



HAROLD BROWN
(b. 1927)

INTERVIEWED BY
ALICE STONE

March 19, 1990

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Subject area

Administration, physics, United States Department of Defense

Abstract

An interview in March 1990 with former Defense Secretary Harold Brown, Caltech's president from 1969 to 1977. He begins by reviewing his pre-Caltech career, his reasons for accepting Caltech presidency, his initial impression of Caltech's faculty, administration, trustees, and students.

He recalls establishing Environmental Quality Laboratory; 1970 admission of women undergraduates; proposed association with Immaculate Heart College. Comments on fund-raising, restructuring the development office, entertaining in the President's House, relations with Arnold Beckman and other trustees.

He discusses the roles of the provost, Institute Administrative Council, and division chairmen; his relations with provosts Robert Bacher and Robert Christy. Comments on successes of his presidency, including reducing Caltech's "insularity" and improving its financial health, its student life, and JPL-campus relations. Views on how Caltech should prepare for coming societal changes.

Comments on faculty's role vs. that of administration. His establishment of five-year review of division chairs. Caltech's process of moving into new research fields. Caltech's finances in the late 1960s and 1970s; fiscal conservatism of Board of Trustees; "flattening" of government support; establishment of campus master plan and Fairchild Scholars program.

He recalls Caltech's lack of adverse reaction to his defense department background. Notes matters left incomplete in his tenure: improving engineering division, introducing applied biology, strengthening social sciences. Comments on hostility of science and engineering faculty to social science. His views on "Big Science." Recalls his involvement outside Caltech presidency (SALT talks) and trustees' attitude toward it. He concludes by discussing timing of his departure.

Administrative information

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Harold Brown at Caltech freshmen picnic supper, 1971



Campus, on the news of Harold Brown's appointment as Secretary of Defense by President Carter, 1977

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH HAROLD BROWN

BY ALICE STONE

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Harold Brown
Pasadena, California

by Alice Stone
March 19, 1990

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BROWN: You asked me first for a brief review of my career before coming to Caltech. I grew up in New York [City] and went to Columbia University for my undergraduate and graduate degrees, all of them in physics. As a result of acceleration during the war years, I was not quite eighteen when I got my bachelor's degree in 1945. But then I was somewhat tardy in getting graduate degrees. It took me about four years more to get my PhD. That was in nuclear physics. The area I actually worked on was beta-ray spectroscopy. I stayed on at Columbia for a year as a postdoc and then came out to California in 1950. I went to work at the Radiation Lab in Berkeley and worked on a variety of projects that were generally related to the accelerator project that was going to make plutonium by bombarding uranium lattices with high-energy deuterons. That program turned out not to be a very good idea. But when the Livermore Laboratory was formed, in 1952, to be a second nuclear weapons laboratory, I went to work there as a group leader.

I worked my way from basic science into applied science and then into managing research and development. In 1959, I became deputy director, having been first a division leader and then an associate director. I became director in 1960.

I'd already had a fair amount of experience on the Washington scene, both as a member of various advisory groups, including the Steering Committee on the Polaris system, which was a ballistic missile launch submarine. I'd also been on a committee advising on the intercontinental ballistic missile program, and I had participated in the 1958 East-West conferences in Geneva on detection of nuclear tests and the 1959-60 Geneva conference on the limitation of nuclear testing. In 1960, I was named director of the Livermore Laboratory—by then renamed the Lawrence Livermore Laboratory. Then, early in '61, Bob McNamara and Ros [Roswell] Gilpatric, the secretary and deputy secretary of defense, asked me whether I'd be willing to become director of

defense research and engineering, which I decided I would do. I moved to Washington early in '61. By that time, clearly I was no longer a practicing scientist, or even just a manager of applied science, but more of a manager and administrator in general. I was director of defense research and engineering for four years, and then in 1965 I became secretary of the air force and served in that role through some of the most difficult and divisive parts of the Vietnam War.

I first became acquainted with Caltech when I gave some talks there in the early 1960s. I also knew a lot of the people there—among others, Bob Bacher, who was provost at Caltech during the late sixties and had been one of the delegation leaders at the conferences on detection and limitation of nuclear tests.

I met Arnold Beckman in mid-'68, when he was chairman of the System Development Corporation, which was an air force-sponsored federal research center and whose trustees wanted it to become privatized. I had handled that transaction from the government side, and it was a successful process. So I knew Arnold. I was, nevertheless, somewhat surprised late in 1968 when he asked me whether I would be interested in being considered as a prospective president of Caltech, to succeed Lee DuBridge. At the time, I didn't know what I was going to do. The election in November of '68 made it clear that I wouldn't want to stay, even though I had some feelers from people who asked me whether I'd want to stay on in some capacity in a Nixon administration. I thought eight years in government was enough, especially with an administration of a different party coming in. So it was time to change, and I was thinking about various things to do. I thought about a couple of industrial jobs and about at least one job in the financial world, in a venture-capital firm. I also thought I might be interested in going back to California, especially to a place of high quality.

So I said yes, I'd be willing to be considered for Caltech. And as is always the case with people who've been in the government and are being thought of for universities, additional questions were raised, especially at that time—this was the end of 1968. It was not quite at the height of divisiveness over the Vietnam War—that probably came a little later—but it was already a time when the bulk of the American intellectual elite had turned very severely against the war. What I remember is that two Caltech faculty members, George Hammond [Noyes Professor of Chemistry] and Norman Brooks [professor of environmental science and civil engineering], came to see me in my office in Washington—just to see if I had horns and a tail, I suppose. I think they were members of the faculty advisory committee. I thought we had an

open and frank conversation. And I found that for people as strongly against the war as they were, they were nevertheless quite open-minded and very reasonable. I guess they concluded I was fairly reasonable, too.

The next thing that happened was that Colene [Mrs. Harold Brown] and I went out and looked at Caltech and spent some time with the students and faculty. I'm not quite sure when this was; I guess it was probably in November. I remember Colene and I having lunch with Arnold and Mabel Beckman at the Athenaeum. We also went and looked at the President's House. I knew I was not the only possible candidate, but I formed quite a favorable impression of the institute, especially of the quality of faculty and students. I, again, expressed my own interest. I believe it was sometime within the following month when I heard that I was being offered the position. I agreed to take it.

I was going to show up at the end of January. But the new secretary of defense, Melvin Laird, asked me to stay on for a about a month until my successor as air force secretary, Bob Seamens, could take over. I agreed to that. I think I moved the family out at the end of January, went back to Washington for two or three weeks, and then showed up again to become Caltech president in mid-February. I did attend a trustees' meeting in January, where I got to know some of the trustees. I knew some of them already, including Tom [Thomas J., Jr.] Watson, who, with Arnold [Beckman] and Norman Chandler, made up the trustees' search committee. I knew Tom fairly well, actually, from the late 1960s. I also knew Bill Zisch and three or four others. But most of the trustees I didn't know.

STONE: Why did you come?

BROWN: [Laughter] I had been in academic or quasi-academic surroundings for much of my career. And I had been accustomed for a long time, both at Livermore and in two different posts in Washington, to working with high-quality organizations but trying to put them in a broader context. It seemed to me that Caltech—which was pretty obviously the highest quality, or at least one of the very few highest quality, science and technology universities—was ripe to be put into a broader context. The society was changing. I can remember giving a talk in 1965 at MIT, when the [Cecil and Ida] Green geology building was dedicated there, saying that although scientists worked on science because they loved it, society supported it largely because of its

fruits. And that meant that more attention would have to be paid to the fruits. It wasn't a very popular talk at the time, because that was a period when science for its own sake was still prospering mightily. The common analogy—one with which I, as a scientifically educated person, agreed—was that massive scientific investments were the modern-day equivalent of the cathedrals of the Middle Ages, which didn't give the population any immediate material rewards either. But it was pretty clear to me that science wasn't going to continue to get increasing support on that basis, because the cathedrals never got quite as much money as the scientists did. At the same time, I was convinced both that science as knowledge of the universe was valuable in itself, and that technology as a method for humanity controlling the universe, or controlling the human environment, was key to economic and social well-being, and that it was very important to keep working on both. Bridging the differences in perception between the scientists and technologists, on the one hand, and society on the other struck me as a very worthwhile thing to try to do. That, plus the attractions of living in California—because I'd had approaches from other academic institutions—were very important to me.

STONE: This relationship of an institution to the larger picture runs through your whole career.

BROWN: Yes, so it seems.

STONE: Do you have anything to which you ascribe this? Is this just something you came up with? Were you influenced by a grandparent or something?

BROWN: No, I don't really see it as part of a family environment, particularly. I grew up in a middle-class family. My father was a lawyer. To some degree, it may have been influenced by my early schooling, because the Bronx High School of Science, to which I went, prided itself not only on the mathematical abilities and scientific aptitudes of its students but also on giving them a very advanced dose of culture. For example, a pair of special classes in English and in social studies used [Vernon L.] Parrington's *Main Currents in American Thought*, which was a look at American society in broad terms. Then I went to Columbia College, where there was a standard undergraduate curriculum for the first two years in which all the students took humanities. We took the Great Books course, which essentially followed the model of the John Erskine Great Books course from before World War I—the original of the Great Books courses. There was—

and is—also the so-called Contemporary Civilization course, which began with a brief look at classical civilization, then a deep look at medieval civilization. In fact, the outstanding research scholars taught the freshman courses. I remember that my Contemporary Civilization AI course, which dealt with medieval history, was taught by an outstanding medieval history scholar named Austin Evans. All of that, I think, may have had an effect on me, although I haven't tried to analyze it. But I was immersed early not only in science and technology but also in culture and history and literature. That may have had an effect.

But I think there may have been another influence, which is really the other side of the coin. Early in my graduate career, I saw that some of the other graduate students were going to do better in pure science than I, maybe because they were smarter or better researchers, but at least as much, it seemed to me, because they were able to focus very, very strongly on a narrow piece of research that they were doing. And it's often been said that Nobel Prizes are won by people partly through brilliance but largely through a combination of an ability to focus very narrowly and to have an unhappy family life. [Laughter] Those things may go together to produce the intensity that, in addition to brilliance, creates Nobel Prize winners. I concluded that I was not going to be able to focus that strongly because I had too many other interests. That drew me away from theoretical and toward experimental physics first, and then toward applied science, and then toward managing. So I think it may have been a combination of that sort.

