Lee Alvin DuBridge (Part I)
(1901-1993)

INTERVIEWED BY
JUDITH R. GOODSTEIN

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Subject area

Physics, administration

Abstract

Physicist Lee A. DuBridge became president of the California Institute of Technology in 1946. In this interview he recalls the immediate problems he faced, including his dealings with Robert A. Millikan, whom he replaced as chief administrator of the institute; institute financing and inadequate salaries. DuBridge also talks about the advent of federal support for peacetime science and Millikan's distaste for it; his close working relationship with Robert F. Bacher, who came to the institute in 1949 as chairman of the Division of Physics, Mathematics, and Astronomy; his recollections of the meteorologist Irving P. Krick, the physicist Alexander Goetz, and the chemist Linus Pauling; and his attempts to build up the Humanities Division.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH LEE A. DUBRIDGE

BY JUDITH R. GOODSTEIN

PASADENA, CALIFORNIA

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INTERVIEW WITH LEE A. DUBRIDGE

INTRODUCTION TO LEE A. DUBRIDGE ORAL HISTORY

by

JUDITH R. GOODSTEIN

University Archivist

As part of the California Institute of Technology Oral History Project, I interviewed the physicist Lee A. DuBridge, president of Caltech 1946-1968, in the Caltech Archives in Pasadena. DuBridge, one of the most influential American scientists of the last century, was born in Terre Haute, Indiana, on September 21, 1901. In 1918, when he entered Cornell College in Mount Vernon, Iowa, he intended to major in chemistry, but his sophomore physics teacher, Dr. Orrin Harold Smith, inspired him to become a physicist. Smith took DuBridge under his wing, hiring him as a teaching assistant in the laboratory and arranging his appointment, following graduation in 1922, as a teaching assistant in physics at the University of Wisconsin. At Wisconsin, DuBridge plunged into the world of modern physics with a course in atomic structure from Charles Mendenhall, the department chairman, which entailed learning scientific German in order to follow the assigned text, Arnold Sommerfeld's 400-page *Atombau und Spektrallinien*. He took the standard graduate courses in physics for that era: thermodynamics (with L. R. Ingersoll), electricity and magnetism (with J. R. Roebuck), statistical mechanics (with Max Mason), mathematical physics (with Warren Weaver). In the fall of 1925, after completing his dissertation research on the photoelectric properties of platinum, DuBridge successfully defended his thesis, mailed it off to the *Physical Review* for publication, and married his college sweetheart, Doris May Koht. He spent the next nine months at Wisconsin as an instructor in physics, teaching a full schedule and carrying on additional research in photoelectric emission.

DuBridge spent two years at Caltech (1926-28) as a National Research Council fellow under Robert Millikan's direction, followed by six years in the Physics Department at Washington University (1928-34), moving up the ranks from assistant to associate professor in 1933. The following year, DuBridge accepted an appointment as professor of physics and chairman of the Physics Department at the University of Rochester, where in 1938 he became dean of the faculty. At Rochester, DuBridge took up nuclear physics, inspired by the work of the Berkeley physicists Ernest O. Lawrence and Donald Cooksey, and arranged for Rochester to build a cyclotron. By autumn 1938, he later wrote, "we had the equipment in operation, producing protons of energy of about 5 million electron-volts—later raised to 6 or 7. In those days, this was the highest-energy proton beam in existence."

In 1940, a year after war broke out in Europe, DuBridge took a leave of absence from Rochester, moved his family to Belmont, Massachusetts, and set up shop at M.I.T., where he organized and directed a facility whose official name was the Radiation Laboratory but was quickly shortened to the Rad Lab. DuBridge's wartime laboratory developed microwave equipment for detecting the position of enemy aircraft—a

technique later called radar (for Radio Direction And Range)—in the centimeter-wavelength range.

In early 1946, DuBridge returned to the University of Rochester, only to realize that he couldn't easily go back to the prescribed routine of teaching and research in a university physics department. He had been a superb wartime administrator, and on September 1, 1946, he became president of Caltech. When the National Science Foundation was established in 1950, President Truman appointed him to the National Science Board, its policy-making branch. DuBridge served as chairman of many committees and boards in postwar Washington, including, from 1952 to 1956, the Science Advisory Committee of the Office of Defense Mobilization (later the President's Science Advisory Committee). Meanwhile, he continued to build Caltech into one of the finest science institutes in the country, retiring from the presidency in 1969 to become special assistant for science and technology to President Richard Nixon.

Lee DuBridge died on January 23, 1993, in Duarte, California.

Acknowledgments:

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Lee A. DuBridge **by Judith R. Goodstein**

Pasadena, California

Session 1 February 19, 1981

Begin Tape 1, Side 1

GOODSTEIN: In 1946, when you came here as Caltech's president from the University of Rochester, you must have been aware that you had a formidable predecessor [Robert A. Millikan, Caltech's chief executive from 1920 to 1945]. I think it was Warren Weaver who remarked that "no man could possibly succeed as president of C.I.T. [California Institute of Technology] unless R.A.M. can be persuaded to take his hand off the institute." Were there any problems along these lines?

DUBRIDGE: Warren and I were good friends. He was obviously concerned that if I came out here Millikan would still try to run the institute. Well, that wasn't my idea.

Warren, you know, had a great affection for Caltech. He'd been here—he was an assistant professor of mathematics here, 1917 to 1920. [He] was also my mathematics teacher at the University of Wisconsin; I probably took my course under him about 1923. Millikan had tried to persuade him to stay here, but for some reason or other he decided not to. He used to boast to me about it. He said, "You know, I was almost a professor at Caltech." And then later, of course, he was with the Rockefeller Foundation for twenty-three years. But he still kept in close touch with Caltech.

GOODSTEIN: I wondered about Weaver's remark, because Millikan came to the campus every day, I am told.

DUBRIDGE: Oh, yes, but he had his office over in East Bridge [Norman Bridge Laboratory of Physics]. The trustees had fixed up a lovely office for him. When I finally agreed to come in 1946, I was asked to meet Jim Page and Harvey Mudd in New York.[1] I went down from Rochester and met them. Jim Page told me the following: Good old Dr. Millikan was getting old. During the war he had been completely unable to handle a big administrative job, with all the people coming in, the government projects and so on. So Earnest Watson was practically made acting administrator of the institute, though Millikan was still was the head.[2] The day-by-day operation of the institute was under Dr. Watson. Dr. Millikan "kicked and screamed"—said Mr. Page—about many things. For example, in order to get people here—machinists, clerks, secretaries, technicians, and what not—they had to pay considerably higher salaries than Millikan was used to paying, and every time Millikan would run into a salary schedule of some sort, he would just scream to high heaven: "You're ruining the institute! We'll never recover! They'll want to keep on these salaries after the war is over, and we just can't afford to do it." Bill Stott was business manager, and Earnest Watson supervised the war work, and they went ahead and did what had to be done to get all those war projects out of the way.

Well, right at the end of the war—I'm still paraphrasing what Jim Page told me—the question was, What about a new president? [The trustees] had thought about it before, but they decided that it was no use trying to get anybody to come here during the war, so they would wait until after the war. Right after the war, however, Millikan began to assume that he was in charge again. But he still was not himself. And finally, said Jim, "I just had to go to Dr. Millikan and say, 'Dr. Millikan, you're through. You're retiring. We're going to get a new president, and we're going to ask you to move out of this office. We're going to fix up a nice office over in Bridge for you. You will be there and do whatever things you want. But you're not running the institute anymore.'" Jim said that they practically had to carry Dr. Millikan from his office in Throop Hall over to the new office in Bridge. Page could be pretty rough, and I just don't know how rough he was. Yet Page had a great affection for Millikan and a great admiration for him. He talked many times about what wonderful things Millikan did for the institute. But he'd

[1] James R. Page, a local investment banker and industrialist, was the chairman of Caltech's Board of Trustees. Harvey S. Mudd was a vice president of the board.

[2] At the time, Earnest Watson was dean of the faculty. Millikan's official title as head of the institute was not president but chairman of the Executive Council. Lee DuBridge succeeded him in 1946 with the title of president.

passed his prime, and Jim was impatient with him—as other people were. Millikan said, "You can't get anybody else to be president. You can't get anybody else who can run this institute. I've got to keep on with it." So they actually had to push him out of his office.

I had a strange feeling about Millikan's attitude toward me, at first, because shortly after I had accepted the position and the trustees had made the formal appointment, I was attending a National Academy of Sciences meeting in Washington. Dr. and Mrs. Millikan were there. My appointment had been announced.

GOODSTEIN: Was this your first face-to-face meeting with Millikan?

DUBRIDGE: Yes. It was very shortly after the appointment. The public announcement had been made the previous week. Anyway, I distinctly remember that at this meeting in Washington I walked over to Dr. and Mrs. Millikan, and I expected him to say, "Oh, Lee, I'm so glad." I said, "I'm looking forward to being at Caltech." He said, "That's good," and walked away. So I thought, Oh my gosh, he is not at all happy about *my* being his successor! After what Jim Page had told me, I guess I wasn't too surprised, but I did expect a little bit warmer welcome. However, when I got here, things were completely different. He was cordial, he was helpful, he never interfered. And one time he told me, "Lee, you and I may disagree on some things. We'll talk about them in private. I will *never* disagree with you when you bring

Fig. 1. Robert Millikan and Lee DuBridge have an informal conference in front of Throop Hall, 1951. Millikan died in 1953, six years after DuBridge became president. Caltech Archives.

things to the board." He still attended most of the board meetings. And that's exactly what happened. In the trustees' meetings, he would discuss things along with everybody else. Never did he raise a voice against anything I was suggesting.

