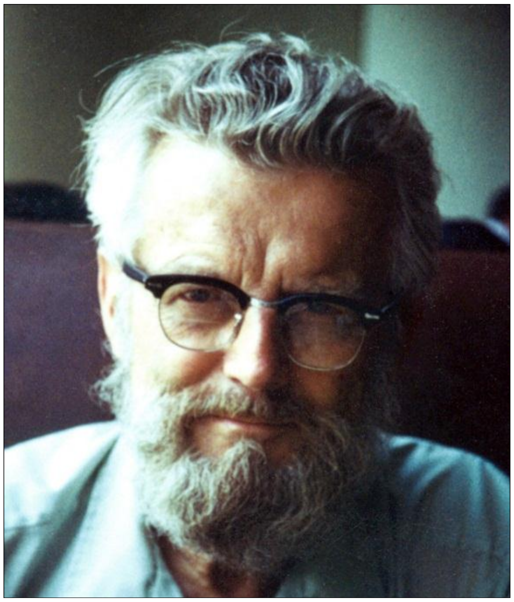

Photo by Doris Neddermeyer, 1971

SETH H. NEDDERMEYER
(1907 – 1988)

INTERVIEWED BY
JOHN L. GREENBERG

May 7, 1984

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Physics, nuclear physics, particle physics

Abstract
An interview in May 1984 with Seth Neddermeyer, emeritus professor of physics at the University of Washington, in Seattle. After receiving a BA from Stanford in 1929, Dr. Neddermeyer took his PhD at Caltech in 1935 with Carl D. Anderson. With Anderson, he discovered the muon, an unstable negatively charged elementary particle, in 1936. During World War II, he worked on the Manhattan Project at Los Alamos, where he proposed using implosion to compress radioactive material to a critical mass in order to make a workable bomb. For these two accomplishments, he would receive the 1982 Enrico Fermi Award.

After the war, he went to the University of Washington, where this interview took place four years before his death. In the interview, he describes his early education in his hometown of Richmond, Michigan; his first two years of undergraduate education at Olivet College and his interest in chemistry; his two years at Stanford; and his years at Caltech (1930-1941) as a graduate student, then a research fellow, working with Anderson. He offers recollections of Robert

Andrews Millikan, whose interest in cosmic rays was closely bound to the work he and Anderson were doing; of Fritz Zwicky, Richard C. Tolman, and Harry Bateman; and of J. Robert Oppenheimer, both at Los Alamos and at Caltech, where he was a visiting professor in the spring term in the 1930s. He recalls receiving the Fermi award and discusses his negative feelings about his work on the atomic bomb.

Administrative information

Access

The interview is unrestricted.

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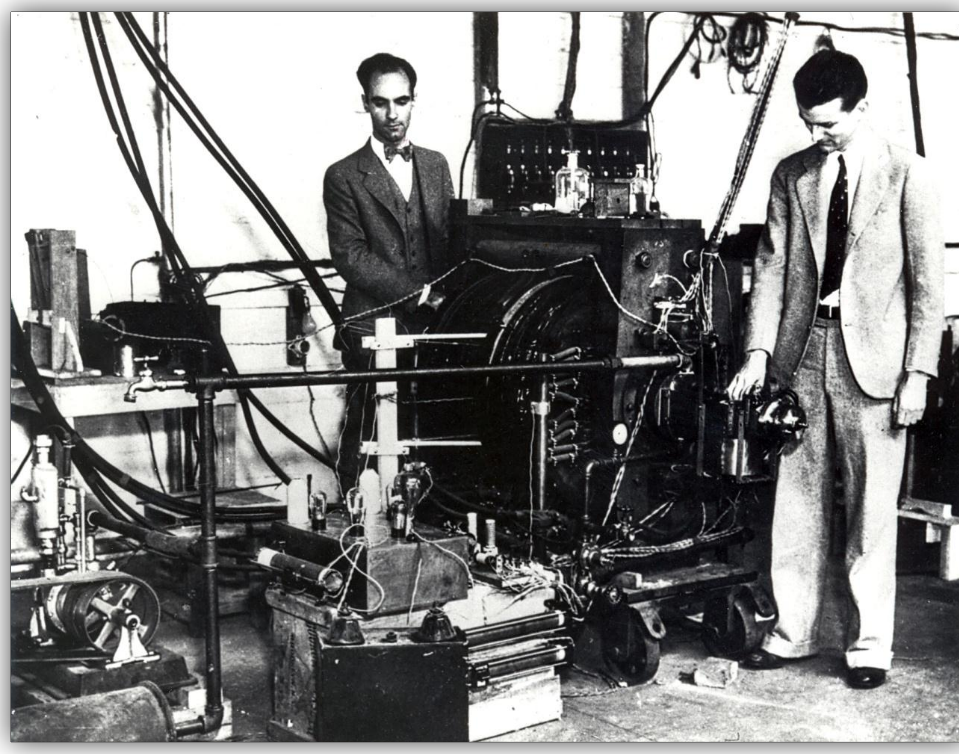
Contact information

Archives, California Institute of Technology
Mail Code 015A-74
Pasadena, CA 91125
Phone: (626)395-2704 Fax: (626)395-4073
Email: archives@caltech.edu



Seth Neddermeyer (on the right) and Carl Anderson, with the magnet cloud chamber in which the tracks of positrons were discovered in 1932, and muons four years later.

Neddermeyer receiving the 1982 Enrico Fermi Award from President Ronald Reagan, April 25, 1983. Photo by Mary Neddermeyer.

