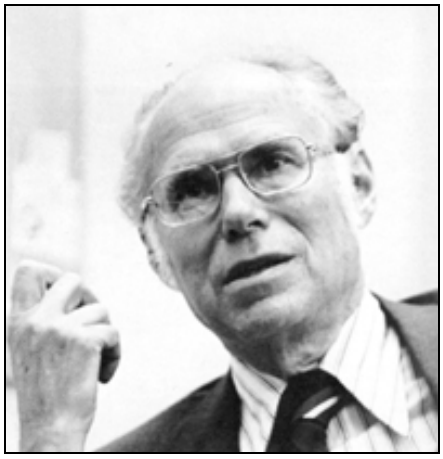

# JOHN R. PIERCE
**(1910-2002)**

**INTERVIEWED BY**
**HARRIETT LYLE**

**April 16, 23, and 27, 1979**

**ARCHIVES**
**CALIFORNIA INSTITUTE OF TECHNOLOGY**
**Pasadena, California**

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## Subject area
Engineering, electrical

## Abstract
An interview in three sessions in April 1979 with John R. Pierce, often referred to as the father of the communications satellite. A leading applied physicist, Pierce went to work for Bell Telephone Laboratories in 1936 after receiving his PhD in electrical engineering from Caltech. He spent the next thirty-five years there, where he made important contributions to the development of the traveling-wave tube and the reflex klystron, rising to become executive director of Bell's Research-Communications Principles Division. Pierce was also a pioneer in communications satellites, playing a key role in the development of two of the earliest, *Echo* and *Telstar*. In this interview he recalls his undergraduate education at Caltech in the late twenties and early thirties, the early years at Bell, radar work during the war, and the beginnings of America's satellite program.

Pierce was also a prolific author of science fiction, sometimes under the pen name J. J. Coupling. In the mid-1960s, he served on the President's Science Advisory Committee (PSAC). He retired from Bell Labs in 1971 and returned to Caltech as a professor in the Division of Engineering and Applied Science, and he comments on the changes (and the similarities) he found in undergraduate education at Caltech. While at Bell, Pierce developed a lifelong interest in computer-generated music and psychoacoustics, the science of consonance and dissonance; in the latter part of the interview, he discusses his work with Max

Mathews on music synthesis. A year after this interview was conducted, he became professor emeritus at Caltech, and in 1983 he joined Stanford's Center for Computer Research in Music and Acoustics (CCRMA) as a visiting professor. Pierce died on April 2, 2002, in Mountain View, California.

## Administrative information

**Access**

The interview is unrestricted.

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**Preferred citation**

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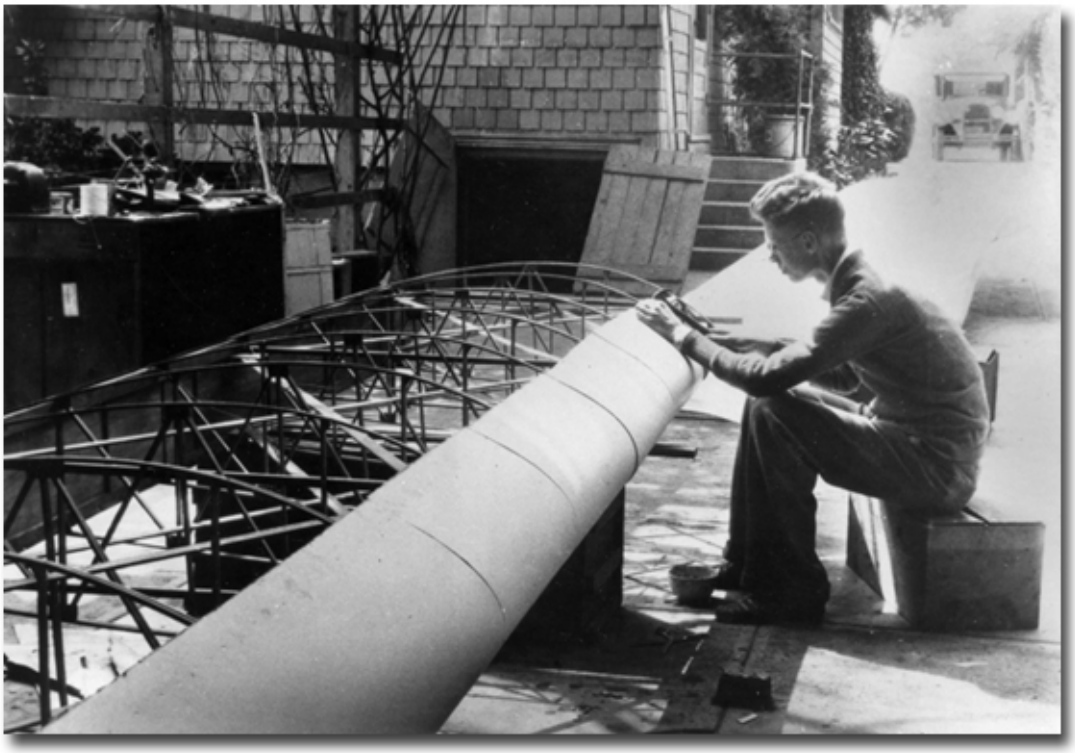
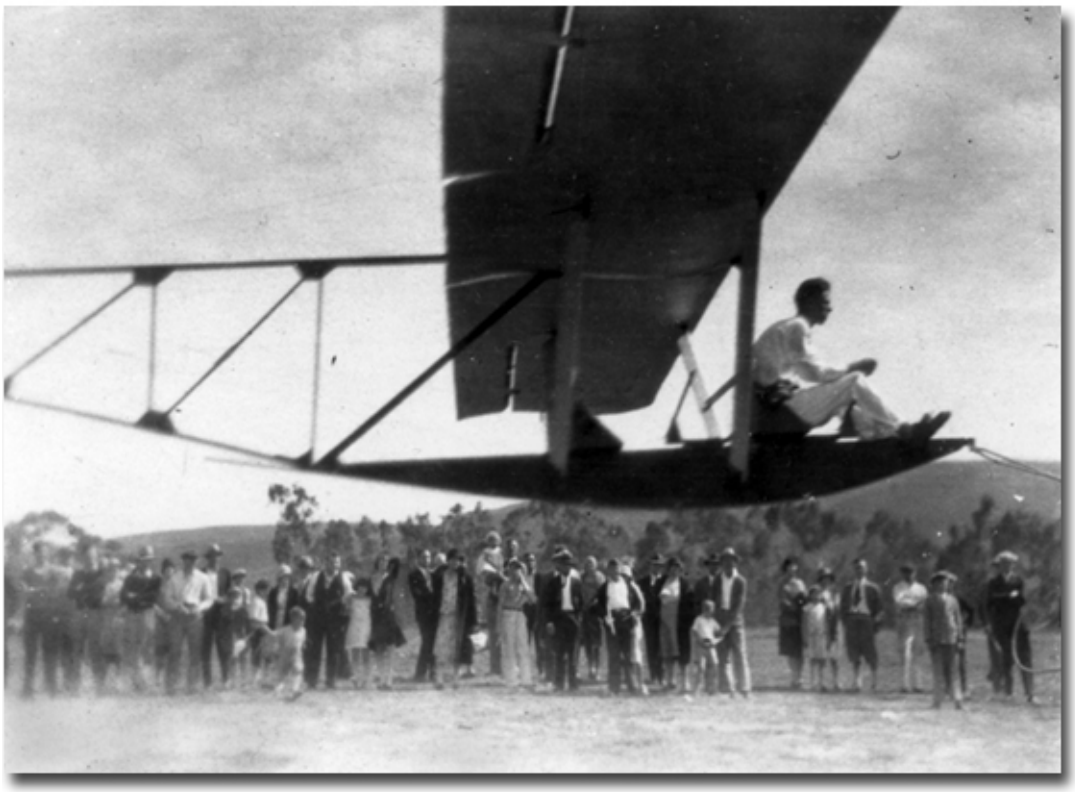


In high school, John Pierce experienced a period of “glider madness.” He eventually became airborne and even won a few silver cups at a San Diego glider meet in 1929. In the same year he published his first book, *How to Build and Fly Gliders* ($1 per copy). Photos Caltech Archives.



CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

# INTERVIEW WITH JOHN ROBINSON PIERCE

BY HARRIETT LYLE

PASADENA, CALIFORNIA

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**CALIFORNIA INSTITUTE OF TECHNOLOGY**

**ORAL HISTORY PROJECT**

**Interview with John Robinson Pierce** **by Harriett Lyle**

**Pasadena, California**

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**Begin Tape 1, Side 1**

LYLE: You were born in Des Moines, Iowa, March 27, 1910. Could you tell me just a little bit about your family?

PIERCE: My father was John Starr Pierce; his father was Henry Pierce. My son has traced my father's family a good way back. My grandfather supposedly came from Cherry Valley, New York, but there's some confusion here, because no record of his birth or life there has been located.

LYLE: Were any of them interested in science at all?

PIERCE: Not that I know. My father was in the millinery business. Before I came to California and attended Caltech, he was a partner with his brother, in Pierce Brothers. They had millinery stores in a number of towns in the Midwest.

My mother's maiden name was Robinson. Her father, Thomas Robinson, came from Canada, I believe, with his wife. He was first a farmer and later "well-to-do"; he was associated with the bank in town and had various business dealings. He had four daughters. He spent his summers with one of his daughters in the Midwest and his winters in California, usually with a daughter who lived in Redlands. My aunt [Stella Wynegar] is the only person of my mother's generation living—she is something over a hundred and still lives in Cedar Falls, Iowa.

LYLE: I was reading your autobiographical writings of your childhood. You mentioned that you felt the glamour of science early, through the science fiction you were reading. I was wondering if there was anything before that, and how you got interested in science fiction.

PIERCE: I was always interested in things of a technical nature, whether Meccano sets or my American Model Builder or toy steam engines or electric motors. I regarded electric motors as a sort of natural magic and got my mother to read me things about them. I tried to learn, but without much understanding. I suppose that science looked to me like the magic of the day. If you wanted to do great things, you didn't ride on broomsticks, you invoked the mysterious forces of science. It was many, many years before I found out what science is all about.

LYLE: In school, you had two interests that you've had your whole life: writing and science.

PIERCE: I think for some people the idea of writing is glamorous. I never quite got over the glamour of words on paper. I first found out that verse scans rather late, when I was a high school junior in St. Paul, Minnesota. That led me to experiment, then and through my Caltech years. Science, as I said, was a more magical idea that was powerful. I got insight—such as I have—into both science and writing much later. I pursued both at Caltech, writing for the school paper, taking courses and gradually learning—not so much being taught as stumbling onto—what science and technology are all about. Then I went to Bell Laboratories [1936], where I learned a little more about science and technology by actually trying to practice them.

