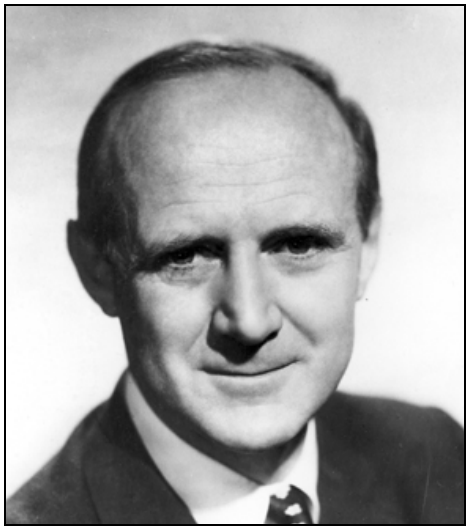

WILLIAM H. PICKERING (I)
(1910–2004)

INTERVIEWED BY
MARY TERRALL

November 7–December 19, 1978

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Subject area

Engineering, physics, administration, Jet Propulsion Laboratory, space flight

Abstract

Interview in four sessions in 1978 with William Hayward Pickering, professor emeritus of electrical engineering at Caltech and director (1954-1976) of the Jet Propulsion Laboratory, begins with recollections of his upbringing in New Zealand. He enters Caltech as an undergraduate in spring term of 1929: influence of A. A. Noyes; travels in Europe during his junior year. Remains at Caltech as a graduate student in electrical engineering and then joins the faculty. Recollections of life at Caltech during the Depression and the war years, including emphasis on power transmission in its electrical engineering department, under Royal Sorensen, and subsequent expanding into electronics. Recalls his work with H. Victor Neher and R. A. Millikan on balloon-flight studies of cosmic rays; travels with them to India and Mexico. Contrasts leadership of Millikan and Lee A. DuBridge. Comments on barrage of Japanese incendiary balloons during the war. Early history of JPL: Theodore von Kármán, H. S. Tsien, Frank J. Malina. Long-range missile development for US Army; JPL's collaboration with Wernher von Braun at Redstone Arsenal, Huntsville, Ala. Advent of *Sputnik* (1957); competition with Soviet Union. JPL's move into space program with *Explorer 1*, first US satellite; establishment of NASA (1958); JPL becomes a NASA lab, administered by Caltech. Ranger program (lunar

probes); *Ranger 6* video failure; success of *Ranger 7* (1964). Simultaneous progress of planetary and lunar exploration programs; *Mariner 2* (1962 Venus fly-by); *Surveyor* series (moon); *Surveyor* as precursor to Apollo program. His reflections on JPL directorship; JPL's relations with Caltech; advantages of being administered by Caltech instead of by NASA as a civil service laboratory. The interview concludes with his comments on his post-retirement work setting up an applied research institute at Saudi Arabia's University of Petroleum and Minerals.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH WILLIAM H. PICKERING (I)

BY MARY TERRALL

PASADENA, CALIFORNIA

Caltech Archives, 1981

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with William H. Pickering **by Mary Terrall**

Pasadena, California

Session 1	November 7, 1978
Session 2	November 29, 1978
Session 3	December 12, 1978
Session 4	December 19, 1978

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TERRALL: I know you were born in New Zealand. Can you tell me what your parents did there?

PICKERING: Yes, my father was a pharmacist in Wellington. I was born in Wellington [December 24, 1910]. When I was four years old, my mother died. So I was then sent to live with my grandmother Pickering in a small town called Havelock. I went to primary school in this small town. It's sort of an interesting coincidence that this primary school was the same one that Ernest Rutherford went to. And Rutherford, of course, later became a nuclear physicist; in fact, he was one of the very prominent early nuclear physicists.

By the time I finished primary school, my father was working in the health department in British Samoa, and rather than take me over there, he arranged for me to go to high school in Wellington. It was a boarding school, so part of the time I was boarding and part of the time I was staying with some friends.

TERRALL: You didn't have any other family besides your grandmother?

PICKERING: I had a younger brother, who died of diphtheria when he was about four or five. My father had lots of brothers and sisters—there were six or eight of them all together, I guess—and

they were spread around the country. But there wasn't any family in Wellington. So I stayed with these good friends—a man by the name of Rowe, who was a banker and had a son about the same age I was. They took me in, and I lived with them for I guess a couple of years, and then I boarded a couple of years at the high school, that sort of thing.

TERRALL: So it was a private school?

PICKERING: No, a public school. It was called Wellington College—the terminology, of course, in the English system is a little different from ours; Wellington College means a high school in our terms. This was a public school. There must have been, say, 1,200 students and perhaps 200 or 300 of them were boarders.

TERRALL: Can you place your first interest in science back then?

PICKERING: Yes, I had an interest when I was still in primary school. About 1920, I got fascinated with radio, which was just then coming out, and read about crystal sets. I remember discussing this with my grandmother, and she said, well, she had an old broken glass which was a nice piece of crystal. [Laughter] I also can remember reading—in some magazine, I suppose—about the fact that in 1924 Mars was going to be in a close conjunction with the earth. An astronomer—I've forgotten now who it was—had some scheme to go down into South America, where there was a vertical mine shaft, and he was going to spin a large dish of mercury at the bottom, which would make a parabolic surface, which would allow him to focus on Mars as it went across the entrance of the mine shaft; and he was going to get a real close-up look at Mars. Well, it was a wild idea, and as far as I know, it never materialized. But I can remember reading about this and thinking that this particular conjunction of Mars was going to be the closest conjunction for a lot of years and hopefully scientists were going to learn more about Mars. That was about the time I went to high school, I guess. So, yes, I had an interest in science in those days. I became a lab assistant, which meant sweeping up the labs and that sort of thing, before I got through high school.

TERRALL: Did you play around with trying to build radios?

PICKERING: Oh, yes. In fact, there at high school we put together one of the very early amateur radio stations in New Zealand. Another fellow and I really did it—a fellow by the name of Fred White, who stayed with radio and became quite prominent in radio research. He worked in England for a while and then Australia. Back in those days, you had to build it essentially all by yourself, out of bits and pieces around the laboratory. You didn't buy very much stuff. We used to be able to communicate with stations in this country, for example. That was back in the days when you had to use Morse Code.

TERRALL: Were you learning from magazines? Would that have been the sort of source that you were using?

PICKERING: Yes. I don't think we got much from our instructors in high school; we got most of it out of magazines.

TERRALL: Did you have teachers who were interested in and involved in these projects?

PICKERING: Oh, yes, we did. One teacher in particular, a man by the name of Gifford. He was really a math teacher, but he was very interested in astronomy. He interested me in science a great deal. In fact, interestingly enough, his son is now a doctor and came up to this country not too many years ago, and I visited him. He was just passing through, but he was in this area for a few days.

TERRALL: So were you thinking of going on with science?

PICKERING: Yes. I was interested in radio and electronics—the physics side rather than the chemistry side of science. Astronomy also interested me. When I got through with high school, my father was back in New Zealand. He'd remarried, and we were living in Christchurch, so I started in at the university down there. My interests at that time were leaning towards engineering; electrical engineering is what I thought I wanted to do.

TERRALL: How strong was the engineering department there?

PICKERING: It was pretty good. The University of New Zealand did try to maintain good standards. In fact, in the early days—and I think even when I was there—they were still sending their final examinations back to England to be graded, so that they would be on a par with the British universities. In fact, there was a legend around the high school about one of our teachers, that the only way he got his degree was that the ship sank on the way back. [Laughter] Anyway, I started at Canterbury College, but I only spent one year there; then I came over here.

TERRALL: How did you happen to come over here?

PICKERING: Well, at that time I had an uncle, really a great-uncle, who was living in Los Angeles. But he also had a house in New Zealand, and he used to go back and forth. I spent quite a bit of time with him, because his home in New Zealand was a working farm. I used to go up on the farm and work during the summers. This man had been a mining engineer in South Africa; in fact, he had worked with Cecil Rhodes in Rhodesia in the early days as a civil engineer. He was retired, but, as I say, he had a place here and a place down there. He suggested that I ought to come over to America for my education. My thoughts had been that I would get a degree from New Zealand and then do graduate work in either England or in the US. But he suggested that I come back with him. He lived in Los Angeles and asked why not come to Caltech?

This was in 1928, you see, so it was in the early days of Caltech. I'd never heard of Caltech. A friend of my father's who happened to be an MIT graduate was down there, so I went around and talked to him. And he said, well, of course, if I didn't want to go to MIT, he thought Caltech was a pretty good school.

TERRALL: What did your uncle know of Caltech? Did he have any direct contact with it, or did he just know it was there?

PICKERING: He just knew it was there. So I said OK, and I wrote to Caltech. They, of course, said, "Well, you've got to take exams, you've got to do this, that, and the other thing." So my

uncle said, “Well, why don’t you just come on over and try and get in when you get over here.” So we did. I came in, then, as an immigrant rather than as a student. This was in March of 1929, so it was the end of the second term at Caltech. They weren’t quite sure what to do with me, so they gave me some of the freshman exams at the end of the second term. I took those exams, and on the strength of that, they admitted me to take the third term of the freshman year and then go on. So that’s what I did. I started out in electrical engineering, with the expectation that I would graduate in that field and then go back down to New Zealand and work down there as an electrical engineer. But [Robert Andrews] Millikan and [Arthur Amos] Noyes wanted me to change over to science. So I got my bachelor’s degree in science [1932], the master’s degree in physics [1933], and my doctor’s degree in physics [1936], with a minor in electrical engineering.

TERRALL: When you first got here, what was it like living in Pasadena and being at Caltech?

PICKERING: Well, it wasn’t so different, really, from New Zealand. After all, New Zealand is another pioneering country, and California, particularly in those days, was a long way out in the West. It was obviously different as a matter of scale and so forth, because this is a much bigger country and many more people and so on. But I fitted into the educational system without any great trauma and found that I could do the work without any trouble—well, not without any trouble, but at least I fitted right into it. In other words, my background in high school down there and the one year that I’d had at Canterbury made me able to go into this third term of the freshman year without any problem. And I guess, in fact, I came out with honors standing in the sophomore year. In those days, they used to have an honors section.

TERRALL: Were you living with your uncle, then?

PICKERING: No, I lived in a boardinghouse on South Wilson, because the dormitories were not yet built. The last year of my undergraduate term, I moved into Dabney; that was the year that they opened the houses.

TERRALL: How did you react to the Caltech environment intellectually, having all these high-powered scientists around?

PICKERING: Oh, I don't know. I obviously enjoyed it. I did quite well in my studies. I very much enjoyed the place and working here with these people. A year or two after I graduated, I did go back to New Zealand, looking for a job. But that was in the middle of the Depression, and there weren't any jobs. Caltech wanted me to stay on anyway, so that's what I did. I also asked myself, "Why not? After all, there are lots of people who work hard to try and get a job at Caltech and spend most of their time trying to move to Pasadena. If I'm already in Pasadena, why not stay here?"

TERRALL: Now, you were married just around the time of graduation. Was your wife a local girl?

PICKERING: Yes, she was a sister of a fellow student who was in the same boardinghouse that I was in.

TERRALL: And she was not averse to staying in Pasadena, probably?

PICKERING: No. Although, at the time we were married, the probability was that I was going back to New Zealand, and she was not averse to that either. In her childhood, she had lived in lots of places in the Southwest. Her father was in the water development business, and so he was going from one place to another, and the family was always on the move. The problem of moving didn't bother her any; in fact, she was remarking just the other day that when we were married she thought we would be doing a lot of moving around, and in point of fact we didn't.

TERRALL: As an undergraduate, did you have contact with Millikan?

PICKERING: No, not very much. As a graduate I did, but as an undergraduate I don't really remember any specific contact.

TERRALL: As an undergraduate, when you were working in electrical engineering, do you remember who the people were who were particularly good teachers?

PICKERING: Sorensen was, and [Francis W.] Maxstadt, and [Samuel Stuart] Mackeown. Mackeown was teaching vacuum tube and radio. In those days, electrical engineering was almost entirely power engineering. In fact, just to sort of jump ahead a little bit, when I did get into the electrical engineering department in the late thirties, I was fighting to get some electronics into the department. And I succeeded; I did get some work done. But when I retired [1976] from JPL [the Jet Propulsion Laboratory] and came back here and had a look at the electrical engineering department—in point of fact I didn't really go back and do any teaching; but if I had, I would have been fighting with them to get some power engineering into the department. Because they have over-reacted. This happens in all academic areas. The academic work is always a little bit out of phase with the needs of industry.

TERRALL: But at that time there wasn't really any electronics?

PICKERING: Well, Sorensen, who was heading the department, was a power engineer. The department had done some very useful work in helping the Edison Company develop the first long-distance transmission lines—from Huntington Lake, up near Fresno, down here. That was one of the first high-voltage transmission lines, and its development was done with the help of our EE department. Sorensen got the High Voltage Research Laboratory out of that, which was a very important laboratory. In fact, there was a test transmission line that went from the high-voltage lab out to the present site of the Athenaeum; there was nothing out there but orange groves at that time. So it was natural that the department was very heavily oriented towards power engineering. Radio communications and vacuum tubes were considered not very important. It was a problem to get the department turned around to recognize that there was a whole new technology growing up here.

TERRALL: Were there other students in your position, who had gotten interested in radio?

PICKERING: Oh, yes. We had a little amateur radio station. There was one professor, Mackeown, who was a good teacher, who was teaching one vacuum tube course; but we needed more.

TERRALL: Could you talk to New Zealand with your amateur radio?

PICKERING: Interesting question. I don't remember.

TERRALL: You decided to go to graduate school in physics. I guess there was no problem in getting admitted, since everyone seemed to have wanted you to stay?

PICKERING: That's right, they wanted me to stay. I had taken some extra undergraduate work, which gave me a degree in science. Let me go back a bit. Professor Noyes became interested in me because at that time there was an undergraduate travel prize, which was a quite prestigious thing, for which two juniors were picked. They left in March of their junior year and came back in time for school in September, so they had roughly six months in Europe. This was an anonymous prize, but my assumption, and I think everybody's assumption, was that it was given by Noyes, who was head of the chemistry department. They would pick about a half dozen people from the sophomore honors section, and in preparation for this trip a man in the humanities division by the name of [John] MacArthur would take the candidates for about six months before the trip and give them some special courses on the art and culture of Europe. I was selected as one of this group at the end of my sophomore year, and Noyes became interested in me.

TERRALL: So there would have been a small group of people, out of which they chose two to go.

PICKERING: Yes, that's what happened. We left in March [1931] and came back in September. So we lost the last term of the junior year. We traveled during this period, and we made a report to a student body assembly when we got back.

TERRALL: Did you travel with the other person who was selected?

PICKERING: Yes. As a matter of fact, traditionally, if some of the other candidates wanted to pay their own way, they were invited to come. Now, in our particular case, we had two other people

with us, so four of us traveled together. It was a very interesting and educational experience. We started out by going to Detroit and buying a Ford, fresh out of the factory, and driving that down to Washington first, then up to New York, and then shipping it to Europe, driving through Europe, and then eventually driving back all the way to Pasadena.

TERRALL: Did you have introductions from Caltech people to people in Europe?

PICKERING: No, we had some records and diaries of previous travel-prize winners. This exercise that we'd gone through with MacArthur was supposed to lead us to all of the worthwhile museums and cultural things of various sorts that we ought to see. The emphasis was very much on cultural activities, not particularly on scientific activities. Although it seems to me that we did visit one or two factories. But there really wasn't any tie-in to the universities.

TERRALL: You were very much on your own?

PICKERING: We were on our own, yes; in fact, that was the point of it. We were supposed to be on our own and supposed to make a worthwhile trip through Europe and come back and tell other people what we did.

TERRALL: It sounds wonderful.

PICKERING: It was. It was very nice. We got as far as Budapest, and up into Germany, Czechoslovakia. The Depression had gotten started, but Hitler really hadn't done anything yet. We got down to Naples, and I forget how far north we went in England.

TERRALL: You went around England as well?

PICKERING: Yes, we did.

TERRALL: You had never been to England or anything?

PICKERING: No.

TERRALL: Did you have family there?

PICKERING: No very close relatives. I do have some second cousins or something, but we didn't visit any. No, we were on our own. As I say, we didn't have any particular introductions and we didn't have any particular objective of seeing scientific or educational things. At that time, by the way, Caltech did have a strong notion that in developing its student activities it should draw on experiences of other universities. The reason I'm bringing this up is that in building the dormitories, the question as to how the dormitories were going to be managed and run obviously came up. At that time, Caltech had some fraternities, which were not part of any national system—they'd grown up just as local fraternities; all schools then had fraternities. The question was, "What are you going to do with the fraternities? Are you going to abolish them?" Also, another question was, "How are you going to actually furnish these dormitories?" Caltech asked the students for advice in these areas, and student committees were formed to help them with the selection of furniture. One student committee was formed to make a trip to England and see how some of those schools ran their dormitories and so on. So Caltech was interested in developing relations with European universities. And when I come to think of it, I guess it's a little surprising that on this travel prize, we weren't instructed to go visit universities. But we were not; we did not.