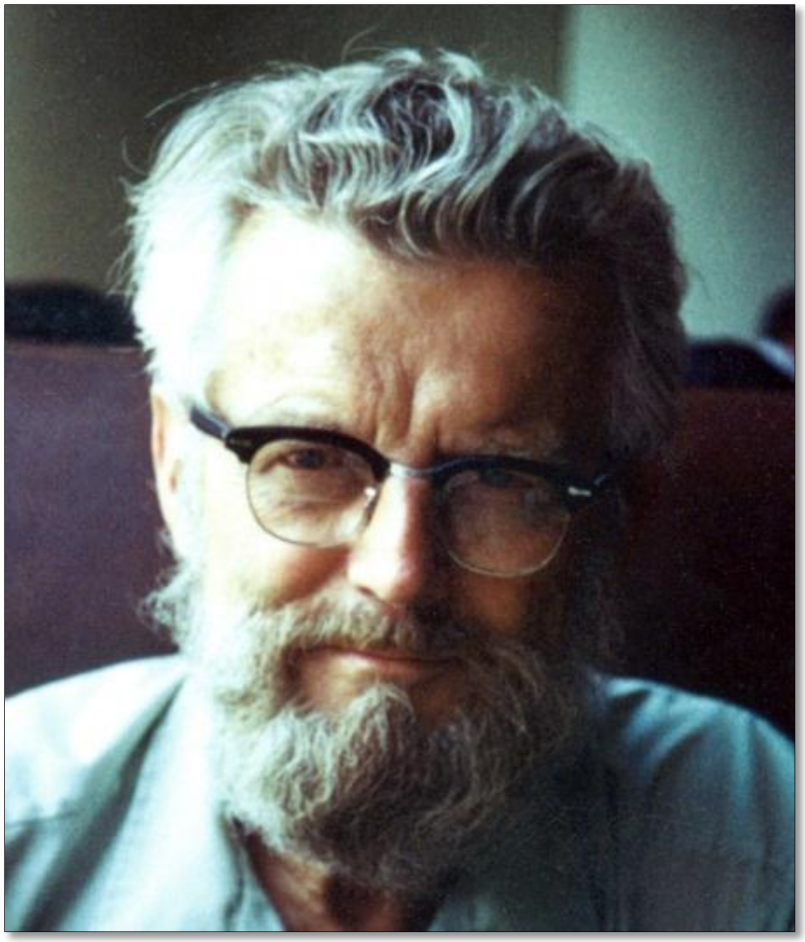
CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH SETH H. NEDDERMEYER

BY JOHN L. GREENBERG

SEATTLE, WASHINGTON



Seth H. Neddermeyer, 1971

Photo by Doris Neddermeyer

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Seth H. Neddermeyer **by John L. Greenberg**
Seattle, Washington

May 7, 1984

Begin Tape 1, Side 1

GREENBERG: Why don't we begin with your childhood. Where are you from?

NEDDERMEYER: Except for two academic years at Olivet College, the first two decades of my existence were spent in Richmond, Michigan, where I was born in 1907.

[Pause] Here's what I was afraid of. I just freeze up. I freeze up physically. [At the time of this interview, Dr. Neddermeyer was suffering from Parkinson's disease —*Ed.*] And I freeze up mentally. It's embarrassing to advertise this fact [laughter] but there's nothing else to do but keep on going.

[Pause] There are a few little anecdotes that are somewhat amusing. Your question about my first interest in science—well, I had an uncle Dwight Lathrop who came around to visit once in a while. He was something of a scholar in the classics; he read German, French, Latin, Greek, Hebrew, and Sanskrit. How good he was I don't really know, but I suspect he was pretty good. He had graduated from the University of Michigan. And by the way, this Olivet College that I spoke of is a small college founded in Michigan in 1844, so it's a hundred and forty years old and still exists.

Well, this uncle was more or less interested in astronomy. Many times he would come and visit us for a while—a week or so in the summertime. He liked to get familiar with the stars and their constellations and think about the lore that was available. Something was known about distances, but there wasn't much of anything known about the sizes of the stars at that time, because there were no fancy interference techniques. Well, he taught me the more prominent constellations and stars—like Leo, Deneb, and Altair in the summer and Orion and Sirius in the winter and Polaris, the pole star, and Cassiopeia. It was sort of fun. He was really very serious about it.

GREENBERG: About what age were you?

NEDDERMEYER: Oh, I would have been twelve or fourteen.

GREENBERG: Neither of your parents, then, were—?

NEDDERMEYER: My mother, my older sister, and this uncle were all graduates of Olivet College and my uncle went on to the University of Michigan afterwards. And I went on to Stanford, from Olivet.

GREENBERG: You grew up in Michigan. What took you off to Stanford, so far away?

NEDDERMEYER: I didn't grow up in Michigan. Those first twenty years, I was going to say, were years in which I didn't grow up. I didn't grow up in those years. In fact, I never really did grow up.

I had some fairly decent teachers. My ninth grade science teacher, a man named Andrew Beam, was a veteran of the First World War and very interested in science and very interested in encouraging me in any way that he could. At that time I also got interested in radio and built a radio receiver and transmitter.

GREENBERG: Did he see something in you that he didn't see in other students?

NEDDERMEYER: Maybe so, I don't know. Yes, he probably did. He was a football coach, but he was also not the kind of guy that you usually think of as material for a football coach. This was a very different fellow from the next football coach we had after he left—a real clod as a teacher.

Your question about the influence of my parents: They recognized the great importance of education. They just wanted to make sure that I got the maximum. My father wanted me to go to MIT. He didn't have any advanced training, but he had heard of MIT and the kinds of things they did, and he thought that was great stuff. So he wanted me to go to MIT.

GREENBERG: How did you end up at Stanford instead of at MIT?

NEDDERMEYER: I'm coming to that. [Pause]

Ah, yes, I had some really interesting teachers. There were two schools. There was a grammar school in the south end of town that took students up to the high school level. At the north end of town, there was a primary and high school that took the students up to the level of beginning college.

[Pause] My mind is just a random population of activated neurons that have no relation to one another!

GREENBERG: Just say things as they come to mind.

NEDDERMEYER: Well, that may work. This man Beam, I remember, made a remark—this would have been in about 1922: "Well, Seth, do you think you'll be gettin' to Mars one of these days?"

GREENBERG: It sounds as if you were primed to be a scientist from very early on.

NEDDERMEYER: We had the two schools, north and south—the Wasps and the Catholics. But we got along pretty well, very good relations between the kids. I remember liking to play baseball. I was lousy at sports of any kind, but in the summer—in late spring before school was out and through the summer—I remember being so excited I could hardly contain myself, to get off with a few of the kids and bat a ball around and play a little informally, where everybody gets a chance to bat in sequence. That was fun.

