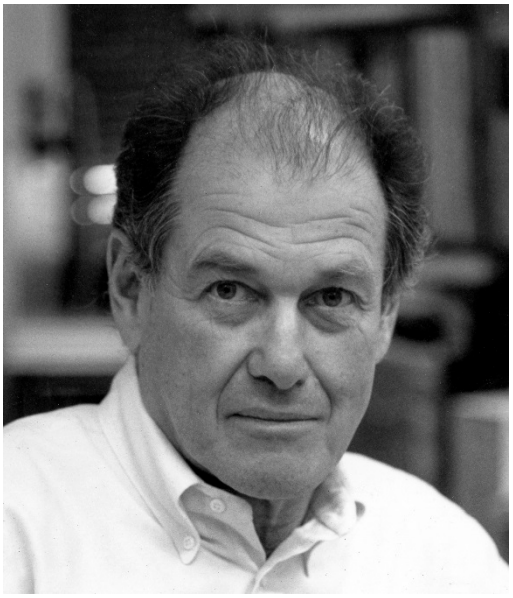

JEROME PINE
(1928 - 2017)

INTERVIEWED BY
SHIRLEY K. COHEN

October 16, 23, and 30,
and November 6, 2001

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, neurobiology, public education

Abstract

An interview in four sessions, October-November 2001, with Jerome Pine, neuroscientist and physics professor in the Division of Physics, Mathematics, and Astronomy. Recalls graduate school at Cornell (MS with Philip Morrison, PhD, 1956, with Kenneth Greisen); instructorship 1956-62 at Stanford Linear Accelerator Center; arrival at Caltech as associate professor in 1963.

Member of Caltech high-energy physics user group at SLAC and Fermilab; early involvement in science education; takes up neuroscience; 1978-79 sabbatical, Washington University Medical School with W. Maxwell Cowan; neurobiology workshop, Woods Hole, summer 1978; summer course with John Nicholls, Cold Spring Harbor, 1979; invents multi-electrode device to record action potentials from cultured neurons; sets up Pinelab; prevalence of physicists in neurobiology. Recalls graduate students; discusses his neurobiology course.

1987-88 sabbatical in U.K. at Medical Research Council Laboratory with Dennis Bray and Kings College-Chelsea on science assessment in schools; NSF grant to study science assessment in elementary schools. Works on elementary-school

science education with James M. Bower in Pasadena school district; with Bower and Jennifer Yuré, visits Mesa, AZ, school district. Pilot program, Field School, later expansion; partnership with Apple; establishment of Project SEED (Science for Early Educational Development). Involvement of Georges Charpak; program in France. Establishment of CAPSI (Caltech Precollege Science Initiative); developing content modules for teacher education; grant from NSF Centers for Teacher Enhancement.

His innovations in Caltech undergraduate physics; two-track Physics 1 course; take-home physics kits. Freshman seminars; teaching atomic physics to juniors. Caltech's lack of interest in CAPSI; CAPSI's research division; Caltech's promotion of student diversity; Lee Browne's minority-students program. Concludes by discussing spread of his science-education programs to Colombia, Estonia, and Sweden.

Administrative information

Access

The interview is unrestricted.

Preferred citation

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ORAL HISTORY PROJECT

INTERVIEW WITH JEROME PINE

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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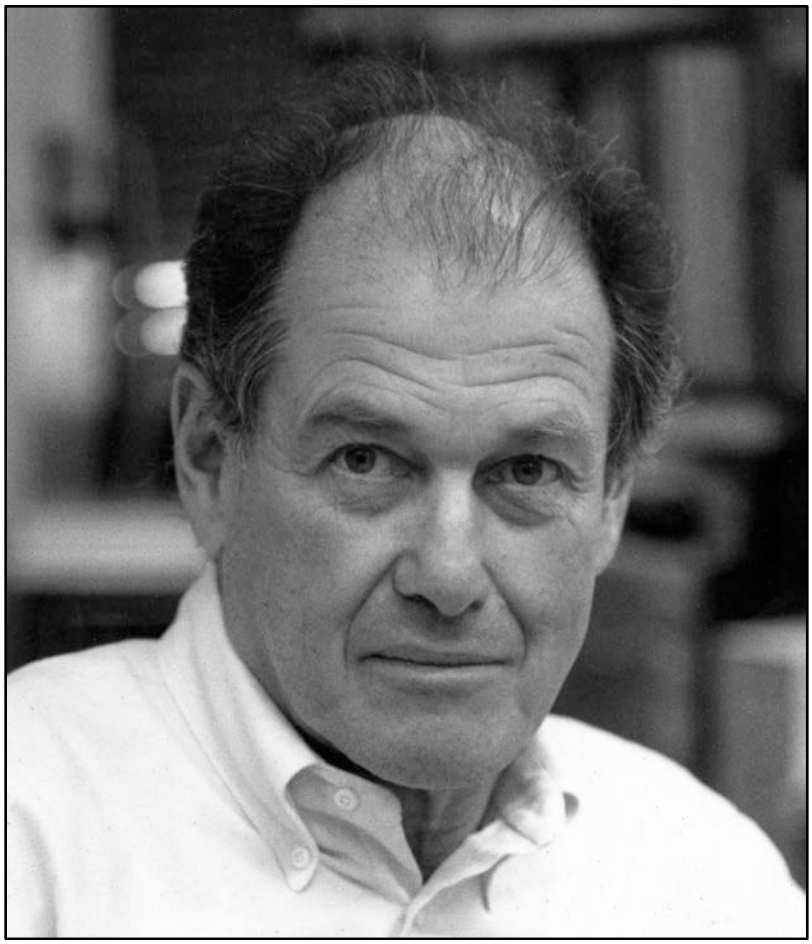
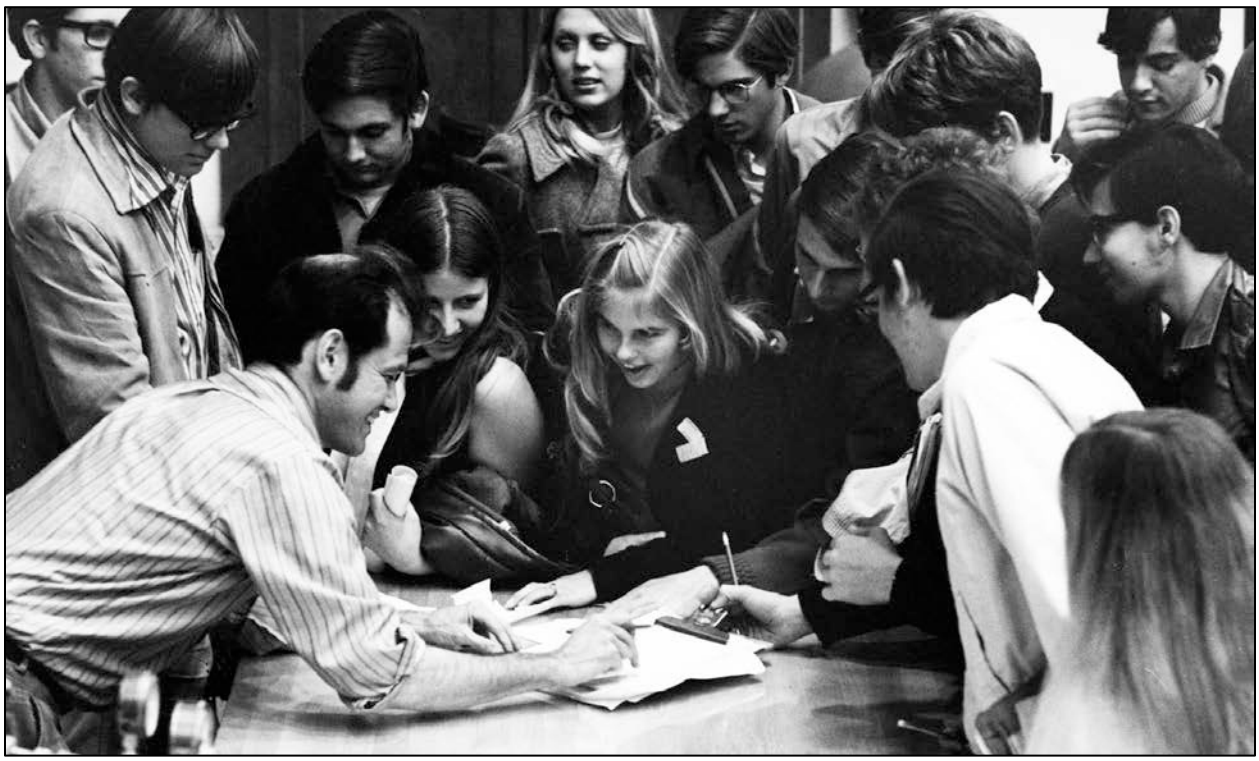


Jerome Pine

Photo by Bob Paz



Jerome Pine in classroom with high school students, answering questions after his talk on detecting sub-nuclear particles.
January 6, 1971

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Jerome Pine **by Shirley K. Cohen**

Pasadena, California

Session 1	October 16, 2001
Session 2	October 23, 2001
Session 3	October 30, 2001
Session 4	November 6, 2001

Begin Tape 1, Side 1

COHEN: Why don't you start by telling me a little bit about your father and mother.

PINE: Well, I can tell you a little, but not much. My father was born in New York City of people who had emigrated from the Ukraine around 1900—Jake and Esther Pine. Jake Pine was a successful plumbing contractor. They lived on the Lower East Side, and he did plumbing around the New York City suburbs. But he died when I was very young, so I never met him. My father was brought up in a non-affluent society. He worked his way through Columbia and he worked nights in a Western Union office. He got an engineering degree around 1920, maybe a little earlier, from Columbia. And he did not get a job. He was Jewish; there were no Jewish engineers—it's not one of the things you do. So while I was growing up, he was marginally employed as a salesman in various vaguely technical sales positions. He ultimately went to law school, when I was eleven or twelve. He became a lawyer, but he never practiced, because he got involved in the export-import business. He was in some ways an influence—but not much of an influence—on me. He was busy getting rich, or trying to, and traveling a lot. Right before we entered World War II, the companies he was exporting for became very popular as a source of expensive parts for the military overseas—Lend Lease—and because he was their export agent, he owned a piece of the proceeds. So he became very, very rich. He flipped out, like many people do who were poor when they were little, and became the world's greatest woman chaser, among other things. He ultimately ran off with a woman and shortly thereafter got in

trouble with the Internal Revenue Service and went to Mexico. So in fact my relationship with my father is minimal but not zero. I mean, I was around watching him flip out, but I was only about thirteen and very square.

My mother was probably born before her family emigrated. She was about two years older than my father, so she was born around 1899. Her family became—I think of them as being marginal farmers, in the New York suburb of Jamaica, which is now, of course, in the middle of New York, but this was at the turn of the century. She was one of several children, all of whom I got to know, whereas my father was one of several children only one of whom I really got to know. My mother did finish high school, I think, although I'm not sure. She commuted from Jamaica a long distance to the Bowery, where she worked in a cigar factory as a typical child laborer. There my father found her. She was very pretty. He romanced her and married her, and she ended up supporting the family more than he did, for a long time. She was a self-taught bookkeeper, so when I was growing up she was working as a bookkeeper. And then when they got rich, she was a middle-class housewife with a vengeance. But then when he ran off with a redhead to Mexico, she got a reasonable amount out of him. She was bitter. She was a typical, very, very unforgiving Jewish person who'd been wronged—as in "He ran off with that *shiksa!*" I think she used to stick pins in a statue— I mean, she was never healthy about it. But she went off to Florida and married somebody she had once known as a child. I didn't see a hell of a lot of her after I got out of college.

So those are my parents. And I was an only child; I had no siblings. But I have a wonderful half sister, whom I've gotten to know only since she was over twenty years old, because she was brought up in Mexico. She's a redheaded, half Irish person who fled to the far northeast of Maine to start her life. [Laughter] She's a delightfully good person, and we're sorry that we don't see her more often, because she lives too far away. So I do have one sibling that I actually have a connection to.

COHEN: Where did you go to high school?

PINE: Brooklyn Technical High School, along with David Goodstein [Gilloon Professor of physics and applied physics, emeritus; vice provost 1987-2007], another famous Caltech person, but not at the same time—earlier. It was very elite to go there, but in fact the kids who went

there typically did not have enough resources to go to college. So it was not like the present Bronx Science and Stuyvesant world; it was a vocational high school. It was spun off from Manual Arts High School in Brooklyn, and it had a very selective vocational school environment: You had to pass an exam to get in. But a great majority of the students didn't go to college and became pseudo-engineers; they studied engineering skills in high school. So it was a good invention; it didn't propagate itself, but it was a good invention.

During World War II, I was getting ready to go to college. I had a friend at Brooklyn Tech named Mike, whose father was a terrific entrepreneur—not very successful but one of these really hardworking Jewish businesspeople who was forever having a new business. And Mike was sort of like that, too. He was a politician. He was the one who said I should go to Princeton because if you went to Princeton you would be connected to the power structure.

COHEN: It's true.

PINE: Oh, absolutely! [Laughter] I'll tell you about him! It all worked out the way—when he was only seventeen—he thought it would. We went to Princeton together at a time when there were no students, because the war was still on. So we got in—otherwise we would never have gotten in. We were the first people who ever went there from Brooklyn Tech. [Laughter]

COHEN: Yes, but your family already had some money.

PINE: My father was at that time very well off and chasing women, and we had money, and we had moved into a very fancy house in Jamaica Estates, a small, wealthy enclave. So there was no money problem.

COHEN: How did you find Princeton?

PINE: Oh, it was pretty interesting. I don't remember it as being an unhappy period. But it was very difficult. There were almost no Jews, and it was the last bastion of the Southern aristocracy—and still is. So there were these eating clubs and other rather selective activities that generate a certain amount of stress. And I was just a nerd. My friend Mike immediately became a member of the junior varsity football team and a well-known campus figure among the

power structure—which is what he had gone to Princeton for. He was very smart. He did well; he got to be an engineer. I was a nerd and studied physics and billiards—I did a lot of pool playing. I quit engineering as a goal, because I found out that physics majors and physicists could work on whatever they wanted and engineers had to do what they were told. To me it was the same thing otherwise, because the physics I did was always engineering, right from the start. I was never a longhaired physicist.

COHEN: You were not a theorist, is what you're saying?

PINE: I was not even a very good experimentalist. I was not a very good student. There were only seven physics majors in my class, and I think my grades were either number five or number six. But I was very good in the humanities. My grades were far better in English than they were in physics. But I actually liked doing physics—I was getting paid for playing with my electric trains, as my roommate at Princeton used to say. He said that I just wanted to get paid for playing with my electric trains, and he was right. He was also a not-very-well-off son of an entrepreneurial family, like my friend Mike, and he also did very well. Both of those guys came out of Princeton with what they went there for—entrée to riches. And they both found it. And in those days it was for doing things, not for playing the stock market. Ultimately my friend Mike became the chief executive officer of the Van Heusen shirt company and had a triplex on East End Avenue. And one morning he woke up when he was about fifty-five or sixty and realized that he'd married this Jewish-American princess and his life had been a disaster, and he left. He left her all the money and everything.

COHEN: Where did he go?

PINE: He founded a new business and started all over. And he was very successful in about five years. [Laughter] He took a few people from Van Heusen with him who didn't want to work for a big company. Unfortunately, he had a heart attack and died not very long ago, so he didn't live very long. But it was a good story. He had a very, very good second wife; he married a travel agent, a charming person.

COHEN: So you went on to graduate school.

PINE: I went to graduate school at Cornell, because I decided I should do that as a physics major. You had to go to graduate school—it was just built into the system.

COHEN: It didn't occur to you to do anything else?

PINE: No. And I got in, which was surprising. It was very surprising. I had an interesting career there, because I didn't do very well in my class work. So I was assigned to a great man, whom you have probably heard of—a man named Phil Morrison, who became, as you know, interested in education. In those days, Phil was a young associate professor, or maybe only an assistant professor.

COHEN: What year was that?

PINE: I graduated from Princeton in 1949, so I went to Cornell in '49, and Phil was there. And evidently—even in those days, I guess—he was always interested in trying to do good. And he became involved with a number of students who were told to get a master's degree and get out. And to do that, you had to do a thesis, so Phil had a succession of master's people. And I was sent to him, as the exit route. I'm his only existing experimental physics student. So I went off and did a rather bizarre master's thesis under Phil's aegis—experimental.

COHEN: What did you do?

PINE: Well, he invented a geophysical concept of why the isotope ratios of helium were bizarrely variable, depending on where the helium came from. And being an intellectual, this was not going to make any money for anybody. But he was interested in this issue—how could this happen? He's a nuclear physicist, so it was natural for him to deal with this problem. And so he invented a nuclear physics explanation, based on the nuclear physics of natural radioactivity and the trace elements of naturally radioactive substances. He hypothesized that the helium isotopes came from a particular series of nuclear reactions that depended on the environment in which the radioactive decay of the uranium or thorium took place. So he said, "This is a great project, and I want you to take some rocks down to a very, very quiet place, which is a salt mine under Lake Cayuga, and count the radioactive decays and essentially detect the neutrons." His hypothesis

was that the alpha-particle decay of the naturally radioactive substances produced neutrons in (alpha, n) reactions. Alpha particles come out and they hit the neighboring materials—which don't have to be special—and they can knock neutrons out. The neutrons that the (alpha, n) reactions produce can be absorbed by lithium-6, and some of them can be absorbed by everything else. If they're absorbed by lithium-6, they make helium-3, whose abundance varies widely. I've just re-created what I did fifty years ago, give or take. So I was sent to the salt mines. It was actually very beautiful.

