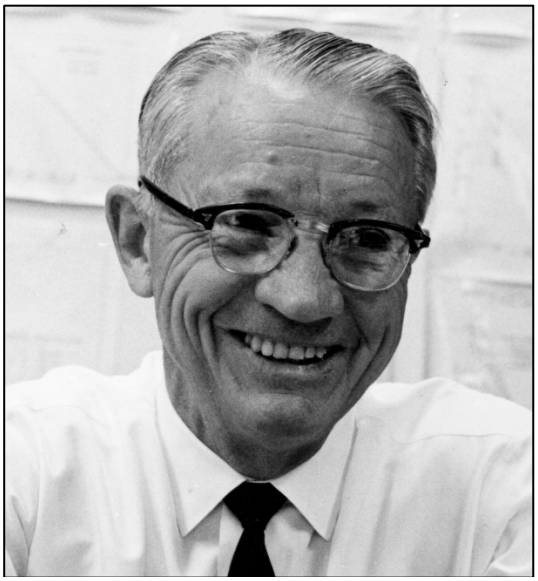

H. Victor Neher, 1970

H. VICTOR NEHER
(1904 – 1999)

INTERVIEWED BY
RACHEL PRUD'HOMME

April 27 and 28, 1982

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
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Abstract
An interview in two sessions, April 27 and 28, 1982, with Henry Victor Neher, professor of physics, emeritus, in the Division of Physics, Mathematics, and Astronomy. Dr. Neher received a BA in physics from Pomona College (1926) and became a graduate student at Caltech in 1928, earning his PhD three years later.

In this interview, he recalls his undergraduate years at Pomona, his growing acquaintance with Caltech, and his work as a graduate student on the scattering of high-energy electrons. Recollections of Robert A. Millikan, Earnest Watson, William H. Pickering. He describes Caltech's cosmic-ray group—Millikan, Carl Anderson, Seth Neddermeyer, Ira S. Bowen—and the plane trips he made throughout North and South America to measure cosmic rays. Recollections of Richard Chace Tolman. Comments on his PhD orals, on teaching undergraduates, on some of his memorable students (Kip Thorne, Howard Berg, H. Guyford Stever). The 100-to-1 Shot Club; John Anderson and Russell Porter. The effect of the Depression on Caltech. War work at the MIT Radiation Laboratory. Changes at Caltech during presidency of Lee Dubridge. Postwar balloon flights

to study cosmic rays. Comments on Frank and J. Robert Oppenheimer and their political difficulties during the McCarthy era. Early campus atmosphere and changes over the years. Donald Glaser and development of the bubble chamber.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH H. VICTOR NEHER

BY RACHEL PRUD'HOMME

WATSONVILLE, CALIFORNIA

http://resolver.caltech.edu/CaltechOH:OH_Neher_H

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with H. Victor Neher **by Rachel Prud'homme**

Watsonville, California

Session 1 **April 27, 1982**

Session 2 **April 28, 1982**

Begin Tape 1, Side 1

PRUD'HOMME: Professor Neher, tell me a little bit about yourself. Where were you born and brought up?

NEHER: Well, I was born in Kansas. My folks had been back there about four years before I was born, so my older brother and older sister were also born there. We moved back to California when I was nine months old. So I was raised in the San Joaquin Valley, almost due east of Watsonville. I went through grade school in a one-room schoolhouse. We lived in the country. My father was a grain farmer and farmed several thousand acres—mostly barley and some wheat.

PRUD'HOMME: And you went to Pomona?

NEHER: I went to Pomona College, yes, largely on the recommendation of one of my high school teachers.

PRUD'HOMME: What did you study there?

NEHER: Well, I thought I was going to be an electrical engineer, so I began taking courses in physics. I took a general course in physics my first year. My teacher was Mr. [George Gale] Hitchcock, who had a Model-T Ford. He would take us over to Caltech for the Friday evening demonstration lectures given in 201 Bridge [Norman Bridge

Laboratory of Physics]. So my first contact with Caltech was in the fall of 1922. There was not much then at Caltech. The east wing of Bridge had been built, and they were in the process of building the west wing. Of course, Throop Hall was there.

PRUD'HOMME: What made you decide to go there, as opposed to some other institution?

NEHER: Well, I thought it was a good school. And [Robert A.] Millikan [Caltech's head (chairman of the Executive Council), 1921-1946] impressed me. He gave one of the evening demonstration lectures there that we heard in the fall of 1922. I was very much impressed with his lecture and what he had to say.

PRUD'HOMME: Can you remember what the lecture was about?

NEHER: Yes. At least part of it was on magnetism. At one stage, he asked a question of the audience: "How does the strength of a magnetic field, like the Earth's, change with distance?" He looked right at me, so I took it rather personally. [Laughter] My knowledge of physics was rather slim at the time. Anyhow, he used to do this; he would pose questions to the audience.

PRUD'HOMME: Is he the one who persuaded you to change to physics from engineering?

NEHER: No. I didn't know him personally all through my Pomona years, although I went over to Pasadena on a good many occasions for the Friday evening lectures.

PRUD'HOMME: What was Caltech's application process then to be a graduate student? Did you have to go through rigorous interviews and tests and so on?

NEHER: While I was at Pomona, I got to know some of the people at Caltech. I received my bachelor's degree from Pomona in 1926. In 1925, there was a new instructor in physics, who had come from Colorado College. His name was [Roland R.] Tileston. He was an extraordinary teacher. He is the one, I suppose, more than anybody else, who got me changed from engineering to physics. He used to load his Dodge up with his students,

and take them over to these Friday evening demonstration lectures and also to other lectures. So I gradually got to know some of the staff at Caltech. One of his students at Colorado College was [William R.] Ralph Smythe, who was, of course, on the faculty at Caltech [professor of physics, emeritus; d.1988]. I knew him early on. Then I stayed on at Pomona for two years after I got my bachelor's degree, to help out in the physics department; Tileston was expanding the department and needed some help. He was getting in a lot of new equipment for the laboratories and for his lectures and so on. That was a great opportunity.

There was an interesting episode in connection with that. During my senior year at Pomona—in fact it was in the early spring of 1926—Tileston had said that Dartmouth would take any student he recommended for graduate work. This was because Tileson had come from Dartmouth, and they were anxious to get graduate students. He wanted me to go to Dartmouth. However, I had already made up my mind to go to Caltech. I was talking with him one day, and he said he had to get somebody to go back to Dartmouth rather soon, because this position would not be open very long. He asked me if I knew anybody who might be interested. Well, my roommate [M. Stanley] Livingston was majoring in chemistry. I thought maybe he might be persuaded to go into physics. So I went over to the chemistry lab and asked him if he wanted to be a physicist instead of a chemist; I told him what the situation was. He came over and talked with Tileston. Tileston gave him twenty-four hours to make up his mind whether he would take this or not. He decided within twenty-four hours that he was going to be a physicist! Livingston later worked with [Ernest O.] Lawrence at UC Berkeley on cyclotrons and ended up at MIT. He became one of the country's experts on high-energy particle machines.

PRUD'HOMME: When you came here as a graduate student, what was the campus like? Where did you live? What did you do with your social life? When you wanted to get away from it all, where did you go?

NEHER: Well, I lived up on Del Mar Boulevard. It turned out that the mother of Miss Inga Howard, who was Dr. Millikan's secretary, had a house on Del Mar Boulevard, north of the Institute, and she had some little cottages in the back. They were about three

or four individual rooms along a kind of an alleyway, and then a kitchen in the back. So I stayed there with three or four others. My roommate was Lynn Rumbaugh, who was also a first-year graduate student. One of the other people who lived there was Arthur [Maj.] Klein. He is now professor emeritus of aeronautics at Caltech. He was one of the occupants of these small rooms. We ate our dinners at a restaurant up on Colorado Boulevard. And our lunches we ate at the Faculty Club. This was a large two-story building, which was located where the Athenaeum is now located, facing Hill Avenue. It was painted yellow with white trim. The dining room was in the southeast corner.

PRUD'HOMME: Did all graduate students eat there at lunchtime?

NEHER: A lot of students and a lot of faculty ate there, including Richard [Chace] Tolman [professor of physical chemistry and mathematical physics; d. 1948]. He had a particular place where he would usually sit. Everybody knew that that was Tolman's table, and nobody ever dared sit there if Tolman was anywhere around, which he usually was. The same thing was true later at the Athenaeum—right at the window on the east side of the Athenaeum dining room. I believe there is still a round table there that seats eight people. And Tolman would always sit next to the window, facing the room. It was Tolman's place. He just loved to talk to graduate students and faculty—whoever was there; he was quite a conversationalist.