LYLE: I'd like to discuss Bell, but before we go on, I know that you were making a lot of gliders. And you mentioned that your mother went up in one of the gliders with you, and to me that was very striking.

PIERCE: I didn't know any better than to ask her. She was game for anything. She was the mechanical member of the family, rather than my father. I didn't think of asking my father, and I doubt if he would have gone up.

LYLE: Did she help you build the glider?

PIERCE: No, she did not. I built them, out of sheer ignorance and effrontery. When I was a senior in high school, [I got together with] two people—Apollo [Milton Olin] Smith and Oliver Larue. Apollo was later known as Amo [for A. M. O.] Smith. He is a graduate of Caltech and runs some sort of consulting business—he worked for Douglas Aircraft earlier. We three built a glider after graduating from high school and we flew in it.

After I got to Caltech, I built a two-place glider and I took my mother up. These were open-frame gliders; you sat on the skid, and there was nothing between you and the world below. Later I built a sailplane, in which I was up about a half an hour on one occasion.

LYLE: Was she very interested in this, in what you were doing?

PIERCE: My mother was interested in anything I did.

LYLE: In high school, did you have any favorite classes?

PIERCE: Yes. Mathematics and chemistry and physics were my favorite classes.

LYLE: How did you hear about Caltech?

PIERCE: I really don't know. I went to three high schools. I went the first two years in Mason City, Iowa, and then a year in St. Paul, Minnesota. Then we moved to California and I was graduated from Woodrow Wilson High School in Long Beach in 1928. Somebody there must have told me about Caltech. Neither of my parents was a college graduate. I remember—vaguely, no details—looking over various requirements for entrance into schools. There was an entrance examination at Caltech. There was also no language requirement, and I couldn't have met a language requirement. Caltech was nearby. I was full of ignorance.

It was just God's good luck that I applied to Caltech. Ever since then, I've recommended that people be lucky above all things. Someone once asked if the people to whom I give this advice take it, and I say, "The successful ones do."

LYLE: You wrote that when you were a freshman at Caltech, you had to choose an objective. You didn't really know what to do, so you decided you would be a chemist at Eastman Kodak.

PIERCE: Yes. I can't imagine why, except that I was an amateur photographer at that time. I took pictures of the gliders that I and other people built. Also, the only person I knew with any technical background whatsoever was Orsino C. Smith, a chemical engineer, who was the father of Apollo Smith, with whom I built the gliders. I knew a chemical engineer, I was interested in photography, so I should be a chemist. I would work for Eastman Kodak. I did very poorly in chemistry, and Eastman Kodak was just a shot in the dark.

LYLE: Were you in the habit, already, of choosing goals—or were you asked to choose a goal?

PIERCE: I was just asked; I've never been in the habit of choosing goals. I've done things that interested me, of course, but by some quirk of circumstance I stumbled into them. When I went to Bell Laboratories after Caltech, I was put to work on research on vacuum tubes. That was one thing I'd never thought of before going to Bell Laboratories. I can hardly think of a thing that I knew less about.

LYLE: You came to Caltech and you didn't like chemistry.

PIERCE: It didn't like me.

LYLE: What did you find that you did like about Caltech, at the beginning?

PIERCE: There are two sides to this—one is personalities and the other is what I did. If I wasn't to be a chemist, maybe, since I was building gliders, I should be an aeronautical engineer. What I didn't like was drafting. I was very bad at that and I got tired of it, so I sought out electrical engineering, which had less drafting.

As to personalities, I liked lively people with an interest in students. This was not universal. The people who influenced me are not the best known in the annals of the Caltech faculty. One was Clyde Wolfe, who taught freshman mathematics. He was a very sympathetic

person—informal, engaging. Another—Carl Anderson, who later received a Nobel Prize—taught me sophomore physics, and he was very good.

LYLE: What did you like about his teaching?

PIERCE: He would listen as well as speak. I was curious as to why, when you flip a tennis racket up, it turns over—if you flip it up flat, just a little off balance, it turns over once before coming down. Carl Anderson went away from the class and came back and said he'd consulted the textbooks on rigid mechanics and there was a reason. I didn't understand the reason at the time, but he had gone out and looked up something because of my question. I didn't really understand the reason until I took a wonderful course from William Vermillion Houston, who was the best teacher by all odds that I encountered at Caltech. He taught a course, an introduction to theoretical physics, which, being an electrical engineer, I took as a first-year graduate student; the physicists took it in their senior year. Houston was later president of Rice University. He had infinite patience. The people would read the text, and in class they would either work out the problems or they would ask questions. He would explain things most patiently; he never lost his patience. Once he finally cut things off; some student had asked for something to be explained for about the fifth time, and Houston, very gravely and quietly—he was a very quite person, quietly spoken—said, "I believe that has been explained before." But he didn't stop at the first explanation. If somebody hadn't understood, he would explain it again.

LYLE: Did he get the students to participate in explaining?

PIERCE: It was a very open class. I remember there was something wrong in the text. The students pointed this out to him, and he took it under consideration and decided it was indeed wrong. It was a perturbation problem. So there was free interchange in that class, but, by and large, it was he who explained, because he was so very good at explaining.

LYLE: You were interested in the English classes, too. Were many students interested in English?

PIERCE: I don't know whether many students were or not. There was a group of students who were. Max Millikan was one, Robert Andrews Millikan's son. He later became an economist. He was a very talented actor, and he was attracted to these things, as was Alfonso Carlos Bulnes. There were others—Merrill Berkley, who's now with Berkley Instruments, and a fellow named Morgan, of whom I've heard nothing more. Some wrote for the paper, some just went over to [Harvey] Eagleson's quarters in Blacker House and talked in the evening. George Tooby was very interested; he lives in town now. He went on to a career in, I believe it was a milk-processing business; he's also wealthy by inheritance. He was interested in these things. Don Poulson and Lee Carleton were others, and very close friends.

LYLE: We did an interview with Don Poulson and he said that you convinced Clinton Judy that he should read *Paradise Lost* out loud to you.

PIERCE: I did indeed—I think I was a graduate student at the time. I was very much taken with Clinton Judy, who was a fine man. A number of us were interested in such things. I shouldn't forget my friend Nick Weinstein, but he wasn't in on that, because he had left for the Soviet Union earlier. Judy lived at that time in a huge library with living quarters attached. We asked him—probably at my instigation—to read *Paradise Lost* aloud, which he did beautifully, in a measured voice, not like Sir Laurence Olivier but in a transparent manner in which both the thought and the metrical structure came through. Then we would sit before the fire and talk, not only about *Paradise Lost* but about life in general. It was a memorable experience. I believe that in the end all he got was the contact with the students and a box of cigars.

LYLE: Are you aware of anything like that that goes on with the students today?

PIERCE: I'm not very aware of the students today, except a few individuals. I spent thirty-five years in a very different environment, at Bell Laboratories. Since I came back in 1971, I've gone through the motions of teaching, which I've found very challenging but very difficult. I have seen some undergraduates and some graduate students, but the gap is great. I've not become involved with the students in any large way, as [James] Mayer has, as head of the student houses. Also, the things I read in the *California Tech*, the student newspaper, I find on the whole a bore,

because I'm not a student anymore. The paper was a matter of great excitement when I was young, but it seems very remote now. So I just haven't had the contact with the students that I think is so important to the students—and that Clyde Wolfe had, that Harvey Eagleson had, that Judy had, that Houston had in class, if not outside of class, and that Carl Anderson had in class. There are some faculty members who have considerable contact with the students outside of class. Horace Gilbert, who is still alive and with us, was one with whom one could have contact outside of class. Some faculty members I remember as being very close and important in class but unknown outside. Others were known outside.

LYLE: So you went into electrical engineering. In your writings you mention that you were a little bit cynical, or discouraged, at the time you were graduating.

PIERCE: I really didn't know where I was going. I didn't know what the world was like. Those were the days of the Depression—very different from this. I had not learned, I think, to appreciate worth. I wasn't well oriented. I could see the merits of some people—Fred [Frederick C.] Lindvall I could see was a clear-headed person. I took a graduate course from him. [Samuel Stuart] Mackeown gave me a real view out into life, both because he gave me a job dissecting radios for patent infringements—he worked with Lyon & Lyon as an expert in various legal cases—and a job mowing his lawn. Mackeown was a very interesting man. But my orientation to life was not very broad or sensible. I didn't understand Millikan's greatness, which somebody should have really pointed out to me. Young people tend to be cynical about things.

LYLE: So you were not particularly impressed with Dr. Millikan?

PIERCE: The students looked on him as a sort of publicist. I remember there was a steam shovel on the campus and somebody had written on it, "Jesus saves," and some student had written below that, "but Millikan deserves the credit." Young people tend to be iconoclasts. They tend to be cynical. Also, in retrospect, I feel that simple and obvious virtues are not always apparent to smart people. I feel that perhaps lesser faculty members—and I won't accuse them—also didn't appreciate Millikan's greatness and goodness, which was less than Einstein's. Somehow,

we got a slightly cynical view of the world, besides that which we generated for ourselves. I've come to admire platitudes, because, at least by definition, a platitude is something that is true. And a lot of the things that pass around may not be platitudes, but they aren't true, either.

LYLE: Can you give me an example of a good platitude?

PIERCE: Oh, dear! What I think of turns out to be irrelevant—"All work and no play makes Jack [a dull boy]." It's a platitude and it's true; it's a frivolous one. I can tell you a medical-practice one—that the common diseases occur more often than the uncommon diseases. This is of help to young doctors in trying to figure out what's wrong with a person. "It's better to be smart"—that's my own platitude, but it's true. "You accomplish more by working hard than by not working." I'm not full of conventionally phrased platitudes, but there's a tremendous amount of folk wisdom that sounds rather dreary because you hear it over and over again. Eventually you find out that it's really so.

LYLE: So you graduated and were faced with the prospect of finding a job in the Depression.

PIERCE: I graduated in 1933 with a BS, and again in 1934 with an MS [and in 1936 with a PhD]. There weren't many jobs around that time, and I was not a very well-oriented young person; I wasn't very good at looking for jobs. In 1936, I got a job at Bell Laboratories. I think that was entirely the doing of Professor Mackeown, for which I'm eternally grateful. He couldn't have done better.

LYLE: So you went to Bell Laboratories. How did you feel about going there?

PIERCE: I was glad to have a job. I'd always lived with my parents to save money. They had moved to Pasadena when I went to school at Caltech, and I was glad to get away. I was glad to go to New York, which sounded glamorous in those days. It was an entirely fresh start in my life.

LYLE: It must have been exciting.