COHEN: I've never been in the salt mines. How did you get there?

PINE: They're very deep, you know. They're 2,000 feet down under Lake Cayuga, and they're like a cave of diamonds. They're on the Cornell side of the lake—well, Cornell's at the end of the lake, west side of the lake, five, ten miles out of town. You park your car, go down this long 2,000-foot hoist, which is used to haul people and salt, and carry your physics down to the bottom and build an experiment and do it. You see, I was an engineer—I always was an engineer. So I did it, and we won a prize, because Phil—in addition to doing everything—knew that there was a prize for research in natural radioactivity, and there was no research in natural radioactivity at that time. So when he found this prize, he said to me, "We're going to win $500." And I said, "Oh, that's very nice. How come you're so sure?" And he said, "Because nobody else is going to submit an entry for this prize." [Laughter] So that was fun. And the bottom of my car rotted out, because it was parked next to the salt mine. And I got my master's degree so successfully that I was recruited to stay.

COHEN: Did you continue working with Phil Morrison then?

PINE: No, I could barely pass his course. But I went to work for a very nice guy who at one time wanted to be a divinity student, which meant that he was very gentle and nice. His name was Ken [Kenneth I.] Greisen, and he became a dean and all sorts of bureaucratic things, but he was a great person. I did a cosmic ray thesis for Ken, and I was a TA [teaching assistant] for Ken in a freshman physics course for the smart kids.

Then I got my PhD degree [1956], and I had a very interesting mind-set—which was that the best thing I could do was go to a small liberal arts college and teach. I remember having

some discussion with Phil about this and discussing where I should look for a job. And I was on the way—I'd been interviewed at Reed and I'd been interviewed at someplace else, and I probably would have been very happily ensconced in those jobs, except that somebody from Stanford called up Ken Greisen and said, "I need a graduate student who knows how to run a cloud chamber." Engineering again. And I had become a great expert on my PhD thesis with a brilliant person whom you should have known—my co-student, probably the smartest graduate student Cornell ever had. His name was Dick [Richard Joseph] Davisson, and he had the problem that his father was a Nobel Prize winner [Clinton Joseph Davisson, 1937], and his uncle on his mother's side, Owen Richardson, was a Nobel Prize winner [1928]. Both of them in physics! And [Thorstein] Veblen, the famous economist, was related to him. So I had this wonderful colleague who was so fantastically smart—but he never got his PhD. We worked together building cloud chambers, and he never finished.

COHEN: He didn't want to compete with his family.

PINE: He couldn't. It was terrible. I've known a lot of Nobel Prize winners' children—god, what a group! And not a single one I know— Just the other day my wife, Nancy, asked me to contact some Nobel Prize winners, because there was a free trip to China. I know eight Nobel Prize winners—it wasn't so hard to find eight that I could e-mail. I e-mailed a couple from here; I don't know all of them from here, but I know about six others. I don't know any of their children who did well. But anyway, that's neither here nor there.

So I stayed at Cornell and became a cloud chamber expert and got a PhD. And then Stanford said, "You can have a job at the Stanford Linear Accelerator Center [SLAC]. We have these jobs called instructorships, and they are renewable for two three-year periods, and then you have to leave." But it was big-time physics.

COHEN: You went as a postdoc?

PINE: No, no, it was a faculty job—full-time teaching job. They could afford to pay people as instructors rather than assistant professors; this was the last generation of lower-than-assistant-professor faculty—that's everywhere, not just at Stanford or Cornell. So I succumbed to the temptation to go to the big time. I went there and had six years of fun doing high-energy physics

and helping to build the big accelerator, which was invented while I was there. Pief [Wolfgang K. H.] Panofsky—a great man—was the boss of the high-energy lab on campus, and he became the boss and creator of SLAC. He was wonderful. And after six years I had to find a job.

COHEN: So it really was a finite amount of time.

PINE: Yes, except that while I was there they broke the rule for one of us. They did hire Burt [Burton] Richter as an assistant professor, because they felt he might win a Nobel Prize, which he did, of course [1976].

COHEN: Some criterion!

PINE: No, that's not why—he was very successful, highly successful. Also, the era of the instructor was going away, because we had six really sharp people, except for me—people who were at the peak of their powers—and they all got extremely good jobs when they left.

COHEN: Well, I think coming here would be considered a good job.

PINE: Yes, but I just barely got a job here—I got a job here by accident. I got this job because Matt Sands [professor of physics 1950-1969; d. 2014] knew from previous things that we had been involved in together, and from my career at Stanford, that I was an educator.

COHEN: Let's go back to your Stanford period.

PINE: Well, I was an educator as well as a researcher.

COHEN: What do you mean by that? You enjoyed your teaching?

PINE: Yes, and I taught the big course for non-science majors—the pre-med-and-other-people course.

COHEN: You had good students.

PINE: Oh, yes. Well, Stanford students aren't bad. Well, Cornell students aren't, either. I mean, I don't think there's a great difference between the prestigious places and the not. In fact, most of the students I know here are not any better than the ones at the less prestigious places. In many ways they're worse, because they've been handicapped by too much testing. Most of the successful graduate students here—that I know, at least—came from small liberal arts colleges, not your hotshot fancy places. Anyway, that's neither here nor there.

Matt Sands was a great, interesting physicist—extremely creative both in theory and experiment and in engineering. He was a founder of the Caltech Synchrotron group, along with Bob [Robert L.] Walker [professor of physics 1949-1981; d. 2005]. He and I met at various meetings that had to do with education.

COHEN: Now, when you say "education," you're talking about—?

PINE: College education. I got involved in precollege education because of Phil [Morrison], and that was after Stanford. So at Stanford I would meet Matt at meetings of people in physics who were interested in education. [Sands served (1960-1966) on the Commission on College Physics, which carried out a national program to modernize physics instruction in American colleges and universities—ed.] And here at Caltech he was very important. Although Dick [Richard P.] Feynman [Tolman Professor of Theoretical Physics, d. 1988] did a lot, Matt in some sense engineered that situation. It may well be that Matt is the person we should thank for the Feynman lectures.[1]

COHEN: Well, wasn't he one of the people who—?

PINE: Well, his name is on the book. There were three slaves who didn't do anything except publish Feynman, but they may have actually had some input into what Feynman wrote—told him that certain things were just too outrageous. And Matt was very smart, so he could have done that.

So Matt decided to have a midlife crisis and leave for UC Santa Cruz, which, as you know, was invented as an educational institution. It was the campus of the sixties. So he went

[1] Richard P. Feynman, Robert B. Leighton, & Matthew Sands, *The Feynman Lectures on Physics* (Reading, MA: Addison-Wesley, 1963).

off, carrying the torch, and [later] so did two great biologists. [Professor of biology William B.] Wood went [1978]. And [professor of biophysics] Robert Sinsheimer [1977].

COHEN: Sinsheimer was the chancellor there [1977-1987].

PINE: Yes, exactly. My son went there, actually; it was very good for him. So, to make a long story short, Matt was concerned that there be somebody [at Caltech] who cared about education, and I was looking for a job. I had done high-energy physics, and Matt was part of the high-energy physics group. So he sold them a bill of goods on me, although I had published one paper maybe—not many, I'll tell you. So I came to Caltech [1963] and became an educator here, just as Matt intended me to do.

COHEN: As an associate professor?

PINE: Yes, tenuously—associate professor without tenure. There was a moment of truth after three years, but by that time I had connected to two wonderful people, Ricardo Gomez [professor of physics, d. 1996] and Alvin Tollestrup [professor of physics 1962-1977], and we had established the Caltech high-energy physics user group, which became essentially the Caltech high-energy physics group, because the Synchrotron on campus became obsolete. Bob Walker wasn't ready to take a lead in that, but he joined it. So we had a very strong group, and my engineering skills were put to very good use. We became very successful, so in research I got enough out of that to get tenure [1966].

Meanwhile, back at the ranch, I had become involved in science ed in elementary schools in the summer of 1962. In 1962-1963, the year between Stanford and Caltech, I was a visitor at Imperial College in London. And on my way to Imperial College I was roped into a summer workshop in Newton, Massachusetts, by Phil Morrison, where hands-on science education for elementary-school children was invented—truly, essentially invented. Very exciting! Phil and two guys from MIT invented the conference.

COHEN: Now, he was still at Cornell, wasn't he?

PINE: Barely. He was romancing Phylis [his future wife], and he was with her at that conference—she was from Bell Labs, I guess. So Phil invited me to this K-6 workshop, which was populated by a wild assortment of people and was a thrilling experience—which somebody should have documented but didn't. That's a complex story. I wish somebody had written it; it would be very nice to interview the managers, of which Phil is the only one still alive. [Philip Morrison died in 2005—ed.] It was an extremely purposeful group of sixty people, and we worked in Newton High School, in a lab. Well, we worked all over the high school—fifty or sixty of us. Of the fifty or sixty, about half were working every day, all day and all night, and the other half were pundits who came and interacted with us—like the best cognitive psychologist in the country, Jerome Bruner, from Harvard, and various other very good people. We were a group of teachers from schools and scientists from universities, working together as partners. I worked in a group, which I founded and led, that produced an elementary-school science unit on electricity—which is still one of the units that's used in every curriculum in the United States that does this. It's probably the major intellectual output of my career, because all the other things I've done became obsolete as science progressed. And there's such a paucity of work in this field that something I did in 1962 is still being used in Pasadena as part of the curriculum. The idea of that whole curriculum was invented that summer. And there are other units—very few—that survive in full detail like that one.

I was commuting from a house in Duxbury, which was a long drive, and I worked hard all day long in Newton, in a group of four people—a wonderful elementary-school teacher, an electrical engineer from Cornell, me, and a guy who ultimately became the headmaster of a private school, who I guess at that time was a private-school science teacher. It was very focused, so I don't have the big picture. The big picture is in the hands of people like Phil. Phil worked in one of the groups—he worked on the unit on sunlight and shadows for kindergarten kids—but he also was an organizer.

COHEN: And what was the idea of going to Imperial College?

PINE: It was just having fun, like academics do. I worked hard there on a project.

COHEN: You must have known somebody there.

PINE: Only vaguely. Others connected to the cosmic-ray world—that's a cosmic-ray place. It was an NSF [National Science Foundation] fellowship, so they didn't have to pay me.

COHEN: Did you have a family already by this time?

PINE: Oh, my first wife, and a child—my son. He was two. And when we came back from England, she left me. So that got me to Pasadena as a loose-gun bachelor, and I had a pretty interesting five years being a loose-gun bachelor. Now I have two other children and another wife.

So I came here and got involved the way Matt wanted me to, in college education. I became involved a little bit in K-6 ed, too, because I didn't just stop after the workshop—but not very much, because I inherited Feynman's lectures. I was the patsy who had to give them after him. I taught freshman physics for several years while I was founding the high-energy physics group. And with my friends—thank god, because I wouldn't have done it by myself—I became a very successful researcher and leader, actually, of big projects in science. I became the PI [principal investigator] of a large five-university project at Fermilab, which was at the beginning of Fermilab. It was one of the first experiments done. I wrote the grant and formed this collaboration. We had twenty-five PhDs—this was probably about 1971 or '72. Meanwhile, between 1963 and then, our group did experiments at every international and national lab in the world.

COHEN: So you were traveling all the time?

PINE: Right, and I was commuting to Stanford for some years, because I helped build SLAC even after I left. So while commuting to SLAC and teaching full-time here, I met my present wife, Nancy, and then we commuted together every summer to the world. And then I commuted during the school year somewhat. After I'd been here for seven years, I went to Bob [Robert F.] Christy [Institute Professor of Theoretical Physics, emeritus; d. 2012]], the provost, and I said, "I want a sabbatical." And you know Bob; he's about a foot and a half taller than I am, and he looked down on me—in more ways than one—and said, "We don't give sabbaticals." So I went home and had a conversation, and then I went back and I told Bob, "I'm taking it anyway." I called his bluff. I don't know how it worked its way out, but I got half salary for a year. I had

designed a sabbatical for myself, which was to go to the Sierras and build a cabin—and Nancy was actually the moving force, because she's a whimsical person. And even though we had no idea how to do it, she thought it was a good idea. We bought the land on a lark sometime earlier, without knowing what we'd ever do with it. So not only did Christy knuckle under with half salary but he helped raise the roof on Bastille Day in 19-whenever. So that was exciting. We had a wonderful time. We had a huge group from here—about twenty-five people. We had to raise a very, very complex, heavy structure on the side of a very steep hill, without machinery.

COHEN: I've been in that cabin.

PINE: You were in it, before it burned down?

COHEN: Yes.

PINE: Oh. Well, you know, it's the same again. We paid a fortune to have it re-created—it's almost like rebuilding a cathedral.

I had found a person in the group to run the Fermilab collaboration while I was on sabbatical, who then quit as soon as I left town. So one week a month, I left the woods, which made my family very sort of jealous in a way, because we lived in a tent for five months, and one week a month I was back with hot water and civilization. We did go to the hot creek every night. We were raising a little child, and we had my older son with us for the year. So we had a good time.

Anyway, this huge collaboration—when I came back from sabbatical—ultimately functioned at Fermilab. And the stress of dealing with a bureaucracy was so unpleasant, and the enormous burden of memo writing for what was at that time probably the largest collaboration there was—certainly equal to—made me want to get out of high-energy physics.

COHEN: And that was it?

PINE: Yes, but it took me six years to get out, because I couldn't leave the project and it took six years to finish it. And during those six years, I looked around for what else to do.

COHEN: You were never really happy?

PINE: Well, I liked doing high-energy physics with my bare hands, which I had done at Stanford. And then with my friends here at Caltech—we did pretty big experiments, but we did them among ourselves. And we worked night and day together, with our bare hands—many, many times forty-eight hours straight—and Ricardo especially. He was a superb co-worker for fifteen years. But this huge collaboration was a bureaucratization that I really didn't enjoy. I designed the goddamn thing, I wrote the grant, but then after that it was a management project.

COHEN: I think Bob Walker got out of it then, too, didn't he?

PINE: Well, Bob and Alvin [Tollestrup] collaborated on a smaller project. He did do an experiment at Fermilab, and the younger guys in our group—Frank Sciulli [asst. professor of physics, 1969; professor 1977-1981] and Barry Barish [Linde Professor of Physics, emeritus]—collaborated on another large project. Caltech had three of the first seven projects at Fermilab. We were very good, in all modesty.

So, as a result of this bureaucratic life, which I didn't like, I started looking around. And you already know that I was not a very good student. So one of the options would be to go into the kind of physics which is very interesting now—which was even interesting then—which is what's done by people like [H.] Jeff Kimble [Valentine Professor and professor of physics].

COHEN: Tabletop kind of stuff.

PINE: Yes. Those people are deeply involved at the interaction of theory and experiment, and they know a hell of a lot of physics. You couldn't compete in that world if you didn't know a huge amount of physics as well as knowing how to build things and do things. And that world did not function on a partnership basis. And I really wasn't in a position to go into a group, like in high-energy physics—I needed to basically not do that sort of physics, because I wasn't going to do it myself. So I was looking at other things to do. I liked to work with my hands, I wanted to do science, and the kind of science I would do in physics did not seem accessible. That doesn't mean I couldn't have kidded myself into trying, but I didn't have to. Because I immediately realized, as soon as I started looking around, that neurobiology was a lot more

interesting. So, during a period of two or three years, when the big project was finally getting done at Fermilab, I was auditing courses here. And ultimately, with the help of some biology faculty I became friendly with, I took another sabbatical.

COHEN: This one was to medical school?

PINE: Yes, this was in 1978, to Washington University Medical School, [in St. Louis]. That was my conversion to neuroscience—two summer institutes and one year.

COHEN: So you had to learn all that biology.

PINE: I'd never learned any before—never even took it in high school. It was terrible—you had to memorize the names of everything. I certainly wasn't going to memorize all those names, so I never studied it, never took it in college. I studied it here. I audited three courses. I took Lee [Leroy E.] Hood's [Bowles Professor of Biology 1977-1992] immunology course—and in some sense I won his heart, because all the courses I audited I did all the work for. I audited his course and got a C, based on not knowing what an organic chemical was. So he was very pleased—or impressed, I should say. Then I got help from Jack [John Douglas] Pettigrew, who was an Australian neurobiologist who was too iconoclastic to survive here, in this rather conservative environment.

COHEN: Now, what do you mean by that?

PINE: This is a hard place to be an iconoclast.

COHEN: You mean, like Clair Patterson [professor of geochemistry, d. 1995]?

PINE: I would imagine, although I didn't know Clair Patterson. Like Ricardo. I mean, being an iconoclast isn't a trivial thing; there's a whole person involved. So you can be like Ricardo or like Clair Patterson, who I'm sure were grossly different personalities, and still be very iconoclastic. [Laughter]

COHEN: Well, not having high regard for your colleagues is one trait—

PINE: Well, some iconoclasts I know are, unfortunately, honest with their colleagues. Ricardo had a lot of problems with being honest; I don't know about Clair.

In any case, Jack Pettigrew left [1981] and went back to Australia. But he helped me find a place to be a postdoc, which I did for a year at Wash. U.

COHEN: I see. So you were actually a tenured professor here and you went to be a postdoc there.

PINE: Right. They got me a joint appointment as a visiting professor of neuro-physics. We invented that word. A month ago, I saw it on a big lab at UC San Diego, because some physicist who does neurobiology there invented it. I didn't copyright it.

So I went to Wash. U., and before I went there, my soon-to-be boss— **[Tape ends]**

Begin Tape 1, Side 2

PINE: So, iconoclasts. Yes. Jack left and he fixed me up at Wash. U. with a guy who turned out to be enormously influential but at that time was just a senior professor, head of a department [the Anatomy and Neurobiology Department], and younger than me. He sent me to summer school.

