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

INTERVIEWED BY
JUDITH R. GOODSTEIN

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
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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

Caltech Archives, 1982

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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.

[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

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[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?

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[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.

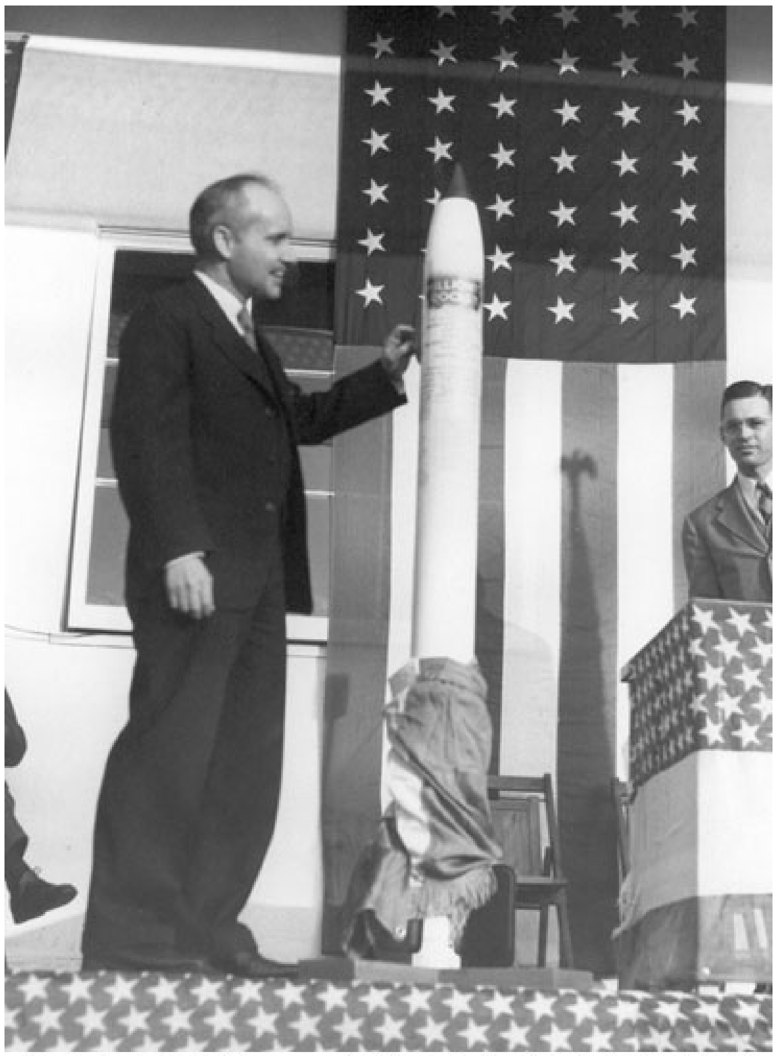

Fig. 3. Earnest C. Watson, administrative head of Caltech's World War II rocket project, with the Institute's one-millionth rocket. The five-inch high-velocity aircraft rocket, nicknamed Holy Moses, first went into combat use in July 1944. Caltech Archives.

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.