PIERCE: I guess it was. Somehow, I've missed a certain amount of excitement in my life, because all the things I've done have seemed to be just what I was doing. I have realized only in retrospect that they were exciting. Taking my mother up in the glider, when I look back on that, that seems really wild. It seemed absolutely ordinary when I did it.

LYLE: How did you get to New York? Did you take the train or drive?

PIERCE: I must have taken the train—one didn't fly in those days. My parents financed a trip to Europe for the couple of months between my doctor's degree and the time I took this job. I remember I went to England, where I bicycled—oh, it must have been two months—I bicycled around England. I visited a friend who was a Rhodes Scholar—Horace Davenport—at Oxford. I went to Paris, and to the north of Italy, to Viareggio. I went to Munich and went to the opera in Munich. I believe that's about that.

LYLE: Did you go alone?

PIERCE: I went alone. When I bicycled around England, I fell in with the son of a greengrocer, and bicycled around England with him. In the other places, I knew no one, really. Then I came back and saw my parents again in California, and took off on a train to work at Bell Laboratories, where I rented an apartment [in New York], first in London Terrace, and then in another place, with a friend I'm still very close to, although I don't see him every day—Chuck Elmendorf, Charles Halsey Elmendorf III. He got a master's degree the same year I got a PhD, and he went to work at the Bell Laboratories. We shared an apartment in London Terrace for about a year, and then on West 18th Street for about a year.

LYLE: When you were in Europe, could you see the effect of Nazi Germany? Could you see the persecution of the Jewish people at all?

PIERCE: This was in 1936. I didn't see that at all. In 1936, it wasn't so much apparent. I remember that when I was in Italy, I was drinking in an open-air café and some of the Italians

raised their glasses—they knew I was American—and said, "Viva Roosevelt!" I raised my glass, and said, "Viva Mussolini!" That wasn't the right answer, apparently. On a train out of Germany, there was an English commercial traveler with whom I talked. He was obviously under the influence of the Nazis, and I thought of denouncing him to somebody, but I didn't know whom to denounce him to.

Looking back at that time, it's clear that I knew there were Nazis, and I knew they were bad. How did I know they were bad? Because this very dear friend of mine, Nick Weinstein, was a Communist, and he knew the Nazis were bad. But he left Caltech in 1932, went to the Soviet Union and didn't graduate. I've been writing to him ever since. I saw him in the Soviet Union in 1973. So, somebody told me what right and wrong is, and that left is right and right is wrong. But when I first went to New York, I was full of technical things rather than political things.

LYLE: Did your acquaintance with him cause you any political trouble later?

PIERCE: Never. He never recruited me into the Communist Party, but I went to a sort of open meeting of a Communist group with him and his sister. They didn't recruit me. I don't think I was a good candidate. I read the *New Masses* in Nick's room.

LYLE: I want to talk about research in the Bell Labs and how that's done. That is, when you first started there, you were working with vacuum tubes. Who decides what problems will be worked on?

PIERCE: That's very different then and now. I was told to do research on vacuum tubes. People sort of just left me alone. They did suggest that I go and see Philo Farnsworth, who was working on electron multipliers and television pick-up tubes, but I was left pretty much to myself. This was very, very confusing to me. I didn't know what to do.

LYLE: Were you doing it alone?

PIERCE: Yes.

LYLE: Did they say, "So-and-so has been doing this and this is where he left off"?

PIERCE: No. I was just supposed to plan something to do and do it. I think that is close to cruel and unusual punishment.

LYLE: And full of anxiety, I'm sure.

PIERCE: Yes, but I didn't know enough to be unhappy. I did crazy things. I did some useful things. I invented an electron multiplier. I was greatly helped at this point, but not so much by the people who were close to such work. I felt a certain secretiveness in the people who were working near to me. They were doing their own thing, and I was doing other things. Heaven knows how I found anything useful to do. I was exposed to things by some of the people who were less secretive. I was very much helped by Bill Shockley, who came to Bell Laboratories about the same time I did. He had been an undergraduate at Caltech but did his graduate work at MIT. He was a very sympathetic person, and taught me a good deal. Somehow I hit on things that were worth working on—electron multipliers and the question of noise in electron multipliers, and later trying to make high transconductance vacuum tubes.

Then, as the war came, I was drawn into microwave tube work, and the outcome of that was a little bit by accident. First, I tried to make klystron amplifiers—I'd heard about klystron. Then I stumbled onto reflex klystrons, which was not a new idea, but I stumbled onto it independently. Gerry [William Gerald] Shepherd, who's now at the University of Minnesota, and I made some klystrons that were in all American microwave radar receivers. The magnetron was the big thing of the day, but we made these beating oscillators for receivers instead.

Too much freedom is horrible. It's like telling a young child, "Do whatever you want to." You've heard this story. There are various outcomes. One is, "Do I have to do what I want to?" Complete freedom is not very helpful to a person who is inexperienced in the world. It's certainly bad to be directed to do things very, very narrowly and with no freedom. It's my guess that for every person who needs more freedom, there are ten people who need more help in finding their way.

LYLE: So, did they tell you why they wanted the vacuum tubes, when you started off?

PIERCE: Not really. I found out some way, inadvertently. Some people were working on electron multipliers, and I made some improvements on them. It became clear that people needed better vacuum tubes for building negative feedback amplifiers, and I worked on that. I don't think I was told this formally; I just found out by talking to people. Then, as the war approached and we got into war, it became apparent that microwave radar was very, very important, and I worked on tubes for radar. It was a process of osmosis rather than direction that led me into these things, as I remember it.

LYLE: How was the research tied in with the general business of Bell Telephone? That is, what kind of a relationship exists between these two parts of the company?

PIERCE: It's a very important relationship. The Bell System has AT&T, which is sort of a holding company, but it also runs the long lines that provide long distance telephone service. It establishes engineering practices for the Bell System. It owns Western Electric, which is a manufacturing organization, and it also owns, together with Western Electric, the Bell Telephone Laboratories.

I remember that during the war we saw a good deal of people from Western Electric, who were going to manufacture the things that we devised. Because all of these people were engaged in telephony, or during the war because they were all engaged in radar and other military things, you got to talk to people who were engaged in the operation of things, who were engaged in the manufacture of things, and you got a picture of the rest of the world which certainly influenced what research you did.

I can understand a university, which does teaching and research. But the idea of a research institute without ties to either teaching or to manufacturing or operational organization seems a terribly sterile idea. You see that in the Soviet Union; there's a lot of good activity that never results in anything. When they want to build automobiles, they hire Fiat to build an automobile plant, instead of relying on what they have learned.

LYLE: During the war, was there a feeling of urgency about the work you were doing?

PIERCE: There certainly was. We all worked really long hours. We also knew what we were trying to do. By that time, radar was important, and magnetrons or modulators or receiving equipment for radar or making accurate measurements were very, very important. That's one side of it. The problems were very, very clear and were agreed upon nationally, as I saw it. I didn't see the nuclear part. People disappeared, and we always said that the body snatchers had gotten them. We knew very well they were working on nuclear devices of some sort. The other thing was that during the war, by and large, people were engaged in fighting the Germans or the Japanese.

By contrast, my assessment of the present state of the country is that we're fighting one another. Whether it's the universities or the private companies or the government, they're all at odds with one another. Or the government agencies are at odds with one another, inside the government. It's horrible to say that there was anything good about the war, but the idea of a unified purpose and of fighting somebody else, rather than the person who lives next door, is a contrast to what goes on now. I hear President [Jimmy] Carter denouncing the oil companies. I can understand the political value of this. But they're the people who produce the oil we need for energy.

LYLE: So you're saying that it's too bad we can't find a bigger goal, that it would pull them together.

PIERCE: It's too bad, but if he's got to denounce somebody, it's too bad he can't denounce somebody we don't depend on—some wicked person off there—so that if you hate them it won't hurt you, because they aren't vital to your life. If you're depending on somebody to feed you, it's better to hate somebody else and be on good terms with the person who grows the food.

LYLE: In a company like Bell Telephone, how does communication occur? Are there systems set up to ensure that there's good communication?

PIERCE: The management keeps worrying about this. In my estimation, there's only one way that really effective communication occurs, and that is through somebody with information and

interest talking to somebody else who has information and interest. I think that what is on paper is useful in an encyclopedic sense, but it's sort of a monument to knowledge rather than knowledge itself. The knowledge, the expertise, everything that is important, resides in the minds and activities of people in science and technology. If there's going to be any effective communication, it must be among people.

At Caltech, the largest effective technical organization, as nearly as I can make out, is the professor and his graduate students and postdocs, and the agency that funds him. It's true that there are other organizations, but this is the strongest organization. Sometimes several faculty members will be engaged in the same thing, though that differs from department to department. There are seminars.

At Bell Laboratories, the money all came from one place, so people weren't tied to something outside. Moreover, the management, if you want to call it that, or the people in the organization, had a legitimate and persistent interest in what their people were doing.

LYLE: That they don't have here, you mean?

PIERCE: I don't think they have that here. What is the purpose of Caltech? In part it's teaching, a unified action which may be overlooked, but the real purpose is the research. The research is attached to a variety of fields and a variety of government agencies and funding sources, rather than being attached to the chairman of the Division of Engineering and Applied Science or the chairman of the Division of Physics, Mathematics, and Astronomy.

**Begin Tape 1, Side 2**

LYLE: I was reading a book on business management. They say that one important thing about a business is that the people know what the goals of the business are—that is, you have to be able to answer the questions about the goals of this business.

PIERCE: I believe it's bad to be bound narrowly by goals and overlook important new things. On the other hand, it's refreshing to know that the people around you have something in common with you. A wise mathematician at Bell Laboratories, David Slepian, once said to me, "Research should be random in the small, but not in the large." That is, he meant that in your day-to-day

activities, in what you'll do next, there should be no constraint on you, but it's nice to know that in the large, other people are interested in the same thing you're interested in, or are seeking to explore the same general territory perhaps along different routes.

LYLE: And what you're saying about Caltech, then, is that…

PIERCE: Except for education, there's no overall goal. This is just the way it is. The organized activity of Caltech is obviously education in certain branches of science and technology. There is a unified end there, or should be, to turn out students who have had a sensible mix of courses, and have been guided so that they haven't become too idiosyncratic in what they have pursued. If they're graduate students, they should have learned how to do research, which is much more a matter of apprenticeship and good sense than it is of the particular field they work in. They must somehow learn to do independent work. As to the nature of the overall field, Caltech has no scientific or intellectual goal that encompasses the institute as a whole. Indeed, even the major divisions of Caltech, such as Engineering and Applied Science, don't have coherent goals.

LYLE: Do they discuss this? It seems to me that it might be wise to think a little bit more in terms of goals and directions.

