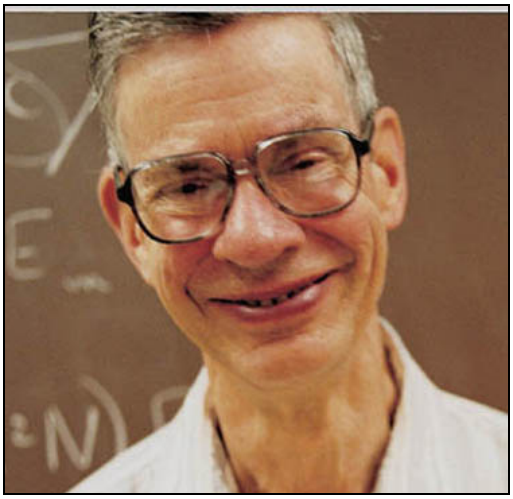

STEVEN C. FRAUTSCHI
(1933 –)

INTERVIEWED BY
SHIRLEY K. COHEN

June 17 and 20, 2003

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Physics

Abstract
An interview in two sessions with Steven C. Frautschi, professor of theoretical physics in the Division of Physics, Mathematics, and Astronomy. Dr. Frautschi discusses his family background and his early years in Madison, Wisconsin. He recalls matriculating, age 16, at Harvard, where his advisor was J. H. van Vleck; graduating in 1954 with an AB in physics, entering Stanford after a year spent bicycling around Europe on a Harvard traveling fellowship. At Stanford, under Sidney Drell, he and James Bjorken worked out the theory for an experiment being conducted by Nobel laureate Burton Richter. After receiving his PhD in 1958 he spent a postdoctoral year at Hideki Yukawa's Institute for Theoretical Physics in Kyoto, followed by a two-year postdoctoral stint at UC Berkeley, where he worked with Geoffrey Chew on Chew's "bootstrap" theory of strongly interacting particles and with Stanley Mandelstam on Regge poles. To Cornell in 1961. Invited to Caltech by Murray Gell-Mann; 1962 paper with Gell-Mann and Fred Zachariasen on Regge poles. Joins Caltech faculty as an assistant professor in the fall of 1962. Comments on the teaching of physics at Caltech in the early sixties; Gell-Mann and Richard P. Feynman; Gell-Mann's interest in linguistics.

Discusses his "statistical bootstrap" theory of the early 1970s for newly discovered strongly interacting particles. Discusses his 1982 paper in *Science* on the entropy of the observable universe. Discusses his work for *The Mechanical Universe* television project. Nine years as executive officer for physics, beginning in 1988; comments on the physics core curriculum and "take-home" labs. Comments on his work as master of student houses beginning in 1997, on the gradual "professionalization" of student affairs, and on his encouragement of the performing arts at Caltech.

Administrative information

Access

The interview is unrestricted.

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Steven Frautschi talks to Caltech students. Caltech Annual Report 2002-2003.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH STEVEN C. FRAUTSCHI

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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Pasadena, California

Session 1	June 17, 2003
Session 2	June 20, 2003

Begin Tape 1, Side 1

COHEN: I'd like to ask you about your growing up—about your parents.

FRAUTSCHI: Well, I grew up in Madison, Wisconsin. No one in my family had ever been a scientist. My father ran a family furniture business, but he actually had a master's degree in history from the University of Wisconsin and several university professors were among his closest friends, so it was really an intellectual atmosphere, although there were no scientists in the family tree.

COHEN: And Madison is a university town.

FRAUTSCHI: Yes.

COHEN: How did your father get to Madison?

FRAUTSCHI: It all started with my great grandfather emigrating from Switzerland. It was just a straight example of the Protestant ethic. He was the wrong son to inherit the family farm, so he learned a trade, which was cabinetmaking. It was very difficult at that time—the late 1860s—to become an independent businessman in Switzerland or France, so he emigrated to America. He worked for two years to build up his patrimony, and then he was able to buy a small shop and open up a furniture store. They also made caskets. The family lived upstairs, over the shop. So that was founded in 1869, and that was the business my father continued.

COHEN: And your mother?

FRAUTSCHI: She was from La Crosse, Wisconsin. She was a homemaker. One story is that in 1931 or so, before she was married, she was working as the secretary to a bank president in Madison, and she discovered that one of the other banks was probably going to go out of business, and it was a bank that held deposits for some of her relatives. And she decided that as a matter of principle she shouldn't pass this information on to these people under the table, and somehow they later found out that she knew and didn't tell them, and they were very displeased with her. That was a striking example of her principles.

My parents, although they didn't have any special scientific knowledge, saw my interest at an early age and, particularly, gave me a series called *The Stars for Sam*—which, obviously, was about astronomy—and I remember reading that. So they encouraged me, and, of course, I did meet some scientists at the university while I was growing up.

I went to a good high school, on the side of town where many professors lived. There was no outstanding advanced-placement high school course or anything like that; it was just a pretty good high school.

COHEN: Obviously you were a very good student and you took all the science available.

FRAUTSCHI: Yes.

COHEN: And then where did you go to university?

FRAUTSCHI: I started at Harvard at age sixteen—so I had skipped one year.

COHEN: Now, that's interesting. What made you think of Harvard? Did somebody encourage you to do that?

FRAUTSCHI: Well, it was of course a famous university. Nothing in particular…. I mean, it was nice, while I was there, that my father did know a couple of the professors. One of them was Clyde Kluckhohn, the anthropologist who was an expert on the Navajo Indians. My dad, as a university student, had spent a whole summer camping in the Navajo country with Kluckhohn

and a couple of other friends; that was the most romantic adventure of his life, really. My dad had also been in college with John King Fairbanks, the expert on China. I later took his course, History and Civilization of the Far East—or, as we students called it, Rice Paddies. The teachers were Fairbanks and [Edwin O.] Reischauer, who later was our ambassador to Japan.

My advisor at Harvard was John Hasbrouck van Vleck, who had grown up in Madison—in fact, there's a Van Vleck Hall on the Wisconsin campus now, named after his father, who was a math professor there. So there were some contacts, but I went to Harvard just because of its general reputation.

COHEN: Sixteen is quite young. Did you feel that when you went there?

FRAUTSCHI: I was very happy there. I was six-feet-two, and I was reasonably good at sports, so I played in a lot of intramural sports and was perfectly happy, getting along with people.

As I said, I had come from a good public high school, but a lot of the other students were better prepared, so the first year I had to work very hard just to keep my head above water. After that—I had a lot of extra energy, so in addition to my regular physics and math courses I used to audit a couple of courses a term, things that I didn't have to take an exam in or write a final paper for, and which tended to be understandable at first sight, unlike a physics course. So, for example, the fine-arts survey of the history of art, even the basic geology course, with all kinds of photographs—

COHEN: Descriptive courses.

FRAUTSCHI: Descriptive courses, yes. The basic music history and theory course, and so on.

On the activities side—I wasn't remarkably active, but in my last year I was the athletics secretary in Kirkland House, which meant organizing a lot of sports.

COHEN: It sounds like those were happy years.

FRAUTSCHI: I had a wonderful time at Harvard. To me it was like a large cookie jar full of wonderful goodies to dip into. You know, much more than Caltech students, the students at

Harvard pride themselves on the art of conversation, and I wasn't especially good at that, but I very much appreciated it and enjoyed being a part of it.

COHEN: Better than the food on the table.

FRAUTSCHI: The food was lousy. [Laughter]

COHEN: OK. So you spent your four years there and finished with a degree in physics.

FRAUTSCHI: Yes, an AB. [1954].

COHEN: Was there anybody that really influenced you there, particularly?

FRAUTSCHI: No single remarkable individual; there were a lot of good people, of course. And Van Vleck, my advisor, was very kind to me. He was not a particularly talented teacher, but he was very kind to me. But I wouldn't single out any particular individual—it was the whole thing.