STONE: I've read some of what you've written. You have a command of prose. Where did you learn that? Did you have a stupendously good writing teacher? Is this innate?

BROWN: I think, again, that it was a combination of the high standards of my teachers both in high school and in college. We had to write a lot in high school, which is unusual for students who are technically oriented. And again, in college, the final examinations in Contemporary Civilization consisted of questions that had already been handed out, but they were stupendous questions, of which three were asked on the examination but fifteen were handed out in advance. That meant that, depending upon how much work you were willing to do to prepare, you had to write out, or at least outline, the answers to a series of questions that covered the entire coursework. That gave me a lot of writing practice. I've still got a lot of that stuff stored away somewhere, and Colene keeps wanting to throw it out. [Laughter] But I've saved it. That's a

piece of it, I think. How much of it is innate is very hard to say. I must say, it still comes hard. I procrastinate a lot when it comes to writing things. And in the end, I dictate them and rework them. I'd probably do better if I started to do them on a word processor.

You asked about my initial perceptions of the institute. The faculty struck me from the beginning as not only very competent but sufficiently interested in what they were doing to concentrate almost exclusively on that. That had its good side and its bad side. The bad side was that the faculty expected things to be taken care of administratively without having to do much about it themselves—and they didn't always appreciate the difficulties that went with that. The good side was that unlike faculty at many educational institutions then and now, they weren't tempted to meddle. I was aware that at many educational institutions the faculty wasn't very good at what they did or very interested in it, and therefore were inclined to spend much of their time criticizing and interfering with the institution's administration. This was not true at Caltech, and that became obvious fairly early.

The students also were very capable, although socially ill at ease. I was able to sympathize with that, partly because as a youth—and well beyond—I was very bookish and addicted to intellectual matters, and partly because I was so much younger than my classmates both in high school and in college. I shared those characteristics with many Caltech students. It seemed to me that to some degree I had overcome it, but it left me with considerable sympathy for them.

The administration of the institute had some good people in it and some who weren't so capable. That's typical of universities, especially the support side of the university, where the support side of the administration is regarded rightly as not the heart of the institution and wrongly as second-class, and paid accordingly, and you don't get very good people.

The other problem was a natural one in a situation where you'd had a single individual as president for twenty-odd years. Lee [DuBridge] knew everybody, knew everything, and was able to deal directly with everybody. The result is that he had thirty people reporting to him directly. I knew from my own previous experience that when you had that many people reporting to the chief executive or president of an institution, a good many of them were reporting to nobody.

STONE: So this could imply that the place, to some degree, wasn't in very good shape as far as the administrative structure and functioning.

BROWN: It was functioning, but not as efficiently as it could, I thought. What was clear was that it couldn't work the same way with me, because I didn't have the same roots in the institution. That meant that if I were going to operate it efficiently, whether or not it had been operating efficiently, I had to introduce some sort of structure. I tried to do that, but it didn't come too easily, because it was necessary to create some other levels and have some people working for other people. And since they had all nominally reported to the president before—and in some cases had not reported to anybody—that made some of them unhappy. It also is risky, because when you do that the natural tendency is to multiply staffs—in other words, have not only a person at each level but have a whole staff working for him. I think I was able to avoid that. It gets harder with time, and I think my successors have had a harder time avoiding that proliferation of staffs. But I'm used to those issues, and I think I was able to do it without increasing the size of the administrative staff.

STONE: There was a report by a consulting firm—Cresap McCormick & Paget.

BROWN: Yes. The most useful thing that came out of that was Dave [David W.] Morrisroe. I think he was the person who wrote it. He had a lot of experience working with educational institutions through Cresap, and was himself interested enough in Caltech so that Bob [Robert B.] Gilmore [vice president for business affairs] hired him [as director of financial services]. Morrisroe and I hit it off very well very early, and he rose to become vice president for business and for finance both [1978] and was able to work very well with the faculty. That was one of the gems of personnel on the administrative side whom I found very, very useful.

Let me say a word about the trustees. The Caltech board, then as now, was an extremely prestigious one. During the sixties it had expanded from being a local Southern California board to include first all of California and then to a national membership. That expansion brought additional people of national reputation onto the board. As with any board, there were some people who devoted a lot of time to it, others who devoted rather little, some who had a great interest, and others who had little. Those characteristics are not quite the same. There are often people who spend only a little time on these kinds of activities but are nevertheless very effective

and have a great interest. The board was not quite as diverse as it has since become. Quite a few of the people on it were highly conservative politically, more so than other boards had remained, even by that time. With many of them, however, I found that this was no problem at all, because they were able to separate their political views and their social views from their attitude toward the institute. What they were paying attention to at Caltech was the quality and the smooth functioning of an institution. Nevertheless, from time to time, individual issues would come up on which social views or political views would intrude. And very early on, one did. You may remember the 1969 Santa Barbara oil spill, which happened the first month or so after I came to Caltech. Although it was really pretty trivial by the standards of later such catastrophes, it excited a good deal of interest and controversy at the time. Fred Hartley, one of our board members, was the chairman of Union Oil, on one of whose rigs, if I remember correctly, the spill occurred. He asked whether Caltech could do a study, an examination of the incident. It was clear that what he wanted, understandably, was to have a report that reflected his own views—"This is a trivial matter"—which in fact it may have been. But the issue that caused a certain amount of unhappiness between him and me was that I concluded right away that we couldn't take money from an individual company to make a study of its particular problem and that we needed to have some sort of intermediary. I forget whether I said that an association of oil companies would do or that the Caltech study needed to be associated with some environmentally sensitive organization. And he couldn't understand that at all. I mean, why couldn't Union Oil give you the money to judge Union Oil?

STONE: Were there any other such instances?

BROWN: That's the one I remember the most strongly. In the end, it worked out all right. Actually, it was partly as a consequence of that incident that I was motivated to encourage the establishment of the Environmental Quality Laboratory, because I concluded that rather than doing one-shot things in this area, we ought to try to institutionalize it. It was clear to me that the environment was going to be a very important issue, not only to California but to the country, and that it was important to set up an organization that would look at the trade-offs of economic considerations versus, for instance, health and aesthetic considerations. I thought that Caltech

was a good place to do it, because it had a long history of participation in environmental issues, including [chemistry professor] Arie Haagen-Smit in smog.

One of the difficulties in establishing EQL was getting the faculty as a group to go along with this. Even among many of the engineers—not all, but many—the attitude was that they wanted to work in their own discipline and not look at this kind of messy interdisciplinary activity, which involved a great many intangibles—not only economics, which at least the engineers understood to be an important consideration, but also politics, which everyone felt was messy. Then you had the question of, “OK, how is service in this kind of thing going to be evaluated in judging somebody’s academic progression?” which comes up everywhere in applied interdisciplinary work in academia. And Caltech, probably because the quality of the people in its various disciplines is so high, is as resistant as—maybe more resistant than—most places on this.

STONE: How did you persuade them?

BROWN: It was a combination of things. The most important thing was to get somebody from the faculty who was well respected to run it. Lester Lees [professor of environmental engineering and aeronautics] served in that role to begin with, and subsequently Norman Brooks. That made the difference. There were other possibilities. John Holdren [senior research fellow] probably was too junior then and didn’t have the prestige. He’s now used his abilities to move into a similar position, and a very influential one, on the faculty of the University of California at Berkeley. Getting Lees, who was very interested in it, to do it, helped.

The other thing we had to do is always a requirement when dealing with the faculty. We had to provide some kind of assurance that the establishment of a new activity would not impact too heavily on their own share of the institute’s resources and that we would be able to raise extra money for this that wouldn’t be available for other things. That clearly was true, and I was able to convince them it was feasible.

Then there were the questions of how do you involve students in this, because if you can’t involve students in a new project at a place like Caltech, the faculty rightly asks what the project is doing here. I left it to the faculty to do that, and the student interest in it was such that their participation was not too hard to arrange.

Begin Tape 1, Side 2

BROWN: Let me try to talk about the economic and social milieu and coeducation together—they overlap a bit. One of the earliest issues that came up when I arrived was coeducation. There had been a decision, in principle, to admit women as undergraduates. The trustees had approved that but had made it conditional upon the building of new student houses for women, which would take at least two years. I concluded that it was not a good idea to wait that long, and I pressed rather hard for an arrangement that would set aside corridors for women in the existing student houses. A good many of the trustees were very reluctant to do that, strange at it may seem in 1990—but that was 1969. Essentially, they did it because I made bringing in women in 1970 an issue of confidence in me.

STONE: So you really pushed.

BROWN: I pushed very hard. In retrospect, I think it was the right thing to do, although it didn't bring to the male students what they had hoped, because it didn't bring in that many women undergraduates. Nevertheless, it seemed to me that it did have a good social effect on the school. And to do it quickly, even in retrospect, I think made sense.

STONE: Who were your real opponents on this new building issue?

BROWN: Well, I don't remember for sure. I know that, again, my friend Fred Hartley didn't like it, and he speaks out when he has ideas. I think Arnold Beckman himself was rather reluctant, and Mabel [Mrs. Beckman], I think, even more so. But he is reasonable, and I think I was able to persuade him that it made sense. There were several trustees who agreed very strongly and pushed quite hard. I know that Henry Dreyfuss did. I have a feeling that Bill Zisch was for it, but I can't really remember. In any event, it was done, and done fairly quickly. And the following year there was something like ten-percent women students. It never grew much above that percentage until this year, when another very deliberate decision was made to try and increase that number, partly for social reasons, partly because—and I pushed very hard for this reason as well—women are half of the [country's] potential students. Now, unfortunately, by the time they get to the college-admission stage, they've been selected out, in terms of being discouraged from taking math and science. I have no well-grounded opinion on whether there's

any inherent difference in male and female brains. But I know that there is social and cultural pressure that reduces the numbers. It had always seemed to me that it was up to Caltech, for its own good as well as the good of society, to encourage those who reached the point of college admissions to go to as good a place as they could. And Caltech was such a place. Moreover, there is a certain positive effect—not enough, but some—in having women who go to Caltech become role models who could reach down and help offset some of the biases that work the other way.