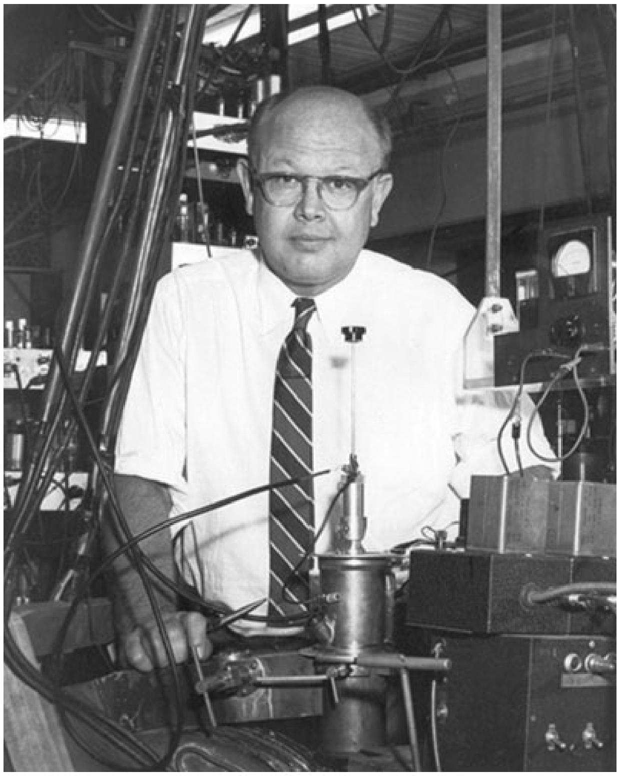

Fig. 4. William ("Willy") A. Fowler in 1956 in the W. Kellogg Radiation Laboratory, where he did much of his research in nuclear astrophysics. Fowler shared the 1983 Nobel Prize in physics for his work on nucleosynthesis. Caltech Archives.

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,

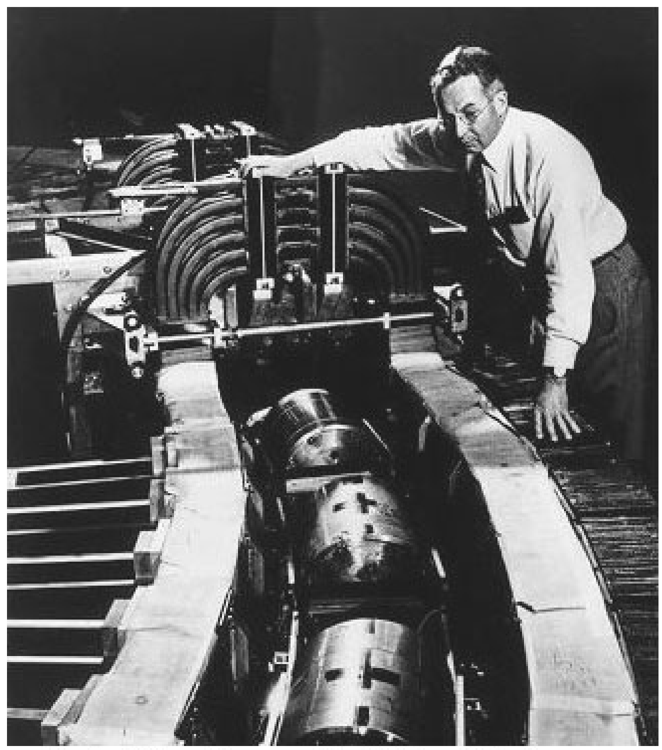

Fig. 5. While chairman of the division of physics, mathematics, and astronomy, the physicist Robert F. Bacher initiated construction and use of a new electron accelerator, called a synchrotron, shown here. In 1962, he became Caltech's first provost. Caltech Archives.

[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?

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[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

Lee Alvin DuBridge (Part II)

(1901-1993)

INTERVIEWED BY
JUDITH R. GOODSTEIN

February 20, 1981

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Physics, administration

Abstract

Physicist Lee A. DuBridge became president of the California Institute of Technology in 1946. In this interview he recalls his dealings at Caltech with Linus Pauling; his memories of George W. Beadle, Theodore von Kármán, and J. Robert Oppenheimer; the military Vista Project at Caltech; and the difficulties surrounding the deportation of Hsue-shen Tsien, Caltech's Goddard Professor of Jet Propulsion.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH LEE A. DUBRIDGE
PART II

BY JUDITH R. GOODSTEIN

PASADENA, CALIFORNIA

Caltech Archives, 1982

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INTERVIEW WITH LEE A. DUBRIDGE
PART II

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:

I would like to thank Loma Karklins and Abby Delman, who transcribed the tapes; Bonnie Ludt, who located the photographs; Sara Lippincott, who edited the text with her usual meticulous care; and Roger H. Stuewer, who gave the final manuscript a critical reading. I am also grateful to the John Randolph Haynes and Dora Haynes Foundation for its encouragement and support of this work.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

Interview with Lee A. DuBridge, Part II **by Judith R. Goodstein**

Pasadena, California

Session 2 (*cont.)* February 20, 1981

Begin Tape 3, Side 1

GOODSTEIN: We had begun to talk about Pauling. You said that the Caltech trustees were unhappy with Pauling from the time you came here, and you told me the background, which had to do with an ex-student who came to live with him.