GOODSTEIN: How lucid was he at the board meetings?

DUBRIDGE: He was all right. He was seventy-eight when he retired, so he was about seventy-nine when I came. He was quite lucid then. It was not until he was eighty-three or so that he began to get very weak and fuzzy.

GOODSTEIN: Was he still coming to the board meetings?

DUBRIDGE: He came to the board meetings all the way through. He came to commencement and everything else. I remember one commencement on the Athenaeum lawn. By that time, he was quite weak and could not walk very well, but he came to commencement, sat on the stage. In the academic recessional, the speaker and I and Jim Page led the procession off. And to follow us were to be Dr. Millikan and Charles Jones, who was a trustee and head of the Richfield Oil Corporation—a very nice fellow, by the way, who gave most of the camellias that we have on the campus. So Jones took Dr. Millikan by the arm to help him down the steps. Well, we were the beginning of the procession, and we went marching out, down the center aisle. We were about halfway down the aisle when I looked around. There was nobody behind us. Millikan was still staggering down the stairway on the arm of Mr. Jones. Well, I slowed down and kept looking back. When Jones got to the foot of the steps, he looked around and didn't see any procession, because we were way up the center aisle. But he saw a side aisle right ahead of him, so he led Millikan up that aisle. Well, it was a dead-end aisle. It ran smack into the old oak tree that used to be there. So the whole procession followed and found themselves dead-ended against this big oak tree, with chairs all around them. People had to scramble away, and move chairs so they could get out and get into the Athenaeum. He was extremely weak and feeble at that time.

But for the first three or four years [of my tenure], although he was old he was still very lucid and very active, and sometimes quite energetic. I remember when the National Science

Foundation bill was before Congress, Millikan was furious that there was going to be a government agency supporting research: "They'll spend millions of dollars unnecessarily." So he was energetic enough to get fired up about that, and he even went on several cosmic ray expeditions. It was only the last year or so [before his death in 1953] that he was really weak and fuzzy.

There were only three issues I can remember on which we had some disagreement, in private. One was the case of Dr. Irving Krick, a meteorologist.[3] Millikan thought Krick was the greatest guy in the world; he had brought Krick here. But everybody around the campus, and other meteorologists and other scientists around the country, said that Krick was a fake.

GOODSTEIN: Even other meteorologists said this?

DUBRIDGE: Yes, many of them. Also, Van [Vannevar] Bush, Karl Compton, and Warren Weaver had looked into his work.[4] "He claims to do things that he can't do. He claims to have done things he didn't do," they said. The particular issue then, which led to winding the whole thing up, was that the U.S. Weather Bureau came to us and said, "Look, we have Weather Bureau teletype equipment here, to give current weather reports." This equipment was over in the meteorology department, in one of the geology buildings. And they said, "But Krick is using that weather information—information the government is furnishing him free and which is supposed to be open information—he is using it privately to make long-range forecasts that are not at all in agreement with the Weather Bureau's forecasting. To commercialize our equipment, which has been loaned to you as a nonprofit institution, is quite against government rules." And they were going to remove it. Millikan came over to see me and said, "Now look, Krick is a great man here. He may have made some mistakes, but you've just got to keep him on.

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[3] Irving P. Krick, a pioneer of long-range weather forecasting and cloud seeding, and a chief adviser to the Allies on weather conditions for the D-Day invasion, established a meteorology department at Caltech in the 1930s and remained at Caltech until 1948, when the department was eliminated. He died at his home in Pasadena on June 20, 1996

[4] Vannevar Bush, who left M.I.T in 1938 to become president of the Carnegie Institution of Washington, headed the National Defense Research Committee (N.D.R.C.) during World War II and was the author of *Science, the Endless Frontier* (1945), an influential plan for postwar scientific research in the United States. Karl Compton, president of M.I.T. between 1930 and 1947, directed the work of the N.D.R.C. division for detection during the war and spoke at DuBridge's inauguration in 1946. Warren Weaver served as director of the Rockefeller Foundation's Division of National Sciences from 1932 until 1957.

Meteorology is a very important subject, and he's a leading meteorologist of the world. And I hope you don't try to get rid of him." I told him some of the things that had been brought to our attention—that [it was] very embarrassing, to say the least, to have [Krick] running a commercial weather station and forecasting service on the campus. So I said, "Well, Dr. Millikan, I don't agree with you." And he said, "Well, all right." And when I brought it up to the board, he never said a word. I then asked Krick to come in, and I said, "We're just going to have to discontinue this thing, and I think the best thing is to wind up our meteorology department." Krick was—at times, at least—a gentleman. And he was a gentleman this time. He said, "You mean you'd like to have me resign?" I said, "That would simplify matters a great deal."

GOODSTEIN: Did you tell Krick the reason you wanted the department taken away?

DUBRIDGE: Yes, we talked about it. I said, "You've been using this Weather Bureau facility, which was intended for nonprofit educational and research use. You're using it as a commercial support for your private weather forecasting service." He was making long-range weather forecasts, a month, two months or more in advance—and selling them at a profit!

GOODSTEIN: You can't do that kind of forecasting even today.

DUBRIDGE: He thought he could. He was getting into the cloud-seeding business, too. He later made his money that way. Anyway, he resigned. He had plenty of opportunities to do commercial work in the field.

Well, that was one thing that Millikan talked to me personally about. There was one other personnel case—that was Alex [Alexander] Goetz, senior. Now, Alex and his first wife, and my first wife, Doris, and I [*Figure 2*] were very great friends when Alex came over here as a research fellow in 1927, directly from Germany.[5] Neither one of them could speak English very well, but they were obviously charming people, and Doris and I became quite attached to them. We'd invite them for dinner and picnics, and they would bring their German-English dictionaries along and ask, "What is this?" and "What is that?" I remember once Doris served them some

[5] Alexander Goetz was an associate professor of physics and first came to Caltech in 1927 as a research fellow of the International Education Board.

Fig. 2. Lee and Doris DuBridge, Busch Gardens, Pasadena, California, ca. 1927. Caltech Archives.

pumpkin pie. After he ate it, he said, "What do you call this?" And Doris said, "Pumpkin pie." "Pumpkin?" Then he consults the dictionary: "That's what we feed the pigs in Germany." He was horrified.

We had many nice times together, and I thought Goetz was a fine fellow. He would tell the most fascinating and thrilling stories, which we later found out were all fabrications. He and Clark Millikan became friends, and [Clark and his] wife-to-be went on trips with them.[6] They loved to go into the desert. We didn't go on any desert trips with them as I recall, but Clark and others did. It was just about the time I was leaving [to go to Washington University, in St. Louis, as an assistant professor in the physics department] that some people said, "You know, Alex told everybody the story about how, when he'd gone out in the desert, he found a car parked at the side of the road. He looked into it and found a dead man there, who'd been shot. He went to the sheriff's office right away and the sheriff detained him and questioned him a great deal, then went out and recovered the body," and so on and on. A long, long story. Well, somebody was out there shortly afterward and went to the sheriff's office. The sheriff said, "Never happened. Nobody ever found a dead body in a car in my district. Nobody ever reported it to me." Goetz had made it up out of whole cloth. Then people began checking on some of his other stories and found that most of those thrilling adventure tales he told he'd just made up.

GOODSTEIN: This is in the twenties?

DUBRIDGE: Yes, I came here [on a National Research Council Fellowship] in '26 and I left in '28 to go to St. Louis. A couple of years after we'd been in St. Louis, Mrs. Goetz, of whom we were very fond, called us up and said, "I'm going through St. Louis. I would like to stop and

[6] Clark Blanchard Millikan, Robert Millikan's son, was the director of Caltech's Guggenheim Aeronautical Laboratory from 1949 to 1966.

see you." We were delighted to see her. She came and stayed with us. I had to go to the office, but she and Doris had a very long talk. She told the saddest tale about the way Alex had treated her.

Then, much later, Alex came east to a meeting and stopped in [to see me] at Rochester. It must have been '38 or '39, when the war in Europe was beginning. He set off on a tremendous denunciation of the British Empire: "They're trying to rule the world, and by golly they ought to be squashed down this time. At last, the Germans are going to show these British where they belong." And he went on and on about this. I was simply appalled and just kept quiet. There was no use arguing with him—he was almost irrational about it. Years later, when I was out here, I told somebody this incident about Alex, and he, unfortunately, told Alex. And Alex came in to see me. He said, "You implied to so-and-so that I was a Nazi at the beginning of the war." And I said, "Well, Alex, you certainly talked like one." He said, "I never did! That is a lie! I never said the slightest word in favor of Nazism." So he simply denied, as he had denied what his wife had told us. He said, "That's also a lie. We separated. We're perfectly good friends. I still write to her. I still visit her when I go to Germany. It was just a mutual friendly decision that we would separate, and nothing happened."

GOODSTEIN: There are in his papers hints that he was pro-Nazi. And he did sign letters, I believe, "Heil Hitler." If one is looking for evidence, certainly that is prima facie evidence of his position.