TERRALL: So [the universities trip] was a separate trip that they sent people on?

PICKERING: Yes. Two people, I think, went over and visited two or three universities and came back and reported on how they were managed. Of course, Caltech eventually proposed a system in which the fraternities were abolished, although in point of fact the fraternities were permitted to move into a house as a unit.

TERRALL: So when you got back, you made some sort of presentation to the student body?

PICKERING: Yes, just a student body assembly.

TERRALL: You said that Noyes had gotten interested in you.

PICKERING: Yes, and I'm forgetting my chronology here; because one of the things I was missing was sophomore chemistry, which I hadn't taken, as an engineer. It ended up that I took this chemistry as a summer course between my sophomore and junior years. This was something that Noyes arranged. Noyes had a house down at Corona del Mar. He had arranged, in fact, for Caltech to buy the Marine Lab down there, and that had just happened. Another member of the chemistry department, Professor [Ernest] Swift, had a summer home down at Corona del Mar. Swift used to teach the sophomore chemistry, and Noyes arranged that Swift would teach a special course in sophomore chemistry during the summer, down at Corona del Mar. There were about four or five of us who took the course. We had a fine time, because we lived in the Marine Lab there—slept out on the balcony. Noyes gave us a canoe, so that we could do a little paddling around the bay. Swift would come down and teach us chemistry. It was a very pleasant summer.

TERRALL: So you actually knew Noyes, but not through taking chemistry from him?

PICKERING: No, I never had him as a teacher. But I met him because he was the man who gave this travel prize, and so he undoubtedly wanted to know something about the people. And then, as I say, he arranged this summer program. But during that summer program we didn't see much of Noyes. In fact, I'm not even sure he was down at his house very much that summer. Swift was the man we worked with.

TERRALL: When you got into graduate school and you started putting more emphasis on physics, did you start working on cosmic rays right away?

PICKERING: Well, pretty soon. What happened was that I, first of all, did a little bit of work assisting Vic [Henry Victor] Neher, who was doing some electron scattering work. I did a little bit of work with him but not very much. Then Millikan got me interested in the problem of building Geiger counters. At that time, the Geiger counter was just coming into existence as a

useful instrument for measurement. So I started building Geiger counters and measuring cosmic rays and developing circuitry so that you could measure coincidences between counters, and so on.

Fig. 1. William Pickering uses an experimental telescope to investigate the source and intensity of cosmic rays on the roof of Robinson Laboratory of Astrophysics, ca 1939-1942. Electrical circuitry linking three cylindrical ion chambers, mounted parallel to the telescope's axis, allows the simultaneous detection and measurement of high-energy cosmic ray particles. Caltech Archives.

TERRALL: Were other people on campus using Geiger counters or building them?

PICKERING: No, I had the first Geiger counters on campus. In fact, I gave the first counters to Carl Anderson to put over his cloud chamber, because it was pretty clear that once we had developed this coincidence circuitry, you determined when a cosmic ray went through a pair of counters, and then it was obviously simple to say, "Let's put the cloud chamber in the middle, and then we'll take a picture." Up till then, Carl had just been taking pictures at random with the cloud chamber.

TERRALL: So he didn't really know what he was taking pictures of?

PICKERING: He didn't know whether any particles were in the chamber. By putting a counter above and below the chamber, and using coincident discharges to trigger the chamber, we opened up a whole new technique.

TERRALL: What about other universities? Were Geiger counters being used experimentally?

PICKERING: Well, they were just beginning to be.

TERRALL: Did you have contact with anyone else in the process of developing them?

PICKERING: Yes, there were a few other people. But for the most part, we were developing it ourselves. Neher and I tended to find ourselves working more closely together. He was a very good experimental physicist and contributed a lot to the development of the counters and the circuits that we were using. Also, at that time he had gotten into cosmic ray work directly, with Millikan. He was building cosmic ray electroscopes for Millikan. Then he had gone off with Millikan on some cosmic ray trips and generally had gotten into the experimental end of cosmic rays. My work supplemented his, because the electroscope and the Geiger counter were two different techniques for measuring the cosmic rays. So we both worked with Millikan, and we went on trips together.

TERRALL: Was the Geiger counter something that was sent up in balloons?

PICKERING: Yes.

TERRALL: Was weight a problem?

PICKERING: Well, yes, we had to make everything as light as we could, because we only had small balloons in those days. But the Geiger counter lent itself very easily to sending a radio signal back to Earth. It was really easier to record by radio than the electroscope was. So the first balloon work was done with Geiger counters [Pickering later corrects himself on this point—Ed.].

TERRALL: Were electroscopes used in balloons?

PICKERING: Yes.

TERRALL: I thought they were just used in the sea voyages. So they were sent up also.

PICKERING: Yes. But the Geiger counters were the first.

TERRALL: I see. Now, what about the problem of recovering the instrument?

PICKERING: Well, we liked to get them back, but if we didn't get them back, we didn't.

TERRALL: But what kind of recovery rate was there? Did you get a lot of them back?

PICKERING: We did, as a matter of fact. We did our first balloon flights from the lab here, from the roof of Bridge [Norman Bridge Laboratory of Physics]. Actually, the very first flight that we made, we got back about two weeks later. It had come down near Blythe [on the Arizona border]. It was about ten miles off the road, and I don't know how the man found it, but he found it and sent it back to us. Flights made from Pasadena generally went off to the east, but we had them go in all directions. We had one that was fished out of the ocean down by Long Beach. We had one that landed in a swimming pool out at Arcadia. This was amusing, because we sent

the thing up late one afternoon; it had drifted away east, and then it had gone up higher and gotten into a wind current which brought it back to the west. Incidentally, what we did was to have two balloons, and one balloon would always burst before the other one. As soon as one balloon had burst, it didn't have enough lift, so it would start coming down and that took the pressure off the second balloon. So generally, it would come down gently, all the way down to the ground. So we knew it had burst fairly close overhead, and that it would land not too far from Pasadena. But it was dark by then, so we didn't know what would happen. Well, the next morning, here's a guy knocking at the door with our instruments. And he said he was the caretaker out at the Arcadia municipal swimming pool. He was locking up for the night, and he heard a splash out there, and he wondered what it was; he thought some kid was out there. He went out and saw this thing in the water, and there's a piece of cloth tape going up in the air. He said he didn't see what was up there, so he pulled it, and after a while the balloon came down. [Laughter]

TERRALL: He must have been shocked.

PICKERING: Yes, he was shocked.

TERRALL: But were you monitoring the radio signals until the balloon burst?

PICKERING: Yes, and even on the way down we were monitoring as much as we could. We had just simple direction findings, so we knew in a general way where the balloons were. I don't know what our recovery rate was, but it would certainly be more than half.

Now, we went off to do experiments in other parts of the world. The most ambitious trip was when I went to India in 1939. Millikan was developing a theory that had to do with the creation of cosmic rays. His theory required, in effect, a measurement of the energy of these high-energy particles. Essentially what we were doing was using the earth as a great magnet to sort out the energies. So we wanted to make measurements near the magnetic equator and other latitudes. And the particular reason for going to India was that the magnetic field of the earth is not quite symmetrical—it's stronger on that side than it is on the other side. So we wanted to make measurements there. We did, in fact, make measurements at three places: Peshawar, which

is up by the Khyber Pass, Agra, and Bangalore. This was obviously a major expedition. Vic Neher and I went over there with Dr. and Mrs. Millikan. Even on that trip we got a fair number of our balloons back. I don't remember how many we launched, but there was quite a batch of them. We must have launched a total of twenty or twenty-five.

TERRALL: How high were the balloons going at that time?

PICKERING: Oh, about 100,000 feet.

TERRALL: I want to go back a little bit, to talk generally about what it was like to collaborate with Millikan on these projects.

PICKERING: Well, he was a very interesting man, and I have a great deal of respect for him, of course. I think he did a magnificent job in setting up Caltech and setting the standards for Caltech and getting the kind of people that he did. To travel with him was a very interesting experience. He was a man with an insatiable interest in and curiosity about everything around him. He was a man who enjoyed very good physical health. He would eat anything. He had stamina that was sometimes beyond what Vic and I had. I remember times when we were down in Mexico. We would drive the truck all day, and we would stop someplace for the night. Vic and I just wanted to collapse, but Millikan wanted to go off and see what the town was like or get to work on something. He was an amazing man. In fact, my wife tells a story about those trips: On this particular trip, my wife and I and Vic and Dr. Millikan were in a truck. By now the war had started. This was December '41—in fact, Pearl Harbor happened two days after we were into Mexico. Anyway, Dr. Neher was doing some work at MIT at the Radiation Lab. My wife and I drove the truck from Pasadena to Laredo, Texas—this was a panel truck with instruments in it—and then at Laredo Dr. Millikan and Dr. Neher joined us, and we drove on down into Mexico. Well, anyway, my wife tells the story that one day Dr. Neher was suffering from a migraine headache, and of course migraines are a miserable sort of a thing. So Millikan was commiserating with him a bit. Then she and Millikan were riding in the back of the truck for a while, and Millikan turned to her and said, "You know, I don't think I ever had a headache." And nothing more was said for a while. Then after a while he says, "Yes, I did have a headache,

when I was about twelve years old." [Laughter] On an occasion, he said, when he drank too much coffee. But he did enjoy very good health, up until about a year before his death.

Begin Tape 1, Side 2

TERRALL: Did working with a person like that make you feel that you always had to do more? Was it a very high-pressure thing, since he put so many hours into his working day?

PICKERING: Yes, that's right, he put so many hours into a working day, and had so much enthusiasm for the work that the rest of us did, too.

TERRALL: Was it hard to keep up that enthusiasm, though?

PICKERING: No, it was easy to work with him. His personality, of course, was very good. He was called the Chief, of course, by the people under him. He really did inspire people to work with him.

TERRALL: So there weren't any feelings of resentment or tension?

PICKERING: No.

TERRALL: What about Neher?

PICKERING: Oh, he's a very good man. As I said, he was a first-class experimentalist. He could build things; he just had a knack for building good things. He was a very good man in the student laboratories as well as building research equipment. Yes, we've kept in touch with him. In fact, as a bit of history, you really ought to talk with him sometime. He's living now up near Watsonville. He retired and decided to build his own house. I admire him for this. After all, he's in his late sixties by now; he's two or three years older than I am. He is building with his own hands this very nice house.

TERRALL: When you first started going on these trips, I think I read about one voyage across the

Pacific to New Zealand. Did you accompany the instruments on that?

PICKERING: Well, actually, I went down to New Zealand while I was still a graduate student; in fact, I went down to New Zealand to see if I could find a job. I did take some Geiger counters along with me to make some measurements on the ship on the way down. Then, when we went off to India, we also made some measurements on the ship on that trip.

TERRALL: On that trip to New Zealand, did you find any variation in the intensity of the cosmic rays?

PICKERING: Yes, there was a so-called latitude effect, which we measured; there's a dip as you get down near the equator.

TERRALL: Did that fit in with the theory that Millikan wanted to test in India? Or was that a different test?

PICKERING: Well, no, it was related to it, in the sense that it showed that in point of fact in order to reach the earth at the equator, the rays would have to have higher energy than to reach the earth at a higher latitude. All it said is that there were fewer rays of higher energy than there were of lower energy. Now, the difference is only a few percent. The interpretation of this difference is a complex thing, but it is an indication that you are in fact measuring the energy spectrum, as it were, of the rays. Now, to do this properly, you really have to measure the energy at very high altitudes, where the situation is not complicated by the rays interacting with the atmosphere. So that's one of the reasons we did the balloon experiments. The sea-level measurements tell you that there's an effect, but it doesn't really tell you very much.

TERRALL: When did Millikan first come up with the theory of the magnetic field of the earth affecting the cosmic rays?

PICKERING: Well, I'm not sure that he did come up with that. He was one of the early experimenters with cosmic rays, because he had developed this electroscope along with Vic

Neher. He had found that there was an altitude effect, and he was interested in that. They would go to a high altitude and then lower the electroscope beneath a lake, so that the mass equivalent of the water would be equivalent to the mass of air down at sea level, and compare that with the sea-level data. In fact, they went up to Lake Arrowhead—that was the first place where they did this experiment. Yes, he was interested in trying to understand the properties of the rays as evidenced by these altitude effects.

Now, the latitude effect, I think, was first proposed by [Manuel Sandoval] Vallarta, down in Mexico City. He did an analysis of the effect of the earth's magnetic field on the radiation and pointed out that if these rays came from outside the earth, as Millikan was pretty obviously showing, and if they were charged particles coming through the earth's magnetic field, the energy required to reach the surface would be a function of latitude. So, various people then started trying to measure this latitude effect.

TERRALL: At that time, it wasn't really known what the particles were.

PICKERING: No. There was a question as to whether they were photons or charged particles. And various things had been discovered. For example, an east-west effect had been discovered, which again, is related to the magnetic field and to the charges on the particles. As soon as you start getting an interaction between the rays and the earth's magnetic field, then of course they have to be charged particles. But at first there was some debate as to whether they were charged particles or photons.

TERRALL: So by the time you went to India, it was pretty well agreed that they were charged particles?

PICKERING: Oh, yes. There was argument as to whether they were protons or electrons. But they were certainly charged particles.

TERRALL: When you went on this trip to India, did your wife go along with you?

PICKERING: She went as far as New Zealand. By that time, we had had our first baby, so she

took the baby down to New Zealand and stayed there while I went on to India, and then she met me coming back.

TERRALL: Did you stop in other places to collect data?

PICKERING: What had happened was that Millikan and Mrs. Millikan went down to Australia ahead of us, because he was going to give some lectures in some universities down there. We were supposed to leave in August, and we weren't quite ready to leave, so we delayed a month. During that time, the war in Europe broke out. So we telegraphed Millikan: What do we do now? "Just come on." So we came on. We sailed in a British ship from Vancouver to Honolulu, Fiji, New Zealand, and Australia. This ship carried some refugees from the first ship that was sunk by a submarine in the Atlantic. It was a little tense on the ship, because there were rumors that there were submarines in the Pacific also. In fact, the first stop the ship normally made was Victoria, to pick up any passengers, and then go on. On this particular trip, they did not go into the harbor at Victoria; they picked them up out in the channel, and the ship was blacked out at the time we did it. So we knew there was a war on. Also on that trip, when they had boat drills, everybody took it pretty seriously. As a matter of fact, that particular ship was sunk by a mine, off the coast of New Zealand, about a year later.

Anyway, we didn't have any troubles. When we got down to Sydney, the Millikans were there, and we were informed that since there was a war on, our tickets for the rest of the trip were, in effect, canceled. Although for the next leg, which was to be from Sydney to Singapore, the shipping line—which happened to be Dutch—said they'd take us up to Singapore, but after that we were on our own. And so we boarded. Now, on the way up, we did in fact launch one balloon, when we were in port in Java—Djakarta, I guess they call it now. We launched one flight, which, if I remember, wasn't very successful—but at least we launched it.

TERRALL: So you knew you were going to get to Singapore, but you didn't know after that how you were going to get to India?

PICKERING: That's right. What happened was we spent four days or so in Singapore. Then we found a little British freighter that was going to Calcutta, so we took that. Well, everything went

on all right, although we had originally planned to come on around the world, but by the time we were ready to leave India the war in the Mediterranean was beginning to get pretty hot. We didn't want to go back through the Mediterranean; in fact, we'd have had great difficulty, I think, if we'd wanted to. So we ended up by coming back across the Pacific. We got an Italian boat as far as Manila, and then one of the old Dollar Line American ships from Manila back.

TERRALL: How long were you actually in India?

PICKERING: On the order of two or three months.

TERRALL: I presume it was all arranged before you got over there, in terms of where you were going to go and so on?

PICKERING: Oh, pretty much. Millikan had things taken care of. And on this trip, of course, there was a lot of contact with other organizations. For example, the Indian meteorological service worked with us. They provided us with the hydrogen to send up our balloons. They had not done any high-altitude work. In fact, one of the by-products of this was that we made some of the first measurements of real jet stream winds. It turned out that we were launching from Agra in December. That time of year is very pleasant over there. It's clear and sunny and just pleasantly warm, and there's very little wind at the surface. So the balloons would go up vertically for a while. Then they'd get in the jet stream and go tearing off to the east. We were able to track them, from the instruments that the weather bureau had, and measure these velocities. We measured a 200-mile-an-hour wind stream once, which is exceptional but not that unusual in a jet stream. These meteorologists were fascinated, because they had never realized that that was what was going on.

TERRALL: Were there any problems involved in collaborating with them?

PICKERING: No, we got along quite well with them. They assigned a couple of people to work with us. Agra was the headquarters, and when we went from there to Peshawar and down to Bangalore, they sent people with us. In Bangalore, we had a very pleasant stay at the Indian

Institute of Science. The director was C. V. Raman, who was a Nobel Prize winner, the first Nobel Prize winner from India. Millikan and Raman had met and had known each other; in fact, I think Raman had been in Pasadena. He was our host while we were in Bangalore, and that was very nice. I was back in Bangalore, incidentally, not too long ago, and met a man whose father had given him a movie he had taken when we were launching our balloons. So he showed me this old movie.