The superintendent, of course, was head of both schools. He was an interesting character—a fairly rigid disciplinarian but with a sense of humor, and he had a good instinct for teaching. He taught several courses—plane geometry and algebra, and maybe some economics or history. One I remember particularly was plane Euclidean geometry. He wanted to test a theorem about the intersection of the media—the medians being the center of mass. So, as a home assignment, he had the kids make scalene triangles of fair size—several inches across—and bring them to class the next day. So he had them get out their triangles, and he went down the line, first one, then the other, and tried to balance them on the end of a pencil. It was an example of what I call fairly high-class instruction—very informal, very simple, but illustrating a very fundamental theorem in plane geometry.

Well, the question of whether to go to college came up along about this time. [Pause] I only made two or three tiny notes here; that's all the preparation I made. Every time I thought of trying to do something with this, I got depressed and couldn't think about it.

So, the question of where to go to college. Olivet was sort of a family college. Incidentally, it was founded by missionaries, but there was no great pressure of religion or anything like that. There was a course in Hebrew history that I took, which counted as history when I got to Stanford and led to my being farther along towards graduation than I would have been if I had had the first two years at Stanford. [Laughter]

I had a pretty good physics teacher in high school. His name was Wilfred Trudgen; we called him ―Doc." I think the reasons for calling him Doc was that he was so meticulous and careful in his speech. Very, very precise. He wasn't a tremendous genius, but he was the kind of person who could help a student who was interested in learning. I don't really know how to evaluate his teaching; the main thing I remember is that he was a decent human being. And, oh yes, in that line, he was the first teacher from whom I got any appreciation of certain social problems, like labor abuses, you know, and injustices of various kinds. He was concerned about justice, and maybe this is connected with the fact that he came from northern Michigan, which is a mining area, where there must have been a lot of labor problems.

[Pause] My mother was active in the local women's clubs and things like that. She read a lot. I remember her reading Sinclair Lewis avidly, because she had had him in a tenth grade English class when she taught high school in—oh, what's that town, in the north, where Sinclair Lewis went to school? I'll think of it. [Sauk Centre, Minnesota —*Ed.*] She was really extremely active; she loved to sew—taught herself and made things of professional quality. She made all her own clothes and made clothes for other women. She liked to cook—made all manner of goodies to eat and good solid meals. She'd have card parties and play games and stuff.

My father was in business. In 1910, he set up a general store—dried goods and furnishings and later groceries. The store was in the south end of town. It survived until the automobile made it too easy for people to get down to Detroit, where they could get a much wider selection of merchandise—that's a pattern that existed all over the country. He and my mother talked a long time about the possibility of moving to California. Come 1926, 1927, business had got so bad that my dad just decided he'd better pull up stakes and go somewhere else. He dealt some in flowers—in nursery stock—and he decided to go into the nursery business in California. He had a fairly active mind. He could identify by name just endless numbers of common plants and shrubs and flowers. He took as much pride in his identifications as he did just in dealing with the things, in the handling and selling of flowers, and giving them away.

So, going to California. Well, the question of going to MIT was moot in any case, because where would the cash come from? So I decided to go to Olivet College, which was much cheaper; besides, Olivet was a kind of a family school. I'd go to Olivet, and then, if possible, if it turned out to be possible, go to Caltech or Stanford or both.

GREENBERG: So at this point, you already knew about Caltech.

NEDDERMEYER: Yes, I knew about it. My dad didn't, not until I told him. Where did I learn about it? I don't know. Oh yes, I read about [Robert A.] Millikan's cosmic-ray research.

GREENBERG: So you already knew about the cosmic-ray research.

NEDDERMEYER: I think I did; I'm not absolutely certain.

GREENBERG: Well, it's certainly possible, because, after all, it was getting a lot of publicity in the popular press, right, in the late twenties?

NEDDERMEYER: Yes, it was. I went to Olivet College in the fall of '25 and took the usual things—French, a couple of math courses. Physics I postponed. Chemistry. [Pause]

You'd think that after being a professor for twenty-five years—lectures and seminars and that sort of thing—that by now I could organize a very simple narrative of a chain of events.

GREENBERG: Well, I don't know. Has anybody ever asked you to give a biographical sketch? Have you ever had to do that before?

NEDDERMEYER: No, not really. Well, there are still some interesting angles I hope I can get to.

Our decision to pull up stakes and move to California pointed to Stanford or Caltech. The Millikan work was of more importance to the decision to go to Tech graduate school. But I haven't really finished with Olivet College yet. I took chemistry from Allen B. Stowe. He was a PhD from Brown, a student of Charles Kraus. He was a very good man. And the man in physics was Eugene W. Skinner, who later went on and got a doctor's degree on the diffraction of X rays in liquids. Then he got interested in the physics of dental materials; he was interested in practical applications, so that was a good field. What did he do eventually? He ended up being the dean of the School of Dentistry at Northwestern. [Laughter] One time, a couple years ago, when I went to the dentist here, I asked him if he'd ever heard of Eugene W. Skinner. He said, "Oh yes, he's the author of our dental-materials bible." You could have knocked me over! Skinner was not a super-fancy, highbrow, polished teacher—he made you learn the stuff. I took a one-year course in calculus from him—clear through the year, a thick calculus book. It was really a good course, a good guide.

And Stowe—well, I was interested in chemistry independently very early, in high school or maybe even in eighth grade. I was greatly influenced by Edwin Slosson's book *Creative Chemistry* [New York: The Century Co., 1919]. It was just tremendously exciting, to take two totally different materials and make a new material that had properties so different from the first two. That was the exciting thing about synthetic chemistry, particularly things like the hard plastics. Bakelite was the famous one at that time, made by mixing carbolic acid and formaldehyde. Well, he honored me by awarding me half of the freshman prize in chemistry. I shared that with a bright girl—not meaning to imply that girls in general are not as bright as boys; they may be brighter.

