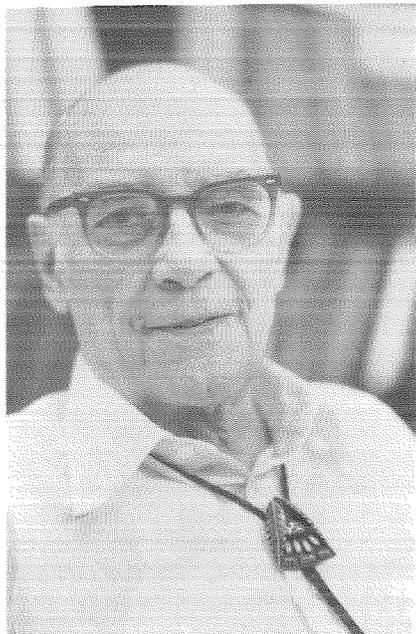


Carl D. Anderson – How It Was



Carl Anderson enrolled as a freshman at Caltech in 1923; in 1976 he retired as Board of Trustees Professor of Physics, Emeritus. In the 53 years between he was active in all the standard academic pursuits, plus a few that were not so standard – winning a Nobel Prize in 1936 at the age of 31, for example, and receiving membership in the National Academy of Sciences two years later. He also served for eight years as chairman of the Division of Physics, Mathematics and Astronomy. Obviously he was a prime candidate to be interviewed for the oral history program of the Institute Archives, and this was done by Harriet Lyle in 1979. E&S presents here an excerpt of his memories of the years 1926 to 1937, the period during which he discovered the positron and the first mesons.

Harriet Lyle: I know you were born in New York City and that you moved to Los Angeles when you were seven. Would you tell me a little bit more about your family?

Carl Anderson: My parents both emigrated from Sweden when they were eighteen or nineteen years old, and my father spent most of his life in the restaurant management business. My grandfather was a farmer in Taby, which is a suburb of Stockholm, Sweden. I visited there in 1926 and saw my paternal grandfather. This was on my Caltech Junior Travel Prize trip.

HL: You had a six-months trip on your travel prize?

CA: It was supposed to be, but we were homesick for California, so we spent probably five months or so actually in Europe. We bought bicycles in Munich with the idea of bicycling up through Germany and into Holland and Belgium and so on, but we never got outside the Munich city limits on them. The trouble was that it rained all the time, and we decided that it was not a very practical way to travel.

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

CA: Yes. I think the prize was \$900, which was just enough to make the trip if you were economical. And then Dr. Noyes slipped us each \$50 just before we left, to spend going up to Gornergrat and the Jungfrau. He loved the mountains in Switzerland. To get to Gornergrat you take a cog-wheel railroad car from Zermatt up to about 10,000 feet. And then you have a 360-degree view of the Alps. We decided to climb Monte Rosa, the second highest peak in Europe.

HL: Were you supposed to meet certain people, or did you just go from city to city on your own?

CA: No, we had no appointments to meet any certain people. But in Munich we did attend a class given by Sommerfeld, who was, as you know, a very famous physicist at that time. We read in the paper

about American Students' Week in Leiden, Holland, and we made only a very slight revision in our itinerary so we could be there during that week. That's where I first met Robert Oppenheimer. He was in Germany at the time and decided to attend American Students' Week.

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

CA: I was majoring in physics when we left to go to Europe, so I was a budding young physicist. All my life, from as early a time as I can remember, I wanted to study electrical engineering. The thing that changed my mind was the third term of the sophomore year at Caltech. There was what they called Section A, which was supposed to be a special section for some of the better students to do the three-term regular physics course in two terms. And then Ira Bowen took the class for the third term and talked about modern physics. It was great, interesting, wonderful, and I learned from him that you could even make a living doing it. So I changed my course to physics, but I got a degree in both engineering and physics because the courses were quite similar.