DUBRIDGE: That's right, yes. And the lawyer he consulted happened to be a lawyer who enjoyed defending left-wingers and other unpopular people. You'd call him a kind of civil rights lawyer now. And Pauling told me that that episode got him interested in politics and especially in standing up for unpopular causes, which he still believed in. When I arrived in 1946, before the political activities began in earnest, he was really a kind of hero of the campus except among some of the trustees. He was on our Executive Council, you see, and they felt very uncomfortable trying to work with him on Caltech matters.

GOODSTEIN: But among his faculty colleagues…

DUBRIDGE: Among his colleagues he was a hero. And I had the same feeling, because I knew of what he'd done [in science]. So I guess I was a little surprised when I found out about the doubts about his political activities—I don't recall that I'd been aware of that before I came. [His causes] turned out to be mostly what the trustees thought were pretty far left-wing. It wasn't only what he thought, which nobody would be concerned about. Rather, it was the publicity-seeking—at least publicity-getting—actions he took, like picketing the White House. He liked to join picket lines for causes he believed in, and there was always a photographer around, and so there were quite a series of pictures in the papers of Pauling marching with a bunch of people.

Fig 1. Linus Pauling and his wife Ava Helen participating in a peace march in Los Angeles, 1960. Caltech Archives, Pauling Collection. Photo by Robert C. Cohen.

GOODSTEIN: Identified as a professor from Caltech.

DUBRIDGE: Oh, yes, sure. The most famous incident was when President Kennedy gave a dinner for the Nobel Prize winners and a lot of other scientific people. Jerome Wiesner was then the President’s science advisor. And he invited people who were then on or had been on the President’s Science Advisory Committee, or had been on other government committees, like the National Science Foundation board, and other scientists who had had some relation to government, as well as quite a lot of government officials. I was invited. And of course Pauling was there. Well, in the afternoon, Pauling was walking along the street in front of the White House on some errand or other, and he saw a bunch of pickets picketing the White House [in favor of a nuclear test ban]. So he picked up a picket sign on the spot and marched along with these picketers, up and down in front of the White House. And of course, a photographer was there and caught him, with the White House in the background. That night, he was a guest of the Kennedys at a formal dinner at the White House. Well, no matter what you think about the picket line, to do that on the afternoon when he was to be an honored guest at the White House seemed not a very tactful thing to do. However, the Kennedys took it in good form, and they both grinned when Pauling was introduced by one of the aides. They'd already heard it on the radio or seen it in the paper. And Jackie Kennedy said to him, "You know what my daughter said when she saw your pickets out there?" Pauling said, "No." She said, "Mommie, what's Daddy done wrong now?"

GOODSTEIN: Did you have any words with Linus that evening about what he had done?

DUBRIDGE: No, I carefully avoided getting into a discussion with him about it. But even before that—I don't know what the series of events was; it was probably an accumulation of things—a couple of the trustees became very, very antagonistic to Pauling and called him a Communist.

GOODSTEIN: Was there a movement on the part of some of the trustees to dump him from the faculty?

DUBRIDGE: Yes. And this is well known on the campus. There were a couple of trustees who said, "He has sufficiently violated the traditions of the university in political activities outside of his field of competence. You know, he's a chemist. What does he know about political affairs?" And always, from their point of view, on the left-wing side. One or two of the trustees—this is back in the [Senator Joseph R.] McCarthy days—said, "He's just a Communist, and we shouldn't have Communists on our faculty." There was then, and there still is, a procedure to be followed if a professor with tenure is charged with any kind of misconduct that might justify his termination. The procedure was to set up a double committee.

GOODSTEIN: Who selected the faculty members?