PIERCE: I think it would be wise to think. But Caltech is a unique institution. By and large, the students are very good, and by and large the faculty is very good. The idea is, you get very good people and they will do very good work. That is true as far as it goes. Not all the people are equally good. Everyone's equal, but some people are more equal than others—somebody's famous words. I don't know that a university should have the sort of overall goal that the Bell Laboratories does or that Exxon does. You can't quite say that General Electric has an overall goal, because while they used to make power generating equipment and light bulbs and things like that, now they make everything under the sun. There's no reason a university should have a unifying theme, outside of education.

LYLE: I wonder for methods and technique of education, how much communication there is on those skills.

PIERCE: If you ask about education in the sense of skills that you evaluate, this is better understood in what we'd call the training of technicians than by people within education. The higher the level of education, the more difficult it is to think sensibly or to do anything about it. The people who are real experts in the lower reaches of education are the United States armed forces and certain large organizations, such as the Bell System. I think the Bell System, as a whole, probably has more training—this is a lower level of education—more teaching and more thoughtful worry about how you do it, than any organization except the armed forces. Public education is much more compartmentalized, scattered, less pragmatically successful, than the sort of training that goes on in the armed forces or that goes on in large companies. It's done less thoughtfully. You don't find highly competent applied psychologists in public education to the degree that you do in the training in the armed forces or in places like the Bell System.

**JOHN R. PIERCE**

**SESSION 2**

**April 23, 1979**

**Begin Tape 2, Side 1**

LYLE: I'd like to start this interview by asking you to describe how your work evolved in the Bell Labs, from working on the vacuum tubes all the way up to the time you did the satellite work.

PIERCE: I continued to work on the vacuum tubes, because microwave tubes became important during the war. Earlier I worked on vacuum tubes because they were building coaxial cable systems, and I was put in a vacuum tube department, and trying to make better tubes for coaxial cable systems was a great challenge. It led in an entirely different direction, because I tried to make tubes in which an electron beam was deflected. This raised problems of focusing the beam and understanding its deflection. The tube turned out to be no good, but I learned a good deal about electron optics and about making electron guns. Then I applied this when we got into the war, and I made reflex klystrons that were used as local oscillators in radar receivers.

Then, I'd been looking for a broadband amplifier and had had ideas of circuits in which the electromagnetic wave would stay in step with the electron beam, but I didn't build anything. I didn't really know what would happen, despite mathematical analyses. During the war, I saw, in a Committee on Valve Development report from Britain, [Rudolf] Kompfner's work on traveling waves. He found that if you have a circuit in which an electromagnetic wave travels and an electron beam with same velocity, you get amplification. This was wonderful. So I worked on traveling-wave tubes for many years. In 1951, Kompfner came to the Bell Laboratories, and he worked on traveling wave tubes. But then [Walter] Brattain, [John] Bardeen, and Shockley invented the transistor; that made vacuum tubes obsolete. Meanwhile, both Kompfner and I got positions in which we had broader responsibilities.

As far as satellite goes, this was linked to microwave tubes, because satellites use microwave radio transmission. It was linked to traveling-wave tubes, because those are used in the satellites as amplifiers. It was linked to our general interest in communication. In my case, it was linked to science fiction. I was always a science fiction fan. I gave talks on space travel,

with pictures drawn from the science fiction literature.

In 1954 I was asked to give a space talk at the Princeton section of the Institute of Radio Engineers, as it was then. I thought something a little more serious was called for, so I made some calculations concerning communication satellites: corner reflectors in orbit—they aren't good, because they send the beam right back where it came from; balloons a hundred feet in diameter that would reflect a certain amount of power back to the earth; and active satellites that would receive a microwave signal from the earth and send it back in an amplified form. This was fascinating in 1954. And in 1955 I published my talk, because somebody who attended it asked me to publish it, in a journal called *Jet Propulsion*.

This remained at the back of my mind. When *Sputnik* went up in 1957, and the next year, when the first American satellite went up through JPL [Jet Propulsion Laboratory], it looked much more serious. I was sort of conservative about satellites; I didn't know how electronics would fare in space. But I found out that a fellow at the NASA laboratories, William J. O'Sullivan, had actually made a hundred-foot metalized plastic balloon, which he wanted to have NASA launch, to measure the air density at an altitude of a thousand miles. That was just what I was thinking about for communication.

In the summer of 1958, I went to a meeting held on behalf of the air force at Woods Hole. Bill [William H.] Pickering [director of JPL] was there, and Kompfner and I talked to him about the possibility of getting this balloon launched as a communication satellite. He said that JPL would collaborate. Then the problem was getting it launched. We went around to all the different agencies—the air force and ARPA [Advanced Research Projects Agency]. They were all interested in much more ambitious things, but finally NASA agreed to launch *Echo*, and during 1959 and early 1960 we built a ground terminal on the East Coast. JPL built a ground terminal here, and NASA paid for the project, and *Echo* was launched in 1960 [August 12, 1960]. By that time, we were interested in active satellites. The effect of *Echo* was to interest a lot of Bell System people in communication satellites, including the chairman of the board of AT&T, Fred [Frederick R.] Kappel. AT&T decided to go ahead and launch an active satellite, *Telstar*, which was launched in [July 10] 1962.

LYLE: Now, what did *Echo* do?

PIERCE: *Echo* reflected a voice signal from the West Coast to the East Coast. Facsimile pictures were sent by *Echo* also. T. Keith Glennan, who was then the administrator of NASA, visited the Holmdel [New Jersey] site, Crawford Hill, on September 22, 1960. We sent a picture via the *Echo* satellite while he was there, to Stump Neck, Maryland, and then over a phone line. When Glennan saw the picture sent by satellite during his visit, he was rather astonished, because everything worked. Maybe we should have been astonished, too.

The very sensitive receiving antenna with a maser receiver that was built for the *Echo* satellite experiment was the antenna that [Arno] Penzias and [Robert] Wilson used almost five years later in discovering the three-degree cosmic background radiation. At that time, it was the only antenna in the world for which one could make an accurate absolute noise calibration. The reason you could is that it was a horn reflector antenna, which would receive things from the sky but wouldn't receive noise from the earth. It didn't have a lot of so-called sidelobes pointing at the earth. It was made that way because we got very little power from *Echo*—only a billionth of a billionth of a watt, and we had to use that to send a voice signal or a facsimile signal.

LYLE: Who designed this?

PIERCE: The actual antenna is a form of antenna that Harald Friis, long the director of the Holmdel Radio Laboratories, invented. The particular antenna was designed by Art Crawford, an old-time person in Harald Friis's division; Harald Friis had retired in the meantime. Crawford did the electrical design, he did the mechanical design, and the whole antenna was constructed in the shops at Holmdel.

The people at Holmdel were very good in that way; they built everything. The nearest comparable person here is Bob [Robert B.] Leighton, who has built a wonderful large millimeter wave antenna over in the old optical shop. As an excellent physicist, he knew what he wanted, but also he invented a way to make it. He is a good mechanical engineer and a good electrical engineer as well as a fine physicist.

The maser receiver at Holmdel, the ruby microwave maser receiver, was the outcome of work by various people in the physical research area of Bell Laboratories.

LYLE: So actually it was sort of your idea and then you had to talk to other people?

PIERCE: I had to get some enthusiasm both inside the Bell Laboratories and outside the Bell Laboratories. A lot of people were drawn into the *Echo* experiment. It took people in the part of the research department that was under my direction and a lot of people outside to even do *Echo*. To do the *Telstar*, Western Electric was drawn in.

It was a little difficult at first to get support for the *Echo* satellite, because Mervin Kelly, who was president of the Bell Laboratories, thought it wasn't a very good idea. But Mervin Kelley retired [1959] shortly after that, and Jim [James B.] Fisk, who was his successor, believed that satellites might be of some use.

We did a lot of interesting things in *Echo*. It was tracked by computer—we predicted where it would be in the sky, and the antenna tracked it automatically. We used the first maser receiver—very low-noise receiver—for satellite communication. We used a particular form of modulation called frequency modulation with feedback to minimize the amount of power we would need. Many of these things—not tracking, because the satellites are now geostationary—carried over into later satellites.

Getting *Echo* launched was a matter of convincing people that it was worth spending some time and money on something they could be enthusiastic about. It just took off; this happens sometimes. As an illustration, we didn't really have to ask people to abandon research on vacuum tubes—the people who had been doing it just got interested in other ideas, such as transistors or masers and lasers.

In general, the best way to go from where you are to something new is for the new thing to be attractive and to attract people away from the old thing. Bell Laboratories had one funding advantage over university funding, in that there was so much money around and it followed whatever work people were doing. It was easy to abandon the old work. Some people claim now that the government financing of research has gotten so bureaucratic that if you want to change what you're doing, it's very difficult. But there wasn't any such problem with that at Bell Labs.

LYLE: So you think that a private company sometimes is more responsive.

PIERCE: It is more flexible in the research. In development not so much so, because there are all

sorts of cases drawn up telling just what is to be done and how many people it's going to take. In the research area, whether it is physical research or whether it's research on experimental, forward-looking communication systems, you write some. You don't write fewer reports than you do now in a university, because in a university you have to write reports for the contractor. At Bell Laboratories, if one thing proved unprofitable and another thing looked good, it was very easy to change from one thing to another. I think that's quite important in research, whether it's in a university or someplace else. If you come to a dead end and something else looks more attractive, it's best to change.

LYLE: Then with the *Telstar*, there was a different kind—it amplified the …

PIERCE: It amplified the radio waves, and you could send television over Telstar. It wasn't a geostationary satellite. It was up about 3,000 miles, so you had to track it. It moved across the sky, but not as fast as Echo.

Then the Bell System was legislated out of the satellite business by the Communications Satellite Act of 1962, which passed shortly after *Telstar* was launched. This act said that all international satellite communication had to be carried out through a new company that had a monopoly. So the Bell System didn't do any work on satellites for quite a long time. Now they're working on domestic satellites.

LYLE: What happened in the Bell Telephone Company when that bill was passed?

PIERCE: They were unhappy.

LYLE: What did they do about it?

PIERCE: Stopped work on satellites—they'd spent $50 million, but there was no future in it—stopped work for quite a number of years until satellites became useful for domestic communication. Comsat did not have a legislative monopoly, a legally granted monopoly, on domestic satellite communication. So now the Bell System has, in cooperation with other people, a domestic satellite called *Comstar*.

Private regulated companies engaged in research have a great deal of trouble, but they have more trouble with the laws of man than the laws of God. The laws of God are physics and chemistry, what you have to do to make something work. Whether you're allowed to do it, to spend the money on it, is a law of man. California got a court decision about accelerated depreciation. The federal government had passed a law that by bookkeeping in a certain way, you could retain some of your earnings. The idea was that it would be used for research. California said that the retained money could not be used for research; it had to be passed to consumers by lowering telephone rates. Right now, the Bell System is not getting this extra money for research from the operation of the Pacific Telephone Company. Moreover, if Congress or the courts don't resolve this, it'll have to pay out this same money also to the federal government as income taxes, because it isn't doing what the federal government said it must do with that money. The law governing companies is very, very strange.