I might mention that Millikan, as far as I know, always taught a class when he was, in effect but not in name, president of Caltech. He was much more than president, and if there hadn't been a Millikan, there wouldn't have been a Caltech. I'm sure of that. He gave a course called "Electron Theory" to first-year graduate students. In the first three or four minutes, he'd write an equation on the board that had something to do with electron theory. But then he would often begin to reminisce. He wore these pincer glasses that he put on one finger, and would then tell about what happened in 1906, for example, in connection with his working on the oil drop experiment and the day he happened to think of using oil instead of water. It was much more valuable than if he had talked in a formal way about electron theory. You could learn that by reading in a book or hearing somebody else.

HL: What did you do about tests and things like that in a class like this? It

seems that students would be a little worried about what they were expected to learn.

CA: Yes, he did give examinations; his reminiscences, although wonderful, did not occupy the whole time in his class.

Several years later I was in Millikan's office talking to him one day about cosmic rays, and the registrar came in and said, "Dr. Millikan, you gave A's, B's, and C's to your students in your class." And Dr. Millikan said, "Yes. Now take this first man, for example. He was a good student; he wasn't top-notch, though, so I gave him a B." And the registrar said, "Oh, I wasn't questioning your assignment of grades to the students. I was really pointing out that Caltech has the 4-3-2-1 system and not the A-B-C-D system." So then Millikan said, "Well, I could change these letter grades to numbers — or we could change the system at Caltech." I thought it was interesting that Millikan saw two solutions to this problem; he could change the sheet of paper, or the system could be changed.

HL: Of the distinguished people who visited Caltech, are there any that you remember particularly?

CA: Oppenheimer was on the faculty at Caltech and at Berkeley at the same time. So he used to commute and spend one term at Caltech. And Oppenheimer, who later became an extremely eloquent lecturer, was not in those days a good one. He didn't speak loudly enough, and he didn't really face the audience. I took a course in quantum mechanics from him when I was a graduate student, and I had no idea what he was talking about. He paced back and forth, and wherever he happened to be at that instant, he would write some squiggles on the blackboard — part of an equation. The parts were scattered at random all over the blackboard.

I didn't have the background to understand theoretical physics at the level that he was speaking. So I went to his office one day and said I would have to drop his course. He sort of pleaded with me not to, and then he admitted that everyone else in the course had already asked to drop it. I was the last. He really pleaded with me to stay, because he wanted to have an official course at Caltech, and he assured me that everything would be all right at the end of the term. So I stayed registered as a student, and he had an official course. At the end of the term, he asked me what the highest grade and the lowest grades at Caltech were. For an instant I thought of

reversing them, but I didn't. So I told him the highest and lowest grades, which he should have known and probably did. Anyway, I got an A in the course.

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

CA: No, I didn't. It was over my head, all the way through. And I'm not the only one. This was in the days when the first papers on the Dirac theory were being published. Richard Tolman got Oppenheimer to agree to give a series of evening lectures two hours long — three a week, I think — on the Dirac theory, for anybody who wanted to attend. So I attended the first meeting of that series, and Oppenheimer talked for two hours. And at the end, Tolman got up and said, "Robert, I didn't understand a damn thing you said tonight, except . . ." Then he went to the blackboard and wrote an equation. And Oppenheimer replied, "That



A very young Professor J. Robert Oppenheimer

equation is wrong." And there was never a second meeting of this attempt on Oppenheimer's part to tell people what the Dirac theory was all about.

HL: I want to talk a little bit about your graduate work at Caltech, which started in 1927.

CA: Actually it started in 1926, when I was still a senior. Millikan was away on a trip, so I talked to Earnest Watson. I told him that I would like to get started on some research because I didn't have enough to do. So he assigned me to work with Lee DuBridge, who had just come to Caltech as a National Research Council fellow, to work on the photoelectric effect. I guess I worked for him for about