COHEN: And when did it become obvious to you that you would go on to graduate school?

FRAUTSCHI: Well, I'd started in physics because I liked science in general, and I also recognized that the part that was combined strongly with math came most naturally to me. I had a rough patch freshman year, in my first physics course, with calculus. Since I hadn't had calculus in high school, the physics started the second semester. I think I got a grade of twenty percent on the second quiz and sixty-three percent on the midterm, and things were looking bad. The problem was combining the math with the physics. So I decided I would work very hard and see if I could overcome this—otherwise, I would have to do something else. So I did all of the problems in the back of the book; in those days, books didn't have as many problems as they do now. And that actually worked for me. So by the end of the course, I was doing very well. I never had as hard a time later on, in any later courses.

COHEN: No, once you got yourself calculus, you were fine.

FRAUTSCHI: Well, it's not just calculus. There's a general art of putting the math together with the physics which, to me, is the central point of freshman physics—more than learning that $F = ma$ a little better. So from there on, it really went very well. And yes, I thought I had to go on to graduate school.

COHEN: So, did you know your choices? Had you traveled around a bit? How did it go in those days?

FRAUTSCHI: Oh, in those days—before I went to Harvard, the previous summer, my family took a family vacation and we swung through a number of the Eastern college campuses and got at least a superficial look at them. There wasn't the sort of mass visitation that everybody does now. For graduate school, there was *no* such tradition of visitations, at that time. I simply talked to Professor van Vleck, my advisor. He pointed out, as usual, that I ought to try going somewhere else. I decided I'd like to try the West Coast, to see a different part of the country—I'd never been there. That left three possibilities: Berkeley, Stanford, Caltech. I did not apply to Caltech, because there were no women there then, and I had already been to a men's college for four years. So for some reason—I don't remember what it was—I applied just to Stanford. I applied just to the one school, which by today's lights would be an incredible risk, and I don't know why I picked Stanford.

When I mentioned this to my dad, his first reaction was, "Well, Stanford is a society school," which in the early fifties was a common opinion. Then he tried it out on one or two physicists he knew, and they told him that Stanford had in fact a very strong physics faculty. [Laughter] And that put his mind at rest.

Before arriving at Stanford, I had a year on a Harvard traveling fellowship, which I believe was called the Sheldon Fellowship. There were a total of four of these traveling fellowships that Harvard granted—three Sheldons and one Shaw, I believe. You had to be among the top twenty people academically to be eligible for that, and then there was an interview process with a very distinguished small committee of professors. I remember Archibald MacLeish from humanities; a prominent math professor—[David V.] Widder—and then there was some social scientist, whose name I can't remember. Each of us had twenty or thirty

minutes with that committee. Some of the other candidates, of course, were great storytellers and spectacular personalities, so how was I going to match that competition?

Now, this was the spring of ’54, and as soon as I got in there, Professor MacLeish said, “I see you’re from Wisconsin. What do people in Wisconsin think of Senator [Joseph R.] McCarthy?” And I decided not to give a generic answer but to tell my father’s story. My dad had been in the furniture business twenty-five years, and at the annual state banquet of the dealers he was going to get a plaque, and they invited him to sit at the head table. He found out ahead of time that the main speaker—also sitting at the head table—would be the junior senator from Wisconsin. So he wrote a letter thanking them for the award and respectfully declining to sit at the same table with Senator McCarthy. Although my father had not intended it, this was leaked to the newspapers, so more than 200 communications of all sorts descended upon him, the majority favorable but some very nasty. For example, there was a letter to the newspaper: “We note that neither Mr. Frautschi nor his brother served in the army during World War II, and now Mr. Frautschi refuses to have anything to do with our patriotic senator.” And so on, and so on. My dad declined numerous invitations to further go public with his opinions on Senator McCarthy; he did not wish to endanger the furniture business or the employees. So that was the experience that just a normal businessman had, as opposed to a screenwriter or a college professor, with the spirit of the times. I managed to take up probably half the interview with that story and answering further questions about it. So that gave me some distinction—but that was a stroke of luck, of course.

So I had a year of subsidized travel in Europe, where I went to a great many operas, among other things. I spent several months in Munich and in Paris.

COHEN: Was this a summertime thing?

FRAUTSCHI: No, it was a full year. This was after graduation.

COHEN: Somebody told me that on these fellowships there was always a list of people that you could stay with when you went somewhere.

FRAUTSCHI: We were not provided with any such list.

COHEN: So did you set up your own itinerary?

FRAUTSCHI: Oh, yes, very much so.

COHEN: And you went by yourself?

FRAUTSCHI: I went by myself. Over the winter, I was getting sick of seeing the gray skies every day in Northern Europe. So in the early spring I went to the Near East—Egypt, Jordan, Syria, and eventually Israel; that was my first experience with a non-Western culture.

COHEN: Your first few months you were where—in Northern Europe?

FRAUTSCHI: Well, I biked around the British Isles in the summer. Then I went down to Munich. There was a friend of my dad's who was a visiting professor at Munich that year. But mostly I was on my own.

COHEN: So in each place you had to find your own place to stay and decide what you wanted to do in those places.

FRAUTSCHI: Yes, sure.

COHEN: Well, that sounds great, but it also sounds a little lonesome.

FRAUTSCHI: Potentially. In fact, some Arabs I met—that was the thing that struck them. They couldn't imagine being away from home and just traveling around independently. Well, of course, if they were traveling in the West—at least, in those days—they would have been more isolated than I was traveling around anywhere else, where, of course, there were other Americans and there were English.

Anyway, I had that year and I got to see a lot of places, and then I started Stanford. I had a very painful first two months just getting the connections of the math again. You know, a year's lay-off, you lose a lot.

COHEN: So you wouldn't recommend it.

FRAUTSCHI: Oh, sure, I would absolutely recommend it. But to get back—when I went to graduate school at Stanford, it was clear that I was going to be more comfortable with theory than experiment. I worked with Professor Sidney Drell, and he put me on a project with James Bjorken, another student. We worked out the theory for an experiment that Burton Richter, the future Nobel Prize winner, was conducting. It was nice, because, first of all, it gave us practice in carrying out the standard techniques, and secondly, it was a local experiment. Nobody else thousands of miles away was likely to scoop us on the details, because while part of the calculation was generic, parts of it were tied to the experiment as it was going to be conducted. So we did not have to work under severe pressure that someone might scoop us.

It was a nice atmosphere there at Stanford. Particularly, once every week or two, the visiting high-energy physics seminar speakers would speak in the evening at Professor Wolfgang Panofsky's house, ten miles off campus. It was just an old beat-up house, but it was a very pleasant atmosphere.

COHEN: So you were there with all the greats of physics.

FRAUTSCHI: Yes.

COHEN: And you immediately started this project, which became your thesis?

FRAUTSCHI: Well, it was the backbone of my thesis. When I'd been there two and a half years, Drell said, "Well, now that we've pretty well wound up this calculation, you should do something more independent to fill out your thesis. And he gave me kind of a journal-review assignment. He gave me a couple of papers to read, and I was supposed to report at the journal club. So I read these two papers. One of them, I decided, was basically wrong-headed. So I reported on that for about three minutes, and he said, "OK, we don't want to hear about that anymore," and he agreed with me.

But with the other one, I managed to get to the essence of what it was, and he became interested enough so that we immediately wrote up a paper on it ["PC Conservation in Strong Interactions"], extending what I'd said. Well, the paper never got published, because after we

sent out the preprint, Sid immediately heard from Murray Gell-Mann, then a professor here, who pointed out that he had already done that. Nevertheless Drell decided that that would be enough for my doctorate. So I unexpectedly finished in three years [PhD 1958], which was fast even then.