Tolman was an interesting person; he had a lot of feeling for people. He was much like Millikan that way. Most of his graduate students thought Tolman was just wonderful, and I was one of them. One day at the lunch table, he was telling the graduate students that he was getting lonesome. He thought maybe the thing to do was to get a dog to keep him company. Well, soon after that, he went up to Berkeley for a visit—he had come down from Berkeley when he joined the faculty at Caltech. The graduate students thought they would do him a favor. So, they found a dog that they thought he might like. They sent him a telegram saying they had a dog for him; Tolman sent back a telegram, saying, "Don't need a dog; got a wife." [Laughter]

PRUD'HOMME: How did you determine what you wanted to specialize in, in terms of research projects?

NEHER: Well, Millikan ran a class for first-year graduate students.

PRUD'HOMME: Was this the electron theory course?

NEHER: Yes, but it had to do mostly with his own work. The real purpose of the course was to learn to know the graduate students. And he regarded this as a very important function of the course as far as he was concerned. And on the basis of that, he would try to assign the students research problems. Well, I took this course from him. I don't know how he regarded me, but anyhow, in the spring—February or March—of 1929 he called me into his office and said he had a research project that he would like very much for me to get interested in and pursue. He said that Ed [Edwin] McMillan, who later went to Princeton to finish his graduate work, was working on the radioactivity of rocks, and Millikan wanted me to continue with this. Well, I told him I would let him know. So I went over to Bridge—Ed McMillan was working in the sub-basement of West Bridge—and I talked with him about it. I also talked with some other people on the faculty, including Earnest Watson [professor of physics, emeritus; d.1970]. On the basis of what I learned, I decided not to continue Ed McMillan's problem but to work on an experiment that Earnest Watson had gotten started but had not, because of the press of other matters, really carried very far. But it was something that was of current interest—namely, the scattering of high-energy electrons. So I told Millikan I would rather, if he didn't mind too much, work on this problem of Earnest Watson's. He said, well, that was all right. So then he got [another graduate student], Robley Evans, interested in it, who then pursued the problem. Later, Robley Evans went to MIT and continued this work on the radioactivity of rocks for many years.

PRUD'HOMME: Would you usually work in conjunction with another student on a research project? Or were you pretty much on your own?

NEHER: As it turned out, I was pretty much on my own. Now, there were other projects in which there were two or three involved. But in those days there were no large groups; they were all small groups.

PRUD'HOMME: What was Millikan like as a teacher? When you first walked into this course as a brand new graduate student, how did he impress you?

NEHER: Well, that is a little difficult to say. He would often get started on topics that were related, maybe, but pretty far off from the subject matter you were supposed to be covering. He would often not follow the assigned work. He expected you to read material out of books and absorb that. He would often ask questions of students in his class. There would sometimes be a discussion of various topics he was covering. Or, his discussion may not have been on anything related to what we were supposed to be covering at all. But in a way, that was very good, because it gave you a broad outlook on things which you probably would not have gotten otherwise.

PRUD'HOMME: Tell me about Earnest Watson. Did you work with him?

NEHER: Well, officially I was working with him. But as it turned out, he was so involved with other Institute affairs that I could only talk with him once in a while. I mostly just reported on what I'd done.

PRUD'HOMME: This is something that fascinates me about both Millikan and Watson—how they ever had time to do their own work, or to teach, or to run the Institute, because they were doing basically all three jobs at one time.

NEHER: That is true. And Earnest Watson looked after a lot of the detail that Millikan didn't have time for. Watson was really Millikan's right-hand man. [Edward C.] Barrett [secretary of the Executive Council] was also involved in a lot of detail—primarily the financial end of things; often decisions were dependent upon financial considerations, of course.

PRUD'HOMME: What were Millikan's research methods? How did he do his own research at this time?

NEHER: It was mostly a consultation type of thing.

PRUD'HOMME: What do you mean exactly?

NEHER: Well, he would spend a good deal of time talking with people who were doing work with him. If you had a problem that you had gotten stuck on or something, an experiment that wasn't coming out the way you thought it should, he was always available to talk about that. But, at least as far as I was concerned, he did not do the nitty-gritty in the laboratory at that time. In the early days, when he was in Chicago working with the photoelectric effect and the charge on the electron, I'm sure he was the one who did the observing. But that was not true, in general, in the years I worked with him. However, there were occasions when we would get together and compare instruments of one sort or another in order to be sure we knew what we were measuring. As a matter of fact, the basement of his house over in San Marino, about a mile from Caltech, was used as a kind of a laboratory to compare and calibrate these cosmic-ray instruments. Actually, that kept up for three or four years at least. I could come and go as I wanted.

PRUD'HOMME: He has the reputation of having an enormous ability to lead the right people in the right direction.

NEHER: Yes, he had a kind of an uncanny sense of the important problems. He would get people started doing research work on those problems that turned out to be very important. Of course, he's the one who got Carl Anderson [1936 Nobelist; Board of Trustees Professor of Physics, emeritus; d. 1991] started on his work. And Charlie [Charles C.] Lauritsen [professor of physics, emeritus; d. 1968] on his work on pulling electrons out of metals. And Ira Bowen [professor of physics 1926-1945; director, Mt. Wilson & Palomar Observatories, 1946-1964] on his work, the far-ultraviolet spectra of elements. They all turned out to be very important fields.

PRUD'HOMME: But he himself didn't actually do any research?

NEHER: He didn't do much actual observing, but he did do some planning and thinking. His main thrust was to get other people interested in physics problems and to find the right people for the right jobs.

PRUD'HOMME: The graduate students you were with when you were a graduate student—were they from all over the country or were they primarily from the West Coast?

NEHER: They came from all over, including foreign countries.

PRUD'HOMME: When did you start teaching?

NEHER: From the beginning. I went to Caltech [as a graduate student] in the fall of 1928. I had a teaching assistantship and taught freshmen my first year there. There's a rather interesting incident connected with that first year. It was during the beginning of the second term. I had a new member in the class. I had a terrible time trying to understand him; he had a cockney brogue I wasn't familiar with. But it turned out that he was one of the best students. When he would write examinations, they were nearly always perfect. It turned out to be [William H.] Pickering [professor of electrical engineering 1946-1976; director, Jet Propulsion Laboratory 1954-1976]. [Laughter] He had come up from New Zealand at the end of their [academic year] there, which coincided with our Christmas, and took a special examination that Earnest Watson made up for him. They admitted him, and he came into my class.

PRUD'HOMME: Were there any other students you remember who were special in various ways?

NEHER: Oh, yes. It's nice to keep track of students as time goes on—and I do, to a certain extent. One of my students was [Neil] Gehrels. For an examination, I made it a habit of writing the questions on the board at the beginning of the hour. Then I would leave the room. I would then come back in a half an hour or so to see if there were any questions. Gehrels used to sit on the front row with his legs crossed, and he wouldn't write a thing until he'd decided just how he was going to work all those problems. And then, during the last half hour or so, he would write it all down, and nearly always it would be correct. That was always amazing to me.

PRUD'HOMME: During this time you were teaching, were you also working on the electroscope techniques? When did you really start working very closely with Millikan?

NEHER: Not until after I got my degree. You see, they used to have a system by which, if you took an examination and did well in it, then you wouldn't have to take that course. There were certain required graduate courses. So when I came to Caltech, Bowen, who was teaching optics at that time, gave me this examination. I did fairly well in it, so I didn't have to take the required course in optics. I had done some work in electricity and magnetism, so that course was somewhat easier than it might have been otherwise under Smythe. And my research went along pretty well. I was able to get through with only three years of graduate work. It usually takes somewhat longer.

Then I asked if I could stay on to work on the expansion of my thesis problem. I was granted permission to do this with a certain stipend. In February of 1932, after I'd been working on this project for a few months, Millikan and Bowen came into my office one day and asked me if I wanted to work with them on cosmic rays. So that's the way I got started.

PRUD'HOMME: When they talk about the cosmic-ray group, what do they mean?

NEHER: Well, at that time, the cosmic-ray group consisted of Anderson—and I've forgotten when [Seth] Neddermeyer [grad. student, research fellow, 1930-1941] came in, but it must have been either that year or the next. The three of us, I guess, were the cosmic-ray group, including Millikan.

PRUD'HOMME: And Millikan would direct all of your studies?

NEHER: Well, we would talk a lot, yes, with him. As I say, he was not engaged in the actual observation, except that we did this work in the basement of his house in comparing instruments; that continued for three or four years, or longer.

PRUD'HOMME: But you published your findings together?

NEHER: Yes, that's right.

PRUD'HOMME: Do you want to describe the Millikan-Compton controversy—the famous brouhaha?[1]

NEHER: Well, that's kind of a hard thing to discuss. I always had the feeling that the thing was blown up all out of proportion to what it would have been otherwise by the newspapers. Both Millikan and [Arthur H.] Compton were rather prominent individuals. They were both Nobel Prize winners. Millikan knew Compton and knew him quite well. Millikan came from Chicago, and that's where Compton was—and I'm sure he had a high regard for Compton. Millikan regarded Compton's discovery of the so-called Compton effect as a very important contribution to physics; I'm sure he regarded Compton very highly. But these newspapers—I got kind of fed up with it. They were out to get whatever they could.