You've asked also about the social milieu. I felt strongly enough about the desirability of broadening the context and the environment in which Caltech students live and work so that I did something that in retrospect looks, if not foolish, at least somewhat quixotic. That was the business of the Immaculate Heart attempt. Immaculate Heart was a Catholic girls' school on the West Side. The Sisters of the Immaculate Heart who ran it were in some disagreement with their ecclesiastical superiors. A good many of them had discarded habits and were looking for a somewhat different environment. It's hard to think of environments more different than those of Caltech and Immaculate Heart College.

I conceived the idea of selling them some space near Caltech. The institutions would have remained, of course, completely separate; but Immaculate Heart would have provided a nearby place with different values and different kinds of students. The Caltech students, many of them, liked the idea. The faculty didn't, because I think many of them felt that inevitably Caltech would be lending its imprimatur to a completely different kind of place. I'll never forget the faculty meeting at which this was discussed and at which Norm [Norman H.] Horowitz [professor of biology] got up and made the following impassioned comment: "Have we forgotten Galileo so soon?" I still laugh at that. So I think it never really had a chance. The trustees didn't like the idea either. They felt that it would dilute the Caltech quality. In retrospect, as I say, I don't think it was necessarily foolish, but it was certainly quixotic. There was very little chance of it happening. But it expresses the strength of my view that the Caltech environment, especially for undergraduates, was narrow and parochial and that broadening was needed. Maybe the admission of a class that's thirty-percent women will help. We'll see.

STONE: Did the Immaculate Heart proposal come after the admission of women?

BROWN: A year earlier than their admission, but about the same time as the decision to admit them.

Fund-raising, which, I guess, is something that relates to the economic milieu at least, was a constant preoccupation—it is for every university president. I had a mixed reaction to it. When it works, it's wonderful. When you succeed, it really elates not only everybody in the institution but whoever it is who did it. And to the extent that the president did it, it elates him or her. At the same time, it can be very discouraging to ask and be refused time and again. That happened with some people. There were some people who, I think, were essentially hopeless from the beginning, but about whom the trustees and the development office would say, "Gee, ask him; he's sure to help." An obvious example is Armand Hammer. As many other institutions have found out, the quality of an institution or its contributions to society are only one, and often not the most important, aspect of an institution that a donor seeks. A donor has his own motivations, which may be praiseworthy or less so, but making the right connection between a donor's motivations and the institution's is the key to fund-raising and development.

STONE: How was that done?

BROWN: When I arrived, we were near the end of a campaign that had been begun some years before. There'd been a series of professionals who had been engaged to do this and had done only moderately well, I would say. Caltech had traditionally relied on its own trustees to provide the funds from the private sector. Of course, the great bulk of the operating funds for research comes from the federal government; neither the trustees nor the development office have anything to do with that. The faculty does it themselves, with some help from the administration on occasion. The development office, again, went through some additional changes. Finally, I concluded that it would be best to try to find somebody from the faculty to run it. And Bill Corcoran [professor of chemical engineering] served that purpose magnificently. This was part of the structure I set up—the business side, the development side, the student side. To have a vice president for development who's a faculty member and who knows the work of the faculty—which is, after all, what you're trying to persuade donors to support—really makes a very big difference. Corcoran was in addition a capable, if occasionally heavy-handed, administrator. Having him in charge meant that the development office people were really

working on development. Typically, development people will use a shotgun technique—get everybody they can to try to raise money from everybody they can. And they're often not in the best possible position to say what's going to work or what's not going to work, to say who the best prospects are or are not, to say what the best projects are to sell to them or not. But a faculty member who knows the institution is in a very good position to do that. So that worked quite well. We were able to meet the goals of that drive and then to undertake another one some years later that was also reasonably successful.

Another source of funding that's quite important for the institute is the fee from the Jet Propulsion Laboratory. This again, is something faculty members may not have appreciated early on. I think, however, that better than at most institutions they appreciated the importance of that source of funds and understood that it was worth using the institute's reputation and name in association with JPL to try to make the two pieces work together. There'd been a series of flops on the part of JPL in the sixties, and NASA was correspondingly unhappy. NASA had a tendency to say, "A great success by NASA" when something works and "A flop by JPL" when it doesn't. But I'd known [JPL director] Bill Pickering slightly earlier and took a substantial interest in JPL and, to some degree, even in its management. I think Lee [DuBridge] had been very interested in the results and the knowledge that came out of the lab but had not much participated in the management. He had just left it to Pickering, who was a great spiritual leader for JPL and not a bad manager; but when it got into trouble it needed some additional attention.

STONE: As a president, what did you do in fund-raising? Did you consume innumerable dinners?

BROWN: We used to give dinners, and Colene's terrific at that. She was, indeed, an enormous benefit to Caltech, because she interacts so well and effectively with people—much better than I. She planned dinners at the President's House, which had not been used for that purpose for a long time. We'd have forty or fifty people in at a time—on two levels, upstairs and downstairs, in the foyer and living room and dining room, all at once, and out on the covered porch. The institute had gone to a good deal of expense to fix the place up, spending what was for that time a lot of money—it must have been \$100,000. Again, Henry Dreyfuss and Arnold Beckman were very supportive in helping get that done. And since that had been done, we felt it was important

to use the place for that purpose. We'd have monthly or every-other-month dinners of potential friends or donors to the institute. That was one thing. Another was that I would myself deliver proposals for funding to individuals or to foundations. I would make telephone calls, trying to get other people to support such requests. Sometimes it worked, and sometimes it didn't—that's how such things are. I would also push faculty to get together to make proposals for interdisciplinary work; the Environmental Quality Laboratory was an example. But there were also some initiatives in the Humanities and Social Sciences Division on the economics side—connecting economics and engineering where possible, for example. Faculty don't always make these moves themselves. I mean, the faculty are very good at getting support for their own disciplinary research; but it's extra work for them to join with others to try to put out broader proposals. It takes some encouragement from the administration for them to do that, often accompanied by seed money for that purpose. To that end, the President's Fund, which we negotiated from NASA as a way of encouraging faculty to do work at JPL, was quite effective. Any small amount of money that the president of a university has as discretionary money can be used as a way of starting something, which then can get additional funds from outside.

We tried to expand the Board of Trustees to reach into different institutions and different places. We brought on a couple of women, for example. We got some people from the West Side, starting with Lew Wasserman, including others—Dennis Stanfill, and so forth.

STONE: Do you think this has been a good thing, beneficial to the board and the school?

BROWN: I think it's been very beneficial to the board, because it gave it, again, a somewhat broader perspective than it had had—although that had already begun, as I said. They'd reached outside of California. But this was a way of reaching outside even within California. I've always been encouraged by the way in which people with quite different social, economic, and political views can come together on a board at Caltech and look at the institute and say, "OK, what's best for the institution?" Even though we may disagree about the context, how do we relate the institution to its broader surroundings? I think that's worked well.

STONE: About the time you came, the board revamped itself along the guidelines of the Cresap McCormick & Paget recommendations. Did they take to this willingly, do you know? The reduction in committees? Reformatting the board's organization?

BROWN: There was another effect that was going on at the time. I came in at the end of it. There was a conflict between Lee DuBridge and Arnold Beckman. Arnold read the bylaws, which said that the chairman of Caltech's Board of Trustees was the chief executive of the institution—which, of course, is very strange. But that is what they said. It was odd, because the chairmanship of the Board of Trustees is not a full-time job, and it's not a paid job. It may have dated back to [Robert A.] Millikan's day, when there wasn't a president of the institute. Millikan was chairman of the executive council, which was a unique arrangement. But Arnold and Lee were almost of an age—I think they are within a year or two—and that made it harder. Although I wasn't there, I'm told that that last couple of years was a difficult time. I had a much easier time, because Arnold is twenty-five years older than I am, and I offered him—I still do—the deference due to age and achievement without feeling in any way demeaned myself. The Cresap report addressed the issue of who is the CEO of Caltech. And Arnold—partly because he and I got along and partly because he was able to feel, correctly, that he'd had a large part in selecting me—didn't feel it was any diminution of his own authority for me to act like the chief executive of the organization. That, it seemed to me, was the biggest hurdle that had to be overcome. When it was, the rearrangement of the board, I think, didn't cause a lot of trouble.

STONE: What was your working relationship with Arnold? Did you talk on the phone every week?

BROWN: We talked more often than that, and we saw each other very often. He was able to regard me—and probably still regards me—as his protégé at Caltech. I find it hard to think of an instance in which he said, “No, you can't do that.” But I know there were times when he said, “Look, back off on this. You can't do it so soon. Let me handle it; let me help with the board.” And he did. So it worked very, very well, from beginning to end. It was one of the joys of working at Caltech.

There were lots of board members who were towers of strength, each in his own way. Howard Vesper really understood the budget, and I think he was pleased to see that I understood budgets too. He and Morrisroe and I worked very, very closely together on those kinds of things. Henry Dreyfuss I have spoken of. Tom Watson, although he wasn't able to give much time to the institute, clearly was interested in it and devoted to it. And Norman Chandler was the same

way. I don't want to leave people out, and inevitably I do, but they were very, very good. Even people with whom I would often disagree, like Fred Hartley, I found extremely useful, because I could be sure that I would be forced to justify things, and therefore I had to think about them. So I think the relationship with the trustees really was very good.

STONE: Arnold retired from the chairmanship at some point during your time at Caltech.

BROWN: 1974, I guess. Stan [R. Stanton] Avery became chairman.

STONE: How was that relationship?

BROWN: In a way, Stan could feel that I picked him, just as Arnold could feel that he picked me. Although it never really works that way. But Stan had a different attitude. Stan didn't have the same kind of devotion to Caltech, or long association with it, that Arnold did. And he had a lot of other irons in the fire—the Huntington [Library] and some other things as well. It worked well, but he mostly presided. He was different from Arnold.

STONE: *Did* you pick him?