I had lined up an extension of my NSF [National Science Foundation] fellowship for a year, so Drell asked me where I would like to go, and, based partly on this course I'd had at Harvard on the Far East—Japan and China—and partly on a course I'd audited at Stanford on Japanese literature, I said I'd like to spend a year in Japan. Now the only question was where. Shortly after that, Professor [Yoichiro] Nambu, from Chicago, came to give one of those seminars at Panofsky's house.

COHEN: Oh, it wasn't just a party afterward. It was the whole seminar.

FRAUTSCHI: It was a seminar, and then refreshments served by Mrs. Panofsky.

So Drell introduced me to Nambu and asked him about schools in Japan. Nambu drew up on the board two columns, one of which was schools where they had a weekly Communist cell meeting. In other words, they were under the influence of Professor [Shoichi] Sakata. And the other was where there were probably socialists, but they didn't have a weekly Communist cell meeting or any particular party line.

COHEN: General MacArthur let them get away with that? I mean, this was many years later, but it must have started sometime.

FRAUTSCHI: Oh, sure. Now, these places with the Communist cell meetings, they would have been very nice to me too, but it sounded like a bit of a hassle. And furthermore, I knew that Kyoto was the only large city that hadn't been bombed and that had a great deal of ancient history in it—gardens and temples—and Professor [Hideki] Yukawa's Institute for Theoretical Physics in Kyoto was in the non-Communist column.

COHEN: OK. Do you think your early acquaintance with Senator McCarthy had something to do with your thinking?

FRAUTSCHI: No. So Drell wrote to Yukawa that he had a student who was doing well and was interested in coming, and had his own financing. The reply came back in seven or eight days. [Laughter] He answered immediately, and the hard part, it turns out, was getting the visa in time. So I went off and spent my first postdoctoral year in Japan.

COHEN: Did you know the Japanese language at that time?

FRAUTSCHI: I did not know it then. I don't know it now. Of course, I know a few words. But no, no; I never learned it well. But I had a wonderful time. Further, while some people had told me, "Well, you know, it's dangerous to go overseas, because eventually you'll want to get a job in America," Drell was able to get me a postdoc for the following two years at UC Berkeley with Geoffrey Chew, who then had a very active program, so it didn't do me any professional harm whatsoever.

So after that, I came back to Berkeley and spent two years there, and that's where I was able to get into a strong line of research that wasn't duplicated readily all over the world, working with Geoffrey Chew.

COHEN: What sort of thing did you do?

FRAUTSCHI: Well, at that time, on the experimental side, all these new particles were being discovered in high-energy physics at the accelerators. On the theory side, the quantum field theory that existed then worked fine for quantum electrodynamics—[Richard P.] Feynman's subject—but had failed to illuminate very much all the new strongly interactive particles that were discovered.

So Chew had a different approach, which he called the bootstrap, where exchanges of these strongly interacting particles would provide the attractive forces that would enable the particles to hold together. The whole thing was self-consistent—the particles and the forces. And Chew was a wonderful group leader. He had a substantial group of graduate students and postdocs, and he had weekly meetings where we learned about what everybody was doing.

I was placed in an office with Stanley Mandelstam, who was an older postdoc. Stanley had made some technical breakthroughs. So, with the help of Stanley, I got involved in something called Regge poles. There was a paradox: If you took Yukawa's idea that the forces

were due to exchanges of particles, and started exchanging the new particles that were being discovered at that time that had spins greater than one, then the forces would grow without limit at high energy. That was hard to understand intuitively, because when two particles collide, if you describe what happens with a cross section—which is essentially an area over which the particles interact—a cross section growing without limit at high energy implies a larger and larger interaction area. But the particle being exchanged had a large mass, and, stemming from Yukawa's theories, a large mass exchange inplied a very small target size, because the large mass couldn't travel virtually very far by the rules of quantm mechanics. So there was almost a paradox there.

Mandelstam pointed us to the solution of this paradox, which was that the high-spin particles shouldn't be treated as isolated individuals but as parts of families, and you should organize the calculation around the exchange of the whole family—the spin-one member, the spin-three, the spin-five, and so on. And then in the relevant kinematic region, the cross sections would come out finite and intuition would be satisfied, and theorems that were extensions of the intuition would be satisfied, and it would all work.

So Geoff Chew and I proposed this, and that's what eventually got me my job at Caltech. But the path was a bit round-about. After two years at Berkeley, I had gone job hunting and I had offers from several schools. I chose Cornell, partly because—well, they were very nice to me. Hans Bethe was very nice to me.

COHEN: What year would this be now?

FRAUTSCHI: This is 1961. I got my PhD in 1958, and in 1961 I arrived at Cornell. Now, just before I went to Cornell, Murray Gell-Mann made a visit to the Rad Lab [Radiation Laboratory] at Berkeley, while I was still there. I didn't normally go up to the Rad Lab; I was normally down at the Berkeley campus. But one evening at home I get a phone call from one of the other postdocs. He says, "You must come to the Rad Lab tomorrow, because Gell-Mann is asking questions, and we can't answer them." They were, in fact, kind of intimidated.

So I went up the next day, and Murray shows up. He had various issues, but he particularly had one issue. It was something that Murph [Marvin L.] Goldberger—the future president of Caltech [1978-1987]—had said in a talk and that Murray was confused about. It had

to do with whether, in this particle family, we should count spin one, three, five, jumping two every time, or whether we should count every one—zero, one, two, three, four, and so on. It was a technical issue involving what's called an exchange potential. Anyway, I happened to have thought about that issue carefully just two weeks before and thought out exactly what to do about it. So I was actually able to tell Murray how to handle this problem, which is a rare event. [Laughter]

So, you know—on the one hand, that's luck. Of course, on the other hand, luck favors the well prepared. So Murray invited me to come and visit Caltech and collaborate on some work at the earliest opportunity, which was going to be along about Thanksgiving vacation. So over Thanksgiving vacation, I left Cornell for a short time. That was my first trip to Pasadena.

COHEN: You mean all the time you were at Stanford and Berkeley, you never came to Pasadena?

FRAUTSCHI: That's right. That led to a paper with Murray Gell-Mann and Fred Zachariasen on these same Regge poles [S. Frautschi, M. Gell-Mann, and F. Zachariasen, "Experimental Consequences of the Hypothesis of Regge Poles," *Phys. Rev.* 126, 2204 (1962)]. So that led to the job offer to come to Caltech.

COHEN: What did you think of Pasadena on your first visit down here?

FRAUTSCHI: Well, Murray on the weekend drove me out to the beach, and I think we visited Nina Byers, who had an apartment in Malibu at that time. Anyway, it was new to me. I didn't like Pasadena as well as the Bay Area, frankly, but the possibility of coming to Caltech, which at that time had Feynman and Gell-Mann active, and wonderful students also, was very attractive. I rejected a couple of other offers and left Cornell and jumped at the chance to come to Pasadena, and I've never regretted it since.

COHEN: Now, you weren't at Cornell very long.

FRAUTSCHI: I was there just a year.

COHEN: Working with Hans Bethe?

FRAUTSCHI: No, not really. By then, he was in his fifties, and he wasn't working on particle physics. He would come over and sit with me at the lunch hour. And like myself, he was a person who ate regularly substantial lunches and liked to talk at lunch. He was very nice to me, but I wasn't really working with him.

So in the fall of '62, I came to Caltech. Almost immediately I had a couple of prospective students walk in the door. And a lot of my subsequent liking of Caltech was certainly generated by the fact that the students I had started to work with at Cornell were good but not superlative, whereas the first couple of people who walked in the door at Caltech turned out to be superlative—Roger Dashen in particular. I was fortunately able to suggest a couple of problems, and Roger, particularly, took the ball and ran with it magnificently. So that was part of the happy atmosphere at Caltech right from the beginning.

COHEN: And you were teaching by then?