COHEN: What was his name?

PINE: Max [W. Maxwell] Cowan. Max left Wash. U. to become—he may have become provost for a while, but he became the scientific head of the Howard Hughes Medical Institute [1987-2000], which you probably don't realize—

COHEN: I know it funds a lot of people.

PINE: Two hundred million dollars a year! Two hundred Hughes professors, $1 million each. Max picks them and promotes them—well, he did. He got kicked upstairs; he's in some other role with Hughes now [Cowan died in 2002—ed.]. So Max said, "You're going to come to my lab." He's a risk taker, by the way, or he wouldn't have invited me.

COHEN: Well, but you already had a good track record.

PINE: But not in biology. He ran a neuroanatomy lab, totally and completely off the wall for me. And he said, "You better go to summer school at the Woods Hole Marine Biological Laboratory. Then you might learn enough to be useful during the year for yourself." Not for him. And so I did that. It was a terrific experience, a wonderful experience. Two wonderful summers I've had: one was this 1962 thing, where we invented K-6 hands-on science, and one was Woods Hole, in 1978. There were only twelve students, and altogether there were thirty or forty faculty, who worked us over in shifts—an intensive experience.

COHEN: Now did any of this have anything to do with the huge effort in the 1950s—you know, where they were upgrading science education and all that?

PINE: That was high school. That was the PSSC [Physical Sciences Study Committee]. Phil [Morrison] was a mover in that, and the people from MIT who later were the founders of hands-on elementary-school science. They invented that workshop. Not the rest of them, just the physicists. Not the chemists, not the biologists; just, in fact, [Jerrold] Zacharias, who was a powerful person. You may have heard of him; I never knew him. When the K-6 program was going on that summer [1962], neither he nor Fran [Francis L.] Friedman was there—Friedman was dying that summer. They were cofounders of both programs, with Phil.

So neurobiology turned out to be pretty interesting, because I went off for that year and naturally decided to do something that was like engineering. I was a postdoc in a huge, essentially nationally known neuroanatomy lab. After summer school at Woods Hole, I came to Max, the boss, and I said, "Max, I had a great idea in summer school. I want to grow nerve cells in culture and study neural networks using an invention that I have conceived of—which is that we should make culture dishes that have electrical connections to the neurons in them." And Max said, "Look, that's not something you can do in a year. And if you stay in my lab, you can work with some of my really good postdocs and get out three or four papers, establish yourself." And I said, "Look, I'm very established. And this idea of mine is more like something that only I could do. Anybody can do neuroanatomy."

COHEN: So that vocabulary was already in place—neural networks and all that?

PINE: Not in the modern buzz-word sense, but in summer school I learned all about neurobiology, eighteen hours a day for ten weeks, two days off. No family allowed. My family cheated; they lived about eight miles out of town—but you're not supposed to do that.

COHEN: Who taught at this summer school?

PINE: Great people, who are *the* best neurobiologists in the country, came in teams. Each team was led by a lab manager and had a specialty. The team from Harvard, which is the birthplace of American neuroscience, taught the introductory two weeks. And then the team from Johns Hopkins taught for two weeks. And then another team taught. Each team came with a leader and a lab full of people. There were only twelve of us and we were in the lab all day long.

COHEN: How did they select the twelve?

PINE: Politics. And applications. Max got me in. But they liked people like me. I mean, Max got me in, but this course was invented for people who wanted to become neurobiologists in ten weeks. There were two old people in the course—I was one. But we had new PhDs in the course who were changing from biochemistry. So you didn't have to have a super track record. Somebody had to say you were potentially very good, or you had a record, and then somebody decided that you could be one of the twelve. I was the oldest, and the youngest guy was a wonderful guy who became a very good friend—who was at that time a Harvard biochemist studying cat piss.

COHEN: What was his name?

PINE: Jose Garcia—and I can't tell you the hyphenated rest of it. [Dr. Jose Garcia-Arraras—ed.] He was known as "Little Jose"—there were two Joses. Anyway, that was great. That was a huge experience that summer.

[Tape ends]

JEROME PINE

SESSION 2

October 23, 2001

Begin Tape 2 Side 1

COHEN: Where did you develop an interest in neurobiology? How did that come about?

PINE: Well, as I said, I began to work in high-energy physics in larger and larger groups. I was involved successfully in a proposal to the new Fermilab accelerator with two Caltech collaborators from our group, and about twenty other PhDs—five institutions in all. We started that project in the early 1970s. It was clear that as the principal investigator, my main role was becoming one of sending out memos and adjudicating conversations among the five groups, and not doing any real work. I was working in the science as an organizer. I was always, of course, on the other side of the world before this time. I worked with our group here, and we were very successful and made a reputation for ourselves doing experiments by ourselves. Beginning about the time I came here, in '63, the Caltech accelerator was no longer at the cutting edge, and a group was formed using my new arrival—and a couple of people who were still here—to propose to do experiments at the larger accelerators across the country. So the Caltech user group was brought into being, and in that user group we had four faculty members and resources from the Department of Energy to hire postdocs, and we did experiments at various places.

The first one I worked on was at SLAC, right after the machine was finished. I was involved in the collaboration there with one other person from Caltech, two from SLAC, and two from MIT. We did one of the very first experiments at SLAC, and I was commuting from here every week—for three or four days a week out of the seven—and teaching full time here. But I was a bachelor. And then I ultimately met Nancy up north and stopped being a bachelor. But I kept commuting to other experiments.

After that first experiment at SLAC, some of the group members proposed a follow-on experiment there. And the two of us from Caltech—Barry Barish, at that time a postdoc, was the other one—were very distracted by the possibility of going to Fermilab, which was just going to turn on and which was a huge opportunity. So we told our collaborators that we were very

pleased but we were not going to join them. We wanted to go to Fermilab to work on large group experiments, and at SLAC we were still a small group. And in fact, while there were two people from MIT and two from SLAC, they were all old colleagues of mine. So there was no personal issue; it was just a question of what looked more interesting. And we decided that Fermilab was very glamorous and that we had some ideas to do things that would be in some way our own ideas and less obvious than that first experiment at SLAC, which was a follow-through on existing, ongoing things from before SLAC—sort of an extension. Our friends proposed to make another experiment at SLAC that was somewhat related, and we said, "No, we're going to go to Fermilab." That was very good, because then they could win the Nobel Prize. [Laughter] They can't give a Nobel Prize to more than about three people, so our bowing out made it good for them. This was Dick [Richard E.] Taylor and Henry Kendall and a very nice guy from MIT [Jerome I. Friedman] who worked with him. So the three of them shared the prize [1990]. Barry and I went off and each did huge experiments at Fermilab. Actually, we had previously done other experiments, which I won't bore you with, between SLAC and Fermilab. It was a very active group.

At Fermilab, I became a bureaucrat. And also, part of the bureaucracy was even worse than the bureaucracy of coordinating twenty-five PhDs, because the power structure at Fermilab was extremely arrogant, dictatorial, and difficult to deal with.

COHEN: Was [Robert R.] Wilson in charge then?

PINE: Yes, he was in charge [1967-1978]. He was not made for that job—he was a simple country boy, so he did not take well to being such a big boss. Whereas the boss at SLAC—Pief Panofsky, who started off in a very similar way—turned out to be a genius at being a boss. So you can't tell.

COHEN: Wilson was a good sculptor.

PINE: Wilson was a very difficult person. He was an OK sculptor. He, of course, could put his sculptures all over Fermilab, which was a very nice thing he had going for him. He helped me decide to get out of the business—I've never really written him a thank-you letter—by being so difficult. He basically put me, as the boss of this group, through a whole series of unpleasant

hoops. It was a very complex political activity in the early days at Fermilab, because resources were very scarce and everybody was lobbying for position. Anyway, that helped me decide. But actually, being in this huge thing also was not what I wanted to do when I was a boy. I decided that I had to move into something where I was part of a smaller activity—not necessarily alone, but like we used to do, a few people or myself and two students. I thought about what I could do, and I decided that it would be very hard to go into tabletop physics, which was beginning to be invented by the condensed-matter physicists, who at that time were called solid-state physicists. People have since discovered that condensed matter isn't all solid; it's kind of fun. However, I knew that the foundation for that kind of work would be a very, very strong background in theory, especially quantum mechanics, and I didn't have that, because I had gone into high-energy physics essentially as an engineer. Even when I was a PhD student in cosmic rays, I was building a huge and wonderful machine. So that was not a terribly attractive challenge. And I thought of other things. And the obvious other thing—I don't know why it was obvious—was neurobiology. I think I already knew enough about neurobiology to know that it had a lot of electric circuitry and physics in it.

COHEN: I see, you're coming from that direction.

PINE: Yes, well, that's where I was in high-energy physics. I was an electronics engineer. I was also a mechanical engineer. So I decided to explore neurobiology while still directing this huge activity at Fermilab—explore it at a low level, obviously. So when I wasn't on the plane or teaching, I went to typically one neuro course at a time here at Caltech, for three years. And that was good, because I liked it and also I learned something—which means I didn't start from square zero. And the faculty that I worked for in those courses knew me. One of them—Jack Pettigrew, whom I've mentioned—I used as my advisor. He told me to visit five places to talk about being a one-year postdoc. I visited three of them, I guess, and it was obvious that the best fit was at Wash. U. Max Cowan, in charge of the neuroscience program there, was a risk taker and wasn't really annoyed by the fact that I was older than he was. He already had a history of having people in neuroscience come from outside biology. He had an appreciation for the beginning period of neurobiology—this is way back in 1978—he had an appreciation back then of the need for neuroscience to involve itself in collaboration with physicists and engineers.

Because there was a big overlap in some areas, but not conceptually—only in technology. So I went there, and then I had that awesome experience at Woods Hole—ten weeks with two days off. Great people, superb faculty, the best in the United States—from all over. In five teams, covering all aspects of neuroscience. And during that course, I realized I got an idea.

So I had an idea when I went to Max. My idea was to combine some of what I knew about experimental physics with what I had learned in the neuroscience course about growing neurons in culture. Max's lab did nothing like that. Max was a superb thinker, and his lab did neuroanatomy. They tried to figure out how things were connected and, by figuring out how they were connected, how they worked—but only by using optical techniques of visualization. He was following in the path of the biggest genius in the history of neuroscience, Ramón y Cajal, who was a Spaniard, the neurobiological equivalent of Picasso. An unbelievable person! He invented modern neurobiology, just by looking. He had a fifty-year career, but he invented modern neurobiology in the first twenty years of it—in the late nineteenth century. And here it was eighty years later, and Max and his lab were using much fancier things but pursuing exactly the same paradigm.

COHEN: Was this appreciated at the time?

PINE: Oh, Max was extremely important nationally. There's a whole breadth of activity in neuroscience in which there is room for everyone. And neuroanatomy is one of the major issues, although it's less major than it was in 1978—much less, actually.

COHEN: Why is that?

PINE: Because now everybody uses much more definitive technologies to study the activity of the nervous system. Just looking at the connections is only part of the problem and doesn't give you the solution. You can't tell how a computer works by just taking an X-ray picture of it, and that's essentially what they were trying to do. It's very hard. Cajal was amazing; he was a genius, because he did things that way. And so was Max, and other people—and a lot of people here who are very good at it.

So I entered neuroscience at a period when, unbeknownst to me or anyone else, the compartmentalization into neuroanatomy and electrophysiology and pharmacology and

biochemistry was going to disappear. And I've been part of that. But I've never managed to plunge into a large multidisciplinary lab, because I was an electrophysiologist when I came here. So I have to go back a minute and tell you.

I went to Max and I said, "I got this idea in Woods Hole, and I don't want to do neuroanatomy." Max was the head of the neurobiology program at Wash. U., which was one of the first two or three in the country and combined activities in physiology, anatomy, and pharmacology. So he had a power structure under him that included more than just anatomy. And it was easy for him to organize me into a physiology lab. But he fought it. He said, "This is a big mistake. You should stay in my lab." I knew it was a superb lab, and I said, "Look, I'd be just overjoyed if you would let me be part of your lab and have an office here and get to know and listen to and talk with your postdocs. But I want to do electrophysiology. Can you fix me up?" And he did. So I spent that year there. I was appointed a visiting professor of neuro-physics, which I thought was a clever idea on Max's part.

COHEN: But you really were a student.

PINE: I was a postdoc. I went there partly because Max was so supportive. The people I talked to were all friendly, but the two leaders I talked to other than Max—at Wisconsin and Berkeley—either didn't have the power structure or the environment that Wash. U. had. I went to graduate courses there, which were mainly populated by MD-PhD students and MDs—huge world-class medical school, one of the three best in the country. And MD-PhDs were getting big then, so there were some MD-PhDs there. Max had the chutzpah to run a general seminar for all the neurobiologists on Saturday morning from nine to one, and those were fantastic, four-hour nonstop orgies, which don't exist anywhere except at Wash U. So I went there partly because I sensed it was the right place to learn. And then I followed my physiology idea in spite of Max. Someone here connected me with an electrical engineer at Wash. U., so I learned how to build micro-devices in the electrical engineering lab and I learned how to do electrophysiology at the med school—in parallel. Near the end of my year, it all came together, at the zero hour, in the last week. I can't say that I was extremely tense or nervous about it. I don't know why I wasn't—I should have been.

COHEN: Maybe you were numb.

PINE: Well, I wanted to find out whether my invention would work, and it did work. So Max gave me a very hard sell and said, "You have to write a paper immediately," and then sent me off to summer school again, at Cold Spring Harbor. In my career up to then, I'd never tried to do or study neurobiology of low-lying invertebrate animals—primitive animals, like snails, leeches. I knew enough to know that they are very different and their nervous systems operate on a completely different information-processing system—utterly different, more like what physicists build in laboratories. At that time even, it was pretty well known that there were a lot of similarities in biochemistry and in cellular neurobiology between high animals, like us and the monkeys, and low animals like *Aplysia* and snails. So it was a very interesting situation, and I wanted to find out what it was like, so I went to the course at Cold Spring Harbor, which was taught by John Nicholls, the world's leading expert on leeches. And so I learned about the nervous system of leeches and about working with animals that had groups of only 500 neurons that did everything, instead of 5 trillion.

COHEN: Isn't their DNA something like 95-percent the same as that of other animals?

PINE: Yes, but we didn't know that then—vaguely, but not really. We knew that the biochemistry is the same, and of course that's related to the fact that the DNA is the same. So there's a huge amount that's the same, but the structure is totally different—which is interesting, considering that the DNA is so similar. The structure is due to a very small percentage of the genome which changes everything. Interesting—never thought about that before.

After that, I realized I wanted to work in mammalian neurons. I had been taught how to grow mammalian neurons in culture at Woods Hole, during the previous summer, and my physiology experiments at Wash. U. had been connected to doing mammalian cell cultures on specialized micro-devices that I invented, and doing physiology on them. So I was very well prepared to do more of that. And I had looked at leeches and decided, No, I'm not going to do that.

COHEN: This was really a hectic fifteen months for you. You must have been overwhelmed.

PINE: No, I don't have any feeling, in retrospect, of being overwhelmed. It was all lab work. There was no paperwork—no paperwork at all for the whole time, eighteen hours a day for

fifteen months. Quite interesting! And no theory. I learned a lot, I got a lot of lecture input, but essentially nothing in the way of written work. Not a lot of lecture input, either—enough of it.

COHEN: So you played at your lab bench.

PINE: Yes, and later with a partner in Cold Spring Harbor, Ana Lia Obaid, who was a wonderful person.

COHEN: You started to tell me that everything came together.

PINE: Well, my device worked, and I recorded from these nerve cells. I was a success. I wrote a paper when I left.[2] I wrote it at Cold Spring Harbor while I was taking the leech course. It wasn't very long; it was just about what I had done. And there was no bibliography, because nobody had ever done it before. So it was kind of interesting; usually, you have to go through a huge ordeal of reading thousands of other papers so that your paper can be properly situated.

COHEN: That was really a seminal paper.

PINE: It was, yes—it turns out it was. However, at the time I published it, it was not quite as important as the work of Ed [Edward B.] Lewis [Morgan Professor of Biology, d. 2004], of course, who took thirty years to be recognized. But the history of my work is that it began to be adopted by other labs, with my help, five or six years after I left Wash. U., and it's now become an international fad. It's not obvious, by the way, what good [a culture dish with multiple recording electrodes] is. Very interesting, separate scientific issue. I thought it was the greatest thing since sliced bread when I invented it. And I came back here and I had a very complex political situation.

COHEN: Yes, what did you come back to?

PINE: I came back to an office in Lauritsen [Charles C. Lauritsen Laboratory of High-Energy Physics], and a division chairman. Robbie [Rochus E.] Vogt [Avery Distinguished Service

[2] J. Pine, "Recording Action Potentials from Cultured Neurons with Extracellular Microcircuit Electrodes." *J. Neurosci. Methods*, 2:19-31 (1980).

Professor and professor of physics, emeritus] was the division chairman when I came back, in the fall of 1979. And then the previous chairman—a wonderful astronomer, whom you know—Maarten Schmidt [Moseley Professor of Astronomy, emeritus], was very friendly and supportive.

COHEN: Well, he would have been attuned to one person doing something by himself.

PINE: Well, and Robbie was an old-fashioned cosmic-ray physicist. Robbie and I had known each other since we were beginners here. We both used to teach freshmen. So there was a perfectly good reason why both of them were nice, but there were plenty of reasons why they could cop out, and they didn't. And the net result is complex. I don't keep a diary, and I don't have a very good memory. But I spent quite a long time with them, dealing with what I was going to do next. I didn't make any proposal. I didn't even want to say anything, I don't think, until I found a neurobiology lab that would allow me to be part of it. So I went to see quite a few neurobiologists.