DUBRIDGE: Anyway, I was appalled and I never forgot it. And yet Alex completely denied ever saying any such things. Well, by the time I came back here in 1946, Alex had made himself unwelcome throughout the whole campus in various ways. He still insisted on teaching a course, but the students complained that he didn't prepare his courses. He rambled all over the place. Some of the other professors said the same thing—and that he was uncooperative and [not a part of] the physics group and doing only consulting work outside. So I said, "Look, Alex, you're just not fitting into this institution and the physics department anymore. I think you'd be better off if you'd resign and take some consulting job, or whatever you want to do. You could get jobs in industry." He said, "No, I have tenure here. I'm going to stay unless you fire me." Well, firing him would have been a problem, because he had done some good physics in the early

days. Some of his meteorological work was perfectly sound. One reason I liked him was because he and I had laboratories near each other in Bridge basement in ’27-’28. We were not working on similar problems, but he was very helpful to me in vacuum technology and instrumentation and the design of equipment, and so on. So we were close professionally as well as personally when I was here way back then. Then to have this whole thing blow up.…It hurt me. I felt terribly sorry. But he would not resign. I said, “Well, in any case, you’re at the end of the line as far as your position is concerned.” I think he was still an associate professor. So he accepted that and stuck around. They assigned no more classes to him, but he went on with his consulting work. He made ends meet somehow.

GOODSTEIN: Do you think he did this out of spite?

DUBRIDGE: If you’re in a university in Germany, you are elevated in stature in the community. And he felt that by being connected to Caltech, and by signing himself “professor of physics,” he could get much better consulting jobs, or much better remuneration from his consulting, than if he were independent or attached to a company. So he used Caltech as a base for his consulting work. But he did no more for Caltech after that.

GOODSTEIN: It seems to me that his low-temperature physics was a conspicuous failure.

DUBRIDGE: That was a sad thing, because when I was here he was still talking about low-temperature work and how important it should be. And shortly after I left, he built a cryogenic laboratory; he made liquid air, and at one time he claimed to have produced some liquid hydrogen. He called people in to see it. Well, he had a little thermos bottle with a drop or two of liquid at the bottom of it. Some people examined it and said, “We don’t know what it is, but it isn’t liquid hydrogen.” He didn’t have a low enough temperature, and with the kind of thermos bottle he had, it would have evaporated very, very fast.

GOODSTEIN: Cryogenics didn’t go anyplace here.

DUBRIDGE: No, not then, so they dismantled the equipment. In any case, Millikan came to me and said, "Now, I understand you're thinking about letting Goetz go. Now look, he's a great guy and he's done some wonderful things and he's just too valuable to let go. I hope you'll make sure that he stays on and gets the promotions he deserves." I relayed to Dr. Millikan some of the things that the physicists had against him—the lack of attention to his teaching, the protests from the students, and so on. And he said, "Well, he's still too good a man to let go." Well, we didn't let him go, but I did report to the trustees that we were not going to give him further encouragement. But Millikan never objected [to anything I said] in the trustees' meeting.

The third issue was the Executive Council. Millikan, when he was running Caltech, was very proud of the Executive Council as an administrative mechanism. He thought it would relieve him of a lot of administrative work—and I suppose it did—for him to be chairman of the Executive Council [rather than president]. The Executive Council consisted of three or four trustees and an equal number of faculty. When I arrived, it was still in existence.[7] One of its members was Linus Pauling. And already the trustees were disturbed about Linus's extracurricular activities; he was involved in defending various sorts of people. In a way, I sympathize with Linus, because [of a story] he told me. He had a house up in Sierra Madre, a lovely place with big grounds and so on. The Japanese, of course, were incarcerated during the war. At the end of the war, a Japanese former student appeared at Linus's place. They were good friends, and Linus admired him for his ability—he was a good chemist. So this former student came to see Linus and said, "Look, I've been in a prison camp and I don't know where I'm going to get a job and it's going to take me a while to get settled. Could I stay with you a few days until I get settled?" Linus said, "Yes, we have the guest house out in back. You live there, and you help us take care of the garden, and we'll give you your room and board." Maybe they gave him some spending money, too, just for taking care of the lawn. Well, the anti-Japanese feeling was still pretty strong around there, and the neighbors began picketing the Pauling place because he was "coddling a Jap."

GOODSTEIN: This is the end of the war?

[7] In 1944 the name was changed to the Executive Committee, and James R. Page took over from Millikan as chairman

DUBRIDGE: After the war, yes. Just when the Japanese were released. Linus told me about it sometime later. Well, Linus was incensed that people would picket him because he had befriended a Japanese student whom he knew and who was a good man and someone he wanted to get started on a career again. He actually talked about bringing suit against some of those who had been bothering them. Anyway, he got in touch with a lawyer, and that was his undoing. This lawyer was a real Communist sympathizer, everyone thought. He defended every Communist that was accused of anything, and he got Linus dragged into the political activity. Linus told me, "Before that, I'd never had any interest in politics, but I was so outraged at this action against this Japanese boy and his civil rights—and mine—that I went to the lawyer," who was sympathetic and who was willing to help him out. And the lawyer put him in touch with other ultraliberals. Whenever the lawyer [was representing] somebody who was alleged to be too liberal, or too left-wing, or too communist, Linus would defend [the person] in some way—not legally, but in speeches, signing petitions, and so on. It was because of the influence of this lawyer, and the trustees were already beginning to get uncomfortable about this. They said, "We just can't have a frank discussion on the Executive Council with Linus there." Anyway, I thought it was a bad management system to have trustees and faculty together on the council. There was a history professor, [J. E.] Wallace Sterling—the greatest guy in the world, but he was a professor, he wasn't chairman of his division. Yet he was on the Executive Council, giving orders to his own boss, Professor [Clinton K.] Judy. Linus was chairman of the Chemistry and Chemical Engineering Division. But one of the other professors was not a division chairman, Earnest Watson. It was a curious inversion of organization.

GOODSTEIN: Wasn't Earnest Watson at that time the acting chairman of physics?

DUBRIDGE: Yes, for a while—William V. Houston, who was chairman of the physics division, left [to become the president of Rice Institute in 1946,] just before I arrived. Earnest was also dean of the faculty. Of course, as dean of the faculty it was natural that he'd be on the Executive Council.

GOODSTEIN: When you came, did you go to Executive Council meetings?

DUBRIDGE: Oh, sure.

GOODSTEIN: Millikan did not go to those anymore.

DUBRIDGE: Page insisted. He said to me, "Now, you're not going to be chairman of the Executive Council. That's no title for a guy to have; you're president. But of course you'll be chairman of the Executive Council *ex officio*." Page was [running] the council and was also the chairman of the Board of Trustees. So we had some meetings of the Executive Council. It was obvious that Page and Harvey Mudd and several others disliked Pauling intensely and just could not bring themselves to speak to him. We finally began not having meetings of the Executive Council but just having the Executive Council's trustee members meet as an executive committee of the Board of Trustees. Well, I talked to Millikan about this. I said, "I think it's best to abandon the Executive Council idea, and to have an executive committee of the Board of Trustees [instead]. And then to have an organization of division chairmen and deans of the campus for the academic policy-making." "Oh, no," he said, "you can't give up the Executive Council. It's one of the most important things we have at Caltech, and it's a breakthrough in university management, and we ought to be leading the way." But again, when I finally proposed to the board that we abandon the Executive Council, [Millikan and I] had no confrontation on it. Then I organized what I called the Division Chairmen's Committee—what we now call the Institute Administrative Council. It was just the division chairmen and the dean of the faculty, and Ed [Edward C.] Barrett, who was then the comptroller, and myself. That was the whole group. We'd sit around the table in my office in Throop Hall. Later there was actually a little conference room right across the hall from my office; the trustees' meetings were in there, too. At that time, the trustees' meetings were attended by no more than ten or fifteen people. But, as I said, Millikan did not publicly object to the [dissolution of the] Executive Council. Both Watson and Sterling supported it, said it was a good idea—that the Executive Council was kind of a strange mixture of people anyway.

The point I'm making is that although Page had to be pretty rough with Dr. Millikan, and although Warren Weaver worried that Millikan couldn't keep his hands off, there was no trouble after I got here. In fact, we were very cordial. I used to go over and see him and ask him about things. We talked about the National Science Foundation and political things—everything in the

most friendly way. There was never a word of disagreement between us. We became good friends. I loved to go over and chat with him in his office; and he'd occasionally stop and chat with me. He was magnificent. Many people would come to me and say, "It must be awkward having Dr. Millikan looking over your shoulder all the time." I said, "It's wonderful. Dr. Millikan is a fine gentleman. He knows that I'm the president and he isn't." He never in any way tried to run the institute or undercut what I was doing.

