David Goodstein, 2000

DAVID L. GOODSTEIN
(b. 1939)

INTERVIEWED BY
SHELLEY ERWIN

November 11, 18, 25, and
December 12, 2002

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Abstract

An interview in four sessions, November and December 2002, with David L. Goodstein, Gilloon Distinguished Teaching and Service Professor, emeritus, and professor of physics and applied physics, emeritus, in the Division of Physics, Mathematics, and Astronomy. Dr. Goodstein received his BS from Brooklyn College (1960) and his PhD from the University of Washington (1965). He joined the Caltech faculty in 1966, serving as Caltech's vice provost from 1987 to 2007, and became professor emeritus in 2009.

He recalls his early education; taking up experimental low-temperature physics at the University of Washington; recruitment to Caltech by James Mercereau. Six-month NSF Fellowship, University of Rome. Teaching Physics 2 at Caltech; friendship with Richard Feynman; their trip to the University of Chicago in 1967. Establishment of applied physics option.

Comments extensively on the production of the innovative 52-part TV series he conceived called *The Mechanical Universe* (released 1985-86).

Recollections of Caltech presidents Harold Brown and Marvin "Murph" Goldberger; Arroyo Center controversy under Goldberger; Goldberger's difficulties with provost R. E. Vogt. His own duties as vice provost; involvement with scientific misconduct; technology transfer and patents; SURF (Summer Undergraduate Research Fellowships); Campus Computing Organization (later, Information Technology Services). Describes his work on various NSF committees, the California Council on Science and Technology, and the Packard Foundation. Concludes by commenting on his love of teaching, both of undergraduate and graduate students.

Administrative information

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH DAVID L. GOODSTEIN

BY SHELLEY ERWIN

PASADENA, CALIFORNIA

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Interview with David L. Goodstein **by Shelley Erwin**

Pasadena, California

Session 1	**November 11, 2002**
Session 2	**November 18, 2002**
Session 3	**November 25, 2002**
Session 4	**December 12, 2002**

Begin Tape 1, Side 1

ERWIN: Could you begin by giving us a little bit of family history, a little background on where you were born and where you grew up, early education?

GOODSTEIN: I was born in Brooklyn and grew up in Flatbush, right around the corner from Brooklyn College, which is where I finally went to college. Neither of my parents went to college; my mother was one of nine children, and they chose one member of the family to go to college, and he never finished. [Laughter] She was born in New York, and my father was born in Warsaw—and probably would have gone to medical school if his mother had lived, but she died at a young age. So in 1916 he and his father—they were not poorly off—traveled around Europe a bit and then emigrated to the United States.

ERWIN: Was he an only child?

GOODSTEIN: My father was an only child, yes. He went to prep school and learned to speak unaccented English, which was very important for an immigrant. But then he started making money working as a salesman, so he never went to college.

ERWIN: But he had some ambitions?

GOODSTEIN: Well, his mother had ambitions.

ERWIN: I see. So it really was the loss of her influence in his life that was a major blow.

GOODSTEIN: Yes, I think so. Anyway, I grew up in the area around Brooklyn College, so I went to that college and I went to the public school, called PS 152, in the same neighborhood. But I did not go to Midwood High School, the local high school; I went to Brooklyn Technical High School instead. I guess, like most scientists, I knew at a very early age that I had a facility for numbers that most children didn't have, and I knew in some vague way that I was going to be a scientist or an engineer or something like that, even though I had no idea what a scientist or engineer did.

ERWIN: This was some time in elementary school?

GOODSTEIN: Oh, yes. Brooklyn Technical High School was designed to prepare its students to become engineers, so when I graduated from high school I thought I was going to become an engineer. Brooklyn College did not have an engineering degree, but it was around the corner, so I started there. The way it worked was, if you were going to be an engineer you started as a pre-engineer at Brooklyn College for two years, then you went off to City College, in Manhattan, to finish your degree. They do have an engineering degree.

So I started at Brooklyn College, but the prospect of spending two hours a day on the subway going to City College convinced me that I didn't really want to be an engineer. [Laughter] So I majored in physics instead.

ERWIN: Do you have recollections of any of your teachers from that time?

GOODSTEIN: Yes, I had very good teachers. One was named [Bernhard] Kurrelmeyer. Another was named [William] Green. There was a third one—a theorist—named [William] Rarita. They were good physics teachers. But by far the best teacher I had in college, and the course that I enjoyed the most, was freshman English, which was a writing and literature course. I absolutely idolized the teacher. Now that I know something about the academic world, I know he was just

an instructor on a one-year contract. His name was Mr. Rose, and he tried to talk me out of majoring in science. In fact, he tried to talk me into becoming an English major. But it was hopeless.

ERWIN: Why do you say that?

GOODSTEIN: Well, because I figured if I was ever going to make anything of myself, I was going to have to work a lot harder than I was doing in freshman English. And physics was a lot harder than freshman English. So I did major in physics, and that's where I met my wife [former Caltech registrar and archivist Judith R. Goodstein]. We were both in student government.

ERWIN: She was not a physics major?

GOODSTEIN: No, she was a history major. But we were both involved—I've forgotten the details of how they arranged the student government, but whatever it was, we were both involved in it, and that's how we met. She did change from straight history to history of science as a result of being engaged to me.

ERWIN: Could you major in the history of science at Brooklyn College?

GOODSTEIN: There was a very, very good professor in the history of science there. His name was Carl Boyer. She took his course in the history of science. Maybe she didn't major in the history of science—maybe she majored in history—but she took the course in the history of science, and by the time she graduated she was going to get a PhD in the history of science.

We both applied to a number of schools, and the University of Washington was the one at which we were both offered fellowships, and that's why we went there. Also, it was as far as you could go without leaving the country. [Laughter]

ERWIN: Could we go back to Brooklyn College for just a second? Brooklyn College was free to the community, wasn't it?

GOODSTEIN: Yes. There was no tuition. There were fees, which amounted to, as I remember, $6 a semester. But it was even better than that: I lived at home, because I could walk around the corner to go to the school, and I had a New York Regents scholarship, which paid $1,400 a year to anybody who went to any college in the state of New York.

ERWIN: So you made money?

GOODSTEIN: I made money, that's right. I also worked as a shoe salesman in college, so in some sense I worked my way through college, but it didn't cost me anything. [Laughter]

ERWIN: Was Seattle a big change?

GOODSTEIN: Well, certainly it was a big change—a big, colorful change. We had both grown up in New York, seldom been very far out of the city, certainly never as far as Seattle. It was different. I don't think we suffered from the difference, I think we enjoyed the difference. But it was quite different. The climate is very different—it rains a lot, but it's mild, and we liked that. Also, the University of Washington—for me, at least—was a great place to go to graduate school, because it was not a searing, painful experience. I made friends at the time with a contemporary of mine who was then a graduate student here at Caltech. I had no idea, of course, that I would wind up at Caltech. But he spent a summer at the University of Washington on some sort of a program. He was a theoretical graduate student, and he had a terrible time at Caltech, because there were people in his class who were so obviously superior that they made everybody else feel that they had no future. There was nothing like that for me at the University of Washington. I just floated right through graduate school without any problems at all. In fact, the only thing that worried me, going into graduate school, was that I wouldn't be able to pass the language exams. In those days you had to pass exams in two languages—that's not true anymore, but it was then. I had taken French in high school—in fact, scientific French, because I was going to Brooklyn Technical High School—and German in college. And I hadn't learned either one. [Laughter] Those two subjects were almost a blank to me.

We arrived in Seattle at the beginning of the summer of 1960. I had graduated in June; Judy had graduated earlier, in January; she graduated after three and a half years of college. So

we arrived in summer. We got married in June, on June 30. Drove across the country—that was our honeymoon, more or less—and we wound up in Seattle.

They offered me the French test. To take the French test, you paid a dollar, and there was no penalty for failing; you could take it any number of times. Judy had had French in college; it was fresh for her. I hadn't had it since high school. She signed up for the exam, so I thought I'd sign up, too, since I had nothing to lose.

Well, it turns out that there were two different French exams—one in the humanities and one in the sciences—and in both cases your job was to translate a paragraph of French into English. The humanities one, which I saw, was really hard. You had to know the names of all kinds of flowers; very specific things. The physics one was a one-paragraph description of something called the field-ion microscope, and although I couldn't understand much of the French, there were enough cognates so that I knew what it was about. And I knew how a field-ion microscope works. So I just wrote a one-paragraph description on how the field-ion microscope works, matching it as well as I could to the cognates. And of course I passed, and she failed. [Laughter]

I got my PhD in 1965. I thought I was going to be a theoretical physicist. My whole life is a series of accidents in which I did the right thing for the wrong reason. I became a physicist not because I wanted to study physics but because I didn't want to go to City College by subway—you know, that sort of thing.

In the first year at the University of Washington, they do something that I think we should do here but don't, and that is, they give all their first-year graduate students either fellowships or teaching assistantships so that they don't have to do research work immediately; they can look around for a while. I was assigned as a TA [teaching assistant] for a professor who had just arrived. He had been at Los Alamos National Laboratory; he hadn't been in the academic world before.

ERWIN: And his name was—?

GOODSTEIN: Greg [Jay Gregory] Dash. He had come to the University of Washington to set up a laboratory in what was then called low-temperature physics, and he needed graduate students to get the laboratory started. Of course, I was TA-ing for him, so he locked me in his office one

day and said he wasn't going to let me out until I agreed to become his graduate student. [Laughter] And since I didn't want to spend the night there, I agreed to become his graduate student, and that's how I became an experimental low-temperature physicist. Once again, the right thing for the wrong reason. I did an experimental thesis for Greg.

When I finished, Judy hadn't finished yet, so I got a position there as—I think they called it research instructor, but it was a postdoc.

ERWIN: So you had your PhD in 1965.

GOODSTEIN: Yes. I got it formally in four and a half years. It's very hard to get a PhD in experimental physics in four years—it seldom happens.

ERWIN: Just to amplify a little bit about your graduate career, how many other people were in low-temperature physics? Did you have a big group? Was it a coming thing?

GOODSTEIN: Oh, yes, it was. And it's grown a lot, especially at the University of Washington—which had a surprisingly good physics department, certainly in the top ten. It grew because it expanded into other areas. It started with low-temperature physics, and now there are two or three professors in low-temperature physics and somebody in surface physics and various other related areas. So it became quite a big thing. But I was almost the first. I would have been the first, except that one of the students had transferred from another university and so already had some graduate experience. He's now working for me at Caltech.

ERWIN: Who's that?

GOODSTEIN: His name is Bob [Robert M.] Housley. He doesn't really work for me anymore, but he does still work at Caltech. That's a complicated story. He's not on the faculty; he works for Rockwell, and they let him spend four out of five days here. For a long time he did work for me, and now I think he works for Tom [Thomas A.] Tombrello [Robert H. Goddard Professor of Physics; d. 2014].

But in any case, as I remember it, it was a fairly substantial group. We used to have group meetings. We'd go out to dinner once a week, and then after dinner we'd come back and have a group seminar. And there were enough people to do that, so there must have been ten or twelve people at least in the group.

ERWIN: Was the physics department there fairly large?

GOODSTEIN: Yes, it was about the same size as the one here—thirty-five professors or so, and a hundred and fifty graduate students, something on that order.

ERWIN: So compared to Caltech, the size was the same, but apparently the pressure was less.

GOODSTEIN: Yes, the pressure was much lower. Their big problem was that their graduate department was required by law to admit anybody who managed to graduate with a major in physics from an accredited college in the state of Washington. So their big problem was getting rid of graduate students who shouldn't have been there. They had to wash them out in some way, so they had an exam—not a candidacy exam, it was a written exam sometime in the first year, and if you failed it you got one more chance. If you failed it twice, you were out. I can remember arguing with Greg Dash bitterly that we shouldn't have this exam—I mean after I took it but before I knew what the result was. I knew he was having a good time, because I found out afterward he knew what the outcome was—that I had come in second out of all of the students who had taken the exam. So he was just baiting me.

But it was a good education. There was a professor there named Ernest Henley, who's still around—he's probably emeritus now, but he's still around. I remember him as the best teacher I ever had. I took electricity and magnetism from him—a wonderful course! It was a good time and a good place to be in graduate school. The only problem with it was that it was not Harvard or Princeton or Berkeley, or one of the places that launch you into a stellar career in science. Usually, if you wanted to go into academic science, you would hope to wind up teaching at a small liberal arts college, something like that.

During my year as a postdoc, I started thinking seriously about looking for a job. I was teaching elementary physics and doing research.

ERWIN: This would have been the ’65-’66 academic year?

GOODSTEIN: That’s correct. Then I started looking for a job. There was a party at Greg Dash’s house in honor of one of his former colleagues at Los Alamos who had come to visit and give seminars. And he told Greg that a man named Jim [James E.] Mercereau at Caltech was looking for people to hire because he was trying to start a low-temperature group at Caltech. So I asked Greg if he would write to Mercereau and recommend me, and he did, and Mercereau invited me to come and give a seminar. And that was the trip on which I met [Richard P.] Feynman and gave my seminar. I remember telling Judy before I went that if they had invited me to give a seminar, they would offer me the job, because I knew I’d give a good seminar. And that’s exactly what happened.

But I also applied for and got an NSF [National Science Foundation] Fellowship to go to [the University of] Rome for a year. And I had two other job offers. In those days, it was much easier to get a job than it is today. One was from Bell Labs, which was a very good offer—a lot of money compared to the other positions—and one from the University of Florida at Gainesville. But the only serious competition was between Caltech and Rome.

ERWIN: The others were not appealing to you?

GOODSTEIN: No, not really. I got both those offers the same day—they both came in the mail the same day—and I was really glum for a while, because I thought I had to choose between them. Then I had an inspiration. I wrote to Jim Mercereau and said I’d gotten this fellowship from NSF to go to Rome. The condition of the fellowship was that I had to start it within one year. So I said, “Suppose I come to Caltech for six months, get the laboratory started, and then go off to Rome for a year?” And he wrote back a marvelous letter in which he said, “Anything that’s good for you is good for Caltech.”

So that’s what I did. We came to Caltech for half a year in 1966, left in February 1967, spent one full year—from February ’67 to February ’68—in Rome, and I’ve been here ever since, except for a month or two a year in Rome.

ERWIN: Let's go back just a little bit over some of that ground. First of all, you came here to give your talk and you met Feynman and maybe some other people. Do you have any stories to tell about how that went and what happened then?

GOODSTEIN: Yes, I'll tell you the stories. Jim Mercereau, the fellow who recruited me, was himself a very famous scientist. It's rather surprising to me that he's not won a Nobel Prize for inventing the superconducting quantum interference device we call a SQUID. He had gotten his PhD at Caltech and gone off to work for Ford Laboratories in Dearborn. Came back. Ford had sent him to a laboratory they had here in Southern California in Newport Beach, and he was working there, and the professor who had led the low-temperature group here left.

ERWIN: How long had the low-temperature group been operating here?

GOODSTEIN: Oh, the low-temperature group goes back way before the war. There had been a professor here who was rather well known in physics—John Pellam—but I didn't overlap with him at all. He left, in 1964, and a committee was formed to decide whether to try to go on with low-temperature physics or get out of it. They decided it was a good idea to go on with it, so they hired Mercereau on a part-time basis to staff the laboratory—in other words, bring new people, faculty. There were a number of graduate students who were still here who hadn't finished their degrees yet. Mercereau was trying to get them finished and start the laboratory, but he was still working at Ford.

Anyway, he invited me to come and give a seminar, and I hoped that the legendary Feynman would be there—

ERWIN: So you knew about the legendary Feynman?

GOODSTEIN: Oh, yes. Everybody in physics knew about Feynman.

ERWIN: By that time he had given his lectures.

GOODSTEIN: He'd won the Nobel Prize. But even before that, he was legendary. He was *not* known outside the world of physics—his work, and the stories about him at Los Alamos, and his cracking safes and playing jokes. The lore was around. But I didn't really think that that was very likely to happen.

So Mercereau meets me at the airport and he says, "Do you mind if we go to lunch with Dick Feynman before you go to Caltech?" And lunch with Dick Feynman meant lunch at Giannoni's, which was a topless restaurant in Altadena where Feynman always used to hang out in those days.

ERWIN: I see. But you didn't know that, though, going in.

GOODSTEIN: I didn't know that, no. So I wound up going directly from the airport to Altadena, where we had lunch at Gianonni's, this topless restaurant, with Feynman and Mercereau. And all I can remember about that lunch was thinking to myself over and over again, "Nobody in Seattle is going to believe this." [Laughter] So that was when I first met Feynman, but that's not when I really got to know him.

Then we went to Caltech and I gave my seminar, and Feynman came to the seminar. And the Lauritsens—Charlie and Tom—came to the seminar. These people were not in my field, but I guess Mercereau had recruited them to come to the seminar, along with the graduate students that were left in the group. That was the whole audience—a very small but very distinguished audience. Then they offered me the job, and I told you the story of what happened after that. I wound up coming here for a while and then going off to Rome and then coming back. I was here as a postdoc for the first six months—I was not on the regular faculty. I came back from Rome as an assistant professor, but I was just a postdoc for the first six months.

ERWIN: So when you first came here, you knew it was a short stay. I know you had one child at this point.

GOODSTEIN: Yes, we had Marcia. She was two years old when we left for Rome and three years old when we got back.

ERWIN: So you were in a sort of temporary stage, with transitional lodgings?

GOODSTEIN: Yes. We had a little house—it's still there, a block from where we live now, on Parkwood. As you go north on Parkwood, it gets less and less elegant…and that's where it was.

ERWIN: Shall we talk a little bit about Rome, then?

GOODSTEIN: There's one incident on the way to Rome that's worth talking about. During the six months that I was here before going to Rome, I was a TA in Physics 2. Gerry Neugebauer [Robert Andrews Millikan Professor of Physics, emeritus; d. 2014], who taught the course, when he learned that I was coming to Caltech, sent me a letter asking me if I would be a TA in Physics 2. He didn't say, "Would you like to volunteer?"—he sort of implied that I had to do this. But I didn't have to do it; it was purely voluntary. So he asked me, in effect, to volunteer. But I wanted to do it anyway, so I taught a section of Physics 2.

We used to go to lunch in the Greasy—it was not called Chandler then; it was called the Greasy—with Feynman. The teachers of the course would go with Feynman, and it was just a few years after he had taught the course, and he was still interested in how it was going. And that's when I really got to know Feynman well. We talked and talked and talked about many things—science, teaching, society, whatever.

ERWIN: How many were in this group?

GOODSTEIN: Well, there might have been a dozen people because of all the section instructors. So we became friends during these lunches, and then we spent more time together.

Now remember, this is 1966, one year after Feynman won the Nobel Prize. So one day he comes to me and he has a letter in his hand, and he says, "Look, I've been invited to give a public lecture at the University of Chicago in February, but I think I'm going to turn it down." Now, he was invited to give lectures every day, at least once or twice a week, and he didn't bring them all to me to tell me about them. So obviously he wanted me to react. So I said, "Well, why are you turning it down?" And he said, "Oh, I don't have anything to say." And I said, "But

what about all these great things we've been talking about, all these ideas we've been discussing?" So he said, "I'll tell you what. I'll go if you'll come with me."

This is going to be in February in Chicago, but it's on the way to Rome. So it was perfect timing. That got me as far as Chicago, and then it would be much less of a trip to Rome, and so that's what I did. He was going to pay my expenses out of the ridiculously large $1,000 honorarium that they had offered him, but as soon as he told Chicago I was coming with him, they invited me. So they paid me.

We went and we spent four days. For somebody like me, who had grown up in Brooklyn, gone to Brooklyn College and the University of Washington—not really been through the academic big leagues—it was a mind-blowing experience. All kinds of things happened on that trip. From my personal point of view, there was a professor at the University of Chicago named Lothar Meyer. He and another man named Earl Long were pioneers in low-temperature physics, and every time I did an experiment or got interested in something, I would find that they had been there first and I was just improving on their work. So I really had a tremendous admiration for Meyer, although I had never met him.

The first day there, there was a kind of panel discussion at which Feynman and I were at the table and students were all around asking questions. After the panel discussion, Lothar Meyer, this man I had admired so much, came up and introduced himself to me and invited me to come to his lab. I didn't have to seek him out; he sought me out. So I spent a fair amount of time with him, and that was a real, genuine pleasure.

One of the little things that happened was that on the way to Chicago I realized in the waiting room at LAX [Los Angeles International Airport], while we were waiting for the flight, that Feynman didn't have an overcoat. I said, "Dick, we're going to Chicago in February. Where is your overcoat?" And he said, "Oh, I've been on many jaunts like this. You don't need one. They just shuffle you from car to car and building to building." But it turned out that we were invited to a dinner party at the Telegdis'—Val [Valentine] Telegdi and his wife—which was about three blocks from the Quadrangle Club, where we were staying, and we had to walk at night, in Chicago, and he didn't have a coat. So he paid the price.

Anyway, there were a number of interesting incidents. One of them was that they simply didn't know what to make of me. They didn't know why I was there or why he had invited me. And they didn't know how to treat me.

ERWIN: Well, would you like to elaborate on why Feynman invited you?

GOODSTEIN: Well, he invited me because we had talked about all these things—

ERWIN: Because he wanted to have some fun—

GOODSTEIN: I really don't know why he invited me, except that he was going to talk about things that we had sort of evolved together. Our official host was a man named Wayne Booth, who I guess is legendary in Chicago history. I don't know exactly what his position was, but he was an important person. So we go to lunch on the first day at the Quadrangle Club. It's a little like our Athenaeum, but not as good. We were sitting around the table and Wayne Booth, to be polite, asks me, "Is there anybody here at Chicago that you'd like to meet?" You know, they had Saul Bellow there and various other people. And I said, "Well, yes, as a matter of fact, there is somebody I'd like to meet. It's John Hope Franklin." Now, John Hope Franklin was a black historian—he's still alive—who had been a professor at Brooklyn College, and Judy had taken an American studies seminar from him, so she knew him quite well. He moved on to Chicago. So she said, when I went off to Chicago with Feynman, "Well, if you see John Hope Franklin, give him my regards."

So I said, "Yes, I'd like to meet John Hope Franklin." So Booth looks around the dining room and he sees Franklin sitting there, but he can't imagine why I would want to talk to him. So very nervously—I could see that he just didn't know what he was getting himself into—he took me over to this other table and he introduced me to John Hope Franklin. And John Hope Franklin didn't know me at all. I shook hands with him, and I said, "My wife, Judy—her name was Judy Koral at the time—was your student in American studies." And he said, "Oh, Judy! How is she doing?" And you could see this wave of relief swoop over Wayne Booth. [Laughter] Because I had a perfectly pedestrian reason for wanting to meet the man. And as a matter of fact, it turns out that he was then the chair of the national Fulbright Committee and he was going to Rome for some official purpose when we would be there. So we arranged to meet and we spent a day together in Rome.