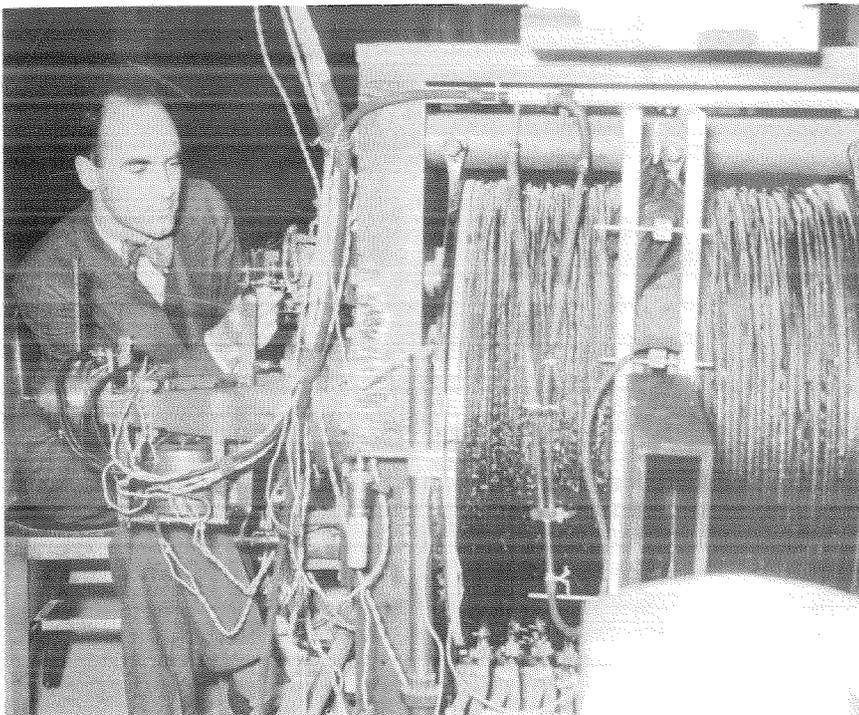
three weeks, building a monochromator for his photoelectric experiment. And then Millikan called me into his office and said that I should be doing something else. I should be working with D. H. Loughridge and not DuBridge. Millikan really assigned research projects, I guess, at least in my experience. So I looked up Loughridge, who was working on the photoelectric effect of X rays with a cloud chamber. He was just finishing up his work for his degree, and he left something like a week or two after I started working for him.

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

CA: Oh, yes, Millikan knew he was going to leave. I guess that's why he felt he needed somebody to carry on that work. I did that for four years, and greatly modified the equipment and did quite a bit more than Loughridge. That was my thesis work. Many months after I started working there, I happened to bump into Millikan and tell him that I didn't have any research adviser, as you're supposed to have. And he said, "Oh, that easy; I'll be your research adviser." So I was his student — although not once during the time that I was a graduate student did I discuss my work with him or was he in my laboratory. So I had a free hand to do things as I wanted.

HL: Millikan didn't come back to check up on you?

CA: No, no. I probably talked with him during those years as a graduate student about my research, but I have no memory of doing so. I do remember my final oral examination was scheduled for nine o'clock in the little seminar room in East Bridge. I reported there on time, and no one else was present. Then E. T. Bell, the mathematician, came in. (I had a minor in math.) He said, "Well, I'll start it off." He asked me about Bessel's equation, and I guess for about 20 minutes or so he questioned me on that. I just happened to know Bessel's equation pretty well, and I wrote it on the board, and he asked me various things about it. Then he said, "Well, that's enough; I'm through." We sat there, and nobody else came in until ten o'clock. I guess they all had classes or something. Bell was interested in the history of mathematics, so I had a delightful 40 minutes or so listening to him tell me all about Bessel's childhood. That part of my PhD exam was very simple.



Carl Anderson in 1933 with the magnet cloud chamber he designed and built to measure the energy of electrons produced by cosmic rays. The instrument was installed in the aeronautics building to take advantage of the powerful (400-kilowatt) generator that provided electricity to operate the wind tunnel.