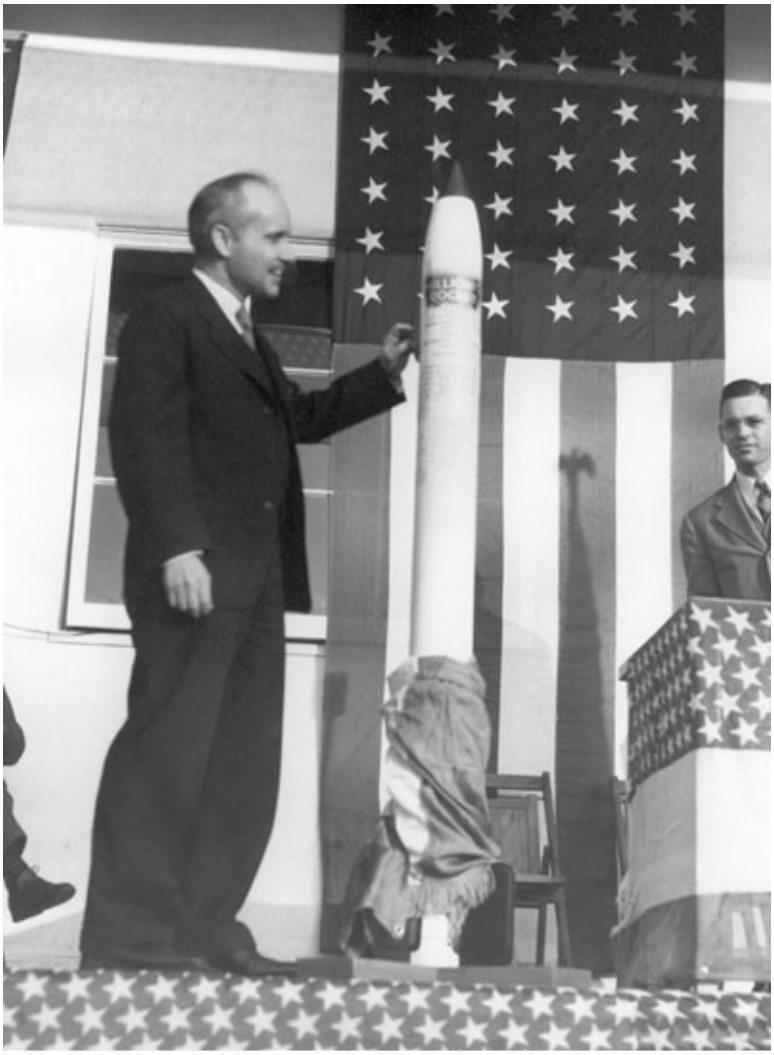

Fig. 3. Earnest C. Watson, administrative head of Caltech's World War II rocket project, with the Institute's one-millionth rocket. The five-inch high-velocity aircraft rocket, nicknamed Holy Moses, first went into combat use in July 1944. Caltech Archives.

GOODSTEIN: Would you have come with the title that Millikan had? He was so proud of *not* being president.

DUBRIDGE: No. You know the famous story: Somebody asked Richard [Chace] Tolman, "What is a chairman of the Executive Council? What does that mean?" And Tolman said, "Well, a chairman of the Executive Council is just like a president, only more so." In other words, Millikan ran the institute, and he used the Executive Council as his shield. He'd bring up some proposal and say, "I want you to mull it over. Each of you think about it." And even though everybody [on the council] spoke against it, he would end up saying, "Well, I'm glad you agree with me. We'll go ahead." He would overrule them continually. He was a real boss. Nobody had any doubt about it, and nobody made any bones about it. He did such magnificent things for this institution, building it from scratch. Already by 1940 it was one of the two most distinguished institutes of science and engineering in the country. People loved to come here, and the faculty directory reads like a *Who's Who*.

GOODSTEIN: Maybe he saw something good in deliberately avoiding the title of president.

DUBRIDGE: Well, he thought so. Most of the faculty I talked to about it took it as a joke. You know, it was just his way of pretending that the institute was a democratic operation and that the Executive Council of trustees and faculty made all the important decisions—and no doubt, they did make many. But if they didn't agree with him, why, he would go ahead anyway. And you just have to admire the results—the way he brought in these distinguished and wealthy trustees, and got the [Caltech] Associates going.[8] People just used to love to come out to the Associates' affairs and were always so anxious to meet Dr. Millikan. When Einstein was here, they had the privilege of going and hearing Einstein, [A. A.] Michelson, and Millikan—all Nobel Prize winners—talk. Jim Page used to say that Millikan would bring a prospective Associate here and say, "Come on, I'll show you something." He would bring over a microscope and say, "Now we're splitting an atom." You don't split an atom under a microscope; Page was just mixed up. But he knew that Millikan charmed his visitors. Then Millikan would show off Carl Anderson's cosmic ray experiments, with all those beautiful tracks of high-energy particles. That was the most spectacular development of the time, with the [discovery of the] positron and later what they first called the mesotron and then the meson.

[8] The Caltech Associates, formed in 1926, is a financial support group for the institute.

All the while that Millikan was organizing the institute, he was doing brilliant research. He and Ike [Ira S.] Bowen did some of the most fundamental spectroscopic work that had been done up to that time, in the far ultraviolet—he pushed the ultraviolet spectrum much farther than anybody else ever had and revealed wholly new spectral phenomena. He carried his cosmic ray work on all over the world, together with Vic [Henry Victor] Neher and William Pickering and others.

GOODSTEIN: Did you ever give any thought to continuing research when you came here as president?

DUBRIDGE: Not really. Willy [William A.] Fowler and Charlie [Charles C.] Lauritsen once said, "Look, why don't you come on over and do some experiments with us in the Kellogg Laboratory [W. K. Kellogg Radiation Laboratory]? We'd be glad to have you join in on some of these experiments." And I said, "I would love to do it, but give me a little time to get organized."

Well, that never happened. I think the reason was that in 1940, when I went to [the Radiation Laboratory at] M.I.T., I had to abandon research and do administration. Then when I went back to Rochester at the end of the war, I started working with the cyclotron and giving freshman lectures again. And I couldn't do it. It just didn't come back. I used to pride myself on the demonstration lectures I gave, but I'd forgotten the experiments, I'd forgotten this and that. I'd simply lost my facility. And then I got into the cyclotron business and began reading about what they'd done in Los Alamos and Chicago and Oak Ridge during the war. That was all far beyond where we had left off in nuclear physics in 1940. It would have taken me an endless amount of time to get caught up. So I finally decided that maybe administration was my field, and I accepted the Caltech job.

GOODSTEIN: You first told people you were not interested in more administration. Then you went back to Rochester. Is it about that time that the call came from here?

DUBRIDGE: Max Mason called me very shortly after I got back to Rochester. As a matter of fact, before that, in late 1945, Frank Jewett had called me at home in Belmont, Massachusetts,

and said, "Lee, somebody told me that you're not interested in administrative jobs. Now, is that right?" I said, "Yes." He said, "Well, all right, I just wanted to know." Only after I hung up did I realize that he was probably talking about Caltech.[9]

GOODSTEIN: You didn't call him back?

DUBRIDGE: Well, there was no reason to call him back; I had simply said, "No, I'm not interested in administrative work." Of course, he reported this back to Caltech and said, "We thought Lee DuBridge would take the job, but we can't get him." So it was dropped there, until Max Mason called me at home in Rochester, saying, "Lee, you've got to come out here." You know, he was my professor of physics at Wisconsin and my advisor, too. We'd been together and done all kinds of various things since. He said, "We need you, you've got to come out here. This is the best place for you to be. And the trustees want you to come." And I said, "Oh, Max, I thought I was going to go back to research and teaching." He said, "Oh, come and forget it." He and I always talked very frankly with each other. So I said, "Well, I'll think it over."

Well, I could not make up my mind. I'd talk about it, and I'd go to the lab and say, "Gee, it's nice to be here, you know, with our nice cyclotron." At about that time, the Office of Naval Research came along and offered to finance a bigger cyclotron, because our little one was way out of date—although it was used for years and years afterward for low-energy experiments. My close associate [at Rochester] and great friend and colleague Sidney Barnes said, "We should have a bigger cyclotron." But I couldn't help thinking of Art [Arthur] Roberts' song, "Take away your million dollars, take away your billion volts. Let's be physicists again." That stuck in my mind. This was going to be a multimillion-dollar project, a big cyclotron. I was somewhat uncomfortable about that. Sid was a little irked with me, that I didn't plunge ahead with enthusiasm. But finally we did, and the contract was arranged before I left.

[9] Max Mason headed the Rockefeller Foundation from 1930 to 1936 and then moved to California to become chairman of Caltech's Observatory Council. He also served as a trustee of Caltech from 1945 until 1951. Frank B. Jewett, the president of Bell Laboratories, was a member of Caltech's Advisory Council.

In the meantime, Max Mason was waiting for my call. One time when we were in the middle of dinner with some guests, the phone rang and Max talked my arm off while the guests were there waiting for me. I couldn't hang up. He said, "Look, time's getting short. You've got to make up your mind." Then he gave me a long, long talk about it. And then they asked me to go and visit Jim Page and Harvey Mudd in New York to talk about the arrangements. This I did.