PRUD'HOMME: It's as though they wanted to prove that scientists were fallible.

NEHER: I don't know what it was. Behind it was the motive to sell newspapers, of course. And the more you can get two people like that to be in conflict with each other, the more they like it.

PRUD'HOMME: [Daniel J.] Kevles [Koepfli Professor of the Humanities, emeritus] suggests that perhaps the economic troubles of the country led people to want to get back at scientists, that people felt that maybe scientists had led the country into overproduction and that this is why the Depression was coming on. It seems a little far-fetched to me.

[1] A debate during the 1930s about the nature of cosmic rays: Compton held that they consisted of charged particles and Millikan argued that they were made of high-energy photons. Eventual observations of the deflection of cosmic rays by Earth's magnetic field proved Compton right.—ed.

NEHER: Well, I never had that feeling about it. In a way, it was a very sad situation. And I don't think either Compton or Millikan liked the way things went. They were just caught up in it.

PRUD'HOMME: You were traveling in this period. Can you tell me about some of your first trips?

NEHER: My first major trip was with Millikan and Bowen. We went to Canada toward the end of September 1932 to measure cosmic rays using airplanes. As a matter of fact, not only did we take instruments along to fly in airplanes but we had also made other instruments to fly with balloons. You see, Bowen and Millikan had made a balloon flight in Texas in 1922. The data from this flight were not sufficiently precise to determine the behavior of cosmic rays at these balloon altitudes. They wanted some more data on high-altitude cosmic rays. So we made up three or four instruments, and these were the ones that Bowen was particularly interested in. After we got through with the airplane flights up in northern Canada, Bowen and Millikan stopped off at a weather station in South Dakota to send up these instruments with balloons. Then I came on south to Colorado and went up on Pike's Peak. I was up there for a week before Millikan came. In the meantime, he and Bowen had made these flights from South Dakota.

PRUD'HOMME: Where did you get the airplane?

NEHER: Well, in Canada, we used a Royal Air Force plane that had just come back from a survey trip to the Big Bear Lake area. The pilot brought back some radioactive rocks. I still have one of those rocks, which is apparently about 50-percent pure uraninite. Millikan had made arrangements with the Canadian government to use this airplane. As I recall, we made three flights up there. And they got up to 21,000 feet, without oxygen. On the first flight, the pilot came down and didn't know where he was. The plane had pontoons and he landed on a lake that happened to have a cache of gasoline; otherwise he wouldn't have gotten back very soon to the place he left. But the next two flights went all right. These pilots were taking a bit of a chance to go that high without oxygen. I had been up in a plane at March Field in Southern California—we made flights before we left

for Canada, and just for the fun of it, I went along, although our instruments were self-recording. I was in the bomb bay of this Curtis Condor airplane. We went to 21,000 or 22,000 feet. I took no oxygen for a while. I had it available, but I wanted to see how long I could stay up there with no ill effects.

PRUD'HOMME: What does it feel like?

NEHER: You get sleepy. And I'm sure if I'd stayed up there without oxygen very much longer, I would have just gone to sleep.

PRUD'HOMME: I would love to get your description of some of your colleagues. For example, Ira Bowen.

NEHER: Well, I had a very high regard for Ira Bowen. I've always said that he could look a problem in the eye and call it by its right name better than anybody I ever knew. He was a very clear thinker and extraordinary, I think, in that he could wipe away the fuzz and see clearly what was involved.

PRUD'HOMME: And Neddermeyer?

NEHER: Neddermeyer I never knew terribly well. Carl Anderson, I think, regarded him quite highly, but I never worked directly with him. They were interested in this cloud chamber. I had, of course, more contact with Carl Anderson than I did with Seth Neddermeyer.

Begin Tape 1, Side 2

PRUD'HOMME: Tell me about Tolman.

NEHER: Well, he had a joint appointment in physics and in chemistry. He taught a course in relativity—both the special theory and the general theory. My last year as a graduate student, I took both his one-term course on the special theory and one or two terms on the

general theory. I thought I understood a little of some of the ideas of the special theory. But I never did feel comfortable about the general theory. The ideas are just too esoteric. I never arrived at the point where I thought I really understood it. Tolman had a special gesture. He would stand up before his class and his pants were usually about to fall off, and he would hoist them up. [Laughter] All of his graduate students knew and waited for this gesture of his, you see, of pulling up his pants.

But he had some very, very good advice to give his students. One of his sayings was that when you begin to feel comfortable about a subject, then you're beginning to understand it. And, as I say, I never did feel very comfortable about the general theory of relativity.

The time came for my thesis examination. In the days when I was a graduate student, there was just one examination. It usually came in the spring of the year in which you were getting your degree, and you were supposed to have finished your thesis. To help graduate students bone up on their subject matter for the examination, there were several very important books. It was all started by Ira Bowen. You see, he came from Chicago with Millikan in 1921 or '22. These books were something like a ledger, with 100 or 200 pages—I don't remember just how many. But after you got through with your examination, each graduate student wrote down in this book the questions that were asked by individual members of his examining committee.

PRUD'HOMME: So you knew what man was going to ask you what?

NEHER: You knew something about the kind of topic that you might be asked about. [Paul Sophus] Epstein [professor of theoretical physics, emeritus; d.1966] would ask certain things; Tolman would ask certain things; [Fritz] Zwicky [professor of astrophysics, emeritus; d. 1974] would ask certain things; and so on. Well, you didn't know specifically, of course, what was going to be asked. But at least it helped a lot in your studying and in your preparation. Well, this examination started at nine o'clock in the morning and was supposed to be through at twelve. It was a three-hour oral exam. I've forgotten who came in first for my examination. But usually you have a committee of maybe five or six examiners, and they would come in and be there for a while. Usually

there were only two or three present at any one time. When an examiner was through with you, he might leave and come back at the end of the three-hour period to get together with the others to decide what they were going to do about you.

The examining board consisted of members of your major and of your minor—usually only one member of the minor. My minor was mathematics, and my major was physics. For my minor, I had Eric Temple Bell [professor of mathematics, emeritus; d.1960] on my committee. He came in—I've forgotten—around ten o'clock or ten-thirty in the morning, something of this sort. And he began covering topics that I wasn't very familiar with. At the beginning I was all right. But then later on, he began covering some very esoteric type of mathematics that I had heard about but didn't know much about. Toward the end of this, Tolman came in. And Tolman saw the way things were going. When Bell was finished, Tolman then took over. He started asking me some very simple things.

PRUD'HOMME: A nice man.

NEHER: [Laughter] It just quieted things down immensely. And by the end, he was covering topics in relativity that I'm sure, under other conditions, I might not have been able to do much about. This is a very important thing, I think, for people to be able to do—this personal contact. Both Millikan and Tolman had it.

PRUD'HOMME: Did you enjoy teaching?

NEHER: Yes, very much. Some of my former students are on the Caltech faculty now. If I count correctly, there are nine present faculty members who were in my classes.

PRUD'HOMME: Caltech seems to keep its own, you know. People go there and then they stay. I think that's wonderful.

NEHER: Well, there's been a lot of criticism about this, of course. Some members of the faculty have argued against this practice in the past. But the way it has turned out is that

most of these people who were former Caltech graduate students have been away and have proved themselves. Then they have come back later.

PRUD'HOMME: It doesn't give the impression of being an insular or isolated institution. So much of the work is being done all over the world, after all, and in conjunction with others.

NEHER: That's right. Now, Howard Berg [professor of biology, 1979-1986], for instance—he got his doctor's degree from Harvard. And Kip Thorne [Feynman Professor of Theoretical Physics] was at Princeton.

PRUD'HOMME: I want to go back to Pike's Peak. You said you went up there for a week. Did you set up experiments there?

NEHER: Yes.

PRUD'HOMME: What was it like up there?

NEHER: Well, at that time it was—and it still is, I suppose—a tourist place for people who drive up there in the summertime. I was up there after most of the tourist season was over. I think it was the first week in October—it was getting cold. I set up this equipment that I had, which I had taken to Canada, in one of the buildings there. It was a crude structure, built out of rocks. The top of Pike's Peak is, of course, all rock. This was a rather long one-story building. I set up in one of the rooms there, and I slept there. I ate my meals at the lunch counter in the small store.

Well, I was bothered for three days with a headache, because of the altitude, but then I was all right after that. When Millikan came up at the end of the week, the altitude didn't bother him a bit.

PRUD'HOMME: He wasn't a young man, either, at that time.

Robert A. Millikan at Pikes Peak with Neher's cosmic-ray electroscope. Millikan stacked up some furniture to gain a little additional height for the electroscope.