BROWN: No. You'd have to say it was the board who picked him. But it was essentially Arnold and myself who concluded that he was a good person to succeed. And maybe that's a sensible way to do it, because when a president has been in one place for six years, as I had been, he knows the place. He doesn't need a chairman who knows the place well as much as a new president does. And yet that arrangement probably wouldn't have worked for many more years, because I would have felt that you needed a chairman who was more active. Stan did a good job, but it was not the same kind of close attention to the place that Arnold displayed or that one can expect Rube [Ruben F.] Mettler [Avery's successor as chairman] to display, again for the same reason. Stan introduced a different set of people. He brought on some other trustees who, again, were a way of expanding Caltech's environment. The pendulum swings back and forth. What happens in organizational things, as in personnel selections, is that every organization has the disadvantages of its advantages, and every person has the disadvantages of his or her advantages. What happens in an institution is that after a while you see those disadvantages and you replace

the person with one who's different and who brings different disadvantages. Or you change to an organization that corrects the old problems and creates new ones. Then after a while you swing back again. It's not ideal, but it's probably the best way to do it.

We were discussing campaigns and buildings. Inevitably, we tend to focus campaigns around buildings, because buildings are a way to introduce new structural arrangements and institute new activities. While I was at Caltech, we finished some buildings that had been committed before I arrived, and we built some new ones. We finished the business administration building [Keith Spalding Building of Business Services]. Lauritsen-Downs [George W. Downs Laboratory of Physics and Charles C. Lauritsen Laboratory of High Energy Physics] was finished while I was there, but Lauritsen-Downs and the business building had been committed beforehand. I guess the new geology building [Seeley G. Mudd Building of Geophysics and Planetary Science] was built while I was there. I myself don't tend to think in terms of buildings. As Robert Millikan said, "It's a lot easier to get them to give money for buildings than it is to give an endowment to pay the janitor." We therefore tried to institute the policy of not building a building unless we also saw the operating funds somehow—whether from an endowment to go with the building or a research-fund endowment, although most of the research support, again, would have to come from government contracts. I think we succeeded to some degree. You never quite accumulate all the money you need. But I think we did better than most places in this regard. I look at other universities, including Johns Hopkins, where I now am, and I see they've gone out and built a lot of buildings with no prayer of getting the money to pay the light bill, let alone the salaries of the people who are going to be in the building. And that's a mistake.

One thing I think we did right was what we did with the administration when Throop [Hall, the administration building] was damaged in the 1971 Sylmar earthquake. When it was concluded that the damage was such that the building would have to be torn down, we had to decide what to do with the space, and we also had to decide what to do with the people who had been in the building. I think it was very properly decided—and, again, Henry Dreyfuss's influence was significant here—that an open space in the middle of the campus would make a lot of sense. I actually had an extravagant idea of building something like the Teatro Verde, which exists in Venice, where you have rows of seats essentially in the greenery. But it's a good thing we didn't do that; it would have been too easy to be a center for demonstrations. But the plants

and the water, and even the fake rocks—now replaced by real ones—probably added to the ambience. I think it made a lot of sense.

Then there was the question of where to put the administration. In the first place, there wasn't time to put it in a new building. But I concluded that this was a good opportunity to try to hold down the size of the administration, so I said, "OK, we're going to be on the third floor of Millikan Library. That's a very restricted space, and that will help keep the size of the administration down."

It worked, actually, to a considerable extent. We didn't take up a lot of space. And the whole time I was there, I resisted very strongly the push to fix up the old chemistry building [Gates Laboratory of Chemistry] and move the administration into it—partly because I felt that having relatively austere quarters would set an example to other people, or at least avoid criticism of the administration, and partly because it was a way of holding down the people. If you don't have space, you don't hire more people. I knew that it wouldn't last indefinitely. What they've got now [Parsons-Gates Hall of Administration] is by no means extravagant or too fancy, but there has been some expansion, partly as a result. I think, from what Dave Morrisroe tells me, that there's not much of an increase in administrators who actually deal with the institute itself. But there's been a necessary addition of people to do all the things that are mandated by the various federal regulations. That you can't do much about.

I haven't said much about student life or the administration of student affairs. I thought Lyman Bonner [director of student relations] was really quite useful in that role when I came, and he continued to be. To show you what kind of a place Caltech is, I remember being at home late one night—this was early on, so it must have been 1969 or '70—when we got a frantic call from the mother of some student who had been arrested for possession of marijuana—odd as it may seem—and who was lodged in the Pasadena jail. I had to deal with lots of misbehavior, including really criminal misbehavior, when I was secretary of the air force, because that was an organization of nearly a million uniformed people and probably half a million civilians, but never on quite this personal a level. What I did was, I got hold of Lyman Bonner the same evening. He acted on it quickly. I think he was actually able to persuade the Pasadena police that this was hardly a capital offense and was able to reassure the mother of the individual involved that same night. I think that is a good thing. Students don't want the institute to act *in loco parentis* in general, but when they're in trouble they don't mind. [Laughter]

Student houses, of course, always left a lot to be desired. Colene and I spent a night each in a couple of student houses, just to see what it was like. And we concluded that we didn't see how they were able to get anything done. [Laughter] Although, by being there ourselves, we may have perturbed the environment enough so that it wasn't typical.

We expanded the size of the administration devoted to students and brought in quite a few people. We brought in Lee Browne [as director of secondary school relations and special student programs]. Jim [James J.] Morgan [professor of environmental health engineering] took over as dean of students and added to his staff. I think probably the [student] environment is better than it has been, but it obviously is not such as to satisfy the students. By and large, I think, students who come to Caltech are heavily motivated already; they're highly driven. The men, at least, tend to be somewhat socially troubled. You have some very bright students from East Asia who are not all that fluent linguistically, although they can probably do very well on even a verbal SAT. And students, again, have the feeling that it's up to the institute to make them happy.

Begin Tape 2, Side 1

BROWN: My answer to students who say that the institute ought to devote itself to their happiness is that happiness is a by-product. That is to say, you don't set out to be happy. You do something else, and if you're lucky or clever, doing that something else will make you happy. Now, that can be an excuse, of course, for the institute's not doing anything to make the environment such that the students find it at least bearable. And I think Caltech needs to keep working on that. But it would, I think, be a little bit naïve to think that somehow Caltech is going to make happy a time of life that most young people don't find happy, especially if they're working so hard and are not quite sure they're going to achieve their goals, as is the case for most Caltech students.

I see that in talking about the faculty, I haven't really talked enough about the role of the provost, which is something that ought to interest you. Because all faculty people and faculty wives worry about that kind of thing.

STONE: What does that mean? [Laughter]

BROWN: [Laughter] It means that the provost actually has to be the president's principal bridge to the faculty. He has to be seen as the person who represents faculty interests but at the same time says no to the faculty. And that's a tough job. The ideal arrangement, from the president's point of view, is that the president is the man who's seen as saying yes, and the provost, of course, is the person who's seen as saying no. And that saves the president from being the bad guy. Bacher, I think, did that very well. And Christy did it extremely well, too. Bacher was provost the first year-and-a-half or so after I came and Christy for the rest of the time.

It's very important that the provost be seen as being fair but tough. And I think both of those people were. That doesn't relieve the president from dealing properly with the faculty and being seen by the faculty as representing their interests to the outside. But the provost is the person who both represents their interests on the inside and fixes priorities in dealing with programs, budgets, and appointments. And that is a tough job.

The Institute Administrative Council, and the division chairmen specifically, who deal with promotion, appointments, and salaries along with the provost, play an extremely important role. In the case of the provosts who worked for me, I asked them to look at the whole budget. Division chairmen, in particular, have the function of insisting that the appointments that are made in other divisions live up to high intellectual standards. That is something that Caltech has managed to preserve very well, it seems to me. The fact that this sort of quality control is exercised by the division chairs, not only in their own divisions but to some degree with respect to the appointments of other divisions, saves the president from having to rule out appointments that are made unanimously by a division but which a division nevertheless knows aren't really up to snuff. Again, I've seen at other universities that the absence of such a quality control mechanism, or the decline of such a quality control mechanism, is inevitably reflected in the decline over the years—over decades sometimes—of the quality of a faculty. Many faculty members have a tendency to appoint people who are like them but not as good and who can therefore be their assistants. That practice has hurt the European universities, and sometimes it hurts U.S. universities as well. The toughest thing is to phase out an activity altogether, because although individual professors retire, groups never do. They have to be turned off, or at least turned down. We did some of that at Caltech—not very much, because there's been more of a tendency toward self-renewal or movement into different areas by faculty members or their successors who see that the leading edge is shifting its position, or shifting its area of interest.

Caltech faculty members have always told me that one of the best ways for them to find out where the future lay was to see what their students were interested in.

STONE: What was the pattern of your working relationship with Bacher and Christy, and the differences you saw in the two?

BROWN: Bacher had been at Caltech and in the provost's role for a substantial time. He knew where the bodies were buried. Each was effective in quite a different way. Bacher was a great advocate of letting things mature. He would be a big man for appointing committees and letting things proceed to a point where they became ripe. And it worked. Christy was considerably more inclined to express an opinion early—always well thought-out—but he was not averse to setting up a committee where the correct procedure was clearly to do that. But he was generally more abrupt in his actions. That maybe made him somewhat less popular with the faculty. But however arbitrary it seemed to them, I think they recognized that he was acting in terms of what was right for the institute, and that mitigated it. He was respected and therefore very effective. At my urging, he got considerably more into budget issues and overall institute issues. I think Lee [DuBridge] tended to deal with people individually and knew the whole institute well. Bacher was used to that and saw his role as being the faculty person, dealing principally with faculty matters. I had sought Bacher's advice on more things outside of faculty areas. Christy was used to my asking him about everything from the beginning, so he took on a broader role for that reason.

STONE: Why did you settle on Christy as provost?

BROWN: Well, actually, we had a search committee. Christy was chairman of the committee, and as often happens, when the committee was unable to find anybody, they looked to him.
[Laughter]

STONE: You said that Christy made up his mind pretty early on in a situation. Once he made up his mind, could he change it?

BROWN: There were occasions when he did. He got backed off occasionally; but not often.

