes up in Owens Valley, when you became chairman?

Begin Tape 7, Side 2

ANDERSON: I can take no credit for starting the Owens Valley radio astronomy project. That was done by, I think, DuBridge, and certainly Bacher played a very large role in it. It was done before I became division chairman. When I took over the job, the site was picked. There were radio telescopes there, operating. I did my best to support it, and it did expand. We did build new antennas during my administration. We had very good people. I had something to do with hiring some of the young top-notch people who were interested in radio astronomy.

LYLE: Did you ever go up to the laboratory?

ANDERSON: Oh, yes, I made visits there, but not frequently, because there wasn't anything I could do up there to help them out. They knew what they were doing. Again, they wanted to hire new people, as everybody else did. We did manage to add to the staff and did manage, mostly through their own doing, to get funds for new telescopes. Radio-astronomy telescopes are expensive things. I think Caltech has one of the best radio observatories of any institution, certainly in the country—I think I could say that England has been strong in radio astronomy for many, many years. In fact, it's interesting that there are no Nobel Prizes given in astronomy. But, nevertheless, two radio astronomers, and I think just recently there was another one, got the physics prize. I see no reason why the prize can't be given to astronomers, because what they're doing is studying physics. It isn't in the lab, it's far away; but it's actually physics that they're doing.

LYLE: While you were chairman of the physics division, there were three people who got Nobel prizes—Feynman and Gell-Mann and Mössbauer. If you have people getting all of this recognition, and they're doing such high-powered work, what effect does that have on the division?

ANDERSON: Well, I think you can enjoy the reflected glory that comes to the division—when I say reflected, I mean as far as the division chairman is concerned, he has nothing to do with it unless he hired the person; then maybe he gets some credit. Well, naturally, everybody in the division, certainly in the physics group, felt extremely good about it. It enhanced the prestige of the division and the Institute. I remember one thing that made me feel good.; it was something Christy said. The day that Feynman got it, there was a physics seminar, and I wanted to get Feynman present at that seminar—we always had tea before the seminar. I knew he was extremely busy because he was beset by reporters and all that. But he did agree to come to the tea. I led, which I don't usually do, the group present there—anybody can attend these seminars, so there was a group of people—and I led them in four cheers for Dick, because in Sweden they always cheer everybody. It's a habit of the Swedes to give, I think, it's three, maybe four, cheers for anything good that happens. So I led the group in four cheers for Feynman, and there was an

Institute photographer there who took a picture of this. I was in my shirt sleeves, as I usually was at Caltech, with my mouth open and one arm raised; and that appeared as the cover picture on *Engineering and Science*. And. Christy said to me a few day's later that he liked that picture. I said, "Why?" And he said, "Because I like to see people in poses that are so completely uncharacteristic of them." So I felt good about that.

No, everybody is extremely happy when anybody in the division gets a Nobel Prize. Of course, one of the problems a division chairman has—and I certainly had it; and this happened even before the Nobel Prizes were awarded—other institutions are trying to build up their staff and trying to get the best people that they can find, so that the top-notch faculty at Caltech, not only in physics but in all divisions, are continually getting offers from other places. When people get Nobel Prizes, that's probably one of the chief problems that the division chairman has, trying to hold onto the best people on the staff, and trying to handle these offers that they are continually getting. And if they get a Nobel Prize, that doesn't help solve that problem. Except, I do want to say that in Feynman's case, he said he had made up his mind; he likes Caltech and he wants to stay here, no matter what offers he gets—and you can be sure he's gotten them from about every place in the world. I heard that when he answered the phone, he said, "Are you going to give me an offer?" If the person said, "No, I'm not at all interested in that," then he'd go ahead and talk to him; otherwise he would say, "The answer's no," and hang up. Maybe that's a joke, but I heard that about Dick Feynman. He's a very loyal Caltech professor.

LYLE: I've just about run out of questions. Is there anything that you think I've overlooked?

ANDERSON: At the moment I can't think of anything, but if I do I'll bring it up.

[Tape recorder turned off]

LYLE: Would you like to tell me about the dinner, when the Nobel Prize winners were invited to President Kennedy's dinner?

ANDERSON: Well, the year was 1962, I think. Mössbauer had just won the Nobel Prize the preceding December. One day, he received a phone call from the White House—and he told me this, so I have it on good authority—asking him if he could be present at a dinner at the White House on a certain date. And he asked them, “Would this be a big affair or a small affair?” The voice on the phone said it would be a pretty large affair, and they told him it was for all the Nobel Prize winners. And Mössbauer said he thought the date was wrong, and suggested that they move the dinner two or three weeks back because then it would more nearly coincide with the National Academy meetings and the Physical Society meetings in April, many people who would go to the dinner would already be in Washington, and it would save a lot of traveling on the part of many people. In any case, the dinner was moved.