So, the Indian trip was in l939-l940; then when we came back Millikan wanted to get some more data, down in Mexico.

TERRALL: Why Mexico?

PICKERING: Well, we had to get to certain latitudes. He was interested in taking measurements at a series of specific latitudes. So we went into Mexico on December 4th or thereabouts. Actually, on Pearl Harbor day, which was the 7th of December, Millikan and Neher and I were out flying a balloon. We were staying at a place called Victoria, which is about fifty or seventy-five miles south of the border, and we had gone out into the brush there, so as to get away from the town to make our balloon flights. When we came back at the end of the afternoon, of course my wife was in a great tizzy, because she had heard about Pearl Harbor on the radio. The news was in Spanish, but she'd gathered enough about what was going on so that she was pretty shaken. So again we had the same situation as when we were going to go to India—namely, we asked Millikan, "What do we do; do we go home now?" And Millikan said, "Well, no, I don't see any reason to go home. Nothing's going to happen in Pasadena that would make any difference whether I'm there now or there a few weeks from now. Let's stay with the work." So we did.

TERRALL: That was the trip that you drove down in the truck. Did you have any particular adventures on that drive? It's a long drive.

PICKERING: It's a long drive. No, it was pretty routine.

TERRALL: Were the roads fairly passable in those days?

PICKERING: Yes, they were all right. We drove down as far as Acapulco. But we didn't have any trouble.

TERRALL: Did you get good results from that trip?

PICKERING: We got some good measurements, yes.

TERRALL: And they fitted with his theory?

PICKERING: Well, more or less. Millikan's theory had to do with the creation of cosmic rays by annihilation of elements. The data more or less fitted his theory, but actually it turned out that the data were more complicated than that, because there were changes in the cosmic rays as a function of time, because of changes in the solar magnetic field and so forth. So you can't really interpret the data the way Millikan did. In other words, Millikan thought the data proved his theory, but in point of fact things were a bit fuzzy. When you really look at the data in terms of what we know now, they didn't prove his theory.

TERRALL: But this was much later then, that this was realized?

PICKERING: Oh, yes. A few years later.

TERRALL: This would have been '41. Were there other trips after that?

PICKERING: Yes. The last trip was to St. George, Utah. In the middle of the war, when Caltech first got into the rocket business, they were doing some work on rocket fuels in Kellogg [W. K. Kellogg Radiation Laboratory]. They had a major fire or explosion there in Kellogg. It killed one man. A graduate student who was going to go with us on the trip, and we were supposed to have left that day or the day before, was just walking between Throop and Kellogg, and he got caught in the blast. It burned him fairly badly, although he lived. But it obviously upset our plans. We did go off a few days later.

Neher and I had made another trip earlier, to fly electroscopes. I said we flew Geiger counters first; I'm not sure that that's right. I think maybe we did fly electroscopes first. Anyway, in 1940 we had made a trip by train, where we had gone on a north-south line across the country—Bismarck, North Dakota, down to San Antonio, Texas, and someplace in Oklahoma. There was another trip down to San Antonio in September 1941.

TERRALL: Now, had you been responsible for developing the radio technique?

PICKERING: Yes, I had worked on the radio end of it particularly, and the general techniques for getting the signal back and recording it.

TERRALL: This was something that hadn't been done?

PICKERING: No, there hadn't been much of that sort of thing. The closest thing to it was that the weather bureau was beginning to launch weather balloons, which had some instruments. Though I think the instruments had to be recovered in those days. In other words, they scratched on smoky glass and things like that. They got a trace that you then had to recover and look at.

TERRALL: You had to rely on people sending them back to you then?

PICKERING: Yes.

TERRALL: Millikan had been working with sending up balloons way back, right?

PICKERING: Yes, that's right. Some of the early ones you had to send back, yes.

TERRALL: So it was only when you started getting involved that they started using radio?

PICKERING: Yes. They had launched some balloons down in San Antonio, but those were the ones that had to be recovered.

TERRALL: The weather service was not interested in cosmic rays, right?

PICKERING: No, they were just helping. They had instruments and they had hydrogen.

WILLIAM H. PICKERING

SESSION 2

NOVEMBER 29, 1978

Begin Tape 2, Side 1

TERRALL: The last time we finished up by talking about all the cosmic ray expeditions. I wanted to ask you about teaching during that time. Were you teaching undergraduates?

PICKERING: Yes, I was teaching in various places. In fact, for a while I was teaching current events and sophomore history.

TERRALL: How did that happen?

PICKERING: Well, back during the Depression, Caltech was pretty shorthanded. Dr. [William Bennett] Munro, who was running the history department, got me over there. I don't remember how long I was teaching, but I did teach both current events and sophomore history for a while. Then I was teaching some physics and some electrical engineering. In a sense, the most interesting work that I was involved in was just before the war and just at the beginning of the war—the late thirties—where I was trying to get more electronics into the curriculum. Because at that time electrical engineering meant mostly power machinery. Electronics was just beginning to evolve, and since my interests were very heavily in electronics, I was interested in developing courses in those areas. And I did develop a number of courses, both laboratory courses and theoretical courses.

TERRALL: Were these graduate courses?

PICKERING: Well, both undergraduate and graduate. In fact, the closest I ever came to writing a book was a book for an electronics laboratory. In fact I think I still have a stack of manuscripts

around somewhere which never did get published—although there was a lot of interest in it, and I had several publishers after me. But I never did get it finished.

TERRALL: Was this true in general at universities, that there wasn't much electronics being taught?

PICKERING: Oh, yes. Well, you see, if you go back to the twenties and thirties, radio communication and radio broadcasting were beginning to develop, but the use of electronics in control systems and all that sort of stuff was just unheard of. Some schools were teaching radio and communications, obviously, but we were not, here at Caltech. And this was partly because the head of the EE department was Professor Sorensen, a very good man whose interests were very definitely in power engineering. So the department was centered on power engineering, including the development of the long-distance transmission of power. The first long-distance power line in the country, I think, was the one that the Edison Company put in from Huntington Lake down to Los Angeles. Most of the engineering for that was done here at Caltech. Also, the high-voltage laboratory was financed through the Edison Company and was used as a test facility for the development of that line. In other words, the EE department was heavily involved in this business of the evolution of power systems. In that period, then, the idea of long-distance transmission of power was just beginning to be appreciated and it was beginning to develop. This Huntington Lake to LA line must have been put in sometime in the middle l920s. By the middle 1930s, a line was operating between Hoover Dam and Los Angeles. Generally, the idea of large networks was beginning to evolve. In fact, the large power systems that we have now really started in about that period. So Caltech was heavily involved in that.

But the idea of electronics was something new and something different. Professor Mackeown was here and taught vacuum tubes and vacuum tube theory. Then I tried to expand this into other areas of electronics, and did so in the late thirties and early forties.

TERRALL: Did you try to attract people to Caltech who might be interested in expanding this part of the EE department?

PICKERING: I don't know if we did any particular proselytizing of either faculty or students. It

just sort of came about naturally.

TERRALL: Well, then the war came along. That shook everything up.

PICKERING: Yes, then we had the V-12 program for navy officers. We also had a special short course in microwaves and radar.

TERRALL: It was one course?

PICKERING: Essentially one intensive course, yes, and maybe only given once.

TERRALL: And you were teaching in that?

PICKERING: I was involved in that, yes. In fact, I think I must have organized it. That must have been towards the end of the war, when there had been enough developments that radar was beginning to come into use and the military needed people trained in these techniques. It was essentially a pre-radar course. People took that and then went to MIT, to the Radiation Lab there, and learned the particular techniques needed for the particular equipment that was being used. So we had that, and we had the V-l2 program, which of course was a year-round program.

TERRALL: You were teaching in that also?

PICKERING: Oh, yes, I was involved in that. Then we had another one, which we called ESMWT—the Engineering and Science Management War Training. This was essentially a series of evening courses which were given all around Los Angeles, and we organized it out of Caltech. I was involved with the management of it and also did some of the teaching. This was mostly done at the junior college level, and it was presented in different high schools around the area. We must have had ten or twenty different courses set up under that program.

TERRALL: How did having the V-l2 students on campus affect the way things ran at Caltech?

PICKERING: Several things. There was of course, first of all, the intensive effort to get these people through as quickly as possible. There was also the fact that a fair sprinkling of the faculty was disappearing off the campus to go into various war jobs. Then we had a kind of a running argument with the navy on the question of maintaining Caltech standards—because we obviously wanted to. But as far as the navy was concerned, what appeared in a man's file was a grade-point average, without real recognition that this was a Caltech grade-point average, which meant a lot more than the grade-point average from Podunk College. This was hard on the individual, because the computer—well, I guess this was before the days of the computer—but the filing clerk would not recognize this difference. We used to worry about that and talked to the navy about it. We probably bent the system a bit to grade a little higher during that period than we otherwise would.

TERRALL: What was the system for admissions? The navy assigned people?

PICKERING: Well, the navy assigned people. I don't really remember now exactly how that worked. But there must have been some filtering, because we got pretty good people. Those classes went very well. In fact, they were not exclusively navy people, because the regular Caltech students were mixed in, too. The whole thing just went right along. There wasn't any significant lowering of standards during that time.

TERRALL: Did they come here for four years?

PICKERING: Three years, because we worked right through the year; therefore the three summers made the equivalent of a year. They got regular degrees from Caltech—just in less time, because they worked right through. Now, I could give you the names of some alumni of that group. One of the most well known of the alumni is Eberhardt Rechtin, for example, who is president of Aerospace Corporation. Eb went through with the V-12 class, and then he stayed on and got a doctor's degree.

But as far as the system was concerned, everything was speeded up because of the war and there was high pressure to get these kids through. The numbers involved were a little more than we usually had, but not very much more.

TERRALL: What about some of the other war research projects you were involved in?

PICKERING: Well, I was doing some microwave work, which led to an association with some of the radar work. I did go back to MIT for about three months to do a little work back there.

There were several war projects, but I think perhaps one of the most interesting was the Japanese balloon project. As you remember, the Japanese were sending over these unmanned balloons that carried bombs, and they hoped that these would drop in the US and cause trouble. Most of the bombs were fire bombs. They frankly hoped to set forest fires with these things, or else, if they were lucky, maybe to set some fires in cities. As a technique, the thing was highly successful. These things flew from Japan to various points in this country. In fact, I think one of them was picked up in Florida, so they really did travel long distances. However, as far as we know, no damage was ever done by these things until after the war, when some picnickers up near Ventura ran across one of them which had landed up in the woods. These bombs had a block of TNT on them, to destroy them. These picnickers were fooling with it, and the TNT went off and killed them. But during the war we don't know of anything particular that happened from these things.

TERRALL: So why were they not successful?

PICKERING: A fire bomb dropped at random isn't necessarily going to start a fire. And when we analyzed it carefully, there were a couple of technical problems [the Japanese] had overlooked. One of them was that these bombs, when they were hung onto the balloon mechanism, had a safety pin in the nose. The safety pin was a piece of piano wire, which was anchored onto the framework of the structure, so when the bomb dropped, it dropped free for a little bit and then the wire pulled tight and was supposed to pull out, you see. Well, after those things had flown at 30,000 feet for a couple of days, that wire became brittle and a good many of them broke, so that the bomb fell with the safety pin still in it and didn't explode on impact. That was one problem they had.

They had another technical problem in the operation of the balloon. The thing was designed to fly at a constant altitude day and night by dropping sandbags from time to time.

Then when it got through dropping all the sandbags, it started dropping bombs, you see; that was the general scheme—quite a clever scheme. There was a mechanism set up to drop these things, involving an altimeter, and this whole thing was operated by a little battery. Now, to stop the battery from freezing at high altitudes, they had made a little plastic box, which would hold maybe two or three quarts of liquid, and they put a low-freezing-point liquid in there. This plastic box, then, had the battery in the middle of it. During the day the liquid would warm up and then at night it wouldn't cool down too much, so the battery wouldn't freeze. Again, quite a nice idea. The only problem was that in order to prevent the liquid from evaporating, of course they put a lid on the box, and they had a little hole through which to fill it and they put a rubber cork in the hole. Well, what happened was that when the balloon got to high altitudes, a good many of these boxes exploded or broke open, because there wasn't any pressure relief on them. So that meant that the liquid disappeared, the battery froze, and the thing didn't work right. So they had a couple of technical problems.

TERRALL: Were a lot of these balloons getting to the States?

PICKERING: Yes. I don't remember what the numbers were, but a quite surprisingly large number.

TERRALL: And they were found and sent to you?

PICKERING: Yes, we had an arrangement set up that any of them that were found in the area for 100 miles or so around here were sent in to us for examination, because nobody was quite sure of just what was going on. So our task was to examine these and to understand them so we knew what to do about them. This business of sending these things in had one amusing by-product. We got a phone call one afternoon that one had been found somewhere and was on its way up to Caltech, and we said, "OK, drop it down in the landing dock area," near what's now the aeronautics building. So they did. Later that afternoon we went over there and had a look at it. And here in the corner of the room was this pile of balloon, and underneath the balloon was the equipment. You see, what happened on a good many of these is that the balloon would burst and the thing would come down to the ground. You'd pick up the balloon and all the equipment, so

you'd have this great mass of balloon fabric and the equipment, which was a metal ring about three feet in diameter underneath, with various things hanging onto it. So we went over and we pulled the balloon off, and sure enough, here was the equipment; and along with it was this block of TNT, which we knew was tied into a detonator on the thing. [Laughter] Here it is, Friday afternoon, sitting in the corner of the aeronautics building. By then we were familiar enough with it that we knew how to disarm the thing. So we disarmed it, but we were still worried about that block of TNT. We decided the best thing to do was to put it up on the roof of Bridge. I don't know why we thought this, but we did. At least it was in the open. So we put it up on the roof of Bridge, and then we called the army to come and get it. [Laughter] They did, and it didn't go off, so all was well.

TERRALL: What was supposed to make the bomb detonate?

PICKERING: Well, there was a detonator that was operated from the battery. After it had dropped the last bomb, it was supposed to light a fuse which would set off this block of explosive. So that, in theory, nothing would land, you see; it would just shatter the thing—the idea being, of course, that we would be mystified as to where these things were coming from. But because of this battery failure, a lot of balloons landed with the block of explosive intact.

TERRALL: So the bombs themselves had already been dropped?

PICKERING: In that particular case, I think so. I don't think there were any bombs on this thing. But that block of explosive was about the size of a pound of butter. We were very nervous.

TERRALL: Imagine how the Japanese would have felt if the building had blown up.

PICKERING: Anyway, that was an interesting project. The other man who was working on that was Bill Nash, who is back on the campus now, working in the Alumni Office. He and I handled that project. Let me tell you one other interesting anecdote. We were interested in whether you could see these things on a radar or not. We patched up the balloon and put the whole thing together so it would look as if it was flying over here. Then we took it up to Oxnard, to a navy

facility, filled it up with hydrogen and put it up on a rope until it was maybe 1,000 feet up in the air. The idea was that various radars around could have a look at it and see if they could see it. Well, while it was up there, the rope broke and the balloon took off. We had taken the precaution to put a tag on it to notify the Los Angeles harbor defense if it was found. Sure enough, the next day, the harbor defense people got a phone call from the sheriff in Flagstaff, Arizona, that he had the balloon. They said, "That's fine, bundle it up and send it in." And this guy says he's not going to touch it. And they ask "Why not?" He says, "Well, just because it's got your name on it, that doesn't mean it isn't some kind of a Japanese trick. And I'm going to have nothing to do with it." [Laughter] And he wouldn't. The army had to send somebody out there to pick the damn thing up and bring it home.

TERRALL: How did you get the ones that came straight from Japan sent to you?

PICKERING: Oh, through the army defense people. In other words, people found these things, and they notified the sheriff or notified the army. In those days, after all, if you found something like that on the ground, you knew there was something funny about it. In fact, a good many of them, if you looked at them closely, had Japanese markings on them in some places. I think they were mostly turned in.

TERRALL: Was this something that people were aware of, that Japanese were sending balloons over?

PICKERING: Not at first; not until we had a pretty good handle on the fact that it wasn't very serious, and then it was publicized. The Japanese made quite a to-do of it. We found afterwards that at that time, this was announced over there as a new weapon that was going to attack the Americans. They would launch these things with a public ceremony, usually down at a beach somewhere, and crowds of people would come down and watch them be launched off on their way to America.

TERRALL: It's amazing that there wasn't more damage done by them.

PICKERING: Yes, it is. In fact, there might still be some bombs out there. Anyway, that was an interesting project.

TERRALL: On the microwave project, were you considering going to MIT and working there?

PICKERING: Well, no, I was so tied up with the teaching program here that it really wasn't considered that I should do that. No, mostly I went to MIT to get up to date on some of the things they were doing. So that even though most of it was classified, at least I could bring some elements of it back into some of the teaching here.

TERRALL: Oh, I see. It was a question of getting it into the teaching.

PICKERING: Yes, getting myself acquainted with it.