[Reading interview outline] “Important and lasting changes your administration brought to the school, present-day view of Caltech, advice and insights concerning its future.” I believe that by the time I left, the institute was much more aware of and involved in the outside environment, broadly construed. In other words, I think I did, to a substantial extent, succeed in doing what I came to do. Partly that was a result of external pressures. I mean that institutions in general find the ivory tower harder to maintain. Caltech was never, by any means, a totally ivory-tower institution. To a substantial degree, [Theodore] von Kármán shaped the aerospace industry in Southern California, Haagen-Smit did discover the constituents of smog, and so forth. But Caltech had become more and more a science, as opposed to a science and engineering, institution. That wasn’t bad, because a small institution can’t cover all of engineering. Engineering, more than science, depends on mass. That’s not entirely true anymore, because there’s now Big Science, whether it be in space or Superconducting Supercolliders, and so forth. Things that cost a billion dollars to build can’t be considered small, although small teams can work in them and be effective. That’s harder, although not impossible, in engineering. But for whatever reasons, Caltech had become, and remains, primarily a university of science rather than of science and engineering. I tried, and to some degree I think I have succeeded, in connecting even the science—certainly the engineering and social science—to societal problems. A place like Caltech can’t start at the societal end, and shouldn’t. What it needs to do is say, OK, which of our areas of special expertise are intellectually important and exciting and are going to have important societal impacts, and let’s give that some weight in our thinking about what we’re going to do. In this connection, I found Christy’s attitude very interesting. Christy, of course, is a theoretical physicist. But when I asked him what he thought about this, he said that he had always found that there were applied problems that had as much inherent intellectual interest [as those relating more to “pure” science]. And if they met that criterion, he was perfectly willing, and felt others should be willing, to look to such applied problems where they had important impacts, economically, environmentally, or whatever. There were enough other faculty people who felt the same way, so we were able to do some of these things. That was one change.

A second change is that the financial aspects of the institution were reasonably healthy when I left and significantly healthier than when I arrived. It became clear to me that the funds wouldn’t automatically be forthcoming at the level at which Caltech had come to require them. In Millikan’s day, a few rich Southern Californians could do it all, and they did. By the end of

DuBridge's day, that had been substantially augmented—in fact, overshadowed—by the federal contracts. But it was clear in the seventies that both of those together wouldn't be enough, and that we needed to draw money from industry and from individual donors outside the Southern California community. We were able to do that.

One way that we did it, which had not been much used before and which seemed to me a very obvious thing to do, was to institute a large number of endowed professorships. I felt that we had—and, of course, we continue to have—at Caltech a sufficient number of nationally and internationally eminent faculty; so that there would be no problem in filling those chairs. That, in turn, would be an attraction, should be an attraction, and has proven to be an attraction, to donors who wanted to memorialize their own names by connecting them with internationally famous scientists, engineers, and other academics. I don't know what the exact numbers were, but we started with a few named professorships, most of which were not funded and were being paid out of institute endowment funds or contract funds. And by the time I left, we must have had twenty endowed chairs. By now, there must be forty or fifty. I don't see any reason why there can't be a hundred, and with endowed professorships costing what they do these days—a million and a half dollars—that is both a requirement and also an opportunity for a very large amount of endowment funds, which the institute needs.

In this connection—that is, in connection with the financial situation of Caltech—I think Caltech is now well known and highly regarded throughout the academic community as a place that manages its affairs well. By that I mean it manages its finances well, budgets sensibly, plans properly, and does OK on investing its endowment. That is by no means the most important, or even the second, and maybe not even the third most important criterion of quality for a university. But if it does badly enough by that quality criterion, it's going to go out of business. [Laughter] So I'm pleased with that. I don't regard it as my primary accomplishment, but it's a significant one. General Eisenhower once told me that he thought his principal achievement as president of Columbia University was to make it financially sound. He regarded that as his principal achievement because he didn't feel he'd brought intellectual or academic credentials. In retrospect he clearly had intellectual credentials, but making a university financially sound was something that he had every right to be proud of.

As important as that, and more important than the buildings that were built, was not only expanding Caltech's relationship to the broader societal context but some improvements we

made in the student situation while I was president. As I said, you're never going to make students happy. It's a difficult time of their lives. The Caltech environment, partly as a consequence of its very high and demanding intellectual standards, is not such as to make students especially happy. But we started bringing women into the undergraduate student body and began diversifying the student body in various ways. We didn't do too well with minorities, but we did something, and there are also now a fair number of foreign students. Sprucing up the student houses, although there are always some slum-like characteristics in student housing everywhere, was important. And increasing the availability of help, counseling, to the students, I think was a significant accomplishment.

The end of the sixties up through the mid-seventies was an especially difficult time for students because of the societal unrest. It had peaked earlier elsewhere, but Caltech was always late in these things. I always described Caltech as being fairly far up the beach on student unrest and activism, with the result that the wave broke a little bit before it got to us. But we did get some of it. Not all of it was bad, but it did put additional pressure on students. For that reason, I'm especially pleased that we were able to increase the availability of student counseling.

We also stabilized the JPL situation, which was in not very good shape when I arrived, and was in quite good shape when I left. Bruce Murray [professor of planetary science and geology] is in some ways not a natural administrator—he has great originality and on occasion some quirky ideas. I think he brought considerable intellectual stimulation to JPL [as lab director, from 1976 to 1982]. He also did a lot to tie the lab and the campus closer together, which was an important thing that much needed to be done, because they had drifted far apart.

I've said what I thought about paying more attention to applications—applied biology, engineering, and the Environmental Quality Laboratory. Fortunately, Caltech never tried to set up a medical school, but it did establish connections to various medical institutions—the Huntington Hospital, the City of Hope, and others—which I think helped the biologists and biochemists to think in terms of the broader context, again.

Let me turn to my view of Caltech now and what I have to say about where it goes in the future. It's very hard for people who've been associated on and off with an institution for as long as I have with Caltech to think about the future in terms that are very different from the present. And I'm no exception. Ever since my close connection, beginning in 1969, Caltech has seemed to me to be doing well. My guess would be that change, although vital, shouldn't be

abrupt in the future either. I think Caltech, like other American institutions, is going to be called on to face a rather different future. It will be one in which national security is measured less in military terms—and this doesn't affect Caltech particularly, so in that sense Caltech is fortunate—but it will also be one in which international economic competition is going to be a much more pressing and difficult national problem. There are really three things, I guess, that I see as bringing about the broadest global societal change. The first two are a diminution of military power as an element of national security and a further internationalization of everything. In both of those areas, Caltech's in a good position. It certainly doesn't lose much on the military side, and it is already an international institution, with a reputation that is perhaps even greater overseas than within many parts of the United States. The third major force for change is this increased competitive economic pressure. That, it seems to me, is going to accelerate the push for research that relates to applications. Correspondingly, it's going to require Caltech to seek funds more from industry; and they'll have to give something in return for getting it. It will push upon Caltech some responsibilities for improving the level of numeracy and scientific interest and capability on the part of young people. Caltech already started to do some of that while I was there, with its sessions for Southern California high school students and the summer research arrangements for such students. I have some real doubts about whether Caltech can actually contribute to curricula at the elementary and high school level. Although being at the leading edge is very helpful for attracting graduate students or even undergraduates, it is sometimes pretty remote from the concerns of six-year-olds or ten-year-olds or fourteen-year-olds. There's some overlap. A Carl Sagan—about whom I have some doubts—or an AI [Albert R.] Hibbs [director of space science, JPL], somebody who can popularize things, is what's needed. And there is some overlap between those people and the leading-edge scientific practitioners, but those are not identical talents, by any means. Caltech may want to do some more of that.

Adding women to the undergraduate body in larger numbers is an important example of what the American educational and industrial structure is going to require, because women are half of the work force, and minorities and immigrants are going to be a larger fraction of the labor force than they have been. I think we're doing well on women; I don't think we're doing well enough on the others. I've only mentioned some additional things that Caltech's going to be pushed to do. The challenge will be to do some of those things, while at the same time

maintaining Caltech’s intellectual quality, the devotion to science and technology as such, and the ability to pick leading-edge subjects before everybody else is already working on them. That approach is what has characterized Caltech in the past. But it’s going to be a challenge to do that and at the same time pay some attention to these changed societal pressures, which are justified. The best approach, I think, would be for Caltech to maintain the focus that has characterized it over the past seventy years, while at the same time modifying itself at the edges so as to be able to do some of these other things more—because society is going to demand that as a quid pro quo for continued support.

Begin Tape 3, Side 1 [Afternoon session]

STONE: I have several questions. The first one is on the way the institute functions. You said before that the faculty was largely too busy with its own affairs to “meddle” much in administration. And yet I have heard that Caltech is the last school in the country that the faculty really runs. Now, can you reconcile those two statements?

BROWN: Yes. The faculty at Caltech determines the curriculum. They have by far the principal role in determining which students are admitted. And by and large—not in every case, but in the overwhelming majority of the cases—they not only decide what research each faculty member does but go out and raise the money from federal grants. Those are the key parts of the university, and in that sense the faculty does run the university. The same is actually true to a large degree at most of the leading research universities in the country. But Caltech probably does it more, because the faculty are, by and large, more eminent, more able to determine the direction of their own research, and more able to raise federal funds for their own research. And because they pay more attention to students than the faculty at most universities—although Caltech students wouldn’t believe that—they have more influence on who the students are.

What is it, then, that the administration does, and that the faculty generally leaves it to the administration to do? The administration—and in this you’d probably have to include the academic leadership down to at least the division-chair level—decides what new buildings get built, which in the long run does shape what research gets done. The administration puts together most of the interdisciplinary ideas, or at least generates them. They are then either

rejected by the faculty or picked up by it and shaped to suit the faculty's own interests. But the administration does initiate that. The administration is also largely responsible for raising the private funds that are necessary. The administration, to a significant degree in matters outside of the faculty members' individual research, is the buffer between the faculty and the outside world—be it the Pasadena city administration or the Southern California Edison Company. And in that sense, the faculty generally does keep hands off. It's an interesting relationship, because clearly the president does have some influence in a gross sense and in a long-run sense. I would suspect that the style and tone of the institute was different in Lee DuBridge's day from what it was in mine, and different again during Murph [Marvin L.] Goldberger's administration, and different now under [Thomas E.] Everhart. But beneath that style and tone, there is a character of the institute that is expressed principally in its faculty and determined to some degree by the dead, or at least removed, hands of previous presidents. Obviously, the institute still reflects a good deal of Millikan and DuBridge, even after all these years. But the day-to-day activities of the faculty and students are determined largely by the faculty.