Actually, for many years, for seventy years, I've been interested peripherally in certain wild things. Guess what? Like parapsychology. I was impressed by things my father did. That's another thing I was going to get at later, but I'll do it now. My father used to make remarks about gross physical effects—people sitting around and concentrating at a table and having the table go through antics. I wondered, "Can you believe this, or can't you?" The fact that my father was involved in it makes me believe that it's for real. There's a lot of work that's been done on this so-called psychokinesis, and I regard that as one of the well-established facts of science. People say, "Yes, but parapsychology isn't a science." The meaning of the word “science” is “to know”; it’s from the Latin word *scire*, to know—any endeavor that’s involved in learning and knowing new things.

Begin Tape 1, Side 2

GREENBERG: What was Millikan like?

NEDDERMEYER: Well, he was much more approachable than you might think he would be. He was a great guy, great scientist. But he was very tenacious about hanging on to an idea. Once he got an idea in his head, it stayed there permanently; there was no way to get it out. But at the same time, he had a lot of decent humanity about him. I was a particular recipient of his generosity—in recognizing something useful somebody had done and supporting me in my work.

GREENBERG: When the time came, in the course of the cosmic-ray research, when you just had to abandon his ideas, was that hard? Were there difficulties there?

NEDDERMEYER: You mean for me? Or for [Carl] Anderson?

GREENBERG: For the both of you.

NEDDERMEYER: I remember one time Anderson saying, when we had gotten another piece of evidence that contradicted Millikan’s ideas, “Now we’ve got the Chief by both balls.” [Laughter] This went on for a long time—just a continual total conflict between the two.

GREENBERG: Between Anderson and Millikan?

NEDDERMEYER: Between the ideas. Between Anderson’s ideas and Millikan’s ideas. But the positive electron he accepted very quickly. Very quickly. I forget how, exactly. I remember.... I think I developed that film. We had just finished a run, and I was developing the film and hung it up on a rack to dry. We were eagerly looking for evidence that would explain these strange tracks we had. [Pause] There were three kinds of tracks, in general, that we observed. One was a slightly ionizing track, moving essentially at the velocity of light, singly charged, as you could tell from the density of the track more or less, close to a standard unit charge. If you interpreted all of these particles as going down, then there were both positive and negative charges. And some of those had to be interpreted as positives on that basis, where it showed heavy ionization. It's a characteristic of protons— This is old hat.

GREENBERG: As far as the argument goes, as you say, it is old hat and written up. But, of course, what isn't written up is how Millikan would hang on to his own ideas till the last. And we were just curious, since he oversaw the cosmic-ray research, how much of a problem this was for you.

NEDDERMEYER: No, it wasn't too much of a problem. He was, after all, a reasonable guy. He wouldn't say that black is white for very long.

[Pause] Well, the point was that particles going down indicate positive charges. Some of them—or heavy ionizing, which should be characteristics of protons, which was a positive charge. Others were in the momentum range where if they were protons they should have showed the ionization but didn't. They were lightly ionized. That's where they had to be of a much smaller mass than a proton. That became the general argument, and evidence of that kind piled up enormously. Then we put this slice of lead in the chamber to slow the particles down to show a measurable change of momentum between the two sides. And there, by golly, we got one going through the lead plate, a positive sign, with an ionization characteristic of a single electric charge. If one like that showed— [Laughter] It's such a trivial kind of an argument.

GREENBERG: I didn't come here to have you redo the physics.

NEDDERMEYER: No, I realize that. Let's see, where was I anyway? [Pause]

GREENBERG: OK. [Theodore] von Kármán was up on the third floor [of the Guggenheim Aeronautical Laboratory], where Anderson had his apparatus?

NEDDERMEYER: Yes, he used to come around and talk. [One time] he walked over to where the controls were mounted on the table, and I was there and had some of the [cosmic-ray] shower photographs. And he'd pick it up, and he said, "How many of your other pictures did you have to put together to get that one?" [Laughter]

GREENBERG: Was it at Caltech, or before Caltech, that you got so interested in Millikan's cosmic-ray research?

NEDDERMEYER: It was before Caltech. I think it was when I was home from Stanford on a summer vacation.

GREENBERG: Did you learn a lot of physics while you were at Stanford?

NEDDERMEYER: No, very little. Still, they had two excellent men in X rays, [David Locke] Webster, who was head of the department, and [Perley Ason] Ross, who was a professor who worked more or less separately from Webster.

GREENBERG: Were you still basically interested in chemistry at Stanford?

NEDDERMEYER: At Stanford in my senior year, I spent quite a lot of time in the library reading monthly notices of the Royal Astronomical Society, and in particular the controversies between [James Hopwood] Jeans and [Arthur] Eddington. They were continually fighting one another over large-scale processes in stars—Cepheid variables, and so on. That was before anything had been settled, for sure, about the source of the stellar energy. I had Eddington's book *The Internal Constitution of Stars* [Cambridge, U.K.: Cambridge University Press, 1926], which I spent hours and hours and hours perusing, trying to do some of the calculations that made it look big then. That was the radiative equilibrium problem—polytropic spheres. And that sort of stirred my interest further in astrophysics. Now, Stanford had no particular interest in that field. No work of that kind going on. There was an interest at Caltech. Caltech was very strong in astronomy and astrophysics.

GREENBERG: Whose work at Caltech?

NEDDERMEYER: Well, there's [Ira Sprague] Bowen's very nice experiment on the identification of the nebulium line as a transition from a metastable state. There was [Richard C.] Tolman, at a highbrow level. Tolman impressed me as being a remarkably fine, decent guy—a brilliant, highly productive scientist. Talked with the common man as if he were his equal, or almost. He was very friendly and warm. And tremendously productive; he did a lot of important work. Oh, there's that anecdote about Tolman. Tolman's last two great works were his book on statistical mechanics and one on relativity and thermodynamics. The development of the book on statistical mechanics had been followed very closely by [J. Robert] Oppenheimer. In fact, Tolman had gone to Oppenheimer with problems, in various situations. Tolman made the remark that he was so indebted to Oppenheimer that the only thing he could do was to dedicate the book to him. And in the case of the relativity and thermodynamics, he dedicated that to the famous chemist at Cal [UC Berkeley]—guess who? G. N. Lewis. He said, "I'm dedicating this to G. N. Lewis. That's to pay an old debt; now the son of a bitch can go to hell." [Laughter]

GREENBERG: And also [Fritz] Zwicky. Zwicky was in astrophysics.