DUBRIDGE: I officially made the recommendation to the trustees, but I certainly discussed it with Earnest Watson [dean of the faculty]. I followed his recommendations, because I had full confidence in his knowledge of the faculty and which ones would treat this with objectivity and care and thoroughness. Well, Bob [Robert F.] Bacher was appointed chairman of the faculty committee, and William McDuffie was appointed chairman of the trustee committee. Bill McDuffie was a fine, loyal, wonderful trustee, and he never went off half-cocked. He was like Bacher, in the sense that he would study a problem very carefully before he came up with a conclusion.

Well, the trustees met separately and the faculty met separately, and then they met together. And the charge—the only charge, really—that would have possibly led us to terminate Pauling's appointment was that he was a regular, active member of the Communist Party. On various occasions Pauling had been questioned by various committees—the House Un-American Activities Committee and what not—and they had always put him under oath and always asked him the question, "Are you or have you ever been a Communist?" And he always refused to answer. He said, "My political beliefs are my business. No government has the right to force me to state my political beliefs. And so I will refuse to answer." They would threaten to cite him for contempt. On one occasion, some Pasadena city body asked him to come and make a statement and answer questions before a city board. It was not a court, but it was some city organization. They asked him the same question: "Are you now or have you ever been a member of the Communist Party?" He said, "I refuse to answer. My political beliefs are my own business." Immediately after that he came to my office and said, "I will not swear under oath, in public, as to my Communist connections, if any. But I will swear to you, Lee, as president of the institute. You have a right to know. And I do swear to you that I am not and never have been a member of the Communist Party." Well, I believed him.

GOODSTEIN: Why at that point do you think Linus Pauling came to you to say that?

DUBRIDGE: Well, I think he knew that the question about his retention at the institute was up for consideration. He knew the faculty committee was appointed—the faculty and the trustees. They interviewed him. They talked to him. They asked him about his activities, his connections, and so on. He told them perfectly frankly that he'd never been a Communist but that he had been interested in people and had friends who were Communists, and that he agreed with some of the things they were doing. Anyway, the two committees, independently and then together, unanimously came up with a recommendation that there was no evidence that Pauling had violated civil or university rules to an extent to warrant his termination. And so the trustees adopted that position, much to the anger of a couple of the trustees who had wanted to see him fired. So that ended that particular episode. He did continue his somewhat conspicuous left-wing advocacy and activities and associations. He in no way changed his attitude or his activities. He was so convinced, you know, that he was right and that he had a right to do this, and that nobody was going to interfere with him.

GOODSTEIN: Do you think it interfered with his chemical research here?

DUBRIDGE: Oh, I'm sure it did, especially after he got the Nobel Peace Prize [1963], which made some of the trustees kind of disgusted.[1] Then he was in great demand as a speaker, and his speeches were not very happy for many Caltech people.

GOODSTEIN: He resigned shortly after he received his Peace Prize. There is some question as to why he resigned. His position was that Caltech had not recognized the honor bestowed on him.

DUBRIDGE: Yes. There's an officer of the American Institute of Physics who's been interviewing various people around the country. He got interested in Pauling and others. He reported to me what Pauling had told him in an interview—that the reason people here didn't like him and didn't treat him fairly was because he was advocating the ban on atmospheric nuclear testing. He told this fellow that that was the reason everybody was against him. Well, that's wholly false, because Bacher and myself and many others were equally in favor of the nuclear test ban. And many other people on the campus were, too. So that would not have put him apart from the mainstream of the faculty at all. The reason he resigned—and he really told me essentially this—was that he got so wound up in his political activities, with making speeches around the country, and writing, that he began to lose his graduate students. He wasn't paying

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[1] Linus Pauling received the Nobel Peace Prize for 1962; the award was announced on October 10, 1963, the same day that the Nuclear Test Ban Treaty went into effect.