Other things happened. One morning—I've told this story many times—I came down to breakfast. Feynman had gone down before me. So I came down to breakfast, I looked around the room, and there he was sitting with somebody else. So I joined them, and there was some mumbled introductions but I didn't catch who it was. I started drinking my coffee and having my breakfast, and as I listened to the conversation, I realized that this was Jim [James D.] Watson. And Watson had a manuscript with him that he was imploring Feynman to read, because he hoped Feynman would write something for the dust jacket. So Feynman agreed to read the manuscript, which was called *Honest Jim*, but it later became *The Double Helix*. It was published a year later.

ERWIN: It was called *Honest Jim*?

GOODSTEIN: That was Watson's initial name for it, yes, and I'm sure his publisher talked him into calling it *The Double Helix*. But it was a good description, because he really told the story as he saw it, through his eyes.

So we went through whatever was scheduled for us that day, and that evening there was a cocktail party and dinner in Feynman's honor at the Quadrangle Club. And I found myself at this cocktail party and Feynman wasn't there. After a while, Wayne Booth came up to me and said, "Where is Professor Feynman?" I said, "Well, I'll go up to his suite and see if he's there." So I go up to our suite, and sure enough, he's there reading the manuscript. And I said, "Dick, you've got to come downstairs; it's in your honor." And he said, "All right." So Feynman comes downstairs and joins the party—the rest of the cocktail party and dinner—and at the earliest moment permitted by civility, he says goodbye and leaves. But the party went on, and I stayed there and enjoyed the party. About one o'clock in the morning, I went upstairs to our suite, and he's sitting there waiting for me, and he says, "You've got to read this book." And I said, "Oh, that's great. I look forward to it." He said, "No. I mean now!" [Laughter] And so from one o'clock in the morning until five o'clock in the morning, with Feynman sitting there waiting for me to finish, I read through the entire *Double Helix* in manuscript form. The only time I said a word to Feynman was at some point I looked up and said, "You know, this guy was either very smart or very lucky," because he kept saying that he knew nothing about what was

going on in the field but he made the crucial discovery. And Feynman, who had been sitting there doodling—as he always did, on a pad of paper—

ERWIN: He sat there with you, the whole time?

GOODSTEIN: He sat there, yes. Imagine the pressure of Feynman sitting up all night waiting for you to finish reading this book so he can talk about it. He had one word he had written in big letters, and then he sort of illuminated, like a medieval manuscript. And the word was "Disregard." And he said, "That's what I had forgotten. You have to disregard what everybody else is doing and just work on your own things. Now I'm going to be able to get back to work again." He had the Nobel Prize disease—he thought he was never going to do anything important again—and it was really working on his mind. And he felt inspired by Watson's book.

ERWIN: And that was the point he had come to, that night.

GOODSTEIN: Yes. And he called Gweneth [Mrs. Feynman] the next morning and said, "I think I've solved my problem. I think I'll be able to work again." [Laughter]

ERWIN: He had a sort of epiphany?

GOODSTEIN: Yes, that's right. You know, he did work again. He did do important things after that, and I don't know whether that was really a turning point or not. I just have no way of knowing. But I had to sit there and read the book, and then it became my assignment the next morning to take the book back to Jim Watson and tell him that Feynman would write something for the book jacket—which he did. In the first edition, on the dust jacket, there is a contribution by Richard Feynman. But I was the messenger.

ERWIN: Great story! And no sleep.

GOODSTEIN: Yes, no sleep. [Laughter] But in those days, I didn't need it. [Laughter]

Anyway, so that takes us to Rome.

ERWIN: You left Feynman in Chicago.

GOODSTEIN: Yes, Feynman went back to Pasadena and I went on to Rome. And, as a matter of fact, I completely forgot his talk, which as far as I know he refused to allow the University of Chicago to publish. The University of Chicago tried to get him to publish the talk and he refused—for whatever reasons. I didn't know this, but he went back and gave it to the Caltech students at the Caltech Y, the same lecture that he had prepared for Chicago. He actually had prepared it the morning of the lecture, because I wandered into his room at seven o'clock that morning and he said, "This is the creative moment. Leave me alone." [Laughter] He was working on his lecture. He usually lectured from just a few pages of scribbled notes. So whatever lecture notes he had, he used them to make the same arguments again when he did it at the Y.

ERWIN: But you had heard it at Chicago?

GOODSTEIN: I heard it at Chicago but I had forgotten completely what he said. So when we published the book *Feynman's Lost Lecture* in 1996 [David L. Goodstein and Judith R. Goodstein, *Feynman's Lost Lecture: The Motion of Planets Around the Sun* (New York: W. W. Norton)], the kid who had sat in the second row at the Caltech Y and recorded Feynman's talk with a tape recorder wrote to us and told us he had recorded it, and he sent us the tape. Judy had it cleaned up—because you couldn't really understand it; you could only understand about a third of it, listening to it on the original reel-to-reel tape.

ERWIN: That person's name was Greg Evans, if I recall correctly. He was the student who recorded it.

GOODSTEIN: Yes, that's correct. So I eventually did find out what Feynman had said at Chicago. [Laughter]

ERWIN: Now, did he title that lecture, or did you?

GOODSTEIN: "The Uncreative Scientist"? No, he gave it that title. Of course, he imagined the University of Chicago as being sort of—I don't know exactly what words he used, but Caltech is the inner sanctum of science. He was worried about all of these humanists at the University of Chicago and other people he didn't know how to deal with. See, he was trying to speak to them. So he called it "The Uncreative Scientist," because he thought that title would appeal to them. But actually what he wanted to talk about was the fact that scientists *are* creative.

ERWIN: As I recall, you and Judy gave a talk about that lecture.

GOODSTEIN: Yes, we were invited to give a talk for the Friends of the Caltech Library [FOCAL]. It was a luncheon—part of their fund-raising. We titled it "Feynman's Lost Lecture," which was the title of our book, but we gave the talk about Feynman's other lost lecture—this one, "The Uncreative Scientist." Tom [Thomas E.] Everhart [Caltech president 1987-1997] was there, at the FOCAL lunch, so he asked us to give the same talk to the Board of Trustees, which we also did.

ERWIN: And did you publish that one too?

GOODSTEIN: No. [Pause] Shall we get back to Rome?

ERWIN: OK. This was your first time to go to Europe?

GOODSTEIN: Absolutely the first time to go to Europe.

ERWIN: And you had a wife and a two-year-old.

GOODSTEIN: A wife and a two-year-old, that's right. We had arranged in advance to buy a car—a brand-new Fiat. It was called a Fiat Millecento, which means "eleven hundred," which is the engine displacement in cubic centimeters. It's a very small engine! It was a station wagon. We

picked it up the first day we were there, from the Fiat dealership, and I wanted to go off to the lab. We had been put up in an old *pensione* a few blocks from the university.

ERWIN: The lab was at the university, in Rome?

GOODSTEIN: Yes, in Rome, on the campus. The full name of the university is La Sapienza, which means something like "wisdom." There are now three universities in Rome, but in those days there was only one, and it had about 200,000 students. It was quite different from an American university, but in some ways it was similar, because it had research laboratories and did research.

ERWIN: Was your fellowship associated with this particular laboratory?

GOODSTEIN: I had applied to work for a particular professor, whose name was Giorgio Careri. And the reason was because there was a young assistant professor at the University of Washington named Bill McCormick who had spent two years in Rome. He told us stories about his two years in Rome, and one of the ideas was to apply for an NSF Fellowship and go to Rome. And he had worked with Careri, so I applied specifically to work for Careri and I exchanged some correspondence with Careri. And he wrote a letter of recommendation and I got the fellowship. It was not a princely sum of money. As I remember, it was somewhere between $7,000 and $8,000 a year. This is a long time ago, but still that was not really enough to live on, and I also had to travel on it.

But we went, and we got a car. And we were staying at this *pensione* for the first few days until we found an apartment. Since the university was three or four blocks away, Judy put the baby, Marcia, in the car and drove me to the university. I got out and waved goodbye—she was going to turn around and go back to the *pensione*. I spent a good solid four or five hours in the laboratory and then I walked back to the *pensione*. And I got back at about the same time as Judy. [Laughter] Because Rome was full of one-way streets, so you don't just turn around and go back. [Laughter] And by the time I got home, she had seen all of Rome, with a two-year-old and a car she didn't know how to drive—it was a four-speed stick shift, and she'd never driven

one. Well, I guess the old 1950 Chevy that she used to drive in Brooklyn had a stick shift on the steering wheel, but not on the floor.

Anyway, she learned Rome that day. She spoke no Italian. She couldn't ask anybody for directions. But she made it back. [Laughter]

ERWIN: A test of her fortitude and ingenuity.

GOODSTEIN: Right.

ERWIN: Did you speak Italian?

GOODSTEIN: No. We had made a few half-hearted attempts to learn a few words of Italian before we left, but I somehow had the vague notion that if you lived in the country, you'd pick up the language, and that turned out not to be true. It's just not so easy. Being there helps a lot if you want to put some effort into learning, but if you don't put any effort in it—I've known lots of people who have lived in countries for years and years, including the United States, who don't speak a word of the native language.

So I was there in the lab with a group of young people roughly my age, and it was very important for them to learn to speak English. In science, you *must* speak English—it's the universal language of science. But I wanted to learn to speak Italian. So the question was, What would we speak in the lab? Would we speak English or Italian?

We didn't start immediately speaking Italian—at first, we spoke English. But I started going with Judy to Italian lessons at the Society of Dante Alighieri, which is devoted to teaching Italian to foreigners. Lessons were held in a place in downtown Rome, which still exists—they're still in business—at Piazza Firenze. So two mornings a week, we would go to Piazza Firenze, for two hours each time. The instruction was all in Italian, because the class had no common language other than Italian. We learned the structure of the language—basically grammar—in about three months. Then after that I just thought I would speak Italian. They could speak English to me if they wanted to, but I would speak Italian. And they would immediately drop the English. So we spoke Italian only, in the lab.

ERWIN: So you pushed yourself. [Tape ends]

Begin Tape 1, Side 2

GOODSTEIN: I certainly wasn't going to get that in a language class. While I was still there, I gave seminars, and I generally gave them in English. But I was invited at a certain point to give a seminar somewhere, and the person who invited me said, "That's wonderful, because it will be so nice for the students to have somebody do a seminar in Italian." So I was trapped; I had to give a seminar in Italian. I worked very, very hard on this seminar. It was as hard as the first seminar I'd ever given in English. In particular, I made sure that I looked up all the technical words that I needed to know, but Italian has adopted a lot of English words and particularly technical words.

I wanted to use the term "shear stress." I asked all of my friends how you say "shear stress" in Italian, and none of them knew. [Laughter] And I eventually found an eighteenth-century Italian technical dictionary that had the term in it—it's *sforzo di taglio*, which means "cutting force"—"cutting stress," actually. And when I used it in my seminar, nobody knew what I was saying. [Laughter] But I was a purist, you see; I wasn't going to use an English word.

Anyway, so I gave my first seminar in Italian, and ever since, when I've gone back, I've given all my seminars in Italian.

ERWIN: Now, did this laboratory where you were working have a name?

GOODSTEIN: Well, the professor was the professor of what was called the structure of matter—*struttura della materia*—and he held that chair and this was his group. But it was basically the same kind of group I'd come from. It was a liquid-helium, low-temperature physics group.

ERWIN: I was wondering if the university structure was, as one hears, different in Europe, enough so that it affected your outlook or your view of the academic life and the scientific life there?

GOODSTEIN: It was similar enough to what I knew so that I could function—aside from having to learn the language—I could function the way I would function in an American university.

For the first six months, I tried a number of different things that didn't work too well. And then about halfway through, I had an idea for an experiment that was actually useful. And I talked to Careri about it. He encouraged me to go ahead and do this experiment and asked two of my colleagues there to work on it with me. So the three of us designed, built, and then did the experiment, and the experiment was a great success.

I learned a great lesson from it. This was testing a new theory—a theory that concerned the work that we were doing in the lab. In examining this theory, I realized that there was a definitive experiment that could be done which would show that the theory was either right or wrong. It made a prediction that was really unusual and that you wouldn't expect to be correct, other than for this theory. So if the prediction was right, it would be very strong confirmation of the theory. And if the prediction was wrong, then the theory was wrong, period. You seldom get a situation quite as clean as that—no auxiliary assumptions, nothing; either the theory is right or wrong.

So we did the experiment and I was really hoping the theory would be right. But it turned out the theory was wrong and the experiment showed that absolutely unambiguously. So we did it, we wrote it up. I remember that I dropped the manuscript in the mailbox on the way to the airport when I was leaving Rome to go back to California. So we just finished it in time. And then it went off and got published in *Physical Review*, and in every reasonable sense it was a success [Goodstein, D. L., Buontempo, U., & Cerdonio, M., "Ion Mobility Discontinuities in Superfluid Helium: A Test of the Huang-Olinto Theory," *Phys. Rev.* 171(1): 181-186 (1968)]. But because it showed the theory was wrong, the theory and the experiment that showed it was wrong were immediately forgotten by everybody. If we had shown that the theory was right, it would be famous, that experiment. So what I learned was that it doesn't pay to show that a theory is wrong when it's brand-new—only when a lot of people have accepted it and it's part of the accepted wisdom does it really pay to show that a theory is wrong.

ERWIN: Would you say that this was the major thing you did, as far as experimental work, in Rome?

GOODSTEIN: Yes. It was really the only thing I accomplished scientifically. I did other experiments later on in Italy that were important and that contributed to scientific knowledge. But that year, that was the one thing I did.

ERWIN: Well, your connection with Italy and Italian science is ongoing. Did you form ties in this year, then, that continued?

GOODSTEIN: Yes, I did. I made many friends. I still occasionally see them when I go to Italy. In fact, on one of our last visits one of them offered to invite everybody I knew to dinner. So we had a party at which all these old people I hadn't seen in thirty years came to dinner. It was really quite an experience. Not all of them are still alive, because a number of people of my age have died, including two in one year—it was really terrible. But in any case, I made a lot of friends. One in particular was Franco Scaramuzzi. And you know his daughter, Carlotta. He was working not at the university but at the National Laboratory at Frascati, which is just outside of town in the open hills outside of Rome. In those days, it had a very strong connection to the university, in the sense that there was a bus that ran almost hourly between the university and the laboratory, so it was very easy to get there.

ERWIN: So the National Laboratory in Italy is similar to what it is in the United States?

GOODSTEIN: Yes. They had built the world's most powerful synchrotron there. It was a joint Italian–German project. At that time, it was the world's most powerful. Each of these instruments, as it gets built, is the biggest for a while, until a bigger one is built. But they wouldn't bother building it if it weren't the biggest at least for a short time. So it was the biggest at the time—biggest in the sense of having the highest energy flux. It was a synchrotron laboratory. You have to have a lot of stuff associated with a synchrotron laboratory, and one of the things you have to have—or you had to have in those days—was a group that could make cryogenic liquids like liquid helium or liquid hydrogen for use as a target, because those were typically the targets for these particles. So they had a low-temperature group, and the low-temperature group's job was to provide these targets.

ERWIN: Was Franco in that group?

GOODSTEIN: He was the head of that group—well, I'm not sure that Franco was the head in those days. There was another man, named Ivo Modena, who may have been the head. The two of them were about the same age; they were very close friends and still are. But anyway, he was in the group, and that's what matters.

But the group had not published a scientific paper in seven or eight years. And so when I came to Rome, I got to know Franco, and then I started going up there one day a week to help them do some work. And eventually, after my year at the university, when I went back to Italy I didn't go back to the university, I went back to Frascati. And what I did was to make use of the resources available from the low-temperature group there to get really good experiments, really good science done. So I had a number of very successful years with them in science there.

ERWIN: So you were collaborating with that group, with Franco Scaramuzzi?

GOODSTEIN: Yes, with Franco Scaramuzzi and another colleague, Adolfo Savoia. And there was typically a student or two around and a number of technicians. For a while, we had what was known as an NSF cooperative; these are bilateral treaty agreements. So in different years I went in different capacities. For a while, I went under the NSF on this bilateral agreement, and I think Franco came here a couple of times on the same thing. Once, I was a NATO visiting professor there, and sometimes just the laboratory paid my way. And so on. It was just a different arrangement; we cooked up something every year.

ERWIN: Do these sorts of things work in other countries as well?

GOODSTEIN: Oh, sure. I've had a bilateral agreement in Brazil, too. I had a Brazilian postdoc who went back to his university in Campinas, Brazil. He wanted to have this collaboration, so we did, and I went to Brazil twice as a result.

ERWIN: We could talk about your return to Caltech. So you're back at Caltech.

GOODSTEIN: I came back in February of 1968, as an assistant professor. I don't remember now what, or whether, I taught that first year, coming back in the middle of the academic year that way. I may have taught a seminar on low-temperature physics or something like that. But I did immediately go to work on getting the laboratory running, setting up experiments. The work I did in Italy was quite different from the work I'd done for my thesis—basically in another field. And when I came back to Caltech, I set up a little of both—some of the liquid-helium work that I had done in Italy and some of the two-dimensional phase-transition work that I had done on my thesis. So I got both of those fields running, and I acquired some graduate students and did a number of experiments, published the results—did all of the usual things. I taught mostly graduate courses.

One important thing for Caltech was that a committee of three of us—Hans Liepmann [Von Kármán Professor of Aeronautics, emeritus; d. 2009] and Floyd Humphrey [then an assoc. prof. of electrical engineering] and I operated as the steering committee to set up an option in applied physics, which didn't exist before. This is 1970 I'm talking about now.

ERWIN: So this was something new for the physics division?

GOODSTEIN: No, it wound up in the engineering division, actually. It was originally joint between physics and engineering—it's now solidly in the engineering division.

ERWIN: And what was the impetus for that?

GOODSTEIN: Well, the impetus was that we had students who were doing theses in what would be called physics anywhere else, but because they weren't in the physics department it was being called something like aeronautics. We had students doing liquid-helium experiments—my field—in aeronautics simply because Hans Liepmann had an intellectual interest in liquid helium. In fact, now that you remind me, one of the very first things that happened when I came back, and almost the first day that I came back, was that Feynman met me and said, "I want to introduce you to somebody." And he introduced me to Hans Liepmann. Hans Liepmann was then the head of GALCIT [Graduate Aeronautical Laboratories, California Institute of Technology]. But he had been offered a job as the head of a Max Planck Institute in Germany—

because he's German. He came from a liberal family that had to leave Nazi Germany before 1939, but he is from Germany. So Caltech was trying hard to keep him, and Feynman was one of the people who was trying to keep him. He introduced me to Liepmann because he thought that our similar interests in helium might help.

ERWIN: Was Hans trained as a physicist?

GOODSTEIN: No. Hans was trained as a fluid dynamicist. But helium presents some very interesting problems in fluid dynamics. This is what we call a superfluid, and under certain conditions it flows without any viscosity at all, and that should give it spectacular properties. And Hans basically didn't believe any of the experiments that had been done with liquid helium beforehand, so he had to repeat them all for himself. [Laughter]

So Caltech gave him a little fund that he could spend on this little hobby of his, and I became his consultant. I told him how to design experiments and all that kind of stuff—all the technical stuff you had to know in order to be able to do experiments like these. And he very quickly caught up with the rest of the world and started doing experiments that were new and different from what had been done before. I did help him through that—not as much as he gave me credit for. He was very lavish with credit, you know, when he gave talks and things like that. He really did it all himself. But it did help—I hope it helped.

ERWIN: This was the end of the [Lee A.] DuBridge presidency.

GOODSTEIN: Yes, DuBridge was still here as president. I think he stepped down—well, he became the President's science advisor in 1969. So I was too junior, I don't think I really knew him then. We became good friends later on.

ERWIN: Well, one reason for bringing that in is because it seems that after his long tenure as president, new things were just bound to happen. And I wondered if you felt that at the time, particularly as it involved your own division. Did you feel that either your division was ripe for a change, or were you still too junior really to have that kind of perspective?

GOODSTEIN: Yes, I was too junior to feel the reverberations of anything that was going on at that level. Although I was not too junior to become the executive officer of applied physics in 1970, which I was. I may have been the only untenured executive officer we've ever had. [Laughter] But it was a new option that was being created.

ERWIN: So that was one of the new things that happened. Anything else around that time that you'd like to comment on?

GOODSTEIN: Well, let me tell you another one of these stories I've told many times. In those days, Feynman used to like to come down to the low-temperature laboratory. The reason he liked to come down to the low-temperature laboratory was that we had a very pretty secretary, who later became Mrs. Mercereau. But in any case, for whatever reason, it didn't matter; he liked to come down to the low-temperature laboratory. So he would just casually walk into my office and he'd say, "Well, what are you doing?" And that ruled the day; you just forgot about whatever plans you had for the day.

ERWIN: Did that mean, then, that you'd just be talking with Feynman for a long time?

GOODSTEIN: Yes, for a long time. I mean, it might break up at lunch if he came in the morning. But until he had some reason for going away, we would talk. And I wanted to tell him about the experiments we were doing. So I'd start to explain the experiments, and as soon as I started to explain to him, he started asking questions. And he would divert the discussion to some other direction than I wanted it to go in, but I couldn't steer it back to where I wanted to, because he would insist on asking all these questions. They were always good questions, but he wouldn't allow me to say what I wanted to say. He made me tell him what he wanted to know, which was different.

So this went on for four years, and then, after four years, I was asked to give the department colloquium for the first time. After three or four years, we do a review of all untenured faculty members, so it must have been part of that review. So I gave my colloquium, and for the first time ever, Feynman had to shut up, more or less, for an hour and listen to what I wanted to say. [Laughter] Feynman was teaching a course at the time—Physics 205, I think it

was, Advanced Quantum Mechanics—and he was teaching it to a very big class. I don't know how many of them were actual graduate students, because the entire physics faculty was sitting in on this course, including me. I went to his class the next morning, and he said, "I heard the most wonderful thing yesterday." And he gets up to the blackboard and he proceeds to start giving my seminar—probably much better than I did. [Laughter]

A couple of weeks go by. And one morning he does one of these things where he walks into my office and says, without any preamble at all, like "Are you busy? Do you have a minute?"—nothing like that—he walks right up to my blackboard and says, "Look, it's obvious that…" And he started writing down. And my jaw just dropped and hit the floor, because he described for me a theory, an idea, that was so beautiful to explain some of our data—to explain something that I had said in this seminar. It was so original and so beautiful that I just couldn't believe what I was hearing. So I quickly grabbed a pad of paper and started taking notes on what he was saying, and when he left I put our data in a form that could be compared to predictions of his theory. And it really worked pretty well—better than I would have expected.

So I started writing a paper on it. And while I was writing the paper, I got a preprint from England of a paper by two theorists that was based on exactly the same idea. And this theory, which is now known as the Kosterlitz-Thouless theory, became the most important new theory in statistical mechanics in decades. And I've always been convinced that if Feynman had shut up at any time during the previous four years and let me tell him what I wanted to tell him, it would be called the Feynman theory.