FRAUTSCHI: Yes. Carl Anderson was the chair of the physics division at that time. He said he would start me on a course in my own field. So I got to give Physics 231, which was the general course on high-energy physics—not the most technical parts of the theory but the phenomenology together with the theory directly related to experiment. I certainly hadn't taught that before, but I had a good time putting together a course that was quite successful—particularly not getting any more technical than necessary.

COHEN: Could you give some idea of the experiments, the theory, the work that was going on at that time at Caltech, with Gell-Mann, with Feynman. It must have been extremely active.

FRAUTSCHI: It was extremely active. Gell-Mann had recently come out with the Eightfold Way, or SU3. Among the visitors was Yuval Ne'eman with his version of SU3. George Zweig was just coming out with his physical formulation of the quarks, which Murray certainly had entertained also as more formal mathematical objects. So there was a lot going on.

COHEN: It was really a heady atmosphere, then.

FRAUTSCHI: Oh, yes, it was wonderful. And shortly after I arrived, Murray essentially stopped teaching conventional academic courses and started teaching his seminar… [Tape ends]

Begin Tape 1, Side 2

FRAUTSCHI: …which was a weekly two-hour report on whatever was new in particle physics. He would digest all the new developments in a very understandable way and present them to us. It was for students, but also postdocs and some of the profs. He had taught conventional courses before that, but by this time he was independent enough, or however you want to express it, so that he started doing that. It was really wonderful for all of us to have somebody digesting all these confusing and often badly stated developments and capturing the essence of them.

So right through the sixties, he was doing that, covering the entire subject. As the seventies wore on and into the eighties, he tended to narrow the focus progressively. There came a time when he was pretty much concentrated on supersymmetry and supergravity, for example. But at first, he was covering the entire field, and that must have been close to unique.

COHEN: What else was going on at this time?

FRAUTSCHI: Well, the lead experiments in the field—Berkeley had had the lead in covering new particles, but when the accelerator energy jumped from 6 billion volts to 30 billion electron volts, that machine was not constructed at Berkeley. From then on, you had national-scale labs—at CERN [European Organisation for Nuclear Research] in Geneva and at Brookhaven, in New York—so the experimental focus shifted there. In some of the early work at these new accelerators, for the first time they were really able to test out our suggestion—referring back to Chew at Berkeley—of the Regge pole exchanges. What the forward scattering peak would look like. We had predicted that it would shrink in a certain way as the energy rose, and some experiments in the mid-sixties verified that, and that made these Regge poles fashionable in the field, for a couple of years at least. Plus, more and more particles of higher spin, fitting onto some of our Regge families, were discovered in the sixties at Berkeley and places like that.

COHEN: Now, you were also having to teach at this time. With whom were you most directly working here, when you first came?

FRAUTSCHI: I worked more with the graduate students than with the other profs—after that first paper with Gell-Mann and Zachariasen, it was more with the students. Carl Anderson had me alternating: The first year it was a graduate course in high-energy phenomenology; the second year I think it was junior quantum mechanics—which at that time consisted of the survivors from the two-year Feynman teaching experiment. [Laughter] It turned out that we had to start quantum mechanics over again, even though there had been a term of quantum mechanics in the second-year core that Feynman taught. But we had to start the conventional Schrödinger equation approach from the beginning, because Feynman had mainly described Heisenberg's matrix-mechanics approach, and many of the students had been left without a firm grasp of either method. Some of the best students, of course, understood everything, but for the average student it hadn't stuck very well.

COHEN: Nobody said that maybe Feynman shouldn't teach the introductory physics course?

FRAUTSCHI: Well, he just taught it for two years, and then this industrious team of high-powered people, including Matt Sands and Bob Leighton and Gerry Neugebauer, helped turn his lectures into the textbook. Caltech continued to use the book [*Feynman Lectures on Physics,* Addison-Wesley] for a number of years. But Feynman taught the course for just those two years. He actually said he was exhausted afterward; he'd really thrown himself into it.

Remarkably, at the same time he was finishing up the introductory physics lectures, Feynman also taught a graduate course on his thoughts about the quantum theory of gravity. It was really almost the first serious work on that subject, which is by now a huge industry. I attended all those lectures.

COHEN: So that was common—for professors to attend Feynman's lectures?

FRAUTSCHI: Well, when it was something new like that. This was not his normal course—because Feynman did teach normal courses, whereas Murray was teaching this seminar on what the latest research was.

COHEN: But he continued to teach regular courses, like everybody else?

FRAUTSCHI: Feynman did, yes. Plus his famous sessions with the undergrads, where he would entertain any question whatsoever.

COHEN: Was that a separate course?

FRAUTSCHI: It was just a weekly seminar, and it really was for undergrads—professors and other older people were banished.

COHEN: So he really gave a great deal of himself, as far as teaching was concerned.

FRAUTSCHI: Oh, enormously! Yes. In fact, the students regarded him as their patron saint at Caltech, [and the reputation] is quite deserved.

COHEN: You said he didn't have many graduate students.

FRAUTSCHI: That was another unusual feature of Feynman. Well, as he put it, if the subject was interesting, then he couldn't refrain from doing it himself immediately, he couldn't wait. And he certainly was not interested in founding a school, or anything like that.

COHEN: Was the Kellogg Lab established at this time?

FRAUTSCHI: Oh, yes. It was very active, with Willy [William A.] Fowler. One of the postdocs that Willy brought in was John Bahcall. John was one of my close friends while he was here. Eventually—a little later, in the late sixties—John got me involved in writing a couple of papers on neutrinos from the sun, which I'm very happy to have participated in. Actually, even when I first came to Caltech, another of the attractions at Caltech for me was all the astronomy here, because that had always been a side interest of mine, and Caltech was a place where that side interest would be vividly present.

STEVEN C. FRAUTSCHI

Session 2

June 20, 2003

Begin Tape 2, Side 1

FRAUTSCHI: This is an illustration of how lively, or exciting, things seemed with Murray Gell-Mann and his group. I don't have the date, but maybe it's the late sixties. One of his hobbies was linguistics—particularly family relationships among languages—which sort of paralleled his interest in classifying elementary particles, which also have family relationships, although of course the map is different. Anyway, there was a world authority on language families at Stanford named Joseph Greenberg, who was famous for unifying the African languages into a couple of superfamilies, and who then, with more controversy, had recently unified most of the American Indian languages. And Murray had the idea of inviting Greenberg down to give our high-energy physics group a couple of talks.

COHEN: Although he was a linguist, not a physicist.

FRAUTSCHI: [Laughter] Greenberg was a linguist. So, in preparation for this, Murray gave us several one-hour seminars on the foundations of linguistics that Greenberg would be using.

COHEN: Who was invited to participate?

FRAUTSCHI: It was basically all the high-energy theorists—and also mentioned to the experimentalists. But it wasn't institute-wide, or even physics-department-wide. It was just kind of Murray's circle. So Murray gave us several prefatory lectures, with lots of examples. For example, *pater* in Latin becomes *Vater* somewhere back in German history. But that wasn't just one word; it was a sound shift that affected lots of other words, and he gave us tons of examples. One example I remember was, he took Japanese, where there's a standard way of counting from 1 to 10 that derived from Chinese. But then the original Altaic way of counting is 1, 2, 3, 4. And then the words kind of double up: 5, 6, 7, 8. In other words, they were based on the long fingers, rather than on ten fingers—they discriminated against the thumb. So he took the 1

through 4 in the old Altaic system and then traced the sound shifts across Eurasia to Mongolian and eventually to Turkish—well, you end up with something that sounds completely different in Turkish, as a result of a small, finite, discrete number of shifts. So he had all these examples, and he also went into glottal chronology, which is a rough index of how fast these sound shifts occur and therefore how long it takes for language relationships to get hopelessly garbled—which is thousands of years.

OK, so then we were all ready for Greenberg, and he came and gave two wonderful talks, and we had him out to a nice dinner, and so on. That was just fun. And of course, it was just Murray's hobby.