COHEN: Were there a lot of neurobiologists here at that time?

PINE: Typically around ten—that's the typical number that's been here all along.

COHEN: That's a lot for Caltech.

PINE: Yes, well, biology only really has neuroscience and molecular biology. It doesn't do any other conventional things. It does now have a superb developmental biologist—two; probably always had—so it did three kinds of neuroscience. But the developmental biology is neurobiology. I wanted to do physiology, so that limited whom I went to. I went and visited them all. It was very awkward, because they didn't quite know what to do with an aging physics postdoc. Nobody here was as nice as Max Cowan was; of course, Max was the only one with a one-year situation. But Henry Lester [Bren Professor of Biology] was very nice, and Henry just happened to have an accidental situation in his lab, which was not very big. He was only a non-tenured professor then—an associate professor without tenure. But he was very adventuresome and told me, "There's an office in the middle of my lab that belonged to Cornelis Wiersma"—a great man, a Dutchman. I don't know anything about him except by reputation. Very, very

smart invertebrate neurobiologist—probably the reason there's a lobster over the door into Kerckhoff [William G. Kerckhoff Marine Biological Laboratory]. There was invertebrate neurobiology going on here—[Felix] Strumwasser [professor of biology 1964-1983; d. 2007] was a big name, but Wiersma started it all. He died shortly before I arrived—so soon that his office was just sitting there. So Henry said, "If you really feel that you need to be connected to a lab, you can be connected to my lab—there's this spare office. And you have to go talk to Lee Hood [then Biology Division chairman]." Well, Lee was one of the people I had taken a course from—an immunology course—and as I told you last time, I got a C in the course, which really impressed him, because he knew I didn't know anything. To most people a C isn't very impressive, but he also knew that I had to work all night, one night a week at least, just to do the homework. So he was impressed, and he was my friend. And I guess I had met him in some other context, which had to do with education. His partner Bill [William B.] Wood, who went to Santa Cruz, was interested in education. And maybe even Lee was, because Lee ultimately became very interested after he left here. He was interested here, too.

So Lee welcomed me—he said, "Great!" And then I looked around for some help rehabbing the office into a lab. I set up a lab there at the same time I somehow got a grant from the System Development Foundation, whose board chair was Arnold Beckman. The SDF was the leftover funds from some boondoggle in computer science, which became a foundation. Either Maarten Schmidt or Robbie knew that Arnold could be approached, and I think I wrote a proposal; I must have written a proposal—and of course I can't possibly find it.

COHEN: But you did get the grant.

PINE: Yes.

COHEN: Then they probably have a copy.

PINE: I'd be delighted to get some history. And I'd also be delighted to hear how Lee Hood decided to let me stay in biology. Anyway, the net result is that I got money from the System Development Foundation, which is the spin-off of the money left over from some boondoggle called the System Development Corporation, which I know nothing about.

COHEN: And that was something that Arnold Beckman had a hand in?

PINE: He must have had a hand in that, to become chairman of the board of this foundation. And Arnold didn't know anything. He was a chemist. Arnold didn't know beans from bananas about neurobiology, so I'm not sure he could have read the proposal. So I don't know who, what, or why, but Arnold came through with the money.

COHEN: So you had money, you had an office.

PINE: And my office was converted into a lab.

COHEN: Now, were you still a member of the physics department?

PINE: Always.

COHEN: So you had to teach physics.

PINE: I was originally teaching freshman physics after Feynman, as I told you. So I was the successor, with that terrible challenge of following him. I had some very amusing phone conversations with him, by the way, late at night—because I had to give the Feynman lectures but I wasn't Feynman. There were things in that book that I did not understand at all, and occasionally I felt that I really should understand it if I was going to give a lecture. So I would call Dick. Most of the time when I called, he said, "You know, I didn't understand that, either." But I did call him several times. It was fun.

So I was doing that job. I was doing other things, too. I invented a whole new way of teaching juniors atomic physics somewhere along that path—because I have always been an educator, not a researcher. One of the things that's going to come out in a minute is that my lab has always been an educational activity, which is not typical. When I started with my little grant, I had a lab, which I equipped in this office, because I could in that space do what I had to do. I needed about half an office to contain the apparatus I needed. And some money. And I had a SURF [Summer Undergraduate Research Fellowship] student, Julie [Julia A.] Kornfield [professor of chemical engineering], who's now a chemical engineer. And I had my first

graduate student, who was a theoretical-physics student, who really was snowed and became my first helper in building the technology here that I had built at Wash. U. His name was John Gilbert, and he, too, had the misfortune of being the son of a Nobel Prize winner [Walter Gilbert, Nobel laureate in chemistry, 1980]. I think I told you about all the children of Nobel Prize winners I know, who've been in various ways—what's the right phrase? I don't want to use too strong a word—

COHEN: Emasculated?

PINE: They've been kind of intimidated in some ways. Anyway, John tried to do high-tech neurobiology with me, and after four and a half years he said, "Look, I just don't have it. I just can't handle this kind of uncertainty and sort of ill-defined science." And I said, "John, OK. I know you want to stay in neuroscience, at least for a while, and I have another project." I had made connections with people who wanted to do another pioneering experiment, which was to make X-ray microscope pictures of live neurons, which no one had ever done—dead neurons are easier. John did that for a PhD thesis.[3] So I already had people coming into my lab from outside of biology. While John was there and I was building things with him, other people came knocking on the door, and I had a sequence of people from engineering and physics. Some of them became my graduate students, some of them I sent to other labs, and some of them I helped find postdoc appointments or graduate schools.

COHEN: Now, these were people who wanted to do this kind of experimentation?

PINE: They all wanted to do neuroscience, never mind what kind of experimentation. All they knew was that Jerry Pine did neurobiology. They were physicists and engineers, and a lot of them knew me, I guess, because I had taught them freshman physics. I also taught a neurobiology course to graduate students and postdocs and a neurobiology course to upper division students on high-tech neuroscience—my kind of technology. Those courses have osmosed now into a new course, which I invented five years ago. But those courses created a

[3] John Gilbert (1992) "Soft X-ray Imaging of Whole Wet Cells," Dissertation (Ph.D.), California Institute of Technology.

certain amount of knowledge about neuroscience and me. So I became the resident guidance counselor for physicists and engineers.

COHEN: Were you really unique in coming from physics? I know that Seymour Benzer [Boswell Professor of Neuroscience, d. 2007] came in from physics.

PINE: No, I wasn't. It turns out that ever since neurobiology was invented—in the fifties—much of the important work has been done by physicists. Seymour and a couple of other physicists became molecular biologists, but other physicists and engineers became electrophysiologists—in England, not here. One or two here—very few! You can count them on the fingers of one hand over twenty years, so it was not a big stream of people. It is true, though, that quite a few of them are here. But all the time that I've been doing the thing in my lab, people have become more and more interested in moving, so now being the guidance counselor to physicists and engineers is a serious occupation, and I can point out my progeny to you. My progeny is very impressive—I love them! They've all been successful in neuroscience, except John, my first student, who, after he did the X-ray microscopy, became a computer programmer and entrepreneur, and he now runs a small company that sells us software products for building micro-devices. Of course, he learned from me how to build micro-devices.

COHEN: So he's made a good living.

PINE: Oh, he makes a living. I'm not in close touch with him, so I don't know how rich he is. [Laughter] One person I helped to become a postdoc used my technology as a postdoc, and I helped him write the grant to get the fellowship, and he was very successful. He's a full professor at Harvard in biology—Markus Meister. I helped him to a PhD here [1987]. He was a physicist who wanted to get into biology, so he's one of the ones I advised. I advised about everyone in general. There's a lot of people in biology who need physicists—get the annual report and you'll find them all, and go knock on their doors and interview them. And Markus talked to Howard C. Berg, the great man who studied bacterial swimming who left here. He was our only bacteria person here; he's now back at Harvard as a full professor, of course. He went back to Harvard—they always go back if they're offered the chance. So Markus went on a postdoc and ended up at Harvard and became a full professor. While Markus was doing

bacteriology with Howard, it was biophysics, and he was a very strong physicist, but bacteria are boring. He was in the Beckman Biology Lab [Mabel and Arnold Beckman Laboratories of Behavioral Biology], and all around him he saw neurobiology, and he said, "This is much more interesting." So he took my neuro course, and he came to me and asked, "Where can I be a postdoc?" and I helped him decide. He had to write a fellowship proposal, which I helped him do—because he had an idea about how to use my multi-electrode array technology. So it was very easy for me to work with him. And then he and I together, while he was a postdoc at Stanford, did a pioneer experiment using my technology, which ultimately made him a bigshot professor and made him rich and famous.

And Wade Regehr was one of my students. He was a really nice guy—quite different, utterly different from Markus. Wade was just the opposite. Markus came out of this high-pressure, up-tight German academic world, and Wade was a simple country boy from Alberta, who was a physics major at a small college. He came here in applied physics and immediately discovered that it wasn't what he'd hoped it would be. But the professor he was working for, Dave [David B.] Rutledge [Tomiyasu Professor of Electrical Engineering], was interested in working in micro-devices, and I convinced Dave to be my collaborator in making micro-devices. John Gilbert, the student whose father won the Nobel Prize, had learned already how to make devices over in Dave's lab, because we couldn't make them in my little office; we had to have a real place to do microfabrication. Anyway, Wade came, and Dave put him to work on a project of mine. And during that summer Wade decided he didn't want to leave, that this was really fun. But the spring before that, he had decided he was leaving, and he booked his sabbatical year in the South Pacific. He's a neat guy—very low profile. He came back after his sabbatical year in the Cook Islands and went to work in Dave's lab as an applied physics graduate, working for me. So he learned how to do neurobiology and build gadgets from the two of us, and he did a couple of postdocs, and now he's a full professor in the Harvard Medical School.

Chi-Bin Chien was a student who my wife said was severely gifted. He graduated from Johns Hopkins when he was fifteen, with a major in physics and a major in biology. And his academic parents—bless them—decided that they should slow him down. He obviously had not been force-fed; he just was off-scale smart. They gave him a year off in England, doing something at Cambridge, and he came back here as a physics graduate student at sixteen. And after a year of physics graduate school, he knocked on my door and said, "I hear you do

neurobiology, and it's very interesting to me. I'm a theoretical-physics graduate student, but I know something about biology." He'd had a full major in biology. So I said, "OK, try it during the summer and see how it plays out." He had a good summer, and he became my graduate student and teaching assistant in physics to support himself until we got him a fellowship. And he's now a professor at the University of Utah in developmental neurobiology [Chien died in 2011—ed.]. And that's probably the best department of developmental neurobiology in the country.

Meanwhile, there are a lot of undergraduates I can name—I don't remember all of them—whom I read about now in the journals, because they've all become PhDs in neurobiology. They came to my lab; I put them to work wiring circuits—not doing biology at all, but learning biology. They took my courses, and they all went to graduate school. Neurobiology graduate schools now, in the last decade, *love* physicists and chemists and computer scientists, because it's a completely interdisciplinary business—much more so than the rest of biology.

COHEN: Well, that's what this building [Beckman Institute] is supposed to be, I think.

PINE: Of a sort. The *real* building is Beckman Biology. In those labs, there are physicists and engineers and chemists all working in the same labs. That isn't true in this building—maybe true to some extent in Scott's lab [Scott E. Fraser, Rosen Professor of Biology, 1990-2012].

So that's the story. I've been an educator and I've turned out a *huge* number of neurobiologists. The first ones are all professors and the next generation are all young professors. **[Tape ends]**

Begin Tape 2, Side 2

PINE: There probably are physicists in every lab in neurobiology, or almost every lab. They didn't all come out of my guidance clinic, but a lot of them did—probably half of them. And some of them, actually, have come from my course. Some of them are CNS [Computation and Neural Systems, a multidisciplinary program—ed.] majors, which is this new invention, which I was once part of. CNS attracts physicists and engineers, and they inherently connect to neurobiology labs, because there's nothing else for them to connect to. So that's another source.

COHEN: Are they interested in the biology part?

PINE: They vary. I have a CNS student doing a rotation for me now in wet biology. And I had two last year: One went into wet biology and one went into computer simulations.

COHEN: What do you mean by wet biology?

PINE: I mean real neurons. One went to work in Laurent's lab [Gilles Laurent, now (since 2009) director of the Max-Planck-Institute of Brain Research, Frankfurt, Ger.], studying crickets—or something like crickets, if not crickets. And one is with Christof Koch [Troendle Professor of Cognitive & Behavioral Biology and professor of computation and neural systems, 2000-2013], doing simulations. There've been a series. Some CNS students stay in wet biology. Jim [James M.] Bower [professor of biology 1985-2002] had a couple who stayed. I think I've had two biology students—one superb biology student, who was the best in her class, and one good one. Both were very good. So I haven't just had engineers as graduate students. And I generally interview a lot of the incoming biology students, because there are more and more of them interested in this craziness. Some of the incoming ones don't have much engineering or physics and they're nervous, and some of them are from here. Right now, there are two or three of them who want to come into my lab in the next year or two.

COHEN: But they have to know physics and engineering to do this.

PINE: Some. My very good person learned it in my lab—she was super smart. Hannah Dvorak. So it's not a necessity. You can learn biology from the other side easier—much easier. And I have a postdoc now in my lab who's an electrical engineer, who's originally from Spain but got his PhD in England.

COHEN: So who's funded you over these years?

PINE: Oh, a lot of people.

COHEN: You used up that little bit of money [from the System Development Foundation].

PINE: That's right; that kept me going for three years. And then the NSF kept me going for three or four years. And then the NIH [National Institutes of Health] kept me going for six more years.

COHEN: Now, you must have moved into bigger quarters.

PINE: Yes, by accident and good luck. After the System Development Foundation period, or at the end of it, Felix Strumwasser left [1983]. And there was a lab there that was a real lab—not quite this big. Whoever was chairman then [Lee Hood] said I could move in. And Robbie Vogt, who was head of the physics division at that time, contributed some money to rehabbing.

COHEN: Is there anybody else who rides two horses, like you do?

PINE: No. It's everybody's constitutional right to do this, but nobody's ever done it—at this place.

COHEN: Because they're always saying that that's the strength of Caltech—you know, you walk around and you talk to everybody.

PINE: Well, that's true. But actually moving from physics and doing full-time neurobiology—or doing full-time anything other than where you started—I don't know if it's happened anywhere to anyone, but it's your right to do it, supposedly. The chairmen involved were terrific, so in that sense Caltech gets all the credit. So for various reasons, I got to have enough room to do what I just told you. It's not large; it's a small lab. It's very crowded now—326 Kerckhoff.

I have another protégé—my most recent protégé, Steve [Steven M.] Potter. He was a postdoc shared between me and Scott Fraser, because he discovered me and I couldn't pay for him. And I said, "I think I can figure out a way to pay half of you, and you're the kind of guy that Scott would love." Scott and I actually understand each other. So I sent him to Scott, and I was right. So then he had a joint appointment, and he worked in Scott's lab doing one project and in my lab doing other projects for quite a long time, on the order of four years. He had an idea, which was to use a combination of every technology he'd ever worked on here in my lab and Scott's lab, all together, to study an artificial brain in a culture dish. Now this is OK in the

New York Times and the *LA Times*, but you can't sell this to the NIH like you can sell it to the newspapers. There's a very nice guy, Ted [Theodore W.] Berger, at USC, who sells it to the newspapers all the time—and he has money. He probably sells it to the Defense Department, because they'll buy anything.

So Steve invented this idea. And he told me and Scott that there was an NIH request for proposals which this idea would fit.

COHEN: Now, this is to create a brain in a dish?

PINE: Brain in a dish. I won't tell you all the details, but that's the newspaper version of what he's doing. That's what you can call it, if you want to stretch it a little bit. He's been in the magazines; he's been on television. So he came to me and Scott, and we said, "Well, if you want to write a proposal, because they're asking for this, and you want us to be your support, we will." Because Scott and I both felt that he was terrific. He's an amazing character.

Anyway, as a senior postdoc he wrote an NIH proposal, with me and Scott on it only as co-principal investigators, which doesn't even appear on the cover page. A postdoc as PI never happens, essentially. And it was for a lot of money. It turns out that the panel that reviewed his proposal was specially convened, because the request for proposals was for some things that were off the wall. So he won! His was by far the best one, from what I've heard. He got $1.4 million for four years. Let me tell you, it goes very fast when you pay people. [Laughter] He has two postdocs and that's it. That's what you can buy for $1.4 million.

COHEN: And what is his position? He's a senior postdoc?

PINE: He's not even a postdoc now, because Caltech fired him. [Laughter] Caltech fired him because the paperwork wasn't done by Scott in the right time frame to have a special appeal made for him to continue as a postdoc past the statutory number of years.

COHEN: So what's happening?

PINE: His salary is paid by his grant. He's a member of Scott's lab, because he's in the Biology Division; he's a member of Scott's lab as a senior technical assistant. And he has a tenure-track

job with Georgia Tech. He's going to have a *big* lab at Georgia Tech in a new effort in neuro-engineering. So he's done very well. He's my latest protégé.

COHEN: You need people who, as they say, are outside the box.