STONE: One of the things you did was to establish a five-year rule for division chairmen. What effect do you see this having on the institute?

BROWN: I established the five-year rule in part because there was otherwise no basis for division chairmen to retire. [Laughter] In fact, the five-year rule didn't limit them to a single five-year term; it just said that they had to be re-examined. What the five-year rule meant was that you could no longer stay on indefinitely, as had been occasionally the case earlier. You could have a situation where a division chairman stayed on—oh, not retired on the job; people don't do that at Caltech—but had stopped really having any ideas or any incentive or any wish to change things after ten or fifteen years. The faculty in the division might not like that kind of thing, but there was no way to have a change in chairman without embarrassment of some sort. The great advantage of a review after five or seven years or some specific period of time—and this applies not only to division chairmen but to provosts and presidents as well—is that it gives everybody a chance to make a change without it's seeming to be a dismissal or a judgment of unsatisfactory performance. Because during the course of a review, or even before one is made, there's enough

sense on both sides to know what would be a sensible thing to do. I think that's one policy change that had a good effect.

Now, I can point to some cases where I think the division may have lost something by changing division chairmen, but clearly they thought, and probably were correct, that they were gaining something, too. To a large extent—although not entirely—I think it should be up to the faculty to decide what style they like and what deficiencies they would rather put up with for the next five years. Again, it's a matter of the pendulum swinging back and forth. The faculty of a division may well feel, and the president and the provost may well feel, that after five years of having a tight administrator who really gets things in order, it's preferable to have someone who will be very sympathetic and who will lose all the papers behind his desk. But at the end of that five years, they're going to want to change back again. [Laughter]

STONE: Do you think this five-year thing has shifted power in any way within the school?

BROWN: It is certainly true that a division chairman who stayed in office for fifteen years had a lot of power. Going to five-year terms has limited that. On the other hand, the division chairman who manages to keep the confidence of his faculty for five years and is willing to serve another five, can be in office long enough to do almost anything it's reasonable to expect him to do. I think it has probably given more authority to the leading non-administration faculty members in each division. In some ways, it's also given a little more authority to the president and the provost, because they can have an influence in this five-year period. They could before, but, again, not without some embarrassment. From what I hear, there have been cases where even under the five-year rule the change in chairman hasn't been handled as well as it could have been. I'm sure there were some instances like that in my time, too. But although a university should not be a political democracy, that principle of a political democracy—that there's a review, and possibly a change after a predetermined period—I think is a good idea.

STONE: What insights can you offer into how the institute moved, and moves, into new research fields? What's the mechanism? How does that work?

BROWN: The impetus can come from faculty members, and generally does. Or it can come from the administration, by which I mean the president, the provost, and the division chairmen. The

process moves slowly, is my experience. And that's probably a good idea, because Caltech, more than most places, has been able to avoid fads. Occasionally, but very occasionally, the motivation will come from a trustee or somebody outside who says, "Isn't this worth Caltech's looking into?" But after the original idea is broached by one of these possible sources, there is a lengthy period of consideration within divisions. Then, if the idea is of sufficient moment, there'll be a faculty meeting, where the chairman of the faculty plays a significant role in deciding what format to use, and so forth. At that stage, or generally before, the question comes up, "OK, who is going to do this?" Because at Caltech, although ideas are greeted with great interest, the legitimacy of those ideas is often dependent upon the names of the people with whom they're associated. There are at Caltech, I would say, faculty members in every division who are regarded as especially serious or weighty by the faculty all through the institute. And there are others who are regarded as less weighty in institute-wide matters, however worthy their own research efforts. The name that gets associated with an idea does determine the faculty's response to a considerable degree.

STONE: You intimated this morning that the institute going into the late sixties was in better financial shape than most schools. How did this happen? Was there some guiding light on the board or in the administration who helped things? There were schools that expanded rapidly during this period, and over expanded. And Caltech didn't.

BROWN: That was the difference. During the fifties and sixties, Caltech grew in size, but it didn't grow by nearly the same factor as many other private and public institutions. This was a consequence, I think, of two things. One was the insistence on quality by the faculty and by the administration, which automatically set a limit. The other was that the trustees, at least some of them, kept a close eye on finances. And that inhibited a phenomenon that took place at many other places and to which I have previously referred in part, a phenomenon of saying, "Let's start the program and build the building—we have the money for that—and we'll worry about operating funds later." Caltech trustees were very cautious about that. People like Howard Vesper and Arnold Beckman clearly worried about that a lot. And even trustees who have a reputation for being expansive, like Si [Simon] Ramo, paid a lot of attention to that.

STONE: There was a pretty rapid downturn in government support right after '67, '68. Was this foreseen in any way by the administration or the trustees?

BROWN: Government support of basic research didn't turn down after '68, but it did flatten. People had been accustomed to continued rapid growth, and that was no longer forthcoming. I think the possibility of that happening played a part in the fiscal conservatism that characterized Caltech in the late sixties and through the seventies. But I can't say it was anticipated. The possibility was anticipated, but I don't think it was really expected.

STONE: We've talked a little bit about the board and its character. I have heard that there are people on the board who have a sixth sense of what is to come. Is this true? Who are they? Can you give any instances in Caltech history?

BROWN: That's a new thought to me, because I don't believe in sixth senses. I think that the Caltech business office, especially during Dave Morrisroe's tenure, has had an excellent grasp of the potential sources of revenue, expenses, and cash flow. And partly as a result of my prodding during the seventies, it has laid out a planning process that makes the numbers more believable than they are in most educational institutions. I mean, I put in a five-year plan process, whereas before there hadn't been one. That made not only the budgets but also their future implications much more realistic than before.

STONE: Sort of a rolling five-year forecast.

BROWN: Yes. Every company does that, and government institutions now tend to do it, too. That, I think, provided a basis upon which the president and provost and the trustees could act. The business office has always had a close relationship with the budget committee of the board—Howard Vesper first, then Rube Mettler later on. These were people who were accustomed to dealing with budgets and could work with the administration or rein in the administration when it seemed that plans were getting out of phase with potential realities. Moreover, you always have had on the board—and in the president's office, too—people who had close enough connections to government to be able to anticipate—not to predict, but to anticipate—what the range of possibilities was with respect to government funding, whether it

was NASA funding for JPL or National Science Foundation, NIH [National Institutes of Health], or Department of Defense or Energy funding for various pieces of research on campus. The rapport was better; the system was better; and the process was laid out better than it is at most universities. That may, when it's done, appear to somebody outside to represent a sixth sense about what is coming. So I think that may be an explanation of that phenomenon.

STONE: Was there a lot of land bought around Caltech when you were there?

BROWN: Again, we laid out a master plan for the campus. That included the buying up of individual lots—houses—as they came on the market, so as to make space for these things. That was begun by Gilmore and elaborated and carried out by Morrisroe, working closely with the Pasadena city authorities. As a result, we were able to close off streets and make the campus much more of a campus than it was in 1969, when I arrived. It's still not Santa Cruz or Princeton, but it's an identifiable space. That was a deliberate action. That was one thing the administration did—and that was something in which the administration really had to be the interface with the community.

At the same time, we devised a program whereby the houses and lots that had been acquired for long-term purposes but weren't being used for them immediately could, in the meantime, be used for faculty housing with some modest degree of subsidization. The housing-cost pressures aren't nearly the same in Pasadena as they are on the West Side of Los Angeles, or in San Diego, for example. But they're severe enough so that that was a useful adjunct. We also used some of that housing for the Fairchild Scholars program—which was one of the more successful accomplishments that took place while I was there. Coming up with that kind of idea—and using it to get a very substantial amount of money from the Fairchild Foundation—made Caltech in still another way the envy of other institutions.

STONE: How did that come about? Was that your idea?

BROWN: As in many of these cases, it's hard to remember whose bright idea it was. I don't know whether it was mine or Christy's or somebody else's. It probably was a product of more than one mind. But whoever came up with the kernel of the idea, it was very rapidly jumped on by all the division chairmen, because it seemed like a great way to augment the faculty on a

temporary basis without increasing the size of the institution. That was one of its features that not only made it very popular within the institute but also a great selling point to the Fairchild Foundation. The idea of a small, high-quality place being able to amplify its efforts this way, without taking on permanent commitments, was very appealing.

STONE: You went to the [Sherman] Fairchild Foundation first and only?

BROWN: I think we took that only to them. I'm trying to remember whom we knew where. One person I knew was Fowler Hamilton, who had been in the State Department in the Kennedy administration and was a trustee of the Fairchild Foundation. Walter Burke also was a trustee. I think Tom Watson, on our board, knew Walter Burke and pointed me at him as well.

STONE: It is a gracious, lovely program.

BROWN: And I guess it continues.

STONE: Yes, it does indeed. You mentioned that one of your accomplishments was cleaning up JPL.

BROWN: It was restoring the relationship between JPL and NASA, which had gotten bad in the late sixties. There was not only the failure of a couple of missions but also a feeling on the part of NASA that JPL was not being properly managed—that Caltech, in particular, was not paying enough attention to the management of JPL. I think some of the trustees had felt the heat from Washington, and they in turn had put some heat on Lee DuBridge. Lee and Bill Pickering were personally very close, and I think that made it difficult for Lee to get Bill to do anything that Bill didn't want to do. My belief is that corrective actions had been taken by the time I arrived that paid off after I arrived, and to some degree I was willing to take the credit. For example, they had put in a deputy director—actually, it was Rear Admiral [John E.] Clark, whom I had known before—whose purpose it was to see that the place was managed in a businesslike rather than an academic way. I'm not sure that somebody who comes from the military and the Defense Department really was then, or is now, in a position to say that the way things are run there is a more efficient and less costly way to manage things. But the people who were put in—Clark

before I came, [Charles H.] Terhune after I arrived—both had experience in running very large organizations in the government or dealing with the government. I think that did help clean up some of the problems. Now, what really cleaned them up, of course, was that the missions started succeeding. And that put a completely different tone on things. But clearly NASA had felt that the Caltech administration had not been paying enough attention to JPL, and I was able to redress that.