attention to them. The research fellows and postdocs no longer signed up to work with him. He had a huge laboratory over in the Church building [Norman W. Church Laboratory for Chemical Biology]. When that Church building was built, it was built for Pauling and [George W.] Beadle and their combined chemical biology program. He had a big suite of laboratories there; in the early days he was very active and had a lot of graduate students and research fellows and younger [research] associates working with him. Well, many of those dropped away—not because they disagreed with him politically but because he just wasn't there and wasn't paying attention to them. So his laboratory became emptier and emptier. Well, it got to the point where the rest of Church building was getting pretty crowded. I think Ernest Swift was then chairman of the chemistry division. They saw this almost unused big suite of laboratories and offices. So Ernest Swift, who is a gentle and sensible and fine guy, said to Pauling, "Look, you're not using all this laboratory and office space. Can't you give up some of it for some of the people that don't have adequate space?" Well, Pauling just choked on this. This, he thought, was the ultimate insult—to ask him to give up his research space. And that was that. That was the trigger, at least, that made him say, "Well, I think I'd better leave. You don't appreciate me here."

GOODSTEIN: Was there any embarrassment at Caltech when he received the Peace Prize? I went back and looked in the files. There was perhaps a twenty-four-hour period before the institute released a statement.

DUBRIDGE: I don't recall how long it was. I was asked by the *Pasadena Star-News* to comment on this award. I tried to be diplomatic, and I said that in spite of the fact that many of Pauling's activities had been criticized he still had done some great things and I was delighted to see him get this award—something of that sort. But I did add that qualification. Linda Pauling called me in high dudgeon. She said, "You insulted my dad. He's done so much for Caltech and everything." And I felt bad about that, because I was fond of Linda. Fortunately I guess she's forgiven me, and now we're very good friends. But you know, there were many much nastier statements than that, as to why this Communist-leaning guy was given a Peace Prize. He hadn't done anything to promote peace, they said. It was given on the basis of his advocacy of the nuclear test ban, which was thought to be, by the prize committee, an important step toward peace. And I agree. So I saw the merit in the award, and yet I could not quite go all out and say he's the greatest guy in the country. But I was glad to see him get the award.

GOODSTEIN: There is a lingering anti-Pauling legacy on the campus. If you read back in the files, one of the objections seems to have been not so much that he didn't have graduate students and postdocs using his research space but that he brought in essentially serfs—people who would spend their life turning out his kind of research. They had no teaching obligations. They had no teaching responsibilities. So that he, Pauling, was not discharging his responsibilities as a professor. He wasn't professing, nor were the people in his laboratory being trained to profess. And that if this was an educational institution, then we were being derelict in our duty.

DUBRIDGE: I'm glad you mentioned that, because it slipped my mind. I know there was unhappiness on the part of Ernest Swift, and presumably the other professors in the division [of Chemistry and Chemical Engineering], that Pauling just wasn't carrying his load in the division, in research or teaching. And yet he was using this laboratory space. Ernest came and talked to me. He was terribly distressed about it and hated to push Pauling around. But he said, "We've got to have this space, and Linus isn't using it. He's not carrying his weight in the division. And I think as division chairman it's my duty, on behalf of the other professors in the division who need the space and are carrying loads they otherwise wouldn't have to carry, to ask him to give it up."

GOODSTEIN: Were you relieved when Pauling left?

DUBRIDGE: Yes [laughter]. Yes. You know, we were talking about this at the table yesterday at lunch, after I left you. [Richard P.] Feynman was with us, and he and another physicist got into a talk about physics things. And the question of Pauling came up. Someone said, "You know, Pauling is probably the greatest chemist in the world today." Nobody objected to that. And if you look back at the things he did while he was an active chemist, they were worth the Nobel Prize in chemistry. Then somebody said, "Well, what about his vitamin C?" Well, you know, he may or may not be right about vitamin C. I said, "Pauling has had many intuitions about things, even before he had proof, and many of them turned out to be correct."[2] But then Feynman spoke up and said, "Many of them were very wrong." And Murph [Marvin L.] Goldberger mentioned a case where Pauling had thought out some theory of nuclear forces or nuclear structure that Murph said was just crazy.