NEHER: Oh, no. Let's see, this was '32. He was born in 1868. What's that, sixty-four? No, he was not a young man .

During the first night, I didn't sleep very much, and I heard noises all around. I couldn't figure out what was going on. I had some dates in my suitcase and I had left it open. In the morning, most of the dates were gone. The thieves turned out to be mountain rats. These are bushy-tailed rats, not the smooth-tail variety.

PRUD'HOMME: They're the ones that look like squirrels.

NEHER: Yes. So I went down to the place where I ate my meals and talked with the cook about the rat situation. He said that these rats were a plague up there. And as a matter of fact, there's a marker up there which has written on it, "Erected to the memory of so-and-so, eaten by mountain rats in the year …"—I've forgotten what the year was. Well, he gave me some traps. And I set these traps. I think there were three traps, and I got a rat in each one of them. So I caught rats up there for the next few days. [Laughter] Well, these buildings had just a dirt floor. I think they were used as storage sheds, and there were all kinds of holes around.

PRUD'HOMME: You were working with Millikan. He was up there then?

NEHER: I was up there alone at the beginning and he came up later.

PRUD'HOMME: What was he like to travel with, to be with as a traveling companion?

NEHER: Well, he was very good to travel with.

PRUD'HOMME: Because you were a very young man and he was a very old and distinguished scholar, were you scared of him?

NEHER: I suppose there was something of that sort to begin with. But after a while I got to know him. I always had a high regard for him. He was quite a talkative person and talked a great deal when we were on trips.

PRUD'HOMME: He had a wide range of interests.

NEHER: Yes. And he had a wide acquaintance of people. But he'd always take books along or articles or pamphlets or something or other to read. And he would write. He would cross his legs and write on a pad. And even on a moving train, he could write pretty well—much better than one can on a desk type of thing.

PRUD'HOMME: Where did you get all the money for these trips? After all, this is the Depression time.

NEHER: I know. For a number of years, the three of us—this cosmic-ray group—got by on $15,000 a year. This was a yearly grant from the Carnegie Institution of Washington and kept up throughout the thirties.

PRUD'HOMME: But you went all over the world?

NEHER: I know it. Well, I always suspected that Millikan had some other funds. But he never talked about it, so I may be wrong.

PRUD'HOMME: Did you have to pinch pennies in terms of your equipment and travel arrangements? I mean, did you have that sense of always being worried about money?

NEHER: Well, as a matter of fact, in those days the big expense came in the stipends or the salaries of the workers. There was an expense, of course, connected with making equipment. But most of this equipment was made in our shops at Caltech. Carl Anderson's cloud chamber was made in the shops of Caltech. All of the equipment that I had made was made in the shops. At that stage of the game, you couldn't buy equipment that you wanted to use for research, usually. What you did was to make it. That is one place where research has changed over the years. We used to think in terms of things you could make. Now people think in terms of things they can buy.

PRUD'HOMME: I also think now, too, that the people don't start out in research unless they think they can get the money to support it.

NEHER: That's right. And to a large extent it's buying equipment or having very complicated equipment made. Some of this physics—well, especially this high-energy kind—that's being done these days is really expensive. Huge things that cost millions of dollars. And it isn't just one person who works on those things; it's maybe a dozen, maybe headed by a faculty member, but all kinds of graduate students working together on these projects.

PRUD'HOMME: Which of your trips stands out in your mind?

NEHER: I suppose the trip to India in 1939-1940 was the most extensive and most memorable.

PRUD'HOMME: In what way?

NEHER: Well, contact with the people involved, including Mrs. Millikan, who was along.

PRUD'HOMME: What was she like? Her life must have been rather difficult, because he was away a good deal of the time, and he was heavily absorbed in the Institute.

NEHER: That's right. She traveled a lot with him in Europe. For instance, when he made these trips to Europe, she was usually with him. I remember her telling us of being in Paris. They had three boys; Clark was the oldest, and then Glenn was the middle one, and Max was the youngest one. On one of these trips to Europe, when they were in Paris, the boys wanted to go down and go through the tunnels where the sewers go. Mrs. Millikan thought that that was just not the proper thing to do, I guess. So she talked the boys into going around to the Louvre with her. And she said, well, if they went through the Louvre with her and spent so many hours, that she would then spend so much time with them going through the sewers of Paris. [Laughter]

But she was interested in the students. The Millikans used to entertain not only the graduate students but all the freshmen students, at their home. They'd divide them up into groups, you see. She would make it a point to learn their names as much as possible, which was quite a task, actually. And many of those boys she never saw again.

During the Depression, Mrs. Millikan thought she ought to do something for the people who didn't have too much. Through some agency, she got a woman to come out to clean her house. And the woman drove up in a car that was a much finer car than the old Chevy that Mrs. Millikan was driving. [Laughter] That didn't go down very well.

PRUD'HOMME: Were there academic jealousies between any of you in this cosmic-ray group?

NEHER: I don't think so.

PRUD'HOMME: Or within the physics department?

NEHER: Well, I think the feeling that most of us had in those years, back in the thirties, was that it was just a happy family. There was no back-biting of any sort that I ever knew about.

PRUD'HOMME: It seems there was a great deal of sharing of information, too. I mean, you didn't keep your findings close to your chest.

NEHER: We all knew what was going on. The Institute was small enough so you had friends in the biology department, the humanities department, and engineering department, and all over. It was one big happy family.

H. VICTOR NEHER

SESSION 2

April 28, 1982

Tape 1, Side 2 (continued)

PRUD'HOMME: Tell me about the "100-to-1 Shot Club." What was that?

NEHER: This was a small group that was formed at Caltech in about 1934 or '35. The way it originated was this. John Anderson, who was at the Mount Wilson Observatory, had an office at Caltech when he was working on the 200-inch telescope, back in the thirties. One of the members of the staff was a young fellow by the name of John Strong [professor of physics and astrophysics, 1937-1942], who had his experimental equipment in the same room in Bridge as I did. John Strong was over talking to John Anderson one day. John Strong was always interested in ideas of one sort or another. He was an inventor if there ever was one. John thought that there ought to be a group that considered far-out ideas of one sort or another.

PRUD'HOMME: For example.

NEHER: Primarily ideas connected with something scientific or mechanical, or something of that sort. And John Anderson said, "Well, what you are suggesting is to discuss things that have one chance in a hundred of working." And so, this is the way the 100-to-1 Shot Club was formed. They got a group together which consisted of John Strong, John Anderson, Russell Porter, Roger Hayward—who did that picture up there above the fireplace—and then some others not connected with the Institute, like Byron Graves. And there were a couple of patent attorneys in the group.

Well, I didn't get into it right away. I guess it was about 1936 or '37 before I became associated with it. We met once a month at various members' homes. It was mostly discussions of ideas in connection with astronomy or with physics. There may have been some mechanical things. One of the members was George Mitchell, who

designed and made the Mitchell camera that was used in Hollywood for years. Another was George Beadle [professor of biology 1946-1961], who joined after World War II.

PRUD'HOMME: Did anything ever come out of it?

NEHER: No. It wasn't meant to be that. It was just a place where you could just discuss anything you wanted. What I was going to say about Russell Porter was that he was a very versatile individual. He made the pencil sketches of the 200-inch telescope that still hang in Robinson [Laboratory of Astrophysics], I suppose. Some of his sketches were very detailed in nature. He made the sketches, for instance, of the control box that controls the telescope and the dome there at Palomar. It consisted of a lot of gears and various things in a console that was four or five feet long, several feet deep, and three feet high. The people who made up the blueprints took Russell Porter's drawings and made up the blueprints from his drawings. These drawings were three-dimensional views, cutaways of, for instance, this box of gears. And when the people who made the blueprints worked this all out in detail, they found everything as it should be. No gears interfered with each other. Everything worked according to the way Russell Porter said it ought to. He had a way of seeing things in three dimensions that very few people have.

In his younger days, he spent time in the Arctic with both Perry and Cook. He had paintings that he had made in the Arctic—mostly watercolors. But they were very good—of polar bears and terns or igloos and whatnot. Not only was he good in architecture, art, and painting but he was also a musician. He would compose music. He enjoyed taking the scores of Beethoven to bed with him and reading them. He would get pleasure out of just reading the scores. The amazing thing was that he could apparently hear this music in his mind as he read the scores.

PRUD'HOMME: I'd like to go back to the Depression. How did Caltech cope with the sudden lack of funds caused by the Depression? Did the salaries get paid? And could you get the equipment that you wanted to and travel the way you wanted to?

NEHER: I don't know all the details, of course, of how Caltech really coped. My salary in my first year—after getting my degree—was $1,200 a year. Sara, my wife, was still

teaching. I think she was getting $1,600 a year. So between us, we managed all right. But I'm afraid I don't know the details—except Millikan used to tell me that he and Barrett would have to do a lot of borrowing here and there and other places in order to meet the obligations.