That sort of thing doesn't occur in universities. Universities can do almost anything—but by and large they can do only what the federal government will supply funds for doing. Really, the government has become all-pervasive in determining what people can do in science and technology, in one way or another. In the case of the Bell System, they had to abandon satellites because a monopoly had been given to somebody else. In the case of the universities, mostly you can do research only if you can get it funded—though there is some other money. The funding process has been fairly reasonable in the past, but people worry about doing only what the government will allow them to do. In recombinant DNA, which is a long way from me, they almost clamped down on work entirely, for other reasons.

LYLE: Why did the government give this monopoly to another company?

PIERCE: In retrospect, it's a little hard to tell. I think the strongest reason was that the government had spent a lot of money on space through NASA, and they wanted to see that no existing company should use for their profit any of the results of their expenditures. This is very odd, because for years the government has done agricultural research and has been very anxious that people should use the results of the agricultural research. I just don't understand it.

Also, there were people in the government at that time, and still are, who believed that anything done by, any money spent by, any service rendered by a nongovernmental agency was

ill spent or ill rendered. They managed to make Comsat look sort of like a government agency, and they gave it a monopoly on a very profitable but very narrow technology. The members of the board of directors are partly appointed by the government. It was sort of gilded by the government, so it wouldn't be bad.

I was very unhappy about that, but you get used to this. Recently somebody said something about fairness, and I said, "Well, life just isn't fair, you know," and a mother who was sitting next to me, said, "That's the most difficult thing to explain to children." People have certain ideas of fairness, but they're what people call normative ideas—they're the way things ought to be, they aren't the way things are.

LYLE: In all of this time that you were working at Bell Labs, did you continue your writing?

PIERCE: Of science fiction—I wrote a little science fiction for many, many years [under the name J. J. Coupling]. When I came to Caltech, I got so busy that I only wrote one story, I guess, and it wasn't very good and didn't get published. I found that adjusting to a very different and a very interesting environment took a lot more time than continuing in the old rut I'd been in at Bell Laboratories—and it was a rut.

LYLE: You mean after 1971?

PIERCE: Yes.

LYLE: I was just wondering about the writing in general. Did you get an agent?

PIERCE: I got an agent only toward the end of my writing career. Most of my writing I did without an agent.

LYLE: So you would just send your writing off to different publishers?

PIERCE: Yes. I'd just send it off to magazines and get either a check or a rejection slip. The agent was helpful in one way, because he almost sold a story to *Playboy,* and they wanted it

rewritten. It was eventually published in *Penthouse*. I'd never have thought of sending a story to *Playboy* or *Penthouse*.

LYLE: When you are writing, do you have a routine for writing? How do you do your writing?

PIERCE: Oh, usually I have some idea, and I think it over, and I think it over, and I think it over. Then on some occasion I just sit down and write.

LYLE: But you don't get up early every morning and write two hours.

PIERCE: No. At home, when I wake up early I sometimes do a little writing there—not of stories but of other things I want to write—because it's undisturbed. Writing is a thing that is very hard to do if you're disturbed during the process. I know a science fiction writer, Harry Harrison. When he lived in California—he now lives in Ireland, because there's no income tax for writing there—he had an office out in the garage, and his children and his wife absolutely could not go there when he was writing. Robert Heinlein had a special room. He often wrote at night. He would unplug the telephone and write. I think that many people who write find it very difficult to write if there are any distractions or disturbances.

LYLE: And you were able to write more, though, when you were working at Bell Labs? Now, why was that?

PIERCE: The chief reason was that there weren't any classes. I'm not teaching now; I'm here on half-time, but for the first few years I was here, I taught. At Bell Laboratories there was no such thing as having to be at a particular place at a particular time. There weren't many fixed obligations of that sort. You did work, and sometimes there'd be committee meetings, but very, very few of them. There weren't students, so there were fewer people to come around and talk to you. It didn't mean you didn't talk to a lot of people, but students need longer conversations to get something across than colleagues. I think my time was more my own while I was at Bell Laboratories. It's just the way the place is organized and what it's doing.

LYLE: I would like to know how much leeway you had in doing research at Bell Labs.

PIERCE: The research at Bell Laboratories exists because somebody believes that in some broad sense it's germane to communication. They don't work on genetics, for instance, because no one could think of a relation between genetics and communication. If there were a relation, one could. They do work in superconductivity and semiconductors and people have got Nobel Prizes in these areas of solid state—Phil [Philip W.] Anderson most recently [1977]. They work on microwaves—Penzias and Wilson both work on communication problems, but they also spent part of their time measuring noise in the sky and received the Nobel Prize [1978] for that. If a person is very good, no one will much question what he is doing. My observation was—this is not a policy of the Bell Laboratories, but it's my observation of how things work—that work will be encouraged either because it's of a very, very high quality or because it's very useful. The less inspired it is, the more useful it has to be to be encouraged, and the more inspired it is, the less immediately useful it has to be to be encouraged.

LYLE: You worked on the President's Science Advisory Committee.

PIERCE: My boss, Bill [William O.] Baker, was very much interested in Washington and spent a lot of time there. He had been on the President's Science Advisory Committee in an earlier time. Somehow I was made a member, from 1963 to 1966. In the early days, the President's Science Advisory Committee did very important work in connection with defense, because then the armed services hadn't acquired the sophistication and the resources that they now have. Although it did good things while I was there, I felt that the President's Science Advisory Committee was perhaps not as great an influence as it had been earlier under [James R.] Killian and [George B] Kistiakowsky. A fellow member, friend of mine, whom I guess I won't name, said that he had never seen so many smart people reaching conclusions on so little evidence.

LYLE: How much time did you have to spend?

PIERCE: Oh, gosh, I can't remember. I think it was a couple of days a month. I spent quite a lot more time on a couple of occasions. I felt toward the end of my tenure that I really hadn't done

anything while I was there. I'd listened to a lot of interesting things, including how the war in Vietnam was going, which wasn't well. So I got a committee formed on computers in higher education, and I chaired that committee and we got our report adopted by PSAC. It had some influence, though the big spending days were over at that point, so it didn't get as much money for computing in the universities as we had hoped. PSAC looked into a large number of things—and I'd have to see all the reports to tell you what. I think PSAC was a useful activity, but one thing it didn't face up to was the allocation of funds.

The traditional way to get money for science was to keep all the money you already had and then propose something very, very jazzy and new that would get more money. I raised the question: There's only so much money, and maybe if you got it for something you'd have to take it away from something else. That was a very unpopular idea in those days. At Bell Laboratories, there were a more-or-less fixed number of people—oh, it increased recently—and a sort of fixed income, and people dropped things as the things got older, and that made the people and the money available for new things. Somehow with the government spending money, it seems very unlikely that anything will ever stop.

LYLE: What was the function or the purpose of the committee?

PIERCE: The purpose of the committee was to advise the science advisor to the President, who then kept the President informed about things, or to make studies of timely topics. As I said, in its early days there were lots of defense matters, which are handled differently now, and it was invaluable in bringing a sense of science and technology into the White House. I attended one meeting with President [Lyndon] Johnson. I think he'd forgotten who we were. He thought we should get out to the hustings and help him, as nearly as I can remember the tenor of the thing.

LYLE: What do you mean?

PIERCE: That's where you vote, in English novels. He wanted support for the administration. Indeed, in many cases, PSAC was called upon to put out fires or to have wonderful new and novel ideas that would electrify Washington. It was a mixed business. Of course, the President's Science Advisory Committee was finally abolished under Nixon. Then a presidential

science advisor was created in the Carter administration and was given a very small staff and no advisory committee.

I wish I could have taken notes or could remember everything that went on in PSAC. But I'm afraid that aside from the work I did on the committee, on computers for higher education, I have a very vague memory about those years.

LYLE: OK. You decided to return to Caltech, and I was wondering why?

PIERCE: First of all, I was offered a position here that was very attractive. Second, California is very attractive, and Caltech is very attractive. I'm now sixty-nine; I'd have been retired for four years [if I had stayed] at Bell Laboratories, where you retire at sixty-five. It gave me an opportunity to be active for a little longer than if I'd stayed at Bell Laboratories, and I thought it was nice to leave Bell Laboratories a little early, while I still had more of my active life ahead of me in the university, than to wait until the last moment. I was sixty-one when I came here, in 1971.

LYLE: Coming back to Caltech and electrical engineering, and all this time had passed—how had the approach to teaching and doing research in general, in electrical engineering, changed?

PIERCE: In some ways a great deal—the content has changed. In some ways, not so much. It's still a very small faculty, and there are more students, more enthusiastic students, in electrical engineering. There are more students than anyone knows how to teach. It's a very attractive and lively subject from the point of view of the students. From the point of view of the faculty, the EE faculty—I don't remember the exact numbers, but if you compare it with engineering and applied science in general, the majority of the faculty appointments are in other fields and an awful lot of the students are in electrical engineering and computer science. The sort of thing that goes on in Steele, the device work that is a part of applied physics, all of those have lots of students but small numbers of faculty. But electrical engineering had a very small faculty when I was an undergraduate and graduate student, too, and it relied very much on the good teaching in other fields of physics and chemistry and so on.

LYLE: When you were a student, Royal Sorensen was here, and they were working on the high-voltage power lines. Are there any projects going on now that are kind of very visual, the kinds of things that students could see?

PIERCE: Let me say something about the power lines just in passing, because it takes me back to PSAC. There was the great blackout in New York [1965] when I was a member of the President's Science Advisory Committee, and of course that was of concern to them. They sent out a search for experts who might know what was happening. They didn't find anyone in any of the universities anymore who knew anything about large power systems. They found a few people in a corner of the General Electric Company. But large power systems, which were such an important thing at Caltech in the time of Sorensen, completely passed out of the larger universities. Power is coming back in some, at MIT for instance. But in some ways, engineering in universities has got divorced from the life outside.

I can cite another example. In European universities and research institutes, there's a lot of work on architectural acoustics. There's very little in this country, and as a result many of the buildings turn out badly, or else the person who does the acoustical design is from some other country. Cyril Harris at Columbia, who redid the concert hall at Lincoln Center—for the last time, we hope—is an exception. Large areas of science and technology that are very important aren't represented in universities. It's a peculiarity of the American university system. But no, there's nothing of that magnitude.