STONE: And what about relations between the campus and JPL, which can sometimes be a little sticky?

BROWN: Well, they had been sticky before and remained sticky for a while. There are two problems: One is lack of sufficient interaction between the faculty and JPL and the other is a feeling on the part of JPL that they really are a separate institution. They're like everybody else with two hats. They use each hat to defend themselves from the owner of the other hat. The JPL administration would, when pushed by Caltech, say, "Well, we really are working for NASA." And then when NASA got too nasty, they'd say, "Look, we work for Caltech." In fact they work for both, and it is a problem for them.

When I came, I thought that the big problem was an inadequate overlap of interests and activities which led the Caltech faculty to say, "Look, this activity has nothing to do with us. Yes, we know we get a fee, but why don't you go get the money some other way instead." When things went wrong at JPL, the faculty felt that it tarnished the Caltech image. They also wondered whether you could really have a compatible relationship between two organizations of such differing size and quite different objectives. Caltech, after all, is a small institution with research directed at the goal of increasing basic knowledge. JPL is an applied institution with an enormous organization. Although it also is aimed at increasing knowledge, a far larger fraction of its effort inevitably has to do with big technology rather than with small science, which is Caltech's main direction.

Now, how do you fix this? What you do is find common interests. This was relatively easy to do for a few people whose interest was in radiation from or in space, or in planetary geology. Once you got beyond the moon—which already was very interesting—you found a solar system, if not a universe, full of new and interesting phenomena that preoccupied some of

the faculty—although even among those, many really just wanted to look at the data and didn't want to be involved in gathering it, which is an important connection to make.

I concluded that we needed to find a way to get more Caltech faculty interested in the details of what went on at JPL—to find overlapping areas of research. At the same time—and here I don't think I was all that successful, but I went some distance in that direction—I wanted to see whether JPL's size and nature as a leading engineering organization, which it is, could in some sense compensate for the limited size of Caltech's engineering division. Universities that lead in engineering tend to have engineering faculties that are five times that of Caltech, or even more. It's just not feasible or desirable to expand Caltech's engineering faculty by that factor, even though there are pressures to expand it some, as more undergraduates and graduate students want to be in engineering.

But JPL was not just a factor of 5 bigger in engineering; it was a factor of 100 bigger in engineering. And although it wasn't uniformly of the academic quality—it couldn't possibly be—of the Caltech engineering or science faculty, in some things it also was at the leading edge. So I tried to interest the engineering faculty, too.

We got a President's Fund to support such dual appointments—research on the campus of relevance to JPL and vice versa. That made a big difference. It took several years, and not everything I wanted to do was accomplished. But there was enough increase in overlap of interests and cooperative work so that the sense of alienation and even adversarial attitudes was much diminished. It didn't go away.

STONE: It helped, perhaps, when [Bruce] Murray took over; because he was from campus.

BROWN: Yes, but Bill Pickering had been from campus, too. It's another aspect of this very long tenure problem. Pickering had been there for fifteen years, and his connection with the faculty was much attenuated. It's another reason why such appointments should be made for a limited time, with the possibility of renewal. But you're right: When Bruce went up there, he was fresh from the faculty. He was very interested in emphasizing faculty-JPL connections, which Bill Pickering probably was at the beginning, too, but wasn't at the end, for obvious reasons. And that made a big difference.

STONE: About that time was the so-called Orange Report that [professor of physics Rochus E. (Robbie)] Vogt [then chief scientist at JPL] did.

BROWN: Right. It was true that I was able to find people on the campus, many but not all of whom had had substantial involvement with JPL themselves—like Robbie Vogt, [professor of physics] Ed [Edward C.] Stone, and a few others—who were willing not only to do cooperative work themselves but to encourage others to do it. That was very important, because it’s only by getting some faculty interested that you can get other faculty interested.

STONE: In your writing about the school, early on, you used the word “insularity” about Caltech. Could you define exactly what you meant?

BROWN: What I meant was what I said earlier in this interview, which is that most Caltech people are so good at what they do and get so much satisfaction out of doing it that they are not likely to be very motivated to connect it with anything outside.

STONE: Do you think that has changed? Do you think you helped change it?

BROWN: I think I helped change it. I think I have not, nor would I have wanted to, created a reversal in which Caltech people are totally driven from the outside. They’re not. In fact, on balance, they’re still much more inner- than outer-directed. That is almost a *sine qua non* for high-quality academic research in science. I would not have wanted to change that, and I didn’t. I think they are more sensitive to, and more able to respond to, external influences than they were. I think I had something to do with that. The times also had something to do with it. But I was sensitive to it from the beginning; I promoted it, and I think I had an effect.

More of the best Caltech people now see some value in spending some of their time thinking about external influences or possible applications of what they do. A few of them are actually able to shift for a period into activities that largely deal with the outside. They are apprehensive, and rightly so, that once they do that, they may not be able to come back, because if you fall behind, it’s very hard to catch up again. But some of them are even able to shift back and forth more than before, and more of them are able to devote part of their time to interacting with the outside world. I see quite a difference between 1969 and 1977 [when I left] in that

regard. It's harder for me to tell what's happened since, but I think actually it's proceeded still more in that direction.

STONE: You said that when you came to Caltech, the faculty viewed you with a little suspicion because of your defense connections.

BROWN: Some did. And probably a few did right to the end, probably less so than would have been the case at most other first-rate universities. I know there were some predictions that there'd be riots over my appointment. Nothing like that ever happened. I never sensed any overt hostility—really almost none. I can't point to a single faculty member, and I don't think I can even think of any students, who ever tried to make a big issue out of this.

I had to prove myself, in the sense that if I had been seen as behaving in a way that conformed to the stereotypes of the military-industrial complex I have no doubt that there would have been a great deal of unhappiness and opposition. The difference between Caltech and some other places is that the people at Caltech, even those who had some apprehensions, were prepared to wait and see what happened and didn't start out with a conclusion before seeing the data.

STONE: Good scientists all.

BROWN: Yes. [Laughter] Partly that; partly that they were just interested in their own work. They weren't looking for an outside cause, so they would wait and see what impact there was on what really was important to them.

STONE: Do you think you did prove yourself? When you look back, how do you feel about it?

BROWN: I think that after the first few months, nobody thought of me as a former secretary of the air force; they saw me as the president of Caltech. In that sense, it worked very well.

Begin Tape 3, Side 2

STONE: Was there anything that you left undone that you would like to have done?

BROWN: I would like to have seen the engineering division be more nearly at the front rank, as compared with Stanford and MIT, than I was able to manage, or as has happened since. To some degree, that was a consequence, and remains a consequence, of the division's small size. There's no way to fix that without changing Caltech in a way I don't think we would want to change it.

STONE: That's the only way to change it—to make the engineering division bigger?

BROWN: I've not been able to find a different way. As I said, I tried to do it by using JPL to that effect, and to some degree that may have worked. But nowadays Caltech's connections with JPL are less close with the engineering division than they are with geology and geophysics or physics, math, and astronomy. So that didn't really work. Actually, in a few engineering areas, Caltech is still at the leading edge. It's true of earthquake engineering; it's true there's been a revival of aerodynamics in the Guggenheim [Aeronautical] Laboratory. But in information theory, and even in electronics and integrated circuits, despite Carver Mead and Tom McGill, who are as good as anybody, the institute doesn't really rank with the leaders in university engineering. And unfortunately, it is in computer science and related areas that engineering is having its biggest effect these days. I don't even speak to more recent issues, such as manufacturing engineering, which is extremely important to the United States and which bigger places—MIT and Stanford—can at least try to do. I don't know whether they'll succeed. I think it would be a hopeless mistake for Caltech to try to get into this area, because, again, you can't do everything. And that does take scale more than computer science takes scale, in my judgment. That was one thing I was unsuccessful at doing. I still think it remains to be done.

I also was unsuccessful during my tenure in bringing about an applied biology function. I started to do that, and now, as a result of other people's efforts, it's bearing considerable fruit—the Beckman Institute, for example. But I was frustrated that I wasn't able to get further in that while I was still there. I didn't begin it until my last couple of years, so perhaps I didn't have much of a chance. I'm glad I had the chance to start it. That was another unfinished piece of business which I think has a better chance of now being very successful than my unfinished business in engineering.

I'd also hoped that we could do more in social science than we were able to do. We had a couple of good economists and political scientists, but they never really formed a sufficient

critical mass to attract others, or to even keep all the very good ones we had. They have tended to go off to other places where there are more of their kind to speak with. I think that's unfortunate. There was, I think, a severe split among the faculty on this matter. I think that was part of the reason, but not by any means all of it, for the lack of complete success there. A good many of the science and engineering faculty regard social scientists with much more hostility than they regard humanists—partly because they feel that the word “science” in social science is a lie and see it as an attempt to appropriate some of the prestige that correctly applies to the physical, biological, and mathematical sciences and even to technology and engineering. A good many of the science faculty say that whereas the humanities have a distinct different dimension to bring to bear, social science is pseudoscience and that any relationship to “science” is nonsense. That attitude created some interesting faculty meetings. But in retrospect, I can't say that that prevented the institute from going ahead and making social-science appointments, or that the social scientists were driven away by this attitude. The best and most prominent of them have quite as much self-esteem as many scientists and engineers, although not as much as some of Caltech's scientists. The biologists always describe themselves as having a very high self-esteem—or, at least they've been described by others as so having. [Laughter] So, I don't think that's what drove the social scientists away. I think, instead, it was the lack of a large number of colleagues with whom they could work, and a correct feeling that they would never have the prestige among their Caltech colleagues that they would have if they were at Stanford or MIT, both of which have really outstanding economics departments.