PRUD'HOMME: I'm sure the value of their portfolio went down.

NEHER: Oh, yes. The Institute had a lot of stock on which they lost a lot of money.

PRUD'HOMME: And at that point, I know the federal government scientific agencies were slashed by quite a percentage.

NEHER: But Robinson was built during the thirties; the geology buildings were built during the thirties.

PRUD'HOMME: And Millikan would go out and find this money?

NEHER: He certainly would. When he came to Caltech, he thought he would not have to do this sort of thing so much. But when he really got into it, he said he just found that he had to.

PRUD'HOMME: And he could do it still, even during the Depression—that's an extraordinary compliment.

Begin Tape 2, Side 1

PRUD'HOMME: When the Second World War came along, what was the effect of the war on Caltech?

NEHER: I was recruited early in the game. Ernest Lawrence from Berkeley, whom I had known since 1931, was on a recruiting expedition to get people to go back to the Radiation Laboratory at MIT, where work had started on radar. So Ernest Lawrence

came down to Caltech and persuaded me to go back to MIT. He said he thought that the way it would work out was that I would be clued in to what some of the problems were. He thought this might take three or four weeks; then I would bring a certain problem back to Caltech and work on it. As it turned out, instead of staying three or four weeks, I stayed five years. [Laughter]

PRUD'HOMME: Always on leave of absence?

NEHER: Always on leave of absence, yes.

PRUD'HOMME: You worked on construction of apparatus for microwaves.

NEHER: I was interested in the receiving end of the microwave equipment. I worked on amplifier tubes as well as what were called local oscillators in the receivers.

PRUD'HOMME: Did you bring other people from Caltech with you?

NEHER: Guy [H. Guyford] Stever, who got his degree in 1941, came right after that and was there for about a year before he went over to England. He lived with us, actually, while he was at the Radiation Laboratory. Later on, some others came from Caltech, but not very many. Most of them were in Pasadena, working on the rocket project, which was the big thing run by Willy [William A.] Fowler [Institute Professor of Physics, emeritus; d. 1995] and Charlie Lauritsen.

PRUD'HOMME: Were you tempted to stay at MIT after the war?

NEHER: Well, I didn't have an offer from MIT. I had offers from three or four other places. [Laughter]

PRUD'HOMME: But you went right back to Caltech.

NEHER: I went back to Caltech. As a matter of fact, GE at Schenectady—especially Dr. [Albert] Hull, who was in charge of their research laboratory—put a lot of pressure on me to go to GE. I could have gone at about twice the salary that I was getting at Caltech. And Bell Labs wanted me. I also could have gone to RCA.

PRUD'HOMME: What is it that brought you back to Caltech?

NEHER: Well, I don't know. I felt I would be happier there and I would be able to do the things I wanted to do.

PRUD'HOMME: But how is that freedom created? Because it seems that all the people I've talked to have talked about the liberty one has to do the project that one is interested in. There's a tremendous personal freedom.

NEHER: Well, to many of us, that is much more important than the financial end of things. We'd much rather be doing what we would like to do than have a fine home, for instance, or a swimming pool or what not.

PRUD'HOMME: Who was running Caltech during the war? Because Millikan was very old at that time.

NEHER: That's right. But he had been asked to stay on by the trustees even though he was beyond retirement age. He stayed on something like twelve years beyond retirement age.

PRUD'HOMME: How did he reconcile his dislike of government funding with the enormous military research that was being conducted?

NEHER: He thought this was a necessary thing, I think, while the war was going on. But he was hoping that things would return more or less to normal. That is, he was looking back, I think, to the way things were before the war started and hoping things would return to that kind of activity. He was interested in the nuclear bombs developed during

the war. But he thought that the world supply of uranium was not sufficient to be of any big use for electrical generating plants, for instance. Of course, he never lived to see the day when this really took place—this huge development in nuclear energy.

PRUD'HOMME: So, when you came back to the Institute, it was really quite changed.

NEHER: It was changed, yes. And especially when [Lee A.] DuBridge [Caltech president, 1946-1969] came, things changed rather fast. Of course it changed a lot, as far as I was concerned, because Millikan was in charge of things in the thirties when I last was associated with him. Then after the war, he kind of dropped out, you see. So I felt a little bit lost at the beginning.

PRUD'HOMME: It must have been very difficult for Millikan to let go of the reins, I would think—to give it up.

NEHER: Oh, I'm sure it was.

PRUD'HOMME: Did he continue his research after that? I know he had an office.

NEHER: Well, he had an office over in Bridge. He began writing his autobiography soon after the war. That occupied him pretty much for quite a few years.

PRUD'HOMME: What was the reaction to the selection of Lee DuBridge and his arrival on the campus?

NEHER: I had known him, of course, quite well before. He was head of the Radiation Laboratory at MIT, where I was. And other people knew him, because Lee DuBridge had been a National Research Fellow at Caltech, 1926 through 1928. He had quite a reputation for running a big enterprise, and a very successful enterprise, there at MIT. As far as I remember, the faculty at Caltech was very much behind Lee DuBridge right from the start.

PRUD'HOMME: Did you feel that you could go to him and give him recommendations? If you wanted things changed in the physics department, could you discuss things openly with him?

NEHER: Well, I didn't deal very much with Lee DuBridge in those years. I worked through Earnest Watson, or through Bob [Robert F.] Bacher [chair of the Division of Physics, Mathematics, and Astronomy, 1949-1962], who came in soon after Lee DuBridge did. He was charming, and he was a very fine person to work with.

PRUD'HOMME: Was DuBridge's role primarily as a fund-raiser?

NEHER: I wouldn't say primarily. But that was one of the big things he did. He got a group together specifically to raise funds, and he was very successful at it. This, of course, meant a big expansion of the Institute.

PRUD'HOMME: Did you find that the students had changed, from prewar to postwar?

NEHER: Well, of course, right after the war we had a good many GIs come back. And most of these fellows were very good.

PRUD'HOMME: They had to pass the same entrance requirements?

NEHER: Oh, yes. They knew what they wanted to do. They were older. And in general, they did very well. It was a very good time to be in contact with the graduate students, as well as with the undergraduates.

PRUD'HOMME: Were you still teaching undergraduates?

NEHER: Yes. I guess I was always involved with the undergraduates. Back in the thirties, I was teaching and running some of the laboratories but primarily involved with the freshmen and sophomores. After the war, I was involved primarily with the sophomores

for many years. I did some development of the laboratories, both the sophomore and freshman laboratories.

PRUD'HOMME: Did students usually know what they were going to major in, what they were going to specialize in, by their sophomore year?

NEHER: Yes, most of them did. Of course, they all took the same math and the same physics the first two years, no matter what field they were going into.

PRUD'HOMME: I found that the title of Division of Physics, Astrophysics, Mathematics, and Electrical Engineering—it seems like a very large administrative bundle to handle.

NEHER: Well, of course, electrical engineering broke away from that division and became part of the engineering division. So it's now physics, mathematics, and astronomy, and it is a large division.

PRUD'HOMME: Does this give the graduate students flexibility to work in several fields at once and to have projects that cross the lines of one specialty?

NEHER: I'm not sure just how much crossover there is—or was at that time. But I think it's quite natural that mathematics, physics, and astrophysics would be all together. As a matter of fact, many of the physicists go to the astrophysics seminar.

One of the changes that took place after the war as far as physics was concerned, was that it outgrew Bridge Laboratory and expanded to other buildings. This, I think, was a little unfortunate, because people, for instance, in high-energy physics don't associate much with people from Bridge Laboratory. And the distance is quite an important element, I think. It has resulted, I think, in a kind of a loss. We used to all go to the physics seminar. I don't know how many people go to the general physics seminar in Bridge now.

PRUD'HOMME: What is the general physics seminar?

NEHER: During the twenties and thirties, it was held in 201 East Bridge on Tuesday and Thursday afternoons. After World War II, it was held only on Thursdays. There's a tea beforehand, and then there's an hour talk by someone—either a visiting person, or maybe a graduate student or some member of the faculty on some of his research work. For the most part, it was held to a level such that people who weren't in that particular field would get something out of it. What has happened is that various other seminars have come into existence—there's a seminar in high-energy physics; there's a seminar in nuclear physics. There used to be only one, you see.

PRUD'HOMME: Tell me about your work when Millikan worked with you. He came on some of your trips, didn't he, when you were working with cosmic-ray particle changes. In the summer of '47, you began sending up a new series of balloon flights, so you were starting all over again on your travels around. Who helped you with these trips?