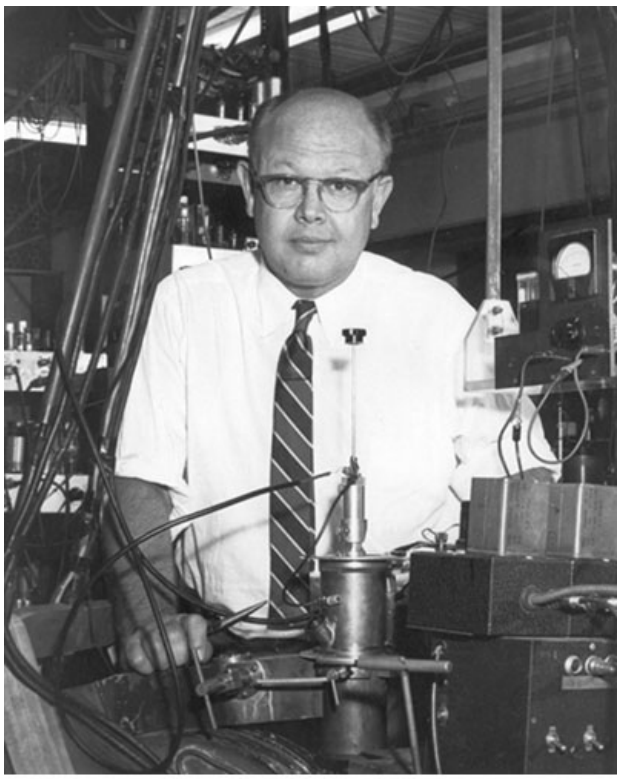

Fig. 4. William ("Willy") A. Fowler in 1956 in the W. Kellogg Radiation Laboratory, where he did much of his research in nuclear astrophysics. Fowler shared the 1983 Nobel Prize in physics for his work on nucleosynthesis. Caltech Archives.

Finally I went to President Alan Valentine at Rochester and said, "I've decided to go to Caltech." "Oh," he said, "That's awful. What are we going to do? You know, all of our good people are leaving." Poor Alan, I felt really sorry for him, because some other people had gone away to the war and didn't come back. He was struggling. I finally told Doris. I said, "Well, I've told President Valentine that I'm going to Caltech, going to California." She heaved a sigh of relief, and said, "I've been wondering what you were waiting for." She had wanted to move the whole time. The Rochester climate didn't agree with her or with the kids. It was a joyous thing when it was finally settled.

GOODSTEIN: Do you think that you hesitated on the contract for the cyclotron at Rochester because in your own mind you weren't sure if you wanted to go forward with research?

DUBRIDGE: I didn't want to get into the big-time Berkeley Radiation Lab type of operation. But I finally agreed that we'd go ahead with it, and it was built, and it was a very successful cyclotron for many years after I left. Well, I don't know. I was in a confused state of mind, I guess, at the end of the war.

GOODSTEIN: This brings us to the letter that you wrote to Jim Page on April 14, 1946, in which you stated that it is "not my personal ambition to become a money raiser." By then, you had made up your mind to come here. At that point, you wrote a series of letters, setting forth some conditions.

DUBRIDGE: Yes. I was a little worried about some of the things Jim had mentioned. I thought we had better have a clear charter before I started. I expressed the view that Caltech had not held its own in comparison to other colleges and universities during the previous ten years. It is true that the Depression was very hard on Caltech. The salaries were cut. There wasn't enough money to do a good deal of the research that people wanted to do. The Kellogg Laboratory was struggling for funds. There was no Office of Naval Research or National Science Foundation then. There were not many key people left. The Lauritsens, Carl Anderson, Willy Fowler, Linus Pauling, and others were loyal and stayed on. I heard—only by second hand, really—that the morale at Caltech was a little low, and that they were having serious financial problems around 1936 to 1940.

Well, when the war came along, of course, Caltech was completely turned into a war research laboratory and naval training school, and they did a magnificent job. Still, when I came to visit here in 1946 and talked to some of the people, I realized that the faculty morale was low. The salaries had not changed since before the war, and they were very low. Millikan was not inclined to change them. He didn't believe in big salaries for faculty. He told me that. He said, "The sunshine is enough to make a difference of a few thousand dollars in their salaries compared to what you'd get in New York or Boston or Rochester or what not." He believed that, and some of the faculty believed it, too.

I think Warren Weaver also said, "You know, they're in trouble, in financial trouble, and it's going to be a job getting Caltech back on the path ahead." And so this was what I meant, in writing Mr. Page. I also pointed out that largely because of its youth, Caltech had not had time to become a well-balanced institute of technology. Millikan had not wanted that balance. He wanted Caltech to take on just those few areas that we could afford to. So there were some curious inconsistencies, I thought, in the total program—especially after the war, when there were so many fields opening up that Caltech ought to be into.

GOODSTEIN: Do you remember any in particular? I have heard, for example, that Royal Sorensen saw only one aspect of electrical engineering and that was power transmission.

DUBRIDGE: That's true. That's what he did—the high-voltage laboratory and electric power transmission.

GOODSTEIN: And he simply wasn't interested in communication.

DUBRIDGE: Oh, no. You see, there was no electrical engineering department, as such. It was part of the Division of Physics, Mathematics, and Electrical Engineering, as it was called then—that was before astronomy [became a part of the division]—so Royal Sorensen reported to the chairman of the division. The high-voltage laboratory was really almost a physics laboratory. They worked on high-voltage things not connected with power, but a lot of it was connected with power transmission—switching, corona discharge, insulators, and so on. They made high-voltage transmission possible for long distances, and it was a great thing. And over in the Division of Civil and Mechanical Engineering and Aeronautics—well, the only distinguished thing in engineering then was aeronautics. [Theodore] von Kármán had built a fine aeronautics department, but the civil engineering was routine. Mechanical engineering was routine, except the part that was connected with aeronautical problems. Chemical engineering was very good under Will [William N.] Lacey, but it was very small and not in the engineering division—it still isn't. It was just Will Lacey and Bruce Sage, practically, alone. Chemistry and biology were in good shape. As for geology—there were feelings around the campus and outside that the geologists were still back in the nineteenth century, analyzing rocks.

GOODSTEIN: Was the feeling that this was the direction provided by [John P.] Buwalda?

DUBRIDGE: Yes. Well, Buwalda and Chester Stock. Stock was a paleontologist, and many people saw no future in paleontology, at least at Caltech. Stock had a tremendous collection of fossils, mostly from the La Brea tar pits. He'd been a key person in exploring and identifying the fossils there, and working out the history of the tar pits. So it was a good outfit but, again, it was a one-man show, practically, with a couple of assistants. Buwalda was a classical geologist

and a good one—a "rock geologist" they called him. Nothing in the new fields of geophysics had been initiated, although Beno Gutenberg had built a fine Seismology Laboratory. Anyway, I had this general impression from talking to various people that there was a job to be done in initiating some new and more modern activities.

Shortly after I came here, we transferred electrical engineering into the engineering division. Fred [Frederick C.] Lindvall was chairman of the division. He was an electrical engineer, so he didn't neglect electrical engineering, but he pursued the other areas, too. George Beadle was brought into the Biology Division about the time I arrived; I had nothing to do with his appointment. That was a delight, to see what Beadle was doing in biology. I had no qualms about that. They were already talking about a closer relation between chemistry and biology. And we talked with Warren Weaver at great length about that; he's the one who made that possible, through the Rockefeller Foundation.

GOODSTEIN: Then there's physics.

DUBRIDGE: Physics I had no special qualms about either. Millikan and his cosmic ray group, [Carl] Anderson and his cosmic rays, the cloud chamber—it was a very powerful and very productive operation. Millikan by that time had given up his hot-spark spectroscopy. I also felt that with the coming of Palomar there ought to be an astronomy department on the campus, which there wasn't. Ike Bowen really was Millikan's spectroscopy associate, but he got into astronomy because the astronomers came to him with some spectral lines they couldn't identify. And Ike identified them as the spectra of highly ionized atoms—just the same as he'd been working with in his spark chamber. They thought the spectrum was from a new element—nebulium—and he identified it as iron, or something else, in a highly ionized state, which had never been examined before except by Bowen and Millikan. Well, that got Bowen interested in other astronomical spectroscopic things. He began comparing the spectra they were getting [from] nebular clouds of gas and dust and so on and was able to identify quite a number of other spectra of other highly ionized atoms.

It was clear that we really needed some astronomers to work at Palomar, and [that we ought not to] depend on the Mount Wilson group to do all the Palomar work, even though they had designed Palomar and supervised the building of it. Max Mason was the chairman of the

Observatory Council, which supervised the actual construction and building of the dome and the telescope. He was a very ingenious designer and physicist, but he was not an astronomer. The Robinson building [Henry M. Robinson Laboratory of Astrophysics] was here, but astronomy on the campus didn't amount to much.

So there were all these things that we had to get going. And by that time I realized that the Office of Naval Research was already active in building basic research in the universities. By the time I got here in 1946, the O.N.R. had been in touch with Charlie Lauritsen and others—because Charlie was close to the navy and he knew the O.N.R. people pretty well, as we did at M.I.T. So already there were a number of important O.N.R. projects on the campus. The O.N.R. was helping to finance the Kellogg Laboratory and Anderson's work. I realized that here was a great opportunity to get support for top-notch research. Now, Millikan didn't want government support—he distrusted it and disliked it.

GOODSTEIN: So he distrusted the Office of Naval Research support, too?