ERWIN: Or at least the Feynman-Goodstein Theory?

GOODSTEIN: No, no! The theory is his. I made no contribution at all to the theory, aside from showing him data that could be explained by the theory.

ERWIN: I see. But you triggered his thinking to come to the end point.

GOODSTEIN: Yes, it triggered him. That's right.

ERWIN: Well, that's kind of exciting, in its own way.

GOODSTEIN: Yes. I immediately went to him with this paper and I showed it to him. And for just a second I thought there might have been an instant of disappointment. And he said, "You know, when two guys in different parts of the world"—he apparently thought Kosterlitz and Thouless were one guy—"When two guys in different parts of the world, thinking about different problems, come up with the same idea, it's got to be right." Instead of being disappointed, he was quite pleased by it. So we published the paper [Elgin, R. L., & Goodstein, D. L., "Thermodynamic Study of He-4 Monolayer Adsorbed on Grafoil," *Phys. Rev. A,* 9 (6): 2657-2674 (1974)] and I published it as a theory we had gotten from Feynman. But he didn't want to be an author on the paper, so we put him in a footnote saying that the theory was Feynman's and that we had received the preprint from Kosterlitz and Thouless with exactly the same theory. And it eventually became the Kosterlitz-Thouless theory. They were first; that was correct. [Tape turned off]

DAVID L. GOODSTEIN

SESSION 2

November 18, 2002

Begin Tape 2, Side 1

ERWIN: I'd like to ask you a little more about the history of Harold Brown's tenure at the institute [Caltech president 1969-1977]. When he came to Caltech he said—at least he told the Archives—that Caltech was "ripe for change." Do you think that's a fair assessment?

GOODSTEIN: Well, I think it is. First of all, let me say something about the process of choosing him as president, which was sort of interesting. Lee DuBridge had stepped down. Bob [Robert F.] Christy [Institute Professor of Theoretical Physics, emeritus; d. 2012] was acting president for the interregnum, and they searched for a new president. The trustees decided they wanted Harold Brown, who at the time was secretary of the air force. The faculty was dead set against having a military person come as president. I remember very clearly a faculty meeting we had in physics. There was a vote taken, and it was seventeen to one against Harold Brown.

Then he was brought to the campus and he met every single person, every single faculty member, and he turned the faculty around completely within three days—on campus, completely turned around the entire faculty. And when we voted in physics again, the vote was in favor, and everybody was in favor, and he became president. So that's a tribute to Harold.

ERWIN: That is. Well, of course, that was still at the height of the Vietnam War.

GOODSTEIN: Yes, 1969, that's correct. The other thing I remember is that—and I think I've got this straight—there was an academic convocation held to welcome and install the new president. It was in October. And in the week before that, it was announced that Max Delbrück had won a Nobel Prize, and that morning it was announced that Murray Gell-Mann had won a Nobel Prize. So we had two brand-new Nobel Prize winners at Caltech to celebrate Harold Brown's installation as president. It was a truly spectacular afternoon.

ERWIN: Yes. Well, what were some of the things that Harold Brown did to change the institute?

GOODSTEIN: I was in too junior a position at that time to really perceive that. I had recently come as an assistant professor, and I didn't see it. Harold, today, has a legendary reputation for having given us a lean, mean administration. He came after the DuBridge years, which lasted for quite a long time—from 1946 to 1968. And what needed to be done, I just don't have the sort of the high-level view of the institute at that time to be able to say very much.

ERWIN: Well, do you remember discussions about revamping the humanities at Caltech?

GOODSTEIN: Yes. Harold, I believe, wanted to leave as his legacy a PhD in economics, social sciences. This would have been the first PhD outside of science at Caltech and it created a big uproar in the faculty. I was actually one of the leaders of the uproar.

ERWIN: Against, you mean?

GOODSTEIN: Well, I was on the Graduate Studies Committee, I guess, at that time—one of the committees that had input on this. And we brought in outside people in economics and asked them what were the ten best economics departments in the country, and they never named Caltech as one of them. If you took any other department at Caltech, it would have been named as one of the ten best. So that was the principal issue, as I remember. But Harold got what he wanted anyway. We have a PhD in social sciences.

ERWIN: Right, but it's not just in economics.

GOODSTEIN: We kept saying, "This is a department of economics, and it's not one of the better departments here." And they kept saying, "No, it's not a department of economics. It's a department of the social sciences, and it's the only one, so it is the best." [Laughter]

ERWIN: I see. [Laughter] A rose by any other name.

GOODSTEIN: Let me tell you a story. I had a number of contacts with Harold over the years, but one in particular. I had come to Caltech and joined this low-temperature group, and I was not the only one; there was another assistant professor in the group, and he decided to leave—he probably was prompted to leave. He left behind one graduate student; Stephen Rockwood was his name. Stephen Rockwood had been in the air force ROTC in college, so he owed the air force four years after he got his PhD; it had been deferred because of his PhD. While he was the graduate student of my colleague, my colleague kept writing letters to various lieutenant colonels in the air force, trying to get him a decent appointment when he got out—get him a research appointment rather than being head of an officers' club or something like that. But he never got anywhere, just got the runaround for all these years, and suddenly Rockwood was my problem. And he was air force material: He was straight as an arrow, and a pretty sharp kid, very smart, but also—

ERWIN: He was in the right place, going in the right direction.

GOODSTEIN: He was in the right place, going in the right direction, yes. He wound up many years later as head of the laser division at Los Alamos, so he's had a very successful career. But in any case, we wanted to get him a good initial position, so I was trying to figure out how to do this, and my wife Judy said to me one night, "You know, Harold Brown was secretary of the air force. He must have some influence in the air force. You should go and ask him."

So I made an appointment to see him. This is now in old Throop Hall. When he first came, his office was in Throop.

ERWIN: So this is early on.

GOODSTEIN: Yes, before the '71 earthquake. So I made an appointment to see him, and then I struggled and struggled on how to put this. I didn't want to walk into his office and say, "I want you to use your influence in the air force." I had to put it in some other way. So I finally came up with my opening line. I went into his office and I explained the situation—that we had a student who was just about to graduate with his PhD. And I said, "I would like to ask you to help me to get him a position that would be best for him and best for the air force." And Harold

said, "You mean you want me to use my influence?" [Laughter] So I said, "Exactly." He asked me some questions and I told him something about Rockwood and what he was like. And then he called in his secretary and he said, "Get me So-and-so in the Pentagon." A few minutes later, the call came through. He got on the line, and there was some small talk about the Department of Defense being down at the moment, but the Russians would make a mistake and soon we'd be on top again—this kind of chat. And then he said, "OK, I have something I need your help on. I have this student." Harold had an excellent memory; he hadn't taken notes, and he just repeated back, verbatim, exactly what I had told him about Rockwood. Then there was a little bit more small talk and they hung up, and that was the end of it.

Rockwood had wanted to spend his air force career doing research at the Kirtland Air Force Base in New Mexico but hadn't been able to get anywhere with them. Early the next morning he came into my office with eyes big as saucers and said to me, "The commandant of the Kirtland Air Force Base just called *me* up!" Needless to say, he did just fine in his air force career.

ERWIN: Soon after that, you decided to write a book.

GOODSTEIN: Yes. I had been teaching a new course, of my own invention, called States of Matter. It was the graduate course that separated applied physics graduate students from physics graduate students. It's still being taught. In developing the course, I formulated a way of understanding phase transitions—especially so-called critical-point phenomena—that was vastly simpler than what theoretical physicists had been writing about in the open literature. But I didn't know what to do with it, because it wasn't really new, it was just a better way of understanding something that was already known. So I decided to write the book, *States of Matter*, and the part on critical-point phenomena became the sixth and last chapter.

ERWIN: You felt you could get it out better that way than just writing a scientific paper?

GOODSTEIN: Yes, I wanted to publish that.

ERWIN: But you felt this was also something special to your book.

GOODSTEIN: Yes. And I know that in all the experimental groups in the country in this field, that chapter in my book became so dog-eared that people couldn't read it anymore.

ERWIN: They had to go through that chapter.

GOODSTEIN: Yes, they had to go through that chapter, because it explained all the things that the experimentalists normally don't understand, because the theorists do it in some high-falutin' way. But this book is on an experimentalist's level.

Anyway, so I wrote chapter six, and I outlined the rest of the book, and I sent it off and I got a contract. The book came out in '75, so it was probably March of '73 we got the contract, with a promise to deliver the manuscript in March of '74. The contract called for a 500-page book, 225,000 words, to be delivered at the end of March of '74.

And so, during the ensuing year, my wife and our friends the Intriligators went off every Saturday and Sunday with the kids so that I could be left alone to write. Because obviously I was doing this evenings and weekends rather than the regular week, when I had other things to do. I delivered the manuscript two weeks ahead of schedule, which probably broke a record. [Laughter] I don't know anyone who's ever done that before. And it got published by Prentice Hall in hardcover.

ERWIN: And then reprinted. Were there any changes to the book? Did you update it at all?

GOODSTEIN: Only typos.

ERWIN: So it still stands today, in its original form?

GOODSTEIN: Yes, in its original form. It went out of print because of a change in the tax laws that caused Prentice Hall not to want to store copies anymore, so they just put it out of print and then they offered the remaining inventory to me for a dollar a book—it was a $30-odd book—which I took. I've forgotten how many books there were—some hundreds of books. I heard the story of Henry David Thoreau, whose first book was not successful, and he was returned 750

copies from the publisher. And he went around telling people, "I have a library with 800 books in it, and I wrote 750 of them myself." [Laughter]

So I bought all the remaining books, and they came in big cartons full of books. And then I made a deal with the Caltech bookstore that they would sell them—they would be the agent to sell them. And they would sell them for $24 a copy and give me $20. I made much more money from that than I'd made in ordinary royalties while the book was still in print. And then the last seventy or so books were sent off to somebody who distributes science books in the Third World.

Then there was the Dover edition [1985]. When we did *The Mechanical Universe*—this is jumping way ahead—we needed to find a publisher for the textbooks that went with the series. So I contacted many publishers to see who might be interested, and one of them was Dover. Dover wrote back saying, "We only do reprints, we don't do new books." So I wrote back to them and said, "Oh, I've got a great reprint for you." [Laughter] And they said, "Sure, send it in," and it had gotten good reviews, so they bought it. What they do is they just simply buy it for a fixed fee and after that they never again tell you what the sales are, or the royalty, because they own it. So, as I mentioned before you turned on the tape recorder, it was for many years in paperback and cost about $10 to $12, and then it kind of dropped out of sight. I thought maybe they'd gone out of print at Dover as well. But I got a catalog today, and now they've brought it back as a hardbound book at $42.

ERWIN: I think that would imply that it sells well.

GOODSTEIN: Well, I know people around the country, including Dudley Herschbach, the Nobel Prize-winning chemist at Harvard, who uses it as a textbook in his physical chemistry course. And Guenter Ahlers at UCSB [University of California at Santa Barbara]. So, yes, I know people who still use it. The amazing thing is that after more than twenty-five years, it still functions as an up-to-date textbook. I remember at the time there was a theory that if you make a book current up to time minus-*t*, it will become obsolete in time plus-*t*. So it got published in 1975, and I made it current up to 1967. I purposely did that, to give it an eight-year shelf life. And now it's had more than twenty-five years of shelf life.

ERWIN: Speaking of books—maybe a slight detour here—you came to campus after Richard Feynman gave his lectures on physics. But were you at all involved with the people who put the lectures into book form?

GOODSTEIN: Well, I wasn't involved in any way with the publication of the book [Richard P. Feynman, Robert B. Leighton, Matthew L. Sands, *The Feynman Lectures on Physics* (Addison-Wesley)]. But, of course, I did get to know the people and I told you that I started teaching physics, too, when I first came, in the fall of '66, and Feynman was still involved with us and used to have lunch with us. And [Gerry] Neugebauer was the one who was giving the lectures.

ERWIN: Yes. But there was a group of people—Bob Leighton, Matthew Sands—

GOODSTEIN: Yes, and Robbie [Rochus] Vogt [Avery Distinguished Service Professor and professor of physics, emeritus] was involved also.

ERWIN: —who were taking those lectures off the tape and making books out of them. I was wondering if you had any memories at all about that.

GOODSTEIN: No, I know nothing about any of this. That happened before I got here. But the one thing I did have to do with the book is that the publisher came out with a twenty-fifth-anniversary commemorative edition—those are the blue books—and I wrote a new preface to it. In fact, Gerry Neugebauer and I jointly wrote a new preface.

ERWIN: Do you want to comment at all on the legacy of those lectures? Or how they've been viewed in the physics world? From your point of view.

GOODSTEIN: When I asked Feynman what he thought was his most important contribution, he said those books were more important than anything else he did. He thought they were quite important, and they are. I remember one summer—we went to Italy for the summer, and I wanted to use my spare time there to prepare to teach the course that I was going to teach the following fall. I don't remember what the course was, but I do remember that I knew I needed to

have the Feynman books. It had been arranged that we were going to stay in the house of a scientist—a biologist—in Frascati, right on the border, on the edge of town, and I was absolutely 100-percent confident that the books would be in this house. And they were. That's just an indication: If you're a scientist in this era, you have to own those books.

ERWIN: Well, of course, now they're being published in audio form.

GOODSTEIN: Yes. I know of people who listen to them in the car.

ERWIN: Have you listened to the audio?

GOODSTEIN: Only the CD of the one that we turned into *Feynman's Lost Lecture*. When Judy and I looked at the five unpublished lectures to see if any of them were interesting, I listened to the five lectures then.

ERWIN: Yes, I think those were three review lectures and a couple of—

GOODSTEIN: Well, one on gyroscopes, which was sort of obsolete.

ERWIN: And then the one that you brought to life in *Feynman's Lost Lecture*. And the original title of that lecture was…?

GOODSTEIN: "The Motion of Planets Around the Sun."

ERWIN: Well, I guess we could think about the [Marvin L. "Murph"] Goldberger [Caltech president 1978-1987] years. I've marked the institutional history off according to the presidents.

GOODSTEIN: I have one small item.

ERWIN: You have something to insert before that?

GOODSTEIN: Yes, an anecdote, very small, on the Brown years. At a certain point, I was on a faculty committee, and it became my job—for whatever reason—to write the report of the faculty committee and its findings.

ERWIN: Do you remember the name of the committee?

GOODSTEIN: It had something to do with minority students, but I don't remember any of the details at all. All I remember is what I'm going to tell you. This is in the period now where Throop [Hall] is gone and the administration is on the third floor of the Millikan Library. I sent my report—it may have been an administrative rather than a faculty committee; it reported to the president. So I sent my report to Harold, with a little note, and the little note sort of made fun of the fact that I was a faculty member writing a report to the administration. There's always this interplay between the faculty and the administration—certainly a lot friendlier here at Caltech than at almost any other university, but there is always that friction. So I made some remark—it was really a funny remark. I have no memory at all of what it was, but it was clear enough that Harold knew what I was talking about, and he sent me back a little note saying, "Thank you for your excellent report. You have all the marks of a great administrator." [Laughter] And then almost immediately after that, he became secretary of defense. To have a note from Harold Brown saying you have a great future as an administrator, and here he is one of the most powerful administrators in the history of the world. I carried that around in my wallet for many years until it completely disintegrated. [Laughter]

ERWIN: But in fact you did move in that direction.

GOODSTEIN: Yes, but that happened a decade later.

ERWIN: There was something you said about administrative committees versus faculty committees. I'm not sure we've heard much about that in our oral histories. Maybe you could explain that a little bit.

GOODSTEIN: Yes. We have two parallel organizations at Caltech: One is the administration, the other is the faculty. And the faculty is organized into the Faculty Board, which meets every month, and the faculty, which meets in every term. In fact, there's a faculty meeting this afternoon.

ERWIN: Now, that's the whole faculty?

GOODSTEIN: The entire faculty. And the Faculty Board takes care of, more or less, the routine business of the faculty, and if there's a major policy decision it's usually made at the full faculty meeting.

ERWIN: Is the Faculty Board elected by the faculty?

GOODSTEIN: The Faculty Board is elected, yes; there's a ballot for the Faculty Board each year. And then there's a whole set of standing committees as prescribed in the bylaws of the faculty—they are very clear bylaws. And the members of these standing committees are also elected—at least in principle. In practice, you get a ballot and it says, "If you want to vote for everybody on the list, just check here," and almost nobody ever dissents from electing the nominees to the standing committees. So there are standing committees of the faculty, and they include the Freshman Admissions Committee, for example, which is a very hard-working and very important committee. You can find the list in the catalog of all those committees, and the charges to the standing committees are found in the faculty bylaws.

Then there's a whole set of standing administrative committees—which you can also find in the catalog—and they report to members of the administration. There are a number of them that report to the vice provost.

ERWIN: They are presumably appointed?

GOODSTEIN: They are appointed, yes. And these committees can include staff members, serving *ex officio* or just serving because they have been appointed. And all the committees—both faculty and administrative committees—generally have students on them.

ERWIN: Is that since a certain time?

GOODSTEIN: Yes. I can't remember exactly when that happened, but at some point we decided that it was a good idea to have students on the committees, and ever since then we've had students on all the committees.

ERWIN: I'm guessing it might be the seventies.

GOODSTEIN: It might be the seventies.

ERWIN: Well, that's helpful. You know a lot about the Caltech governance. We might come back to some of those points later on. Right around that time there's a change in presidency; that is to say, Goldberger had not yet arrived and, as we said before, Robert Christy was the interim acting president. You became the vice chair of the faculty. Was that an elected office?

GOODSTEIN: Yes, that's right. The Faculty Board has three officers, basically; these are officers of the faculty and of the Faculty Board. There's a secretary, who's responsible for taking minutes and distributing them. There's a vice chair, who is also chair of the Membership and Bylaws Committee, and that was me. And then there's the chair. So I became vice chair and then I became chair.

ERWIN: I think you were vice chair in '77, and in '79 you became chair.

GOODSTEIN: Yes. Each election was for a two-year period.

ERWIN: So, what did you do as chair?

GOODSTEIN: Tried to keep things as quiet as possible. [Laughter]

ERWIN: [Laughter] Were there any exciting moments?

GOODSTEIN: Not really. It was a pretty quiet period. I don't remember any controversy. I don't think there were any cases that went to the Academic Freedom and Tenure Committee during that period. I was fortunate enough so that the two years went by and I had an enjoyable time going to Palm Springs and some of the other perks that go with the position.

ERWIN: Well, what are some of the routine things that you do?

GOODSTEIN: You have to make up an agenda for each meeting of the Faculty Board and of the full faculty. So, whatever issues are around, they have to be considered, discussed. There's a Steering Committee that actually makes up the agenda. The Steering Committee is also chaired by the chair of the faculty. The vice chair really does nothing unless the chair is out of town for a meeting, in which case the vice chair substitutes.

Various issues came up. I don't remember what they were, and they weren't important enough. In later years, we had *real* controversy—things like the Arroyo Center—and the chair of the faculty had to run faculty meetings that were very tense and full of controversy. But nothing like that happened during my tenure. But things did come up that had to be dealt with. I remember that we would have discussions in the Steering Committee, and there was always a question of whether the Steering Committee should get into the substance of the issue or should just put it on the agenda. I always wanted to discuss the substance of the issue, because I wanted to hear what people's opinions would be in order to anticipate what was going to happen at the Faculty Board meeting—so that if there was anybody I ought to invite to that meeting, I would know who it might be, and so on. Various more-or-less routine issues came up and we handled them.

ERWIN: Right. So, obviously, different people had different ways of occupying the position and doing business.

GOODSTEIN: Yes, sure. One of the things that happened when I was chair of the faculty was, we used to in those days have an annual retirement dinner when faculty members retired. And the chair of the faculty was responsible for running that dinner—that was one of the responsibilities.

It was later discontinued when Sunney Chan [Hoag Professor of Biophysical Chemistry, emeritus] became chair of the faculty. He cut that off, and we haven't had it since then. But in those days, that was the tradition, and I was not opposed to the tradition. But I got sort of sucker-punched, because they changed the retirement rules after my first year as chair of the faculty. I can't remember in what way they changed it, but they changed it in such a way that instead of the usual three or four people retiring that year, seventeen retired that year.

ERWIN: So you had to do seventeen dinners?

GOODSTEIN: No, no, no. We had one dinner at which all the retirees were honored—if they wanted to be, if they showed up for the dinner. So I surveyed my seventeen retirees, and five of them wanted to show up for the dinner. Well, if you have five people being honored, and you ask their friends to get up and make speeches about them—show pictures and all of that—it would go on for six hours. You couldn't do it.

So I undertook to do the entire thing myself, which was a *lot* of work. But by doing that, I contained it to a reasonable-length presentation at the end of the dinner. So that was one big accomplishment. [Laughter]

ERWIN: [Laughter] Well, you won't have to do it again.

GOODSTEIN: That's right.

ERWIN: Were you at all involved in the selection of Murph Goldberger as president?

GOODSTEIN: No. That was done by, as I recall, a committee of faculty and trustees.

ERWIN: Did you know him at all?

GOODSTEIN: No. I didn't know him before he came here.

There's one thing that I'd like to interject, having to do with tenure, so let's go back. When I first came here as an assistant professor—that was after the year in Italy; I came back in

1968—Carl Anderson was the chair of the PMA [Physics, Mathematics, and Astronomy] division, and I remember having a discussion with Carl in which he said, "Assistant professors at Caltech have *de-facto* tenure. We don't turn people away. So you don't have to worry about it." So I went through my six years before they actually reviewed me for tenure literally not worrying about it. I mean, I did my thing: I wrote a book; I did my teaching; I ran my lab and did experiments, published papers. I did all the usual things, but not feeling that I really had to put on a show, because I'd been told it was not a problem.

Then the year I came up for tenure, Bob Christy was provost, and he told the chair of the physics division, which was Bob Leighton at the time, "You have three assistant professors coming up for tenure this year. You can have only one. You'll have to eliminate two of them." The three were Ed [Edward C.] Stone [David Morrisroe Professor of Physics], Frank Scully—who's a very good high-energy experimentalist now at Columbia University—and me. So all of a sudden what should have been routine became a question of which two of us were going to be eliminated.

ERWIN: And you heard this? Is that correct?

GOODSTEIN: Oh, yes. That was not kept quiet. There were certain things that were kept quiet, but this was not. Instead of being a routine matter, it became a nerve-wracking matter. But the physics department just stood up to Christy and said, "No, we want all three," and they got all three. They were very good in the physics department—they were mutually supportive.

ERWIN: In general, do you think that's true of most divisions?

GOODSTEIN: I can't speak for other divisions. I've been for all these years going to meetings with the faculty in physics, in which we usually discuss physicists and astronomers—not mathematicians. Mathematicians make their own decisions. And recently, Richard Murray [chairman of the Division of Engineering and Applied Science] has decided I'm a member of his division, too, because of my applied physics. So I've attended some meetings in engineering, and the style is a little different. But I can speak from first-hand knowledge only about those two divisions.