COHEN: But he wanted to get the whole high-energy physics group involved. Now, how about your own research? You wanted to talk about that and the people you worked with here.

FRAUTSCHI: Well, we previously talked about the Regge poles. Then in the early seventies—'71, '72—I went back to the bootstrap idea that I had pursued as a postdoc at Berkeley. By that time, a very large zoo of hadrons, strongly interacting particles, had been discovered, so I had the idea that a statistical treatment might be of some value. I developed what I called the statistical bootstrap, where they were all made of each other—the heavier ones were made of lighter ones. So that counting generated a rapid exponential growth of particle species, as the mass increased. I was now picking up on the idea of a German at CERN whose name was Rolf Hagedorn. I took his statistical notions and sharpened them and wrote some papers that fit various experimental facts, actually, as far as they were known at the time. And I'm particularly proud of those papers, because they weren't collaborations—I did them all by myself.

Both the exponential growth in particle species that came out of the statistical bootstrap and the features of the particle spectrum that had emerged earlier in Regge poles are still very much alive. In particular, our treatment of Regge poles had an equal-spacing feature between masses of successively higher spin—actually, the rule was that the spin went as the mass squared, if you followed the family up to higher masses. Nowadays, both of those developments—exponential growth in particle species and the equal spacing of mass squared in the spin family—are viewed as outgrowths of string theory. But at the time I had no idea I was working on string theory.

COHEN: Was string theory even mentioned at that time?

FRAUTSCHI: No. It was very nascent; it hadn't really developed yet. For example, the equal-spacing rule of higher spin— Well, let's see: What's familiar are things like piano strings, violin strings, where the vibrations on a string of a particular length that's clamped down at both ends have a particular set of equally spaced frequencies. By twanging the string, I get a fundamental tone and various overtones, whose frequencies are all integer multiples of the lowest tone. Now, for relativistic particles we had a similar equal-spacing rule, which turned out to be most easily explained in terms of counting states on a string—vibrations on a string. This is not the present very-high-energy strings that people talk about but what's called the old-fashioned strings that have quarks at each end and a gluon string in between. So Geoffrey Chew and I had stumbled upon evidence for strings, although we thought we were working on an entirely different problem.

Originally, we had just drawn a straight line between two points, because two points were all we had for the data. And then as more data occurred, the straight line continued through the next particle discovered and through the Yukawa exchanges in a different kinematic region. So the straight lines we'd originally drawn for our Regge particles turned out to be a pervasive feature, and eventually that came to be regarded as very strong evidence for strings. That was a remarkable turn of events. So the Regge poles and statistical bootstrap were perhaps the most famous pieces of research I worked on.

In the early eighties, around '82, I wrote a paper on a different subject—namely, the entropy of the observable universe. [Frautschi, S., "Entropy in an Expanding Universe," *Science* 217 (4560), 593-99 (1982)] I'd been influenced by Stephen Hawking, who had spent a whole year on campus, in the early seventies—it must have been '74 to '75. That was the first time we had any handicapped access visible on campus; it was for Stephen Hawking.

COHEN: Was he still able to speak at that time?

FRAUTSCHI: With great difficulty. It was actually easier to understand him on the telephone, because he had a special telephone that amplified some. His British graduate student came along and could understand almost everything he said; he spent part of the time interpreting. But yes, Stephen could still speak at that time.

Stephen had done famous work, which was new at the time, on the entropy of black holes. And one of the things I made use of in my accounting of the entropy in the universe was that black holes were the dominant source of entropy if Stephen was right. The more philosophical result of this was that in school I'd heard about the heat death of the universe—that eventually the universe would grind to a boring halt. But what I realized with my accounting, and was able to show, was that in the expanding universe, that never happens. In fact, instead of approaching a dead equilibrium, we fall farther and farther behind equilibrium as time goes on and as different parts of the universe get farther and farther away from each other.

COHEN: So the expanding universe goes on forever, is what you're saying?

FRAUTSCHI: Well, this was in either the open or the barely open universe picture. Yes, entropy would keep on growing, but it would also keep falling farther and farther behind equilibrium, or maximum growth of entropy. So the heat death just doesn't occur in the expanding universe.

I wrote one paper on that. This idea was, of course, sort of interdisciplinary, and it had a final section where I tried to model how intelligent life might try to keep going into the indefinite future, so it presented some difficulties for the referee at *Science*. I sent it to *Science* magazine—not a normal venue for me—because this paper was of general interest. Finally, they sent it to Freeman Dyson, who was one of the few people who had braved the stigma of trying to think scientifically about the future instead of writing science fiction, and he recommended it for publication and even sent me a nice letter, saying he didn't agree with everything I'd said but that he felt this subject really deserved an airing, even if it was difficult for the editors. [Laughter]

COHEN: To understand?

FRAUTSCHI: Well, because it was potentially controversial and so on. So that got published in *Science* magazine, and it's one of my papers that has been mentioned several times subsequently, in *Scientific American*, which is an indication that you did something that wasn't just narrow but had some broad interest.

COHEN: So you were pleased with that paper. Could we just backtrack a bit? You were mainly working by yourself during this time, is that correct?

FRAUTSCHI: Well, I worked with a string of graduate students—I'm not sure how many. Some of them were shared; it was twelve or fifteen graduate students.

COHEN: I wasn't particularly talking about graduate students, I was talking about colleagues.

FRAUTSCHI: No. It was very important to have the group there, but primarily I was working by myself or in conjunction with grad students. By the mid-eighties, I was largely running out of fresh research ideas, and a greater part of my attention shifted to doing things involving students.

COHEN: Is that when you took up the first of your formal jobs with students?

FRAUTSCHI: Yes. Well, first I had a stint working on *The Mechanical Universe* television project. David Goodstein [professor of physics and applied physics] was the leader, and he recruited me, and of course Tom Apostol [professor of mathematics, emeritus] also got involved, so that we wouldn't treat the math too shabbily. So that introduced me to freshman and sophomore physics, which I really hadn't paid any attention to before, and since then I've frequently—not always, but frequently—taught, particularly freshmen physics. David will be much better at telling you the full story of *The Mechanical Universe*. It was an exciting period, getting to know people in a different culture.

COHEN: Did you actually write scripts, or did you just edit scripts?

FRAUTSCHI: Oh, I edited scripts. David did quite a bit of script-writing, but they kept me mostly away from that. I didn't have the right perspective to do it effectively. There were all kinds of things to do; it was an enormous project.

COHEN: Would you say it took up, except for your teaching, all the rest of your time?

FRAUTSCHI: For about two years, yes. I'm very happy I did that. But then in 1988, I became the executive officer for physics, which I continued to do for nine years. The executive officer deals with students in many ways. Before then, I'd been the physics option representative for many years, taking care of physics grad student affairs, so I'd had some of that kind of experience, but now I was thrown in with undergrads for the first time.

COHEN: What is an option representative? You looked to see who could be a graduate student, or once they were here, you looked out for their welfare?

FRAUTSCHI: Followed their welfare, both as far as TA [teaching assistant] assignments and financial support and oral exams, thesis exams, and so on. But the executive officer for each option has broader responsibilities. The key responsibility for him is assigning who teaches what, and that's really a short-sheet problem, because you're never going to have enough popular teachers to cover all the courses.

COHEN: Or even enough people who want to teach, perhaps. [Laughter]

FRAUTSCHI: Well, yes, it requires some of the same abilities that negotiating in the marketplace does. You're trying to make each professor happy—not get them out of teaching, but make them relatively happy with their assignment—while at the same time covering all the required stuff, while at the same time getting a critical mass of effective professors into the bigger classes. And, of course, there's no way to satisfy everybody. It's an art form, and it requires, certainly, negotiations with the profs. So that was, of course, a core duty there.