NEDDERMEYER: Oh, yes. I liked Zwicky very much. Oh, Jesse Greenstein [Dubridge Professor of Astrophysics, d. 2002] gives him a bad time; he says he's a self-proclaimed genius. And he did have some unsound ideas, but he had the courage to look into things that he thought might be interesting. Like the secondary structure that he and [Haakon M.] Evjen worked on for some time. That failed—but nevertheless he [Evjen] finished his thesis [PhD 1929]. I don't know whether the proof of the failure of the idea came about from Evjen's thesis or a later development. I remember when Zwicky and [Walter] Baade found the first supernova. Boy, was he excited. That was a great achievement—such a completely radical hypothesis without having a strong basis for it. He stuck by his guns.

GREENBERG: OK, you had an interest in astrophysics, but when you came to Caltech you didn't go into astrophysics.

NEDDERMEYER: I was in the process of poking around for a research problem. I talked with various people, who talked with Zwicky. Zwicky advised against my working for him, because I didn't have really such a fancy record.

GREENBERG: Did anybody work for Zwicky? He had a notorious reputation among students as being rather difficult to deal with.

NEDDERMEYER: Well, I found him rather easy to deal with. I really didn't communicate too much with Tolman in physics. He was way over my head. He did really substantial things that held up with time. [Harry] Bateman. The things I remember about Bateman: One was that I took his course in partial differential equations and darn near flunked it. I remember particularly his introduction to Green's functions, doing the example of a one-dimensional case: that is, two straight-line segments intersect the X axis at both ends, and then putting in a weight function that depends on the position of the break, and then integrating it out and looking at the findings. The thing you have when you do that is a solution to an inhomogeneous equation, instead of a y''=0. That is, a solution to y''=f(x). That was cute; it was the simplest possible example of a Green's function, and gives you an understanding that you don't quite get in mastering more complicated cases. Needless to say, I really didn't master the complicated cases. And another time Bateman came to class and, wringing his hands like this, said [mimics Bateman's speech], "I shall now prove that Mohammed's coffin could not have been suspended in mid air." [Laughter] Specious arguments about boundary conditions that would be violated. I don't know, I didn't catch it all. [Laughter] He was a tremendous guy. He just had a whole universe in his head.

GREENBERG: And you were able to utilize a little of what he tried to teach?

NEDDERMEYER: Oh, yes, a little bit—but darn little. Millikan made a remark—an amazingly sad remark. [Pause] Bateman hadn't done nearly as much for the institute as he hoped he would. Well, I shouldn't say that, because I can't trust my memory, but there was some disappointment there.

GREENBERG: Well, that fits with some other evidence.

NEDDERMEYER: Bateman tended to have wild, crazy ideas, like building a ship out of ice during the war, to be used for a couple of traversals of the Atlantic. [Laughter] He suggested tying a balloon to an airfoil—for what purpose, I forget. Very strange character! Campus life in the thirties.

Now, let's see. I got to Stanford from Olivet College. My college chemistry teacher wanted me to go to Tech and wanted me to work with Millikan in physics. My physics teacher, on the other hand, wanted me to go to Stanford, because he was an X-ray man. Because of Webster's and Ross's X-ray business. Of course, then he quit completely and went into solid state.

When it came down to the business of going to graduate school—having graduated from Stanford, you know—we just couldn't make it for the fall term of 1929. My dad desperately needed some help to get his business going, so I stayed out that term, instead of starting. Nevertheless, my application for admission to Tech was accepted. I went there in January [1930], registered for a full schedule of three courses. The first term I managed to survive. I took [William R.] Smythe and Bowen and [William V.] Houston's classes.

GREENBERG: And this was without having had a whole lot of physics.

NEDDERMEYER: This was without having had the first term. Well, when I was at Olivet, Skinner gave a little course in mathematical physics. He wasn't any great authority in mathematical physics; he followed the text closely in the book by R. A. Houston, which I haven't seen around for many years.[1] He followed that pretty closely. So there was a smattering of fluid mechanics especially, Fourier analysis, potential theory, finding gravitational forces on various distributions of matter.

GREENBERG: I suppose you had to work very hard that first semester at Caltech.

NEDDERMEYER: Yes.

GREENBERG: Well, for example, you took Smythe's course [on electromagnetism] your first term?

NEDDERMEYER: Yes.

[1] R. A. Houston, *Introduction to Mathematical Physics* (New York: Longmans, Green, 1912).

GREENBERG: That's reputed to have been a very difficult course.

NEDDERMEYER: I forget now, I got a C or a B. The thing that amused me [laughter] very much was many years later, decades later—ten or fifteen years ago—Smythe remembered me as an A student, but I never remotely got an A from Smythe. [Laughter]

To complete the story about that, I ended up with credit from Houston and two terms to take care of everything. So I took an exam. It was an exam that was given for that course in lieu of that course, and I passed. And Bowen—I didn't do so well and had to repeat part of that, but I finally cleared myself completely through Bowen's course, by examination. Then there was the advanced calculus that I had to take. Had to take the exam, even though I had already taken an advanced calculus course in Stanford, in which I think I got an A. And I took the exam in Zwicky's mechanics course, also in lieu of taking the course. He passed Olin [C.] Wilson and me for two terms. This whole thing wound down finally [laughter]; I had everything satisfied except Zwicky. There was one term missing. So I went to Zwicky, and he said, "Oh, well, why don't you come into my office and we'll talk a little bit." So I never had an easier exam. I was weak on the Hamiltonian formulation. He said I didn't know quite enough about that.

GREENBERG: Zwicky's course was also supposed to have been very hard.

NEDDERMEYER: No. It's curious, this business of what's hard and what's easy. I was somehow programmed accidentally to have the right responses. So I got professional advancement.

Then my doctor's oral exam [1935]. Let's see, who was on that committee? Anderson, Millikan, Tolman, Houston, Bateman, [Paul] Epstein, and Bowen. For some reason, they had an abnormal number of professors on that committee. Anyhow, Houston told me the committee thought I was going to flunk, so they gave me a margin instead. [Laughter]

GREENBERG: I think five was the usual number.