ERWIN: Other than that, your progress to tenure was smooth.

GOODSTEIN: Yes. Until almost the time of the review, I wouldn't even think about it.

ERWIN: And then you couldn't probably have done very much about it even if you'd wanted to.

GOODSTEIN: Right, sure.

ERWIN: So Murph Goldberger came. And he was, again, a physicist.

GOODSTEIN: Yes, he was the chairman of the physics department at Princeton.

ERWIN: Right. So when he came to Caltech, maybe we can start by asking whether his coming had an impact on the physics department and the physicists?

GOODSTEIN: Well, every president at Caltech had always been a physicist.

ERWIN: Well, that's right. Maybe in a way that's not a very smart question. [Laughter] We'll have to wait until we get to the next president.

GOODSTEIN: That's right.

ERWIN: But I think Goldberger did give some talks or speeches on how he wanted to redo physics at Caltech. Whether he really did or not is unclear.

GOODSTEIN: Yes. It's not clear to me either, actually.

ERWIN: So that wasn't probably a focus or a major outcome of his presidency.

GOODSTEIN: Right. This would have been what, 1978?

ERWIN: He came in ’78, in July I think he arrived, and would have taken up the reins at that time.

GOODSTEIN: Right. So I got to know him almost immediately because I became vice chair and chair of the faculty. So we had our interactions.

ERWIN: At this point, maybe we should talk about *The Mechanical Universe*. Because I think we’ve come to that place chronologically, and we’ll come back to Goldberger after.

GOODSTEIN: Yes, all right. When I was vice chair of the faculty, the chair of the faculty was Robbie Vogt, and Robbie said, “This is a big important position, and because it’s such a taxing position, I should get teaching relief.”

ERWIN: As chairman of the faculty?

GOODSTEIN: As chairman of the faculty. He was relieved of all teaching duties so that he could devote his attention to the big job of being chairman of the faculty. In fact, I talked Robbie into running for this position. He was reluctant to run for the position. I was on the nominating committee at the time and a member of the Faculty Board on the nominating committee. Our job was to choose—there was only one candidate; you run unopposed—so our job was to choose somebody to run for the position. And there was strong support for Robbie if he was willing. So I went and personally talked him into doing it. So he became the chair, and he got his teaching relief.

As soon as he stepped down as chair of the faculty and I became chair of the faculty, he became chair of the PMA division, and he called me into his office one day in 1979. We had been teaching from the Feynman physics books; we had been using them as textbooks ever since Feynman had given the lectures, from ’62 to ’64. And they had just gotten too hard. It was great for the teachers; I loved teaching from his books. But for the students—if you didn’t already know physics, trying to learn physics from those books…. Seeing physics with fresh eyes all over again, it’s wonderful—that’s why every scientist in the world owns a set of these books.

But to learn it for the first time from those books is just impossible. You basically need to know physics, in order to appreciate them.

So it was finally decided that we were going to give up the Feynman books and create a new course that was more like the conventional courses that are taught at other universities. So Robbie called me in and asked me to create the new physics course—at least the first year of the new physics course. And I said, "Robbie, when you were chair of the faculty, you asked for complete teaching relief. Now I'm going to be chair of the faculty, and you're giving me the hardest teaching job in the whole institution. Don't you think that's a little unfair?" [Laughter] And Robbie was unflappable. We actually cut a deal in which he gave me a postdoc. He gave me full financial support for a postdoc so I could hire somebody to help me on my research group while I was doing all this. That worked out very well.

So that was my anecdote.

ERWIN: Right. So that was the beginning of *The Mechanical Universe* project?

GOODSTEIN: Well, what happened was, I taught Physics 1.

ERWIN: Your new course was called Physics 1?

GOODSTEIN: They never changed the name of the course; it was always Physics 1.

ERWIN: I see. But not from the Feynman books.

GOODSTEIN: But not from the Feynman books, right. We used some conventional textbook—I don't remember what it was, but I sort of redesigned the course. Among other things, one of the unconventional things in the course under the Feynman books was that electricity and magnetism wasn't until the second year, whereas everywhere else it's taught in the first year. So I put E&M back in the first year. Fluid mechanics, electricity, magnetism and relativity in the first year—that's what I taught, those were the subjects.

By the time I started teaching it the second time, I started to get worried, because there were only two possibilities: One is that I would go on teaching the same course forever, and the

other is that I would leave it and somebody else would teach it and it would become a completely different course, because there's no way of preserving the memory of the reforms I had instituted. One way of preserving memory is to write a textbook, but I had already written *States of Matter*, which is a graduate-level textbook. So I'd had that experience—you know, been there, done that. I didn't want to do that. And then it occurred to me that television was bound to play some role in the future of education. I didn't know what. This was all the way back in 1980, when television was different from what it is today. But it was bound to have some role in the future of education, and Caltech—as usual—should be a leader and not a follower in whatever role they play.

What I vaguely had in mind was that the lecture could be taped by a television camera at the back of the room. **[Tape ends]**

Begin Tape 2, Side 2

GOODSTEIN: …so that there would be some memory of what the course had been, so it wouldn't be forgotten. And that's really the only ambition I had.

So I went to Murph and I said, "Television has got to have some…." I said exactly what I said to you, and he said, "I'll give you a little money to look into it." So he gave me a little money—I think $50,000—and I hired a man named Don Delson, who was then head of AV here, audiovisual, to be my assistant in this. Don was superb. And we started poking around into what educational television was all about. We learned all kinds of things that I had no inkling of.

ERWIN: Did he have a background in television?

GOODSTEIN: No, he had an undergraduate degree in communications from the University of Florida. So in a sense he had an academic background, but not a professional background. He fairly quickly linked me up with stuff—now, the details grow a little bit hazy. But I do remember one morning sitting at home having breakfast, reading the *Los Angeles Times*, and reading a story that Walter Annenberg had given $10 million a year for fifteen years to make telecommunications materials for higher education.

ERWIN: Had given it to whom?

GOODSTEIN: Well, he created something called the Annenberg CPB Foundation. It's associated with the Corporation for Public Broadcasting—or was. And they created an office within the CPB, which was called the Annenberg CPB Foundation. And its job was to give out Annenberg's money.

ERWIN: He also created some schools.

GOODSTEIN: He did a number of things. He created schools of communication at USC [University of Southern California] and at the University of Pennsylvania, and various other things—including pulling the plug on the Annenberg CPB Foundation after nine years and giving the money to the historically black colleges and universities. That had to do with things that happened at Annenberg CPB long after I had anything to do with them.

What I can't remember now is whether Don had already gotten me in touch with Sally Beaty or not. But in any case, Sally Beaty was the head of the company that made educational television programs—it was a production company. I very quickly got in touch with Sally Beaty and with KCET, a local station, and we wrote a proposal.

KCET at that time was in deep trouble, because it was on the verge of going belly-up. And they tried to load the entire overhead of the station on our project. So the budget was just ridiculous. And on the day that the Annenberg board met to consider the first round of proposals, I got a phone call from them asking how much would it cost to do this if KCET were not involved. I gave them a figure which was about half the figure we had asked for, and they said, "Thank you," and we got the award—with KCET not involved.

In a way, it was a loss, because KCET was going to be our flagship station and they would have presented it. And now we had no flagship station, but we had the money to do the project. And that's the way the project began.

In the course of doing that, it was no longer a television camera at the back of the room. It was now a production, with programs, and all the production values, and all that stuff. And it all evolved into something far beyond anything I had imagined.

ERWIN: Well, who was responsible for that evolution?

GOODSTEIN: A lot of it was what I learned through Don Delson and Sally Beaty. Don found out about what goes on in educational television, and it turns out that there is what I would call—there was, and probably still is, because I haven't been doing it for many years now—what I would call an academic shadow world called instructional television, ITV. It doesn't exist at Caltech, but it does exist at many universities. The ITV department at a university typically has a catalog and it offers courses on television. The courses come completely packaged; they're broadcast from a local PBS station, or sometimes there's a local educational television station that will broadcast them. There's an instructor of record at the university. In some cases, the class actually meets on campus once a week; in some cases, they never meet on campus. There are packaged examinations; there are packaged study guides—everything you need to offer this course is presented to you by the company that makes the telecourse. You offer it in your catalog—you pay a license fee for doing it—and you get either tuition or fees from the state for having students who take these courses. Most of the professors at these universities are unaware of the existence of this ITV shadow world. And so there was already a well-established system—"system" is too sophisticated a word—but there was this group of people and a number of companies that made instructional television programs for the purpose of these ITV courses. And they had developed a fairly standard way of going about these things—you know, with advisory committees of academics and then there would be a producer. And typically the programs—you will be immediately familiar with this kind of program—consist of interviews. They'll sit an academic down for an interview for some number of hours, and then they'll take these talking-head interviews and just sort of sprinkle them into the programs. This is *very inexpensive* footage; you have to understand that. In making these programs, the way they actually stretch the budget is to use these talking-head things, or scroll on a picture—you know, move the camera instead of having anything else move, to make it more interesting. Everybody's seen documentaries like this. Typically the instructional television programs are made only of elements like that. They're as inexpensive as they possibly can be.

So Sally told me what it costs to make programs; she said it cost about $75,000 to make a half-hour program. So we put together a budget that called for that, and a certain number of programs—I can't remember how many programs there were in the original budget, but the original grant was for $750,000. We eventually got $6 million from Annenberg CPB. We went

back many times and ratcheted up the figure in various different ways, very much against their will. Instead of starting out making programs that cost $75,000 each, what we did instead—and it was part of the proposal, actually—was to make a pilot program that cost hundreds of thousands of dollars and took three years to make. And that was a make-or-break—either they accepted the pilot program or the project was dead.

ERWIN: Did the pilot program have a name unto itself?

GOODSTEIN: Yes. It became the second program in the series. It was called "The Law of Falling Bodies."

ERWIN: So it was incorporated into the series.

GOODSTEIN: Yes, it was incorporated into the series, although the original pilot was thirty-five minutes long and the programs had to be twenty-eight minutes and thirty-eight seconds long, each—that's the standard PBS program. So it changed a little, but basically it was the pilot program.

So we spent three years making this absolutely beautiful pilot program. And when we finished the pilot program—which cost $350,000, not $75,000—everybody loved it and they said, "Go ahead." So then we started making programs of that quality. And that meant not just slicing in talking heads and having pictures of things and then moving the cameras, but actually doing very classy computer animation and having actors. We had actors; we sent crews to Europe. We did all kinds of things you just don't do in educational television.

ERWIN: You were really taking this to a whole new level.

GOODSTEIN: We had taken it to a whole new level. And then we went back to Annenberg. First, it became obvious after a little while that we were not going to be able to finish the project—I'm getting ahead of myself.

One of the things in the original proposal was that Caltech would raise matching funds. So it would really be $1.5 million—they would put up $750,000. In the following year, Caltech

put in a lot of effort to try and raise matching funds, but this was a time when the economy had a downturn, and all the big companies that might have put up funds—like Phillips or Merck or various other companies that actually did these things in good times—were not doing them then. So we raised not a penny.

ERWIN: Who was doing the fund-raising for you?

GOODSTEIN: Well, the vice president for development. He was a professional fund-raiser. And he personally—I know—went to Phillips in Oklahoma, wherever they are headquartered, Tulsa, I think, because they made educational television before. He personally flew there to try and talk them into putting up money for this, and they just weren't doing any.

ERWIN: So this was high up in the administration?

GOODSTEIN: Yes, yes. It got good visibility. So we went back to Annenberg, to the foundation, and documented the work that had gone on to try to raise the funds without success. And so they kicked in the other $750,000. So that was their first unwilling contribution.

Then it became clear that we weren't going to make the original—I think we had originally proposed thirty programs—that we weren't going to make the original thirty programs on the $1.5 million. And since we were losing money on every program, I put in a new proposal to expand the series to more programs. In other words, I was going to make up, by increasing the volume, for the fact that we were losing money on every program. [Laughter] So we increased the project to fifty-two programs, to cover all of physics instead of just the first year of physics at Caltech, and that was accepted. So that bumped the budget up some more. I think that first proposal was sixty programs instead of thirty—double the number of programs.

Then we went back to them one more time, later on in the series, and said, "We can't do it. We can't do the programs at this cost." And in this one, I remember David Morrisroe played a big role in helping us do it. He was then vice president of business and finance. We said, "If you give us an additional million dollars"—we were at that point up to $5 million—"if you give us an additional million dollars, we'll make fifty-two programs instead of sixty programs." So we reduced the number of programs and increased the funding, and they were so angry about

this. And I said that the alternative was that we'd just make programs of a lower quality: "You can't make programs of this quality for the amount of money that you've given us." They were being blackmailed and they knew it, but the programs were really great, so they finally agreed, but they rewrote the contract. In the original contract, we were to share the revenues with them. They rewrote the contract to say that we couldn't share any part of the revenues until we had paid them back the new million dollars. A million dollars in educational television—it may as well have been $100 million; it didn't matter, because nobody ever made a million dollars on educational television.

Five years after the series went public, we got a letter from them saying, "You've crossed the threshold. You now share in the revenues." [Laughter] So we did it, actually. But by then, there were no more revenues. [Laughter]

ERWIN: So for you it was sort of a wash—I mean, for the program.

GOODSTEIN: Well, I didn't care about the revenues. That wasn't the purpose of doing this. What we wanted was to finish the programs and get them out there. And we succeeded in doing that. But it was an *enormously* difficult task. Later on, we applied to the National Science Foundation for an additional $3 million to turn out a high school version—that is, a version without calculus.

ERWIN: Oh, that reminds me. Was this really intended for a Caltech audience?

GOODSTEIN: No. I had to have a level—I had to have a definite audience—in mind, and my audience was high school physics teachers. There are 25,000 high schools in the United States, approximately, and the number of qualified high school physics teachers—nobody really knows what the number is, but it's somewhere in the range of 2,000 to 4,000. So most high schools do not have anybody qualified to teach physics, and physics is taught in most high schools. There are some 20,000 high schools that teach physics. And it's typically taught by— I thought it would be taught by the basketball coach [laughter], but that turned out not to be the case. Many of the so-called crossover teachers, who were teaching physics but weren't trained to do it, were home economics teachers. Home economics had fallen out of favor. [Laughter]

ERWIN: They didn't have anywhere to go. [Laughter]

GOODSTEIN: [Laughter] Or they had backgrounds in chemistry or biology or mathematics, but they knew nothing about physics, and my goal was to make it possible for them to teach physics. So the strategy was that the broadcast *Mechanical Universe* series that was funded by the Annenberg project was a college-level course—not Caltech-level, but college-level, university-level—including calculus, and then we would create the high-school-level course at a slightly lower level, so that the teachers who had learned their physics from the college-level course could then teach it at a slightly reduced level for their students in high school.

Now, it sometimes works that way. Very often, the better teachers actually show the college-level course to the high school students—particularly in AP [Advanced Placement] classes, where they do use calculus. We made the calculus pretty famous.

ERWIN: From what I know now, calculus is widespread, of course, in high schools.

GOODSTEIN: Yes, but at that time it was not required in most places—it was not even required at Caltech—and it was not so widespread. So we thought we would teach calculus as part of *The Mechanical Universe*.

Tom Apostol [professor of mathematics, emeritus; d. 2016], as soon as he learned that we were thinking about teaching calculus, immediately volunteered to become part of the project. He didn't want physicists teaching calculus. And he really made extremely valuable contributions. He made us do it right.

So we put together a team at Caltech that consisted of Tom Apostol, Steve Frautschi [professor of theoretical physics; emeritus 2006]—we were the academic team.

ERWIN: So that's one mathematician, two physicists?

GOODSTEIN: Right. And I advertised nationally for somebody to come as a hired hand, and I eventually chose Richard Olenick, who was a physics professor at the University of Dallas, which is a small college. So he came, with just bounding enthusiasm for doing this sort of thing,

and spent a couple of years as a visiting associate on the Caltech faculty but also as a fully paid employee for *The Mechanical Universe.* His basic assignment was to write the textbooks that went with the television series—he wound up being the principal author of the textbooks. Steve Frautschi's basic assignment was to write a higher-level textbook that could be used at Caltech. We had what we called the advanced edition and the regular book called *The Mechanical Universe*.

ERWIN: And you knew from the start that you were going to do a textbook to go with the program?

GOODSTEIN: That was part of the original proposal. There was a standard sequence of things that you did for educational television in those days, and one of them was you had to have a textbook. So we were fitting into the standard business of courses that could be taken off the shelf, that could be sold off the shelf. Now, I always knew from the beginning that *The Mechanical Universe* would not be that, because physics is just too hard to learn that way; you just can't do that. Nevertheless it had to fit the mold, because CPB Annenberg had that kind of evangelism for ITV. They were going to make ITV an alternative form of education, like the Open University in Britain.

ERWIN: Yes, where it's all telecourses.

GOODSTEIN: Right, exactly. But I had looked at some of the other telecourses made by Annenberg afterward, or made by other people before, and they are nowhere near this level.

The other member of the team, who was not then at Caltech but was at JPL [Jet Propulsion Laboratory], was Jim Blinn. And he was crucial—he was the animator. Somebody once said that of the six best computer animators in the world, three of them are Jim Blinn. [Laughter] He was really good.

ERWIN: How did you find him?

GOODSTEIN: He was the animator for JPL. He would do animations of their upcoming planetary encounters. They still do animations for the planetary missions. There was a computer graphics group at JPL. He probably wasn't the head of the group—he was never an administrator—but he was their leader. So I learned about him and got to know him and enlisted him in this project, and we paid JPL, just like anything else at Caltech—if they work for us, we pay them. We paid him out of *The Mechanical Universe* budget, and throughout the entire time, right up to the very end, he worked for JPL, on loan to us, basically. Computer animation is very important in *The Mechanical Universe,* because that's the blackboard—that's where you teach the physics. Everything else is window dressing; that's where the physics happens. I estimated by the end of the series that if we had had to farm out the computer animation to a computer animation house, we would have had to storyboard every frame for them; they wouldn't have understood a word of the physics we were trying to teach—and it would have cost, at the going rate, anywhere from $1,000 to $4,000 per second. The cost would have been on the order of $90 million just for the animation. It actually cost us about $1.5 million out of the $6 million. And furthermore, it wasn't necessary for me to storyboard every scene for Jim. Jim had a background in physics—he had an undergraduate degree in physics and a PhD in computer science. We got to the point where we were doing relativity—this is in the second half of the series. And I walked into his studio one day, and I said, "Jim, how are we going to visualize the Lorentz transformation," which was the essential picture of special relativity. And he said, "Well, you know, the way I've always visualized it is this." And he sketched out for me how he visualized it, and that became the animation. That particular program, on the Lorentz transformation, won the Japan Prize, which is the biggest prize you can win in television. But that was typical. I mean, instead of an animator who doesn't understand exactly what to do, Jim was a colleague. He had most of the visual ideas. It is true that in the early going I made very detailed descriptions of what the animation should look like, but I usually overdid it; that is, they were too elaborate, they didn't work well. Jim would refine it.

We worked together extremely well. He was the kind of a man—probably still is; he's a Microsoft Fellow now and lives in Seattle; I haven't spoken to him in a very long time—he was the kind of man who wasn't in sync with the ordinary world. He lived on something like a twenty-five-hour schedule, so his day gradually shifted around the clock. [Laughter] During the times when we were working on *The Mechanical Universe*, when I went to Italy—this was in the

early eighties—the Internet was something that the world didn't know about but the scientists did. So it was set up that they could send me scripts on what we now call e-mail—e-mail didn't really exist in those days. I would get the scripts electronically and then we would have conference calls and discuss them. And we'd have conference calls at three o'clock in the afternoon my time. That was six o'clock in the morning here, which was when Sally Beaty got to work and about when Jim quit work. [Laughter] So that was the only time when the three of us were all there. [Laughter]

Jim was the critical link in *The Mechanical Universe*. Everything else was redundancy—the producer, the directors, the production assistants, everybody else, maybe except for me. In the last two years of the project, we were turning out two programs a month, which is an absolutely breathtaking pace. He was turning out three-and-a-half minutes of animation for each program, and he was the kind of a man who, if he got too stressed out, would just take off work and commune with the spirits for a week. I couldn't let that happen; I needed him, because I had to deliver those two programs each month in order to get a check so I could pay the people who were working for me. Talk about running a small business, that was it! So during those final two years, I spent an hour with Jim every single day, seven days a week—just talking to him, just to make sure he knew that I cared about what he was doing. Of course, it produced the desired result.

ERWIN: Well, I think I know the answer to this question, but who wrote the scripts?

GOODSTEIN: I remember when we first got the grant, and we had to turn out the pilot program. I met with the people involved, and the crucial person, aside from Sally Beaty, was a man named Peter Buffa. Peter was the producer. The producer is the one who does everything—puts together the whole team.

I had had a Brazilian postdoc who had gone back to Brazil, and he wanted me to have what was called a cooperative US–Brazil research agreement. And so, during this period when we had just gotten the grant, I went off for two weeks to Brazil, and one of those weeks I spent in Rio. And I remember wandering up and down the length of the Copacabana, trying to figure out how the hell I am going to put "The Law of Falling Bodies" on television. How am I going to tell this story? I had never even seen a television script; I didn't know what one looked like.

Talk about not knowing what you're doing—I knew nothing! But mostly during those two weeks in Brazil I wrote the first draft of a script. I knew it was terrible—it was awful! Nevertheless, it was the first draft of the script. And I sent it back to Peter Buffa—or to Sally, who gave it to Peter—and I expected it to come back with some comment like, "We can't do anything with this." Instead, it came back with a comment from Peter: "If this guy"—he didn't know me really, we had met briefly once, so the comment was to Sally, who passed it on to me. He said, "If this guy is going to do this for every program, I'll think I died and went to heaven." [Laughter]

He had edited my script, but I was shocked at how little editing he had done. What he had done was—the first lesson he taught me about television—he had written transitions between scenes, because if you don't have a smooth transition from one scene to another, people will think they've accidentally changed channels. [Laughter] Which is literally true. I've seen it happen since then. But I didn't know any of these things. So he just smoothed the transitions between scenes—that's all he did in the first rewrite.

ERWIN: So you were a natural.

GOODSTEIN: Well, no, I wouldn't say that. Fortunately, the script evolved a lot after that—but that was the first step.

ERWIN: Was it just something you did instinctively or intuitively? Did you have a visual feeling?

GOODSTEIN: No, I did it as a teacher. You know, I'm a teacher—that's what I do for a living. I have an audience out there, and I know roughly the level of the audience. And I tried to put together a program as best I could under the circumstances. Later on, as the thing evolved, Buffa—who's a wonderful writer, in addition to being a producer—wrote scenes, particularly for the pilot program, because that was make or break; it had to be wonderful. And he wrote scenes that he himself couldn't produce! He wrote scenes that were too difficult to shoot. The opening scene, which went under the title, "The Law of Falling Bodies," was supposed to be of some window washers who were washing windows on a skyscraper but the trolley they were on was

defective and was just about to fall. And they were going to become the falling bodies. But he couldn't really shoot that. [Laughter] He wrote it, though.