TERRALL: So you weren't involved in the development of wave guides.

PICKERING: Well, I was doing some wave guide research. Studying some of the electrical characteristics of wave guides was especially what I did at MIT. In other words, I was not working on a particular piece of radar equipment but rather on some of the basic research underlying it.

TERRALL: Were there other people here at Caltech working on wave guides?

PICKERING: Oh, I don't think so. No, all the intensive work on microwaves was done at MIT at the Radiation Lab. The big thing that Caltech was doing during the war was the rocket work—and actually there were two projects, the Eaton Canyon project and the JPL [Jet Propulsion Laboratory] project. Of those, the Eaton Canyon project was much more active during the war, because it was really manufacturing rockets. [William A.] Fowler and [Charles C.] Lauritsen were the leaders in that. The JPL one was the aeronautics department, and that was [Theodore] von Kármán and Clark Millikan. I got involved in that before the end of the war, too.

TERRALL: Had you had anything to do with the aeronautical lab before that?

PICKERING: No. They got me into it when they got into some instrumentation problems. So I went up there to try to help with some of those things.

To go back over the history of JPL a little bit: JPL, of course, started out in the late thirties, when some aeronautics students wanted to do some rocket research. They got a little bit of money to support that. Von Kármán's interest at the time—in fact all of their interests at the time—really aimed at how you could use a rocket to make an airplane fly better, or faster. And the JATO—Jet-Assisted Take-Off—rocket was developed in the very early days of the war. As soon as it was appreciated what could be done, they got a lot of support from the army, which then had the Army Air Corps under its wing. But the thing grew so quickly that the Aerojet Corporation was spun off as a commercial company to handle the manufacture of JATO rockets, whereas JPL would continue with the research. Then, later on in the war, the army asked JPL to look at rockets for missiles and to really begin the development of a long-range missile. By then, the German V-2 activity was becoming understood, so we were asked to start a research program aimed at developing something like the V-2—well, we didn't put it that way, but to develop long-range missiles. That happened about the time I went up there. My work tended to be more and more tied up with this long-range missile problem.

TERRALL: Now was this soon after JPL was started?

PICKERING: Yes, in the '44 period—it began to move in that direction.

TERRALL: At that time, they were physically where they are now, at the end of the Arroyo?

PICKERING: Yes. What happened was that when they started doing rocket research, they started in the aeronautics building, until they filled the building up with nitric acid fumes one day and they were told to get the heck out of there. So they went up in the Arroyo and arranged with the city to get a little piece of land.

TERRALL: That was completely undeveloped then?

PICKERING: Oh, yes. There was just a dirt road up there.

TERRALL: Did they have any problems with accidents in those early days?

PICKERING: No, the accident record at JPL has been remarkably good. I don't know if JPL has ever hurt a man very seriously with rockets; it's been very good on that.

TERRALL: You mentioned last time that there was an explosion at Caltech; that was earlier?

PICKERING: Yes, that was the Eaton Canyon project. That was in the basement of Kellogg.

TERRALL: What was the relationship between the two projects? None?

PICKERING: They were quite independent.

TERRALL: So they had completely separate funding?

PICKERING: Yes, the Lauritsen project was navy-funded; the other one was army.

TERRALL: So there wasn't any concern that the efforts were being duplicated?

PICKERING: No, they were going off in different directions.

[Tape recorder turned off]

TERRALL: You were saying that von Kármán asked you to get involved in the missile project.

PICKERING: Yes, so I got involved up there and went up there to do instrumentation work. As the missile project started developing, I got more and more heavily involved in that. One thing led to another, and I took over the lab in 1954.

TERRALL: But the first project you were working on was telemetry?

PICKERING: Telemetry systems, yes. We started putting telemetry on some of the rocket test vehicles. The first big project was a large test vehicle called the Corporal. Then the army asked us to make it into a production missile from a research tool, and we modified it and came up with the Corporal weapon system. I was project manager for that.

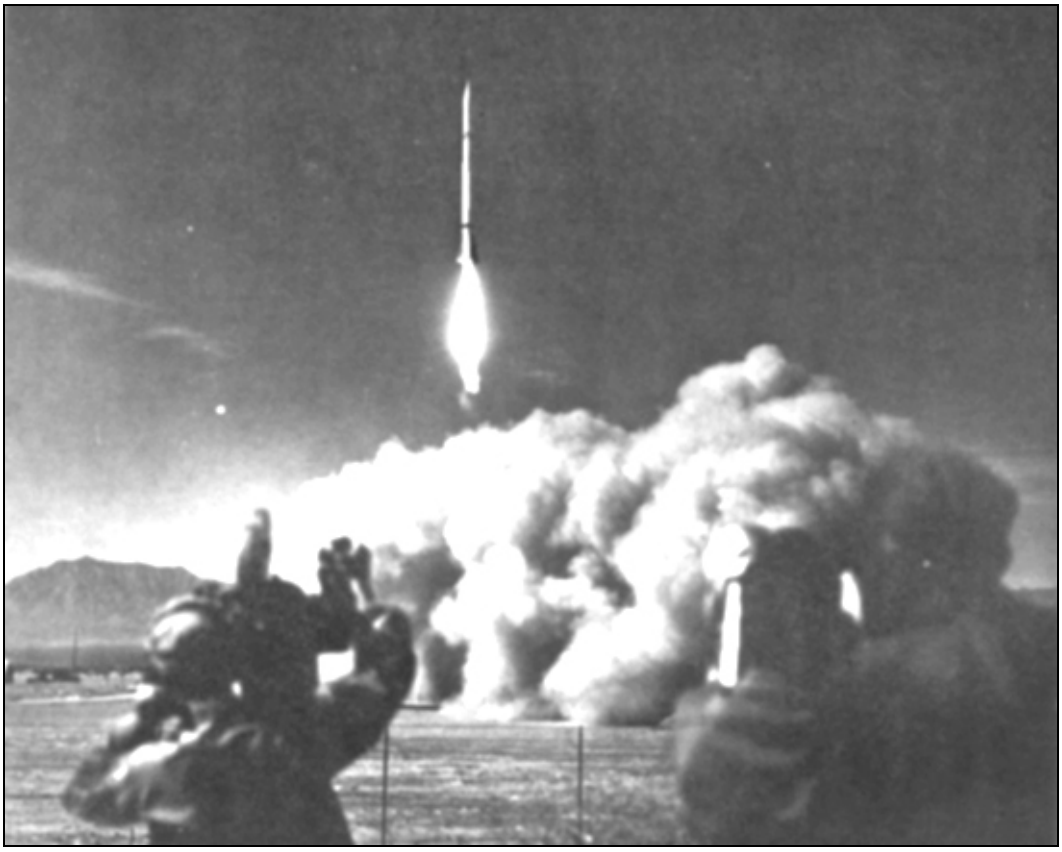

Fig. 2. The launch of a JPL Corporal missile at the US Army's White Sands Proving Ground in New Mexico, ca 1954. Photo from "GALCIT: The First Twenty-Five Years," Caltech Archives.

TERRALL: Did the telemetry technique you were using grow out of what you used in the cosmic ray experiments?

PICKERING: Yes, it was an evolution from the cosmic ray stuff, because in that, we were sending information down from balloons. So this was very much an expansion of some of the techniques

we were developing.

TERRALL: But this was also a control mechanism?

PICKERING: Well, no. The telemetry is independent of the control. The telemeter is just sending information back. The control involved something internal to the rocket, as to how you stabilize it and how you guide it. We evolved a system for the Corporal which involved radio guidance as well as some internal equipment.

TERRALL: The Corporal wasn't finished before the war was over, was it?

PICKERING: Oh, no. No, work on the weapon system didn't even start until 1949.

TERRALL: What about the telemetry; that wasn't used in any of the earlier rockets?

PICKERING: Oh, yes, it was used in the test vehicles. The first Corporal test vehicle was launched about '46, I think.

TERRALL: Would you say that that project took longer than the army expected it to? Were they hoping that it would be used in the war?

PICKERING: Oh, I don't know. They wanted us to get working in the field when they realized that the Germans had been doing something. So they wanted us to get started, but I don't know that they had any feeling that we were going to come up with something, unless the war went on for many years. The Eaton Canyon rockets, on the other hand, were used in the war. They were barrage rockets.

TERRALL: They were less sophisticated?

PICKERING: Yes, they were just simple rockets, with no guidance at all except fins on them. They were used in large numbers on the landings on the Pacific islands. They were used as a

barrage, because you could get a very large amount of explosives on the beach to clear the beach before you tried to land.

TERRALL: Were you involved in Aerojet at all?

PICKERING: No. That had been established before I joined JPL. I had nothing to do with that group.

TERRALL: But von Kármán was in both places, right?

PICKERING: Oh, yes. By the way, the first director of JPL in its present form was really Frank Malina. I think Frank doesn't like to call himself the first director; he wants to say von Kármán was the first director. But anyway, other than von Kármán, it was Frank Malina. He's now in Paris. He's an artist, and he enjoys life in Paris. Kinetic art has been what he's working on, and he also publishes an art and science magazine, *Leonardo*.

TERRALL: Is this just since his retirement?

PICKERING: Well, after he left JPL in 1946, when Louis Dunn came in [as director], he went over to Paris and he worked with von Kármán, who was in Paris also, working for NATO. Malina was with him, but developing his art. He went through various phases, finally ending up with this kinetic art, which is abstract art using moving light.

TERRALL: I knew his name from the early days of JPL, but I didn't know what had happened to him. But von Kármán in those days was away a lot, in Washington and so on?

PICKERING: Well, during the war he was, although he was based here at Caltech. After the war—I guess I don't know when he moved to Paris—but he set up what was called AGARD, Advisory Group for Aerospace Research and Development. It was a group of the NATO countries, banding together in aerospace research. They were headquartered in Paris. Von Kármán was primarily responsible for it, actually.

TERRALL: How well did you know von Kármán?

PICKERING: Oh, not as well as I knew Millikan, but I knew him quite well. After the war was over, for example, I went over to Europe with him on a fact-finding expedition. So I saw quite a lot of him. He used to live down here; he had a big house down on South Garfield, with his sister, Pipa. He never married, but his sister used to look after him.

TERRALL: Was he a very social person?

PICKERING: Well, not in the sense of having big society-type parties, but he was always very sociable with his students. He was a very warm individual.

Begin Tape 2, Side 2

TERRALL: I wanted to ask you about the Air Force Scientific Advisory Board. Did you get into that through von Kármán?

PICKERING: Von Kármán set that up. That was a by-product of his war work. I was a member of the first board.

TERRALL: What was the purpose of that?

PICKERING: To help the air force with its research planning.

TERRALL: But you were involved in investigating the state of Japanese electronics?

PICKERING: Well, after the war, of course, everybody went rushing around to find out what was going on in the enemy countries. I was involved with one of these trips with von Kármán, in which we went over to Germany to find out what was going on there in aeronautics, principally aeronautics—and then to Japan also. This meant visiting various laboratories and talking to

people and trying to find out what we could.

TERRALL: What did you find out about the Japanese technology?

PICKERING: Oh, they were not very far ahead. In the electronics area, neither country was as far as we were.

TERRALL: One of the other people in that group was [Hsue-shen] Tsien. Wasn't he on that trip to Japan?

PICKERING: No, he didn't go on it. No, Tsien worked very closely with Kármán, and I guess Tsien made one trip to Europe with Kármán, but he was not on the trip that I went on.

TERRALL: Had you worked with him at JPL?

PICKERING: Tsien? Oh, not very much.

TERRALL: He later had problems, during the McCarthy period.

PICKERING: Yes, Tsien, of course, is a very famous case of the bungling of the US bureaucracy, really. Well, it was partly the McCarthy business but partly also just bungling on the part of the US government. Tsien had been very close to Kármán, and Kármán regarded him as one of his best students. Tsien worked very closely with him in the air force planning and research activities; he was a member of the Scientific Advisory Board. He worked up at JPL, and one of the things he did up there was to organize a rocket training program for the army people, to teach them something about rockets. Well, a few years after the war, he wanted to go back to China to visit his father, because his father was getting old. The Chinese, of course, have great respect for their ancestors, and he wanted to make a trip back. One group in Washington decided that he knew too much and he shouldn't be allowed to go. They literally almost grabbed him off the boat, and essentially put him under house arrest up here in Altadena. The excuse for it was that they found a copy of his training manual in his luggage. At the time it was written, the training

manual had been classified, but by then it wasn't classified at all. But anyway, they held him, because they just felt that he knew too much and they were afraid to let him go to China. About six months or a year later, another group in Washington decided he was an undesirable alien and ought to be deported. And they deported him. By then, of course, Tsien was so furious he said he was never going to come back. And of course he didn't

TERRALL: He had been here for quite a long time, and he had worked on all sorts of things, hadn't he?

PICKERING: Oh, yes, all sorts of advanced stuff with Kármán. He was a very bright man and he is now in a very senior position in Chinese science.

TERRALL: So Kármán couldn't intercede for him?

PICKERING: No, the thing just got out of our hands altogether here at Caltech. Now, if it had been handled correctly, of course, they shouldn't have stopped him in the first place. They should have had a real discussion with him as to what his plans were and a real understanding. And I think Tsien would have come back and worked with us. But, in the first place, grabbing him almost off the boat and putting him under house arrest—he just couldn't move out of the Pasadena area. I guess he came down to Caltech and did a few things. Then this other bunch just saying he had to be deported as an undesirable alien. The poor guy! In fact, things got pretty tense. I think there was one incident of burning a cross on his lawn up in Altadena, of all things.

TERRALL: But he must have had plenty of friends around.

PICKERING: Oh, yes, sure, and there were Chinese among them. There were quite a few Chinese at Caltech in those years. In fact, one of them came back here on a visit not so long ago, who was a physicist. He had gotten his doctor's degree from Caltech about 1930, and he had worked around here; actually he worked during the war on what was called the Morris Dam project, which was a highly classified project that the navy had up here, doing some torpedo research.

And then after the war he elected to go back to China. I don't think he got kicked out; he just wanted to go back. So he went back and stayed there, and he's now one of their leading scientists. He came through here a couple of years ago, and he and I talked quite a bit. He was obviously getting homesick for Pasadena, because he was reminiscing about all the people he knew here and wondering what had happened to them. And actually, as I say, there've been quite a number of them through here.

TERRALL: Did anybody else have the same kind of problems that Tsien had?

PICKERING: No. I don't know that any of the others were either kicked out or prevented from going.

TERRALL: That was just sort of freak case, then?

PICKERING: Yes. Although there were loyalty investigation cases involving some of the people who'd been up at JPL. There was a man by the name of [Sidney] Weinbaum who was jailed on a perjury charge as part of this McCarthy business.

TERRALL: And what became of that?

PICKERING: Well, he was jailed for a few years. I really don't know don't happened to him.

TERRALL: Because, actually at Caltech, people really weren't touched that much by it.

PICKERING: No, no, because Caltech generally is not politically motivated. But this involved a question of whether there'd been some Communist cells, or something more or less equivalent to that, in the thirties. You know, this touched all kinds of people. Of course, the really terrible example here is Robert Oppenheimer. He was badly mistreated, really, for what he'd done in the thirties, whereas he'd made a tremendous contribution to the country.

TERRALL: But this person at JPL, was it generally felt that this was exaggerated?

PICKERING: Oh, I think most people felt that these guys were not in any sense activists—that they were just sort of intellectually interested in socialism or communism. Particularly in the thirties, in the Depression days.

TERRALL: During the time of the investigations, then, in the fifties, were people at JPL worried about this sort of thing?

PICKERING: Oh, there was a little bit of worrying about it, but not very much. Let's see, McCarthy was about '52; the Tsien case must have been pretty close to that time. I'm not exactly sure when. I think the Weinbaum case may have been earlier; in fact, I'm pretty sure it was earlier.

TERRALL: So this would have been someone who was working on classified research?

PICKERING: Well, in general, the work up at JPL was classified, up until the time we joined NASA.

TERRALL: So everybody had to get security clearance?

PICKERING: Yes.

TERRALL: When you were appointed director of JPL [1954], did that come as a surprise to you?

PICKERING: Yes. Louis Dunn was the director at that time, and Louis and I were pretty close. Louis told me, of course, that he was going to leave, but I was surprised when [Caltech president Lee A.] DuBridge asked me to take it over. In fact, I wasn't sure how long I wanted to keep the job. At that time, we were working on the Sergeant missile. I told DuBridge that I would stay at least to get the Sergeant missile project finished, but after that, I didn't know.

TERRALL: You were thinking of going back to the campus?

PICKERING: To the campus, yes. And of course what happened was, before the Sergeant missile project was finished, the space project started, and then I got involved with that.

TERRALL: How did you feel about putting so much of your time into administration?

PICKERING: Well, it's a different life. But on the other hand, I had sort of gone into it gradually. In other words, if I may make a distinction between my case and that of Bruce Murray [JPL director 1976-1982], who also went from the campus up to the lab, I had been spending a lot more time and had been a lot more involved in JPL administration over the years. I had been spending full time up there for several years.

TERRALL: You were heading up one of the sections anyway, right?