NEDDERMEYER: Well, it was probably five. [Pause] Well, I don't know.

GREENBERG: From the way you talk, one would get the impression that you were just an average student at Caltech. Is that true?

NEDDERMEYER: Yes. Well, average or below average.

GREENBERG: Average or below average? Really? In other words, it wasn't until you really began to do research—?

NEDDERMEYER: Well, I don't know. I feel as if I sneaked through Caltech without really demonstrating my competence in anything.

Begin Tape 2, Side 2

GREENBERG: I guess before Los Alamos, you and a group of Caltech people packed up and went off to Washington, D.C., to do work on proximity fuses. You and the Lauritsens and Fowler.

NEDDERMEYER: Yes. Oh, I forgot to tell you about my room and board when I got to Caltech, in January 1930. I found a place where I could get room and board for $25 a month; I used that for six months. I forget what happened, but finally I just plain dropped out for a while. I had a couple of droppings out—was it one or two? When I came back, we found another source of cash.

GREENBERG: You had to drop out because of money problems?

NEDDERMEYER: Oh, yes. But I went back. And this was after I'd got only candidacy requirements out of the way. Maybe this was the stage where everything was done except Zwicky, or maybe one other.

GREENBERG: Did you earn anything as a teaching assistant?

NEDDERMEYER: I never had a teaching assistantship; I wasn't good enough to get one at Caltech. But Millikan supported me from his research grant to the tune of, first, $25 a month, and then.... [Pause] Let me see—I'm in a fog again. At some stage or other, I went to see Millikan and asked him for another ten bucks a month, which he gladly gave me—that's $35—and then on a later occasion, again at my request, he raised it to $50. When I got my degree [and became a research fellow], it went up to a $100 a month, $1,200 a year.

GREENBERG: You had your meals at the Athenaeum?

NEDDERMEYER: No, I couldn't afford the Athenaeum.

GREENBERG: Did you go to the London Conference [International Conference on Physics] in 1934?

NEDDERMEYER: No. Neither Anderson nor I went. Millikan gave the paper.

GREENBERG: Because there just wasn't money for everybody to travel?

NEDDERMEYER: Oh, no. Millikan had $25,000 or $30,000.

GREENBERG: So Millikan was the only one who went to that conference, is that right?

NEDDERMEYER: Yes.

GREENBERG: Was the shortage of cash a big problem for the doing of physics, during the thirties?

NEDDERMEYER: Yes, it was. It took a powerful guy like Millikan to raise money.

GREENBERG: Did you build your own—?

NEDDERMEYER: Tech had the astrophysics shop [the optics lab in the Robinson Laboratory of Astrophysics —*Ed.*]. It was set up for building the big telescope [Palomar]. That shop was used to build Anderson's first apparatus. That was a remarkable thing; they did that in about a year. It was basically a simple design, but the whole thing was a substantial design, construction, and engineering job, with corners cut at every possible [juncture]. Kellogg got its support from W. K. Kellogg, I guess. Did they also get some government support? I don't think so.

GREENBERG: No.

NEDDERMEYER: Millikan was an amazing guy. At the age of sixty to seventy, traipsing all over the world with [high-altitude] balloons. It was a tremendous job. Hauling equipment like that around—lead, and electroscopes, and papers, and tracking—clothing for different climates. It's almost incomprehensible [to me] because I never did anything so complicated.

GREENBERG: You worked with Anderson. Did you work very closely with anybody else besides Anderson?

NEDDERMEYER: No. I remember [laughter] one time Dick [H. Richard] Crane and I got together and we were going to do an experiment. Something with neutrons; I can't remember what in the hell it was. But Charlie [Charles C. Lauritsen, director of Kellogg Radiation Laboratory, 1931-1962] didn't like a guy from another group [i.e., Neddermeyer —*Ed.*] getting his nose under Lauritsen's tent, so he gave an order to tear the machine down and correct some things, fix things. So we dismantled the tube.

GREENBERG: That's interesting. You mean, he didn't want—

NEDDERMEYER: I don't know whether he actually dismantled it or not. But he started to carry out a plan—maybe a revision in design, or maybe a correction of— But I got the distinct impression that it wasn't really necessary. I was just somebody out of the group that he didn't want around [chuckle]. I never talked to Crane about that. I forget what it was we were going to do. Lauritsen was a strong character.

Begin Tape 2, Side 1

GREENBERG: Who discovered the muon?

NEDDERMEYER: I hate to argue about it, because it's really being petty about it. I just think I should have been given credit for the muon. There are some early tracks that actually indicated very strongly the presence of particles more massive than (a) the electron or (b) known at the time—positive particles. They were just there, among the photographs. You can always say that in physics, but—there they were! Well, Anderson was a little too conservative. He should have been more radical. And I was sort of pulling stuff from which to— I'm not trying to malign Anderson, I'm trying to understand what actually happened. And what actually happened is that those evidences were there and they didn't get published.

GREENBERG: Though you and Carl Anderson are usually credited with having discovered the mesotron, right? [Note: The muon was first called the mesotron, and later the mu meson —*Ed.*]

NEDDERMEYER: Yes.

GREENBERG: So what you're saying is that the credit is for some other evidence—some evidence different from what you're talking about now?

NEDDERMEYER: That's what it amounts to. Well, you see, one of the interesting phenomena that happened was that the electrons that were scattered all through passing particles, appeared as nice, curved tracks— [Unintelligible] The frequency of those could be understood in terms of the already known cross-section for such processes. So I grabbed onto this and started the data, analyzed them, and I figured out [that] the observed distribution, when corrected for the energy losses of the electrons in the absorber, were really important for— Also, there were strange things that— [Long pause] I can't understand why, when something is perfectly clear to me, all of a sudden everything can become a complete fog.

GREENBERG: Yes, I know. There is an article by a fellow whom you know, because I believe he told me he interviewed you. He's at Stanford; his name is Peter Galison. He has written an article on the discovery of the muon and the various cosmic-ray research groups and the roles that they played in the discovery.[2] One of the things that was so interesting is that the groups were so different. There was [J. C.] Street and [E. C.] Stevenson, and then [Bruno] Rossi. I guess that's where I got the idea that there was some sort of controversy involved in trying to

[2] "The Discovery of the Muon and the Failed Revolution Against Quantum Electrodynamics," *Centaurus* 26:3, 262-316 (1982).

decide who actually discovered the muon—and when. Different kinds of things constituted evidence for different groups, and it gets into a lot of interesting issues. For example, I gather that Millikan didn't like scintillation counters, right? You used the cloud chambers, and other groups didn't like the cloud chambers and used the scintillation counters.