At ten o'clock several people came in. I can't remember who they all were — I think it was Bowen and Millikan and Paul Epstein. And I made one horrible blunder. Millikan asked me to give a review of the history of the photoelectric effect. Of course, the photoelectric effect is involved with visible light, and Millikan became famous for showing that the Einstein equation applied, as well as for measuring the charge on the electron. Those two things were what he got the Nobel Prize for. Well, I forgot all about light and the photoelectric effect of light. Because it was my thesis topic, I gave a history only of the photoelectric effect of X rays, the experiments and theories and so on, and I ignored or forgot about visible light. I don't know if he ever held it against me or not, but the next morning I met him and he said that that was a corking good examination. It wasn't until later that I realized what a horrible blunder I had made.

HL: Did Millikan have any other students?

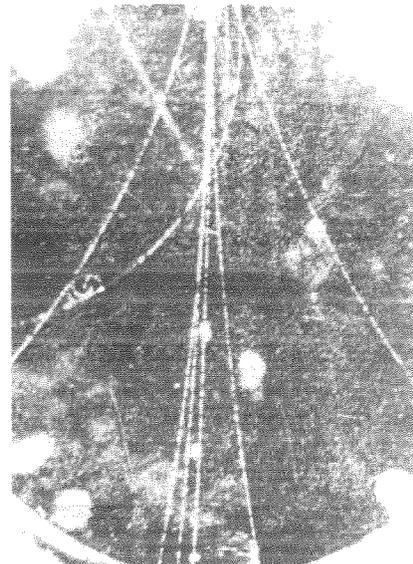
CA: Yes. He was active, and he had a knack of sensing very early what were the important fields of research in physics. He was the first man in the United States, I'm quite sure, who worked with cosmic rays. It turned out in later years that a lot of

physics came out of the study of cosmic rays. And this isn't the only instance. There's the far ultraviolet work that Millikan started. He put Bowen on that when Bowen was his graduate student, and that brought forth all kinds of important new things. He started Charlie Lauritsen on the cold emission — we all know a hot wire will emit electrons, but for cold emission you have a cold wire on which you put a strong electric field to pull out the electrons. That was very new at the time, and led directly to Lauritsen's building the world's first one-million-volt X-ray tube.

One day I asked Millikan directly: "How were you able to sense the importance of fields of physics when they were hardly known to people and nobody was thinking about them? How come you got interested in them?" Millikan's answer was, and he said it as though he was completely serious about it, "I read science abstracts." Well, I told him, "I read science abstracts, too, but I don't get these ideas."

HL: Why did you decide to stay on at Caltech after you had your PhD?

CA: About a year before I was to get my PhD, I went to Millikan and asked him if there was any way I could spend one more year at Caltech. I had two things I wanted



Anderson's cloud chamber unexpectedly produced this photo, which shows the tracks of electrons and positrons. The particles come down from the top and as they move into a magnetic field, the electrons move to the left (there are five electron tracks) and the positrons to the right (there are two positron tracks). Anderson received the Nobel Prize for the discovery of the positron.

to do: One was to learn something about quantum mechanics. I was having a difficult time, and every physicist had to know something about quantum mechanics. And then I had an idea (which grew out of the work I did for my thesis) of working with gamma rays of higher energy than X rays, but with a cloud-chamber technique. In other words, I wanted to study the interaction of gamma rays with matter at as high an energy as I could. And the highest energy gamma rays then available were those from ThC'' , which were 2.6 million electron volts. I was going to shoot those through the cloud chamber in a magnetic field.

HL: You already had this magnetic field?

CA: No, I didn't have a magnetic field for the photoelectric effect. It would mean building new apparatus. Another reason I wanted to do that was that C. Y. Chao, who was a postdoctoral fellow, was working with ThC'' . He was finding anomalous effects of scattering and absorption of gamma rays, but he had no way of observing the details of what he was doing. He was using electroscopes, which sort of integrate things, and measuring intensities at various angles in relation to pieces of lead absorber, and so on. It wasn't known at the time, but he was

actually observing the annihilation radiation of positive electrons.

I went to Millikan to ask if I could spend another year at Caltech doing the same type of work that Chao was doing, but using a cloud chamber, where you could see the details of what's going on. And I'm quite sure that if I had done that, the positive electron would have been discovered before it actually was. That was the direct way, in hindsight, to attack the problem.