One of the achievements of my nine years as executive officer was this: Our senior lab had become stuffy; it hadn't changed much since the early sixties. That's in contrast to the sophomore lab, which was run by Don Skelton, a longtime staff member. He kept incrementally evolving it, and it was a pretty effective set of courses.

COHEN: He was in charge of the lab even though he wasn't a professor?

FRAUTSCHI: There was a professor nominally in charge, and sometimes the professor took a great interest, but other times the professor only went through the motions. Skelton was a constant, who was there all the time and kept a very healthy system going.

Senior lab hadn't been so lucky. So we searched around, to get it updated. Nobody was enthusiastic about taking the same experiments and updating them, but we did have good luck in finding Ken [Kenneth G.] Libbrecht [professor of physics], who had the idea of starting another lab course, pointed in a different direction, which was lasers. Ken developed what's been a very successful one-term set of experiments involving lasers, which has been very popular.

COHEN: Were they doing anything with that in electrical engineering?

FRAUTSCHI: Well, our students had wanted more and more to take their labs over in applied physics rather than this stuffy old senior physics lab, and basically Ken's new course brought them back. Applied physics had a mix of things, including a little bit on lasers. Ken's course was a more coherent, full treatment of that subject. We also got Michael Roukes [professor of physics] started on trying a new condensed-matter lab, but that never got finished. But we had the one lab with Ken Libbrecht and the lasers, which invigorated our senior lab.

I would say my biggest failure was that we had nothing in our core curriculum, Physics 1 and 2, on chaos, and chaos by that time had become a very interesting subject. It's important. It's philosophically interesting. Parts of it can be taught at a very nonmathematical level and don't require much background, but it wasn't encountered at Caltech, except in a few very advanced courses in various specialities. I would have liked to get it into the core physics curriculum, at least a week or two of it, to give the students that extra perspective—but the core was already crammed full of more traditional subjects. Plus the whole physics profession had the same difficulty: that is, there was no text—and there still is no text—that works chaos in at the elementary level in a nice way. Nevertheless, we could have made our own material for two weeks, so in the end we were defeated by the time pressure. And then eventually our set of core classes got shrunk from six quarters to five quarters, back around 1996. So I was simply defeated on that project of getting chaos into the core, where I think a little bit of it belongs.

COHEN: Are they learning any chaos now?

FRAUTSCHI: No, they're still not! It's still encountered only in very advanced technical classes. It's just regrettable.

I should mention one more thing about my tenure as executive officer. About the time I started as executive officer, Jerry [Jerome] Pine [professor of physics] introduced take-home labs into the electricity and magnetism part of Physics 1.

COHEN: You mean these little bundles of stuff?

FRAUTSCHI: That's right. They had been developed largely at MIT, with Jerry's participation here. The students would do the work in their rooms. It was an inexpensive bundle of material—a box containing less than $100 worth of gear. We couldn't afford oscilloscopes; it was just things they could do in their own rooms. And these were introduced because of a feeling that more and more students came in with no hands-on experience. They didn't make model radios anymore—as Feynman had. They didn't fix cars anymore, because car motors had become a series of black boxes. The idea was to reintroduce some of that into the curriculum. So we did that, and the professors liked it.

COHEN: Did the students like it?

FRAUTSCHI: Some of them did and some of them didn't. There was persistent resistance. But also, of course, we were limited to a fixed number of units for the core physics course, and we had simply added something. We couldn't add more units, because the other departments wouldn't let us, so the students complained that we'd made the work too long. We responded by trying to cut back on homework and so on. And that meant we would leave them without sufficient practice time to really master the analytical material.

So finally, after several years of that, we had a catastrophe at the end of Physics 1, in the final exam, in which the students did terribly. So I convened a meeting of all the interested professors who knew something about the undergraduate program, to discuss what we were going to do. Everybody wanted to keep those take-home labs; they were regarded as very important by those who were familiar with them. I would say that there were as many opinions as to what to do as there were professors present, and then I had to go home and make a decision. And I made the decision to break the electricity and magnetism portion of the course into two

tracks, which we subsequently called practical and analytical, and either have the students do these take-home labs—that is, the practical track—and not learn as much sophisticated math as we'd been teaching them. Or, the other way around; they could use the Purcell text [E. M. Purcell, *Electricity and Magnetism*, *Berkeley Physics Course, Vol. 2*] get the sophisticated math, and not do the labs. We left them the option of a reading course in the labs, if they wanted to do that, but we didn't require it anymore.

Now, there were various professors—including David Goodstein, our most effective freshman lecturer—who were quite concerned about this course of action, but it turned out to be a great success and it's been a great success ever since.

COHEN: So what's the course, and how did they choose?

FRAUTSCHI: Well, the first year [1992-93], a majority chose the practical track. But subsequently it's settled down to roughly sixty percent analytic, forty percent practical. It's been pretty stable. It does require more teaching resources, such as lecturers in both areas and so on.

COHEN: So it's like two courses. It *is* two courses.

FRAUTSCHI: It's two courses. It's been very popular with both the profs and the students. The students—first of all, they've got a choice. For example, the typical biologist would find that the practical track makes more sense for them. Most of the physics-major-bound take the analytic.

COHEN: I see. So it broke that way. The physics majors took one and the—

FRAUTSCHI: Yes, well, electrical engineers are split down the middle. And we don't prescribe. So one thing the students like is that they've got a choice. Secondly, those who simply don't want to do a lab don't have to anymore. And each course got to focus on one approach or the other, instead of straddling both, which you could never do as well. So it's been a success ever since.

COHEN: And that's how it's taught now?

FRAUTSCHI: That's how it's taught now. We have a dedicated group of professors, kind of a fly-wheel of institutional memory, carrying both of them on. The name at least, and a bit of the split in approach, has been emulated in the math department—there is now a practical and analytical track in math. So that was a big success arising out of a crisis and a need to do something.

Another hard problem is time scheduling—that's another thing the executive officer does. After four o'clock, the day is devoted to sports. You haven't got Saturday. You haven't got twelve to one, because that's devoted to lunch. Nine o'clock in the morning, most students are still in bed. So fitting classes in is a terrible and growing problem.

COHEN: Willy Fowler taught at eight o'clock in the morning.

FRAUTSCHI: Well, I think now—except for one or two labs in other departments—I think there's none of that anymore.

COHEN: The students have won.

FRAUTSCHI: Well, it's not so much that they've won as that one thing leads to another—for example, they stopped serving breakfast in the houses, because it wasn't well attended, but if there's no regular breakfast, then even more students will stay up late and get up late. But most formidable is that the students do their homework together, and therefore you sort of have to do it when the other kids are doing it—and that's in the middle of the night. So even if you're trying to resist that, you're swimming against the stream. It's a problem.

My next position dealing with students started in '97, when I was asked if I would become MOSH—which is master of student housing. The title is historical, and it accurately describes what the holder of the position did before about 1990. But after that—until very recently [early 2003]—there was a professional, called the director of residence life, who handled almost all the discipline aspects, the rules for which students go to which rooms, room picks, all those things, and the master of student houses has largely been involved with trying to get students together socially, outside the classroom, with professors and staff, trying to get them to the occasional cultural event off-campus, and things like that.

COHEN: Now, there's a house provided the MOSH, but you did not move in.

FRAUTSCHI: I didn't happen to live in the MOSH house, but it was potentially available, and it would have been a nice perk—I'd have had free rent. Which I didn't take, simply because my wife and I were very settled into our existing house, that's all.

COHEN: Was this a full-time job?

FRAUTSCHI: No, no, a half-time job. I kept teaching. If I'd been still doing the full-time research, I would have had a very hard time. I would have had to beg off teaching, or something would have had to give. But basically I wasn't doing much research anymore, and that's where the time came from. You do a lot of organizing dinners with professors and students. We do that with all the freshmen, and we give seniors the option. And we organize various events off campus.

COHEN: Now this is interesting. You say they have just changed this again.