NEDDERMEYER: Geiger counters.

GREENBERG: Yes. [Pause] You said Anderson was very conservative. He was reluctant to publish your evidence?

NEDDERMEYER: Yes. Finally, when he did agree to publish it,[3] it was only after Street and Stevenson had published this abstract in the *Physical Review*.[4] On the other hand, Street and Stevenson based their work on our earlier work, which already was showing strong indications of twenty-five percent, so they reached in and grabbed the discovery from under our noses.

GREENBERG: Though you still are credited with the discovery, as far as I can see.

NEDDERMEYER: Yes, well, I think that's partly because of the scrap that Anderson put up about it.

GREENBERG: When you found the particle, you knew right away that this wasn't the Yukawa meson. Is that right?

NEDDERMEYER: All together, these particles had traversed god knows how many meters of lead without showing strong-interaction forces.

GREENBERG: So nuclear forces weren't involved.

NEDDERMEYER: So presumably nuclear forces weren't involved. I didn't know how to think about [Hideki] Yukawa's theory; I wasn't enough of a theoretician to have grasped it.

[3] Seth H. Neddermeyer, ―The Penetrating Cosmic-Ray Particles," *Phys. Rev*. 53: 102-3 (1938).

[4] J. C. Street & E. C. Stevenson, ―New Evidence for the Existence of a Particle of Mass Intermediate Between the Proton and Electron," *Phys. Rev*. 52, 1003-4 (1937).

Oppenheimer recognized immediately the importance of the fact that these particles were so abundant—you see, all these penetrating particles were so abundant.

GREENBERG: I wanted to ask about theory and what you did about theory, how you handled theory. I guess Oppenheimer was not your theoretician—is that right?—in the way that he was, for example, the Kellogg Radiation Laboratory's theoretician.

NEDDERMEYER: Yes.

GREENBERG: In fact, in talking to Willie [William A.] Fowler [Institute Professor of Physics, d. 1995] recently, Fowler told me that if it weren't for Oppenheimer, in most instances the Kellogg physicists would not have understood the significance of what they were doing. Oppenheimer was very close to Charlie Lauritsen and the group. He didn't play a big role in your work, is that right?

NEDDERMEYER: Well, he followed it pretty closely.

I was just trying to think of this detailed balancing argument. [Long pause] Well, if these particles are so abundant at sea level, there has to be a very high cross-section for the process to occur in the top of the atmosphere [unintelligible]. [Pause] And if the cross-section for production is so high, the cross-section through absorption has also to be high, by detailed balancing arguments. This was the argument that Oppenheimer used frequently. However, if you assume that there are two different kinds of particles, one kind which is produced by the primary interaction via the strong force, and the other one— And then those primary particles decaying into other particles, which are the muons, then on that basis you could understand something new about the processes that were going on. Maybe it was a double process: the creation of one particle and its decay into another, classified in physics as the low-penetration particle. Primary particles, high-production cross-section, low penetrability. Secondary particles, the product of that particle, low cross-sections for further interactions. Oh, well, I've said it badly.

That was really a very fundamental point Oppenheimer made. Where he slipped was in not going the whole way in making the definite postulate that there is a high cross-section for producing the primary particle, whatever it was, in high probability for its decay into another particle, which had low absorption probability. That was all you needed to get the muon. So it remained for [Victor F.] Weisskopf and [Hans] Bethe and [Robert E.] Marshak—Weisskopf and Marshak, maybe Bethe and maybe not, I forget—to make the definite postulate. The primary particle was the Yukawa part, and the secondary particle was the muon.

GREENBERG: But Oppenheimer did follow your work.

NEDDERMEYER: He did follow it closely. I remember he was back and forth between Berkeley and Pasadena, as you know.

GREENBERG: We've gotten the impression so far that when Oppenheimer did come to Pasadena for his term in the spring, he seems to have spent an awful lot of time with the nuclear physicists. In other words, they got the lion's share of his time.

NEDDERMEYER: Oh, yes, they did. No, Anderson and I should have been more careful about cultivating Oppenheimer. He was a super-bright guy, and he was impatient with stupidity. Naturally so—super-bright people are.

GREENBERG: You mean he was intimidating.

NEDDERMEYER: Yes. But you can't blame him for that. If I was that bright, I'd be intimidating, too. [Laughter]

GREENBERG: But I wonder. Millikan left the nuclear physics group alone, right?

NEDDERMEYER: Yes. He left them pretty much alone.

GREENBERG: Whereas in your case, he was sort of the chief?

NEDDERMEYER: Oh, yes.

GREENBERG: How did Millikan and Oppenheimer get along?

NEDDERMEYER: Not too well. Millikan didn't recognize him. Oppenheimer got a dirty deal in some ways.

GREENBERG: Because I know that sometimes Oppenheimer tried to give Millikan some advice where his ideas about cosmic rays were concerned, primarily where the atom building, the interstellar synthesis of elements, was concerned. And Millikan just wasn't about to pay any attention to anything Oppenheimer had to say—or very little, at any rate. So I can't imagine how that could have made your dealings with Oppenheimer any easier. I mean, having the man, Millikan, who's after all the man who initially began the cosmic-ray research— I'm just trying to imagine how things went. Did that interfere with your relationship with Oppenheimer—the fact that Millikan didn't get on so well with him?

NEDDERMEYER: No, I don't think so. But Anderson and I really did some dirt ourselves, by just not acknowledging consultations with Oppenheimer—just in general, without any question on a specific thing. He was in and talking about things, and all this discussion stirred things up, kept ideas stirred up. And just for that, we should have given Oppenheimer a thank you. And Millikan never picked us up on this—that we'd better give Oppenheimer some credit. We really should have. I only later realized our [hands] weren't clean, either [chuckle]. But I remember one thing on the basis of the early evidence for the positive electron, or a particle of mass considerably less than the proton. Oppie made the remark: "One thing is certain; it has nothing to do with the Dirac [theory]." Instead of having gone and examined the consequences, he assumed that. Where that came from, I don't know.