There are important coherent areas of work at Caltech, such as the quantum electronics that [William B.] Bridges and [Amnon] Yariv do. But the most organized work is going on at JPL rather than on campus. The way for faculty or students to be associated with that is to work part-time at JPL, as I do, or for the students to have part-time work. And, of course, we've drawn on JPL—[Edward C.] Posner and [Lawrence L.] Rauch are teaching here on a continuing basis. Other people come down from JPL to teach in electrical engineering, and that gives a sort of experience or interchange with large projects that one wouldn't otherwise have. In Sorensen's day, he was very close to these large power companies, and you had an association with large projects in that way.

LYLE: Well, what's your feeling about this association with JPL?

PIERCE: I think it's very good. It means that the students can come in contact with a wider range of things through working at JPL, or having people from JPL teach them, than if all the association were with Caltech faculty or all the teaching with Caltech faculty. We have a small electrical engineering faculty here, even by Caltech standards. You can only have good people and good work in a few areas. The world is much larger, and through an association with JPL—in electrical engineering, at least—you can draw on a broader field of expertise than can be represented in the faculty.

LYLE: If you look at the engineering school here, how does communication occur between electrical engineering and hydraulic engineering?

PIERCE: Rarely.

LYLE: Is anybody working on that problem?

PIERCE: I don't know. Maybe Roy Gould is; maybe [Caltech president Marvin L. "Murph"] Goldberger is; I'm not. I worked a lot in trying to bring about closer relations between JPL and the campus in electrical engineering. That's been my thing. You can talk about everything, but you can't do everything. I think what I have done has been moderately successful.

There is this, another problem. Universities tend to be a lot of very independent people pursuing divergent goals. It's hard to assure communication. I suppose one way is through going to seminars. I find that I don't go to as many seminars as I should, so I don't find out what the other people are doing.

LYLE: What are you doing at JPL?

PIERCE: That's a little hard to say at the moment. I'm a member of a group of people that I'll meet with tomorrow in Bob Stevens's division, to consider long-range plans. I work on a few particular problems concerning modulation and quantum effects in communication, and on the degradation of resolution in radars, that I've become aware of through going up there. I've done

work myself, and some of my students have worked on these problems. I intend to spend somewhat more time at JPL, as I spend less time on the campus this winter.

LYLE: What do you think about the kind of research they're doing at JPL, as compared to Caltech?

PIERCE: Research at JPL is somewhat constrained by the lack of independent funds for doing research. Most everything has to be funded. JPL work is more in touch with real engineering problems. Whether this makes it better or worse, I won't say. Research there is apt to arise not because it's a particular field of science or technology being pursued but because there's a real problem concerned with radars going over Venus, or how do you measure things on Mars, or something like that. You find out that no one has really thought hard about something. People certainly don't know all the answers, and you wonder if there isn't a better way. Perhaps, as the space probes have to go closer and closer to planets and maybe land things on them, navigation has to be better. You may need better ways of navigating. This raises the question of using Very Long Baseline Interferometry [VLBI] as part of navigation, which ordinarily the radio astronomers would use because they're interested in the heavens.

Facing up to such engineering problems makes people think of new approaches. Since there aren't these large engineering problems on a university campus, engineering research tends to arise through faculty consulting arrangements, or because something is hot in technical meetings and is well funded by some government department. Often in university engineering research, you don't see much of the connection between the research itself and some overall problem.

LYLE: At Bell Telephone you said that the research had to somehow be related to communication. Is there such a restriction on the research at JPL that you can think of?

PIERCE: Oh, it has to be somehow related to something you can get money for there—that's a little different restriction. It doesn't have to be space; it can be energy. But the relation at Bell Laboratories was very broad. A lot of very good mathematics was done there in certain fields; and, as I said, superconductivity and the behavior of the solid state.

I think that it's very important for large laboratories to have some sort of charter.

**Begin Tape 2, Side 2**

PIERCE: When I was on PSAC, I took part in a study that never resulted in the writing of a report. The question was: How good or bad are government laboratories, and what, if anything, should be done about them?

We visited a lot of government laboratories. My feeling was that government laboratories had good people and poor people in them, and that the average was quite good enough to do important things. Many of the laboratories weren't doing important things, because no one was really depending on them. No one said, "It's really your responsibility to see that progress is made in this area that is of interest to the government—to see that things are done well, and that they really get done." Instead of that, some laboratories were what I'd call job-shops; they had a lot of little contracts. People would give them a little money to do this, a little money to do that. There was no unifying theme; there was no overall challenge, as there was in JPL for many years and still is. JPL has a responsibility for deep space planetary missions. JPL will be judged as good or bad depending on whether this whole area of work thrives or not, whether it is productive. I think that for a large laboratory that doesn't teach, it's very important that there be some responsibility and some general goal, so that you can measure progress against a broad goal and see whether various activities are really pushing you in the direction you want to go or you're just wandering about.

I saw a report made by a committee in the Department of Energy recently that said that many of the Department of Energy laboratories needed new charters that would tell them what they were really responsible for. Sometimes laboratories lose their function. No one is looking toward them to do something. That's a terribly discouraging state. In essence, no one cares what they do anymore. It's much more exciting to be on the spot, as Los Alamos was when it was making atom bombs and nothing else, and the whole success of nuclear weaponry depended on them. Universities are different, of course: They teach students, or at least they're supposed to.

**JOHN R. PIERCE**

**SESSION 3**

**April 27, 1979**

**Begin Tape 3, Side 1**

LYLE: I'd like to ask you to discuss behavioral science a little bit.

PIERCE: After a reorganization at Bell Laboratories, which I could date if I thought back, I found the work on speech and hearing in my division, and also a few psychologists. My boss had decided that if psychology, or behavioral science, was really a science, it ought to have some contribution to make to the Bell System.

In teaching, I believe that the Bell System, next to the U.S. Army, is largest; it has more teaching, in the sense of training, than any other organization in the United States—certainly far more than the schools or colleges. And it just might be that psychologists could help in handling management affairs. So they had imported some social psychologists and some experimental psychologists.

The social psychologists lived a life of their own; they didn't really interact with the other people. They studied communication in small groups and got subjects from nearby colleges. I got one of them to go out to California, to the Southern Division of the Pacific Telephone and Telegraph Company, for a month. The vice president there treated him very well. He sat in on committee meetings at all levels. When he came back, I talked with him. He said he'd had an interesting time. I asked him, "Well, is there anything in your art or science that could be applied in what you've seen in the Bell System?" He said yes, that if he could hire an assistant—who was presumably less able than he was—something might be done. I myself thought that the problems were probably very complicated and that a less able person couldn't cope with them, and this fellow didn't want to cope with them. Eventually we came to an amicable parting of ways. He went back to a university, where I believe he collaborated in writing a book on stopping wars. I haven't looked around recently to see whether or not this book has succeeded. He did not make any substantial impact on the Bell System.

LYLE: You had hoped that he would be able to get communication better within a group at Bell

Telephone?

PIERCE: Yes, or that he could do something that would make the organization work better—Bell Laboratories, or the operating telephone company, where he was shown around for a month. He was interested in larger and somewhat more abstract problems. The experimental psychologists, on the other hand, made friends with mathematicians and with engineers and interacted a great deal. When an engineering training program was set up by AT&T near Chicago, one of the experimental psychologists went out to help them evaluate their work and make it more effective. The experimental psychologists were much more interested in their surroundings than the social psychologists were. One was very interested in programmed learning—that was a great thing in those days—and he actually went out into an operating telephone company and located a course and provided programmed instruction methods for teaching it. This proved to be very much more effective than the traditional way of teaching the course, probably because the various teachers didn't pay much attention to the material they were supposed to teach. The experience was that as the course was ordinarily taught, a fairly large fraction—maybe thirty percent of the people—didn't learn much. With programmed instruction, it took longer for some to get through the material than for others, but they all learned a substantial amount, going through at their own rate.

Programmed instruction isn't the only way to teach, and some of the psychologists at Bell Laboratories have been interacting with people who write instructional material and material teaching craftsmen to perform their tasks. Some of the psychologists work in a deeper way, on short-term memory and long-term memory. All of this I could understand; it was very much like engineering on the one hand or science on the other. You got quantitative measurements of things. What you did either worked or didn't work, and you understood it or you didn't understand it. I never felt as easy with the social psychologists. I picked up some very nice phrases from them, such as "the pathetic fallacy" and "cognitive dissonance." These are useful in a general conversation; they tend to floor the other person. But I didn't feel that I had a firm grasp on what they were trying to do.

I became very friendly with the psychologists. They have a terribly difficult field, because human beings and human behavior are so complicated. But psychologists come in a number of varieties. Experimental psychologists insist on confronting the outside world in a

detailed way, but there are doctrinaire psychologists who make up sweeping theories—a good deal like Karl Marx's theory of economic behavior—that may or may not have anything to do with the world. There's sometimes a strong scholastic emphasis, or current, which causes some psychologists to produce very closely reasoned ideas that aren't checked at every point with experiment.

Some of the theoretical linguists are like that. They are extremely plausible. But I know a fellow, Victor Yngve, who tried to write a transformational grammar of the English language, a reasonably complete one. It took him years and years and he never got it written. He kept finding difficulties that don't appear when you have a few nice examples of what a transformation of grammar is supposed to be all about.

LYLE: Well, what about psychology in Caltech? Have you met any of the psychologists who are working with the students?

PIERCE: No, that's an entirely different thing—that's clinical psychology, and I've had only personal encounters with clinical psychologists. They're like doctors, and in some degree a little bit like doctors before antibiotics. Antibiotics always cure certain things. Indeed, since they have appeared, the biologists have found out why they do it and how they do it. I think a lot of wisdom went into medicine before there were so many surefire remedies. I'm sure a lot of wisdom can go into clinical psychology, but I'm not at all familiar with it.

LYLE: With your interest in psychology, have you gotten to know any of the neurobiologists?

PIERCE: Yes, I've known some of the neurobiologists. I've known Roger Sperry for many years and have admired his work and understood it to some degree. And I know in general what [James] Hudspeth and [Masakazu (Mark)] Konishi are working on. I've looked into Konishi's experiments a little. I feel deprived in some ways that I haven't spent more time looking into those things. At Bell Laboratories I was in an entirely different position. It was my business to know what the people in my division were doing. I even published a paper with an experimental psychologist at one point.

I think that neurobiology is a wonderful field. Things are going very rapidly. They are

learning things that not only push the science forward but tell us about things important to human beings—for instance, that certain aspects of the visual sense develop very early, and if people are deprived, they don't develop right. I've heard it said that if failure to fuse the images seen by the two eyes isn't corrected early, people don't develop a sense of depth. Although it doesn't come directly from the work at Caltech, I think it's now generally recognized that if children are deaf it's important to give them hearing aids very, very soon, in order that they won't be handicapped in the development of language. These are things that rest on hard knowledge and can be extremely valuable to people.

LYLE: You're going to Paris on Monday, and it sounds like you're going to be doing some computer music work there, so you must be very involved with that and interested in it right now. Could you talk about that?