DUBRIDGE: He never actually objected to me about it, but I [knew] it made him uncomfortable— especially after he blew up about the proposed National Science Foundation. I would go over to his office quite often to ask him questions, and he would come to my office. So there was considerable back and forth. I don't know why I went this particular time, but in the course of our conversation, he suddenly pulled out a paper and said, "Have you seen this act that's before Congress to create the National Science Foundation?" I said, "Yes. As a matter of fact, I testified before a committee of Congress at the request of Vannevar Bush, advocating the establishment of the Science Foundation, explaining what its value would be to science and to the security of the country." So I was all for it. But Millikan was absolutely irate. He got very upset and said, "Look, they're just going to set up a big bureaucracy there. It says here there'll be a director of the Science Foundation at a salary of $15,000 a year." He said this in horror. "An assistant director at $12,000 a year! Another assistant director at $12,000 a year! That's just a big bureaucracy. They'll come in and they'll try to run our research and our universities, and there'll be government control of everything." Well, I said nothing, because already the Office of Naval Research was doing an N.S.F. kind of job. That's the reason we were able to build up the Kellogg Laboratory and the other things around the campus. I did feel that having

all the basic research supported by the navy was wrong. And I made a statement to that effect, which was later thrown in my face many years later—but that's another story. We needed a civilian organization to support science.[10]

Now, originally, the arrangement was that O.N.R. would pay the salary of the faculty people on a research project for the summer months. In those days, all faculty people were on a nine-month stipend, and many faculty felt they needed to earn some money during the summer or they would go away and get a job teaching someplace else. So in the beginning only the summer stipends were supported by the research contracts. Then, sort of nationwide, people started saying, "Look, if a professor is spending half his time on research for a particular government contract and only half his time teaching or on other university duties, then it's only fair that the research-supporting agencies should pay their share of his salary." There were many professors who didn't like that, but there were others who said, "Well, fine, we don't care where the salaries come from, except we'd like to know what happens if the contract gets canceled." And we assured them that if their contract was for any reason abandoned or canceled, the university would continue their salary at the same level. That's one reason we went to the twelve-month salary plan, at Earnest Watson's suggestion. He said, "Many of our people are getting twelve months' salary, effectively, but three months comes from O.N.R."—and later N.S.F. or N.I.H. [National Institutes of Health]—"and the other nine months comes from the Caltech budget." Earnest was desperately anxious to increase the salary levels at Caltech, because as I told you, Millikan had kept them down and screamed when they seemed to be going up during the war.

And I was completely in sympathy with [raising salaries] and thought that was one of the first jobs we had to do. As a matter of fact, when I had talked to Jim Page, he said, "I don't think you'll have to worry about that. You know, during the war Caltech was almost entirely financed by government funds—the naval training program, military research activities, and so on. Hence, much of our normal income hasn't been used. We've got it stuck away in a reserve fund." There was three million dollars there, I think. He said, "We've got this money and it's invested, but it's free to be used for whatever we want to do." That's how we could increase

[10] The act establishing the National Science Foundation was signed by President Harry S Truman on May 10, 1950, "to promote the progress of science; to advance the national health, prosperity, and welfare; to secure the national defense; and for other purposes."

salaries. So with the money that was accumulated during the war, we were able to make substantial salary increases almost immediately.

Then Earnest came up with this twelve-month plan. Now, the people already on summer stipends were not so much affected, but they got a raise and at the same time we made an across-the-board merit increase. This enabled us not only to keep up the salaries of those doing the research but to spread it across the campus, so that other people shared in it even though they were not on research contracts. Outside of a few initial objections, and a few continuing ones, the faculty accepted this twelve-month plan, mostly with enthusiasm, because it meant a big change in their financial situation, which was badly needed. Good people around the campus were getting only three or four or five thousand dollars a year in salary when I came. It was ridiculous.

GOODSTEIN: I noticed in your letter to Page that you said you "might have to depart widely from certain well-established traditions and practices."

DUBRIDGE: I had the Executive Council in mind already. When I talked to Page and Harvey Mudd in New York, they said, "You know, there's some big changes that are necessary there." One of the things they had in mind—I didn't quite realize it then—was to get Linus Pauling out of the administrative picture and try to "pin his ears back," as Harvey Mudd said. This worried me a bit, because I didn't quite know what they meant by that. I wasn't going to pin anybody's ears back.

GOODSTEIN: When you were here as an N.R.C. [National Research Council] fellow in the twenties, did you know Pauling?

DUBRIDGE: Yes.

GOODSTEIN: Was he a hero yet?

DUBRIDGE: Well, he had not done his greatest work yet, though he had done some very good chemistry. He was certainly regarded as an up and coming fine chemist. But he hadn't attained

the distinction then that he later did with the theory of molecular bonds, the protein structure work, and so on. He was doing good work and he was admired because he had taken the time to get to know the people in physics who were acquainted with the new quantum mechanics. He could write his book, applying quantum mechanics to chemical reactions, which was a great achievement and a great contribution. I don't remember when that book was published, but it was sometime after I left in 1928.[11]

GOODSTEIN: I think it might be useful to have a session on personalities—Pauling is one and von Kármán is another. And I'd like to talk about Bob [Robert F.] Bacher.

DUBRIDGE: I could go on about Bob Bacher for a long time.[12] Bob Bacher said a week or so ago, "Lee, there are some things I'd like to talk to you about. We haven't had a good chat for a while." So on Monday he called up and said, "What about getting together today?" So I went to his home and we sat by the pool and talked until lunchtime. Then we joined a little group at a round table at lunch; then I went back to his office and talked till three o'clock. We went over everything, all of our mutual interests and experiences. You see, Bob is probably—certainly at Caltech—one of my oldest friends. I knew Carl Anderson and Charlie Lauritsen before I knew Bacher,

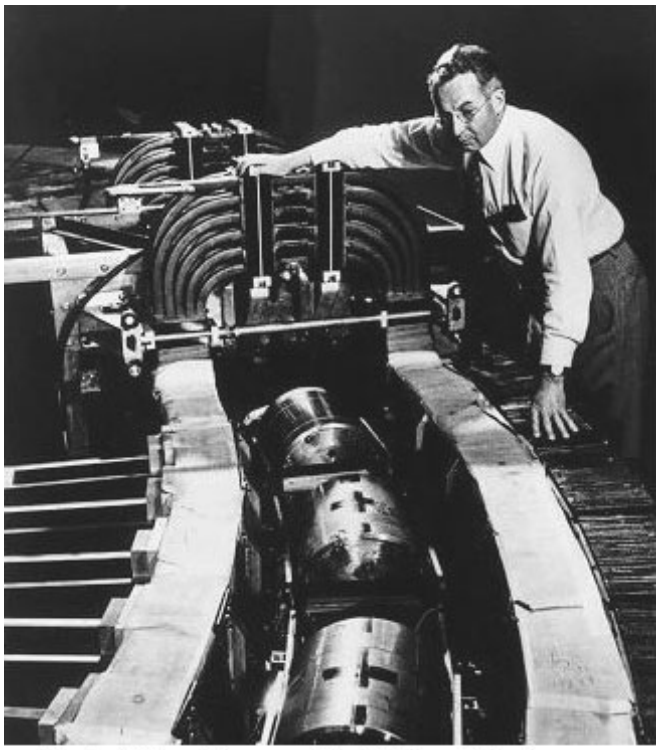

Fig. 5. While chairman of the division of physics, mathematics, and astronomy, the physicist Robert F. Bacher initiated construction and use of a new electron accelerator, called a synchrotron, shown here. In 1962, he became Caltech's first provost. Caltech Archives.

[11] Linus Pauling, *The Nature of the Chemical Bond and the Structure of Molecules and Crystals* (Ithaca: Cornell Univ. Press, 1939).

[12] Robert Bacher, an atomic spectroscopist turned nuclear physicist, guided Cornell University's nuclear physics laboratory to prominence in the 1930s. During World War II, Bacher worked first at the M.I.T. Radiation Laboratory and from 1943 to 1946 at the Los Alamos Laboratory on the atomic bomb project.

but I met Bacher the year after I left the fellowship at Caltech. I went to the summer symposium at Ann Arbor in 1929, and Bacher and Jean [Dow] were there—they weren't married yet. Our paths crossed in so many ways, over such a long period of years. He was at the [M.I.T.] Radiation Lab two or three years. He was nearby at Cornell, and we met often. We were on the President's Science Advisory Committee together, and I was on the General Advisory Committee of the A.E.C. [Atomic Energy Commission] while he was an A.E.C. member. Our lives have been tangled in a marvelous way for all of those years. We've had our disagreements, of course; when he was dean of the faculty, we didn't always agree on things. He sometimes thought I wasn't handling certain things right with the trustees.

GOODSTEIN: Are you responsible for bringing Bacher to Caltech?

DUBRIDGE: Oh, yes. Well, I don't want to take full responsibility for it. Bill Houston had just left, you see. It was clear that Earnest Watson should not try to be dean of the faculty and chairman of the division [then the Division of Physics, Mathematics, and Electrical Engineering]—he was acting chairman. So Carl Anderson and Willy Fowler and Charlie Lauritsen and I talked over the chairmanship a great deal. I think at first Charlie thought that maybe Willy Fowler or somebody inside should be made division chairman, but as soon as I mentioned Bacher, they said, "Oh, he's the guy!" So there was agreement there. Bob had been here as an N.R.C. Fellow and they knew him. Sure, Bob was the guy, but we couldn't get him because he was on the A.E.C. I said, "Well, he won't be there long. Can we wait for him?" And they said, "Sure, we'll wait for Bob." So it was a mutual thing. I was certainly delighted when they agreed that he would be the right appointment for chairman.