PICKERING: Yes, sure. I was doing less and less teaching as time went on, so that I sort of phased into it. Whereas in Bruce's case, although he had done work up there, he hadn't been involved in the administration of the place to the extent that I had. It was more of a switch in his case.

TERRALL: But did you find that it took you away from the research end of things?

PICKERING: Yes, sure. I wasn't doing research, of course, so I found myself just worrying about people and projects and so on. And the teaching I used to enjoy. Particularly because this was a period, after the war, when there was a lot of evolution in the curriculum. A lot of stuff that was coming out of the war years was getting into the academic programs. There were lots of new things and lots of interesting things to teach.

TERRALL: So you had actually been teaching between the end of the war and the time you moved up to JPL.

PICKERING: Oh, yes. My last class was in '52. I think I had a graduate student up until about

the time I took over.

TERRALL: So how did it work out? Was it something that you found you liked to do—the administration?

PICKERING: Oh, yes, sure. I was obviously already pretty deeply involved in it. And as I say, I told DuBridge I'd certainly stay to see the Sergeant out. It was because of my interest in what I was doing.

TERRALL: What was the relationship between Caltech and JPL at that time? Were there a lot of people who had joint appointments?

PICKERING: No, very few. Historically, it started out as a very close relationship, because, after all, everybody was a graduate student to begin with. In the early days of the rocket work, there was a close association with the campus. And, actually, in the early days of the missile work, there was a close association, because JPL had some equipment—namely, supersonic wind tunnels—which didn't exist on the campus, and there was a lot of interest in the aeronautics department. So the aeronautics department and JPL were pretty close in those early years. But they started to drift apart when JPL became more and more involved in the engineering of missile systems; the security classification started getting higher and it became more difficult to communicate. So by the middle fifties, the relationship between the two groups was pretty slight. In fact, there was a period around 1950 when the Board of Trustees questioned whether or not Caltech ought to continue to operate JPL, because we were getting more and more into this highly classified stuff. The question, "Why do it?" came up. The army, in fact, sent out some high-ranking generals to talk to the Board, and they agreed to maintain it.

TERRALL: So the trustees were actually also the trustees of JPL?

PICKERING: Oh, yes. JPL is legally a part of Caltech. And all the legal documents—the contracts and so forth—have to go through Caltech signatures.

TERRALL: And the army wanted to keep it that way?

PICKERING: Yes, the army was quite happy with it and wanted to keep it. As I say, they sent some senior people out to talk to the trustees about it. But the day-to-day contact with the faculty and with the students was pretty slim—partly because of this classification problem and the physical separation and so forth. Apart from myself and Homer Joe Stewart, there were some others—Frank Marble, Duncan Rannie, Pol Duwez; just a few people who were shared between the two places, but not too many.

Then when we joined NASA [1958], both DuBridge and I said, "Well, now this is a new ball game, because now we're unclassified, and now we're more scientifically oriented. And therefore, we ought to develop more relationships." And that has been a more difficult thing to do than I think either DuBridge or I visualized. I think I understand why—it's really because the faculty and the laboratory have completely different motivations. The faculty are motivated to do research that they're interested in on a time scale that suits their convenience, whereas the laboratory is motivated to respond to a request from a contractor—be it the army or NASA—to do something for them, and we have to work on schedules and budgets and so on. Also, we have to do things that somebody else wants to do, not necessarily what we want to do. So you end up with people with different objectives in life. It's not as easy to work together as it sounds at first.

TERRALL: When you took over, the army was still funding JPL. Were there problems in running a lab for the army?

PICKERING: No, actually it was easier to run it for the army than it was to run it for NASA. [The army] didn't bother us. The army was very satisfied with us, because we were doing good work for them, and they did leave us alone to a much greater extent than NASA did. Then the other thing which was actually very helpful from our point of view was that because of the classification business, we did all our field testing down at the classified site, down at White Sands [New Mexico]. We didn't have the TV and everybody looking over our shoulder. That made it a lot easier to do some things. So, yes, we worked very hard for the army, and I think we accomplished a lot for the army. But the army gave us a free hand to a very large extent. Then NASA came in, and one of the first things they did was that the first director of NASA, Keith

Glennan, and his deputy Hugh Dryden, came out and talked to me. One of the first things they said was, "Well, now things are going to be different. Because instead of being run by a bunch of colonels in the Pentagon, we at NASA have a headquarters staff of research Ph.D.'s. And we're going to have a lot more to say about what you do." And that, as a matter of fact, led to some problems in the early days of NASA.

TERRALL: But the army people [had kept] pretty close track of what you were doing.

PICKERING: Oh, sure, yes. But they did not have a whole lot of PhD's in the Pentagon to second-guess us. A small office in the Pentagon with one or two people in it essentially ran the project. The result was that we would tell them at the beginning of the year what we wanted to do. They would say, "Gee, we haven't got that much money," and then we'd argue abut it a bit, and then they would give us the money and off we'd go.

TERRALL: And whenever you needed to do testing, you went down to White Sands?

PICKERING: Yes. As a matter of fact, if you look at some very early maps of White Sands, you will find that they're labeled ORDCIT, which was Ordnance Department, California Institute of Technology. And in fact the original choice of White Sands was made by a group that included Homer Joe Stewart. You ought to talk to Homer Joe about some of this stuff. The early maps show it as the ORDCIT extension of the Fort Bliss Bombing Range—that's what it was called. And then it eventually became the White Sands Proving Ground.

TERRALL: So it was used originally just for JPL testing?

PICKERING: Yes, the first rockets that were launched down there were JPL rockets. In fact, the first rocket launch from Cape Canaveral was a JPL rocket, too. It was a so-called Bumper-WAC, where we put the WAC Corporal, which was a smaller version of the Corporal, on top of a V-2, and that was launched from down there. So JPL has an involvement in the development of both of those ranges.

TERRALL: Did you know DuBridge before he became president of Caltech?

PICKERING: Well, I'd met him when I was at MIT for a while. But no, I didn't really know him.

TERRALL: How closely did he work with you [when you were] director of JPL?

PICKERING: Oh, he supported me, but he didn't really interfere with what was going on there. With regard to the question as to whether or not JPL would continue to be a Caltech contract, he was very supportive of us in that.

TERRALL: How do you think Caltech changed when DuBridge took over?

PICKERING: Well, Caltech changed, and I could almost say that if Millikan had still been the president, Caltech would have changed, just because of the war and the way in which science and engineering changed during the war. I was going to say that DuBridge was a lot closer to a lot of the Washington activity than Millikan ever was. But I've got to reflect back that Millikan in World War I was just as active in the war effort as DuBridge was in World War II. But after World War I, the government didn't play the role in the universities that it did after World War II, and so there had to be a closer contact with Washington. And of course, DuBridge provided that. DuBridge also ran what I would call a more businesslike operation than Millikan. Millikan tended to operate Caltech more as a personal activity. He never called himself president, for example; he was chairman of the Executive Council. And that was a reflection of the fact that he wanted this thing to operate in a very democratic and a sort of loose fashion. And it operated well, because he had some very good people and they worked together well. In DuBridge's day, you could say it was the next phase of growth; the very fact of growth called for changes in the way you operated. DuBridge put in more businesslike methods of operation.

Both he and Millikan had the gift of language. They knew how to talk to an audience, no matter who it was. They knew exactly the right things to say, and they did it very well. Whether this was trying to get money out of somebody, or addressing a technical group or a bunch of faculty or whoever it was, they knew what to say.

TERRALL: It seems that one of Millikan's main ideas was to have the best person in each of the different fields, so that he could rely on them.

PICKERING: Yes, that's right. He tried to get the best people and then turn them loose.

TERRALL: And that changed when the place got bigger?

PICKERING: Oh, I suppose so, although the ideal of excellence always remained.

TERRALL: But the idea of having one person in chemistry, one person in biology?

PICKERING: Yes. That had to change.

TERRALL: When would you say that space exploration was first seriously talked about at JPL? Were people thinking along those lines?

PICKERING: Oh, yes. There were some studies made back in 1945 or thereabouts. As soon as we began to appreciate what you could do with a rocket, some studies were made of satellites.

TERRALL: Did you approach the army [about this]?

PICKERING: No, these were just long-range studies. I don't think that at that time we visualized going into a satellite program. We were too busy trying to get a rocket to fly a few miles. But we began to appreciate the scientific use of rockets for upper atmosphere exploration and began to want to do some work in those fields. The WAC Corporal was the first high-altitude sounding rocket capable of being used for that purpose. Now, in point of fact it was never used very much, because the effort at JPL was switched to the development of the Corporal. The WAC Corporal was picked up by Aerojet and became the Aerobee, which was used very widely as a research rocket.

JPL has always been more engineering-oriented than science-oriented, but we recognized the possibilities of scientific research in high-altitude rockets, and eventually in satellites, way

back at the end of the war. When the US brought a batch of V-2s over here at the end of the war, and they wondered what to do with them, some scientists came along and said, "Let's use them for upper-air research." I was involved in that, in the sense that an upper-atmosphere rocket research panel was formed. I was on that panel. But we didn't have any experiments that we developed at JPL. So JPL interest was more in the engineering capability required to do these things. And, of course, when the decision was made that the US would launch a satellite, then we dusted off some of this earlier stuff. By then, we were working pretty closely with the Huntsville people [the Army Ballistic Missile Agency at Huntsville, Alabama—Ed.]. They came up with a proposal, based on using the Redstone and some upper stages. We took their proposal and said, "They've got the wrong upper stages," and we redid it for upper stages, which we felt were the right ones. This was *Explorer 1*.

TERRALL: I think I saw somewhere that [President Dwight D.] Eisenhower announced support for a satellite back in 1955. How did that develop?

PICKERING: Well, what Eisenhower said was that the US would have a satellite experiment during the International Geophysical Year [1957-1958], and that this experiment would be developed as a nonmilitary rocket system—completely independent of military rockets, keeping it outside of the classified area. And he assigned it to the Naval Research Lab, which is a lab that does classified research but that had been active in the upper-atmosphere research program as scientists using the V-2s. In fact, NRL had developed a rocket of their own called the Viking, which was a very junior version of the V-2 but which could be used for upper-air research. So it was fairly reasonable to say to NRL, "OK, you guys take your rocket experience and your science experience and go ahead and develop a satellite." That was called the *Vanguard*.

By 1957, it was clear that *Vanguard* was in serious trouble and was going to have great difficulty meeting the schedule. So we had, as I said, dusted off our old proposals and had basically worked with the army, with the Huntsville people, to [the point] where we had something we were pretty sure we could fly. At this time in history, there was very strong rivalry between the three services for the rocket programs. They were [each] very definitely going their own way in everything.

Anyway, this study we'd made with the Huntsville people turned out to have an

application for the Huntsville work on what was called the intermediate range ballistic missile [IRBM], which was a 2,000-mile missile. They wanted to do some reentry tests on it, so we took our satellite study, and instead of putting it up to satellite speed, we said, "OK, we'll leave off one stage and we can do this reentry test at about the range you want." So we had made a number of tests of their reentry nose cone using a technique that could easily be modified to launch a satellite. And so by the middle of 1957 we had a lot of confidence that we knew how to launch a satellite. In fact, in about August '57, Eisenhower publicly showed one of our reentry nose cones, which was recovered 3,000 miles out in the Atlantic and brought into Washington. So that program was going along.

Then, when the Russians launched their first satellite [October 1957], of course we were very disturbed about it. But we got strict orders from the army to stay away from it. They told us we had no authority to launch any satellites.

TERRALL: Why was that?

PICKERING: Well, because the *Vanguard* program was the official US [satellite] program, and the *Vanguard* was coming along—so, "Stay out of it. That's a nonmilitary program and you have nothing to do with it." Then, when the Russians launched their second satellite a month later, the army got a go-ahead. Then we very quickly got together with General [John B.] Medaris and [Wernher] von Braun and carved up the pie, and [we] got moving on it.

TERRALL: What happened to the *Vanguard*?

PICKERING: Let's see, the first Soviet satellite was October; the second one was November. *Vanguard* announced they would launch in December. We had been told in November—I think literally a day after the [second] Soviet launching—to get moving, as a backup. We said we could launch by about the end of January. Well, the December launch of *Vanguard* was a flop; the thing went up about four feet and fell back in a ball of fire. It was on nationwide TV, so that was pretty embarrassing for the country—let alone the navy and everybody else. So then we were going to be next, because we had said we would make the end of January. It was pretty tense because we knew that everybody was watching us—not only in this country but really

around the world—because here the Russians were making a big propaganda hit out of how they were launching satellites, and we were dropping rockets in a ball of fire on our launching pad. We did launch successfully, then, at the end of January. That was a very interesting period to live through.

TERRALL: When *Sputnik* was launched, do you think the alarm that people felt was just a question of national pride or were they worried that it was a military event?

PICKERING: Well, first of all, I think, as far as the general public was concerned, it was an awful shock to them that the Russians were launching something that was flying overhead in the US and there wasn't anything we could do about it. There was all sorts of talk about dropping bombs on us from satellites, and all this kind of stuff. You've got to remember also that up to this time there had been no intercontinental missiles. The Russians, by the way, launched their *Sputnik* with their military missiles, as we did our *Explorer*. But I think the Russians launched a test missile a little bit before they launched their first *Sputnik*, and I'm not sure when we launched our first ICBM [intercontinental ballistic missile]; I think it was later. So there was concern that the Russians were ahead of us, and there had been talk about intercontinental missiles but nobody really appreciated what they could do. And all of a sudden you wake up one morning and here's this doggone Russian thing flying overhead, looking down at the US every hour and a half or whatever it was: "The Russians are doing this, and what's the matter with us?" Oh, no, there was a great deal of disturbance—not only in this country but around the world. Because all of a sudden, the Russians, instead of being backward peasants, were coming up with technology that the Americans hadn't been able to achieve. Therefore there was tremendous pressure to have us do something and do it quickly.

I mean, there were people who tried to discount it—official statements out of the Pentagon—

TERRALL: That it wasn't such a breakthrough?

PICKERING: Yes, that sort of thing. But the fact of the matter was that as far as the man in the street was concerned, he was very shaken.

TERRALL: There was the feeling that these could be used militarily?

PICKERING: Well, not only that but it proved that the intercontinental missile was a reality, and therefore you could sit in Russia and drop a missile on Kansas City. After all, if you could shoot something around the world, you could sure shoot it to Kansas City. That was the thing that shook people.

TERRALL: In the International Geophysical Year, you were on the satellite panel, weren't you?

PICKERING: Oh, I was on various things. When the US established this satellite program, they set up a satellite panel, which was formed out of the old upper-atmosphere research panel, and I was a member of that.

TERRALL: Now, did you have any information about what the Russians were doing?

PICKERING: Oh, things used to drift out of Russia a little bit, and there were various IGY meetings. In fact, there was an IGY meeting in Washington that started a few days before the first *Sputnik* launch. The *Sputnik* launch was on a Friday, and the meeting started on the previous Monday and ran through the week, actually. This consisted of reports from various countries on what they were doing in their IGY programs. The Russian, a man by the name of [Sergei M.] Poloskov, happened to make his report on Monday. In his report, he said that the launching of a satellite was imminent—although the translation at the time did not use the word "imminent"; it used the words "in the near future" or something like that. But a man sitting next to me who knew Russian said, "That's not what he said; he said it was imminent." And in fact, it was launched that Friday.

TERRALL: That's interesting, because it was played up as such a big shock, and here the IGY was supposed to be international cooperation.

PICKERING: That's right. No, the Russian said it was imminent. That was October, and I know

a month or two before then, out at JPL, we were speculating on when the first Russian satellite would go up. I don't know what sources of information we had—mostly rumors, I suppose. But the general opinion at JPL was that they were going to launch one in a few months, even then. And that's what was worrying us, because we felt we could do it in a few months also, if somebody would say, "Go!"

Incidentally, an interesting anecdote on that actual launching: On the Friday evening, the Russians had a cocktail party at the embassy for the IGY people. We were up there drinking vodka and so forth. Walter Sullivan from the *New York Times* came in and he spotted me. He said, "What have they said about the satellite?" I said, "What do you mean?" And he said, "Well, my New York office told me to hightail it over here and find out about the satellite, which Radio Moscow says they've launched." So I got ahold of Dick Porter, and then we got ahold of Lloyd Berkner, and we went off in a corner. Sullivan repeated this story to us. Lloyd, who was the senior U.S. scientist present, said, "OK, I'll handle this." So he went over to [Anatoli A.] Blagonravov, who was the senior Russian [scientist], and said he wanted to propose a toast. So there was silence all over the room. Then Lloyd announces that the Russians have launched a satellite. [Laughter]

TERRALL: He wanted to be the first to say it.

PICKERING: I don't think the Russian knew it. I think Blagonravov knew the launch was going to be soon but he didn't know when, and his communications with Russia I don't think were that good. And it was the *New York Times* that picked it up from Moscow somehow and relayed it down to Washington. That's the way it happened. So Lloyd announced the launching of the Soviet satellite.