So we do have an opening scene that does involve window washers and a skyscraper, but there's nothing wrong with their trolley. It's the title, "The Law of Falling Bodies," that actually falls—literally falls off the screen. [Laughter] But you see these bodies way up in the air, and you know what the joke is.

Fortunately, he and I have similar senses of humor, which was crucial. So we kept sort of bouncing off each other. He knew nothing about physics and I knew nothing about television—and that didn't get in our way at all; we were still good friends. He later on became mayor of Costa Mesa. He's a very good person, and he had a lot to do with the success of the program.

So eventually, when we got into action, we hired scriptwriters. I think we had six or seven scriptwriters and a story editor. Jack Arnold was the story editor—the story editor is the chief scriptwriter—and he would hire the scriptwriters. They didn't get paid an awful lot, but they got paid.

ERWIN: Now, these were people who were professional scriptwriters?

GOODSTEIN: Professional scriptwriters but not members of the guild—we were not paying union wages. But they would write the scripts and then we'd work on them.

The programs, which were half-hour programs, did not exactly match the lectures I had given at Caltech, because you had to divide up the content a little bit differently. So I wrote a series of what I call "shadow lectures," which was the lecture I would have given if I were going to cover that amount of material at Caltech. And the shadow lectures became the source material for the scriptwriters. But the fact is that it really didn't work, because the scriptwriters, with, I think, no exceptions, knew nothing at all about physics. They just simply couldn't do it.

So we instituted the practice, as well as we possibly could, that each program would have a storyline, and the storyline was meant to complement the physics, but it wasn't a physics story. Sometimes it was straight history; sometimes it just had to do with the history.

ERWIN: Well, what would an example of that be—like Kepler or Newton?

GOODSTEIN: In the program on Newton's law that was called "The Apple and the Moon," the basic question is, If gravity makes the apple fall out of the tree, why doesn't the moon fall? How can the same law of gravity explain what the moon is doing and what the apple is doing? It basically tells the story of how Newton figured out the answer to that question. So in order to do that, we had to have an actor play Isaac Newton, so that we had visual stuff. And the technique—or policy, almost, I would call it—of *The Mechanical Universe* was that we would have actors playing these famous people but we would never see them doing anything specific. We never saw Newton observe an apple falling from a tree, because we don't know that that ever happened. We didn't try to re-create any specific instance—no fictionalization. We just had an actor walking around in an apple grove and you saw apple trees and apples being collected, and so on. But the actor who played Newton never did anything but walk around, sit down, write in a book with a quill pen. It was just background material while we discussed what Newton had done. I remember I saw the raw footage from that particular program when we came back after the shoot. The shoot was done in Yucaipa, California, in an apple orchard. This was the young Newton. The older Newton was actually done in Cambridge; we sent the crew to England, and the actor playing Newton was sitting at a table with a servant serving him dinner while he's trying to work, and he waves her away—this, again, was nothing that had anything to do with physics, nothing specific.

But in Yucaipa there was this one particular scene in which the script called for Newton to come walking from off-screen right, along a path through the woods, and then leave off-screen left. And the narration had something to do with the path that Newton was taking. The path was metaphorical—the path through his thoughts on physics—but we have him walking down a real path. You know, that kind of thing. So they had Newton dressed in seventeenth-century costume—the young Newton, appropriately played by a twenty-five-year-old actor. You know, looked like he could have been Newton, with the wig and all this kind of stuff. And all you see are the woods and the path. But just hidden out of sight, barely out of sight, is a road. And Newton comes walking out, gets halfway down the screen, and a car goes by [laughter] and the director says, "Cut!" And then Newton comes walking out again, and another car goes by. [Laughter] So they had to shoot this scene a number of times.

The academic team worked in a house here, at 305 South Hill, and the production team took offices in Hollywood. It became clear pretty quickly that I had to keep the two teams

separate, because they were like oil and water—they didn't mix well. I had the flexibility so that I could deal with the Hollywood people; it wasn't a problem. But Tom Apostol would just bristle. He and some of the people in Hollywood hated each other. So we had these two sets of offices, and we did all the work in which we planned programs, met with scriptwriters, tried to come up with these themes for where the various programs would be set.

ERWIN: But now, are you not writing the shadow script?

GOODSTEIN: I wrote all the shadow lectures—they're not scripts. Then the scriptwriters would write the scripts. But then I would have to write all the technical scenes. So I wrote about half of every script.

ERWIN: *You* wouldn't, for example, have had the idea of Newton walking down the road? That was somebody else?

GOODSTEIN: That's right, that was somebody else, some scriptwriter. These things got massaged many, many times. All the scripts got written and rewritten and rewritten, over and over again. And then, in the technical part, it would call for computer animation, and Jim would do the computer animation. And the crews would go out and shoot the scenes. If they were historical, we might have an actor playing Galileo or an actor playing Newton.

ERWIN: And the producer was responsible for getting all that part together.

GOODSTEIN: Yes, the producer is responsible for getting all of that. Three separate times we sent crews to Europe to get footage of various things there. We had Galileo scenes done at the—what is the name of the library near South Central LA, with Renaissance architecture?

ERWIN: The [William Andrews] Clark [Memorial] Library?

GOODSTEIN: Yes, the Clark Library—that was the scene of a Galileo episode. Another Galileo episode was done in the organic chemistry library here at Caltech. And so on. Then, there were

some programs in which we didn't do that; instead, we had some storyline. The gyroscope program was about a little boy who has a flat tire on his bicycle and goes into a bicycle shop. We use bicycle wheels to illustrate various principles about gyroscopes—both in animation and in live action.

ERWIN: Now again, that would be your idea, though, right?

GOODSTEIN: Yes, I probably suggested the bicycle-shop idea, but a scriptwriter then took it over and wrote a script in which there's actually a story: The little boy goes in, asks if they'll fix his bicycle. They say, "Oh, sure!" He comes back and the bicycle is fixed. It's not a very sophisticated story, but it's a storyline that carries the program along. There's one piece of apparatus that we built to illustrate a gyroscope principle using a bicycle wheel, and so in live action we have the bicycle technician spinning the bicycle wheel and then disconnecting the apparatus in such a way that it becomes a gyroscope. I can't quite describe it to you, but I used this in a lecture that I gave last week—that apparatus. This is something you will never find in a bicycle repair shop, but nobody who sees the program realizes that. [Laughter] It seems so natural.

So….the script would call for scenes, footage, and animation. Eventually the scenes would get shot and the animation would get done. Of course, we did many programs in parallel—at the same time. On my blackboard, or actually whiteboard, in the office I had as director of the project at 305 South Hill, I had an elaborate diagram of all the programs—where they were, at what stage each program was, and so on. Then the basic footage would come in that was shot by the crews. And the animation would come in—that was always the diciest part. Then they would put together what they call a rough cut, and for the rough cut they would have one of the producers or directors read the script at roughly the pace of the real narrator. And then, with that voice, they would lay in the various scenes. This was all done in-house—on editing equipment they had at the production facility. Then they would come to me and I would go through the rough cut.

A number of important decisions had to be made at that time. One was, if the program was within one minute of being twenty-eight minutes and thirty-eight seconds long, then it could be adjusted by compressing or expanding various little things. But if it was outside that, then we

had to rewrite the script, and typically it was outside that. [Laughter] So a lot of rewriting had to be done after that point.

The second thing was that the animation was whatever it was. We never had the narration for the animation in advance, because I didn't know exactly how long the animated scenes were going to take. I would write narration for those scenes. The scenes had a numerical sort of clock on them, just frame notes, so I could refer frame by frame to what's being said while we're seeing this animation—exactly which frame should be up when a certain word was being said, that kind of thing. I would go through the animation, write the narration, and leave it for Jim, and then Jim would come in in the middle of the night, or whenever it was convenient for him, and he would look at my narration and see if he approved of it. He almost always did—I don't think we ever disagreed. And then finally, we would put together the final script, and the narrator would read the narration. The producer or director would actually direct him. It's just like shooting the scene. They would go over and over and over again, to get exactly the right intonation. Sally Beaty also did some narration on some of these programs; we had a female voice for certain things, and she did that.

ERWIN: Don't you appear onscreen as the teacher?

GOODSTEIN: I'm what's called the bookends. This is also pretty standard in educational TV. You have somebody who's the host of the program, who does the first two minutes and the last two minutes. And that was my job.

ERWIN: Really? So your voice—

GOODSTEIN: They would never put my voice on when my face was not on, because they said it would just be too strange—I didn't have a trained, professional voice. As long as you could see me, it was OK, so they would use my voice only when I was visible—or when you were looking at the students and you knew you were supposed to be hearing me. But at any other time, we had a professional. And the actor who did the voiceovers—Aaron Fletcher, who also played Galileo in most of the Galileo scenes—by the time we got to the end of the high school version, after the

college version, he was dying of emphysema. So the person who used to be the voice of JPL, Al Hibbs, did the voiceovers in the very last high school version, because Aaron couldn't do it.

ERWIN: How long did all this take, all told?

GOODSTEIN: The whole thing took five years—from about 1982 to 1987. But of those five years, three years were spent on one program, the pilot. And then the rest of it, once we got good at it, we really churned them out.

ERWIN: An assembly line, as it were.

GOODSTEIN: Yes. Fifty-two programs. Twenty-six hours of television. Seven-and-a-half hours of animation.

ERWIN: How did you do that? Did you have to stop doing everything else? Presumably you didn't.

GOODSTEIN: No, I didn't. Caltech, in the person of Robbie Vogt, who was by then the provost, gave me a position at my command; that is, I could hire a visiting professor. I could hire anybody I wanted, to come and help me keep my research group going and teach at Caltech. I had various different friends of mine come as visitors during that period.

ERWIN: For five years?

GOODSTEIN: It may not have been the whole five years; it may have been three years, I don't know. But anyway, it was very generous of Caltech to do that. I think there was only one year when I didn't teach. And in one of those years, we actually created a course called Scriptwriting for Television for the students. I'm always a teacher, so this was a chance to teach something new. I wasn't the teacher of record, because it was a course in the humanities division, and the journalism professor we used to have, Edward Hutchings, was the teacher of record on this course.

ERWIN: Now, what was *Beyond the Mechanical Universe*? That wasn't the high school version?

GOODSTEIN: No, no. When we first started talking about writing a proposal, very early in the process, I remember, I was walking down the Olive Walk with Sally Beaty on a nice sunny Pasadena day, and she said to me, "What are we going to call this?" And I had never even thought about that before, and instantly I said "The Mechanical Universe," because I was reading at that time a book called *The Mechanization of the World Picture,* by E. J. Dijksterhuis, one of those big, ponderous history books, so I just had that in my head. But *The Mechanical Universe* described mechanics only. So *Beyond the Mechanical Universe* was the second half—the second twenty-six programs. There are twenty-six programs on mechanics, and then the other twenty-six programs cover the rest of physics: electricity and magnetism, optics, thermodynamics, quantum mechanics, and so on. The fifty-two programs were designed with the idea of a thirteen-week television season, so there were four thirteen-week cycles—two programs a week for two cycles. Two programs a week for thirteen weeks, and then another two programs a week for thirteen weeks, and those were the terms of a college year. [Tape turned off]

DAVID L. GOODSTEIN

SESSION 3

November 25, 2002

Begin Tape 3, Side 1

ERWIN: I'd like to start by going back in time, continuing our discussion of *The Mechanical Universe* project. Perhaps you could tell me something about the experience of shooting the scenes you were in.

David Goodstein, on the set of *The Mechanical Universe*, December 1982

GOODSTEIN: All right. It was customary in these educational television programs—most documentaries, I guess—to have a host. And the host did what they call bookends—that is, the first two minutes and the last two minutes of each show. The producer, Peter Buffa, decided that I would be more comfortable lecturing to a class than just speaking into the camera.

ERWIN: I see. So as a host, you would be in class.

GOODSTEIN: The depiction would be that you, as the viewer, were actually in a Caltech lecture hall, and then that would fade into a normal television program. Then we'd come back to the lecture hall at the end, and I'd finish the lecture. Those are the bookends.

The first thing that had to be done was to choose a venue—a lecture hall. So I said, "Well, we should use the one at Caltech where I actually lecture." And Buffa said, "Oh, no, that doesn't look like a lecture hall." And I said, "What do you mean it doesn't look like a lecture hall? It *is* a lecture hall." Well, at the time, a very popular television program called *Paper Chase* was set at Harvard Law School, and so, as a television person, his concept of a lecture hall was the lecture hall at Harvard Law School in which *Paper Chase* took place. He wanted the place to look like that.

Well, finally, the logistics won. It was much easier to do it at Caltech than anywhere else around Los Angeles, and they didn't find any place that looked like what he thought a lecture hall would look like. So we wound up doing it in 201 East Bridge, which is where I lecture even today.

ERWIN: So authenticity won out?

GOODSTEIN: Yes, that's right. And 201 East Bridge was a place where famous physicists had been lecturing since the early 1920s. Every physicist of the twentieth century has lectured there, so it's an historic place. When they ripped the place up for the shooting, they put in fake wooden paneling to make it look more like Harvard. And I believe I'm correct in remembering that when Ed Stone, who was then the chairman of the physics division, saw the fake paneling, he liked it and had real paneling put in. Here's a classic example of life imitating art. Anyway, it was decided that we would do the lecture scenes in 201 East Bridge.

Then the next thing that had to be done was they had to get a wardrobe for me. So I was sent out shopping with this woman who had a sideline of dressing successful young lawyers—you know, buying their wardrobes for them. She would take me into a place like the old Bullock's, which is now Macy's—the men's department. She would tell them that she was going to buy clothes and that everybody else should get out, she'd commandeer all the

salespeople, and then she would talk to them about me as if I weren't there. [Laughter] This was an experience! But she did wind up buying me a number of outfits, and that was the only material thing that I ever got out of *The Mechanical Universe*. I wore *Mechanical Universe* clothes for years and years after that. [Laughter]

ERWIN: So you actually felt comfortable enough in them. They were you.

GOODSTEIN: Yes, they were nice clothes. And for each lecture, the producer would have a blackboard of instruction, including my wardrobe—what I was to wear. So there was a wide choice of things, because we had all kinds of combinations. He decided that I would always have either a tie open at the neck, with no jacket, or a jacket and no tie. So that was the degree of formality, or informality, that I was to assume for all these lectures.

ERWIN: But you could choose?

GOODSTEIN: Oh, no, no, no! I had no free choice whatsoever. He told me.

ERWIN: Even down to the details of which tie?

GOODSTEIN: Yes. And his favorite outfit for me was gray slacks, blue shirt, and maroon tie. And I had a feeling that he must have had a professor he hated in college who always dressed that way and he was going to get his revenge now. But anyway, if you look at the programs, you'll see I'm dressed that way in close to half of the programs, even though I had many other possible choices.

ERWIN: Did you wear makeup?

GOODSTEIN: Actually very little. They would sort of pat me down when I started to perspire and get shiny on the screen. They would pat me down with a little bit of makeup. But they didn't do any really serious makeup.

When you see the scenes in the lecture hall, you see two kinds of shots. And the television audience would be completely unaware of this. I know this, because I've spoken to thousands of people who've seen it and nobody's aware of it. There are two kinds of shots. There are shots in which you see me, over the heads of the students. And then there are shots in which you see the students reacting to something I'm saying—you hear my voice but you see the students' reaction, you see their faces. Those shots were taken at different times. All of my scenes were taken during the summers of 1983 and 1985 for the two series—*The Mechanical Universe* and *Beyond the Mechanical Universe*. And for those scenes, the third and fourth rows of seats were removed and replaced by tracks with a dolly and lighting structures. The remaining first two rows were filled by Hollywood extras who pretended to be Caltech students. Those were the backs of the heads. And then, on the day before regular Caltech classes began, some months later, they induced the entire freshman class to come in for four hours of audience reaction shots. They gave them free pizza.

ERWIN: So those were *real* Caltech students.

GOODSTEIN: Those were real Caltech students, that's right, and they sprinkled in the Hollywood extras, the ones whose faces you'd start to recognize. Because we also had an exit and entrance scene. Not only were the students sitting and watching me but at the beginning and end the students were walking into the hall or walking out, and you saw their faces sometimes. So he would sprinkle in some of those extras along with the real Caltech students. And then he would say, "OK, I want a small laugh on three. Ready? One, two, three." And everybody would laugh and the camera crew would be around to take pictures of various people as they laughed. And then when you see the program, when you see somebody laugh at one of my jokes, the laugh actually came months after the joke. [Laughter]

So we took the shots, as I recall, two programs a day—one in the morning and one in the afternoon. And we did this, I think, four days a week, or maybe three days a week, I'm not sure. It was very intense. And we shot all of the lecture hall scenes before any of the programs were shot, so that the programs had to fit the lecture hall scenes. The scripts would start with whatever I said in the opening and closing, and then the scriptwriter would have to fill in the middle, and I had written these shadow lectures that I mentioned, so we knew the content of all

the programs. Then I also had to figure out what would be on the blackboard at the end of a lecture about that subject, because we'd go through the opening scene, then I would fill all the blackboards, and then we'd go to the closing scene, and the blackboards were all full of whatever equations or diagrams I would have put down.

ERWIN: Did you write on the blackboard?

GOODSTEIN: Yes.

ERWIN: During the filming?

GOODSTEIN: No. Well, seldom—occasionally. It depended on what the director wanted.

ERWIN: But mostly it was just a shot of the blackboard filled.

GOODSTEIN: Yes, but occasionally I actually wrote something. Each scene was shot many times. I didn't have a script—I didn't work from a script. I worked from my own lecture notes, because everything I said on those openings and closings were things I had said in real lectures. But I did have notes, and I knew approximately what I was going to say. And it is really very strange, working with these people who are accustomed to working with actors reading from scripts rather than me sort of making it up as I went along. So I would speak for a while, and then the director would say, "Cut," and then he'd say, "OK, pick it up again from 'in between.'" And I'd say, "Did I say 'in between'? [Laughter] I don't remember saying 'in between.'" But we worked it all out.

ERWIN: Did you have to argue for that—to get to speak extemporaneously?

GOODSTEIN: No, that's what they wanted. And they only used one camera but they liked to cut close-ups, medium-range, and long-range shots. So we had to do each scene at least three times. If it went perfectly, we did it three times. But it almost never went perfectly, so we always wound up doing it at least ten times.

ERWIN: And you said the scenes lasted two minutes?

GOODSTEIN: Well, in principle it was two minutes, but sometimes it went much longer than that. And then they had to decide what to do. Once, when it went much longer, I was delivering sort of a biography of Isaac Newton that they wanted to use, especially because the program otherwise would have been too short. But they didn't want to have me on the screen for that long, so they had the professional narrator, who narrated the regular programs, read word for word what I had said. They scripted him to read what I had said extemporaneously, so it sounded to me a little peculiar. [Laughter] But I don't think the audience ever noticed.

It's amazing how little an audience notices about what's going on in a television program. The audience often was completely convinced that they were seeing me live, because I used to get phone calls. I got a phone call, I remember, one morning from somebody in North Carolina, saying, "Professor, I couldn't read what you drew on the board this morning. Could you write bigger tomorrow?" [Laughter] And we had shot that program five years earlier.

ERWIN: Right. [Laughter] Well, this is an interesting commentary on reality and the media.

GOODSTEIN: Right. And when we had the Hollywood extras standing in as students, these were really—well, some of them were students; my daughter actually played an extra in some of the programs; she was then college age. But most of them were experienced actors who played students, and they would refer to me as "the talent." [Laughter]

It was very hard work, figuring out what needed to go on the blackboard. I had a whole notebook full of things to put on the blackboard. We would do the first half, and then everybody would take a break. And I would have to fill up the blackboard, and then they'd come back and do the second half. And then they'd go for lunch, and then they'd come back for a taping in the afternoon for a different program. So it was very taxing, it was very hard work.

ERWIN: So it took, again, about how long? A whole summer?

GOODSTEIN: Well, as I said, we did two programs a day typically, and three or four days a week—so six to eight programs. And we had twenty-six programs in each series. And sometimes we had to do them over, because they didn't come out right. There was one that we never used. One of the things the woman bought me was a blue blazer, and we shot one scene in which I was wearing the blue blazer. And it turned out that with the blue blazer in front of the big blackboard, I just disappeared. So they couldn't use that scene, which was sort of a crisis, because we had shot exactly the right number of scenes. To reassemble everything to shoot one opening would have been a big, big problem. As it turned out, I told you that the way it worked was they put together a rough-cut program to see if it was too long or too short. And there was one program that came in *way* too long—about fifteen minutes too long. We decided, in that one particular program, just to cut out my "bookends" completely. And the reason we could get away with it was because in that particular program the theme was that I was a high school student, sixteen years old, in 1955, the year Albert Einstein died. And this whole scene took place in a movie, and the movie had one of those newsreels, and the newsreel told the story of Einstein dying. My son Mark, who was sixteen years old that year, played me at the age of sixteen. And since they already had me in the program, they didn't have to have me at the beginning and the end. So they just opened with Mark going up to the box office and buying a ticket. And so that's the way we got away with that one program. That blue blazer became my favorite jacket for many years.

ERWIN: Well, it sounds like it was a great deal of fun, even if it was hard work. Now, could you tell a little bit more about Jack Arnold's role? You mentioned him last time.

GOODSTEIN: Yes. Jack Arnold was the story editor, or chief scriptwriter, so he hired all the scriptwriters, gave them their assignments, and then he would polish all the scripts, give them a common language. I mean, you can recognize immediately the narration of *The Mechanical Universe*—it had a voice of its own. So he made a very important contribution to the project. Fortunately, Jack Arnold and Peter Buffa and I had similar senses of humor, so we agreed on the jokes and we played off each others' jokes.

ERWIN: How did Jack come into it? How did you find him?

GOODSTEIN: I didn't find him. He was hired by Peter, who had previously worked with him. In fact, I think that Peter had once worked for Jack on a different kind of project, and now Jack was hired by Peter.

ERWIN: You mentioned that one of the episodes won the Japan Prize. Actually, *The Mechanical Universe* won a lot of prizes. Would you like to comment on that? I wonder if some of them were more significant to you than others.

GOODSTEIN: The Japan Prize was easily the most significant. That came as a complete surprise; I had no idea that we had been nominated for it. I got a telegram saying that Prince Akihito—who's now the emperor—was going to present this prize in Tokyo. It was much too late for anybody to go and receive it, but we did eventually get the trophy and the scroll in Japanese.

ERWIN: Is there just one Japan Prize?

GOODSTEIN: No, there's a Japan Prize for television and a Japan Prize for various other things. And this is the Japan Prize for television.