So Pauling has made his mistakes; nobody can be perfect. Still, so many of his ideas were ahead of their time and were right—and he had other ideas that were wrong. You know, that's the way human beings are. The point is that these people said, "Yes, as a chemist, Pauling in his chemistry days—the protein structure, and the rest of it—had brilliant achievements." After he got the Nobel Prize in chemistry [1954], he was again in demand; and his political activities were taking a lot of time. I don't recall that he did any very spectacular chemistry after that. He was close to the DNA thing, but he didn't quite make it out.

When Pauling got the Nobel Prize for chemistry, we followed our tradition of having a big party. And [professor of literature J.] Kent Clark wrote a skit. I was asked to give a talk—this was over in the old Culbertson Hall, before Beckman was built. I tried to be complimentary and light. I didn't even hint that there was any doubt about Pauling's activities; I just said he was a good chemist. But I couldn't resist making this kind of crack, which brought down the house. I said, "Of course, you know, it's a little silly to celebrate the award of a Nobel Prize, because it's given when the man has already made his great achievements." And I said, "Pauling isn't any greater a chemist today than he was the day before yesterday, before the Prize was awarded. As a matter of fact, if he isn't careful, he may be a worse one." [Laughter] And that has happened in so many cases. You know, a Nobel Prize often gets a fellow out of his research. It didn't happen to many, but it did happen to a number. The Nobel Prize either went to their heads or made such demands on their time that their research work thereafter was of less quality or less energetic.

GOODSTEIN: Was there an on-campus party for Pauling when he won the Peace Prize?

DUBRIDGE: No.

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[2] Pauling's *Vitamin C and the Common Cold* was published in 1971. He continued to advocate megadoses of vitamin C as a preventive for a number of major disorders, including cancer and heart disease.

GOODSTEIN: Was a party discussed?

DUBRIDGE: I don't recall that it was. We'd had the party for Pauling in 1954, and we thought another one would be laying it on.

GOODSTEIN: Was he irked by that?

DUBRIDGE: He never brought it up, so I don't really know. Of course, Pauling went from Caltech and immediately joined Robert Hutchins at the Santa Barbara enterprise. And many people said that that couldn't last—that he and Bob Hutchins would never get along. And it did not last. He was pretty soon off on his own. And now he has his own operation.[3]

GOODSTEIN: Do you think there's anyone comparable on the campus today, as a scientific hero?

DUBRIDGE: In fact, there are several: [Richard] Feynman, [Murray] Gell-Mann, [Carl] Anderson, [Max] Delbrück—and now [Roger W.] Sperry. They have the same kind of respect and admiration that Pauling had in his best days and they're all Nobel Prize winners too. Beadle was a hero here, too, until he went to Chicago; everybody wept when he decided to go.[4] You will still find students, faculty, and trustees speaking in praise of those Nobel Prize winners. And now [professor of biology] Leroy Hood has become a new hero among younger ones.[5]

GOODSTEIN: Let's turn to Beadle.

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[3] Pauling became a founding fellow of Hutchins's Center for the Study of Democratic Institutions, a nonprofit educational institution in Santa Barbara. Later he held professorships in chemistry at the University of California, San Diego (1967-69), and Stanford (1969-73). In 1973, he founded the Linus Pauling Institute of Science and Medicine. He died on his ranch near Big Sur on August 19, 1994.

[4] George W. Beadle left Caltech in 1960 to become the chancellor of the University of Chicago.

[5] Leroy Hood, Bowles Professor of Biology, codeveloped the automated genetic sequencing technology that enabled the Human Genome Project. He left Caltech in 1992 to found the Department of Molecular Biotechnology at the University of Washington, and in 2000 he cofounded the Institute for Systems Biology, a private nonprofit research institute in Seattle, of which he is president.

DUBRIDGE: Well, there isn't anything except good I can say, thank goodness, about Beadle. I didn't appoint him. He was appointed after Thomas Hunt Morgan died.

GOODSTEIN: I think Alfred Sturtevant and several others provided an interim administration in biology.

DUBRIDGE: Yes, but Beadle was certainly regarded as the permanent replacement for Thomas Hunt Morgan. He came [in 1946] with a distinguished reputation as a biologist.