The economists, in particular, were perhaps at the cutting edge of this dispute. In many ways, they are the most prestigious of social scientists, because they purport to be able to predict or influence the real world more nearly the way scientists and engineers do, than can the sociologists or the anthropologists. At Caltech, many of those who were skeptical about them—and in this I tended to share their skepticism to some degree—said, “The economists who have the most academic prestige and win Nobel Prizes are the ones who are most analytical and pretend to be most like the scientists.” In fact, of course, in order to be analytical, they have to assume away most of the driving forces in real economic behavior. By creating the ideal economic man, they eliminate all the real psychology, and that's what determines economic behavior. Interesting. I find it a very appealing intellectual conflict, with very important consequences for political behavior in the real world.

In any event, that's another area where we weren't able to do what I wanted to do.

STONE: Caltech seems to be moving toward what they call Big Science. Do you have any views and words of wisdom to offer?

BROWN: It will be interesting to see whether we can bring it off in a way that preserves Caltech's own culture and appeal. That attraction, that devotion to quality and to the kind of rational discourse that is a lot easier among a small number of people than among a much larger number, is one of Caltech's strengths. In going to the Keck Telescopes, to the gravity-wave experiment [LIGO, the Laser Interferometer Gravitational Wave Observatory], to more space experiments, and to the applied biology of the Beckman Institute, Caltech is obviously going to be dealing with, and will be responsible for, much larger institutional structures. Whether it can separate the inertia, bureaucracy, and impersonality that are probably inherent in, but are certainly associated with, such large institutional structures from its own core activities in a way that preserves the collegiality and other advantages of small size remains to be seen. There are some precedents: JPL is such an experience, and the results from that are mixed. I, at one time, had thought about, and we talked about at Caltech, the possibility of setting up a whole group of applied activities that would deal with other things, as JPL does with space exploration. The Environmental Quality Laboratory was such an idea. And by keeping it on the campus, we've actually managed to preserve Caltech's chief characteristics, but at the cost of forgoing big expansion in the EQL. We talked about doing something similar in biology without having a medical school, and so forth. In a sense, these developments are now starting to happen, although they're not necessarily in exactly the same areas. Some of it is just inherent in Big Science, as you said. Even though it's not applied, it's big, and Caltech faculty have learned already to participate as members of user groups. In the initiatives that we're talking about, Caltech is taking the managerial responsibility as well. In many cases, however, Caltech faculty will probably participate in ways analogous to user groups—certainly going to observe in Hawaii, or working at the other end of a continent-wide gravity detector, or working on an experiment in space involves some of the same professional difficulties.

I think we'll be able to manage, but it's not going to be easy. And inevitably we'll lose something. The question is whether we'll gain more than we'll lose. I think we probably will, because the leading edge of science in some of these activities is going to be in Big Science.

STONE: You had quite an influence on the institute. For example, you have said that it's not as insular as it was. So the institute learned and changed from you. What did you learn from Caltech?

BROWN: One thing I learned from Caltech—it wasn't the first time I learned it, but I learned it perhaps in intensified form—is that an institution depends on a number of very high-quality people. That number can be small or large. I don't think I had been at a place before that had quite such a concentration of intellectual power in a not-so-narrow—but not universal, either—area of human ability. It reinforced in me the belief that people who are very good at what they do are likely to be more understanding of other people's talents than people who aren't very good at what they do and who therefore try to do other things that they're not very good at either. That was one thing.

Caltech gave me an additional dimension of experience in dealing with different constituencies. I already had some of that as director of Lawrence Livermore National Laboratory and from my work in the government, because you had different constituencies there, too. Especially so in the government, where you deal not only with the Congress and with the other executive branch agencies but also with the media all the time. But Caltech, not to my surprise, was a much less hierarchical institution than any I had run before. In that sense, it gave me some additional experience and I hope some additional skills.

Finally, I learned a lot about science and engineering. The most fun I had at Caltech was on those days—and I tried to do it at least once a month—where I'd spend the whole day just going into the offices or the laboratories of four or five faculty members and spending three-quarters of an hour with them, just listening to them talk about their own research on a very personal, interactive basis. I'm still using some of what I learned then. That's in addition to the environment in general, where you couldn't help but learn a lot about what was going on in science and technology just by reading the proposals for research funding, all of which passed through me—and a surprising number of which I actually read—or by attending such popular-

level talks as the Watson lectures at Beckman Auditorium or lectures of other kinds. That was all intellectually very stimulating and very enjoyable. But the most fun was talking to individual faculty members and hearing them talk about their research—what they were doing, what they wanted to do, and where they hoped to go.

That was also useful, of course, in thinking about the future of the institution. But personally, it was the most informative and fun thing that I did. And I must say that if I'd been asked when I was a graduate student how would I most like to have spent my life, that probably—if I'd known about it—was what I would have said I would most like to do.

[Laughter]

STONE: Caltech was quite a break from all your other government jobs.

BROWN: Remember, I'd been a graduate student at Columbia, so I knew that much about universities. My work at the Radiation Laboratory was in a university-run laboratory—probably quite analogous to JPL. In fact, I always used to think, when I drove up to JPL, how much that setting looked like the hill above Berkeley, where the radiation laboratory in Berkeley is—it didn't look like Livermore at all—except that the scale was enormously bigger at JPL than in Berkeley. The mountains were a lot higher, the area was bigger, there were more buildings, but it was like that. There weren't many interactions with the faculty or the university at Livermore, but there were some. It is certainly true that I did not come to Caltech through the usual progression of university administrations that most university presidents take. And, although Lee DuBridge had run the MIT Radiation Laboratory, he had a very distinguished academic career before that, at Rochester, as a professor and I guess as physics department chairman. And of course my successors have had similar records—so I'm different in that regard, no question about it. But I think I may have been the right person for the time.

STONE: You had spent some years in Washington before coming to Caltech.

BROWN: Eight years.

STONE: Washington is a heady, addictive, seductive place. Did you feel that you were suddenly in a backwater in Pasadena?

BROWN: No, I didn't feel that way at all, for a couple of reasons. In the first place, although Washington is a heady place, eight years of it was enough. Especially those eight years, which saw such a change: from the high spirits and "We can do anything" attitude of the New Frontier and into Lyndon Johnson's great reform efforts of '64 and '65 that set the tone for the Great Society—the change and improvement in race relations and so forth—to the problems of the Vietnam years. I was not sorry to leave Washington. I didn't miss it in that sense at all.

Moreover, I didn't sever my connection. I was on the SALT [Strategic Arms Limitation Talks] delegation, starting in November or December of 1969, the same year at whose beginning I had arrived at Caltech. Although I didn't spend a very large fraction of my time on that, it probably was twenty percent. And that was a leading-edge Washington activity. I maintained my Washington connections to that degree. I spent hardly any time at the Defense Department, or as a consultant to the Defense Department, but SALT kept me involved in one of the more interesting things that was going on in the government.

To some extent, I think it troubled some people at Caltech in different ways. I think the faculty were to some degree troubled because it suggested to some of them that my main focus might not be at Caltech—that I was only thinking about when I might go back to Washington, which was not in fact the case. I did get interested again, but not until just a year or so before I left. I was, as I think most of them could testify, sufficiently in touch so that the mail went back and forth and I was hands-on even the few weeks at a time that I was away.

Interestingly enough, I think some of the trustees were also troubled, but for a very different reason. I think some of them may have felt uncomfortable in a situation where Harold Brown had a reputation as Harold Brown, not just as the president of Caltech. I don't think the faculty cared so much that I'd had an independent existence or an independent reputation. But I think some of the trustees saw it differently. If all I was was a president of Caltech; they were my Board of Trustees and they could say whether I could keep being the president of Caltech. But if I had an independent national reputation for doing something quite different, well then, I wasn't their creature anymore, and that's a problem. I know that didn't bother Arnold Beckman very much, and it didn't bother most of the others, but I think it may have bothered some.

STONE: What would you like to put in the record that you have not said?

BROWN: Actually, most of what I had left out has come out, not necessarily in answer to your questions this afternoon but stimulated by questions that you asked this afternoon.

You haven't asked me what my complaints were. [Laughter] Mostly I don't have any. I had some frustrations—not many. And even those I had, I want to be a little tactful about.

It seems to me that although the institute is more than anything else its faculty, the faculty perhaps take the attitude that they are the whole institution. They give perhaps too little attention to the part that the administration plays, or that the students play, or even that the supporting non-professional people play, in the life of the institute. I must say, I never felt the target of that much personally. I never felt that faculty considered me to be essentially the garbage collector. But I did think that occasionally they felt too much that way about other members of the administration. And among the faculty there is a group that regards students as a nuisance and another group that pays enormous attention to them. Maybe that's the way it has to be. It's not right to ask that everybody worry about everything or pay equal concern to everything. If the faculty, on the average, takes the right position, it's understandable that some maybe pay too much attention, others too little. But that did concern me from time to time.

I guess another thing that occurs to me is this: I think Colene was immensely liked by everybody at Caltech—by students, by faculty, by everybody. More liked than I was; and deservedly so. But I think she was imposed upon a little too much, in the sense that people expected more of her than they should have—although not more than she was willing to deliver. I think some people considered that if they needed something done, they could easily call on her, and she would be expected to spend full time at it. That is a common situation with the wives of administrators and university presidents. I'm sure she's not the only one who's experienced it. I think she felt it less than I felt it on her behalf.

I really don't have many complaints. Those were among the most challenging and at the same time enjoyable years that I faced. I do think that toward the end, the last year or two, I probably concluded that I was at a point not of diminished returns but of obviously diminishing returns. I didn't see the sense of staying on to retirement age in the same position—although it was easier to think about that at Caltech than at some other places, because it would not have become intolerable. And the old saying is true: Friends come and go, but enemies accumulate. But they don't accumulate too fast at Caltech. When I came, I was forty-two, which was about the same age that Lee DuBridge had come at, I think. Styles in the length of university

presidents' tenure had changed. It was clear that it didn't make sense to stay on indefinitely. I wasn't looking for something else to do. But had the government position as secretary of defense not come along, I suspect I probably wouldn't have stayed more than another few years. There are some university presidents who stay on a long time, even these days. Steve Muller stayed on at Johns Hopkins for almost twenty years. Derek Bok's been at Harvard for almost twenty years. Without reference to their individual performances, I think that's too long. So I've offered you another piece of opinion.