WILLIAM H. PICKERING

SESSION 3

DECEMBER 12, 1978

Begin Tape 3, Side 1

TERRALL: I think last time we'd been talking about *Explorer*.

PICKERING: Yes, *Explorer* and *Sputnik*.

TERRALL: *Explorer* had been held up because they didn't want to use military missiles.

PICKERING: The IGY program was going to be done with nonmilitary hardware, and therefore the US was going to build a new rocket to launch *Vanguard*. The new rocket got in trouble, and the Russians went ahead and launched *Sputnik*. After the launching of the second *Sputnik*, we were told to go ahead and use the army rocket and get a satellite up there. So we did. We used the Redstone rocket, plus a three-stage solid-propellant rocket that JPL developed, which was put on top of the Redstone. And we successfully launched the first US satellite.

TERRALL: How did the fact that this was during the IGY affect the way this went? The IGY, as I understand it, was supposed to foster international collaboration, right?

PICKERING: Well, the International Geophysical Year was what it says—namely, a worldwide attempt by scientists to gather scientific data about the earth. This meant a lot of people who made simultaneous measurements of magnetic fields and all sorts of odds and ends. The satellite was brought into the picture as a new tool for looking at the earth and gathering information about the earth. It was in 1955 that both the US and the Soviets announced that they would try to launch a satellite as part of their contribution to the IGY program. The IGY program was formally organized on an international basis—each country had its own IGY committee. The one in the US was chaired by Professor Joseph Kaplan from UCLA. These committees then organized the work within the various countries, and then there was an international committee

that put everything together. In fact, it was at one of these regular reports of progress at which Poloskov made the announcement that the Russian *Sputnik* launching was imminent.

TERRALL: Were the data that the satellites were sending back open?

PICKERING: They were open data, yes. Yes, and in fact the Soviets have been quite good about that. Scientific data from their satellites are published in the regular journals. In fact, they also tend to publish quite a lot of it in their newspapers—at least they did in the early days. In fact, one could even make the comment that the Soviets were more prompt about publishing data than we were.

TERRALL: I was just wondering whether the obvious competition between the two countries wasn't counter to the principle of the IGY.

PICKERING: Well, the theory, of course, at that time was that there wasn't any competition. In fact, Eisenhower and the government tried to go out of their way in saying, "We're not racing with the Russians to do anything particular, we're just doing science." In fact, there were members of the government who pooh-poohed the whole business when the Russians first went up there. But as far as the man in the street was concerned, he was very conscious of the fact that the Soviets had launched a device that was flying overhead at regular intervals and there was nothing we were doing about it, or could do about it.

TERRALL: Now, when they decided to let you go ahead with *Explorer* out here at JPL, von Braun was assigned to that project?

PICKERING: What happened was, it was done under the army. At that time, the army, navy, and air force were each developing intermediate-range ballistic missiles, having a range of about 2,500 miles, and work was beginning on the intercontinental ballistic missile, with ranges of nominally about 5,000 miles. Each of the services had conducted some studies as to how they would launch a satellite. Although there was an interesting situation that existed at the time—namely, the air force had put out an official statement that said that the air force was not

interested in satellites and nobody in the air force was going to talk about space programs. That was sometime in ’56, perhaps, that that was put out. Since the navy had the *Vanguard*, they were not pushing other studies of potential satellite-launching mechanisms. So really, when it came right down to it, the army study of how to launch a satellite was the only real competitor of the *Vanguard* program. So when the country was in trouble, it was natural that the government came to the army and said, “OK, go to it.” Now, that meant that General Medaris, who was the general in charge at Huntsville at the Army Ballistic Missile Agency, was given the assignment. Von Braun and I met with Medaris, and we decided on how the work would be done. Really, what it amounted to was that von Braun’s people would provide the Redstone rocket, the first stage. In fact, they would beef it up a little over its normal performance. JPL would provide the three upper stages, and would build the satellite itself, and would build the tracking system. So we had most of the new work to do. Their assignment was essentially to take the Redstone and get it launched.

TERRALL: Had you known von Braun before or worked with him before?

PICKERING: Yes, we had worked with him. In fact we had worked with him on the so-called reentry test program, which was a part of the long-range ballistic missile program, which was trying to solve the problem of getting a high-speed missile to come back into the atmosphere without burning up. We had used this technique of solid-propellant rockets on top of the Redstone as a means of throwing a test nose cone some 3,000 miles out in the Atlantic. So that had been done already, and that’s why we were quite sure we could move ahead into this satellite program with confidence. We had also developed a backup guidance system for the long-range missiles, because although everybody agreed that an inertial guidance system would be the best way to guide them, there was some uncertainty as to whether a sufficiently accurate system could be built. So, as a backup, we developed a radio guidance system that would have done the job. So yes, we were working closely with them.

TERRALL: So they did that work and prepared the Redstone.

PICKERING: Well, yes, they had the assignment within the army of developing this long-range

ballistic missile. Now, the Redstone was not developed for that purpose; the long-range missile was a thing called a Jupiter. The Redstone had been developed as a sort of a grown-up version of the V-2, which might have some possible military applications. What happened was that at the end of World War II, the US brought von Braun and a good many of his people over here to this country, and then after wondering what to do with them, they finally decided that they ought to go ahead and redesign a rocket that was something like the V-2, only a little bigger and better. So they did, and they called that the Redstone.

TERRALL: How was it possible to get the *Explorer* launched so quickly?

PICKERING: We'd been working on the reentry test vehicles, and actually all we had to do was add one more stage. In other words, the techniques for mounting these solid-propellant rockets on top of the Redstone and of spinning them so that they would be stable in flight—these had all been worked out.

TERRALL: What about the satellite itself?

PICKERING: Well, we whipped that up out here in Pasadena. I think I mentioned that there existed an IGY satellite committee, which was doing the planning for the scientific payloads to go in the *Vanguard*. I was a member of that committee. During the course of these discussions [James] Van Allen and I got together on a what-if basis and decided that if the *Vanguard* was delayed or didn't work, Van Allen's payload, which was being designed for the *Vanguard*, could be easily modified to go on the army rocket. So when we got the go-ahead, it was necessary then for me to get approval from the committee [to] use Van Allen's payload—approval that was easy to get. It was a little more difficult to get approval from Van Allen, because at the time he was down in the Antarctic on a Coast Guard research vessel, doing some cosmic ray research. In fact, we had a sort of an amusing situation, because what we did first was to go to the navy and say, "Send this message to Van Allen." After two or three days, the navy hadn't gotten the message through. So somebody up at the lab said, "Why don't we try Western Union?" We did and the message went right through. [Laughter] So we got the reply back from Van Allen that of course said, "Yes, go ahead." We then got hold of his graduate student—a man by the name of

George Ludwig—and told George to pick up all the bits and pieces in the laboratory and hightail it out to Pasadena as fast as he could. So he did. Then we put the satellite together out here.

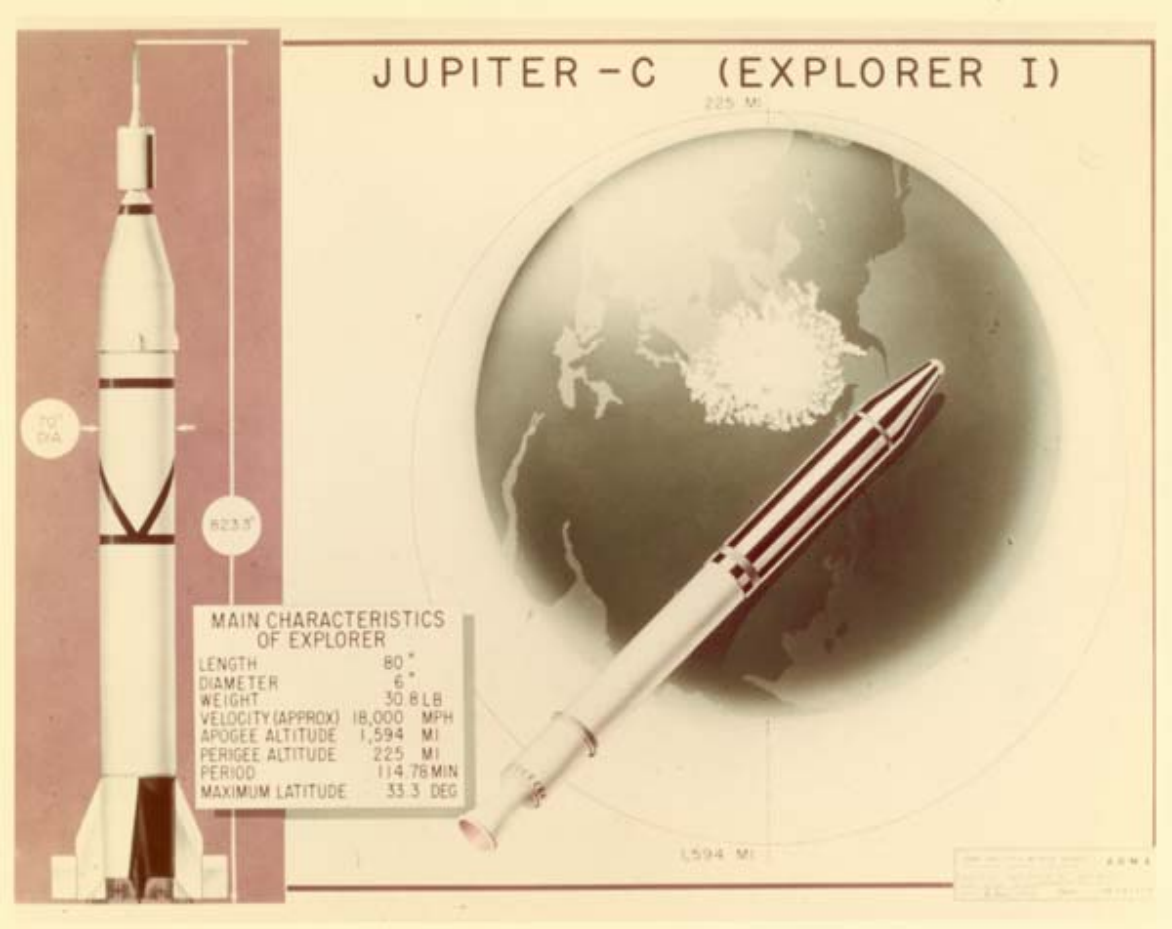

Fig. 3. The design features of Explorer 1, built in Pasadena in 1957-58. The satellite plus rocket motor measured only about 80 inches or 2 meters long. Caltech Archives.

TERRALL: This was all with Van Allen not present?

PICKERING: Van Allen was still down in the Antarctic until the time of the launch [January 31, 1958]. At the time of the launch, he was back in this country. In fact, at the time of the launch, Van Allen, von Braun and I were in the Pentagon. There was a big JPL contingent down in Florida for the launching, but the army wanted to have the three of us there in Washington, assuming it was going to be successful. So we in fact sat out the launch in a room in the Pentagon with the secretary of the army, whose name was [Wilbur M.] Brucker, and maybe a half dozen other people. We had, by today's standards, very primitive communications—

namely, one teletype to the launching down at the Cape and the commercial telephone service. So we would telephone from time to time to see how things were going. Then when the Cape announced that the launching had gone, and that it looked good, we decided that we would not consider it a success until the satellite had been heard on the tracking station here in California. This had been arranged, and JPL had set up a receiving station down near Borrego Springs, at a place called Earthquake Valley. Frank Goddard was up at JPL with a telephone line to Earthquake Valley. There was a calculated time when they were supposed to pick it up. I placed a call from Washington to Frank Goddard, and then Frank and I held the line open until we got the word. It actually took about eight minutes longer than it was supposed to. That was a very long eight minutes, because here we calculated we should be receiving it and nothing is happening! So Frank and I were making small talk on the telephone line with all these people glaring at me there in the Pentagon, until finally the word came through. [Laughter] -

TERRALL: That was your first track station, then? That was the first place that was going to pick it up?

PICKERING: Yes. On the first round, yes. We had tracking stations also in Singapore and Nigeria which we had set up in order to give us round-the-world coverage. But on this first orbit, California was the first one to pick it up. And it was picked up by other people. Navy men were listening for it down in San Diego, and they picked it up almost the same time we did. Well, it was interesting, because this was now sometime after midnight in Washington, D.C. It was a cold, rainy night, and they told us that they wanted us to go over to the National Academy of Sciences for a press conference. So von Braun, Van Allen, and I went over there in a car to the press conference and they brought us in the back door of the academy. I don't think any of us realized what we were getting into, but of course when we came into the room the place was jammed to the rafters, in the middle of the night, with TV and movies and everything. It was a very exciting period. There's a classic picture, which you may have seen of the three of us holding up a duplicate of the satellite. The satellite was only this big around and that long, so that was the rocket engine plus the satellite on the end of it.

TERRALL: How long did the press conference last?

PICKERING: Oh, a couple of hours; it went on and on. It was very exciting. Of course, that was our first real realization of what this meant to the public. We knew that we were very much on the firing line, and people were getting very excited about the satellite situation and the Russians' ability to launch these things.

Fig. 4. (L-R) William Pickering, James Van Allen and Wernher von Braun triumphantly hoist a replica of Explorer 1 at the Washington press conference to announce the successful launch of the first US man-made satellite. February 1, 1958. Courtesy of JPL.

TERRALL: And the *Vanguard* had already failed.

PICKERING: And the *Vanguard* had made a very public disaster in December.

TERRALL: It's a good thing it worked.

PICKERING: Yes, it's a good thing *Explorer* worked.

TERRALL: Was the fact that you picked it up late at the tracking station just a miscalculation?

PICKERING: It went a little faster than we expected, so the orbit went out a little bit farther and so took a little longer to go around….It was interesting, though, at that time how much public interest there was in this thing. For example, a few days before the launch, I went to a professional society meeting in New York. Some enterprising reporter discovered I was out of town, and he was bound and determined that I must be going to the Cape and that we were going to launch. We had not announced when the launch date was going to be. That was being kept quiet. But my secretary at the lab said I'd gone to New York. The reporter obviously didn't believe her, because I had

just checked into my hotel room when a United Press man [called] and said, "Just checking to make sure you really are here."

And obviously there was enough knowledge around so that they all turned out in the middle of the night, but the exact time and the exact circumstances of the launch were kept quiet until after the event. As a matter of fact, we did have to slip it a couple of days, because of upper atmosphere winds. If you run these big rockets up through the jet stream, they're subject to some rather large and not very well understood forces. We didn't want to risk it; so we had to wait.

TERRALL: There was then a series of other *Explorers*, right?

PICKERING: Yes, some of which worked and some didn't.

TERRALL: Were the failures the result of too much pressure to get them launched too quickly?

PICKERING: No, I think just that the whole art of rocketry in those days was not nearly as reliable as it is today. We were obviously very careful with this first one, to make sure that it worked.

Maybe we got a little overconfident. It's interesting to reflect on how an art like rocketry develops. At the very beginning, you run a rocket motor test in the laboratory, and if it doesn't blow up it's a success. And then when you think you know how to build a rocket engine, you stick it on to a missile, and again, if it leaves the launch pad and heads in the right general direction, it's a success. Then a few years later, it's not a success unless it lands where it's supposed to land. One can sort of go back and see that history. The first problem was to make the engine work. The next problem was to make sure that you knew how to fly them, and then how to guide them into a target.

TERRALL: But you can't have test launches.

PICKERING: No, that's right. Rocketry differs from everything else in that you can't really run a complete test of everything. The only time you ever really test is when you push the launch button, and then it's too late. In fact, that's one of the things that has to get imbued into the

consciousness of all of the engineers who work on these things—you don't get a second chance. You test what you can, but the only real test is the time you push the launch button.

TERRALL: So when [a subsequent] one didn't go, how was that received?

PICKERING: I don't know; I've forgotten exactly which ones didn't work. Well, first of all, if we go back to the first one, the data received by Van Allen were very difficult to understand. It looked as though his Geiger counter was registering an increase in counting, right as you'd expect it, and then it went dead, and then after a while it came back on again. But the data were incomplete, because we had only these three stations scattered around the world, so you got a little piece of data here, a little piece of data here, and a little piece over here. There was some uncertainty as to just what was happening. It wasn't until we launched the next one, and had more data, that we put it together and realized what was happening. One of the properties of a Geiger counter is that if you expose it to very intense radiation, it quits counting. This was what was happening. Then we realized that there was intense radiation up there. From this, then, came the Van Allen radiation belts and our first appreciation of the intense radiation in the belts.

TERRALL: Were the Russian satellites getting data on radiation as well?

PICKERING: They had some radiation measuring equipment, but they missed the radiation belts. That was done with our Geiger counters.

TERRALL: But once the first *Explorer* was a success, the fact that several of the later ones didn't go wasn't a big problem?

PICKERING: No, it wasn't a big thing. People sort of expected those things to fail occasionally. I think we ought to point out that the first *Vanguard* went only about a month after the first *Explorer*. They, in fact, did launch one successfully in March. There was obviously a fair amount of feeling between the two teams. The *Vanguard* people felt that we had, in a sense, snatched all their glory away from them, and they were only a few weeks behind us. But the fact remains that with what was going on at the time, it was essential that we go ahead and do it.