Millikan's answer was a very definite no. He said, "You have done your undergraduate work here, you've done your graduate work here. You're getting very provincial. You've got to go somewhere else." The only way you could go somewhere else in those days was to apply for a National Research Council fellowship. So I did. I wrote to Arthur Compton at Chicago and described the proposed experiment to him and said that I had applied for a National Research Council fellowship. He wrote back a very nice letter and said that he would be glad to have me there, and he would do his best in providing facilities, equipment, and some money to build this equipment.

But it never happened. Millikan called me into his office one day and said he wanted me to stay on at Caltech for another year. By that time I had sold myself on the idea of going to Chicago to do this experiment. So I used all of the arguments with Dr. Millikan that he had used on me, and he said, "Yes, that's all true; but your chances of getting a National Research Council fellowship would be very much greater if you had another year at Caltech." It turns out that he was a member of the National Research Council selection committee at the time. So I stayed on at Caltech and worked on this experiment that he wanted me to do, which was quite similar to the one that I wanted to do, except that I wanted to use gamma rays and he wanted me to use cosmic rays.

HL: Did you talk to him any more about the experiment that you wanted to do? Why was he so against it?

CA: Well, he knew that I had used and was familiar with cloud-chamber techniques. As a graduate student, I was measuring mostly the space distribution of X-ray photoelectrons, but to some extent the energy distribution. It was generally believed at that time that the primary cosmic rays from space were like gamma rays, were photons. There was no proof of

that; but Millikan had a theory of the creation of cosmic rays, namely, the atom-building hypothesis — that atoms were being built in free space. A bunch of electrons and protons would, in some very mysterious way, arrange themselves in a certain pattern and then coalesce into an atom, and that would give off a calculable amount of energy, presumably in the form of photons. I didn't believe the theory, and I think most people did not believe it.

What Millikan wanted me to do was to measure the energy of the electrons that were produced by the cosmic rays. He had measured the penetrating power of cosmic rays, and they were much more penetrating than any other radiation known. His hypothesis was that they were gamma-ray-like in character, but of much higher energy. One can measure the energy of the gamma rays by measuring the energy of the electrons — the Compton electrons, in those days. So my job was to build an apparatus to measure the energy of the Compton electrons that were produced by the primary cosmic ray photons.

So I started to build a piece of apparatus. Of course, it took almost a year to build it, and in the very first experiments, it became clear that the picture was much more complicated than what was then thought to be the absorption mechanism of the primary cosmic rays — namely, by Compton electron collisions. Immediately, as many positive particles appeared as negative particles, which said something new was happening. The mere presence of the positively charged particles showed that something different was going on than the Klein-Nishina absorption of gamma rays, which was the process by which gamma rays were absorbed, so far as anybody knew at that time.

HL: Do you think that the experiment that you had designed originally would have been better?

CA: No, I'm not saying that. I think it would have found the positive electron sooner than it was found, because in the experiment I was going to do you knew what the incoming radiation was and you knew its energy. In working with cosmic rays, you didn't know what the radiation was that was coming in; in fact, you knew nothing. You didn't know how the particles interacted with matter or what the particles were that you were observing in the cloud chamber. This was chiefly because the energies were so high that it was impossible in a cloud chamber to learn much more than the momentum of the

particle and its electric charge. Many of the cosmic ray particles have energies of billions of electron volts.

My apparatus was in the aeronautics building, because the magnetic field had to be as strong as one could possibly get — to deflect the cosmic ray particles to a measurable degree. So I designed a magnet to take the full power of the aeronautics department's generator that provided electricity to run the wind tunnel. That was, as I remember, a 400-kilowatt generator, which could be overloaded for periods like an hour or so at 600 kilowatts. So I designed the equipment to handle 600 kilowatts.

HL: How did you do it?