ERWIN: And that was for "The Lorentz Transformation"?

GOODSTEIN: For "The Lorentz Transformation," right. And I believe it was for the spectacular animation of "The Lorentz Transformation." That was all the idea of Jim Blinn. It was his vision and he made it work. On a number of occasions, Caltech offered him a position as professor of computer science, because computer graphics is a big thing. But he didn't want to teach, he didn't want to be bothered; he wanted to do his own thing, so he didn't accept those offers. Then, after *The Mechanical Universe* finished and he started working for Tom Apostol for a while, he found that not as much to his taste, and he decided suddenly that he *wanted* to be a professor at Caltech. But by then they had hired two young people in computer graphics and it was the wrong time. So he left, and he left somewhat embittered. We had a going-away dinner for him at the Athenaeum just to say goodbye, and there was a question of what to give him as a

gift. It was very hard to imagine what I could give Jim Blinn that would matter to him as a going-away gift. But I knew immediately what to do. I gave him the trophy from the Japan Prize, which he had seen all those years in my office, and that hit home. That was a very successful gift.

ERWIN: What is a Cindy?

GOODSTEIN: Cindy ["Cinema in Industry"] is the equivalent of the Emmys, but for documentary rather than entertainment programs.

ERWIN: So that's a pretty big award?

GOODSTEIN: Yes. It started out originally having to do with films made for industry, but then it evolved into any kind of nonfiction, documentary work. We were nominated for various Cindy awards, and as you can see, we won a number of them.

ERWIN: Yes, there are four here.

GOODSTEIN: Yes. But the Cindys are arranged so that they have many, many categories and subcategories, each one with a gold, silver, and bronze. And the idea is that everybody who's nominated for one of these awards comes to the award dinner, so they give out a lot of Cindy awards. They're somewhat less special, but the gold Cindy awards were pretty good.

ERWIN: Yes. And I see that one of them was for "Effective Use and Design of Computerized Animation." And, again, "The Lorentz Transformation" received it.

GOODSTEIN: Yes. And we won at various film festivals and things like that. I think nobody had ever made an educational television series like that before, and nobody has made one since. And as I told you, I managed to squeeze $6.5 million out of the Annenberg CPB project, which they were extremely reluctant to give me, but I knew how to turn the levers. They then adopted a policy that nobody would ever get that much money out of them again. So they decided to do a

chemistry series that was consciously patterned on *The Mechanical Universe*—it was called *The World of Chemistry* and the host was Roald Hoffmann, a Nobel Prize-winning chemist from Cornell, a wonderful man. The chemistry project came to me for advice, and they wanted me to be on their board. And CPB Annenberg refused to let me be on the board, for fear that I'd give them advice on how to get more money. [Laughter] And it was not a successful series. It just was not in the same league as *The Mechanical Universe*. Roald Hoffmann, for example, understood that perfectly. They just didn't have the means to make it the way we did.

ERWIN: Yes. Well, we could shift gears now, perhaps, and fill in some of the details of the late eighties here at Caltech—a little bit of institutional history, maybe, and personal history. You were chairman of one of Caltech's important committees—the Academic Freedom and Tenure Committee.

GOODSTEIN: Yes. Well, fortunately on my watch we didn't have any cases—I'm pretty sure that that's true. That committee doesn't even meet unless we have a case.

ERWIN: So you did that for about two years?

GOODSTEIN: Yes. It's an elected position—you get elected, just like the chairman of the faculty. I did that for two years, and I think I was on the committee as a member for another two years.

ERWIN: One of the other big things that happened on campus toward the end of the eighties was the Arroyo Center controversy. Would you like to talk about that—what you recall about it and if you were involved?

GOODSTEIN: Sure. I was not personally involved in it. I was not part of the administration; it was after I was chairman of the faculty and before I became vice provost [1987], so I had very little direct involvement. As I understood what happened, Murph Goldberger made a promise to the army to create for them a think tank somewhat like the one for the air force. And just out of his back pocket—Murph sometimes acted somewhat impetuously. He was talking with the secretary of the army and he said, "Sure, we'll do that for you."

Well, the faculty rebelled. I remember one faculty meeting in the Athenaeum. Donald Cohen [Powell Professor of Applied Mathematics; emeritus, 2003] was chair of the faculty then, and he ran the meeting. There was a very serious discussion. Caltech has a thoughtful, responsible, serious-minded faculty, and this was a thoughtful, responsible, serious-minded discussion, but it became very clear that the faculty wanted no part of this, so it was killed. And I think it did irreparable damage to Murph; I think that was the end of his presidency.

ERWIN: Really? So the fact that he left fairly soon afterward was connected to that, in your mind?

GOODSTEIN: Yes. Now, I don't know the exact mechanism by which it worked, but I'm sure that that was the cause.

ERWIN: On what basis did the faculty come out against this?

GOODSTEIN: We were willing to do research for the Department of Defense. There's quite a lot of it, although it's by no means our principal funding here, but we have many projects for the Office of Naval Research or for the air force or the army. But to have Caltech's name associated with a center like this—that is, a think tank for the military—just didn't sit well with Caltech faculty. They felt—as I recall—that a lot of it would be social-science research, which did not have the same degree of accountability that mainline science research has. And Caltech was rightly very jealous of its reputation; our continued success depends on it. I remember from the discussion that people could just imagine the Arroyo Center advising the army to do some stupid thing that would eventually wind up being blamed on Caltech.

ERWIN: Moving toward your assumption of the office of vice provost—that happened in 1987. Now, just as a prelude to that, there'd been a lot of tension—and it's documented in a lot of places—between Robbie Vogt, who was the provost under Murph Goldberger, and Goldberger himself. Did that have a fallout on campus? Did people even know about it?

GOODSTEIN: I think people knew about it. And if I remember correctly, Murph got rid of Robbie before the trustees got rid of Murph.

ERWIN: That would seem to be the case from the chronology. On February 13, 1987, Vogt's resignation was announced by Goldberger, and then Goldberger, not very long after that, announced his own resignation.

GOODSTEIN: Right. They didn't get along, and I was not in the inner circle at that time so I can't tell you the details of why they didn't get along. I do bear some responsibility for this, because when they were searching for a provost, I remember giving Murph advice, telling him that he had to choose Robbie because Robbie was by far the most qualified person for the job. He had various objections—not having to do with what eventually drove them apart, but the fact that Robbie had problems and things like that. And I said, "You know, Murph, you really have no choice. He is the best." So I don't know how much influence my advice had, but whatever influence it had, Robbie became the provost and it didn't work out. It worked for a while, but it didn't work out in the long run.

Robbie, in my view, was an excellent provost. When I deal with administrative matters at Caltech, I see Robbie's footprints all over everything we do—over all of our procedures, over all of the way we do appointments, the way we do reviews. It was all done by Robbie. He really put the academic administration of Caltech on a rational footing.

ERWIN: So that was an improvement over what had been going on?

GOODSTEIN: Yes, yes.

ERWIN: He was provost from 1983 to '87. He was replaced by Barclay Kamb [Rawn Professor of Geology and Geophysics, emeritus; d. 2011].

GOODSTEIN: Yes. Murph left and Barclay became provost almost at exactly the same time. Barclay announced that he was looking for a vice provost to take the place of Chuck [Charles D.] Babcock [professor of aeronautics and applied mechanics; d. 1987], who was ill, or perhaps had

already died—in any case, he was no longer vice provost. And I had a very big hole in my life, because *The Mechanical Universe* was finished. I had done research and taught during all that time, so it wasn't as if I was going to go back to being a professor—I had always been a professor. But I had run the enormous project and now it was gone. So I thought I would like to do something else. So I went to Barclay and I said, "If you're looking for somebody, I'd be willing to do it." He immediately accepted my offer and I became vice provost. I actually overlapped, very slightly, with Murph, in the sense that Barclay, not having a president at the time, conferred with Murph on my salary. [Laughter] He told me that.

ERWIN: So that might have been Murph's last act.

GOODSTEIN: It could have been Murph's last act. I have to say that I have enormous admiration for Robbie and for Murph, both. I don't really understand fully why they couldn't get along with each other, but I don't have any problems with either one of them.

ERWIN: So you started being vice provost and still are. And how is that position defined? I wonder if it has changed during your time.

GOODSTEIN: People often ask me, "What does a vice provost do?" And I always tell them that the influence and prestige of the position arises from the fact that nobody knows what a vice provost does. [Laughter]

ERWIN: So if they don't know, you can do anything you want. [Laughter]

GOODSTEIN: You can do anything you want. Neal [Cornelius J.] Pings [professor of chemical engineering and chemical physics, 1959-1981] was the first one, and he had the position for a good number of years [1971-1981]. It was long enough before my time that I don't know exactly what he did. I do know that we were in somewhat related scientific fields, Neal and I. In particular, there was one Gordon Conference in which Neal was an organizer and I was an invited speaker. Neal asked if I would go there with him, and I said sure. He said, "Well, I'm flying first class." And at the time I was an ordinary faculty member; I wouldn't be flying first

class. So he got permission from whoever was funding me at the time in the government for me to fly first class with him, but at my expense—I had to take it out of my own budget. [Laughter] David Morrisroe told me that Neal, when he traveled, always insisted on staying in a suite at the Pierre or someplace like that in New York. He always had grand ideas for himself. What he did exactly, I don't know.

When Babcock died, or left his office, they brought in somebody from JPL to fill in for a while as Robbie Vogt's assistant, and the only way they found out what it was that Babcock was doing was to look in his file drawer and look at titles of file folders. Nobody had any idea what he did.

Then I became vice provost. And it's one of these jobs that you define by doing it. So Barclay started giving me things to do that were things that somehow were his responsibility but he didn't want to do. The very first thing he handed me was the Ombuds Office. We had a woman who was the ombudsperson for the campus. In various ways, she was both an asset and a problem, so I became responsible for her.

After a while, she wanted some sort of an evaluation of how she was doing. Of course, by the nature of the job, everything she does is confidential. So I put together some sort of committee that ran an inquiry of people on campus, and what came back was disturbing enough so that she resigned. Then I had the problem of finding another person. And we did. She was a young woman. All I remember about her was that she talked like a Valley girl; she ended each sentence with an upward inflection so it sounded like a question, and that just drove me crazy. [Laughter] But she was taking a degree somewhere at the time and she wasn't going to be a long-termer.

In the meantime, we were required by law to have what's called a Staff and Faculty Consultation Center, which deals with problems of substance abuse, alcohol abuse, and various other things related to that—and we contracted that out to an organization and they occupied offices next door to the Ombuds Office. The employee they had running this office was a woman named Helen Hasenfeld, and she was very good. So when this second ombudsperson left, I talked Helen into moving over to Caltech and taking over the position—both running the SFCC and being the campus ombudsperson—and she has done that up until now. She is now phasing herself out and phasing in Janis Schonauer, who's also very good. Ever since we've had Helen, it's been a very successful operation. So that was the first thing.

Then, I can't remember the order in which things happened, but I can give you a kind of catalog of the various things I do. It won't be complete, because I'll forget something. There are so many things that inevitably I forget some of them. But the libraries report to me—that's a big one.

ERWIN: Now, was that a first with you? Has that been the case before?

GOODSTEIN: I simply don't know. The director of the libraries was Glenn Brudvig, and he predated me by quite a bit. The libraries had very serious problems with Glenn as director, and we eventually developed a sort of golden parachute and eased him out of the job. Then we had a national search for a librarian and chose Anne Buck, and she's been in charge of the libraries ever since. Inevitably, because of nepotism rules, the Archives had to be split out of the library at that point and report directly to the provost. That's why the Archives reports to the provost, and not to the libraries and not to me.

ERWIN: Right, because your wife is head of the Archives. At the time that Glenn Brudvig came, that was the time of the introduction of automation, which was a centralizing force in the whole library setup.

GOODSTEIN: Well, the Millikan Library was built around 1965, and that brought all of the—well, not all of the satellite libraries, because there are still satellite libraries—but it brought most of the libraries together in one building. But of course it was never a good library building; it was a terrible library building. Glenn's job was to automate the libraries; we were still using the card-catalog system, and his job was to automate the library. And he failed. He brought in the team he had used at the medical library at the University of Minnesota, and they tried to install their system. They had a system that, as I recall, with very clever programming could make use of a primitive computer. What happened with time was that we got more and more powerful computers and we needed less and less of that kind of tricky programming and more and more straightforward stuff.

ERWIN: Did that cost Caltech a lot of money?

GOODSTEIN: Yes, sure. So a fundamental decision had to be made soon after I became vice provost, and the decision was on whether to try to rescue this Minnesota system or just write it off as a bad decision and start over from scratch. And I decided to write it off and start over from scratch.

ERWIN: That was your decision?

GOODSTEIN: Yes, that was my decision—after consultation with a committee, but yes, basically I made that decision. I remember a faculty meeting at which one of our engineering professors got up and said, "Our library's in the thirteenth century." And our medievalist got up and said, "What's wrong with the thirteenth century?" [Laughter] That was John Benton—it's the only amusing moment I can remember from thirty-five years of Caltech faculty meetings.

Anyway, I did not want Glenn to be in charge of automating the library. I wasn't prepared to fire him either. He expected to be canned. Quite frankly, he expected to be canned. I wasn't really prepared to do that, but I didn't want him directing this project. He had an assistant library director named Min-min Chang, who was very strong, and I made her the director of the automation project. She did a very good job, and the job got done. So that's the library thing. We've had various problems with the libraries over the years, but you do what you can.

So that's one responsibility. The Industrial Relations Center was another one that I immediately got. At the time, there was an oversight committee chaired by Frank Marble [Hayman Professor of Mechanical Engineering and professor of jet propulsion, emeritus; d. 2014]. The IRC was supposed to report to the oversight committee, which was supposed to report to me. The oversight committee was extremely skeptical of the IRC, because it was something that existed for historical reasons only, and if it hadn't existed we would never have created it. So they were really looking for ways of putting it out of its misery, so to speak.

ERWIN: Could we insert a quick history here? Do you know when the IRC began and for what reason?

GOODSTEIN: I don't really know the early history. I know there was a professor of economics who started it, and I can't think of his name. And at a certain point, Nick [Gaylord E.] Nichols, who had been an administrator at JPL, was brought in to run it, and Frank Marble and this committee was put in charge. And they were at loggerheads, because Nick was trying to run his organization and the Marble committee wanted to do away with it.

I looked at it and decided that, first of all, it was probably the only profit center at Caltech; it was the only thing we did that ran at a profit. What they do is to run courses for technical executives, and they bring in outside lecturers. They run these courses and charge tuition. Two-, three-day courses, sometimes a week, and they market their courses, and it's a whole big operation. It turns out that in bad times—like we've had for the past couple of years—this executive training is one of the first things to go. So the IRC suffers a lot; they can barely keep their head above water. But in good times they do extremely well. It is the third largest executive-training school in the country.

So I looked at this operation and I said, "Well, it's true that we would not create this if it weren't already here; on the other hand, it *is* already here." It did run at a profit and it had put aside a very large reserve in case of bad times. And it gave us a window on a community that we ordinarily don't have a window on. In addition to the classes, they would also run these executive forums and monthly dinners at which they would have a speaker from industry, and the local executives—CEOs and so on—of local companies would come and have dinner at the Athenaeum. This gave us contact with people we otherwise would not have any contact with at all. I decided it just simply was not a bad thing; in fact, I thought it was a pretty good thing. So I dissolved the Frank Marble committee and told Nick to do his thing, and he's been doing his thing ever since.

You know, these are all matters—the libraries, the IRC, and so on—matters that are not really at the core of what Caltech is about, but they play a role and somebody has to pay attention to that. You don't want to distract the provost's attention by having him taking care of such things, because the provost has really important things to do. But it's not a bad idea to have somebody who can make decisions and who has Caltech's interests at heart and has the academic credentials. So those are the sorts of things that I do.

ERWIN: All right. Well, you became, as vice provost, kind of a point person, maybe, for questions of ethics and then that became a bigger part of your activity—the whole question of scientific misconduct.

GOODSTEIN: Yes, right. I started to realize very early on—1988—that the government was going to soon impose a rule that would make it necessary for every university that accepted funds, especially from the NIH [National Institutes of Health] and the NSF, to have regulations on what to do in case of misconduct. We had never had a case of misconduct at Caltech, and so we had no regulations.

ERWIN: Where did this come out of, this sudden government…?

GOODSTEIN: It came out of a number of high-profile cases, including the Baltimore case and the Gallo case, which were in the newspapers at the time. This is the 1980s, when all those cases came up.

ERWIN: A sign of the times.

GOODSTEIN: Yes, it was a sign of the times, with congressional hearings and all this kind of stuff. So it became clear that this was going to happen and nobody at Caltech knew anything about this, the slightest thing. So I decided I had better prepare myself to do this, and I read everything I could get my hands on to learn what I could about it, and there wasn't much.

ERWIN: Well, there were things like the faculty handbook.

GOODSTEIN: But there was nothing in the faculty handbook, not a word.

ERWIN: Nothing at all about ethics?

GOODSTEIN: Nothing, no. What there was was the AAU—the American Association of Universities—which had put out some sort of a handbook or report, and various other

Washington organizations of that kind who had put out things. So I drafted a set of regulations. [Tape ends]

Begin Tape 3, Side 2

GOODSTEIN: And I brought these regulations to the Faculty Board. And as Caltech faculty does, they took it very seriously. I got back suggestions for things to change, and some of them were excellent and I made changes wherever. I eventually brought back a final document, which the Faculty Board adopted, so that's the point at which it went into the faculty handbook.

ERWIN: Now about when—'88, '89?

GOODSTEIN: About '89, something like that, before the government rules came out. Then the government put out rules saying you had to have your own internal rules, but we already had them. And in fact, we had one case that came up before we had even adopted them. This is a case—I can't name any names, obviously, but a famous scientist, a Nobel Prize winner at a different university, had written a letter to the director of the NSF saying that a Caltech professor had committed fraud and shouldn't be funded by the NSF anymore. And he made fairly specific allegations about what the professor had done—basically, that he had claimed to have done things in theory, theoretical science, that this Nobel Prize winner, as a world famous theorist, knew could not have been done.

We had no regulations at all on what to do. But Tom Everhart, who was president at the time, knew that I had drafted these regulations, so I asked him what we should do and he said, "Well, we haven't adopted the regulations, but at least we have regulations. Let's follow them." And the regulations called for an inquiry phase—a very discreet inquiry, in which you try to find out whether the allegations are justified. And the inquiry phase leads to a decision about whether or not to hold a formal investigation. The inquiry is supposed to be conducted by the chair of the division in which that person resides. The chair of the division didn't know anything technically about the field of science that was involved, but it was fairly close to my field, so at the request of the division chair I conducted that first inquiry—the first inquiry we ever had. And it was very, very hard. I spent a solid month doing nothing else—just dropped everything and spent a

month conducting the inquiry. I interviewed people; I constructed chronologies; I read all the papers that were involved.

In the end, I decided that what had happened was that our professor had worked out the theory. And the theory made certain predictions. And the predictions were based on what is called, in science, an adjustable parameter—that is, something that's unknown so you choose its value in order to make the best possible predictions. And he had written a series of papers in which all of this was quite clear. Then he had what he considered to be a big breakthrough, and he had written another series of papers—he and his collaborators—in which they made even more enthusiastic claims and the adjustable parameter no longer appeared as an adjustable parameter; that is, they didn't explicitly tell you that there was an adjustable parameter involved in making these predictions.

The second set of papers had not yet got published. They had been written, though, and he'd gone around giving talks at various conferences and so on, and it was at a conference talk that the outside person had heard this and said, "Oh, no, I know that you can't do that."

It took a long time for me to figure this out, and when I did figure it out, what our professor said was, "Well, the adjustable parameter's already published. It's in the earlier paper, so I didn't think it was necessary to repeat it in the second round of papers." But I knew, from reading the second round of papers, that even if you had read the first round it wasn't obvious that the breakthrough had made it unnecessary to have an adjustable parameter.

So I wrote my inquiry report, in which I said that the problem here was really basically an excess of enthusiasm and not an attempt to deceive. But I recommended that the second set of papers be withdrawn and rewritten to make it clear that there was still an adjustable parameter to make the theory agree with experiment. And that was done, and everybody was satisfied, and that was the end of the case, of the investigation.

ERWIN: So the person who launched the complaint was informed of this and therefore withdrew?

GOODSTEIN: Yes. Well, he had written this letter making the allegation to the NSF. So we conducted an inquiry, and I believe that a copy of the inquiry report was sent to the head of the NSF which explained everything that had happened. And the decision was made that no further

investigation was necessary, that there had been no real misconduct, although there had been some misunderstanding and it was all straightened out.

Then we had a real high-profile case—involving [James] Urban and [Vipin] Kumar, two postdocs in Lee [Leroy E.] Hood's [Bowles Professor of Biology, 1977-1991] lab—which got into all the scientific press. By then the procedures were firmly in place and we followed them to the letter and Caltech was widely praised for doing a good job of handling a misconduct case where others had failed.

ERWIN: And what was the upshot of that case?

GOODSTEIN: They were both found guilty.

ERWIN: So that was really the first time that Caltech . . . ?

GOODSTEIN: It was the first time that we had a high-profile case at Caltech. It was the first time we had a case in which everybody knew who the players were and it was in the press, and so on. So, having done all of this, I became—to the extent that there was an expert at Caltech, it was me. I was just a little less ignorant than everybody else. [Laughter]

But when I learn something new like this, I always make it kind of academic; I incorporate it into my life as a professor. So we created a course that Jim [James F.] Woodward [Koepfli Professor of the Humanities; emeritus 2010] and I offered jointly, and we've been doing that for more than ten years now. We wanted to call it Scientific Fraud, but the faculty board wouldn't let us teach scientific fraud to students, so it's called Research Ethics instead. And over the years, I've written a number of articles, and Jim Woodward and I have written a number of articles jointly, and I think rather important articles, about misconduct in science. When people here talk about interdisciplinary research, they're usually talking about biology and chemistry. But we do physics and philosophy—that's, I think, *real* interdisciplinary research.

ERWIN: Who takes the course? It's an elective?

GOODSTEIN: Well, it's offered for advanced humanities credit for undergraduates, so the undergraduates take it for that reason. And the NIH has a requirement that NIH grantees, graduate students, get training in ethics, so occasionally the Biology Division has decided to make our course the training in ethics. In that case, we get a flood of graduate students. At other times they decide no, the graduate students are wasting too much time going to class, we'll do it as a luncheon seminar—so then we lose all the graduate students. But it doesn't matter; the undergraduates all sign up for it, so we always have a fairly large class. And I understand that as of this year the NIH is requiring that postdocs do this, so we might end up with a lot of postdocs.

ERWIN: Do you offer the course every year?

GOODSTEIN: We offer it every year, one term, so it will probably be in the spring term.

ERWIN: Another thing you've talked a lot about and written on is science education. One of my favorite articles is the one called "The Big Crunch," which appeared, I guess, in a variety of places.