PINE: Well, that's who I get. I don't get guys who are not outside the box. That's why I'm lucky. People say, "How do you get all these great people?" Well, the answer is, they self-select themselves into my lab. I trust my judgment, and I do judge people very well. I don't think I've made any big mistakes. But, you know, I haven't had many chances, because I haven't had many duds come to me. [Laughter] There's a guy in the lab now, Daniel Wagenaar [research professor and director of the Neurotechnology Institute: Division of Biology and Biological Engineering 2016-present—ed.], a physics graduate student, who's the next famous protégé of mine. Wagenaar is a Dutchman, and he found me on the Web before he came here. Daniel corresponded by e-mail and came to visit me as soon as he got here. He was very well educated in Holland, so the physics course work and such didn't slow him down a great deal, which it usually does. He could actually come into the lab, part-time, early in his first year. At that time, Steve was just turning on, so I looked around the lab and decided the best place for Daniel was working with Steve's group, which had two postdocs and Steve and a *huge* computer activity which is connected to this project—whereas my lab only had one postdoc and me and a very small activity. So I suggested that Daniel join them, and he's been awesome. And he's not going to go to Georgia Tech—he could, of course, but he's not. Steve is going to be a professor at Georgia Tech and Daniel could go as a graduate student, but he's not; he's going to stay here and be a graduate student in my lab. So we're beginning to plan to integrate him and his own project into my world. So, once again, I have another very off-scale, good young physics person.

COHEN: You have this really full-time operation running very well.

PINE: Except that I'm running CAPSI [Caltech Precollege Science Initiative]. [Laughter] And teaching! Actually, I'm teaching a new course—well, I'm teaching a special version of a course.

COHEN: But you obviously choose people who don't need a lot of guidance.

PINE: No. It's the way I teach them. I think it's my style. First of all, I get self-selected people—that's the first thing. And then I do not micromanage. I'll tell you about another great success, actually, because it's a great frustration—he was the second or third strongest student I ever had. He was a postdoc—he had a physics PhD at Cornell. Mike [Michael P.] Maher. He came to my lab to learn biology. He was *fantastic*! And he's decided to go into industry; otherwise he'd become a full professor in biology.

COHEN: And what is your operation's overriding goal?

PINE: My overriding goal has always been the same: I want to study how small groups of neurons connect and behave.

COHEN: That's a big umbrella.

PINE: Twenty years. We've built a lot of gadgets to do that, and we haven't succeeded yet.

COHEN: So a student doesn't come to you and you say, "You can fit into this box doing this piece of the project?"

PINE: Well, I can say that we're trying to make this kind of machinery work to understand how small groups of neurons work. And in the case of Mike Maher, the guy who should have been a professor but isn't, he went to work on both the neurobiology and the technology. They all have basically done something under this one umbrella. I haven't said, "I have a lab that can do anything—just think what you want to do." No one can say that, except maybe Jim Bower. He isn't here, but if he were still here, he could say that. His was the only lab here that did essentially almost every kind of neuroscience. All along, since I came to Wash. U. with this idea—this is the idea I came to Wash. U. with, in 1978.

COHEN: You've had many successes along the way.

PINE: Many successes in education, many successes in other labs using my technology. Many successes in everybody deciding to emulate it. Lots of invited lectures about how I do it and why. But no papers—to a first approximation.

COHEN: I see. And you still love your teaching?

PINE: Yes, yes. Now I like it a lot.

COHEN: I don't want to embarrass you regarding your age. But many people your age are retired already, because—not that they aren't still working, but they don't want to teach anymore.

PINE: Well, that's not *my* problem. I actually teach freshmen with great enjoyment. Well, I teach graduate students. I teach two courses—one to freshmen and one to everybody. My neuro course, which I teach in the spring, is open fundamentally to physics seniors and grad students—they have priority. But it gets filled up by a motley crew, because the physics seniors and the graduate students don't fill it. It's limited to fifteen, and there are no lectures. But the fifteen are a great group.

COHEN: And they do individual pieces of work and present it? Is that how you teach?

PINE: They have to basically talk about the readings every week, and then they present reports on papers I give them. And then they do a term paper. Not the Caltech way.

COHEN: So that's not a huge burden.

PINE: Not after you've done it twice. It's a huge burden the first time, I'll tell you that, and even the second time.

COHEN: It takes three times to get it right.

PINE: Yes. And I'm still fairly conscientious about reading the reading assignment and so on. So it turns out that it's under control but it's still work; it's three hours a week.

COHEN: Who's funding you now?

PINE: Almost no one, but I still have a small grant from Tanner Research, which is in town, which is a sub-award under an NIH grant, which I helped write. It's a kind of grant that's given away to industry on the basis of a partnership with academia, and this small grant is enough to pay for a postdoc. The grant is for $170,000 for two years. A short time before that, I funded my lab without a postdoc. Because I can fund my lab on $10,000, or $15,000, a year and not even feel pressed—either at home or at work. [Laughter]

COHEN: But that won't pay for a postdoc.

PINE: No, but it will pay for a really good graduate student, like Daniel. Daniel, by the way, won a very good fellowship, so he's in great shape. It turns out, having read *Science* over the last few years, I've realized…. There was a long article in *Science* a year ago by people who liberated themselves from the grant structure by financing their own work. That's becoming fashionable.

[Tape ends]

JEROME PINE

SESSION 3

October 30, 2001

Begin Tape 3, Side 1

COHEN: I see you spent a year in Kings College [London] in 1987-1988. Maybe you have some remarks regarding that.

PINE: I actually spent that year at two places. I spent the year in London, half-time in a neurobiology lab with a good fellow named Dennis Bray and half-time at what was then, may not have even yet been, Kings College. It was in Chelsea. It might have been Kings College-Chelsea, or it might have been something else. But it was an education school—a high-brow education school. Kings College has more than one campus in London. It has a main campus, where I was not. This used to be—I think it's Chelsea, but I'm not sure—the home of the education researchers and education workers for Kings College. And I worked there half-time.

COHEN: How did you know about them?

PINE: I knew about them because I was interested at that time in a research project—well, I guess it was a research project—in science assessment for kids, and there was a national effort in England, which was led by Kings College-Chelsea, and I knew about it. I had at that time just received, with a friend of mine, an NSF grant to study science assessment in elementary-school children. So I went there and learned what the English had been doing.

COHEN: This was before you did any effort here?

PINE: That's a good point. In 1987 we had just started—we were just sort of messing around. So the genesis of this project was not having worked here; the genesis of this project was being on an advisory board for a project at RAND, which was run by a professor of statistics, fundamentally. And on that advisory board I was the only research scientist. In the eighties, I was on several boards as the token scientist.

COHEN: And the rest were more education people?

PINE: Of various kinds, mostly researchers—not many practitioners, mostly researchers. The head of this project [at RAND] was a man named Rich Shavelson. At that time, he was a professor at UCLA and he worked at RAND part-time, running this grant and other things for RAND. He ultimately became a professor and dean at UC Santa Barbara and a professor and dean at Stanford—which he just retired from.

COHEN: In education?

PINE: In education. We've been working, more or less, in various ways together since then. And on this committee, which was advising his project, we were concerned about assessments. I suggested this NSF project to Rich Shavelson that was an attempt to explore the use of unconventional performance assessments rather than paper-and-pencil tests. We got the grant. And about the time that we got the grant, I took a sabbatical in England and I prepared by studying what the British did—in what was called in Britain at that time the APU, the Assessment of Performance Unit. Then I came back and worked with Rich and a graduate student for four or five years on that project. And that was in some way in parallel and only tangentially related to the connection to the Pasadena schools.

COHEN: So the Pasadena schools had not come to you in any way?

PINE: No, we came to them—Jim Bower went to them.

Well, the other half of that year, 1987-1988, I was in a very good neurobiology lab at the Medical Research Council Laboratory, which is in the theater district in downtown London. And I learned some neurobiology.

COHEN: It sounds like a very interesting year.

PINE: It was a good year. My wife had a similar situation. She had two places that she went to. She studied elementary education at Froebel College [of the Roehampton Institute], which is a

very famous place. Friedrich Froebel invented [the kindergarten system], almost 200 years ago. And at some nice London teacher's college whose name I don't remember.

COHEN: So she was in London also, with you.

PINE: Yes, and our kid was in school. It was a good year.

COHEN: So that was another sabbatical.

PINE: Absolutely! I try to take them every seven years. I didn't the last time, but that's life. I didn't ask for it—been too busy. Anyway, yes, we took several—three.

COHEN: So then you came back here.

PINE: Right, and I went to work with Rich and his graduate student on this education research project. And interestingly enough—I guess just before I left, in '84-'85—my friend Jim Bower, who's a neurobiologist, had a kid go into the first grade and became interested in public education and realized that there was no science education. So Jim came to me and said, "We have to solve this terrible problem—there's no elementary-school science education." I had been involved, as I mentioned, in 1962-'63, inventing elementary-school science education in some sense—inventing what we now call inquiry-based science. I'd also been involved after that in a couple of instances, but not in an ongoing way. About the time that I invented this research project in assessment, I was also on a committee—this was a National Academy of Sciences committee—which was involved in a very similar project. These committees were both funded to compete with each other. This was regarding elementary-school science. I met on that committee a lot of people who were highly involved in nuts-and-bolts education—mostly academics, but mostly connected to real schools. So I learned about the state of the world in science-ed reform twenty years after I'd been involved—which was interesting, because I hadn't really been in touch for twenty years. I was doing neurobiology and various other things, and doing a little bit of outreach at Caltech. We had various programs, which I haven't mentioned, having to do with helping local kids.

So I got plugged into the state of education—and I had been there at the beginning—and when Jim said that we had to save the world and invent elementary-school education, I said, "It's been invented." I told him I had been involved in the old days and in recent days. And he said, "OK, great, then we're going to do it together." And I said, "No, you can be the one who does it this time." And he said, "No, we have to do it together." And I said, "OK, I will tell you everything I know. And I'll also tell you about the state of the United States world." Because being on this committee—and also having been involved in something new that happened in 1985, which was the founding of the National Science Resources Center in Washington to support reform—I had that option of telling him what was going on.

So Jim went to the school superintendent of Pasadena, a very boring guy who's mostly known to me because his wife owned a quilt store in The Colonnade [a shopping complex on Lake Avenue in Pasadena—ed.]. That was a more interesting part of him than he was. He was a very introverted superintendent—a peculiar oxymoron, almost—somebody who couldn't talk to his staff. A nice man, a sweet man. And Jim went to him and said, "We want to save the world of education in science-ed, and we think we know what we should do, but we have to learn how. And so we'd like to work in one school in Pasadena, at no charge to you." And he said, "Well, I have a science coordinator. Why don't you go talk to him?" So, without making a long story about it, we ended up in one school—Field, which was a fairly new school in the upper-income east side, just below Sierra Madre. We didn't go to Field because of its socioeconomic level; we went there because it had a science room. In the district at that time, it was the only school where there was a teacher who was devoted to teaching science in elementary school. A school-improvement program had funded her. It was the only such school, so it was a natural.

Well, we had to figure out what to do. And in order to do that, being scientists, we had to find out what everybody else had already done—which never happens in education—to a first approximation. It's always invented from scratch. We needed some funding, and we went to Murph [Marvin L.] Goldberger [Caltech president 1978-1987; d. 2014]. We told Murph that we wanted to save the world and we needed money to travel, to look at other school districts. And we needed some money to buy materials to put science into this trial school. Murph gave us $25,000 out of his slush fund, whatever he had—he had some available, non-constrained funds. So with that, we started the project by doing some traveling to look at other places, and we took

with us the teacher from this school, who was the science teacher—who was a hotshot educator but not a science person.

COHEN: What was her name?

PINE: Her name is Jennifer Yuré. At that time [1986], she was running this little science room because the school asked her to. She liked science, sort of emotionally. She was an elementary teacher of the highest quality. She had taught some science to her kids, and then she became a language arts expert, consulting teacher, and then they made her the science lady. She had never had any formal education in science. So this was a hotshot teacher who learned fast. We traveled to several places. Jim and I traveled to many places; Jim and I and Jennifer went to the nearest of the school districts that had an established program to visit. We spent four days there.

COHEN: Where was this?

PINE: In Mesa, Arizona—a famous place; a suburb of Phoenix. At that time in the United States, there were only five districts—including Anchorage—that had districtwide inquiry-science programs, even though in the sixties the NSF had put millions into it.

COHEN: Into elementary schools?

PINE: Into elementary schools—three multimillion-dollar curriculum projects in some sense complementary, but providing three slightly different ways for kids to do science. And it didn't work. The strategy that worked was invented by these school district people at the five places. We went to Mesa and became, basically, their protégés. We brought from Mesa materials and a knowledge base that we got by listening to them and watching them for four days. Then we recruited the Field School into that program, and we did that by importing Bill Smith, a Mesa resource teacher—a Mesa teacher who coached teachers and taught them how to teach science.

COHEN: Now do these teachers in Mesa have a science background?

PINE: No. The rule is that nobody does, in elementary school. The people who do well in elementary are expert elementary teachers, not secondary teachers. So the rule is no, no science. Bill Smith came and ran an after-school workshop at the Field School. I had asked the boss in Mesa—the boss in Mesa is named Susan Sprague—I asked Susan if we could have a resource teacher come to Pasadena and do a workshop at the Field School, which he did. It was an afternoon workshop for about two hours after school. The teachers all said that it was the greatest thing and they wanted to do it. So then we got from Susan some materials, and Jennifer volunteered to help make it happen. And Jim volunteered to help the teachers learn how to teach science. He invented a system of teacher professional development, in which teachers went through, with a scientist, the materials that they were going to teach. The scientist didn't tell them about the materials; the scientist worked with them and went through the lessons with them and basically provided a model of how to do scientific inquiry. No answers, just fun. Jim had a good-size lab, so there were enough people in his lab that he volunteered to do the training at the Field School.

We had a dichotomous activity at that time, which lasted throughout our fifteen-year partnership. There were a lot of things to do, and we each had ideas about things that needed to be done and an idea of how to do them. Jim's idea for training teachers he did, and what I did was to become in some way a national expert on what was going on and explore the potential for writing an NSF grant so that we could expand this program after it was a success. I also entered into a complex negotiation with Apple computer, because they had a laboratory school in West LA which they thought was going to save the world by putting Macintoshes in every classroom and having the kids use Macs to learn from.

Jim and I had a view that the curriculum of the sixties—the curriculum I knew about and that had been refined for twenty years and that we got from Mesa—really ought to be modernized and that kids ought to do science the way we did it, which is that we used computers in a very, very substantial way to analyze our data and display it, and basically as a laboratory tool—a hands-on tool, not a simulator. I politicked a partnership with Apple, and the Field School got computers for every teacher so that we could pursue this goal of ours. Somehow another partnership was generated, with a school in Carson, which was the pet school of a TRW division. They were in a school there that they were trying to support in science. So the program in Pasadena matured and teachers taught four units a year, a very serious effort. They

got better at it after about two or three years. Well, after two years, they became known, and people came and looked at them.

COHEN: These were teachers who had been there already?

PINE: Yes. The philosophy of this educational program is that a classroom teacher must teach the science. Although there were people who invented the idea some time ago who thought that the same science materials could be taught by having one expert do it. But there are lots of things wrong with that, and we knew it. And the people who were good at it—the five school districts—all depended on the classroom teachers. For two or three years, the program—Project SEED, Science for Early Educational Development—grew and got better. And we had these partners, so we had enough money to hire somebody who was a super-teacher in Pasadena to go around and work with the two other schools, the Apple school in West LA and the TRW school in Carson. So we had a three-school project, and we had computers.

Well, this situation evolved in two ways. The first way it evolved is that the Pasadena superintendent changed and a new superintendent—a neat guy from Los Angeles whose name I've forgotten, partly because his name wasn't what his real name was; he's an African-American guy, a big black guy—and he came with an associate superintendent named Mike Klentschy. They had been sub-district supe and assistant supe in LA and they became Pasadena supe and deputy supe. The supe decided—after he had examined what was going on in Field—that he was willing to invest some money in expanding the program, provided we wrote an NSF grant. So I wrote the NSF grant, and I used my connections to write a grant that was based on a lot of good knowledge base. I diagrammed in detail how we were going to train teachers and how we were going to move this program from one school to twenty-three schools. And how much money we needed. We got the grant. It started in 1990, but we started writing it in '88. Jim was very important, but I did the actual writing; he conceptualized.

Meanwhile, Jim was busy expanding the program with Jennifer into five more schools in Pasadena, using the superintendent's speculation money. And those were carefully chosen to cover the district geographically and ethnically and economically. So that was a real test. And Jennifer, all by herself, started trying to train five schools' worth of teachers; she ultimately got one staff person to help from the district. The program was then funded and expanded, and by

1993 or ’94 it was operating in every school and had become very well known. It had a very special attribute, because the five districts that existed when we started were all middle-class white school districts. Pasadena was the first urban reform district in the country, actually.

COHEN: You must have had a lot of visitors come to see the program.

PINE: We had a lot of visitors. And Susan Snyder, our program officer at NSF who supported this program in what’s called Teacher Enhancement—because all the money goes into teacher professional development and the district has to buy the equipment, but it’s relatively cheap—Susan thought the program was successful and needed to be institutionalized in NSF, and she invented a program that was written up in the NSF announcement to sound like ours. It was called Local Systemic Change, and it began about 1995-’96. It funded certainly over thirty multimillion-dollar districtwide reforms up until two years ago, so that’s for about four years. She’s now retired, and she’s traveling around the country in an RV. The amount of money they put into each of these was two or three times what we had in Pasadena, which was very nice, because we ultimately kept asking for more in Pasadena, but we still only ended up with about half of what she offered. And a lot of visitors, and she sent a lot. So ever since several years ago, Jennifer has had a monthly visitation day, which consists of a lot of looking at classrooms in the morning and a lot of talking about the program over lunch and afterwards. We’ve had, over the years, about twenty visitors a month, on average—from everywhere. And there are a couple of book chapters about the program.