CA: To design a magnet is a very complicated thing. But I knew I had to have magnetic fields that were stronger than you can get by using a magnet of orthodox design because of the saturation effect of iron, which is something like 12,000 gauss. (I guess now you say "webers," or something, but in those days it was gauss.) So what I built was essentially air-core coils, with iron wherever there was any room for it. At the center of the magnet, where the field was the strongest, there had to be a cloud chamber, and you had to be able to see it, which put limitations on the use of iron.

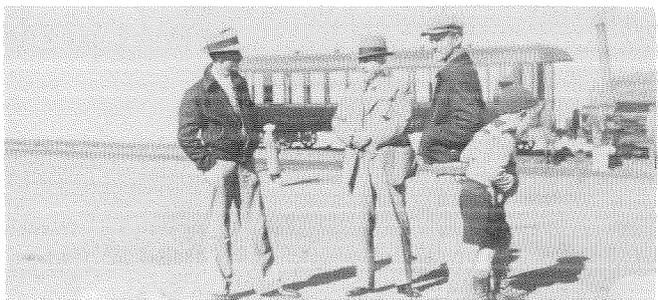
That magnet was used by other people later on, and they thought it was very poorly designed, but they didn't know the purpose it was designed for. As an orthodox magnet, it would have been very poor. But we did get 25,000 gauss over a volume with a diameter of six inches and several inches in depth. It was water-cooled, and we put 40 gallons of water through it a minute. The water came out, not quite but nearly, boiling hot. We were in the aeronautics building, because there's where the generator was, and we were on the third floor because that's where the space was available. And the discharge water used to run out of the magnet into Throop Alley; then it would cross California Boulevard and run down Arden Road. Under certain climatic conditions, there were clouds of steam half a block down Arden. Some of the neighbors objected to that.

HL: Tell me how you found the positron.

CA: That's sort of a long and complicated story. The first thing that came immediately out of the cloud-chamber pictures was a set of high-energy particles of unit electric charge — roughly half posi-



En route to the summit of Pikes Peak in 1935, Anderson and Neddermeyer's 1932 Chevy truck and trailer (rear) are given a tow by a Pikes Peak Company truck.



Anderson (left) and Neddermeyer (right) had a distinguished visitor to their summer outpost at Pikes Peak in 1935 – Robert A. Millikan. The cog-wheel railway car was used to bring tourists up the mountain.

tive and half negative. The fact was that there was no way of knowing anything about the positive particles except that they were positive and had a very high energy. One didn't know what their mass was, for example. But the only known particles of positive charge were protons. So the assumption was that atoms were being broken up by this very high energy radiation into the fundamental building blocks — protons and electrons — the only particles known at that time.

In a cloud chamber, you can, in a magnetic field, make measurements of mass only on slow-moving particles. By slow, I mean moving with a speed of appreciably less than the speed of light. Now, these energies were so high that most of the particles were moving at 90 or 95 percent or more of the speed of light. All you could tell was their charge and momentum. You measured the momentum from the magnetic field and the charge from the density of droplets along the cloud-chamber track. Some of these particles, the positive ones, were moving slowly enough so they should have (if they were protons) exhibited an increase in ionization, which they did not do. Another not very good explanation was that they were electrons going the wrong way, that is, going up. I said to Dr. Millikan, "You wouldn't expect it, but there must be electrons that are going up." These tracks weren't heavy enough to be interpreted as protons.

Millikan said that was ridiculous. They couldn't be moving up — any number of them anyhow — they must be protons. So I decided to put a plate of lead in the cloud chamber, which would prove whether they were moving up or down. Then one day a particle of low energy, so it was very clear that it was moving at much less than the speed of light, went

through the lead plate. In fact, it was moving upward. It was a clear-cut case, and that's when it became clear to me that these positive things were mostly positive electrons and not particles as heavy as protons.