GOODSTEIN: It appeared in a variety of places, yes. The first was *The American Scholar.*

ERWIN: There are really two aspects to it. First of all, there's the education of scientists, and the other aspect is the education of the public.

GOODSTEIN: Yes, those are really two different things.

ERWIN: Yes, they really are. But you've written about both.

GOODSTEIN: I've done both, yes. When I first became an assistant professor at Caltech, I realized quickly that all of a sudden it was more difficult for my students to find jobs than it had been for me to find a job, and I wondered why that could be true. Then I came across a book called *Little Science, Big Science*, by Derek de Solla Price, who was a historian at Yale at the time, and it pointed out that science had been growing exponentially since about 1700 until the

1950s, when he wrote this book. And that couldn't go on indefinitely, because if it did, every man, woman, and child in the country would have a PhD in physics by the middle of the next century. And I understood at the time—around 1970—that what had happened was that the exponential had saturated. Once the exponential saturates, you've got a big problem, because a typical professor in a research university, like me, will turn out something like fifteen PhDs in the course of a career. If you have an active research group and somebody graduates every couple of years, over thirty years that's fifteen PhDs. Now, if the majority of those PhDs really got their PhDs because they wanted to become professors in research universities, you obviously have a problem, because you've replicated yourself fifteen times, and that just can't keep going on—that's the engine of exponential growth.

So I wrote a memo to my colleagues on the Caltech faculty and I pointed out in this memo, or white paper, this whole story—the history of exponential growth in science and the fifteen-to-one business. And I said this can't go on and we're saturated and I think Caltech should set a dramatic example for the rest of the country by reducing sharply the number of graduate students we have and compensate by increasing the number of postdocs—soak up some of the excess pool and at the same time reduce the number we are turning out. Because assistant professors of physics know how to solve anything, right? So I was an assistant professor and I knew how to solve this problem.

Well, my colleagues read this white paper and they had a unanimous reaction to it. The reaction was, Yes, you're right about exponential growth. They couldn't deny that, because they'd all studied differential equations and they'd all learned that the positive exponential is the one you can throw away, because it's nonphysical. Nothing can sustain the positive exponential. They all understood that. But they said that the correct solution to the problem was for everybody else to stop giving out their PhDs and Caltech would go on doing exactly what it always did.

Of course, over the years, as I lectured and wrote about this, I got exactly the same reaction at every university I ever went to, so nothing has changed as a consequence, except that now it's no longer a new discovery; it's an old story.

ERWIN: Well, of course, it is a difficult situation.

GOODSTEIN: It's a problem, yes. But, you see, it's a problem that we in physics came to relatively late. Historians and English majors and all those people came to it much earlier. And our students are more versatile; they can find other kinds of work once they find out they're not going to be what they always wanted to be.

ERWIN: In "The Big Crunch," you talk about scientific literacy as well. I'm forgetting the exact term you used, but the fact that we have, in this country, the best scientists but the most miserably educated public—that paradox.

GOODSTEIN: Right. I quote the paradox of the scientific elites and the scientific illiterates. We have the best scientists in the world and a public that is abysmally ignorant of science, and it happens in the same system of education that produced all these wonderful scientists and all this terrible ignorance of science. And my answer to that is that our whole system of education in science is a mining and sorting operation. We scientists sort through the human debris that comes our way, looking for diamonds in the rough that we can cut and clean and polish into glittering gems like ourselves. We have no interest in the others; we just throw them on the slag heap. So the whole system is designed to produce exactly that result. This wonderful group of scientists and lots of illiterates.

ERWIN: But then you go on to remark that there's a problem with having a scientifically illiterate populace.

GOODSTEIN: Well, there is possibly a problem, in that they pay the taxes that pay for our research and maybe they ought to know something about what we do. But there's another, deeper point, and that is that if you reversed that situation and did what was necessary to have an educated population in science, you would probably also solve the problem of the overproduction of PhDs. There was a time, before the war in Europe, when a person who taught in high school would normally have a doctorate and would be called "Professor" and have enormous respect in the community. If you had people like that in our high schools today, we would not have all this ignorance of science, and we also would not have the problem of excess

production of PhDs. It goes against the grain to say we're educating too many people. What we should be doing is using those educated people in the best possible way. But we don't do that.

ERWIN: Right. I guess the system needs to be reformed from the top down *and* the bottom up.

GOODSTEIN: Yes, that's right. It'll take a long time to happen, if ever.

DAVID L. GOODSTEIN

SESSION 4

December 2, 2002

Begin Tape 4, Side 1

ERWIN: Last time we talked, we decided there were some more things to say about your vice provost duties. And let's begin with technology transfer.

GOODSTEIN: OK. Up until around the middle 1990s, Caltech was a very science-oriented, rather than technology-oriented, school. It still is—much more so than, say, MIT. In any case, we didn't think much about tech transfer. But we had, at the time, an intellectual-property lawyer named Mike Keller, who was active in trying to get people to patent things and maybe even looking for somebody who might be interested in licensing the patents.

ERWIN: What happened prior to that time? Certainly there were patents.

GOODSTEIN: We always had a patent lawyer and we always had patents—in fact, I patented something way back in 1969. But the situation with regard to patents in universities in general had changed dramatically in 1980, with the passage of what's known as the Bayh-Dole Act, which gave universities the right to patent and earn royalties on any inventions that were made using government research funds. The purpose of this was to prompt universities to get ideas out into the marketplace. It didn't take hold for a while, but eventually the universities started to realize that this was potentially a very useful thing to do, and so they became more active in promoting technology transfer. The Bayh-Dole Act is still in force.

We did not pick this up immediately, but by the middle nineties Mike Keller was very active in pushing us to do things. And I, for various reasons, was worried about the future of our funding budgets and so on. At the time, we had a little bit of royalties coming in, but it was only a few hundred thousand dollars. And I felt that we could build this up into something that could bring in $15 million a year or more, if we really tried. But I didn't want to change the nature of Caltech.

So the first thing I did was to draft a set of rules on conflict of interest. If we obeyed them, we could go into this technology-transfer business and still protect our core values.

ERWIN: Had there been a statement of conflict of interest?

GOODSTEIN: There was a brief one-paragraph statement on conflict of interest, but it really didn't say anything. And everybody was always afraid to write rules of conflict of interest, because they were always terrified they would leave something out. So if you look at conflict-of-interest rules at other institutions—for example, at the University of California, it's a book!

ERWIN: It's huge?

GOODSTEIN: Yes, it's huge. It's extremely prescriptive. And that's not the way we do things at Caltech.

So I drafted a set of rules, which were approved by the Faculty Board after some rewriting. It's called "Conflict of Interest, Conflict of Commitment, and Technology Transfer." It wasn't adopted exactly as I wrote it. I read it to the Faculty Board, and there was some discussion. I remember at one point I got a delegation of biology faculty members who came to me and they were very disturbed about one of the rules. I can't remember exactly what was disturbing, but I remember we talked about it for an hour and at the end of the hour, I said, "Oh, so you'll be satisfied if I just change this 'and' to an 'or'?" They said, "Yes, that would be fine." [Laughter] And that was it.

Anyway, so we did that. Then we formed an Office of Technology Transfer. We had a national search for a director of technology transfer. We put a tiny little ad, a quarter of an inch high, in the *New York Times* and the *Wall Street Journal*, asking for a person with technical and legal qualifications—a PhD in a technical subject and a law degree—and we got 450 applications. And these applications were very difficult to read. I can read an academic CV and I know in an instant what kind of person we're talking about, but these were people of completely different kinds of backgrounds. We eventually narrowed the pool down to six people, who were invited to the campus for interviews with the committee. And of the six, we

picked Larry Gilbert. Larry did not have a technical PhD, but he had a law degree and he had been working at MIT and at Boston University for a long time. And he accepted the job.

ERWIN: So he was the first choice, and he was the first person to occupy the position?

GOODSTEIN: He was the first choice, and he was the first person to occupy the position of director of technology transfer. He has built it up into an operation. I think the absolute number of patents we have is the third largest in the country, after the entire University of California system, which is number one. The second is either Stanford or MIT. But all these other places are much, much bigger than we are. Finding ourselves in that kind of category is really strange. We've spun off something like eighty companies, which is close to one per professor in those areas where applied research might yield patents.

There have been some problems, as inevitably there are, but by and large it's not my credit—it's Larry's credit. Larry has been a pioneer and a visionary and created something that I might have, at the time, guessed at the bottom line or something, on the order of $15 million a year, which is roughly where we are. But I didn't know how we were going to get there and Larry figured it all out and made it work. He made it work in the first instance by walking around and talking to everybody. He got to know personally all of the professors in the areas where this stuff is done. He gained their trust. It was all a matter of personal relationships. It's not a mechanical process.

ERWIN: So the Bayh-Dole Act was designed to encourage this. But were any limitations put on these activities—in the sense that if someone invents something using federal funds and makes a lot of money patenting and selling it, how does that work out between the government and the institution and the individual?

GOODSTEIN: The policy at Caltech is that if you make an invention, you're supposed to disclose it to Larry. There are a lot of technicalities. For example, we take a provisional patent on every disclosure, just to protect it. Then you have one year to decide whether to pursue a full patent application, and they make the decisions on what to pursue. Most of this is done by outside counsel; they have to farm it out. It costs us a certain amount of money to do this, but of course

it's a very good investment. But once the idea, the intellectual property, is patented, then it's ours and we can license it in any way we see fit. The Caltech policy is that if you invent something and you're the inventor, Caltech patents it and tries to license it. And if we succeed in licensing it, and there's royalty income, we will give you, the inventors, collectively, twenty-five percent of the royalty income. You also have the right, if you want to, to plow the twenty-five percent back into your own research. And if you do that, Caltech will match it, so you get upped to fifty percent of return—that's research funds rather than as personal funds.

It happens that just today I'm going to the Faculty Board with a recommendation to change that rule, for IRS tax purposes. It turns out that our faculty members who have done this are vulnerable, if they're audited, to have to pay income tax on the income that they could have had but didn't take. They could, if they wanted to, declare it as income and then declare it as a charitable donation to the institute and it would be a wash. Except that there are limitations on how much you can donate as a charitable donation. But they haven't done that. And because they haven't done that, I don't know exactly what would happen—whether they would have to file amended returns or pay penalties or what. And in some cases, the amounts of money are much bigger than they are allowed to give as a charitable donation—in which case they would be liable for a very big tax.

Plus, Caltech is obliged to report the income, and we've never done that. So the rule, as written now, gets us in trouble. So the change that I'm going to present at the Faculty Board two hours from now says that you're entitled to twenty-five percent of the royalties. If you wish to return any of that to Caltech as a charitable deduction, the institute will match it in a research fund. So that means that now you are responsible for declaring it as income, declaring it as a charitable deduction—which in most cases will be a wash—and the policy of matching at Caltech is still in effect.

ERWIN: Do you know if other schools do anything similar? Do you know how this works other places?

GOODSTEIN: It's different at every school. Every school has its own rules. There is no general rule. In most schools, it's much more complicated than Caltech. So in most schools, the royalty

income is divided between the school, the department, and the inventor in some complicated fashion.

ERWIN: You have some responsibility for the research programs at Caltech overall. Could you explain about that?

GOODSTEIN: Yes. In fact, I belong to the National Organization of Senior Research Officers—which means, as the vice president for research, or some other appropriate title, at each university there is somebody who is my counterpart who is responsible, has overall responsibility for the research programs. I go to meetings with these groups two times a year. The Office of Sponsored Research—which is currently under the directorship of Dick Seligman—reports not to the provost's office and not to me but to the vice president for business and finance. Nevertheless, he and I work together absolutely seamlessly. It just shows you that at Caltech, organizational charts are not really important. What's important is how things work.

There is a Sponsored Research Committee, which is a committee of faculty members that formally reports to me. He is an ex-officio member of that committee. The committee—or I—see every research proposal that goes out of Caltech before it goes out. Or after—usually after, because there's a disclaimer that if we don't pull this back within two weeks, then consider it to have been submitted. Because usually people don't submit proposals on the day of the deadline for submission. You can't hold them up to review them. So we send them off with this disclaimer. But the people at Caltech are so good at what they do that this is not really a very onerous task. Every once in a while, I've had to ask somebody to change something in their proposal, because for one reason or another we didn't want to put it that way, but it's very rare. Most proposals are just fine, and most Caltech people are very, very good at getting funds that they want for the research they want to do.

ERWIN: Then what are you looking for? What are the issues?

GOODSTEIN: Well, there are many issues, and I often discuss these issues with my counterparts at other universities. But the one that always sticks in everybody's craw has to do with charging overhead. If it's a research proposal to the federal government, then there's a standard fixed

overhead rate that's negotiated by each university. It's negotiated with an agency that oversees—each university has its own agency. Caltech's is the Office of Naval Research; we negotiate our overhead rate with ONR. Other places have other agencies.

ERWIN: Is that just a historical…?

GOODSTEIN: Yes. ONR used to have its western office right here on Green Street, so there was a close relation between Caltech and ONR.

So there are two problems—well, there are more than two problems; there are a lot of problems. One problem is that, according to what we say—that is, we, the university administration, say—the overhead rate that gets negotiated is unrealistic and doesn't really pay the real cost of overhead. In many universities, the overhead is treated as funny money—it's treated as just a kickback to the university to do whatever it wants with. But that's true for public universities, where the state has built the buildings and so on. At a place like Caltech, the labs, the buildings, and everything are built out of the same funds and we have to operate with those same funds. So the overhead costs are real. And, in fact, if you calculate the real cost of doing research, we lose money on every grant we get. We're subsidized by the government, though.

ERWIN: What is Caltech's rate right now?

GOODSTEIN: Right now, it's sixty-two percent—I think it just went up from sixty percent. Caltech's view is that we are doing research for the government and so the government ought to pay the full cost of doing research, including the indirect costs—things that would be extremely difficult to put on as line items in each budget, like the library. The government's view is that it is giving us money to allow us to do the research we want to do and therefore it shouldn't have to pay the full cost. And the two views are completely irreconcilable; they will never be reconciled.

Two other factors can make matters worse. One of them is that the faculty members go out and get a grant for $150,000, let's say, for a year. We take sixty-two percent of that and say, "That belongs to the central administration." So, of course, the faculty members want the overhead to be as small as possible. The university administration wants it to be as big as

possible. The government wants it to be as small as possible. So the administration is caught in the middle between its own faculty and the government.

We also have a number of granting organizations, foundations, that have a policy of not paying overhead—or at least paying no more than a small amount of overhead—for example, the American Cancer Society, which awards research grants but refuses to pay more than ten percent overhead. So the real overhead recovery, on the average, is much smaller than government—not much smaller, because only a small fraction of our funds come from that kind of source. But still, if we told a faculty member, "You can't accept this money because it doesn't pay overhead," we'd have rebellion. The faculty member would say, "I have a chance to get some money for this very important research that I want to do, and you're telling me that I can't take it because you don't get your kickback?" So all of this is part of the mix.

ERWIN: This seems to indicate that all the overhead money goes into one pool.

GOODSTEIN: Yes, it goes to the general budget.

ERWIN: So it's actually shared.

GOODSTEIN: Sure, because it pays for all of the expenses, everything, at Caltech.

ERWIN: In other words, if someone in biology gets money from the American Cancer Society…

GOODSTEIN: Well, "overhead" is by definition a pool. So we actually *do* make money on some divisions and lose money on others.

ERWIN: But in the end, though, Caltech does not make money.

GOODSTEIN: There are two different ways of looking at it. All the indirect costs associated with doing research at Caltech are not covered by the amount of money we get from the government. On the other hand, the marginal benefit of getting the next grant funded is probably positive. So

it's not as if we want to stop writing grants. But nevertheless we are subsidizing the government research from the endowment and out of tuition and so on—out of our other sources of income.

ERWIN: Maybe we could talk about your responsibility for the animals—kind of a fine line.

GOODSTEIN: Oh, yes, a great subject. Each institution that has experimental laboratory animals has to have an institute official who the government knows is the responsible person. A few years ago, the chair of the Biology Division—Mel Simon—was the institute official, but he was also an animal user in his research, so he had a pretty serious conflict of interest, just because of the way the animal facilities were run. So he asked to be relieved of that duty, and the government offices that oversee these things like to have it in the central administration anyway and not in the departments. So one day Steve [Steven E.] Koonin [provost, professor of theoretical physics] said, "Would you mind being responsible for the animals?" And I said, "Oh, sure." [Laughter] Big mistake!

It turns out that I've had to spend a lot of time dealing with animal problems. Not a small matter. The animal facilities, some of them, are in very good condition, but some of them were in very bad condition. I personally am liable to find myself on the evening news because some animal-rights group decides to hold a demonstration. The only thing that protects Caltech now is that the animal-rights groups don't know that we have animals. And there are various other problems that are associated with it. But by far the most intractable problems have to do with the history—things I inherited, that came from before I was in charge.

Essentially, there are two completely separate animal operations. One of them reports to what's called the Office of Laboratory Animal Resources—OLAR. It's headed by a veterinarian, Janet Baer.

ERWIN: Is she a full-time Caltech employee?

GOODSTEIN: She's a full-time Caltech employee. I think under the terms of the contract that was written before I had anything to do with this, she's permitted one day a week of consulting. So she does not work at Caltech all week; she's away on Thursdays. But she's a full-time employee in every sense of the word, and she has a staff, including a second veterinarian now

and a number of laboratory-care technicians. These range from people with undergraduate degrees in biology who have some skill to minimum-wage workers who essentially wash animal cages. Although they do tend to learn enough other skills so that after they work here for a while, they get stolen away. We have a big problem with turnover, and we've worked very hard to increase their salaries to make that stop happening. So that's one kind of problem.

But there are two separate organizations. The other one is an organization that reports to a woman named Shirley Pease, who is not a veterinarian but is an expert on a number of technical operations having to do with making animals—that is to say, making transgenic animals, breeding them, mating them, bringing them into being. And she is very good at what she does. Now, the problem here is that her loyalty is to the faculty members who created her operation.

ERWIN: And who created her operation?

GOODSTEIN: Mel Simon and a small number of people allied with Mel. Janet's responsibilities are, first, to keep the animals as healthy as possible so that we don't have rampant disease and things that would spoil everything, but second, to obey all the government regulations. Which is not always what the researchers want. An animal-care veterinarian is a very strange kind of person, because the veterinarian's job is to keep animals healthy, whereas the experimenter's job is to make them sick. [Laughter] There's a *real* conflict there. As I say, we have these two different organizations. If they'd been created intelligently, there would have been one organization with one person at the head of it, instead of two organizations with two people at the head. And they each have their loyal followers among the faculty, and neither one wants the other one to be in charge. After years of working on it, I now have them working together.

For example, they used to compete on charges—per diems—for the animal hotel. The faculty members have to pay to keep their animals in this hotel, a certain amount per day. Shirley's operation is a barrier operation. You have to actually change your clothes and shower and get into other clothes to get into it. And it's for transgenic mice, so everything obviously should be more expensive there. Janet's operation is largely outside the barrier. There were times when people were keeping their animals inside the barrier because it was cheaper than having them where they belong, which was outside the barrier. The barrier should have cost

more, but it was being subsidized by the division. Furthermore, Janet cannot go inside the barrier if she's been with other animals in the last three days—I think that's the rule.

ERWIN: It's a three-day quarantine?

GOODSTEIN: Yes, I think so. Or it may now be down to one day, because of the health of the overall stuff, but in any event she can't just walk anytime into the barrier facility. So the barrier facility is essentially unsupervised from the veterinary point if view. And she says that sometimes they don't tell her when animals are sick and they *should* be telling her. So there are all these little conflicts.

Now we've got it all working together, in the sense that all the per diems have been ironed out together. The barrier is slightly more expensive. They all move together, they change together. We have a meeting once a year in which we decide on next year's per diems jointly. I have them training each other's staffs, so that we don't have the professors thinking that Shirley's staff is more sympathetic to their experiments while Janet's staff is more alert to violations of federal laws. Instead, we do cross-training to make both staffs aware of both—what the researchers want and what the government wants. There is an institute Animal Care and Use Committee—every facility must have one. The chair of it is Jim [James H.] Strauss, a professor of biology—not much of an animal user; he seldom uses animals himself. And he's been heroic; he's just been wonderful. We have just applied for accreditation by a national organization that accredits animal users. If you're not accredited, then you're liable to have unannounced visits from inspectors from the Department of Agriculture. If you are accredited, then the Department of Agriculture, in general, uses its resources elsewhere, because if you're accredited, they trust you, so there's a big advantage to being accredited. Caltech has never been accredited. We have our site visit in August of this year. We got provisional accreditation, provided we clean up a number of things by December 4th, which is Wednesday, and I've almost got them cleaned up.

ERWIN: Two days from now.

GOODSTEIN: Yes, two days from now. So we're hoping to have full accreditation soon, and we will eventually get full accreditation.

ERWIN: This is something you have pushed, then?

GOODSTEIN: This is something Janet's pushed—I deserve no credit for it. Janet's pushed it and made it happen. Janet has done a lot of great things, but she has also alienated a lot of faculty by forcing them to do things they didn't really want to do. The problems are not with animals; the problems are always with people. [Laughter] But this has been a big thing.

ERWIN: It's very interesting. I hope this isn't a silly question, but are we just talking about mammals when you're talking about animals?

GOODSTEIN: Most of our animals are rodents—that is, mostly mice, a few rats. But we also have finches and owls, frogs, sea urchins.

ERWIN: So sea urchins would still come under all the rules?

GOODSTEIN: No, no. The federal rules that govern us and give out accreditation and all that kind of stuff, that's principally for vertebrates. I don't know, I think the birds and the frogs also don't count—there's some other kind of designation. We do have monkeys, too—a small number, but some. But the overwhelming majority of the animals we have are mice.

ERWIN: What would you like to talk about next? Students?

GOODSTEIN: OK, we can go to students. Sure.

ERWIN: SURF—Summer Undergraduate Research Fellowship.

GOODSTEIN: One of the first things that happened when I became vice provost is that Jim [James J.] Morgan [Goldberger Professor of Environmental Engineering Science, emeritus], who was at

that time vice president for student affairs, asked me to come to his office. We're good friends. And he said, "David, would you mind taking over the SURF program?"

ERWIN: It seems like a lot of people have come to you and said, "Would you mind taking over?"

GOODSTEIN: Yes, well, that's my job. I've always said my job in our administration is the utility infielder; I just do whatever it is that has to be done. The problem was that SURF had been founded [in 1979] by [then professor of chemical engineering] Fred [Frederick H.] Shair. And it's absolutely a brilliant program. Fred deserves every ounce of credit for doing this. And Fred is also a good friend of mine. But Jim and Fred just couldn't get along with each other. It was a sheer matter of personality—they couldn't get along with each other on a tennis court. It's not a political fight or anything like that, it's just that they rub each other the wrong way—it's as simple as that. So Jim said, "I just can't get along with Fred. Could you take this over? And you have to worry about it, because it's getting much too big."