NEHER: Well, in 1947 there were four graduate students who were helping out on different phases of these projects. I think the trip we took in 1947 was from San Antonio, Texas, on north to Saskatoon, up in Canada. Millikan insisted on going along on this trip. We sent up balloons with Geiger counters that were arranged in the form of a telescope to study the radiation coming in more or less at the vertical—studying it as a function of latitude. One of the difficulties, which turned up in that series of flights, was that there were apparently changes in the radiation itself coming on to the Earth which were superimposed on the latitude effect we were trying to study. So it wasn't possible to get out of those flights the latitude effect that we hoped we could. That suggested that we try and tie things together somehow. So the next year, 1948, we worked through the Office of Naval Research and arranged to do some experiments using a B-29 aircraft.

PRUD'HOMME: I don't see how you managed to get it all there, get it calibrated for the different temperatures, and not have it break down. [Laughter]

NEHER: We had six different pieces of equipment in that B-29 which were doing different things. We flew from Inyokern to [the University of] Chicago, picked up an experiment that John Simpson wanted to do, and one of his students went along. I had a

graduate student with me—Art [Arthur T.] Biehl. We flew up to Hudson Bay at 30,000 feet, turned around, and landed at Tampa, Florida, all flying at 30,000 feet. We spent a day at Tampa, then we flew back to the latitude at which we started to let down coming to Tampa, so that the two flights would connect on to each other, you see. Then we flew to Panama, following a given geomagnetic longitude. And this took us over Panama. From Panama, we went back up and hooked on to where we had let down over the Caribbean on the way down to Panama, and then flew south to Peru and landed at Lima. We were in Lima the rest of the day and took off and flew a hexagonal course at 36,000 feet above Peru to get what is called the east-west effect. We would fly on one leg of an octagonal course, then we would turn and fly another leg of that octagonal course, and we continued on around, you see. So we knew our heading all the time. We had this equipment, which was pointed out at 45 degrees, sweeping out the sky and counting these cosmic-ray particles that were coming in.

PRUD'HOMME: And the navy provided you with the planes for all of this?

NEHER: Yes. That was a wonderful trip, not only from the scientific but from the scenic point of view. When we were flying over the Andes, the Andes just looked like hills. [Laughter]

PRUD'HOMME: Yes, they'd have to, from 36,000 feet.

NEHER: We'd fly over the Andes and then out over the ocean and back over the Andes.

PRUD'HOMME: The contrast between Canada and the Caribbean must have been enormous. And you got what you wanted! Is there anything else you'd like to discuss about your work at that point? You worked with Pickering?

NEHER: Yes, I worked with Pickering during part of the time. Pickering went on this 1947 trip; he was there with us for a while but didn't stay. He had some other things he had to do. He, of course, was on the India trip.

PRUD'HOMME: Now, at this point did you have graduate students working with you, the way you used to work with Millikan?

NEHER: Oh, yes. I usually had, during those periods—those forties and fifties—two, three, or four graduate students working with me. One of the last was Hugh [Riddell] Anderson. When I retired from Caltech, I turned all of my equipment over to Hugh Anderson. He had worked at JPL also and helped out on these satellite and space probes that we had equipment on.

PRUD'HOMME: Your publications at that time were done by A. T. Biehl and [W. C.] Roesch, and you?

NEHER: And another student who was working with me at the time, [R. A.] Montgomery, who was not in on this publication. He published a paper later on the absolute intensity of cosmic rays.

PRUD'HOMME: You did an enormous amount of publication at that particular point. Were you involved in the Oppenheimer case?

NEHER: There's a story connected with Frank Oppenheimer that might be interesting. Frank Oppenheimer is now director of what is called the Exploratorium in San Francisco. It is a very successful type of thing. He has a tremendous ability along that line, of conceiving demonstration equipment that appeals to the public. But there's a story about Frank Oppenheimer that may not be very well known. Should I tell you?

PRUD'HOMME: Yes.

NEHER: All right. During the thirties, both Robert Oppenheimer and Frank Oppenheimer were disillusioned as far as our government was concerned, I think, and as far as the economic system was concerned. And they were exploring other possibilities. Both of them had friends who were of a different persuasion than our government, and you probably know some of the story involved in that. When the war broke out, their interests

were concerned with other things. Of course, Robert Oppenheimer went to Los Alamos, and Frank Oppenheimer worked with that same project.

After the war, during the McCarthy era, both of them were called before congressional committees. Frank Oppenheimer, after the war, had gone to the University of Minnesota as a member of the physics department. During these inquiries by the House Un-American Activities Committee, Frank Oppenheimer was called to Washington. He was, I suppose you would say, a little less candid than he should have been about his activities in the late thirties. So the committee requested that the University of Minnesota dismiss him, let him go. Well, you know what the pressures were in those days, I guess. So they did. They asked him if he would leave. Well, he took this very hard. And he disappeared, as far as his colleagues were concerned. I had heard rumors of what had happened, but I didn't have anything definite. He left the University of Minnesota in 1951 or '52, sometime in there.

In about 1957 or '58, the University of Colorado at Boulder began getting some freshmen who were better prepared in math and physics than any of the others. The first year or two, they didn't pay much attention to it. But then someone discovered that these kids were coming from the same high school, down in southern Colorado. They got curious enough to send somebody down there to see what was going on. And they found Frank Oppenheimer. He was working on a ranch down there, but he was spending his spare time and his evenings working with these kids in high school. Then they talked him into coming to Boulder. And he was there for several years, and put up a series of demonstration experiments on the top floor of the physics building, where people could come up—primarily students—and work with this apparatus and just play around with it. And it's an expansion of that idea, you see, that's taking place now at the Exploratorium in San Francisco.

PRUD'HOMME: So maybe it was all for the best.

NEHER: Maybe.

PRUD'HOMME: Did you know Robert Oppenheimer well?

NEHER: I knew him fairly well, yes.

PRUD'HOMME: What sort of person was he?

NEHER: Well, I had a difficult time trying to understand him.

PRUD'HOMME: In what sense?

NEHER: In the sense that you would go to his lectures, and he would walk back and forth in front of the blackboard. And then at one end of his journey, he would write something on the board which was so small you could hardly see it. Then he would continue to talk about this thing that he'd written on the board. In the first place, he talked with a very low voice, so you had difficulty understanding him. And in the second place, I was no theorist, and it was very difficult for me to follow what he was saying.

Carl Anderson has a good story about this—I think it must appear somewhere in his oral history. Carl's story is this. He registered for one of Robert Oppenheimer's classes, on quantum mechanics, that he was teaching at Caltech. Most of the people who were attending the class had not registered for the course; they were in there just as visitors or to get out what they could without any credit. Well, after the course was about half finished, Carl went to Robert Oppenheimer and said that he would like to drop the course, because he just wasn't getting as much out of it as he thought he should. And Robert Oppenheimer said, "You can't do that; you're the only one who's registered." [Laughter]

PRUD'HOMME: So he was not that popular with the students.

NEHER: Later on, he improved. And toward the end of the thirties, he was doing much better than he was when he first started giving lectures at Caltech. He was spending half of his time at Caltech and half of his time at Berkeley. And he would bring from Berkeley to Caltech a whole carload of his graduate students. And these graduate students would stay with him. Those graduate students thought he was just wonderful.

PRUD'HOMME: When he was denied the security clearance, what was the reaction on campus?

NEHER: Well, I think the feeling was very strong in upholding the point of view of Lee DuBridge. I think most scientists, most physicists, thought that those hearings just were not what they should have been. They thought that justice had not been done. It had a tremendous effect on Oppenheimer himself. I don't think he was the same person after this.

PRUD'HOMME: It's as though your support systems have been kicked out from under you.

NEHER: Well, this was a terrible era.

PRUD'HOMME: Did this, in any way, make one frightened to do research on government projects?

NEHER: Well, I think this was one reason that Millikan did not like government support. He thought that sooner or later the government would be dictating what you were doing. He did not like this sort of thing at all. I think his dislike was primarily because of this—of the government getting in and dictating what research people should do through their funding.

PRUD'HOMME: And, of course, he was proved true in the mid-fifties.

NEHER: That's right. It was a terrible time. Robert Oppenheimer gave a talk at Caltech in the late fifties or early sixties. It was a wonderful talk. He was a poet at heart. He had feelings that didn't come out too often, but when they did, they really were wonderful.

PRUD'HOMME: What did he say in the talk?

NEHER: He tried to give some of his feelings of what he had been thinking through the years. He was a complex individual, there's no question about that. It largely had to do

with his attitudes through the years—from the early years through Los Alamos and on to after the war. He wasn't very specific about anything, as I remember. He never did mention, as far as I know, this trouble he had in Washington—in public like that. But you could easily see that this was in the back of his mind as he was talking. Let's see, later he was given the Enrico Fermi Award. This helped out some.

PRUD'HOMME: But it never made up.

NEHER: It never made up for what happened before.

PRUD'HOMME: I did want to ask you about the meeting in Rome in 1962 of cosmic-ray experts. What was that, and who sponsored that?