I have nothing but admiration for the people on the faculty that you've mentioned, except, you know, I had troubles with Linus and difficulties there. I never got very close to W. B. [William Bennett] Munro. He was another anomaly. He had been the chairman of the Humanities Division and [treasurer of the Board of Trustees] and he was one of the Executive Council members.

GOODSTEIN: That's right. He obviously was thwarted in his ambitions to build up the humanities here. Did you ever get that impression? In fact, we haven't really discussed the

humanities at all and their place in the institute. There were apparently some very famous faculty meetings in the thirties where the issue of the humanities and its role in the institute came up.

DUBRIDGE: Well, everybody admired dear old Clinton Judy, and they loved Harvey Eagleson. Wally Sterling was a hero. Also Winch [Louis Winchester] Jones. The fact is that people were not very enthusiastic about Munro—he was sort of gruff. You know, he was head of the Humanities Division at first, but then Clinton Judy became head and Munro gave more of his attention to the board, and to buildings and grounds matters. He didn't seem to be a part of the faculty. But there was a very warm feeling toward many of the individuals in the division when I came. I guess maybe a reflection of Millikan's attitude is a remark that Millikan made to Winch Jones, which Winch reported to me. Millikan wanted the humanities here, but only as a teaching group. Millikan once came to Winch Jones and said, "We want you to be registrar." Winch said, "Well, Dr. Millikan, you know, I've had no experience in that, and there are other people around the campus that would be much better at it than I am." And he named some people in the science divisions. Millikan said, "Oh, they're too busy on important things. I want *you* to do this job." So Winch did. In other words, Millikan did not regard the Humanities Division as important in building the name and fame of the institute, but they were serving a useful purpose in teaching the boys their English and history. Millikan had a great admiration, however, for Munro. They were very good friends. He brought him in from Harvard [in 1925] to start the Humanities Division, and he wanted very much to have a good humanities division. Now, how much Munro pushed for something more than what Millikan wanted, I don't know.

GOODSTEIN: Did you play a role in bringing Hallett Smith here?

DUBRIDGE: Yes, I appointed him. I asked, "Who do you think ought to come in?" when Clinton Judy retired. I said that we ought to get a top-notch scholar in the humanities field.

Begin Tape 2, Side 1

The Humanities Division did search the country for a good candidate for the job, and they finally said, "The best man we've been able to get track of is Hallett Smith of Williams College. He's an Elizabethan scholar. He could involve the Huntington Library people." I think Louis Wright was here then, an Elizabethan scholar, and he thought highly of Hallett's work. And so, on a trip east I made a date to see Hallett and had a long talk with him. I was well impressed with him and came back and talked to Earnest Watson and the humanities people, and I said, "I agree, I think he'd be a good man to do it." He *was* a good man. Yet he lacked administrative skill and that eventually got the Humanities Division into a little trouble.

GOODSTEIN: You chose him for his scholarly qualities?

DUBRIDGE: Right, yes. He'd been head of the department, so I thought he'd had a little administrative work, at least. It was important to get people who were recognized as good scholars here. Clinton Judy and Harvey Eagleson were great teachers, and they were scholars in the sense of their knowledge of the world of literature, but not in the sense that they did research. But Hallett was such a scholar, and I thought this was a good move to make—to bring a little more scholarly research atmosphere into the Humanities Division.

GOODSTEIN: In what ways do you think Hallett's lack of administrative skill got the division in trouble?

DUBRIDGE: Oh, that's a long story. It looked like a very fine arrangement for quite a long time, as I recall, but apparently the division people felt that Hallett wouldn't move on things that would be to the advancement of the division. He was slow in looking for new candidates for positions and just wasn't decisive and energetic enough in leadership. He did not want, really, to build the social sciences end of the division—he wasn't very keen on that, and yet it would seem that some of that should be done. A number of people in the division admired Hallett Smith's scholarship, and they said he ought to be a scholar and he ought to be at the Huntington Library—which is where he is now and apparently is highly admired for his work there.

GOODSTEIN: Well, I was interested to read that you did not want to come as a fund-raiser.

DUBRIDGE: I know. Nobody ever does [laughter]. Jim Page thought probably the institute could go ahead just with the Caltech Associates and the normal contacts with the public and so on, and that the gifts would come in—and of course, to some extent, they did—without a drive. But pretty soon it got to the point where there were just too many things that the campus needed—new student houses and a student union, getting rid of all those old barracks from World War I.

GOODSTEIN: How did you make out on the first drive for funds?

DUBRIDGE: I think our goal was sixteen million dollars. We raised eighteen. That built an awful lot of stuff, you know—all the new student houses, Winnett Center, the [Scott Brown] Gymnasium. We had some money that we found someplace to add to the old engineering building—what is now the Franklin Thomas [Laboratory of Engineering]; that didn't come from the drive. And I guess the Gordon Alles [Laboratory for Molecular Biology] came about that time. I'm surprised when I look at the number of buildings that eighteen million dollars would build then. Oh, yes, and the graduate houses. It was clear that we were just bursting at the seams in some of these areas. Jim Page had first resisted a drive. I said, "I want to make a report on the financial situation to the board." He said, "Oh, sure, everybody knows we need forty million dollars, but there's no hope of that."

GOODSTEIN: It's interesting. He resisted changing the way money had traditionally been raised here?

DUBRIDGE: Yes. You see, Millikan never had a drive, in the sense of an organized effort with professional help. He just got acquainted with the rich people and made them beg to give money to Caltech, as he put it. They got so interested that they just wanted the privilege of having a building named for them on the Caltech campus. And it worked fine. Millikan had a charm about him that did attract the money, but I couldn't work that way. I felt, and a lot of the faculty

and many of the trustees agreed, that the only way to get some of these things done is just go after it—set a goal and go after it. So we drew up plans for the campus, including housing, a student union, and other facilities, and went ahead. Once we got started, Jim was very good. He helped promote it and helped a lot. I brought in Chuck [Charles] Newton to help, who had worked with me at the Radiation Lab.

GOODSTEIN: Oh, what did Chuck do at the Radiation Lab?

DUBRIDGE: He was a writer, helping write reports and other literature about the laboratory, and he stayed on to edit the final twenty-eight-volume report. But he also did a lot of work on helping various people around the lab in writing reports. It was a writing and public relations job, only we didn't have too much public relations then. He was especially a good friend of, and admired greatly by, Louis Ridenour, who was also at the Radiation Lab and had been at Caltech. And Louis said to me, "You know, you're going to have to raise money at Caltech. If I were you, I'd get Chuck Newton out there to get busy with his typewriter."

GOODSTEIN: Did you yourself find it difficult, at first, to go out and do what Millikan had done so well?

DUBRIDGE: Yes, I couldn't do it that way. My most comfortable way of getting people interested in Caltech was by giving talks. I felt that I could do well in front of an audience—tell them the glories of Caltech and of science and so on, and get them interested that way. I think it was one of the associates who at one time said, "Lee, your talks are just great. You know, when you get through talking, we all just get out our checkbooks" [laughter]. Of course I did interview a few individuals. Some people came to me after hearing me talk, or hearing something about Caltech, and said, "Well, I'm interested in what you're doing. How can I help?" But for me to go to them and say, "Look, I want some money"—I just found that hard.

GOODSTEIN: Did Chuck Newton push you to do that?

DUBRIDGE: Yes. Of course, *he* didn't do it, but he did at least contact all the trustees to get their cooperation as to what their financial contributions would be to that first campaign. I think he did a good job in presenting the Caltech needs to the trustees and a few of the leading associates. When he said, "Look, you ought to talk to so and so"—well, sure, I did. We were lucky in some cases. For example, the Norman Church Laboratory [for Chemical Biology] came completely out of the sky, at Norman Church's own suggestion. When he heard about the Pauling-Beadle chemical biology program, he said to Millikan, "That's great. You probably need some help on that. Why don't you and DuBridge come down and see me?"

GOODSTEIN: So you and Millikan went down to see him?

DUBRIDGE: Yes.

GOODSTEIN: How often did that occur?

DUBRIDGE: That was the only time that I remember. Do you know the story of why Norman Church was interested in Caltech? Church raised and ran racehorses, and bred racehorses. Somebody made the charge that some of his winning horses had been doped. He was outraged. He came to Millikan, and he said, "Look, there must be a way of testing these horses to see whether they've had any dope or not. Don't you have a good chemist who could run such tests?" Millikan said, "Sure, Linus Pauling could do it for you." So Linus did. He made blood tests and urine tests and so on on these horses. And he reported back that there was no trace of dope.

GOODSTEIN: Was this before you came to Caltech?

DUBRIDGE: Oh, long before that. This was before the war. But Norman Church never forgot that. He owed a debt to Millikan and Pauling for getting him out of trouble. I don't know if it went to court or not, but at least the results of the tests became widely enough known that the charges against Norman Church were forgotten. But he never forgot it. And then when he heard that Pauling and Beadle were joining together on this new enterprise, he called Millikan and

asked him to bring me—he hadn't met me yet. He said, "I think you probably need a new building for this new chemical biology program, don't you?" Millikan had guessed that this is what he might say. And on the way down Millikan said, "Now we must remember that we must try to tell him that we want not only the building but an endowment to keep it going. Because what's the use of having a building if you can't keep it running?" But Norman Church carried the whole conversation. He said, "I'm going to give a million and a half to the building, and if I give the building, you can keep it going." So he did, and we got the building and not an endowment.