LYLE: That’s impressive that they responded.

ANDERSON: I think that’s very interesting—the fact that he suggested that they do it. There was another person who received a phone call on the same day—I guess he also got the prize that year—and then didn’t hear a word from the White House for a matter of weeks and wondered why this was. And of course we know the reason. Well, anyhow, there was no speaker’s table there; there were just round tables, each one seating ten people. I asked somebody there about what the full attendance was, and I think I was told 169, which means about seventeen tables of ten each, all round tables, in three or four different rooms. Everybody got a little card with his table number on it; I got table seven. They separated wives, so my wife was in another room. Well, when the time came, I looked around for table number seven and found it, and then as you always do, you walk around the table to see who the other people are at your table, which I did, I found that Mössbauer was there, and that next to me was Mrs. Hemingway, and then next to her the President and next to the President Mrs. George Marshall, and next to her Mössbauer. Well, anyhow, I thought, “Gee, there’s one chance in seventeen of getting to sit at the same table as the President,” so I was pretty lucky. Mrs. Hemingway sat next to me at my right, and on my left was the Swedish ambassador’s wife, whom I had met before. Well, Mrs. Hemingway spent most of her time at the beginning of the dinner talking to the President. Then she turned to me and apologized for talking so much to the President and neglecting me. I, s-aid that was fine, so she said that she was talking to him about Cuba—this was shortly’ after the Bay of Pigs. And the Hemingways, as you know, had spent many years of their lives in Cuba. She said she thought the President ought to know a little bit about Cuba, so that’s why she was talking to him.

I talked only a little directly with Kennedy. After dinner, a waiter passed some cigars around, a big cigar box with full-sized cigars. I didn't take any because I didn't smoke cigars. Another waiter put a small leather pouch in front of the President, and he opened it and took out a small cigar and offered me one, which I took. I put it in my pocket; I guess I still have that cigar. I looked at it later and it was made in Havana, Cuba.

He gave a short speech. There was sort of a pouch on the back of his chair, and he reached back and got a big stack of papers and looked at only the top one, which apparently was the speech. He took out a pencil and did some crossing out and scribbling. Then he got up and made a speech, a very short speech. During that speech he said that he thought this was probably the biggest array of talent that had ever been in the White House except for the time that Thomas Jefferson was there alone. He didn't at all refer to any of these notes. I thought that was pretty funny. Well, after dinner, he picked up Mrs. Hemingway's place card and Mrs. George Marshall's place card, and put them in his pocket, I suppose with the idea of autographing them. So I thought that if he could do that, I could pick up his place card, which I did and still have.

Linus was there that evening, and everybody was introduced to the Kennedys in the reception line and shook hands with them. And Linus had picketed the White House that afternoon; I guess it was an anti-Vietnam demonstration, but it was right in front of the White House. Somebody had given him a placard which he carried, and there was a picture of him in the paper carrying this placard, picketing the White House. In the reception line, Jackie Kennedy said to him that she wished he wouldn't picket the White House, because every time there was a picket line in front of the White House, Caroline who was then a very little girl, would come up and say, "Mommy, what's Daddy done wrong now?" That was quoted in *Time*, too, I think.

LYLE: Did you talk to Linus that night, at the reception?

ANDERSON: No, I don't remember talking to Linus. There were a lot of people there. I do remember that after dinner the Marine band played, and Linus and Mrs. Linus started dancing in the hallway—it was sort of a marble floor. I don't think dancing was planned for the evening, but then something like eight or ten other couples joined in and danced for a while.

LYLE: Actually we didn't talk very much about Linus Pauling. We might go back just a little bit.

Did you see much of him?

ANDERSON: No, I have seen very little of him since he left Caltech, which was some years ago. He went to Santa Barbara. And now, he has an institution named after him. The Santa Barbara thing was political, current events and so on, a non-scientific group, I forget the name of the group. I don't know too much about this current institute.

LYLE: I think they're working on vitamin C.

ANDERSON: It has to do with scientific things in some way and maybe from a medical point of view; I just don't know.

CARL ANDERSON

SESSION 8

February 8, 1979

Begin Tape 8, Side 1

[Several minutes of commentary on old photographs has been deleted]

LYLE: Looking back, could you just briefly mention what times have seemed the most important to you?