I said, "OK, I'll take it over," and then I talked to Fred, and Fred said, "Well, we have students who want to do research. Are you really going to keep them from doing research because we have 150 people doing research? Is there any reason to limit the size of the program?" And I said, "Well, to tell you the truth, if you can raise the money, no, there isn't any reason." So now it's up to 400.

A couple of years after I took it over, Fred left Caltech to become the dean of science at California State University, Long Beach, and that left us with a crisis, because the SURF program, which he invented and shepherded into being, no longer had a director. It's typical of things at Caltech that a single faculty member will do something miraculous, but nobody else can step in and do that same thing. It's his creation.

Maybe I should say a few words about how the SURF program is organized. The SURF program is a ten-week-long summer program in which students are paid a stipend of $5,000 to do a research project. So, to start with, if they want to do a research project this coming summer, they have to find a faculty member who will be their mentor. And the student and faculty member, together, write a proposal for a project that can actually be done by a student in ten weeks—or at least sounds like it can be done in ten weeks. Then there is a committee of mostly Caltech faculty members who read all the proposals. And since the proposals are jointly written

by the faculty members and students, they're usually pretty good, so they approve about eighty percent of them. And the SURF program goes out and raises funds for those $5,000 stipends. It raises about a million dollars a year. Very often, if the SURF student's going to work for a professor who has grant money, then the professor will put up half the SURF, and the SURF program will put up the other half—that's one way of raising a lot of money. But a lot of it is just raised in the community by donors, people who endow SURF.

ERWIN: It's partially a private fund.

GOODSTEIN: Yes, a private fund. And they're trying to get it endowed, so that they won't have to raise funds anymore. They have actually built up about half of the endowment they need to endow it permanently. So that's the basic program.

ERWIN: Now, is this open just to Caltech students?

GOODSTEIN: No. It started with just Caltech students and then it expanded. We now take applications for students from outside Caltech. We have a special minority student program—primarily students from outside Caltech. Other adjunct students.

So Fred left, having invented this thing, and then the question was, Who should be the director? There's the SURF board of directors, including most of the rich donors who give money to it. And they wanted to have a national search for a director of SURF. In the meantime, there was this woman here who had come as the wife of a postdoc. She was working part-time, I think, in engineering somewhere. I don't remember the details—Carolyn would have to tell you all the details of this.

ERWIN: This is Carolyn Merkel.

GOODSTEIN: Yes. She then got some part-time work, working for the officers of the faculty. The officers of the faculty needed some secretarial help. Maybe that's all she was doing; maybe that was her job. And Fred came by and said, "Listen, would you mind helping us out with SURF a little bit? It wouldn't take you much time," and she said, "Sure." And that grew into a

full-time job. When Fred left, Carolyn said to me, "You know, I would really like to be the director of SURF." Now, here Carolyn came as a secretary—she had no college degree—but she was very smart and very able. I made her acting director of SURF and eventually talked the SURF board into making her the regular, full-time director of SURF. She eventually got a college degree as well—she got a bachelor's degree. So this is a remarkable success story—and truly a remarkable story, because she's built SURF into twice what it was when Fred left, and she's raised all of this money, and she's run the program for all of these years. It's just an example of getting a very good person in the right place. Nobody believed that an ordinary mortal could substitute for a Caltech faculty member. [Laughter] But that turned out not to be true.

ERWIN: That's a nice compliment to Carolyn. So it takes all year to do it, even though the students are just here in the summer. It's a full-time job—having to raise all the money and set up all the application process, and so on.

GOODSTEIN: Yes, she has to raise money all year. And she still does the work on behalf of the officers of the faculty. She has a small staff; it's called Student-Faculty Programs Office.

ERWIN: So your relationship to this is that she reports to you, but pretty much she runs it herself?

GOODSTEIN: Absolutely, yes.

ERWIN: What do you do exactly?

GOODSTEIN: Well, you know, I've always said that when people report to me, they don't really work for me, I work for them. Carolyn comes to me every once and a while and says, "You know, I need such-and-such," and then it becomes my job to go out and get her whatever it is she needs. Or she comes to me and says—as she did just a few weeks ago—"We're having a meeting of the Southern California Conference on Undergraduate Research"—which has been going on for ten years. It started at Caltech, and now this was the tenth meeting. And this is the twenty-fifth-year anniversary of SURF. So to celebrate it, we had it here at Caltech again, just a

couple of weeks ago. And she asked me if I would welcome the group. Carolyn is very polite, you know; very nice, and she asked me if I'd be willing to do it. But, in fact, you know, I work for her; she tells me what to do.

ERWIN: So you welcomed them?

GOODSTEIN: Yes.

ERWIN: You told me that another group that you also work for is what used to be called CCO, Campus Computing Organization, and is now called ITS—Information Technology Service.

GOODSTEIN: Yes. This was given to me as a responsibility back in the days when it was headed by a man named Charles Ray. I can't remember whether it was then called Campus Computing Organization; it probably was. CCO was left over from the days when Caltech had a mainframe computer and did batch processing—as every university did. That's the way it was done when I was a graduate student. It had evolved over the years; there was no mainframe anymore, and at a certain point Charles decided to retire.

ERWIN: Is this early in your tenure as vice provost?

GOODSTEIN: Yes, it was pretty early. So Charles decided to retire. Once again, there was some pressure to have a national search for a director of this organization. I had heard some pretty good things about one of his assistant directors—Rich [Richard] Fagen. So I did the same thing with Rich that I had done with Carolyn—I made him acting director, and I convinced the computing advisory committee, which is the administrative committee of faculty members that oversees these things, that Rich was a good enough director. And he became the director and he was the director for ten years. And he was superb. Then he left—he went to Idealab.

Then I did the same thing for John Dundas, who was Rich's chosen successor. And he became director of what was by that time ITS. And now Rich has come back, so now they're each director of half of it. But it no longer reports to me, because information technology has become such a big thing at Caltech that it became clear that we needed what would be called

anywhere else a CIO—a chief information officer. Barry Simon [IBM Professor of Mathematics and Theoretical Physics; emeritus, 2016], who's the head of the CAC [Computing Advisory Committee], pushed very hard for this. We eventually got Dan Meiron [Jones Professor of Applied and Computational Mathematics and Computer Science]. In order to do this job properly, you can't be like me. I use computers, but I don't *love* computers. You have to love computers to do the job properly, and Dan is a real computer buff, so he was an ideal choice for this position, and he became—I think they call him associate provost for information technology, or something like that. So I was very happy to have him take that over. That was another big drain on my time.

ERWIN: Well, what was the purpose of CCO when you first had the oversight of it? What was its job here at Caltech?

GOODSTEIN: Well, its job was fixing people's computers and installing computers and selling computers. At the time, we didn't have Wired—that's where you bought your computer. And eventually e-mail became a very big thing—that happened gradually—and so CCO became in charge of the e-mail. Computing evolved very rapidly. Also, site-wide licenses for software is really a big deal, because we're always sort of flirting with being illegal and having people to do software that was available on campus but might not be licensed in such a way that they were supposed to be able to use it.

ERWIN: Yes. Well, it was never clear to me exactly what CCO's turf was. For example, one always had the impression that a division, or a research group within a division, might just be going off on their own, completely….

GOODSTEIN: Yes, a lot of that happened. A lot of research groups had their own systems administrators. Eventually ITS created an office in which they would hire the systems administrators and rent them out to you, and then the individual groups had to decide whether they wanted to be part of the Caltech system or run their own. And the way ITS competed was by doing its job extremely well.

Then there was ACS—this was the Administrative Computing Service—and that had to do with the installation of Oracle.

ERWIN: That came into existence at what time?

GOODSTEIN: Roughly at the time when we started moving toward Oracle.

ERWIN: A new Business Services.

GOODSTEIN: Yes. It may have existed before then and then got used for that purpose. But because the administrative financial services had its own computer group, they became sort of competition for ITS. They didn't both report to me—one reported to the vice president for finance and the other one reported to me. So that was a problem for a while. And there were personality problems between these two organizations. Eventually we got that reasonably well ironed out.

ERWIN: Was computing, and the growth of computing, more fragmented here on campus than in other places, do you think? Or was this typical?

GOODSTEIN: Well, it grew up in a chaotic way, and I can't compare it to any other place, because I haven't been anyplace else. So I just don't know.

ERWIN: The tendency, though, has been toward a more rational organization, if you will—more centralized? Would that be fair to say?

GOODSTEIN: No. Computing has become less centralized, because we no longer have these mainframe computers.

ERWIN: That's right. But the administration of….

GOODSTEIN: The question is, How are you going to support computing? And do you pay for it out of overhead or do you put it on line items and on budgets. All of these questions have to be resolved in some way, and I imagine every university has its own way of resolving it. We've had different ways of resolving it at different times. We've tried out different things, and a lot of it is not at all simple or transparent.

For example, Caltech is better off if we pay $1,500—maybe much more than that; I don't know—but some amount of money to buy a site-wide license for some particular piece of software, like Office Suite from Microsoft. Caltech collectively is better off, because for every group to buy it and have it legally would cost many times that. But the central administration is worse off, because nobody pays us back the money we paid to license it, and we have no mechanism for collecting it, and so on. So all of this has to do with the way Caltech is organized. And Caltech, I have to say, is a much simpler organization than almost any other university.

Then, another thing that happened—not necessarily on my watch but while all the rest of this was going on—was that we got into the supercomputing business. Supercomputing is very different from ordinary office computing, or even ordinary calculational computing. And for a while—for eighteen months—we had the fastest computer in the world at Caltech. The idea of massively parallel computing was invented here at Caltech—and was invented by two people who couldn't stand each other.

ERWIN: And who were they?

GOODSTEIN: Geoffrey Fox and Chuck [Charles L.] Seitz. Basically what happened—and I was not part of this, and now I'm really speaking secondhand—but basically what had happened was that Geoffrey Fox was a high-energy theorist, a physicist, and he invented the basic idea of parallel computing. And Chuck Seitz invented the routing chip that made it possible to actually do this. And the town just wasn't big enough for both of them. [Laughter] Geoffrey was the pioneer and wanted to do big things with it, and Chuck said, "All right, you invented it, but now it's time for the big boys to take over. So get out of our way." [Laughter]

ERWIN: Are they both gone?

GOODSTEIN: They're both gone. Chuck went off to found a business, and the University of Syracuse built a big computer center for Geoffrey.

ERWIN: Yes. But is that computer science? As opposed to utility computing—or whatever it's called.

GOODSTEIN: Well, that's yet another thing. Now you're talking about things that are increasingly getting far from what I'm an expert in. But the computer scientists don't want anything to do with running actual computers. They're interested in the theory of computer science.

ERWIN: OK, but that's not what you meant when you were talking about Geoffrey Fox and Chuck Seitz?

GOODSTEIN: Well, Chuck Seitz *was* a professor of computer science. Then it was involved in computer science. But when we created a center for having these machines… [Tape ends]

Begin Tape 4, Side 2

GOODSTEIN: —the director of that [Center for Advanced Computing Research] was Paul Messina, who had a PhD in computer sciences, but the CS department wouldn't even think of appointing him as a professor, because what he did was actual algorithms and things that worked. They were interested in the theory of computers. It would have made life simpler in many ways if Paul Messina had been a professor—and he probably had the intellectual credentials to be a professor, but he wasn't doing what the CS people thought of as respectable computer science.

ERWIN: Well, we should, perhaps, leave computer science for another day. One of the other things I wanted to ask you about was your work on advisory committees. You've done a lot of different things and maybe we don't need to catalog them all, but perhaps you could talk about

some of the ones you like the best, or where you thought you were most effective. I'll let you choose.

GOODSTEIN: I'll say a few words about a couple of committees, two or three things. One is, I was on a National Science Foundation committee called CEOSE—Committee on Equal Opportunities in Science and Engineering—which is supposed to promote diversity and is responsible for people with disabilities, underrepresented minorities, and women. Those are basically the three areas it's supposed to concern itself with—getting them into science.

I was on that for three years [1991-1994]. We would have meetings some number of times a year in Washington and each director of the NSF would come and tell us about all of the wonderful programs that were going on. And this committee had been going on for twenty years, but the statistics, the numbers, had not changed at all in twenty years—the fraction of underrepresented minorities, the fraction of women, nothing had changed over the twenty years. So all they were doing was just spinning their wheels.

Then I became a member of the National Advisory Committee of the Mathematical and Physical Sciences Directorate of the National Science Foundation. And I served on that for three years [1997-1999], and the third of those three years I was the chair of the committee. The job of that committee is to give advice to the directorate on how it spends its resources and various other things. The meetings were interesting and useful. I learned a lot about how the NSF works and what's going on inside Washington. I don't know what contributions I made, except for one. There's something that nobody outside Washington's Beltway has ever heard of and that consumes everybody inside the Beltway. That's GPRA—the Government Performance and Results Act. It's designed to make sure that the agencies are operating efficiently and well and doing their job. And every agency had to come up with some system of self-evaluation.

The NSF had drafted a document about how it was going to evaluate itself, and I remember getting up and making a speech at the meeting. Typically the advisory committee would meet and the NSF staffers would be sitting in the back rows. And this document had been written by NSF staffers—I didn't know who they were, but they were, I'm sure, in the room. We were sent this document as part of the material we were to read. So I read it on the plane on the way to Washington, and when I got up I said, "I've never before in my life read thirty pages of

grammatically correct English prose that has no meaning whatsoever." [Laughter] So they scrapped that and came up with a plan that actually—

ERWIN: They didn't scrap you.

GOODSTEIN: No, they didn't scrap me; they scrapped that document. They came up with a new plan that actually was widely applied in government. They really did much better the second time around. The other organization that I've been a member of on behalf of Caltech is the board of directors of the California Council on Science and Technology. This is a very strange organization that was created by state law, that asked the research universities of the state of California to form a body to give scientifically sound advice to state government in California. But it didn't fund it [the council]; it just created it. And so it was created, and its members consisted of the University of California, Stanford, USC, Caltech, and the California State Universities—and we later added community colleges. These were the so-called founding members, but without the community colleges. When we added community colleges, we had big problems about what to call the people who actually paid the dues, and I suggested the name "sustaining members." So for the privilege of giving good advice to the state of California, Caltech pays one-eighth or one-ninth of the administrative budget of the council.

There is a council and there is a board. The board runs the business of the council, and I'm on the board. I'm now, in fact, secretary/treasurer of the board. So I sign the checks and everything.

ERWIN: And you've been doing that for quite a while.

GOODSTEIN: Ever since it was created.

ERWIN: Since 1989?

GOODSTEIN: That's correct. That's thirteen, fourteen years. Then there's a council, and the council has twenty-five people on it now, from both academia and business. It considers issues having to do with California. It does projects and issues reports and has 130 or so so-called

fellows, who are people they can call upon when needed. And when somebody in state government needs advice on some issue, CCST will get one of their fellows to come and try to help out. Or we write reports, funded by state agencies. So the state agency will fund them to do a study of some particular thing—solid wastes, for example. Or sometimes the CCST decides that it wants to study some issue and it raises money from a foundation. The Hewlett Foundation gave the money to study what was called the critical path, which was the study of how California's education works and why it's not supplying the number of engineers and such. California creates a lot of high-paying jobs, but California citizens don't get to take them, because we have a lousy education system. So it studied that problem and it came out with a report on that.

ERWIN: So all the way from that to really technical things—it could be environment, it could medical care.

GOODSTEIN: Yes. In fact, they just did a study of the electricity crisis of last summer. They haven't done anything recently on medical care, although it is an issue that's come up. Water is another big problem. And so on.

By not accepting any money from the state, it remains independent. On the other hand, it struggles to stay alive, because Caltech has to put up $70,000 a year to be a member of this and we don't get any benefit from it.

ERWIN: No, but it's good public relations—if anybody knew about it.

GOODSTEIN: If anybody knew about it, but nobody knows about it. [Laughter]

ERWIN: You're on the scientific advisory committee of the David and Lucile Packard Foundation [since 1988]. I think you mentioned something to do with fellowships.

GOODSTEIN: Yes. That committee's job is to choose the Packard fellows each year. Packard fellows are young people in their first three years of a tenure-track faculty position. Fifty universities are invited to make two nominations each, each year. The rule is that no more than

one from any one university gets awarded each year. So we, the committee, get a hundred nominations. In the first round, each nomination is read by four people. There are twelve of us on the committee, so that ends up being about thirty nominations. Then we rank them, and all the ranks are put together; and then we take the first cut of about forty. And then we go through them and pick twenty out of those forty to win the fellowships. The fellowships are $125,000 a year for five years, no strings attached, so it's very nice.

ERWIN: Well, you yourself have won some awards that I'd like to have you mention here. I remember when you won the Oersted Medal [1999]. What's the history of that award?

GOODSTEIN: Hans Christian Oersted was a Danish scientist, and in 1820 he discovered that you could make a magnet move by running electric current—he created a magnetic field, as we would say today. That was an extremely important discovery. And presumably because he made this great discovery in the midst of a demonstration lecture, it's awarded by the American Association of Physics Teachers as their most prestigious award—it carries $5,000. That's the biggest award; they have other awards which are smaller. It's a very prestigious award. And, of course, I had to give a speech.

ERWIN: You're in some distinguished company, because didn't Feynman also…?

GOODSTEIN: Well, the only other two people from Caltech who have won it were Feynman and [Robert Andrews] Millikan, so it's good company.

ERWIN: Yes, that's nice company to be in. Congratulations on that. And then, also, there's the John P. McGovern Science and Society Award, awarded by Sigma Xi.

GOODSTEIN: Sigma Xi is the scientific research society. I had never heard of the John P. McGovern award before I won it, and I don't really know exactly what it was for.

ERWIN: So it doesn't mean quite as much as the other one?

GOODSTEIN: Well, the previous winner immediately before me was Condoleezza Rice. And the winner the year after me was Roald Hoffman, who has a Nobel Prize in chemistry and is a splendid, vibrant speaker. So I'm in pretty good company there, too.

ERWIN: We talked before about the awards won by *The Mechanical Universe*. Well, we could finish up by just picking up some odds and ends. Anything else you'd like to talk about? One thing that does come to mind is your teaching—the fact that you've continued teaching, I believe, all the time without really any break at all.

GOODSTEIN: I have taught all this time. When I first came, I taught graduate courses in solid-state physics and seminar graduate courses in low-temperature physics, things to do with my own research. Then I became a little frustrated, because I didn't think that solid-state physics—I thought that you should talk about all the states of matter, not just solid-state physics. So I created the course called States of Matter, which we talked about. I taught that for a few years in the early seventies. Then I moved on to teaching a combination of the introductory physics courses, which were then being taught out of the Feynman books—which I really enjoyed. You always enjoy teaching out of the Feynman books, but being a student is terrible. [Laughter] And I taught a course called APh/ME 17, after we had created the applied physics option. It was cross-listed in applied physics and mechanical engineering—APh/ME 17, which is thermodynamics and statistical mechanics at the sophomore level. States of Matter is exactly the same thing at the graduate level. So I really enjoyed teaching that for three years or so, and then they asked me to revise the introductory physics and step away from the Feynman books. And that became *The Mechanical Universe*. So ever since then, I've taught only the first and second year of physics.

ERWIN: Well, you've had a lot of contact, particularly with undergraduate teaching.

GOODSTEIN: Yes. And I should say that I have my fifteenth graduate student now doing his final work, and I think probably he'll be my last. So I will have turned out fifteen graduate students. And if you go through the fifteen theses, you'll get the history of all the research that I've done.

ERWIN: What were some of the high points of your research?

GOODSTEIN: Oh, there are many.

ERWIN: Things that you were most satisfied with?

GOODSTEIN: Well, I did a lot of work having to do with the influence of dimensionality on the properties of matter in phase transitions. If the world were two-dimensional instead of three-dimensional, how would matter be different? Would there be solids and liquids and gases? What would the phase transitions between them be like, and so on. This was a big issue, and we made very large contributions to understanding that, and I think we largely have understood it. There are other things that probably we haven't understood, but that's the way things are in science. You never really understand everything.

For a while I was involved in some very beautiful experiments. If you heat something, the heat just gradually diffuses through it very slowly. You put in heat here, it becomes warm over here sometime later. But there are certain materials—and in particular, single crystal sapphire at very low temperatures—in which that's not true. If you put in heat, it travels away at the speed of sound. Because heat is sound—sound is a form of heat in a crystal. It's just crystal vibration, that's all there are. But if you put it in any ordinary matter, the sound immediately starts bouncing off impurities and grain boundaries and all kinds of things—or other sound waves, for that matter—and doesn't get very far, so it takes a long time for the heat to go anywhere. But in sapphire at low temperature, there aren't a lot of other sound waves around, and the material is so pure—it's a single crystal—that there's nothing for the sound to bounce off of, so it literally travels at the speed of sound. We did many beautiful experiments in which we'd fire off a burst of sound waves and then see them reflected from the far wall and come back. There were special directions in which the sound gets focused in a crystal. Then the question of how efficiently it bounces off the far wall depends on the properties of the far wall. And all kinds of experiments like that.

And then I've done a lot of experiments having to do with superfluidity in liquid helium. Right now, I'm involved in a big project to do a space experiment—an experiment on the Space

Station—to look at properties of the phase transition in superfluid helium from normal to superfluid phase. That would not be possible to study on Earth.

ERWIN: Were there some particular practical outcomes or applications?

GOODSTEIN: My research has never been of any use to anybody. [Laughter] I think I can say with absolute certainty that I have never done anything that turned out to be useful.

ERWIN: Well, you and Millikan.

GOODSTEIN: Yes. [Laughter]

ERWIN: Did you want to say anything more about the students you had? You said fifteen graduates?

GOODSTEIN: I've had fifteen graduate students, all of whom were absolutely outstanding, and many of whom are now professors at important universities, doing various things. I think I know where all of them are.

ERWIN: So you've kept in touch.

GOODSTEIN: I've kept in touch, yes. And as far as the undergraduate students are concerned, because I've taught the introductory courses, there were many times when every single student on the campus knew who I was, because they had all gone through my hands. Sometimes that's not true—sometimes I'd be teaching the sophomore class, so the freshmen wouldn't know who I was, and I'd switch from sophomores to freshmen, so that one class would pass through without having me at all. Things like that. But, you know, I'd like to think that I've had some influence on a very large number of Caltech students over the years—and of course, through the television series, on millions of others. And I keep hearing about that, because I get e-mail all the time, still. Just this week I got a request from a girl in Pennsylvania for an autographed photograph for her class.

ERWIN: Of yourself?

GOODSTEIN: Of myself, yes. I get that all the time. We keep a stack.

ERWIN: Well, you're a star!

GOODSTEIN: Oh, absolutely! [Laughter]

ERWIN: Well, maybe that's the ultimate. This has been a good interview. Thank you.

GOODSTEIN: Thank you very much.