TERRALL: Did you hear about Russian failures at all?

PICKERING: No. We used to speculate about them, but we didn't hear much about them. The reporting on the Russian stuff was done at the meetings of the International Astronautical Federation or else the International Council of Scientific Unions, which set up an organization called COSPAR, which was the Committee on Space Research. COSPAR meetings or IAF meetings were the times when reports were made. Generally speaking, of course, these were just reports of successes. In other words, particularly in the early days, the Russians didn't give you any engineering data. All they gave was scientific data—the argument being that, as the Russians said, they were using the military hardware and the engineering data was all classified and they couldn't talk about it. So they'd just talk about science.

TERRALL: But the *Explorer* was using military hardware also.

PICKERING: Yes, I know. But in this country we're more open with that sort of thing. Although we didn't publish all the engineering data on the *Explorer I*, either.

TERRALL: When you were working on the *Explorer* then, JPL was still working on other military projects?

PICKERING: Yes. At that time our major project was the Sergeant, which was a replacement for the Corporal missile. Sergeant was a properly engineered missile system, as distinct from Corporal, which was sort of a patched-up research rocket made into a missile. And Sergeant was a very good design. It put the laboratory in a very interesting situation, because between Corporal and Sergeant, we had covered the spectrum of what you can do with long-range missiles. Corporal was a liquid-propellant, radio-guided one; Sergeant was a solid-propellant, inertially-guided one. Corporal, as I say, was a bit of a mish-mash of all kinds of research equipment, and Sergeant was designed so it could be worked by GIs wearing gloves out in the Arctic, that sort of thing. There was a real attempt to make it a real field-worthy piece of equipment.

TERRALL: How were decisions made about what was going to happen next with the satellite program? There was no NASA at that time.

PICKERING: No, there wasn't any NASA, but it didn't take long. Let's get the history here. The first American satellite was the end of January 1958. The conclusion then was that the US must have a space program. The Congress began to try to put together the way in which this would be done. By the middle of the year, they had passed the bill that set up NASA—I don't remember the exact date, but sometime in the middle of 1958. There were various hearings, committee meetings, and all the rest of it; I participated in some of those, and so did DuBridge. The conclusion the Congress came to was that they should take the NACA—the National Advisory Committee for Aeronautics, which had been very successful and had established a series of laboratories and had done a great deal in developing the engineering knowledge necessary to build good airplanes—they should take NACA and grow a space program onto NACA. This was done then, and the new agency was set up to go into operation, either the 1st of October or the 1st of November 1958. By about the 1st of December, they had incorporated JPL as the first new lab to be added to the old NACA laboratories.

TERRALL: It was very fast.

PICKERING: Very fast. Actually, in setting up the agency, they had been told that they inherited the old NACA and they basically had a hunting license to look for other laboratories the government owned that would be appropriate to transfer into the space program. And they grabbed us, over the protests of the army. There was an agreement that we would complete the work on the Sergeant for the army—phase that out in a sensible fashion—but then we'd work for NASA. At that time NASA also wanted to grab von Braun; the army dug their heels in on that, because they were still in the middle of the long-range rocket program and they felt that they just couldn't give up both von Braun and JPL.

TERRALL: How was the army persuaded not to put up more of a fight over JPL?

PICKERING: Oh, this NASA bunch had a lot of authority. If it had been a real showdown, they could have gone to the President and said, "That's it." The other thing was that JPL wanted to join NASA. In other words, we were very happy to do it. Also, looking at it from the army's point of view, they had their major rocket work going on in Huntsville. JPL was finishing up the Sergeant, and it wasn't obvious where we would go from there. And Huntsville, furthermore, was owned by the army; it was a civil service laboratory, whereas JPL was a contract operation.

TERRALL: So in a way this was a logical breaking point?

PICKERING: Yes, this was logical. It did cause some problems for the army. I was invited in to discuss this with the army chief of staff. But the conclusion was that this was a logical thing to do. The country was going to set up a program and the army should make this contribution to it. Now, on the other hand, as I say, when it came to von Braun's bunch, the army did object, and that objection stood for at least one year, maybe two years; I've forgotten now. What happened then was that the Huntsville group was broken into two pieces on the same grounds in Huntsville, which was a large army base, so that both the NASA laboratories and the army laboratories were on the old base.

TERRALL: So you were in favor of transferring to NASA?

PICKERING: Yes. It wasn't obvious what we should do next for the army. We were working on a classified program, and here was a new program with an opportunity for a lot of imaginative engineering and science to be done. It was unclassified. It had a closer tie-in to the campus—it just looked like all sorts of good things. This was the wave of the future, as it were, instead of just turning out another rocket for the army. Therefore, there wasn't any argument about it. The only argument at JPL was what part of the program JPL ought to go out for. You see, once we had made the transfer, then NASA began to formulate a program, and we helped them. The question then was: What role should JPL play in this NASA framework? The choices were whether to get into the manned program—because by then, the manned spaceflight program had begun—or do satellite work or do deep space work. I elected to go the deep space route, because this was the route that had the most exciting engineering and science ahead of it, and also

because this looked like a scale of activity that was suitable for JPL. The manned spaceflight program was too big for JPL and obviously was going to involve all kinds of other people, whereas this one looked like one we could get our hands on and do ourselves. Therefore we stuck with the deep space.

That decision was made, I don't exactly when, but sometime in '59. Then we started immediately to draw up what we thought a deep-space research program ought to consist of—what kind of rockets, what kind of experiments and so on.

TERRALL: How did the relationship between the Caltech campus and JPL change with the change to NASA? Was there really a change?

PICKERING: Well, there was not as much change as either DuBridge or I thought there would be. Both of us welcomed the new directions and the fact that we were no longer classified. We felt that this put a new emphasis on science which hadn't been there when we were building rockets for the army, and that therefore we should work much more closely with the campus. In point of fact, the first thing that was really appreciated was the fact that this was a national program and we were not going to make the science selections. The scientific payloads would be selected by Washington, not by us. In fact, that very definitely is the way it should be, because this was a national program. Therefore only some fraction of the science would come to Caltech; most of it would be spread around the country. Secondly, JPL up to then had been an engineering laboratory, and the interests and motivations of the people were still very closely tied in with the engineering design of the devices that would carry these things out into space and communicate with them and guide them and so forth. That's where our skill lay and that's where most of our emphasis would be, particularly since the science selections were going to be made outside of JPL anyway. So we found that although our relations with the campus improved, they did not really become as close as perhaps both of us had thought they might.

TERRALL: Were there Caltech scientists who were advising NASA on the scientific payloads?

PICKERING: Yes, there were Caltech scientists involved. Most of the flights had some Caltech people involved in them. Also, historically, we've got to reflect that we found ourselves in the

problem of the engineers and scientists not understanding each other, which became a bit of a difficulty in the early days of this program—not only for us but for some of the other people, too. The difficulty was that a scientist, particularly an academic scientist, had always been used to doing things the way he wanted them done, whether it was a case of making a particular kind of measurement he wanted or whether it was using a particular instrument or designing a particular instrument. On the other hand, we were under very strong pressures to make sure that the engineering design of the thing really worked, and that in fact when this thing was 100 million miles away it would do what we wanted it to do. Inevitably, then, we ended up with the engineers feeling that they were not going to allow some scientist to come and stick something onto their piece of equipment that was going to cause all kinds of havoc at some later time. Therefore they were going to want to get deeply involved in the design of instruments and even, for that matter, in the design of experiments. Because if a scientist, just for instance, says, "I want to fly by this planet in just this particular way," the engineer might very well say, "Look, my guidance accuracy isn't good enough for that; you're going to have to fly in this way." So you ended up with arguments of various sorts. Or if a scientist says, "I want you to fly on the sunlit side of the planet" and another scientist says, "I want you to fly on the night side of the planet," then what are you going to do? So, inevitably, there were arguments and there were problems. These, I think, have been worked out to where we get along quite well with the scientists now. But in the early days, the existence of this engineering versus science problem, and the fact that the science was selected by Washington on a national basis, really meant that the tie-in to Caltech was not quite as close as it might have been. But, on the other hand, I don't really find any fault with that. I think it's just a fact of life, that that's the way it was going to be.

TERRALL: But you kept up your appointment in EE, right?

PICKERING: Yes, I suppose I had the longest leave of absence of anybody in history. I was given a leave of absence to take over the directorship of JPL, and it just went on.

TERRALL: But you never wanted to break that tie?

PICKERING: No, no, I didn't want to break the tie. In fact, at various times during the first ten

years or so of the program, there were occasions when people in NASA raised the question as to whether or not this should be kept as a contract operation or whether we ought to be a civil service laboratory. I always felt that there were all kinds of benefits of having a contract with Caltech that were perhaps mostly indirect rather than direct benefits but nevertheless were there—that it affected the quality of people we could hire and it affected the quality of work we did, and that we could bring something to NASA that the government laboratories could not bring, and therefore that they should let it continue the way it was.

Now, this problem reached its most dramatic period when we got into trouble with the *Ranger*. The *Ranger* program was the first major program we did for NASA. It was designed to go to the moon and collect data. The first five *Rangers*, for one reason or another, did not attain their objectives. The first two were supposed to go into Earth orbit and check out various parts of the system. The launching rocket in both cases misbehaved, so those two were wiped out. The next three were supposed to fly by the moon. One of them had a launching-rocket problem and two of them had a *Ranger* problem. But as far as the public, and in particular the Congress, was concerned, the first five *Rangers* had all failed and a lot of money went down the drain, and what's NASA doing, et cetera. So this resulted in congressional investigations and in NASA investigations and a general concern about the whole business and whether or not NASA shouldn't change the contract. Well, there were some very deep feelings at that time. It could very easily have gone the other way. NASA might have just canceled the contract and picked up the lab. But they didn't, thanks to DuBridge, who supported us, and the Board of Trustees—although they were plenty worried, because this was reflecting on Caltech. Nevertheless, they had confidence in us and stayed with us.

TERRALL: So NASA's other option would have been to take over the laboratory completely and run it?

PICKERING: Yes, run it themselves. In fact, part of the option, of course, was to kick me out and then take it over and set it up as a civil service laboratory. As a matter of fact, this was even more complicated because of the fact that the man who was running NASA at this time, Mr. [James E.] Webb, had a philosophical belief that a government laboratory was the right way to do these things—that they should be done by government laboratories and not by this sort of

contract operation. In fact, when he first took over NASA, he made a statement—I'm pretty sure it was a public statement—to the effect that if he hadn't already had the contract with Caltech, he would never have started it. So it was pretty rough. Then the other complication was *Ranger 6.* Let's see, after *Ranger 5* we said, "OK, we'll hold for a year."

TERRALL: This would have been in the early sixties?

PICKERING: It was '63. So during the 1963 period, we said we would hold for one year and then we'd launch *Ranger 6*. We did launch *Ranger 7* [January 30, 1964], and it was a beautiful flight, went all the way to the moon. It was supposed to crash into the moon and take TV pictures for the last fifteen minutes. Well, the TV didn't come on. That, I guess, was the real low point in our history.

Begin Tape 3, Side 2

TERRALL: So it got to the moon but it didn't give you the data?

PICKERING: It didn't give us our pictures. It flew beautifully, as I say, to the moon. Let's see, the arrival at the moon was late in the evening, Pasadena time. There were a lot of people, a lot of press and everybody over in the [Von Kármán] auditorium, waiting to get the word on the pictures. Homer Newell, Ed [Edgar M.] Cortright, and I, plus one or two other people from NASA, were up in what is now the spaceflight control center, which at that time had been built but was not quite operational; nevertheless, we were up there. This voice from Goldstone came on, ". . .and still no video." We watched the clock running out. We knew we were going to impact in about fifteen minutes. All this was happening, but still no video, and dead silence everywhere. That was a terrible time, and it impacted with no video. There was nothing we could do. They tried various things, sort of last-minute attempts. But nothing happened, still no video. Well, after that, both the Congress and NASA were indeed on our necks. In fact, in retrospect, I don't really know why they had confidence in us, except that we did an analysis pretty quickly that pinned down what that problem was. It turned out to be a problem with a piece of equipment we had purchased from RCA. We had not really looked into that design as thoroughly as we should have. We understood what happened. On the strength of that, we said

we'd launch another one in three or four months. That was *Ranger 7*. And *Ranger 7* not only worked, it worked perfectly.

TERRALL: With TV pictures?

PICKERING: With TV pictures—beautiful pictures—coming from the moon, which were presented live to a big crowd of press and JPL people. Everyone was chewing their fingernails, of course, to see what was going to happen. That was a time when the laboratory really let go and it's the only time that we really broke out the champagne. I didn't know the champagne was all stashed away, but it was there.

WILLIAM H. PICKERING

SESSION 4

DECEMBER 19, 1978

Begin Tape 4, Side 1

TERRALL: We got up to the successful *Ranger* last time.

PICKERING: Yes, we had the problems of the unsuccessful ones and the oversight committees from Congress and from NASA that were worrying about what we were up to and threatening to change the contract and change personnel, et cetera. But when it all shook down, we moved ahead with the *Ranger* program, and the remaining three *Rangers* were completely successful, 100 percent successful. Since that time, JPL has launched numerous spacecraft, generally with a very good record. In some cases, launchings were not successful because of rocket problems, but the spacecraft generally have worked very well. In fact, the only one I can think of at the moment that has been a bit of an embarrassment was quite recent—*Seasat*, which was launched last June and operated beautifully for about three months, then lost its power supply and is now dead. Fortunately, in that three months it collected all the key data that it was supposed to collect. This is being analyzed, and I think it will essentially have accomplished what it was supposed to do—namely, to demonstrate what you can do with a satellite of that type when it comes to looking at the oceans. However, the expectation had been that *Seasat* would continue to operate for a lot longer than it actually did. The flights to the planets, though, when we've gotten one on the way to the planets, have worked.

TERRALL: I have in my notes that *Mariner 2* was before the *Ranger* shots. And that made it to Venus, is that correct?

PICKERING: Yes, that made it to Venus in 1962. It sort of staggered out there, but it made it, and it collected the data that it was supposed to collect.

TERRALL: Did it crash into Venus?

PICKERING: No, no. It was supposed to fly by Venus and look at the planet with various instruments. And it did, and quite successfully—it sent back the data. It failed about two weeks after leaving Venus; the temperature control was bad and it got too hot. It started out as a lucky spacecraft, because the launching rocket actually went unstable for a time and then stabilized itself again in the correct position. So it got a good start. Then it had some other troubles during the flight, some troubles with the Earth-sensing equipment. In order to point its radio beam back to Earth, it had an optical device that looked at Earth and kept the beam focused on Earth. And this optical device was not working properly until about the middle of the flight and all of a sudden it cleared up. So it was a lucky mission. But you're right, it was before the *Ranger*. And that was one of the things, which we had to point out to people, that while we were working on *Ranger*, we were also working on *Mariner*, and *Mariner* did succeed in this mission. Furthermore, *Mariner* was the first spacecraft to fly by another planet. That was one place where we had gotten there ahead of the Russians. In the case of the moon, the Russians had gotten there first.

TERRALL: Why did you decide to go to another planet before going to the moon?

PICKERING: Well, we had had a dual program—namely, a lunar exploration program and a planet exploration program. We wanted to carry them both. And in fact, that continued. We had *Mariner 4* to Mars, at about the same time as the *Surveyor* program to the moon.

TERRALL: So the *Surveyor* program was the next one after the *Ranger* series, then, going to the moon?

PICKERING: Yes, to the moon.

TERRALL: And that was successful?

PICKERING: Yes. Well, not every mission. There were seven *Surveyors* launched and five were successful.

TERRALL: Did NASA and Congress continue to give you a bad time when one of these missions failed?

PICKERING: No, after the *Ranger* was so successful and was such a good public success, because we were able to do live television of it flying into the moon—after that, we didn't really have much trouble with Congress.

Fig. 5. In the control room at the Jet Propulsion Laboratory, members of the mission team keep tabs on a Surveyor moonship, September 1967. The Surveyor series, run in 1966-1968, was the US's first attempt at a soft lunar landing. Photo by Western Electric News/Caltech Archives.

Just an anecdote on the live television: We had started out with some concern about letting the networks handle this live, but by the time we got up the *Ranger 9*, we were confident and we indeed let them handle it live. Well, when it came time for the first *Surveyor* [launched

May 30, 1966], which was to actually land gently on the moon and then look around and start taking pictures, we were much more nervous. So we were not at all sure we wanted the networks to really do that live. But we finally ended up by agreeing to let them do it, and we kept our fingers crossed and hoped it was going to be all right. But the thing that startled me was that about a half an hour before it was due to land, one of the network people said, "Oh, by the way, we're live all over the world," which really sort of shook me. [Laughter] Fortunately it worked, and in fact sometime later a friend of mine told me that he was in Paris, and he just idly turned on the television set and there was *Surveyor 1* landing on the moon.

TERRALL: Do you think the fierce competitiveness of the space race was productive in spurring NASA to work faster than it would have otherwise? Or was it counterproductive?