ANDERSON: Well, I think one would be when I was a sophomore taking that special course that Ike Bowen taught, where I first really learned what physics was all about, and that you could also make a living doing it. It was a lot of fun. That probably had a lot to do with what I did for the rest of my life. As I look back, too, going to Caltech and getting to know Millikan well, whom I think is certainly one of the greatest, by far, people that I've had almost an intimate acquaintance with; and the effect that he's bound to have on you in many, many ways, many of which I'm not conscious of. I certainly owe him a tremendous debt for my whole career,

LYLE: And these are personal feelings you're talking about.

ANDERSON: These are personal, yes. I knew, of course, that he was a great scientist, and beyond that a great administrator—a really creative guy—and that if it hadn't been for Millikan, there would be no Caltech now, It certainly wouldn't have the character it has now, which is unique.

LYLE: Did other people also feel that strongly about him? Did they really like him that much?

ANDERSON: I think the people I know who worked with him had a feeling of extreme admiration. I haven't gone and taken a census of: What do you think about Millikan? But he was known as the Chief; that was common throughout Caltech. You never said "Dr. Millikan"; you said "the Chief." I think that was more widespread than just the people in physics, who happened to be closely associated with him. I'm not completely sure of that, but I think you might ask some chemists—ask Ernie Swift if he was known as the Chief. But we always thought of him as the Chief.

LYLE: And this was a term that implied that you liked him?

ANDERSON: Yes. The affectionate part of the meaning of chief.

LYLE: Can you think of any other times, particularly, that are vivid?

ANDERSON: I think so far as research goes, the work preliminary to finding the meson, as it's now called; we called it mesoton, and Millikan turned that into mesotron by our cabling of the "r" that we talked about. That was a period of three or four years, where there was clearly a paradox, something apparently very important that was in the data that we couldn't ferret out, and we just couldn't understand or find any explanations that didn't have contradictions in them in terms of physics as was then known, and then the final resolution of the paradoxes and the contradictions, with the finding of the mesotron. The positron was a purely accidental discovery. There wasn't that buildup or that excitement, that puzzlement that kept you awake nights and went on for two or three years.

LYLE: Another question that I had was, when you were in high school you had a goal of studying electrical engineering, and that this was something you really were looking forward to doing.

ANDERSON: Yes. Ever since as far back as I can remember.

LYLE: I wonder if you remember any other goals like that that really focused your attention. That is, after you got to Caltech, you had reached that goal. What was your next?

ANDERSON: Well, it was changed by Bowen to an interest in physics. I'm glad that events turned out in such a way that I was able to stay on at Caltech and become a faculty member, because I'd never been anywhere else as a faculty member. In fact, this was the only job I've ever had in my life, except as a youngster selling papers or working for a billboard company counting cars, and things that you did as a kid. But I have found that Caltech is a really wonderful place to be, and I think free of jealousies and political maneuverings and the sort of back-stabbing that you hear about often and people trying to get ahead. I'm not an expert because I've not spent much time at any other university, but I've heard many stories and I've talked to people at other places. Caltech certainly has been a unique institution in many ways. It's small. I remember a remark that I. I. Rabi made, who was a professor at Columbia for many, many years and active in national physics activities. He said that in his experience, there were very good people at many universities, but the thing that was different about Caltech was that everybody was good, not just a few people in one department and one or two in another department, but just the general level of the whole faculty. I think there's a great deal of truth in that. I hope it can continue in the future to be the unique sort of place that it has been in the past, but that's going to be more and more difficult.

LYLE: So when you went into the physics course Bowen taught, it changed your direction; you switched and went to physics. Where did you see yourself going then—to be a professor and do research?

ANDERSON: Well, I planned to do physics in a university. And. I realized that all Caltech graduates couldn't stay on and become members of the faculty and I really didn't, I think, have that as an ambition or a serious goal. Although I think it's true that a very large percentage of the graduates of Caltech who are interested in academic careers—many of them are not, but of those that are, probably most of them would like to stay on and be invited to become a member of the faculty at Cal-tech. At least I've known, I think, hundreds of students for whom that was true.

I avoided administrative work, I think, for many, many years. For example, I was asked by DuBridge to be division chairman shortly after he came to Caltech, when Watson was acting division chairman. I turned it down—I think we talked about this before—immediately; I knew I wasn't the person for it. It turned out that they got Bacher to come to Caltech, I think primarily to be division chairman. There's no doubt in the world but that he did a very much better job than I could possibly have done if I had accepted that offer. I was very happy doing research and the small amount of teaching that's required of Caltech faculty. I really enjoyed my teaching; I'm not saying I was a great success as a teacher, but I enjoyed doing a small amount of it. I would not have enjoyed teaching three, four, or five hours a day.