FRAUTSCHI: That's the result of an administrative decision.

COHEN: So now it's a professional doing that job?

FRAUTSCHI: Oh, there's a long-term trend for things that kindly old professors used to do and that are being done by professionals now. Part of it is—taking the MOSH position as an example—that there are more and more government regulations, and what professor is up on all of those? There are more and more serious legal constraints.

COHEN: Could you elaborate on that?

FRAUTSCHI: Well, the alcohol policy is something that the gentlemen in Congress—who, I presume, lubricate their own business with alcohol—decided: that any campus that doesn't have a serious policy in place governing alcohol use by minors would be subject to losing government funding for many diverse activities. In other words, an administrator couldn't look the other way anymore.

There were other pressures from the American legal system, as it developed. For example, you can't in any form tolerate hazing anymore—which may have been present in various practices in the past. A whole chain of things like that, where either the legal system or government regulations—

COHEN: So, in some sense, a person had to be aware of all these rules.

FRAUTSCHI: Very few professors *would* be aware of all of these rules. Yes, there are special journals and so on, with the aim of keeping you up-to-date on these things.

COHEN: Was that unique for Caltech?

FRAUTSCHI: No, no. This is all over the country.

COHEN: I meant unique in that a professor would sort of casually do this job, even though there were all these regulations.

FRAUTSCHI: I think Caltech was relatively late to make the shift to professionalism, because we were small and we had a coherent student body that was on campus, and so on.

The present MOSH still just does the social side, but now on the professional side there's been a massive reorganization. The latest development in professionalization is to take over from the vice president for student affairs, who had been a professor until this past year. The last holders were Chris [Christopher E.] Brennen [professor of mechanical engineering], and then briefly, after Chris had a heart attack, Gary Lorden [professor of mathematics]. Well, anyway, for the first time we've got a full-time professional doing that now.

COHEN: What did that person do? What did Chris Brennen do?

FRAUTSCHI: Well, it was a growing list of things, which in the last twenty-five years had just burgeoned.

COHEN: And you say, mainly because of government regulations …?

FRAUTSCHI: Well, also, demands from all quarters. For example, the Minority Student Affairs Office, the Women's Center. Those are things which years ago we didn't have. The Performing & Creative Arts Program. Years ago, we didn't have much, and then it grew, so these are all complicated things that are very diverse. The Registrar's Office is part of the charge there. The Admissions Office is part of the charge. So the position of vice president for student affairs, when Chris Brennen did it, as recently as a year and a half ago, he was a professor who conducted research and taught and was still keeping track of all that. It became much more complex, and eventually we went professional, and we're one of the last schools in the country to make that shift.

COHEN: So that's progress.

FRAUTSCHI: Well, it's kind of sad, but stopping it is sort of like stopping the tide. Professors don't have the time. Of course, another thing that happened, back in the late eighties, was that the professors stopped interviewing prospective freshmen in their own high schools. That was another example of the same general trend.

COHEN: So you've been just a little bit behind the rest of the universities in the country in adapting to these new circumstances and having professionals run that side of it.

FRAUTSCHI: Yes. Well, if you talk to professors, and often if you talk to students, you'll hear that they'd rather have kindly old professors doing these things—people who they feel may understand them better. But it's just harder to do these things, and it's just less and less realistic.

So, as MOSH, I carried on these traditional things. One important area was funding. I had financial support from Caltech to run the dinners with profs, but just a couple of years before I started, the program of getting students off campus to cultural events or bringing cultural events into the houses had been augmented by an alumni gift from Vic [Victor V.] Veysey [director of Caltech's Industrial Relations Center, 1977-1983], and that was very popular. The funds that Veysey had given ran out after I'd been in office a year. Fortunately there was another alumnus, named James Crawford, who'd been inspired by Vic and who had also made a gift. Vic's original gift had been $100,000, I believe, and James Crawford gave $100,000. But those were fixed gifts, and we were on our way to running out of that one as well. We discussed this with

Veysey, and Veysey had discussions with Crawford. We thought it would really be more constructive, put things on a much stronger basis, if we had an endowment. Also, if you were constrained to spend just the interest from an endowment, then there would be no temptation to overspend, or run out. And Mr. Crawford admirably stepped up to the plate with a series of gifts that eventually amounted to $500,000, which is enough so that you have a reasonable payout every year to conduct these programs.

COHEN: Now, is it always the same people that have dinners with the students?

FRAUTSCHI: You mean professors?

COHEN: Yes.

FRAUTSCHI: Well, whoever are serving as freshman advisors. And then, in addition to that, it's whatever profs are known to the MOSH who are likely to be interested. [Tape ends]

Begin Tape 2, Side 2

FRAUTSCHI: So it was spread out over a large number. I felt that these dinners were a good thing to do. We don't have much of it, and I think it's a very healthy activity. When I was an undergraduate, I had dinner with professors on just a few occasions, and they stand out in my memory fifty years later.

Anyway, I was happy, by the time I ended my five years as MOSH, that through the generosity of Mr. Crawford we had put this funding for the non-dinner events, the extra stuff, on a firm, permanent basis. I'm very happy with that.

One of the off-campus activities that I originated was taking students to the opera. I had done a lot of going to the opera the year I spent in Europe. Taking students with me started when Vic Veysey first made his gift. My predecessor as MOSH, David Wales [professor of mathematics], said to me, "You know, Steve, you like music. Why don't you take students to something musical that they normally wouldn't go to." So I thought, Well, what's an example of something they normally wouldn't go to? And the word "opera" formed in my mind.

Now, in the meanwhile, with a boost from Beverly Sills in New York, it had become standard to put supertitles above the stage during the opera, so that you could actually follow what's going on, line by line. So, on my first try at taking Lloyd House to the opera—

COHEN: How many did you take?

FRAUTSCHI: Well, I thought we'd get just a few girls—this was my stereotype. But I got twenty-five students and about equal numbers of men and women. And on subsequent occasions, we've had up to forty. That happened while I was involved with Lloyd House as a house associate, where you have the resources of the house to provide the cars.

When I became MOSH, I started buying opera tickets for—that is, opening it up to—the whole undergraduate body, and then we'd advertise. And in that case, I'd buy twenty tickets, and they'd give me two free tickets, so I had twenty-two. You need a minimum of fifteen to get the cut rate, twenty per cent off. If it was much more than twenty, then in organizing it myself I found that the cars provided a rather daunting confusion at the last minute. Would they all show up? Twenty was about as many as my nervous system could handle. I stuck to old favorite operas; I didn't try the latest Finnish opera or anything like that—just because the majority of the students were going for the first time, and I wanted a sure-fire winner. It's been a very popular program, and there was a lot of demand when I finished as MOSH that Cathy Jurca [associate professor of literature] would continue to do the same thing. Sometimes I've had as many as fifty or sixty students who wanted to go.

Now, when the house goes, then they tend to charge a ten-dollar co-payment, which the house can readily collect. But when I, as the MOSH, was organizing these affairs, I didn't charge a co-payment—kind of hard for me to collect it. So the students signed up, they got a free ticket, and yet on average I would either have one unused out of the twenty-two or none unused—maybe ninety-seven percent utilization. So that was something that I'm happy about and it's continuing.

The other main offshoot of my MOSH position that I'll mention is that when I started, Chris Brennen asked me if in addition to the traditional duties of the MOSH there was any other problem I would particularly like to work on. And I immediately mentioned the situation regarding the performing arts on campus, which I was aware of partly because I'd been on a

committee appointed by President [Thomas E.] Everhart several years earlier, which had reviewed performing arts along with a lot of other student affairs activities. And performing arts had appeared as sort of the orphan.

COHEN: Now, was Shirley Marneus [director of TACIT (Theater Arts at Caltech)] there already?