PIERCE: Through psychologists whom I've been on good terms with, I've developed a deep interest in the psychophysics of sound, or psychoacoustics—the relation between the acoustic stimulus and what we perceive internally—how it strikes us, what we can distinguish. When speech and hearing work was put in my division at Bell Laboratories, I fell in love with it. There was a lot of computer processing of speech. Instead of constructing an experimental communication system out of hardware, people began to simulate on the computer all the operations of an actual system that you might build. This was an exact simulation, not like a simulation of the economy. What happens to an electrical signal—a speech signal, for instance—is just exactly what would happen in a lot of special hardware. But you get the results in a week or a month instead of a year. In such simulations, after you had reduced the sound to digital form, you reconstructed the sound from a stream of computer-processed digits—it occurred to some of us, especially to Max Mathews and me: Why couldn't you just put in a stream of digits and get out any sound that you wished?

We did that in a primitive way about twenty years ago, and gradually, Max Mathews wrote a number of programs for producing sounds very flexibly by means of a computer. The most recent is Music V. That means that there were four earlier versions. Music IV was written in the current Bell System assembly language. It wouldn't run after we lost the 7094 computer; this was a lesson. Music V is written in a more popular language called Fortran, and Music V or

some variant of it must be in use in twenty or thirty places, largely schools of music in various universities but also at IRCAM, the Institute for Research and Coordination of Acoustics and Music in Paris. It's in use, I know, at MIT and Stanford, and it's been put on other computers. People have used it to produce a variety of very pleasing sounds.

The first computer sounds sounded very electronic. More recent sounds don't sound like ordinary instruments, but they don't sound electronic either. They sound like charming bells, gongs, and other sounds.

LYLE: I don't understand how the sounds are produced.

PIERCE: Any sound is a variation of pressure in the air. You could describe that by a sort of graph of pressure plotted against time. The pressure goes up and down with time. There's a mathematical theorem which says that if the bandwidth or frequency range of a sound wave is not any wider than, let's say, B, if you measure the amplitude of that wave at two B times a second, you can reconstruct the wave exactly from the measurements. That means if you want a frequency band 10,000 Hertz or cycles per second wide, then you must measure the sound pressure 20,000 times a second and get 20,000 numbers a second. These completely describe the sound wave. They describe the sound wave with perfect accuracy if the numbers are perfectly accurate. In a digital computer, no number is perfectly accurate. But if you describe the number to, say, sixteen binary digits, the description is accurate enough. So, with a certain error, by having the computer generate 20,000 sixteen-bit binary numbers a second, it can in effect generate any possible sound wave with a bandwidth of 10,000 Hertz.

LYLE: Yes, but an instrument has a lot of overtones, which are the characteristic of wood or metal. Are these picked up in the wave?

PIERCE: If the overtones all have frequencies below 10,000 Hertz and if you take 20,000 samples a second, all the overtones will be reproduced. If you generate something fresh, what do you want? Do you want to put in some nice geometrical waveform, or do you want to add up a lot of overtones? Well, that's where the rub lay. Obviously you aren't going to specify individually, write down by hand, 20,000 numbers a second. You're going to choose them in some orderly

fashion.

In playing an instrument, the instrument produces waveform in an orderly fashion but a fashion that is constrained by the instrument. Many computer experiments have been made by making up waveforms by adding many overtones together. That's what I'm going to experiment with at IRCAM.

I've done some experiments already. In all instruments except drums and gongs and bells, the overtones are harmonically related to a fundamental frequency. That is, if the lowest frequency and the wave is 200 Hertz, there will be overtones of frequencies 200, 400, 600, 800, 1,000, 1,200, 1,400 and so on. It is my belief that conventional harmony, the way that different notes go together, is dictated by this orderly, harmonic relation of the overtones in which all the overtones are multiples of the fundamental frequency.

Quite a number of years ago, I made some experiments; I published a paper on this in 1966. ["Attaining Consonance in Arbitrary Scales," *Journal of the Acoustical Society of America,* 40:249.] I have believed that you can have very nice sounds with nonharmonic overtones. Indeed, you do have very nice sounds, gongs and bells, in which the overtones are not harmonic. The trouble with gongs and bells is that two sounded at once don't go together very well. Maybe you've heard someone play two parts or harmony on a carillon. It sounds terribly jangly. The harmonic effect doesn't really appear.

I have experimented in the past. I intend to do more work with nonharmonic overtones chosen so that if you sound several notes together, they won't clash, they won't sound dissonant. The idea is to stretch the whole octave. Imagine going up the octave on a keyboard. When you get up to an octave, the frequency ratio has changed by two to one. I intend to make it change, say, two-and-a-half-to-one instead of two-to-one, and stretch the spacing of all overtones in the same way.

A master's student here and a PhD student at Stanford have tried this at my suggestion. Such notes sounded together don't sound dissonant. When you play a piece of music, the melody sounds all right. The chords that go with it don't exactly sound dissonant, but they don't seem right. The progressions don't seem right. And that is something we're investigating.

I believe that one could go beyond this and find more complicated, nonharmonic overtone structures that would go together nicely. I chose this one because there is an alleged law of psychoacoustics that explains dissonance. It says that if too many overtones of two tones

you sound together are too close together in frequency, you'll get a harsh sound. That is indeed so. Just by stretching the octave, this rule tells us that if two notes don't sound harsh before they're stretched, they shouldn't sound harsh after they're stretched, because where the overtones lie is just a stretched version of where they did before. Stretching won't put them close together.

LYLE: So this is how you have combined an interest in music and psychology and electrical engineering?

PIERCE: There's a little bit of all that in it. In the past there was a very serious effort to relate science and technology to music. [Jean-Philippe] Rameau proposed a scientific theory of harmony. In the nineteenth century, [Hermann] Helmholtz knew of Rameau's work, and Helmholtz was the first to feel that dissonance was caused by the component frequencies of tones that were sounded together, being too close to one another, so that they beat and sounded harsh. In Helmholtz's *On the Sensation of Tone*, he speaks a lot of this. Other people, [Reinier] Plomp and [Willem J. M.] Levelt, have pursued this further.

The musicians of this century—except for the ones who have taken to working with computers at Stanford, Bell Labs, MIT, IRCAM, and a few other places—seem to be afraid of science. They love mathematics, in the sense of numerology, but the actual physics of sound production and why it is that things sound different is foreign to their thinking. They want magical recipes for putting things together. I think one can find important relations between science and music, but that takes an awful lot of work.

For instance, there's a fellow named [Johan] Sundberg at the Royal Institute of Technology in Sweden. For many years people have wondered what it is that the singing teachers teach the singers to do so that their voices will sound well and will cut through an orchestra. Sundberg found out in great detail. An account of his work was published in *Scientific American*. [Sundberg, Johan, "The Acoustics of the Singing Voice," *Scientific American,* March 1977, p. 82] Sundberg found that singers learn to configure their throat in a particular fashion and produce a resonance up around 3,000 Hertz, well above the speech range. There's a lot of energy in a good baritone voice at quite high frequencies, where there isn't much energy in the orchestra. So the singer can be heard through the orchestra. Good singing has been taught successfully for many years, but I don't believe that the teachers knew what they

were teaching the singer to do physically, or why it succeeded. They could hear by the ear when they got the right effect. Sundberg comes along, and by a lot of very careful measurements and investigations he finds out what is really happening.

LYLE: Another thing that you've been interested in is Japan, and you've traveled there a lot, and you thought that it had a strong influence on you.

PIERCE: I've been interested in Japan for a long time; I haven't traveled there a lot. I've been there twice, once for about three weeks and once for about three days. I would never have gone to Japan if Harvey Eagleson, who taught English here when I was an undergraduate and graduate student, hadn't collected Japanese prints, and if he hadn't loaned me Arthur Waley's translation of the *Tale of Genji* when it first came out. I found a certain charm in Japan—and escape, perhaps. I read other translations from the Japanese. About thirteen or fourteen years ago, I was invited to visit Japan and talk at a number of universities, and to bring my wife along. I found Japan just as charming as I had expected, though a little disconcerting. In most European countries, and even in Russia, you can make something out of the signs, but not in Japan. In Japan I met a number of people whom I see from time to time. Last Saturday I saw Hiroshi Inose, who's a professor at the University of Tokyo. When we first went to Japan, he went with us from Tokyo to Sendai and to Tohoku University. That is where Professor [Hidetsugu] Yagi invented the Yagi antenna. Most TV receiving antennas are varieties of the Yagi antenna. Tohoku University is a very well known engineering school, but many people who visit Japan don't go there, because they tend to go south to Osaka, instead of north to Sendai. Near Sendai we visited Matsushima. That's a wonderful place on the sea, where there are a host of strange little islets. There they also cultivate seaweed. It's a very lovely place.

I've seen Professor Inose many times since then. Oh, and I met a Dr. [Hanzo] Omi of Fujitsu; I saw him just a week ago last Tuesday. He came and had dinner at our house. He's on his way traveling to the East Coast and to France. He will be in Rome, he tells me, when I go there to receive the Marconi International Fellowship.

LYLE: Why don't you tell me a little bit about this award, and why you're getting it?

PIERCE: Marconi was married twice. This award was organized by one of his daughters by his first marriage, Gioia Marconi Braga—she's married to a gentleman named Braga. It is awarded through the Aspen Institute for Humanistic Studies, with the headquarters at Boulder. The award is made once a year for things appropriate to honoring Marconi. James Killian, who was once president of MIT, received the first award—he's been a sort of statesman of science. He was the first President's Science Advisor. And then [Arthur L.] Schawlow was honored for the invention of the laser. Colin Cherry received the award for his work on human communication. Hiroshi Inose was one of the previous recipients. He has done a great deal of work at the University of Tokyo and elsewhere on electronic switching. He worked on electronic switching long before electronic switching became a reality, and he invented something called time-slot interchange, which is important in making electronic switching more efficient.

I was given the award largely because of my work on communication satellites, because *Echo* was my idea and led to *Telstar*. The award is $25,000, which you can use very freely for work that you wish to undertake. Inose used it in traveling and in writing a book on digital communication—a very wonderful book. [*An Introduction to Digital Integrated Communications Systems*, Univ. of Tokyo Press, 1979.] I don't know exactly what the other people have done. Schawlow will describe what he did in Rome.

LYLE: What are you going to do with your money?

PIERCE: I'm going to do two things. I was asked—because Professor Inose suggested it to the *Proceedings of the Institute of Electronics Engineers*—to write a tutorial paper on the past, present and future of satellite communication. I think this is a nice thing to do, and the money will enable me to visit Comsat and a number of other places and get their views on this. I want this to be more than just an academic search of the literature. I want to talk to the people who were actually doing things and ascertain—I followed it a little bit—what their views are on what are really important. Of course, in the end it will be my views, but I want to hear their views first.