LYLE: So during the war, your work was pretty well defined. Then after the war, did you again have to decide: What is it that I want to do?

ANDERSON: I wanted to continue with my research. There were many unanswered questions. Using cosmic rays as a tool to study the basic particles of matter was still a useful thing to do, in the sense that cosmic rays were a good tool to do that. And I was interested in fundamental particles of matter. That was true for perhaps ten years after the war. Then cosmic rays were not the useful tool because the accelerators had taken over and one could do work in studying the particles in a much better way using the accelerators.

I have never had any desire to become a member of a team to work on a big accelerator, and that's the only way you can do it. They're extremely large, extremely expensive, expensive to operate; time is at a premium; you have to work very efficiently. I hope that the time was not assigned in such a way—and I don't think it was—as to guarantee that certain results would be forthcoming in any given hour. I think the people doing the management of the accelerators have been very wise, in the sense that they have done things where there was no certainty at all that results would result, even realizing the great amount of money that was being spent. Which leads you to wonder what the discovery of a new particle is actually worth in money. Well, this is something that can't be answered, because they're not marketable items. This has to do with the whole reason, I think, why people do science. It's to learn new things about some field that they happened to be interested in.

And, incidentally, I think the public as a whole does not understand what science is all about. I would guess that not more than one percent of the general public, if that much, really knows what a research biologist or a research chemist or research physicist is really doing, and why he's doing it. And I think this is a very serious thing. But to correct it is a job of education, and there's no way to educate the public in matters of science. It's going to be more and more difficult as science becomes more and more complicated.

[Tape recorder turned off]

LYLE: You said that after the war, there was so much government funding.

ANDERSON: I think it was available because the people who had the purse strings thought that all basic research had a close and important connection with the defense effort of the United States. And it's true that it does have a connection, however loose it may be, and if one considers extended periods of time—well, practically all of our weaponry in one way or another is based on fundamental research. The A-bomb grew out of Otto Hahn's accidental discovery of fission. So you can't say that fundamental research in physics or chemistry is completely disconnected from weaponry. You just can't predict what part of it will in future years be valuable in building worse and worse weapons. Although weapons are about as bad as they need be, I think, at the present time.

Begin Tape 8, Side 2

LYLE: What do you think about the research in physics; that is, all the work with the accelerators now. Do you think it's a worthwhile thing to do or are you wondering about this?

ANDERSON: Of course, of course. All physics is worthwhile. What else can an ex-physicist say? You sometimes wonder if it's worth putting [so much money into it]. It's too bad that some kinds of research in physics happen to be so expensive. But if one is interested in high energy physics, which is very closely connected with particle physics, and if one wants to get ahead, there is no other way except to spend large sums of money.

LYLE: But do you think that one should do that?

ANDERSON: Do I think society should do that? If you answered the question, how many millions or billions of dollars is a fundamental particle worth, then I don't know. It's faith. It's like a religious feeling. You just have to explore the physical world, including the faraway places that the astronomers study. It's a part of human nature; curiosity is just part of people and there will always be science for the sake of science—I mean, for the sake of pure understanding. I think that will always be part of the world. Considering the ever-increasing rate at which it's changing in so many, many ways, if you try to consider what the world will be like, say a hundred years from now, you'd go absolutely crazy in the attempt to do it. You have no way of knowing what the world will be like a hundred years from now. A lot will depend of course on whether it's a peaceful world or a world with a lot of wars.

LYLE: I want to go back to the question of the goals in your life. When you saw that the cosmic ray studies weren't as necessary anymore because of the accelerators, what did you think then about what you would do?

ANDERSON: That, more or less, coincided with my accepting an administrative job. Now, most people at Caltech who have administrative work also are active in research. My becoming division chairman, as I remember, almost coincided with the accelerators taking over the particle business. I did try to do some research, even after retiring as division chairman and going on half-time, I did some research then, under some difficulties because I was near enough at that time to retirement so I could not expect to take on graduate students; it just wouldn't be fair. After retirement, you are severely limited in funds, because the government will not—in fact, I think it's against Caltech rules to accept government money after retirement in the usual way that you do when you're a college professor. So I did some minor work that had nothing to do with particle physics, but it did have to do with things that I had thought about for many, many years but were less important than what I was doing at that time, namely working with cosmic rays and particles.

LYLE: Did you enjoy doing these last experiments?

ANDERSON: Yes. Not as much as the earlier work.

LYLE: When you became chairman of the department, did you have any thoughts about what you were going to do, any goals in that job?