NEHER: There was a member of the Pontifical Academy of Sciences by the name of [Manuel S.] Vallarta. He was at one time the head of the physics department at the University of Mexico. He had worked on cosmic rays at MIT with their computer in the late thirties—this computer, in contrast to present computers, filled a room at least as large as this living room. He used that computer to solve the problem of charged particles coming in through the Earth's magnetic field. He later became director of [the National Polytechnic Institute in Mexico]. He still retained his interests in cosmic rays. I had gotten to know him quite well back in the thirties and forties, particularly after the Second World War.

Vallarta was asked by the Pontifical Academy to get together a group of people, from around the world who would go to Rome—to the Vatican—and spend a week discussing what they knew about cosmic rays. They were to try to put everything together. So he did that. He sent me an invitation. The Academy said they would pay our first-class fare to Rome, but if we wanted to change it to a tourist class, they would be agreeable, and in that case we could bring our wives along. [Laughter] So that's what most of us did.

Those were all people I knew, of course, so we had quite a time there. We sat around a table in a building right in back of the Vatican, out in the gardens. In a rather intimate fashion, we just tried to see what we could make out of what we'd been doing.

There were lot of questions and answers back and forth. It was a very nice occasion. We were all relaxed, and we all knew each other.

After we were through discussing cosmic rays, we had an audience with the Pope, and that was a very nice occasion.

PRUD'HOMME: This was John.

NEHER: Yes, Pope John XXIII. He talked—I think it was in English—and it was very well done. We all shook his hand.

PRUD'HOMME: All of you specialists on the heavens. Do you have any other recollections about Institute staff, and students, and other personnel?

NEHER: I think somebody ought to talk some time with Maj Klein [professor of aeronautics, emeritus; d. 1983]. He would be able to throw some light on the early days of Caltech. I might just mention one of the things he got involved with. You see, he used to live, as I told you, just across the alleyway, at the Howard place there on Del Mar. So I got to know him early in my days at the Institute. Millikan one day dropped his keys in the alleyway and didn't know it. Maj Klein and Bob [Robert T.] Knapp [professor of hydraulic engineering; d. 1957] had a copy made of his grand master key. And as time went on, there was evidence that somebody was getting into some of the rooms there in Throop. They couldn't find out who it was, so they changed all the locks. That made Major Klein and Bob Knapp rather provoked. So they took small files and broke them off in the locks so nobody could get in. As a result, they were expelled from the Institute. Bob Knapp went back to MIT and took his bachelor's degree there. And Maj Klein stayed out for a year and then came back to the Institute to finish up for his PhD degree.

PRUD'HOMME: Were there many people fired, or was this a rare occurrence?

NEHER: It was a rare occurrence, oh, yes. It's the only one I know of—there may have been other occasions. But the interesting thing is that both of those fellows later became members of the faculty.

PRUD'HOMME: Millikan was a very moral person, an upright gentleman. Did the school have a reputation as being rigid or stiff?

NEHER: I wouldn't call him straitlaced. I would say that he was quite flexible in his outlook. He was, though, what you would call a conservative. He didn't have much patience for people who broke the codes too much.

PRUD'HOMME: And the codes were fairly universal?

NEHER: In those days, the Institute reacted very favorably to that kind of atmosphere. Divorces among faculty members were practically unknown—of course, it wasn't popular in those days outside in the normal society, either. But very seldom was there any scandal connected with anybody at the Institute. Everybody knew everybody else. It was a family affair, pretty much.

PRUD'HOMME: As a couple at the Institute, was your social life largely concerned with other members of the faculty at the Institute—rather than with the greater Los Angeles or Pasadena communities?

NEHER: As far as we were concerned, it was mostly with members of the faculty, and primarily, of course, younger members of the faculty. I remember that Sara and I were at a dance at the Athenaeum the night of the Long Beach earthquake. That was 1933, I believe. We stood around the radio listening to the radio reports, when the girls thought we ought to be dancing with them. [Laughter] You probably know the story of [Beno] Gutenberg [professor of geophysics; d.1960] and Einstein. They were walking between Throop and the Athenaeum, deeply engaged in some kind of discussion, and didn't feel the earthquake at all. [Laughter]

PRUD'HOMME: Millikan brought many, many famous people to the campus when you were a student. This must have been an extraordinary time.

NEHER: Oh, it was.

PRUD'HOMME: Can you describe some of them? Or in what respect you saw them or were connected with these people?

NEHER: Well, I particularly remember some of them more than others, of course. But H. A. Lorentz was one of those I remember most. He was a theoretical person. He spoke quite good English, in spite of his being Dutch. He had a way of speaking that was just delightful. Earlier, he had really laid the groundwork, to a large extent, for Einstein's special theory of relativity. He was a fairly old man, but he had kept up with physics pretty well. And his lectures were put into book form. He was a wonderful person.

PRUD'HOMME: These people were brought to lecture specifically?

NEHER: Some of them; not all of them. People like [P. A. M.] Dirac would be there, maybe, for a few days. [Arnold] Sommerfeld gave a whole term, a series of lectures. And Sommerfeld was one of the top theoretical physicists in Europe in the twenties and thirties.

PRUD'HOMME: As a student, could you go and listen to these people? Did everyone one campus go and listen to them?

NEHER: Lorentz, for instance, held a regular class; we met three times a week in 201 East Bridge. Certainly all the physics graduate students went, and a lot of other people, too. Einstein gave quite a few talks there.

PRUD'HOMME: What was he like?

NEHER: He talked in German. I understood some German, but my German was not very good. It would have been better, as far as I was concerned, if he had spoken in English.

PRUD'HOMME: Did he have a translator there with him?

NEHER: No. Most all of us knew some German. Of course that was one of the things we had to do then—to take examinations in German. The faculty, most of them, knew enough German to get really quite a bit out of it. But one of the difficulties with people like Einstein is that if the outsiders know it, then they just fill up the rooms. Sometimes it's hard to deal with this sort of situation.

PRUD'HOMME: Do you think that the atmosphere of the Institute has changed a great deal?

NEHER: I haven't been in contact with the Institute very closely for the last twelve years. But I think there has been some change, of course, from what it was back in the thirties. I think it's what you might expect from just a larger institution.

PRUD'HOMME: Did the sixties have any particular impact on the Institute?

NEHER: No, not terribly much. There were student demonstrations, of course, at lots of universities and colleges in the country. And I once asked my class why they didn't have a demonstration. Well, they didn't see much need for it. [Laughter] I think there have been some changes. I don't believe, from what I hear, that students attend classes as often as they used to. Back in the thirties, it was very seldom that any one student was absent from a class session. Your classes would vary in size. The undergraduate classes would be on the order of twenty, and you got to know them quite well. It was very seldom that anyone was absent. Now, during the sixties, this was not so. Sometimes half the class would be absent. You'd ask them about it and they would say, well, they thought they were getting all they needed out of the material they were supposed to read and they didn't need to come to class. I remember the Bergs—he's in biology now, in biophysics, there at the Institute, and his wife, Mary, has been teaching a course in Latin American history in the humanities. When she was here last summer, she said this was one of the disappointing things to her—that a lot of her students wouldn't show up for class.

PRUD'HOMME: Of course, this may be changing back now. They say now that students are very much more serious than they were ten years ago, because they're scared of not getting a job.

NEHER: Maybe. Are they living back on campus, too? There were a lot of them living off campus.

PRUD'HOMME: I think so. I think students get housing wherever they can and as cheaply as they can, because the cost of going anyplace is so expensive. You do what is easiest for you financially. Being on or off campus doesn't make that much difference anymore. What are you most proud of in your work? What has been the most satisfying to you, professionally?

NEHER: Well, those two terms are not quite equivalent.

PRUD'HOMME: All right, let's start with the most proud.

NEHER: Well, that's difficult to say. I suppose I'm apt to think in terms of two categories: One is in terms of the students I came in contact with, who, in general, have been wonderful people. And I've kept in contact with a lot of them through the years. I think one of the most satisfying parts of my professional life has been these contacts with students and seeing what they have done after they leave the Institute. I enjoy that as much as anything, I guess.

PRUD'HOMME: What have they done, your students? What kinds of things have they done? Are a lot of them still academics?

NEHER: Well, take Kip Thorne, for instance. I had him as a student back in 1959-1960. We went through the usual sophomore physics material. He was a good student, no question about it. When he got his degree from Caltech, he went back to Princeton. He worked with John Wheeler there. John Wheeler I know fairly well; he's a theoretical physicist, and his particular field is general relativity. Kip Thorne got his degree there in

three years after he went back, and then he came back to Caltech [1966]. Caltech was after young men who were promising, so he seemed to be one of the ones. When he came back to Caltech in the mid-sixties, in that three or four years he had mastered essentially the general theory of relativity. So he could just play around with it like I do arithmetic. It was just a marvel to me that a young fellow like that could master that very, very esoteric sort of stuff in that length of time. It was just amazing to me. Well, he is now involved in trying to get up an experiment to measure gravity waves. He has a group there that's working on this. He's doing the theoretical end of things. I hope they succeed.