The rest of the money I want to devote essentially to writing a book on the psychophysics, or psychoacoustics, of musical sounds. Not about music, because that is too much for me, but about the sounds used in music—whether they are natural sounds or computer

sounds. About things that are important, for that matter, in high-fidelity reproduction. About what we know about the process of hearing that tells us what will be deleterious in a hi-fi system or in a sound. What is known about consonance and dissonance. How this ties in with what is known about hearing. I want to try to take into account the phenomena of psychoacoustics that I think are very important to music. I will merely mention phenomena that, while they are very interesting to psychoacousticians, are hard to observe.

The ear is remarkably linear. You can find nonlinear phenomena in the ear, even at low sound levels, but you have to have rather sophisticated experiments to measure them. My feeling about that, for instance, is that as far as music goes, the remarkable thing is how linear the ear is—not that if you look very, very closely you can't find nonlinear phenomena.

There have been a number of books about acoustics of music. Helmholtz, of course, wrote a great book, but he worked with very simple mechanical apparatus. Then, Fletcher wrote a wonderful book, not about music but about speech and hearing in communication [Harvey Fletcher, *Speech and Hearing*. New York: Van Nostrand, 1929] He was the first, or one of the first, to use electronic methods in the days of vacuum tubes, before computers, to perform experiments and make studies. Since then, computers have become a very powerful tool for generating sounds and for handling data. A great deal more has been done in psychoacoustics, and I hope to take advantage of this. For instance, one could have conceived of all the sounds I will be working with while I'm at IRCAM in an earlier day, but one could not in any practical way have generated them—certainly not by mechanical means. Stretched strings have almost always harmonic partials, and blown pipes or blown instruments always have harmonic partials. If you make a bell or a drum, you get nonharmonic partials, and if you change the shape, they'll change. But it's very, very difficult to make a shape of a bell that would give partials lying in places where you'd want them.

LYLE: What would you like to see happen at Caltech? That is, how do you envision the future of Caltech?

PIERCE: Oh, everyone has a different Caltech. I have some things that I would like to see—some in general and some in particular. In electrical engineering and computer science—and, to a degree, in the part of applied physics that has to do with communication—I would like to see a

closer relation with the Jet Propulsion Laboratory than we have now, as a source of teachers and jobs and work for students and support for students. There are many interesting new technological projects and planetary projects up there that put devices and computers and technology and processes of communication in a special, challenging context. They illustrate how devices and techniques fit together to accomplish something. They also show you where there are problems—what things one can't quite do now. I think that that can be valuable to engineering students.

I would welcome the idea of a thesis, and perhaps even a bachelor's thesis; they have those at MIT. It gives the student an opportunity—or a challenge; you can look at it either way—to go beyond the material he has learned in various courses, to go beyond the idea that a problem is something that refers back to the chapter you've just read. A thesis makes a student see how things fit together, either in a theoretical project or an experimental project. Students work very hard at Caltech, and they learn a lot. In some of the laboratory courses in electrical engineering, the project laboratories, they learn to fit things together. I think, at least for engineering students, that the opportunity to consider a problem that's broader than the one in the textbook and to bring various knowledge to bear on it and try to reach some conclusion about it is good.

LYLE: This will add another year or something, right?

PIERCE: It wouldn't necessarily add another year in the master's degree. Would it add another year for bachelors? I'm not sure. That depends on what you want to do, how much you want to cram into students. I heard a discussion at a table at the Athenaeum that indicated that in Physics 1 they were trying to cram too much into the students, and the students didn't remember it a couple of years later. It's of very little use to teach students things they aren't going to remember. So maybe one thing is, to find what you want the students to remember, how much you expect them to remember after the four years, and then go backward and see what you'd have to do to ensure that they not only are exposed to this but learn it well enough so they will remember it.

In my own undergraduate and graduate years, I noticed that the only things I remembered were those that I found in several contexts. I'd been exposed to them once, then I sort of forgot,

and then I was exposed to them again and I understood them better. After about three times, I actually began to understand and remember the things.

When I was working with the experimental psychologists at the Bell Laboratories who were working on a programmed instruction, they found that it [if you] tell a person something once and have him work a problem, he won't remember it; he needs more reinforcement in order to remember it. Immediate reinforcement doesn't do any good and may even make him forget. If you do essentially the same thing twice in short succession, it doesn't help. There is long-term memory and middle-term memory and short-term memory. Somehow you've got to get what you teach into the long-term memory. Repeating something twice in short succession doesn't help at all. If, when the student has sort of half forgotten, you teach him again, that has a positive effect. And if you go on doing this at spaced intervals, eventually you may get what you teach into his long-term memory.

This was shown experimentally by a psychologist named [Ernst] Rothkopf. At that time people were blindly and blithely writing programmed instruction in which all the repetition was immediate, instead of being spaced out. This was almost guaranteed not to teach the people anything.

Oh, another thing about Caltech. Somehow the students have got to get to know the faculty better. It's better in some places and with some people. I feel that in biology the students get to know the faculty better. I have advisees; I just wait for them to come in, and they only come in when they have trouble or when they have cards to be signed. I've had several seniors come around who want recommendations written for some university. I have asked them, "Well, what faculty members do you know best?" They don't know any of them very well. The recommendation they'll get is, "Well, he got an. A in my course and he seems to be a nice fellow, you know." At least in EE. Of course the EE faculty is very, very small, and there's a tremendous number of students. But it's very bad when a student gets to be a senior and doesn't know even one faculty member on personal terms, if for no other reason than that he can't give a sensible recommendation.

I don't know what would help. I think it's partly the students. If they don't understand something, they're too proud to go and ask the faculty member; they'll work it out themselves.

LYLE: Are there any activities that are planned where the students and faculty might have a chance to know one another?

PIERCE: I think such activities aren't enough. You could have teas, you know, and things like that—that helps. But students and faculty should interact in connection with course work or laboratory work. One should get the feel of the person on a technical level. I don't know how to do it. I've been very busy around here—I don't know where all the time has gone. I have spent a lot of effort trying to improve relations with JPL. I've got people from JPL to teach on campus. I've encouraged my students and other students to go up and get jobs at JPL. I've tried to get support for student research from JPL. There are many other things that I haven't spent a lot of time on. It's just that a student will become a senior and not be well known to any electrical engineering faculty member. I don't know what to do about it.

**Begin Tape 3, Side 2**

PIERCE: I was talking to a biologist recently, and he said he has a small class of about twelve undergraduates and he divides them up into groups of about three and they work on projects. That is good for the students; it's a good form of teaching. It's good for dealing with the students, because you can deal with them in some way in smaller numbers. You'll deal with the ones that are on one project all together, and you get some interchange with yourself. You'll get more in dealing with them, I think, than if you were trying to deal with every student individually. You learn more about every student.

LYLE: Which professor was that?

PIERCE: I believe it was Ray Owen.

LYLE: But your classes are too big to do it that way?

PIERCE: No, they weren't too big. I just didn't have the right touch in dealing with a dozen or so students.

LYLE: Maybe if you'd had small problems and could assign three or four people to a problem….

PIERCE: There's always the tutorial way of Oxford and Cambridge. Each student used to spend a couple of hours a week with a tutor on a subject. Now the tutor usually takes two or three people in the same hour. Even that's an entirely different way of teaching. Certainly, classes with seventy or eighty people in them aren't suitable for individual interactions. Yet it seems to me absolutely essential that everyone who leaves here as a senior should know one or more faculty members closely.

When I was a student at Caltech, I got to know some members of the humanities faculty well. I have found all sorts of people over many years who remember Harvey Eagleson and were close to him. I wasn't quite as close to the engineers. I got to know Mackeown fairly well and I got to know Fred Lindvall fairly well, and [Francis W.] Maxstadt.

LYLE: Did they have any of these occasions for you to know them?

PIERCE: I got to know Mackeown well because he was engaged with many patent cases, and I did some work for him in tracing the circuits in radio to see if they had infringed on patents. I got to know Lindvall personally because he's such a nice guy and because he taught a project course in which you had to study something and then write a sort of engineering report—that's more personal than just a lot of questions or an examination. Maxstadt was my thesis advisor, so I got to know him well. I got to know an undergraduate professor, Clyde Wolfe, who left while I was at Caltech. When I met him walking, he'd be glad to walk along and talk, chat about this and that—it was just his personality.

LYLE: Is there anything else that you can think of that you would like to see Caltech working on to somehow change a bit?

PIERCE: I'll say this. Fields in which a great deal can be accomplished by an individual person with reasonable resources change from time to time. Biology is a wonderful field. Today resources have to be considerable. But there are just all sorts of things opening up in that area—neurobiology is one of them. You can see that there's tremendous progress. In high-energy

physics, I think progress is getting more and more costly and difficult.

LYLE: Do I hear you saying that what you want to find here at Caltech is research where an individual can still be the one in charge, versus the group?

PIERCE: I guess, yes, that's nice, and the other thing I was saying is that any university lives in a changing world. At any one time, some things just seem to be opening up; just a little more push finds out a great deal. That's one side of it; that's the science side and it deals with the works of God. Then there's the engineering side, and that deals with the works of man. There, some things seem to be very exciting. When I was at Caltech, the most exciting thing was aeronautics and [Theodore] von Kármán, although that wasn't the course or the direction I followed. Now, I think, without much question, computer science and communication—digital communication and the solid-state physics devices that go with it, integrated circuits—are very exciting to students, both because the fields are moving very rapidly and because there are a lot of well-paid jobs in these fields.

A university has a lot of inertia built into it. It is made up of tenured faculty, many of whom work on the same subject from the day they start till the day of their death. Happily, many of them don't; they change fields and change interests. I think one of the great problems for a university is how to adapt to changes in fields of science, so that it always has something going on in fields that are at the cutting edge and haven't become too cumbersome. Caltech has high-energy particle physicists, and that's all right, but if they want to be connected with experimental things they have to be connected with the results of the experiments in large national laboratories, because the equipment for this area of physics.… Millikan can no longer have Vic [Henry Victor] Neher building electroscopes and flying them up in balloons.

This raises a problem for a university: What is being in tune with the times? What times should you be in tune with? How are you sure that you're lively and right with it and at the same time not crazy or unorganized? I don't have any good advice to offer; I'm just saying it's a real problem. Things that don't change get dead after a while, whether they're people or institutions. I see Caltech as very exciting, but you can also see within it this problem of the things that are left over, things that were more popular at one time than they are now.

LYLE: So you're thinking they need some kind of really thoughtful investigation?

PIERCE: I'm sure that [Caltech president] Marvin Goldberger is a very thoughtful person and thinks about these things. I'm sure some faculty members do.