PICKERING: Oh, no, no. I think that the competition was recognized as a Cold War symbol. Demonstrated capabilities in space were important. That certainly paced the program, both in the US and in the Soviet Union. And when [President John F.] Kennedy made the very bold decision to go to the moon in ten years, that really guaranteed that we were going to have a very active program. Furthermore, it meant that we would put an awful lot of priority on the program, because the nation was committed to doing these things in space.

TERRALL: But it wasn't the sort of thing where you felt you were being pushed beyond your capacity?

PICKERING: No, no. If anyone was pushed, it was the *Apollo* people, because they had a deadline, and a tremendous amount of engineering to be accomplished. They could not have accomplished it unless they'd had essentially a top priority to do the job.

TERRALL: And JPL didn't have anything to do with that?

PICKERING: No, we were out of the *Apollo* program, except in some very fringe ways. But the existence of the *Apollo* program guaranteed that there'd be enough funds for us to do a reasonable deep-space program. And in fact, we regarded the *Surveyor* as a precursor to the

Apollo in many ways—and the *Apollo* people did, too—because it was going to land on the moon automatically. We were strongly tempted to put a sign on *Surveyor* that said, "Follow me," but we didn't ever do it. It was arranged that one of the *Apollos* would land beside a *Surveyor*, and that was accomplished.

TERRALL: So there were a number of *Surveyors* actually there?

PICKERING: Oh, yes. We had five of them on the moon. We were very interested in having the astronauts land by one of them and bring back some of the pieces, because they had been on the moon for two or three years before the astronauts got there. They did land beside *Surveyor 3*. That was very interesting, because those astronauts [*Apollo 12*] came out to the lab and had a look at the *Surveyor* and we discussed with them what we wanted them to bring back and just how they would do it, and they would cut the pieces off, and what tools they would need and so forth. So we had quite a lot of contact with those astronauts. Then, when they flew to the moon, the particular program meant that as they were coming in for a landing, the astronauts looked out a window, which was looking away from the moon and couldn't see anything until they [were] about 10,000 feet [from the surface]. Then the spacecraft rolled around 180 degrees, so they could look out the window and see where they were going. I guess it was Pete Conrad who was looking out the window, and the first thing he saw was *Surveyor*, dead ahead. Well, he just about fell out of his seat. [Laughter] Incidentally, it was a very good example of the kind of precision guidance that existed in these systems, working essentially under automatic control.

TERRALL: And they brought back pieces of it?

PICKERING: Yes.

TERRALL: Was that instructive?

PICKERING: Well, yes. We didn't think anything much would have happened, but we wanted to be sure. It was instructive to study them.

TERRALL: I read something about one mission in which an American experiment went aboard a Soviet spacecraft. I was wondering if you could remember how that might have come about.

PICKERING: No. There was a French experiment. The Soviets have flown French experiments on their spacecraft; I'm not sure they've flown American experiments. There was a time when the French were negotiating with the Soviets and negotiating with us at the same time, to fly their experiments. In fact, they had experiments on both Soviet and US satellites. Both the Soviets and ourselves had a stated objective to bring other people into the programs. The Soviets tended to bring the Eastern Europeans—Czechoslovaks, Poles, and so forth—into their program, and we tended to bring the Western countries into ours. In both cases, they were essentially scientific experiments. In other words, when NASA issued a so-called AFO—Announcement of Flight Opportunity—this announcement would go out worldwide. If a scientist in Europe wanted to propose an experiment, fine. Some of them did get to fly.

TERRALL: In your years as director of JPL, did you have to spend a lot of time in Washington?

PICKERING: Yes, there was an awful lot of travel to Washington. I used to think that I made a trip to Washington about once a month. My wife says it was more often than that. I don't know, it was an awful lot of riding in airplanes.

TERRALL: That was one of the things that Malina said. As he got more and more into administration, he was going to Washington more and more, and getting farther and farther away from his work.

PICKERING: Well, that's true, and one just had to accept that. That was the job, really. In other words, in the administration of a big laboratory like JPL, if you ask what the job really consists of, you find that first of all it's making sure that the laboratory has a good program, and that means dealing with whoever the program sponsors are, in great detail. The second thing is that internally the problem of managing a large organization is largely a people problem. You're more concerned with the relationships between people and who's doing what and who reports to whom, et cetera, than you are with the technical details of a program. Now, on the other hand,

just to reflect philosophically, I think that this does not mean management of a technical enterprise should be in the hands of a non-technical person. If I go back twenty or thirty years, there was a tendency to think that managers were trained in business administration or as lawyers or managers, and technically trained people were never top managers. But as time has gone on, there's been more and more appreciation of the fact that a well-trained technical man can much easier learn something about management than a trained management man can learn something about technical problems, and that in fact the technical training is a very good background for management. So management of technical enterprises nowadays is usually done by technically trained people, which was not true fifty years ago—not even very true twenty years ago. But it does mean, though, that if a technical man gets into a top management job, he doesn't do much technical work. But he helps, of course, in technical decisions. If a certain program is being debated as to whether it's going to be done or not, the decision is going to revolve around technical factors. So a technically trained manager is important. Certainly when a project gets into trouble, the top management should be able to understand what the trouble is and what the possible solutions are.

TERRALL: Did you ever consider moving to a different sort of job?

PICKERING: Well, when I first took the job, I thought I'd take it only for a few years and then go back and be a professor at Caltech. But I guess as time went on, I got more and more involved in it, and I realized that that was clearly going to be my life for quite a few years, particularly because of the way in which the space program evolved. You see, I took over in '54; and the first *Explorer* satellite was January '58. Quite obviously, after January '58, I had one of the most interesting jobs in the country, and I was going to keep it.

TERRALL: So when you took the job, you really had no idea where it was going.

PICKERING: No, that's right. When I took the job in '54, I would not have predicted that I'd still be in it in '74. But after '58, I could.

TERRALL: But you kept your appointment on campus.

PICKERING: Well, yes. The institute was interested in keeping me as a member of the faculty. And I was very pleased that they did. So, yes, I kept my nominal association with the faculty. But in point of fact, I did very little for the faculty.

TERRALL: How about in terms of contact with the campus in general?

PICKERING: Oh, yes. One of the problems that used to bother both DuBridge and me was, What can we do to improve the relationships, particularly the working relationships, between campus and laboratory? Both of us used to worry about this and we used to try to do things to help it. One thing, for example, which we did come up with was this idea of the director's discretionary fund and a matching president's fund down on the campus, which gave us some money that could be used to support research activities conducted by faculty, in the sense that we were the able to make the decisions. One of the problems with the laboratory program was [that it was] really dictated from Washington. Now, that didn't mean that we didn't propose what to do, but the decision that said, "Do it," was made in Washington. Whereas when you talk about faculty research programs, if you're going to talk about working together, one thing that you have to say is that this means that faculty research activities have to be related to JPL, and to some extent vice versa. Which means, then, that you'd like to be able to make decisions out here locally, as to what you do. And in the early days we couldn't do that. But when we got this discretionary fund set up, then that gave us some freedom, within at least the limits of those funds, to make local decisions. And that helped a great deal.

TERRALL: What kind of project would that have funded?

PICKERING: Oh, I don't know that I can name one offhand. Usually it was a case of the use of some equipment that existed at JPL to help someone at Caltech with some research he was interested in, or [let him] use some results that JPL had in some research that he was interested in, or maybe even the use of JPL people to work with him. I mean, all of those were possibilities, and all of them were exploited. In some cases, JPL would invite somebody on the campus to do a piece of work for them.

TERRALL: On a contract?

PICKERING: On a contract, yes—a transfer of funds. And occasionally, also, they would invite a campus professor to come up as a consultant. But that's a little different. The fundamental difference between faculty research and the research that's done in a laboratory like JPL is this question of who decides what the research is going to be. And a faculty man wants to make his own decisions as to what he's going to work on and how he's going to approach the problem. Whereas if the government is funding JPL on a contract, they will make those decisions.

TERRALL: What about graduate students?

PICKERING: Yes, we use a few of them. That has been a problem right along, because it would be nice to have some thesis work done up there. But a fundamental rule about thesis work is that it must be done under the direction of a faculty member. So that means that the faculty sponsor also has to be involved up there, and it gets a little messy. The result is that there've only been a handful of graduate theses that have been done at JPL. Now, in other cases, this thing has been handled somewhat differently. For example, at the MIT Instrumentation Laboratory, which was essentially on the campus, they did set that up so that lots of graduate students did their thesis work in the laboratory. But JPL was physically eight miles away, and there were not many faculty who were actually directly involved in the JPL program.

TERRALL: Is this something that is likely to change?

PICKERING: Well, I know that Bruce Murray is trying to change it, and Murph [Marvin L.] Goldberger [Caltech president 1978-1987] is also interested in trying to change it. I'm sure they will both work on it; and I'm sure they're both going to get a little bit frustrated. Because the fundamental problem is a matter of philosophical approach, which is basically different—namely, the faculty professor determines what he wants to do and he does it at his own pace. Whereas the JPL scientist or engineer has to recognize that he's working under contract to the government, and the government determines what he does and how fast he does it. Therefore,

you have a different approach to problems. And that is always going to exist and it's going to make it difficult.

TERRALL: Do you think that this different approach over the years led to antagonism between the campus and the lab?

PICKERING: Oh, I don't know. From time to time, one senses antagonisms, usually sort of focused around, "How come you guys have all that money to waste up there, and I can't get any money for my project?" There's a certain amount of that. I think there's also a certain amount of subconscious jealousy about the fact that we get a lot of favorable publicity up there or a lot of juicy jobs up there. Yes, there's a little bit of that. But on the other hand, I would say that most faculty appreciate JPL and recognize that the image of JPL is an important element in thinking about Caltech. I personally think that the tie-in is important and that it should be maintained.

TERRALL: How would it be different, if JPL were an independent lab?

PICKERING: Well, if it were an independent lab, it would be a civil service laboratory. That does give you a different flavor in the way the laboratory's operated, because if you're a civil servant, then you are a lot more under the control of Washington than if you're a contractor working for Washington. In particular, the laboratory is clearly not a simple case of an industrial contractor; here we're a contractor in a very favored position. This has led to an openness and frankness, on the part of laboratory people, which in the government laboratories you don't see nearly so much. In other words, we will be a lot more critical of the government than a government civil servant will be. Also, we will be much more governed by the necessity to perform on our contract. Now, I don't mean that the government civil servants are lazy or incompetent or anything like that. There are some very good people in the government civil service structure. But they don't have the same incentive to perform that we do. In other words, if NASA gets disgusted enough with us, they can cancel the contract and we can be out of a job tomorrow. Whereas a government civil servant can't be; he knows he's got a job. In fact, even the very way in which they're funded is different. If you look at the way the government laboratories are funded, there is a line item in the budget that pays their salaries and the so-called operating

expenses—the normal running expenses of the laboratory. That comes in whether they do any work or not—that's saying it a little harshly, but there is that basic funding paying your salaries and operating expenses. JPL does not have that funding. The only way JPL gets money is by contracting to do a project. Therefore, the emphasis on the project and doing it is much greater at JPL than it is in the government laboratory. This means that the government gets something different from a laboratory like JPL than they do from their own labs. I think that over the years the government has gotten its money's worth out of JPL, as it has out of other laboratories that have been operated the same way—Los Alamos is one, operated by the University of California; Lincoln Laboratory at MIT; and so forth. Now, I must admit that over the years the sort of creeping bureaucracy has come in, to where detailed government control of the laboratory is looking more and more like government control of the civil service laboratories. I regret this.

TERRALL: Do you mean in things like the number of reports that have to be filed or the amount of paperwork that has to be done?

PICKERING: I heard of a case just the other day, which involved the hiring of someone up there at the laboratory who happened to be a non-citizen. Now, the laboratory does unclassified work; there's no particular reason why we can't hire non-citizens. But the hiring of this person apparently was—well, Washington tried to turn it off. That I would regard as an unnecessary and an improper interference in our affairs. Of course, I used to regard the fact that the government put a ceiling on us as improper interference in our affairs.

TERRALL: You mean how big you could grow?

PICKERING: Yes, how many people. I used to take the position with them, "Look, you give us a contract to do the job; we will give you a price to do that work, and we'll also tell you how many people we want. If you accept the contract, then OK, we ought to go ahead and do it." Now, the counterargument on the part of the government people was that they did regard us as a special case, and they did regard themselves as providing a sort of institutional support for us. Therefore if we hired, say, 500 people to do a certain job, when that job was over, they said to me, "You're going to come back to us and say that you'd better find some jobs to keep these 500 people busy;

you're not going to turn them out on the street." And I had to admit that that in fact is right. Because in the research activity I cannot have people coming and going in the way that you might in some industrial facilities, and indeed I wanted to keep them on. And therefore they said, "OK, if that's the case, we have to make some extrapolation of the amount of work we think we're going to give you and how we're going to keep the employment more or less level and so forth. Therefore we're going to have a say in what your total employment will be." That's sort of an argument.

TERRALL: Did it, in fact, cause problems in any specific cases where you needed more people?

PICKERING: Yes. We would have liked to have hired a good many more people at various times than the government allowed us. Of course, the other argument on the part of the government was, "Well, even though you're different from the other laboratories, you are still a member of the family." And the civil service laboratories are very tightly under this numerical control, because the Congress tells them how many people they can hire. Because that existed in those laboratories, if we rather blatantly ignored it, we would have a problem. But it was the way in which the government started getting involved in our hiring policies and so on that I regarded as unfortunate. I would rather have kept them out of it. Fortunately they did not try to put the civil-service-type constraints on us, because in that case we would just as well have been a civil service laboratory. You see, the Civil Service Commission does put very strict constraints on what the government can do with civil servants. If, for example, the government has to reduce the size of its staff, as NASA has had to do in recent years because the work has gone down, the process of "Reduction in Force"—the RIF process—is very formally spelled out, which means that you can't fire Mr. A; you have to look at his past history and whether he was a veteran, et cetera, et cetera. Instead of Mr. A being laid off, Mr. B may have to be laid off, because he's more junior or something or other. Whereas from the point of view of the work to be accomplished, you'd much rather let Mr. A go. Well, that kind of constraint on the laying off of people has been an awful headache to the [civil service] laboratories. Another one is that the Congress put a constraint on them as to what the average employment grade could be in the laboratory—the civil service has a series of different levels. The Congress would say that the average grade for your laboratory must be so and so. Therefore if you wanted to move some

people up into the more senior grades, the only way you could do it would be to move some more people into the junior grades. And it may be that you were limited as to the total number, and so therefore you had no choice; you could not move these people up to higher grades. So they were under all kinds of problems, and I'm very glad we didn't have that class of problem. But as a function of time, the NASA interference in the internal JPL affairs has become increasingly evident.

TERRALL: What about since your retirement? What have you been working on?

PICKERING: Well, shortly before I retired, I had some contact with a university over in Saudi Arabia which was interested in setting up an applied research laboratory, which had some parallels to JPL-Caltech. They were looking at the way in which these laboratories operated around the country, and they came out to JPL and talked to me. One thing led to another until they said, "Well, you're retiring; why don't you come over to Saudi Arabia and put this thing together?" So I agreed to do that for two years. In fact, in the past two years I've spent about half of my time in Saudi Arabia and half of it here—essentially planning, organizing, and trying to set up the initial program for this research laboratory. The two years was just up a few months ago, in September of this year. So I have been going back and forth to Arabia, because I told them that for this kind of planning, I could do most of it better in this country than I could over there. They agreed. I was last in Arabia in October and spent about a month there at that time. In other words, my initial assignment was completed in September; but I'm expecting to continue a consulting arrangement with them.

TERRALL: And that has nothing at all to do with Caltech?

PICKERING: No, no. Except I might comment that this university over there is called the University of Petroleum and Minerals. It happens to be an engineering and science school of about 2,500 undergraduate students. And this school, in turn, has a consortium of American universities to help advise them on academic matters, and they asked Caltech to become a member of that consortium. So, in that sense, Caltech has a little tie to it. But my activities over there in setting up the research institute had nothing to do with that.

TERRALL: Is the research institute going yet?

PICKERING: Yes, it's going. The building will be finished in two or three years. It's under construction now.

TERRALL: How did you find it working in a completely different situation like that?

PICKERING: In a technical sense, it was clear that this research institute had all kinds of interesting possibilities, because it was visualized as being an applied engineering research type of thing, which would be supported by either industry or government in the country and would concern itself with the problems which were of immediate interest to people over there. A close analog would be the Stanford Research Institute. In fact, it was visualized that it would be supported by contracts, which it would get from either government or industry. It was physically on the campus of the university, and the university was prepared to build a building for it and provide the initial support for it. It would have its own staff, like JPL, and its own administration and so forth. Well, it was obvious that there's a lot of opportunity for that kind of a facility and that kind of capability over there, that it just didn't exist in the country, and that there were plenty of people who felt that this would be very useful to them. So it can be built up. Now, the build-up has not been as fast as I had hoped it would be, partly because of different ways of doing things than we would have and partly because of a physical space problem. Because until we get the building, it means that we just have to go in various nooks and crannies around the university. But it's going along.