PRUD'HOMME: What other students stand out?

NEHER: Well, I suppose another one there at Caltech is Howard Berg. He did his graduate work at Harvard; he also spent a year in Copenhagen. Soon after getting his degree, he began working with bacteria that navigate by rotating their tails; the tail is a kind of a spiral. He noticed that some of these bacteria got their tails caught on the dish that was under the microscope. And when they did, then the body would rotate. In other words, there's an actual rotation taking place between the tail and the body. And the question is, How can this be? What kind of a joint is there that the tail is fastened onto the body with, that can actually allow the tail to rotate? And as far as I know, the problem is still not solved. Well, every time I go down to Tech, I go over and see Howard Berg, and he shows me some of the things he's working on.

PRUD'HOMME: You said you were proud in two areas. One was students.

NEHER: Of course, I might say also that Guy Stever was another one of my students there. He, of course, has gone on to quite a few things. He was at MIT after the war. Then he was head of one of the departments there, and later became president of Carnegie Institute of Technology, and he was there when Carnegie and Mellon combined. Then he was asked to be head of the National Science Foundation. So he was there. He was actually the chief scientist of the Air Force for a while. And then after he finished his

term as head of the National Science Foundation, he became the science advisor to President Ford.

PRUD'HOMME: And in your work, what has made you—

NEHER: Well, I suppose my work, other than with people, has involved development of laboratory courses, laboratory experiments, and so on, at Caltech and development and use of equipment in cosmic-ray research. These are the two parts that it divides itself into.

Neher working in his lab on some of the telemetering devices he used in cosmic-ray studies. January 1956

PRUD'HOMME: Did you ever feel hamstrung at the Institute in not being able to get a piece of equipment, not being able to get the funds or the time or the space to build something?

NEHER: No, I never felt that I was handicapped by not being able to get what I wanted to do research with. I guess other people have found it difficult sometimes, but I never felt that way. Either my demands were not too great, or people were sympathetic, or something.

PRUD'HOMME: And what has been the most satisfying to you professionally?

NEHER: One thing is that I never found it necessary to retract any of the experimental data that I took. I remember at one of the cosmic-ray conferences, one of the fellows there at the conference said that the data I had been taking kept other people straight. [Laughter] This, I think, was one of the things I tried to do through the years—be sure that the data I took were as objective as possible. In general, I was working in areas where you could say quite definitely that these measurements had very small errors. The interpretation is a different story. But as far as the data were concerned, I felt that they were OK. So I feel that whatever has been published, as far as the data are concerned, stands. This is, at least, a kind of satisfying feeling.

PRUD'HOMME: You said you wanted to tell me some stories.

NEHER: One involved Don Glaser. You probably don't know Don Glaser. After he got his Nobel Prize for the development of the bubble chamber, he was at Caltech and he came into my room; he told me the way it all happened. He got his degree with Carl Anderson at Caltech in the early fifties; then he went back and joined the physics department at the University of Michigan. When he got back there, he began looking around for research problems. And he happened to remember that he and Carl had talked about an inside-out cloud chamber, where instead of gas and liquid droplets to show the paths of these particles through the chamber, you might turn it the other way around and use a liquid. The track would be shown, then, by the bubbles in the liquid. So he began thinking seriously about this. And he went over to the library to see what he could find on the behavior of liquids if you suddenly relieved the pressure on the liquid. How does it start to boil? Where are the initiating little elements on which these bubbles form? Well, while he was doing this, he performed an experiment. He went down to the corner

store and bought a six-pack of bottles of beer, and he took those down to the lab. He said he did it at night so nobody would see him. [Laughter]

What he did was to open some of these bottles of beer in the presence of a radioactive source, and others when the source was missing. He thought maybe the tracks, or the paths of the particles that were ejected from the liquid, or from the wall of the bottle, might show up as individual tracks through the beer as carbon-dioxide bubbles came to the surface. But he said that in none of the bottles that he opened could he see any evidence of any tracks in the beer. But he wasn't discouraged. He continued to search through the library, and he found a paper in an obscure Canadian journal on the behavior of liquids in glass vials—when you suddenly relieve the pressure of the atmosphere above the liquid. Well, this Canadian fellow had a table in there, recording the times after you relieve the pressure until the liquid began to boil. He came to the conclusion that the experiment was indecisive, was no good, because these times were quite random—that if you do it on one occasion, there'd be a certain time lapse between when you relieved the pressure and when the liquid began to boil. Then you'd do it again, and it would be different. Well, this was just what Don Glaser wanted, because he figured that it might be cosmic-ray particles going through this liquid that caused the liquid to boil. And the times being random like that would be just what you would expect, because it's a question of chance as to whether it gets hit by a cosmic-ray particle. He took these times that this Canadian had given in his table and made a simple calculation as to what this time would be, considering the size of the vial and the known intensity of cosmic-ray particles. And it turned out to be about right, what you would expect.

So he then went back into the laboratory and repeated this Canadian's experiment. He took pictures, then, of the liquid in this glass vial, when a Geiger counter was above it and another below it. They selected out of all the cosmic-ray particles those that went through the vial. They triggered a camera, and he actually took pictures of this track going through the liquid in this vial. Well, it was just a question then of expanding this whole idea into something bigger. They now use these bubble chambers at Fermilab, at Berkeley, and in many other laboratories. The interesting thing was that he wasn't discouraged by his beer experiment; he just kept on.

PRUD'HOMME: But he knew he was onto something. Often this is what makes a good scientist, this intuition that you're on to the right track; it may not work this time, but you persist in doing it.

Begin Tape 3, Side 1

PRUD'HOMME: Tell me, what is your other story?

NEHER: If you remember in this article I wrote for Fermilab, I ended up by saying, "Like Newton, Millikan often talked of the giants on whose shoulders we stand."

PRUD'HOMME: Millikan said that when you were walking through the botanical gardens in Singapore, right?

NEHER: Yes, that's right, in 1939. We were hung up there in Singapore because the ship we had planned to go on from Singapore to Calcutta had been taken off; it was a British ship. So we had to wait in Singapore. We were there a week or ten days waiting for a ship that we could book passage on to go to Calcutta. We visited the botanical gardens there. And while strolling through the botanical gardens, he began relating about the giants on whose shoulders we stand. It was fascinating, because he told me at the time that he thought that somebody ought to write a book. And I thought that that was what he was going to do when he got back to Caltech—write a book on these prominent people on whose shoulders we stand, like Newton, Helmholtz, Maxwell, and on down the line. But he didn't do that. He got involved with his autobiography.

About a year ago last Christmas I think it was, our librarian daughter sent me a book, *The Harvest of a Quiet Eye: Selection of Scientific Quotations*, [Alan L.] Mackay. Well, I looked up in it quotations of various people that I knew something about, or that I had read something about before, including Sir Isaac Newton. If you look up Newton, you'd find this quote: "I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the seashore, and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me." That's one that is quoted quite a lot.

There is another one, which I had heard was attributable to Newton, which I didn't find in the book. I was surprised that it wasn't in there. It runs like this, as I recall, "If I have seen further, it is by standing on the shoulders of giants."

I wrote to my daughter there in Plattsburgh, and told her that I had not found this quotation and wondered if she knew whether it could be attributed to Newton. She wrote back and said, "Yes, Newton wrote this in a letter to Hooke"—who was a physicist in England, a contemporary. She said that this idea of standing on the shoulders of those [giants] who have gone before was not new—that there had actually been a book written on this subject. The author had traced this idea back to the 12th century. So it was not new with Newton, you see.[2]

Then I got to going through this book some more, and I found a quotation from Gerald Holton at Harvard. He's in the physics department and his primary interest is the history of physics. So I found this in his book: "During a meeting at which a number of great physicists were to give first-hand accounts of their epoch-making discoveries, the chairman opened the proceedings with the remark, 'Today we are privileged to sit side-by-side with the giants on whose shoulders we stand.'" [Laughter] Well, when we were down at the Lee DuBridge birthday party, Carl Anderson and Lorraine, his wife, were there. I told him this story of how Topsy, our librarian-daughter had rummaged around and found where this quotation, attributable to Sir Isaac Newton, had come from. And then I told him about what I had read about Gerry Holton. And while I was telling him about Gerry Holton and that meeting at Harvard, the strangest look came over his face. I couldn't imagine what it was all about. When I got through, he said that he was one of the scientists. [Laughter] I found it rather interesting. He remembered Gerry Holton saying this.

[2] Murray Gell-Mann's gloss on this quote: "If I have seen further than others, it is because I am surrounded by dwarfs."—ed.