Another time, I was approached was by Alfred P. Sloan, who said, "Next time you're in New York, come and see me."[13] I think Warren Weaver worked on Sloan to give the money to convert the old electrical engineering laboratory into a mathematics building. And then later, this must have been 1960 or after, he said, "The next time you're in New York, come and see me." So I went to his office, and we chatted. He said, "Lee, I want to give Caltech ten million dollars." Wow! He said, "Here's what I want it used for. I want it as a free fund at your disposal, to support research that hasn't gotten to the place where you can get government support. You can finance new and risky ventures, and just devote the fund to research. I'm going to give you five million dollars now, and I would assume that you might use a million dollars a year. And in five years, I'll give you another five million." Well, that was a high point! That wasn't during a campaign either. But he died [February 17, 1966] before the five years was up. As soon as I had finished talking with Sloan, I went into Warren Weaver's office next door and told him what had happened. He said, "I know, and I think it's fine. We'll make sure that this gets into the records, so that if Mr. Sloan should pass away, you'll get the other five." But it never did, so we never got the other five. But that was an extremely valuable research fund, because it helped us finance a lot of things that later developed into quite important enterprises.

GOODSTEIN: When you look back now and compare what the campus was like in 1946 when you arrived, and in 1969 when you left, what stands out in your mind as the most dramatic change?

[13] Alfred P. Sloan, Jr., the head of General Motors from 1923 to 1956, established the Sloan Foundation in 1934

Fig. 6. An aerial view of the Caltech campus in 1946, when the school consisted of thirty acres of land and twenty-one permanent buildings. Caltech Archives.

DUBRIDGE: Well, just the extent of it. I amused myself one day by going through the list of buildings on the campus in the catalog and the dates when they were finished. I was really quite astounded to just count up the number of buildings listed in the catalog that were built between 1946 and 1969.

GOODSTEIN: It's equally dramatic if you take an aerial photograph of the campus in the late thirties and compare it with one taken even in the mid-fifties.

DUBRIDGE: That's right. And I've got a big air view of the campus on my study wall at home in Leisure World [Laguna Hills, California], which was taken just as I left. I'm surprised at how much has changed since then, too. The Beckman Laboratories [of Behavioral Biology], the geophysics laboratories [Seeley G. Mudd Building of Geophysics and Planetary Science]; the [Keith Spalding] Business Services building were just going up when I left. And of course, now the Braun and Watson laboratories [Braun Laboratories and Thomas J. Watson, Sr., Laboratories

of Applied Physics]. So, yes, there's been a big change. Did you ever hear about the story about the library?

GOODSTEIN: No.

Fig. 7. An aerial view of the Caltech campus in 1968. During DuBridge's tenure, the campus grew to eighty acres, the teaching faculty doubled in number, and new research fields blossomed, including chemical biology, planetary science, nuclear astrophysics, and geochemistry. Millikan Memorial Library, completed in 1967, is the tallest building on the campus.

DUBRIDGE: We didn't have a central library. It was one of the things I'd plugged away on for a long, long time, and nothing happened. When Millikan died, I said, "Millikan has so many dear friends around here that surely we can raise a couple of million dollars from them to build a Millikan Memorial Library. So I went to one of Millikan's best friends in the Caltech Associates, Seeley Mudd. I said, "Look, Seeley, would you head a committee that would go to some of Millikan's friends and see if we can't get about two million dollars for a Millikan Library? It ought to be right in the middle of the campus, be a centerpiece, and honor Millikan, and fill a terribly important need in campus life." Seeley said, "Yes, that's a good idea. I'd like to see something done there. Let me think it over and I'll call you back." Well, he called back. He said, "Look, I've been thinking it over. Rather than going around soliciting a lot of money, I think it would be easier for me just to give it all myself." Which he did.

GOODSTEIN: So you had no campaign?

DUBRIDGE: We had no campaign. We thought it could be a quiet campaign if we could just go to some of his good friends—and Seeley knew them all, I thought.

GOODSTEIN: Did you bring a particular style in institutional management to the campus?

DUBRIDGE: Well, it was very different from Millikan's style, I think, yes. For example, Millikan had never taken the faculty into confidence about the financial situation at Caltech. This was a matter for the trustees and it wasn't any of the faculty's business, except the members of the Executive Council. One of the first things I did at an early faculty meeting was to tell them about what the budget was for the coming year. They had never heard it before. They were absolutely astounded, and said, "At last, we know something about where the institute stands—why we can't do some things and can do others, what the funding limitations are and what the funding opportunities are. And where the money is going, where it's coming from, how much from endowments, how much from contracts, how much from this and how much from that." I made it a point to do that every year—to give the faculty a report on the budget for the coming year. I think that established a rapport that was appreciated. And then, I think, setting up the Division Chairmen's Committee made the faculty feel that they were now a part of the president's office, and a part of the administration. And we'd talk completely and fully about everything having to do with the academic side of the institute—and even the financial side, when it was appropriate. We spent a great deal of time on the budget; each division chairman would prepare the budget for his division and we'd all go over it and prepare our salary proposals, which Earnest Watson went over in great detail. The division chairmen felt they had a voice in the administration. I didn't dictate what the salaries were to be; we met in the committee, and Earnest Watson was the one who would really look at the total picture of faculty salaries and say, "Look, this division is paying lower salaries for people of comparable quality than this division is. We ought to have some balance. A biologist should not get less salary than a physicist if he's equally distinguished, and equally active, and equally important, and equally a good teacher." So he got the salary structure uniform.

GOODSTEIN: I take it Watson could not do this under Millikan?

DUBRIDGE: No.

GOODSTEIN: Do you think physicists fared the best under Millikan?

DUBRIDGE: I don't think in any significant way. Of course, that was his field, and he worked with the physicists and so on. And he knew physicists and knew how to get good ones. But he built the chemistry department, too—he and [Arthur Amos] Noyes. Of course, Noyes was a great, great friend of Millikan, and Millikan admired Noyes enormously. He gave Noyes a free hand in building the chemistry division. Millikan brought in Professors Stock and Buwalda [in 1926 from the University of California] to build a geology division. He brought in Thomas Hunt Morgan [from Columbia in 1927] to build a biology division. He wanted this to be an institute of science, so he brought in Morgan, Noyes, Buwalda, Stock, von Kármán, as well as a lot of other distinguished people, including Richard Tolman, a physical chemist, who was able to give strength to both divisions. He brought in Munro to get a humanities division under way. I think he somewhat neglected the civil and mechanical and chemical engineering; he apparently didn't push that too hard, because when I came here, civil engineering was mostly surveying.

GOODSTEIN: It hadn't changed since 1910 [laughter].

DUBRIDGE: That's right. No, I think Millikan tried to be extremely fair and broad-minded about having a good group of basic sciences on the campus. I think he was more interested in science than in engineering; however, he felt that electrical and aeronautical engineering were going to be great things for Southern California. He said, "The institute must be an asset to Southern California and must work on things that are of importance to Southern California." He saw electric power and aeronautics as two of the great things that Caltech could help on. Also seismology.

GOODSTEIN: Do you think Earnest Watson, after the war, was a disappointed man about his whole career?

DUBRIDGE: I didn't see evidence of that. I don't think he wanted to be president. I do think that Millikan treated him as an office boy more than as a senior faculty man. He was just Millikan's assistant. Millikan was the director of the Norman Bridge Laboratory, but Watson ran all the administrative business. He bought the supplies and the equipment and the material. He assigned rooms to the research people.

GOODSTEIN: But he never had the title to go with it.

DUBRIDGE: No, that's right. Millikan brought him on as his assistant, and that's what he was. But, you know, he grew and grew in the minds of the faculty. During the war, when Jim Page and whoever else was trying to keep things going, they saw that Watson was a man who could manage and could run things. So he became almost acting president during the war. He did a great job and earned the admiration of everybody around the campus. By the time I came, Earnest Watson was a big shot on the campus. I was delighted that he was dean of the faculty, and the way he plunged into that was just so important for faculty morale—building the faculty and making faculty people feel they were fairly treated in matters of salary and retirement. Millikan had kept the retirement plan—the T.I.A.A. [Teachers Insurance and Annuity Association] retirement plan—a secret. If people asked about it, he said, "Oh, yes, if you want to get into that, why, we can do it." But when I came, some people said, "I never heard about the T.I.A.A. retirement."

GOODSTEIN: It was not automatic?

DUBRIDGE: It was by no means automatic; it was voluntary. And the people who knew about it and wanted to get into it could do so.

GOODSTEIN: And was the institute making a contribution at the time?

DUBRIDGE: Oh, yes. They made the matching contribution—five percent to match the five percent of salary. But some people just said, "Well, I can't afford to do that," and one poor old English professor retired without a dime. He hadn't built up a retirement fund, and we had to

give charitable gifts to him to keep him in food for several years before he died. T.I.A.A. participation was not mandatory. I made it mandatory—at Earnest Watson's strong suggestion, really. He said, "Look, it's wrong to have this voluntary, because some of these people can't understand the financial problem of building a retirement income." We raised the salaries at the same time.

GOODSTEIN: Do you think Millikan kept it a secret because it saved the institute money?

DUBRIDGE: Yes, yes, I think so. And by golly, he had to save money during the Depression. He was desperate, you know, and he just didn't want to add anything to the budget.

[End of Part I