So it became essentially a national model. People visited us from districts that wanted reform. Some of them visited before they wrote their grants. Seattle—because of Lee Hood—wrote a grant, and Lee sent a person from his lab down here, and they wrote a grant modeled directly after our grant, and got it for the whole of Seattle—seventy-one schools. Big bucks! Probably $4 million.

COHEN: This was for training teachers?

PINE: Essentially. Even then, highly skilled teachers weren’t free, and now they’re expensive. So the grant money goes into a combination of funding teacher time to be trained—

COHEN: Are you inferring that you're paying these people more than other people in the school?

PINE: Oh, no! The school district doesn't know how to do that. One of the major problems in education in the United States is that there is no ladder. So the leaders of these multimillion-dollar programs are teachers on special assignment and they get the same salary. If they're lucky, they get put on eleven months instead of nine. Jennifer is the K-12 coordinator now for Pasadena; they just made her a bureaucrat last year, but all the time that the program was big and famous and everything, she was a teacher on special assignment. And our colleague in Las Vegas, which is good to mention because it's such a huge job, has eleven resource teachers and is now probably spending her eighth million dollars, and she's a teacher on special assignment.

So that's where we were. We became nationally famous—and internationally famous, because a funny thing happened. Georges Charpak, from France, a Nobel Prize winner [1992] in physics, visited me—partly by accident. I asked him if he'd like to see what the public schools look like when kids do science, so he came on a tour of a few classrooms one morning. And he decided that this was the only thing to do. He said while he was in a fourth-grade classroom that if he'd only learned science this way, he would have done much better. [Laughter] He's a very, very outgoing, effusive person. So Georges went back to France and immediately went to the minister. Because if you're a Nobel Prize winner in France, unlike here—here, if you're a basketball player you can go to the president. [Laughter] In France, if you're a Nobel Prize winner, you can go to the minister. And he told the minister what he'd seen in Pasadena, and the minister immediately jumped on the issue that Georges told him about, which was that none of these kids was white. The minister was paranoid about the slums of the industrial cities in France. Of course, the French are utterly bigoted about their immigrants, so it's bad there. So the minister said to Georges, "Well, this is a marvelous thing to give to people in Lyon and Nantes and Paris"—and two other cities I don't remember—in the slums, the low-income, nonwhite districts. And they did. They're going; they're doing it. It's a French national incentive to teach hands-on science in K-6 schools, and there's a Web page supported by the French Academy. And they've translated one of the best curricula in this country. There are three—they translated one of the two best ones into French and it's on the Internet. So we have helped the French create a lot of problems for themselves—because their education system doesn't match ours, and they can't replicate what we do.

COHEN: But they can direct from on top, and everybody has to do it.

PINE: Well, I'll tell you, you can't do this with this kind of teaching. You can read textbooks that way. It's a very interesting challenge. And the results have been slow in coming. And in every case that's been successful, they've followed our model by having a higher education institution, full of scientists, working with the district. That's been true in Lyon and in Nantes. I have a very close connection in Nantes for another reason, because I have a reformed curriculum at Caltech which they've adopted at the École des Mines in Nantes, so I've been there. And the same people who do reform teaching in college there, work with the school district, because it's the same philosophy. So we're a national model, and we are busy now thinking of the fact that there's a couple of terrible problems we have never worked on. One is science education for teachers when they're in college, and the other is science education for teachers after they leave school and are teaching.

CAPSI got invented, as a name, around the time we realized we weren't just working with the Pasadena schools—we were working with the country, and the world, actually. The State Department brought a lot of people through, and we had to call ourselves something besides Project SEED, which was the acronym that Pasadena had adopted. So we called ourselves CAPSI—Caltech Precollege Science Initiative. I invented it because I couldn't say the whole thing, and Jim didn't even fight this—he couldn't invent another name.

Jim knew a really neat woman—whose name I've forgotten—who was at that time a high administrator in the geology division. She moved over to be a special assistant to the provost—who I think was then Robbie Vogt [provost 1983-1987]. Jim found out that there was this vacant house on Hill Avenue and suggested to this woman—she's a black woman; she's part of the black power structure, which is not a trivial group of people on this campus, high middle managers. They all know each other—very good group. She knew this house was there and she helped us get some space in it and have it painted and cleaned up, because it had been vacant for years. And as we got more NSF grants—which, by the way, all pay plenty of overhead, sixty percent; they were not small grants—as we got more NSF grants, we had in some ways a sales pitch that we deserved some space. Because we had no space; we were just operating out of Chandler cafeteria and our labs. So we got half of the house on Hill, the bottom half.

COHEN: Where you still are?

PINE: Yes. So as we grew and became called CAPSI, we had more and more projects. For instance, Jim invented the idea of funding postdocs, so we raised some money to fund a postdoc in science-ed, whom Jim then supervised.

COHEN: Was she here at Caltech?

PINE: Yes, she worked for Caltech; she was a PhD in chemical engineering—and she was a volunteer for CAPSI, because she wanted to get involved in education. We have another postdoc from computer science, Jennifer Sun, who's building simulations. Neither of them are postdocs anymore, and they don't work at CAPSI anymore, actually; they work at outside places. But they started at CAPSI as postdocs.

COHEN: Now, you're still concerned with the elementary-school level. You're not moving up?

PINE: No. Deep down inside, we believed that until the elementary-school kids studied science this way, moving up to secondary was a fruitless activity—although people had tried. I know a couple of astonishing secondary-school teachers who succeeded by Herculean effort of creativity and work, even though the kids came to them essentially uneducated. We stayed in the elementary, but we were worried because the teachers in Pasadena came to us totally unable to conceive of what they were supposed to do—because they had been lectured at, in these terrible courses. So we got a grant from the NSF and we built pre-service science courses, which also became influential nationally and which were used at Cal State LA [California State University at Los Angeles] and Claremont Graduate School and Cal Poly Pomona. And in California, you can't major in education, so these pre-service teachers were all majoring in what's called liberal studies. Liberal studies is an invention for helping teachers who are afraid to learn science—or anything beyond child development.

COHEN: Now, they can't have education in the Cal State system, either.

PINE: No, only in the fifth year. It's changing, because they realize that that was stupid. Now they have a new set of programs to help teachers start teaching sooner. You can't plan your career with no experience and then jump into it the fifth year. Bizarre! But people do dumb things.

Anyway, that was quite interesting, and it worked. It's a very radical course—it's so radical that only the people we've personally mentored have used it. But the NSF, again, has always followed our lead, because we always know in advance what they should know. They funded $80 million dollars' worth of programs across the United States to try to reach the same goal. None of them have developed courses that are as far out as ours are, but we hope they've done well. NSF funded eighteen collaboratives for excellence in teacher preparation, at $5 million apiece. They're all centered around a central university where there's a scientist/education-school collaborative. It's a very good model. But what comes out of it depends on a lot of things that you can't predict. I have been on the advisory boards of three of them, so those three know all about our curriculum. And a lot of the other ones know something. So we've had some impact.

After that, we realized we'd better do something about helping our teachers learn science who are currently teaching science and don't know any. So CAPSI got another NSF grant to develop content modules for teacher education. It turns out that those were the same stature and size as a college course. They were forty hours of in-class time.

COHEN: That's a huge amount of work.

PINE: CAPSI has never had a large staff, so we invented a model in the pre-service program. It was my invention and Jim helped—and he was part of one development team. We had three development teams: one in chemistry, one in physics, and one in biology. The development teams were populated by Caltech scientists and collaborators from education. In the case of pre-service, we had collaborators from the Claremont Graduate School of Education; in other cases, we had different collaborators. But always we worked only in collaboration with educators, because we were smart enough to know from the first day that we had to go to Mesa, Arizona, to find out how to do this.

There's a disease of Nobel Prize winners: When they retire, they solve the problems of public-school science education. In fact, two in this country—Leon Lederman [physics, 1988] and Ken [Kenneth G.] Wilson [physics, 1982]—were not able to think that they should first find out what was out there, or learn anything, so they both invented their own things, stupidly. Both of their projects are basically useless—or were. And both of them raised $20 million—it's easier when you're a Nobel Prize winner. But Jim and I are not stupid, and we knew that we didn't know enough. We knew that in creating anything for education people, there had to be education people involved in the creation. We got NSF grants and we paid money. CAPSI became a huge source of organization around grants and sub-awards and honorariums and stipends—and no staff. We had one super guy who ran CAPSI and worked on these projects, too, and one woman who ran all the bureaucracy. And that's it.

COHEN: I remember some years ago, when you were developing your packages that you'd deliver to the schools—

PINE: We didn't develop any kits. It was against our religion to develop those.

COHEN: Was that something you brought straight from Mesa?

PINE: And then it evolved. And those evolved from the kits that were built in the sixties.

COHEN: I thought they were wonderful.

PINE: Well, they're still there. And we didn't invent any elementary curriculum. The new curricula, which are replacing those in Pasadena and elsewhere in new districts only, were funded in the early nineties by the NSF, which realized that it should do some revising of those curricula instead of just letting us all steal from Mesa what had evolved by accident. So they paid a lot of money, and they funded the three curricula I've mentioned. And each curriculum project was funded for $5 million to build seven years of elementary kits. It's all kit-based, meaning the materials come in a big box and the teachers get the box refurbished. So that's the way it worked.

We did in-service education—built these forty-hour courses, four of them—with teams, led by a CAPSI person, one or two Caltech graduate students or postdocs, and one or two educators. We had five school-district collaborators across the United States, because we were building curricula for use in school districts for teacher education—in science, not in pedagogy. But all our curricula are taught with inquiry pedagogy, so they all have a pedagogical ingredient, which is critical in that the teachers who take them learn science the way we hope they'll teach. So we did that in pre-service. We were very pleased. The first trial teaching at Cal State LA blew the kids away. They were just overjoyed, and they were very good. So we did it in this next project, with the so-called content modules. And they have just been printed a year ago and are distributed at $10 a copy to about fifty school districts who've bought them. We have no idea who's using them, except in the original five. And the original five helped us plan what we were going to do, picked the topics, and then took the trial versions of the teacher's guides and gave us personal and long-distance feedback to create the revised versions, which we printed. And with NSF money, they were funded. So once again, we're in the sub-award world. We've raised a huge amount of NSF money that we didn't keep, because we have no staff—and Jim and I don't get paid, at all, ever.

COHEN: So how could you give your time to put into this?

PINE: It varies. It's grown. It was less than half my time, but it's gotten to be about half; earlier it was less. We weren't doing as much earlier. These things grew, from working with Jennifer and the schools in Pasadena—by the way, Jim and I were on the steering committee that ran the Pasadena project with the deputy supe and Jennifer. And it was a really tight collaboration. Absolutely unique! Never been done. And it's on the National Academy Web page, to some extent, and other things we've done are on it. So we are an example on the National Academy Web page, of higher-ed/lower-ed collaboration.

So we grew. And then came the time when the NSF advertised for something called Centers for Teacher Enhancement. They decided they would try to help people who wanted to reform their school districts by giving them some centralized technical support. Well, we'd been doing that informally for years. So I wrote that grant. The first year of the grant, 1994, we got it, and we were the only ones who got it. And that gave us enough arguing position to get the rest

of the house on Hill Avenue, because you couldn't very well manage such a huge project with no space. We also got a little bit of money out of a really nice provost who's an engineer—Paul Jennings [professor of civil engineering and applied mechanics, emeritus]. A neat guy! Sweet man! We had two big grants to do things that involved collaborating with teachers, building things with teachers, pre-service and in-service things, and we had no workspace in the house, we just had the bottom floor. And I got Paul to agree to give us about $20,000, which we used to make a huge improvement, because we modified the house's garage into a laboratory and we closed in a terrace to make another office. So we got a huge amount for $20,000—we got about 800 square feet, something like that. Paul was very supportive. So that's another piece of Caltech support—again, $20,000. At that time, we were operating with a total number of grants totaling about $2.5 million.

In the Centers grant, we asked for $6 million from the NSF to work with fourteen school districts to emulate the Pasadena model. Each school district got money to build a program in one school and most of that money went into paying one teacher. The model was to send the money to the school districts and tell them to follow our lead, and what we provided was a manager for the whole project, who was a genius of a professional science resource teacher, Laurie Thompson. We stole her from Pasadena, with Jennifer's permission. And we couldn't have done this without Pasadena expert resource teachers, who visited teachers and coached them, going to the districts, which are all in California between Tulare and El Centro. They cover a distance of 500 miles but they're all in California.

COHEN: But you must have some structure. For $6 million, you've got to have a structure. I mean, who was passing out the money? You can't sit at your desk and say, "OK, we'll give this one money and that one money."

PINE: No, we have a procedure, we work at it. In the Center, for example, we got from the state—by the way, this is all done by one clerical worker and Laurie Thompson and me and Jim. That's all it takes; you just have to be willing to use your head. We went to the State Board of Education and we asked for all the school districts in California that were more than half minority, and then we sent a letter out to all those school districts, to the supe. That doesn't work worth a damn, because superintendents don't open their mail. Then we sent another letter out to

the science coordinators. So between those two letters, we got applications for a program that we invented. And it was a program of funding work… **[Tape ends]**

Begin Tape 3, Side 2

PINE: We got this grant, and we have fourteen districts. And how did we do it? First, we got the applications and then we had a workshop at CAPSI. Each year we funded only three, so each year we advertised. And if you were interested in competing, you could come to CAPSI for one day. We gave them a high-pressure picture in one day. Jennifer is part of this, of course, so our staff is bigger than I said: It's Jennifer, Jim, me, and David Hartney, who is running CAPSI—he's a marvelous guy, an engineer and educator both—plus a clerical assistant. And that's it. So we run the four-alarm fire, the one-day workshop. We give them forms and they apply, and then we screen them. Then we do field trips to observe the nominated superteacher in the nominated school. And they're not necessarily in the same schools—sometimes the superteacher is from a school other than the nominated school.

So we pick three a year and then we ship money. You asked who does everything. David did all of the organizational, gut-level work. David Hartney was hired to direct CAPSI. We stole him away from the National Science Resources Center, where he was very unappreciated, and we gave him a hugely wonderful life of eighty hours a week of fun. So David actually organized all this and wrote the application form and so on. And we all visited and interviewed and did our work. Every year we picked three schools, for four years. The fourth-year group—the last cohort—is finishing this June. It's a three-year sequence: The money they get, about a quarter of a million dollars, supports their leader for three years and supports the teacher education in that school—which has typically thirty teachers—for three years. And the money we give Jennifer to pay resource teachers' salaries when they go off and work for us and travel, was $1 million. So that's how we spent $6 million.

COHEN: Does one have to account for this?

PINE: No. The NSF is so unable to do anything like find out what happens to its money that it doesn't try. It does spot audits on occasion—which terrifies people. But they can't audit more than one out of a hundred projects.

COHEN: So they don't really know if their stuff was successful.

PINE: No. The worst of it is that in science—you see, NSF is a science funder. There are five science divisions, and one education division. For the science funding, they send the money out and you judge it by the papers that result. You don't get the papers themselves; you get a final report that tells you what was published. It's judged by the world, because they apply for more grants, and if the papers are crappy they don't get any money or any grants. So in the five divisions of the NSF that fund science, the only things that get visited are these $5-million-dollar centers; they're visited spasmodically, so nobody knows what they do, either. And in education, my NSF program officer, Susan Snyder, has all these reform projects under way. There's no publishing—there can't be; we're too busy—and there's no journal to publish in. I once guessed that at one time Susan had about $150 million in grants out, because she was the LSC [Local Systemic Change] person, and those are big grants. And her travel budget was for one trip a year. [Laughter]

So, the answer is—pitiful! *We* knew what was going on in all our school districts and we managed them like mothers. In the first year, a Pasadena resource teacher visited their head teacher in the pilot school every week. So we drove hundreds of thousands of miles up and down California, visiting these districts weekly, in the first year—and then later, less often, using approximately one quarter of Jennifer's staff and paying her for it. Because by then her program was being eroded by the district, and because we gave her this subcontract, she didn't have to fire her people. So we had a symbiotic relationship with Jennifer, based on the fact that we managed to invent this wonderful grant at a time when her people would have had to be fired.

So the Center is still a live issue, and the modules just got published, although we stopped developing them about a year and a half ago. The pre-service project is about to be reborn, because I have discovered from Cal State LA that they have a model system. And I don't care what happened in the other two places, because they've had terrible problems administratively. But the Cal State physics professor who started this—there's a physics and a biology professor who teach this course, because it's half biology and half physics. Ken Anderson is the biologist; the physicist is Marty [Martin B.] Epstein. They have expanded to teaching three sections a year—which is about fifty kids—and they've been doing this for three years. And there are teachers out there in the schools who've been through this course.

So, the Center is still going. We got a supplement of approximately $300,000 to run the Center in order to form a consortium of the successful districts. Now, the successful districts have to be—you have to invent what success means. But of the original twelve, approximately eight are successful. Two are on hold, and two are failures. Which is *way* above average. But success doesn't mean they've done the whole district yet; success just means that they've become a powerful influence, their school is beautiful, their district loves it, and their district is trying to figure out what to do to make it go. We've been trying to convert this group of twelve districts, which have been mentored over the years by Laurie Thompson, into a coherent consortium that can make a model of how reform districts can help each other and become stronger, and we got a supplementary grant to do that. So that's alive.