ANDERSON: I don't think I needed to sit down and try to think up things that Cal-tech should be doing in physics, mathematics and astronomy, that it wasn't doing then. I looked on the job of division chairman as a person who did all he could to help the active research faculty. In the physics group and in the mathematics group and in the astronomy group, new ideas were coming out daily from almost every faculty member of new things that just had to be done. This was the natural state of affairs if you support good people, and they were and are. Faculty at Cal-tech are good people and have good ideas. If they all get their way, the growth will be exponential. And this is wonderful, from the point of view of doing a lot of research, but then there are practical limitations, like funding for example, so that there were some difficult decisions that had to be made as to which field of physics should be supported as against another field. There were limitations on the size of the faculty, for example; to make a new tenured addition to the staff is a tremendous responsibility financially that the Institute has to assume. So probably one of the most difficult things that the division chairman has to do is try to do the best he can to balance the wants of the faculty and the practical limitations that exist. Space and money I guess are the two limiting factors,

LYLE: When you were chairman, did you get the physicists together and really work out what the goals of the group were?

ANDERSON: Yes, indeed. Both in formal ways and informal ways. And by informal ways, I mean talking to individual people and listening to what they just had to do. It just had to be done; there wasn't any doubt about it. They had to hire that exceedingly bright and promising young man who happened to be at the moment in Princeton, and we just had to have him on the Caltech staff. I tried to get input from people to as large an extent as I could. For any important decision, I had a certain few people that I would discuss these matters with and get their opinion. It might be different people for different sorts of plans and activities. And we were also asked by the administration to come up with five-year plans as to the rate of growth, which of course was limited by funding, and as to what the division hoped and expected to do.

LYLE: And were those plans helpful to you?

ANDERSON: Mostly no, I think. Well, yes and no. It got you thinking and it educated you in practical matters like funding. This country is a democratic, free society, and the dollar is terribly important. Personally, I didn't realize the importance of the dollar until this time. I was happy working on a small salary, doing things that I loved to do and was not in any sense trying to make a fortune. But one thing any administrative officer in any organization, be it Caltech or any other organization, has to do is to worry about dollars—the availability and the spending of them and so on.

LYLE: You began to appreciate Millikan's concern about finances?

ANDERSON: Well, Millikan was exceptional. He probably talked to other people, but it seemed as though he made all these decisions by himself; and most of them, on the whole, were very good. Caltech managed under Millikan to be a great institution and to do a great deal of research, even though the funds were very limited. In those days, research was more of an individual effort, and the equipment was much simpler and topnotch research could be done with less funding. It seems to be harder as time goes on—I'm speaking now of physics—to find problems to work on, research projects that are important and worthwhile and still inexpensive. And it's in the very nature of things that particle physics is expensive, because it requires high-energy particles and it's just very expensive to make a beam of high-energy particles. There has to be a cooperative effort, and it isn't a thing an individual can do. It's even reached the point where a single university can't do it; and they have to form a consortium and work as a group and finance a project that several universities would participate in.

LYLE: So after you were chairman, you went back and worked on some smaller problems. How many years did you do that?

ANDERSON: Well, let's see. For a matter of two or three years, I think. I was starting to say it was very difficult because one couldn't get funding even to hire a technician to help do the necessary amount of technical man-hours type of work that has to be done. I understand now that I have this title now as Board of Trustees Professor of Physics, Emeritus. When I was told by the provost, Christy, that I'd been given that title, I asked what it meant. And part of it is that you can if you apply, get funds for research, even though you're retired. But I got this title at such a late date that I had already given up doing research at Caltech. In fact, you don't get the title until you retire. People vary greatly. There are people—I think Max Delbrück is one—who will be as active in research after retirement as he ever was, given proper health and so on. I think the motivation is there. I think in my case, motivation to do research after retirement was not nearly as strong as in many other people.

LYLE: Also your main line of research had changed.

ANDERSON: Exactly. It's very difficult in physics to do things on a small scale that you also consider important enough to be doing. And this is probably more true of physics than chemistry and biology. There, I think, the biologists could think of lots of important problems that can be worked on by small groups and without tremendous budgets.

LYLE: Now that you have retired, what do you want to do now?

ANDERSON: I want to learn to walk again after this prolonged problem with my hip. No, I have not been bored by retirement at all. You read about people who sort of go nuts when they suddenly find they're retired and they no longer have anything to do when they have all the time on their hands and don't know what to do, and they are very unhappy. I think retirement is great and I recommend it highly. I've never felt bored or felt that there was a minute when I didn't have something to do that interested me.