FRAUTSCHI: Oh, yes. It was in '98 that this committee got going. I was allowed to bring in some important profs, including Jack [John D.] Roberts [Institute Professor of Chemistry, emeritus] and Ray Owen [professor of biology, emeritus], and a variety of people—and of course some students and some of the instructors from the performing arts. By this time, much of our performing arts staff had been on campus for twenty years, so it was a very consistent, dedicated, good group. Other professors who heard about this misunderstood the purpose of our committee. In fact, your husband [Marshall Cohen, professor of astronomy, emeritus] was one of these. He thought the problem was to remedy something he might have read about in the newspaper—namely, the situation of James Boyk [lecturer in electrical engineering and in interpretive music], who had been temporarily fired and then eventually unfired. Whereas I thought the problem was that we had minimal facilities for these excellent instructors to work with—just a general situation of not enough support. I thought if we got involved with personnel details, we'd never accomplish anything. It's not for a committee to decide on personnel issues—a committee thinks about structure and how something is organized. So that was one of the challenges, to keep it focused on that, because certainly I had lots of phone calls from professors who supported Jim Boyk's efforts to remain employed. And I had phone calls from the other side.

COHEN: Wasn't the problem that he didn't have a home, and electrical engineering didn't want him?

FRAUTSCHI: I don't want to discuss that problem here—and I didn't want to discuss it then in the committee. So anyway, there was the challenge to keep the focus on structural issues that might with goodwill actually be attacked. Incidentally, it was a problem also to keep the… Every arts instructor wanted to be on the committee; on the other hand, we needed to keep the committee small. So that was a never-ending struggle also, to convince them that we weren't dealing out favors to one group or another behind their backs.

We came up with a report in mid-'98, with the encouragement of Chris Brennen, on what might be done to strengthen the performing arts on campus. This is performing arts for students, not bringing in outside artists to perform. And I've been gratified that some of our smaller requests have been satisfied, and some of our most expensive requests—for extra music practice facilities and a concert hall dedicated to music instead of science talks—have found their way into the fund-raising drive that's being conducted right now. It won't be easy to get those things funded, especially the way the economy is now, but everybody up to President [David] Baltimore has supported that vigorously.

COHEN: Now, when you talk about "for the students," you're talking about the whole music program?

FRAUTSCHI: The music program, the theater program, and the painting and drawing—the visual arts program, which is newer than the other elements and not so familiar.

COHEN: I can remember some years past, that there would be a visiting artist of some kind.

FRAUTSCHI: We had a program like that; we've lost it. That's another element we're working on, but it's not actually our highest priority. Our priorities have been things more like the band practice room, which is under the stage in Beckman Auditorium. It has a very low ceiling. Not only can you not hear the other parts properly, but it must be bad for your ears, with the drums sounding and the horns, and so on. It's just shabby. Or you should go see the tiny space that they try to paint and draw in—no ventilation, no natural light.

COHEN: I don't even know where it is.

FRAUTSCHI: Well, of course. This is all out of sight. But anyway, we need to do better. And that's what we're trying to do.

COHEN: Denise Nash [assistant vice president for public events] has nothing to do with this?

FRAUTSCHI: Oh, she has to do with public events. She's on our committee, and she's a very useful member. But we're talking about art done by the students. And there's a very healthy program, but with shabby facilities and they deserve better.

COHEN: And you're still working on this?

FRAUTSCHI: We're still working on that.

COHEN: And you're no longer MOSH.

FRAUTSCHI: I'm no longer MOSH, but the committee goes on, and I hope to see that through. Of course, I also have to bring in some younger professors, to get them interested. I hope there'll be a parallel with the athletic facilities, where after years and years of effort we got a second, very splendid gym. And until recently, we had two committees on athletics, as opposed to zero committees on performing arts. So the goal is to have better facilities for the arts and certainly to have a faculty committee continuing, like the athletics committee, that keeps this in sight.

COHEN: Now, you've been through several presidents, of course. Have you found some of them much more sympathetic to this?

FRAUTSCHI: Murph [Marvin L. Goldberger, Caltech president 1978-1987] actually got involved in some of the plays.

COHEN: Right, and I think he made Shirley Marneus into a full-time position.

FRAUTSCHI: Yes, and Tom Everhart and his wife have consistently made donations to the arts program, but he didn't see fit to put any more muscle into the organizational structure. In the original review of student affairs that I was on, back while Everhart was president [1987-1997], we mentioned the plight of performing arts, and while he was personally sympathetic, he didn't take any major action. President Baltimore has consistently been more actively supportive. In fact, the very first time I saw him in a group setting, something came up about getting more women undergraduates. And he said that based on his own daughter's experience when she

looked at Caltech, he thought it wasn't just a matter of more clever admissions brochures and so on. He thought we needed to actually make some changes. His daughter's experience had been—she asked Shirley Marneus about what theater we had on campus, and Shirley, being honest, had to tell her that there was no dedicated stage available full time. And that was it for President Baltimore's daughter—she didn't pursue Caltech anymore. So he's been consistently supportive.

If anybody up the chain of command had been not sympathetic, then this wouldn't have flown, because this is a science institute, not an arts institute.

COHEN: Well, how about the connection with Occidental [College] that I hear about once and a while?

FRAUTSCHI: A bunch of our music instructors either have joint positions over at Occidental or started over at Occidental, so yes, there is a connection there, and that's a healthy connection. And the orchestra is, in fact, joint.

COHEN: But students aren't going back and forth between the two institutions?

FRAUTSCHI: Oh, once and a while they do. And here's an example of our lousy practice facilities: As recently as last year, Bill Bing [director of the concert and jazz bands] felt he couldn't in good conscience have his students practicing in the basement under the stage of Beckman Auditorium anymore. They would have to practice full time over at Occidental. Well, that's a hurdle, because it takes time to get over there and back. Occidental has a much better facility.

COHEN: Well, it's also a liberal arts college. So, that's all very interesting. What do you see yourself doing now?

FRAUTSCHI: Well, certainly I will continue as chairman of that committee for the time being. I really want to see that through, and that's going to take some time. Raising money for anything in the present economic environment is going to be—

COHEN: Well, they probably don't even let you.

FRAUTSCHI: Well, you see, for the first time, they did; for the first time they've let performing arts pursue donors. So, for example, the Caltech Associates made this the object of their annual fund-raising drive a year or two ago, and in excess of $100,000 was raised, with Darryl Denning [director of Performing & Creative Arts] putting it to good use on several projects. So that's an example of something that we were never allowed to do before. All we were allowed to do before was, on the programs handed out at music events, or theater events, people were invited to join, say, the supporters of the Glee Club, or whatever, and donate. That was the only form of solicitation that was ever permitted previously.

COHEN: I see. So Baltimore has a much larger view of this.

FRAUTSCHI: Yes, he absolutely does.

COHEN: Which doesn't surprise me. That's good.

Are there any general statements you want to make about life at Caltech? You've been here a long time. I guess you like it.

FRAUTSCHI: I like it. It's my club, if you will.

COHEN: Now, does your interest in the music somehow stem from the fact that you have two lovely professional, accomplished violin-playing daughters?

FRAUTSCHI: Well, it's an interest that both my wife and I had, and we originally thought it would be a sideline for our daughters, but then they took it seriously and became professional violinists. And no doubt that stimulates my feelings for this subject. I must say, though, that the most urgent need, where we're falling down the most on campus, is the most recent part of the program—namely the drawing and painting, the visual arts. That's the one where there are basically more students who want to do it than we've got room for. For example, ceramics we do over at Polytechnic—the high-class prep school across the street—in the evening.

COHEN: Do many students go over there?

FRAUTSCHI: Nearly thirty show up. It's only one evening a week. We don't have nearly enough wheels to keep them all occupied. We'd like to get a facility on campus to keep them all occupied—that's one of our highest priorities. Anyway, that's one activity I'm continuing.

I'm also continuing to teach whatever they ask me to teach. I like teaching very much.