And there's a *huge* curriculum development project, which we've been doing for three or four years in high school and middle school—materials. Because we tried to go from sixth grade to middle school and failed. We could not find materials that could be used to change the way middle school science was taught. And you can't just take the same materials and change how the teachers teach, because they don't lend themselves to this kind of teaching. So we invented the idea—which, again, is original with us, and the NSF has copied it—that you should do a four-year curriculum that bridges from seventh through tenth grade, for all kids. So that there's a science background that's in the hearts and souls of everybody in America. And after tenth grade, they can all go on their wonderful ways. We've been working on this for many years now—three or four, it would be fair to say, seriously for at least two. It's been a huge project and we've raised over $2 million without NSF money, because the NSF refused to entertain it—it's too radical. And we've reapplied for $4.9 million. Jim Bower just left.

COHEN: Where did he go?

PINE: University of Texas [at San Antonio].

COHEN: As a biologist or an educator?

PINE: Biologist. He's always had a huge, successful research lab. He's only forty, and he's had an impact on CAPSI because of his intellect—but not much because of the time he spent on it.

He's terrific at one-shot deals. Brainstorming. Speeches. Travels around the country. Missionary. But the total integrated time that Jim puts in is not very much.

COHEN: Who's taking his place?

PINE: Don't know yet. There's a degree of complexity right now. What I've described to you is approximately $13 million worth of funding over a decade. And CAPSI received from Caltech $25,000 from Goldberger and $90,000 total for the next fourteen years. CAPSI has never been connected administratively to the institute but has always been treated as if we had to make our way like a lab, on soft money. One key conversation was with the vice provost, David Goodstein. I remember that we asked David for our education project to be supported by Caltech, as a Caltech activity, because we were beginning to think about sustaining our efforts, and by then we may have had some ideas for other things. And David told us that the rules were clear—that all we had to do was to raise money like every other researcher and we could do what we wanted to do, and there was no reason for Caltech to give us any. We were told that explicitly by David once, when we complained and said we should be a part of Caltech and you should be paying us with a grant manager and an administrator. So we've done it. We did what we were told to do.

It suddenly was realized a year or two ago that there was more than one project—two or three—that had no administrative chain of command, and we were one of them. And there was a concern on the part of the management, because one of those was not doing well. I don't know if it was a scandal or it was just not doing well administratively—probably it was a scandal.

COHEN: This wasn't your organization.

PINE: Some other. We're scandal-free! [Laughter] So it was decided that there couldn't be these loose-gun activities. Right now, the provost [Steven E. Koonin, professor of theoretical physics, 1981-2004; provost, 1995-2004] is grappling with our issue. I have asked somebody to be a co-director, and she has responded that it would be great, she'd love it, but she wants to see what the provost does about putting us into the chain of command.

JEROME PINE

SESSION 4

November 6, 2001

Begin Tape 4, Side 1

COHEN: It might be interesting to go back over the years and talk about your relationship with Caltech. Where would you like to start?

PINE: Well, let's do the simple thing first. I've done a number of things in undergraduate teaching that were innovative, and in every case the division management—and in some cases the division population—has been sympathetic, supportive.

COHEN: I do remember your making this nice box of tools so that the freshmen had it in hand.

PINE: Yes, that's the latest—that's seven years ago or so. Well, that's an example in which I said I thought if I had to teach freshman physics I should do something that was somewhat more a match to non-physics students. I had some ideas, and one of the ideas was to generate this set of laboratories for kids to do themselves. At that time, the division was being run by somebody nice—I forget who; we've had a sequence of supportive chairmen.

COHEN: Was that Charlie [Charles W.] Peck [division chairman 1993-1998; professor of physics, emeritus; d. 2016] then?

PINE: It could have been Charlie or Gerry [Neugebauer, chairman 1988-1993; Millikan Professor of Physics, emeritus; d. 2014]. It was about seven or eight years ago. Anyway, the division hierarchy said, "Well, why don't you try it out? We'll support you in a trial of this process." And I did. It took some time to evolve, because on the first try I had to get student reactions and we had to make it work better. But over the time of trying it out—

COHEN: Would you mind just briefly saying what you tried out?

PINE: Well, I tried out the idea that students in Physics 1 would do experiments in their rooms with a kit of materials. I borrowed this idea from friends at MIT, who had invented it originally for a summer program but had offered it as an elective to a small number of students—ten percent of the freshmen. And I decided that that was so great that I would like to try it on all Caltech freshmen. And the administration was willing to play along on a trial basis to see if it was a good idea.

COHEN: And they had to fund it, because you had to build all those boxes.

PINE: That's right. And they cost a nontrivial amount of money—on the order of $70 times 200 kids. I think we may have made the kids pay for them right from the start—we do now, on the assumption that these kits are not very different from textbook costs. But the main thing is, I was allowed to do it. I was supported in doing it. And after it was done for a couple of years, there was a double-barreled reform, which I proposed—which was to split the freshman physics course in two and to teach the practical course, so-called, out of a less mathematically demanding textbook and make these home experiments more important. The home experiments would become optional for the kids who did the more abstract course. The division liked the idea of the kits and they approved of making it sort of an institutional venture. So it's been going on.

COHEN: So that continues—freshman physics is in two tracks?

PINE: Yes, the so-called practical and analytical tracks. And the analytical kids can do the kits if they register for three extra units. But it was too big a load to make the kit part of it important and also do the analytical textbook. So for the practical kids now, the kits are quite important. They count twenty or thirty percent, a significant part of the course. Several people have now taught it that way, so it's become in some way institutionalized.

COHEN: Do the kids like it?

PINE: Yes. They're shocked at first, some of them. I was grading papers this morning with kids' questions on them—I ask my kids to write questions. And one young woman wrote about one of

the homework problems, "What's a vise?" The problem said that there's a block of wood held in a vise. She had no idea—it was a foreign word to her.

Anyway, I think in general it's a success. Some of the people who are most reluctant realize that it's actually quite enjoyable. And of course, if it weren't a success with the kids, it would not have lasted, because there's a lot of feedback from the kids to the faculty. So that's been very satisfactory, and it's actually been adopted at Harvard. The course I sort of developed, building on the MIT course, has evolved into a Harvard course. The MIT course is still a very small fraction of the students, so it never became institutionalized. The same idea differently done—quite differently. But that's just a difference in personality in the people and culture.

But before that, I had done other things. And in every case, I volunteered to change something, and everybody said, "Sure, OK. You can do it." And some of the things I did were quite radical. One of the most radical was that I invented the idea of freshman seminars, in which the freshmen had an opportunity to go to a once-a-week seminar with a faculty member on some arcane subject like special relativity or astrophysics and get three or four units. And in order to do that, I had to recruit fifteen faculty members who felt it would be fun to talk to freshmen in this informal but relatively serious way. Because they had to read and study at the seminar.

COHEN: Now these seminars were set up by whom?

PINE: By me, with the permission of the division, on the basis of, "You can do anything you want to do, as long as you do it." And they were not institutionalized—I did not keep doing it.

COHEN: I'm not understanding this. The seminar wasn't on a specific subject? It didn't matter what the subject was?

PINE: Well, the freshmen had a choice of seminars taught by faculty members who wanted to teach whatever they felt like. I mean, they volunteered to teach something.

COHEN: I see. This was something not part of the regular curriculum.

PINE: Oh, definitely not. It was something to get the kids out of their textbook. Freshmen at Caltech do nothing even slightly real—all they do is homework problems out of textbooks. So the idea of this was to give them a chance to do something that was not homework problems out of textbooks and had some connection to the kind of physics they might be interested in. So it was quite successful.

COHEN: So you had to talk these fifteen people into organizing this.

PINE: No, they just had to volunteer to do it, and I told them what to do. They'd have to have ten or more students, because there are 200 freshmen, so you couldn't do this except by having ten or so in a seminar.

COHEN: Every freshman did this, then?

PINE: Well, I'm not sure of that. I pass. Maybe not all of them did it. But a lot of them did it, because it was offered to them and they did it. I had to recruit a lot of faculty, but that was easy, because they thought it would be fun. They didn't have to do anything, really. So that was interesting. That was a long time ago.

COHEN: It didn't last very long.

PINE: No, because nobody took it over, and I wasn't going to do it forever. I had other things to do and other things I wanted to do. So then I taught atomic physics to the juniors differently from the way they'd been taught. I made them write term papers. And that was quite an unusual idea for this place, which generally does nothing but grade homework problems. At that time, Robbie [Vogt] was teaching juniors, too. And we both had them write term papers. And that may have lasted—I never kept track. I now teach my senior graduate student course that way in neuroscience, with no lectures. Over the years, I've instinctively avoided making everything dependent on lectures; everything I've described to you so far is non-lecture. The philosophy of reform in elementary-school science is to remove the lecture, remove the teacher giving speeches, so that may have permeated my consciousness, even though it wasn't overt to me that I

was getting rid of lectures. All of the innovations that I invented were innovations to move away from lectures.

COHEN: And away from textbooks.

PINE: Yes. But the division has supported all of those innovations, based on my own energy. That's one of the things I think is true about Caltech—it's a very easy place to do your own thing and there's not a hell of a lot of people who say that they have to tell you what to do, both scientifically and academically. The result is mixed, of course, because that means you're allowed to do a terrible job of teaching as well as a creative job, because nobody is watching and thinking and caring about it. You can't do a terrible job of research until you've got tenure; afterwards, you can do a terrible job.

COHEN: Well, if you want money, you have to keep on doing something.

PINE: Well, but you can keep getting paid by Caltech indefinitely, so there's a lot of freedom to do what you want to do. And in fact an example is my friend Ken [Kenneth G.] Libbrecht [professor of physics], who's a very good physicist, who's chosen not to raise money and not to work for big money.

So then we get to the education business. One part of the story is the support of the academic world for undergraduate teaching. The labs, by the way, have also been adopted at an elite French university, the École des Mines, which has seven campuses. It has a new campus in Nantes, which is run by radicals. It's all engineering, by the way; it's an engineering school, a real engineering school. There's École Polytechnique, which is a great, highly prestigious place where government bureaucrats study, and then there's École des Mines, which is almost the opposite—where real engineers study. And in between there's an École Normale Supérieure, which is sort of a poor man's École Polytechnique. These are called the elite universities, but they're separate. They're very rich, too. So École des Mines found out about my course in an interesting way—through the visit of Georges Charpak, who happens to be an alumnus [of the Paris École des Mines] and was very involved in French educational politics. And he also passed the word to his friends in Nantes, and they adopted my freshman physics course—my labs, which are now institutionalized there for all their students. They do the boxes and they've

translated my instructor's guide into French. So that was very successful, because not only did it change freshman physics at the [Nantes] École des Mines but the president of the École des Mines became a convert to hands-on education, so the entire curriculum at the École des Mines [in Nantes] is now student-centered hands-on education—unlike here. It's quite an impressive change! It was a new school and this was a radical group, and they did that. And the interesting thing that's happening now, which relates to undergraduate education, which I feel rather good about—first of all, not only did the president of this place change the École but he wrote two books proselytizing the engineering faculty of France to change the way they teach. I've seen them—I can't read them; they're in the most flowery academic French, high-brow French. Half of the first book is about my labs, because that was their first experience. But the thing that's happened lately is that the École des Mines in Nantes has become a hotbed of activity in trying to change education of engineers internationally. They started an international conference last year, which was attended by people from about fifteen countries—all from the first-rate universities of those countries that train engineers. And it's going to happen again. It's become an institution. There is now a Society of Active Learning in Engineering Education.

COHEN: Coming back here, have you had any relationship with the Caltech engineers?

PINE: Only by osmosis with Dave Rutledge, who knew about what I was doing and changed the sophomore electrical engineering course to be a hands-on course. And I'm trying to find out if any other engineers are doing anything, because I'm going to Colombia on Friday and I'm supposed to talk to them in the engineering school.

COHEN: And they're liable to ask you what's going on at Caltech.

PINE: They'd want to know. So that's higher ed. So higher ed's been pretty interesting, looking back on it with you—because I'd never added it all up.

We've stayed in the Hill Avenue house—we got an extra garage, because the president's staff use the house next door and they weren't using the garage. I asked David [Baltimore, Caltech president 1997-2006] for his garage, and he said, "What garage?" We didn't actually get any money from Caltech for that; we paid for that rehab out of a grant—illegally—because we needed the space. So we have two garages and the house. About four years ago, when David

Baltimore came, I said "Marvelous!" We had tried to get money out of [Thomas E.] Everhart [Caltech president 1987-1997] and it was exactly identical to Goodstein's story: "It's not our role in life to support educational reform in precollege education; we just don't have any reason to support it."

COHEN: He said, "So if you want to do it, go ahead."

PINE: Well, yes, of course, but he didn't *have* to say that. We *were* doing it—and in a big way, by then. And Everhart knew all about it. I mean, he visited the schools and the kids gave him testimonials and everything. He just didn't make the connection, that he should give a damn. Then Baltimore came, and I thought, Wow, great! This is obviously a new breed of cat—after six successive college presidents who were physicists, or something like that, we had this guy. Well, we went to talk to him at some length about whether we shouldn't have some connection in which the institute paid some attention to precollege science education and supported some of the infrastructure that you need to run CAPSI, and the response to that was quite amusing—well, I wouldn't say amusing; it's indicative of a frame of mind. It was a long conversation, and the outcome was that I got a one-sentence e-mail from Steve Koonin's administrator that said, "We've decided to deposit $50,000 in an account to help CAPSI infrastructure."

COHEN: You got something.

PINE: Yes.

COHEN: Was that the kind of money you were asking for, or hadn't you put a dollar sign on it?

PINE: We hadn't put a dollar sign on it, but we were asking for something very different. Because this was like asking for charity—this is like giving money to the Red Cross.

COHEN: OK. It was not saying, "We support this program."

PINE: Well, it wasn't even a letter. It wasn't even a human response. I mean, it was a response of a one-sentence e-mail from a secretary. From neither Koonin nor Baltimore have we ever received a written recognition of our requests.

So that was very useful—we had $50K. As you pointed out, it's not peanuts. Partly because we didn't have any legal money that we could spend on infrastructure except by cheating on our grants, because NSF pays huge overhead and they don't expect to have to pay for infrastructure. So the next year I wrote Baltimore a fairly long letter again. And again, did not get a response but got a one-sentence from Koonin's assistant that said $40,000. [Laughter] And that was about three years ago.

COHEN: I don't know why we're laughing. That's terrible!

PINE: Well, it's the way the institute is. We've had approximately a decade since CAPSI was a significant player, roughly a decade—the time during which the Pasadena school system became nationally known and we began to do other things and expand and move into the house on Hill. And in that decade, the total sum of institute support has been $90,000, and the total grant support has been $13 million, give or take. After the second $40,000, I said to Jim, "I am not going to beg with my tin cup again." So the subject died, and we have not received a penny since that last $40,000.

COHEN: Do they ever use you?

PINE: It's a very amusing thing. If you ask Hall Daily [director of government relations 1987-2017], who commutes to Washington to run Caltech's relationships with the government, the thing that most people know about in Washington is us. And if you went to Baltimore's inauguration, the only thing that Maxine Singer [president of the Carnegie Institution of Washington 1988-2002] said about Caltech in her speech, which was very long and complex—she's a good friend of David's—was about us. And the only thing Murph [Goldberger] said in his talk was about us. Everhart did not mention us, but Murph did. Across the country—and maybe even the world—because we're the Caltech Precollege Science Initiative, there's an assumption that we have some interaction with Caltech that's more substantial than a research lab. And of course we've not bought a big full-page ad to disabuse people of that. But when we

get to know people well, they find out. So it is known by our closest colleagues that we are self-sustained on our own wits and activity. This is not terribly unusual, because people who do research in education faculties, like Berkeley and Wisconsin and such like, are not supported particularly as outreachers. They're supported by grubbing money from foundations and the NSF. So in general there is almost no interaction between higher-ed and lower-ed, at the level of them giving a damn. But that's changing, at a reasonably high rate. There are some exceptional cases.

COHEN: Changing, but not here?

PINE: Not here at all.

COHEN: Where is it changing?

PINE: Well, MIT now has a staff person paid by the president to help work in their program for creating high school teachers and doing other things like that. And the University of Arizona created four tenure-track faculty positions in science education—led by a wonderful guy who actually spent a sabbatical, four months, with us. Of course, we invited him. In some ways, he's a protégé of ours, and he led this reform in Arizona. And in Texas and many other large research universities there's now a serious effort to train high school teachers from science majors. And we have a small effort, which is funded by a foundation.

COHEN: USC had a minuscule one going some years ago; I don't know if it's still going.

PINE: Well, we have an effort that's running now which we just started, funded by a foundation. But Caltech as an institution has had no connection, except lately, since they were embarrassed in the *Chronicle of Higher Education*. The embarrassment was that the *Chronicle* had a story in which it pointed out that this year's incoming Caltech class had zero black students, which was a source of great embarrassment. So there's been a big effort made to promote this funny thing called diversity. And actually quite a lot of Caltech money—hard money from the administration—has been used to recruit and create a few people to make us more diverse. I call this a cosmetic activity—very cosmetic. First of all, nobody in his right mind who works with

minority kids would think that this was a tremendously valuable and important thing for them, and it has a minuscule effect on the national picture. So it's just cosmetic. But it's being supported, far better than they've ever supported us, with staff—full-time, expensive staff. There is now a Caltech outreach interest, but it's all self-interest and none of it is interested in improving public schools. By the way, there are a few people who are interested in improving public schools, and they're not Caltech administrators. One of them is Julie Kornfield, who just got a $5-million NSF center grant, and she was forced by the NSF grant to have an outreach program. Now, some people who get an NSF grant generate outreach programs that are just pure horseshit, but Julie is actually supporting some school-based outreach with us. So I know about that one. She was the head of the [Caltech] Y when she was an undergraduate; she's always been a save-the-world person. She's a sweetie. She was in my freshman physics class when she was a freshman.