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

CA: Millikan told me to publish. I think he felt there was enough evidence for that. I was going to write a letter to the editor of the *Physical Review*, but he said, "Send it to *Science*, because you can get it in print quicker than in the *Physical Review*." So I sent it to *Science*. But it turns out that all physicists read the *Physical Review*, and only a fraction of them read *Science*. The positive electron was met with disbelief on the whole. Ed McMillan, who was a good friend of mine (he was an undergraduate at Caltech in the class behind me), asked me, "What sort of nonsense is this that you're writing about?" And I read in Dan Kevles's book, *The Physicists*, that Bohr didn't believe it and just passed it off. I heard, too, that Joliot was very angry with me for publishing in *Science*, which he didn't read, instead of the *Physical Review*, because my paper might have helped him with his work.

HL: How was research financed in the 1930's?

CA: My feeling is that Millikan really ran the Institute. Essentially all faculty members in all divisions, if they needed funds for research, went to Millikan and explained their woes and asked for money. He was the one who made the decisions.

The main thing in those days, I think, was that the research that was done did not need the large sums of money that

present-day research does. And people were accustomed to making do with very little money and a lot of individual effort. We used to make regular trips to the Southern California Edison Company junkyard in Alhambra to buy for a song — or sometimes have given to us — a transformer or a switch or something else that we needed for our research. I asked Frank Capra, who was at the height of his career as a film director at Columbia Studios, for a motor generator. And I got one that had been used by the movies to run their klieg lights at various locations. It was mounted on a 1911 Pierce Arrow truck that must have been parked in the desert for many years, because the sun had turned the headlight lenses a beautiful purple. They used acetylene headlights in those days. So the truck was towed to Caltech, and we used the motor generator set to provide power for our magnet.

Another example of a different way to finance research happened in the summer of 1935, which Seth Neddermeyer, my first graduate student, and I spent at the summit of Pikes Peak. We bought a used 1932 1½-ton Chevy truck for \$300 or \$400 and a flatbed trailer from a used trailer lot. A classmate of mine was then an officer of Bekins Moving and Storage Company, and I went to him and said we needed housing for our trailer. He gave us a whole bunch of great big packing cases, and with our own hands and a hammer and a saw we built the trailer housing to protect our apparatus from the elements at Pikes Peak. Our total load, counting the trailer, was over five tons. In a test run, when it was loaded, we ran from California Boulevard up Lake Avenue to Colorado in Pasadena. Now, normally you don't think of that as much of a hill, but it was a stiff second-gear operation for our truck. We made Hope, Arizona, the first day, by

Anderson . . . *continued*

driving all night mostly in first and second gear. But eventually we did get to the foot of Pikes Peak.

HL: Were you planning to be towed up the mountain?

CA: Yes. First we stopped at the Chevrolet agency in Colorado Springs to have the valves ground and change the oil and have a new clutch put in, and then we started out for the mountain itself. We knew very well that we had no chance whatever of getting up the peak under our own power, but we went as far as we could and then managed to get stuck in the middle of the road, where we were blocking traffic. The Pikes Peak Company had quite a bit of equipment to keep the road in repair and keep the snow under control and so on, and they came down with a big company truck, tied that on the front end of our truck, and towed us. With both trucks working as hard as they could, we did get up to the summit.

The Pikes Peak project was a very good thing to do scientifically. It was a great success because the cosmic rays are more intense at higher altitudes, and intensity was one of our major problems. Also cosmic rays have different components, and the components that do the most interesting things increase very rapidly with altitude. The problems leading up to the discovery of the positive electron were resolved in 1932, but there were other particles that didn't behave like electrons, positive or negative, or like protons. They had peculiar properties. I wrote a letter to Dr. Millikan from Pikes Peak, in which I said I thought we had strong evidence for the existence of new particles intermediate in mass between electrons and protons. There were paradoxes in our data, and the existence of new particles would resolve them. But that was a very radical assumption to have to make. We got more cases of tracks of that kind up on Pikes Peak — oh, a hundred times as many as we'd gotten previously in Pasadena. It was an interesting situation, but we did not feel we had enough evidence to publish at that time. But there was enough to write a letter to Dr. Millikan about — and the particles later turned out to be the first mesons, now called mu-mesons or muons. About two years later, in May 1937, Seth Neddermeyer and I, after many more experiments, published our first formal paper announcing their discovery — this time in the *Physical Review*. □