GREENBERG: Was this his custom, to just make those kinds of statements flat out?

NEDDERMEYER: Well, of course, it was from a particular point of view. And I don't know what particular point was under discussion at the time. It's just a question of, What is this crazy particle that's around all of a sudden and has never been seen before?

GREENBERG: In the Carl Anderson interview—the long version, not the excerpt in *Engineering and Science*—Carl talks about the [P. M. S.] Blackett and [Giuseppe] Occhialini confirmation of the discovery of the positron,[5] the introduction and the idea of pair formation, and the tying it in with the Dirac theory. Anderson said that he just couldn’t get Oppenheimer to explain pair production to him in a way that was comprehensible. Somehow, Oppenheimer just couldn’t communicate the pair formation in a way that could be understood.

NEDDERMEYER: Well, he would come down to the lab, fish that picture out and put it on the screen, and stare at it. It was one of those times when that happened, I think, that he made that remark about Dirac theory. It’s intimately related to what his viewpoint was, from which the statement was made [chuckle], and I can’t fish that out. Maybe if I think about it some more, it will come out.

I just feel as if nothing that I ever did in the way of publication and research was really clean, in the sense that “Here is a clear thing”—with this kind of an explanation—“and that’s it.” Where everything is sewed up, and the explanation fits the observations, and you can consider the experiments done, and verify them.

GREENBERG: Do you mean to say that very often, in physics, the opposite is true—that [things are] very clean and clear?

NEDDERMEYER: Oh, yes, I suppose.

GREENBERG: Does that mean that discoveries in physics are, more often than not, something different than yours were? Or somehow fit more cleanly than they did for you? Is that true?

NEDDERMEYER: Well, I just feel as if I’d been sloppy about things—didn’t do nearly as clean a job as might have been done if I had been a little less cavalier about it. In other words, I feel very unhappy about the net total product of my career. I had golden opportunities for doing a beautiful job on something and sort of muffed it, made sort of a mess of it—a scrambled egg.

GREENBERG: Are you talking about the muon research?

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[5] Blackett, P. M. S., & G. P. S. Occhialini, “Some photographs of the tracks of penetrating radiation,” *Proc. Roy. Soc. Lond. A*, 139: 839, 699-U18 (1933).

NEDDERMEYER: I'm talking about— Yes, maybe. In other words, I'm in a state where I just want to forget about things and read and try to write, whatnot—just anything that can help. One thing I was going to tell you has to do with the [Enrico] Fermi Award [1982].[6] Now, that sort of bowled me over, and I felt that the occasion of accepting that prize should be used to make a few sensible remarks about something—about the way things are going in the world. Three or four items: In the first place, it's not a very happy experience to accept such a prize for something that has such hideous consequences. Another thing is, let's get off the bomb binge. And third, we have to learn how to convert from a wartime to a peacetime economy. We have to learn how to use our genius and productive capacity—all this enormous material and human resource we have that we're applying toward totally destructive purposes—for decent humanistic purposes. So what happened? When I got up and shook hands with that son of a bitch [President Ronald Reagan, who presented the award], I got cold feet, quickly forgot about what I was going to say, and made that asinine, self-demeaning remark to the effect that "Somebody must have made a mistake." That appeared in the paper.

GREENBERG: Yes. I've got it right here.

NEDDERMEYER: Well, I'll never be able to explain it. I felt as if— The first anniversary of that has already passed; it was on the 25th of April, 1983, that I accepted that prize. I'll never live it down. I mean, I can't live with myself that way.

GREENBERG: Well, you've got the opportunity right now.

But still, I think it's easy to say some of the things. I mean, the problem is that in hindsight it [atomic bomb assembly] all looks so simple. It didn't look so simple forty years ago. I wanted to ask about that episode—your invention of implosion and finally the success of implementing the idea—if you are willing to talk about it, because all I've read on the subject is in Nuel Pharr Davis's *Lawrence and Oppenheimer* [New York: Simon & Schuster, 1968], and in

[6] Awarded to Dr. Neddermeyer "For participating in the discovery of the positron, for his share in the discovery of the muon, the first of the subatomic particles; for his invention of the implosion technique for assembling nuclear explosives; and for his ingenuity, foresight, and perseverance in finding solutions for what at first seemed to be unsolvable engineering difficulties."

one of the chapters he talks about Los Alamos. He highlights your work. It all sounds very dramatic. At the time, for you, was it a very exciting period, working out that idea?

NEDDERMEYER: Yes. At the same time, I can't handle that, knowing my own feeling. I curse myself for not making protests. I practically ignored the— Well, I didn't join any of the groups that were writing petitions to get nuclear energy used in a proper way, control the bomb.

GREENBERG: But those sorts of things only came after anyway, right?

NEDDERMEYER: Yes.

GREENBERG: There were no movements to control—

NEDDERMEYER: Well, there were the petitions to not use the bomb on Japan. [Pause] I feel as if what little ability I have, I haven't used very well.

GREENBERG: How did you feel when everybody told you at the beginning that [implosion] was a lousy idea?

NEDDERMEYER: Oh, it didn't bother me particularly. Oppie was not unreceptive to the idea. He didn't walk up with open arms, but he said it was a good idea. There was a lawyer, Ralph Carlisle Smith, who was in charge of the patent office, patent problems. Oppie told Smitty, "There's a guy at the table there—near the stairway—who has a good idea. We should look into it." That was me. So he encouraged looking at it.

No, I didn't have any strong feelings. I don't remember getting tremendously excited. Oh, I don't know. It gave me a funny feeling, to put a layer of explosive around a six-inch diameter, inch-thick-wall steel cylinder and make it from a cylinder into a solid bar. That was almost comical. [Laughter] But— I don't know. I just have this horrible, guilty feeling all the time, like a cur with his tail between his legs. I feel as if I didn't belong in that society. I'm not the least bit interested in who gets credit for what.

[Tape ends]