Anyway, the story is amusing. And it hasn't ended yet, because about a year and a half ago the IAC [Institute Administrative Council]—which runs Caltech, as you know—was dealing with one activity on campus that had been operating more or less independently and had gotten into some sort of trouble. Because I didn't probe, I have no idea what it was. But the opinion of the IAC was that there was a very serious problem, because they weren't in the infrastructure and there was no oversight of their activities. And the IAC decided to look around and they found three people who were operating that way—of which we are one.

COHEN: Did this come as a big shock to them?

PINE: Well, I don't know if it came as a big shock—three is not a very large number. Well, I mean, it wouldn't if they *discovered* it. I mean, clearly, at least [Thomas A.] Tombrello [chairman of the PMA Division 1998-2008; d. 2014] probably knew that we were operating with no connection, because he'd been signing our grants. And the chairman of the Biology Division had been signing grants for Jim. So they knew.

Anyway, what came of this was that the IAC decided that this independence is not a good idea. And nothing happened. But recently we've heard from Koonin that he's very concerned that there be oversight—an interesting connection, contact. The contact is of course ignoring the fact that we *asked* to be connected, off and on for ten years, and ignoring the fact that we needed

infrastructure support. Because "oversight" is a peculiar word and does not imply anything except something watching you. So we've been informed that Koonin is concerned about our oversight and that he is going to have a go-round in which he informs himself about us.

COHEN: He's going to come and see you.

PINE: First, he got a sourcebook from me—for him and a group of six people. And they're going to come to see us. We have no idea yet what's in his head, if anything. Or if he's open-minded and is just operating on the assumption that we ought to somehow or other be part of the system. Now, one option is that Koonin will give up on this idea of having six people come with him, and he'll inform himself, because I gave him the sourcebook, which is very substantial—much more than he ever knew about CAPSI, I'll tell you that. And he may just read that and decide what to do and tell us, before Christmas—because he was trying to meet before Christmas. So we don't know.

But we're very successful now, because our research group has become not quite nationally famous yet, but inside the NSF they've had enormous success. Our research group just hired someone yesterday again. They have nothing but top-flight, superstar young people, led by one very, very experienced leader, Pamela Aschbacher—she's the wife of Michael Aschbacher [Hanisch Professor of Mathematics, emeritus]—whom we recruited. One grant we're finishing, and two grants that are under way, which total $2.5 million.

COHEN: I don't believe we've talked about CAPSI's research division.

PINE: Well, we have a two-year-old research division based on the fact that a colleague and I in school reform were distressed by the instability of these reforms. So we asked the NSF for a grant to study sustainability. And we shared it—it was a million dollars. Half of the million went to CAPSI. And we had to staff ourselves. We had no researcher. We wrote the grant; and in fact it was written by a researcher back east in our partner's place, because they had a researcher.

COHEN: What school was that?

PINE: It was at the Education Development Center in Boston—a huge nonprofit; 500 people on soft money. Can you imagine how many grants that is? The outcome of the sustainability project was that we had to hire a researcher, and we were very lucky to get Pamela, a star researcher, who lives across the street from Caltech and had worked for fifteen years at UCLA. She was very motivated. We hired two people. We wrote a new grant—got $1 million. Hired two more people. Wrote a new grant—got $1.6 million. And just hired two more people. So we now have one of the largest educational research activities in the country dealing with practice, because most of the educational research does not deal with practice. We're going to be very famous, and we're becoming very well known in circles that care about practice. That's brand new, and that's a huge amount of Caltech overhead and income.

COHEN: Even though Caltech isn't supporting you, they're still taking all the overhead money?

PINE: Oh, of course. We don't have anything going for us, because we're surrounded by many philistines. But we have friends. We have some extremely good supporters who are not philistines—who care about and know about what we're doing.

COHEN: Are you talking about professors here at Caltech?

PINE: Well, I'm not. It turns out that the number of professors who care about what we're doing is tiny, but I can name one—Bill [William A.] Goddard [Ferkel Professor of Chemistry, Materials Science, and Applied Physics]. And I can name another one, in physics—Mike Cross [professor of theoretical physics, emeritus]. And there really aren't any others. There may be three or four others that I don't know about, out of the 300. CAPSI has an advisory committee, just formed, because I wanted it to be formed and to have connection across the campus. And on the advisory committee is Hall Daily, who's government relations, and Dick Seligman, who is the director of sponsored research, happens to have a doctor's degree in education, and was the director of finances for the educational school at UCLA in his previous life. A great man! And then Alice Huang [wife of David Baltimore; senior faculty associate in biology], who knows something about education, but she's not a faculty member. And a neat guy named Andy [R. Andrew] Cameron, who's a long-term biology senior researcher [now emeritus—ed.] who cares

a lot about education. Altogether we have six people on this committee, of which one—Bill Goddard—is a faculty member.

But of course, we also have a good friend who's a trustee—Shirley Malcom, who's the outreach expert at the AAAS [American Association for the Advancement of Science]. Shirley is a friend. We don't know her well; she hasn't been here. I've been on committees with her, so that's how we know her. Anyway, Shirley is ammunition. She's a direct pipeline to the trustees, which we haven't used yet but could. There's thirty of them, and I would guess that the majority of them hate public education. They are like George Argyros, head of the Beckman Foundation, who hates public education. So it's a very complex world we live in.

COHEN: This morning I picked up the paper and saw that the Bill and Melinda Gates Foundation gave I don't know how many millions to train high school teachers.

PINE: That's great. They should do that. Of course, they gave a billion to kids. They're doing the right thing. They hired a very good person to help them give away money. That's right; that's good! At least somebody is advising them as to what's important these days. There's a report, which you may not have ever had to see, called "Before It's Too Late." Do you know about that? There was a commission chaired by John Glenn, the former senator—a huge commission of big shots, forty of them. And they wrote a report which I have—shiny, glossy, Department of Education sponsored report. "Before It's Too Late" is about improving high school education. It's a major point, and people like Bill Gates could respond to it. We've used it in our grant to other people; there's even a piece of it in this thing I gave Koonin, in hopes that he might actually understand why we're training high school teachers.

COHEN: What about the projects that Caltech has had through the years that I've had some experience with, like Lee Browne [director of secondary school relations; lecturer in education, 1971-1990].

PINE: That's right. Lee is a good story. I'll tell you, I was heavily involved in that. There were three of us who hired Lee.

COHEN: I did an oral history with him not too long ago.

PINE: Did you? Oh, boy, I bet he gave you an earful. He was a really neat person. John Benton [Dreyfuss Professor of History, d. 1988] and I, and probably Harry Gray [Beckman Professor of Chemistry], were on an outreach committee; they recruited him. At that time [1969], Caltech was trying to diversify seriously, not cosmetically, and the Admissions Committee was out there beating the bushes for minority kids with 400 SATs and high IQs. And we did. We admitted about twenty or thirty really poor, nonwhite kids, of whom about half graduated. And the half who left all graduated from somewhere else. I just met one the other day. [Tape ends]

Begin Tape 4, Side 2

PINE: Lee Browne's minority-student program was funded by soft money with almost *no* support from the institute, ever—typical, like CAPSI. And the Outreach Committee was in some way his liaison to the faculty. I was the chair of that committee, and what we mainly had to do was to defend Lee against the monsters, who were always harassing. And if you think he's paranoid—well, he is a little bit. But he has every reason to be paranoid. I know him well enough to know, because we had long conversations when I was the chair of this committee and he was getting beaten up. We've had long conversations in which Lee's general almost-paranoia was a subject of conversation between us. But he had every reason to be paranoid. Even paranoids have enemies. And the faculty objected to the junior high school kids because they were too noisy on Saturday, if you can believe that. I mean, it was disgusting—it's the only way to characterize it. Ultimately, Lee was made sufficiently unhappy that he quit.

COHEN: Well, then they changed it, and they decided they were going to concentrate on Hispanics. That was the point at which the high school teachers dropped out of it, basically. It suddenly wasn't relevant to what they were doing.

PINE: Well, Lee's career here was complex. I'm glad you mentioned it, because that was another thing I was involved in. In fact, in those early, prehistoric days, Harry Gray and I figured out a program in which we took science to the junior high schools. We had cars with gadgets in them, going up to Washington Junior High School. So that was kind of exciting. That was a long time ago. There are various schemes for liaising with the high school kids for

the Watson Lectures in Beckman, which is very good. That's a scheme that was invented by a nice man whose name I don't remember—all by himself. He's on the Public Events Committee.

COHEN: To have kids come?

PINE: To brief them first. It has to be done well. The net result is that if you ask what the administration has done, it's essentially zero. But they've allowed things to happen, to try to survive—like Lee, and us, and other people. That's their way of doing business.

COHEN: Well, that may be no different from any other institution.

PINE: It is different! It is the lowest of all high-quality universities. There's no point in mincing words. There's no other high-quality university that has as little commitment to the real world as Caltech. Because I know people at all of them. So this is an exceptionally arrogant and opinionated and narrow place. And it isn't going to change—it's a given.

COHEN: Is that because we have people who are just consumed with their own stuff?

PINE: Probably. And some people who are not consumed with their own stuff find the place so relatively unsympathetic that they leave—like Lee [Hood] and his friend Bill Wood, and others.

COHEN: And yet you have flowered here, and done very well.

PINE: That's because I just did my own thing and I didn't try to change anybody's mind. And the people I dealt with in my close departmental connections were not unsympathetic.

COHEN: So you say it's the institute itself?

PINE: The management has been officially—their position is "It's not our concern." That has been the official position. And in this day and age, it's uniquely bad. The University of Arizona is the extreme case, where they're creating tenured faculty tracks. MIT has a staff, et cetera.

COHEN: Do you think it has anything to do with the fact that—maybe this is changing, but certainly in my time the education schools were considered not very good. People who weren't very bright went there.

PINE: Oh, they still are not considered very good, and there's still an enormous amount of snobbism on the part of academics. At institutions where there is an education school and a liberal arts and science school, their connections have been tenuous at best. I happened to work at several of them as an advisor, so I know. Arizona State is a very good example, where the Education School and the Arts and Sciences School are now connected, but they weren't before. They got a $5 million grant to work together. [Laughter] That helps. In Tucson, where I told you there are four new tenure-track jobs, the Arts and Sciences people very foolishly did this around and over the heads of the Education School, because they didn't think they were any good. And now they have a problem, because they realize now that they can't train high school teachers. They don't know enough. They can't use four faculty members to create a school of education. So they are now talking to the Education School.

COHEN: In this training of teachers—well, who trains teachers here are places like Cal State LA or Cal State Northridge. Have you had anything to do with them?

PINE: Well, I know the people in some of these places. I know teacher-ed at Claremont; I know teacher-ed at Mount St. Mary's—because that's where Nancy's been. I don't know much about secondary-teacher ed. It hardly exists. [Laughter] There are very few secondary teachers, and they seem to come from every possible nook and cranny into the profession. They have to get credentials, and they can get them at Cal State or Mount St. Mary's or Claremont. But the idea of an organized educational process for secondary teachers is new. The University of Texas is hugely successful in turning out a hundred a year, where there were none—out of science majors. A hundred a year are getting double majors in education and science. That's only three years old—zero to three years, zero to a hundred students. So now, "Before It's Too Late" is having an impact—plus people. People think, and they know. People who work in schools know. So there's a real change.

And, of course, we're doing it here, but not thanks to the administration. It's kind of a peculiar situation. You can say what you have said—that I have not had any problems. But the

answer is that we spent a lot of energy living without any support. And you know, you can sense from this interview that I don't forgive that. That doesn't mean I'm going to quit. I mean, my options are not so rich; nobody's made it more attractive to leave than to stay. But that does not mean that I forgive or that I respect. So it's a peculiar situation. Every once in a while you meet somebody who comes in here from outside. I have no idea what's going on in the head of Sheryl Gorchow-Stuart, whom I know a little bit. She's the new director responsible for raising money for Caltech from foundations. She came from Claremont. I've talked to her a bit. She knows a little bit about the general lack of support for CAPSI, because we've been talking to her about how to get some. It's always interesting to talk to her about the place, because she's so curious about it and she doesn't know much about it. And her eyes always open wide because she's come from a place which is relatively civilized. [Laughter]

There are some very civilized people here. Dick Seligman is an extremely civilized person, highly civilized. And he survives. He survives like we survive, except that he doesn't do it on his own money. I mean, he has a staff and money. But I don't think he has to deal with the bad people very much. They need him more than he needs them, because he's the guy who makes all the money flow. He's a technician at collecting money. A really nice guy; he's on the CAPSI advisory committee. So that's a good sign. There are a few people.

COHEN: Well, you have done very good work.

PINE: Well, we're very pleased. And we like the idea that the world is pleased with us. I like the idea that I'm going to Bogotá because of the work I'm doing. And I went to Estonia last year. I'm going to Bogotá on Friday.

COHEN: What are you going to do there?

PINE: Talk to the dean of the college of engineering and his friends, and the teachers they're working with, because they are already emulating us.

COHEN: So you really got your message out. In spite of—

PINE: We didn't try; it just got out. Or it's accidentally gotten out. Georges [Charpak] has spread it to Colombia. And he did it, spread it to Colombia—I think he spread it; I may have. I don't know. The Estonians found out about it through an Estonian who works at UC Irvine.

COHEN: So you've got your program going in Estonia?

PINE: They're trying to change the country, and it's a very small country. It's like changing Delaware. It only has a million people. It's a very interesting place. We spent a week there—my friend Karen Worth and I. We ran an institute for elementary education for a week in the summer. And the two people pushing it from the university are chemists—one chemist and his wife, who's an education professor. There are programs under way in Sweden, which we've had no contact with and which are pushing along very well. So it has been interesting and enjoyable to travel, and help people, and enjoy very, very good people that you work with.

COHEN: So in some sense you've had two jobs here. Because you must have done your obligation to the institute all along—your teaching and research.

PINE: Yes. I think I have a light load in physics, but it's not zero. Whereas other people, who have big bureaucratic jobs, don't have anything.

COHEN: So, to sum it up. [Laughter] Are you going to be able to say, "I'm glad I've been here all these years"?

PINE: I'm going to sum it up by saying, "No." I guess I'm going to sum it up by saying that it's been a good career. I think I've had the chance to do what I wanted to do. I'm not sure I should be grateful for that, because the institute didn't have to do anything to do that.

COHEN: But you did have a place to go from. I mean, you did have a roof over your head.

PINE: That's true. But I could have had a roof over my head anyplace else, too.

COHEN: Do you think you would have been as successful?

PINE: Who knows? Maybe more! I mean, the opportunities for doing what we want to do are everywhere. They're doing it in Nantes; they're doing it in Bogotá. It's not that unique for a research scientist to become involved in public education now. It just happened to be one route that was open and I took it and stuck with it. But that doesn't mean that I had to be here in order for it to happen. Definitely not! Categorically not!

COHEN: OK. And you will continue?

PINE: Yes. I'm not discouraged by anything yet. We have no idea what the provost is going to say or do.

COHEN: I guess you don't much care.

PINE: Oh, we do care, because you pointed out something very important. Not only do we have a roof over our heads, but we have a place we come from. And the world thinks we come from Caltech. It's very useful.

COHEN: So there is that respect, that Caltech is esteemed in the world, it turns out.

PINE: I think so, yes. I mean, it has some impact. I'm not sure it would be any different if we came from Northwestern. We are scientists—that's important. Whether it's important to be scientists from here or not, I don't know. But if we wanted to form a nonprofit down the street—of which there are plenty—it would change the rules. So it's very important to be here, from the standpoint of having the charisma of Caltech as seen from outside.

COHEN: Now, how many Caltech students do you get to come and do a little work with you?

PINE: Undergraduates not at all. They're useless; they're too busy to do anything useful. SURF students a little bit, but that's it.

COHEN: I think I went to one of your presentations once, and there were certainly some kids working with you who were Caltech students, doing computer things.

PINE: Well, I did have a Caltech undergraduate who was a great computer programmer, who is now very rich. A nice man—Jay Obernolte. We're still in touch—we exchange Christmas cards. We do a lot of collaborating with postdocs and graduate students. Undergrads are basically not useful, except as SURF students.

COHEN: Who work in the summer?

PINE: Yes. Well, mostly they do things for themselves, not for us. We hope they'll learn about education. The graduate and postdoc people have been recruited by a person we used to work with, who doesn't anymore, who was herself a dropped-out postdoc at Caltech. She just did it by networking. So that's the way it works.

COHEN: So you will continue.

PINE: Yes, we think so. We're very successful in research. We have a huge grant written in curriculum development. And we don't expect to get it. But it's for a project that we've already raised $2 million for the hard way. And we'll probably just keep doing that. So we have two big projects. And we have a third project, which is the $6-million Center project that's now in the state of continuation at a low level, but it needs to do something earthshaking, and we don't know what. So we have three things going on, and that's another interesting thing. Plus the high school teacher training, which is now a pilot project—which could become a very big project. We might have to expand beyond Caltech.

COHEN: Do you ever think of becoming emeritus, and just doing this full time?

PINE: Oh, no, because I have more fun in my lab. I don't want to lose my lab, and my graduate students and my postdocs. No. I'm not attracted by the idea of a single job. I guess I've had three jobs for so long that I'm used to it. It doesn't take a huge amount of effort to do any of them because there are so many good people around. CAPSI, the research department, is so superb. I do work in it, and I created it, invented it. But I don't have a day-to-day responsibility. I don't have a desk at CAPSI. I did for a while, and it got to be a joke, because I was never using it. So that means that I can be a director of CAPSI without a desk. And I have an office I don't

use in physics. But I have an office in biology, which is part of my lab. So I've got everything going at once. It's very enjoyable, in kind of a stressful way. [Laughter]

[Tape ends]