GOODSTEIN: Biology at that point must have been in need of some new blood.

DUBRIDGE: That is true. Thomas Hunt Morgan had done a marvelous thing in developing genetics. He brought with him several people who had worked with him—like Sturtevant and others—collaborators and independent workers. It was a lively place. But it was somewhat confined to that narrow business of studying Mendelian genetics. Beadle brought in some new ideas and new approaches to different branches of genetics. He got his Nobel Prize because he proved that a gene is responsible for producing an enzyme—one gene, one enzyme. This was something that had escaped the Morgan group. They were looking at different shapes and eye colors and so on of fruit flies. They identified the genes and even their positions on the chromosome. But the specific chemical action of a gene—the structure of DNA—was not known. The fact that DNA was the gene chain had not been established. So Beadle's pioneering work was to show that the gene had a certain chemical action, which was critical. Then, of course, he got more interested in molecular biology. He and Pauling immediately hit it off, because Pauling was interested in organic chemistry, and the protein molecule was a vital thing in the life chain and in genetic development. So they proposed this combined new enterprise in chemical biology. It attracted great attention across the country. And as you know, it drew a big grant from the Rockefeller Foundation, through Warren Weaver, who thought this was the coming area in science. And that attracted Norman Church to give the money for the building. Well, not only did Beadle do some brilliant scientific work of his own, but he picked awfully good people to come and work, either with him or in other fields in biology, so that the Biology Division greatly broadened the scope of its activities. It never got into classical anatomy, zoology, natural science business. It stayed in the fields of genetics, microgenetics, molecular genetics, chemical biology, biochemistry.

GOODSTEIN: He was a good administrator?

DUBRIDGE: Oh, yes. He ran the division with such skill. He did it in a democratic way. And yet he was clearly the leader and was very perceptive about picking new members of the faculty. He was a respected voice in our Division Chairmen's Committee, always. He was in a sense a hero of the campus—and certainly more so than Pauling, after Pauling got in trouble. Then in 1958 Beadle got his Nobel Prize. Of course, we created a nice celebration on the campus on that occasion.

GOODSTEIN: Did you try to keep him?

DUBRIDGE: Oh, yes, I tried to persuade him that he was still blossoming in his research, and that his division was blossoming in these very important and exciting new fields. We very much wanted him to stay. I said, "I wanted to do administrative work, but I was not a research man when I did. And you, Beadle, are." And he, at first, turned down the Chicago offer [to become chancellor of the university]. He went there and looked around, and finally came back and said, "I don't think I want to go there." But somebody at Chicago became very excited about this—that Beadle had turned them down. He said, "We've got to get that guy." And so they organized a trustees' committee and decided to add all sorts of trimmings to their offer—that he could have his research laboratory and he could continue his research, and so on. They asked him to come back for another visit. As he was leaving, he dropped in to say hello and goodbye. I said, "Now, look, don't let them tempt you." "Yes," he said, "but suppose they make an offer I can't refuse?" So he had a hunch that the new approach they were making to him was something that would be too attractive to turn down. When he left there was great sorrow on the campus.

GOODSTEIN: Did he do any research there?

DUBRIDGE: Oh, yes. He still does. As a matter of fact, [professor of biology] Norman Horowitz told me just the other day, “You know, we tried to persuade Muriel and George Beadle to come out for a few months last winter, and Muriel was all for it. She didn’t want to spend another winter in Chicago. So she was delighted at the prospect and said they would talk it over. George said, ‘No, I’ve got my greenhouse going, my corn plants going, I’ve got to attend to them. They’re coming, you know, into the critical spot. I’ve just got to be here to cross-pollinate them.’” He would not leave his greenhouse. The genetics of corn had been an interest of his way back, and he came back to it. Now he’s in a big controversy with another biologist [Paul Mangelsdorf] as to what was the original corn—from what kind of plant did our present maize develop? Beadle thinks he’s found a plant [teosinte] that grows wild in Mexico and has the same chromosomic structure, essentially, that would have led to the modern corn. This other biologist has a different idea. Beadle gave a talk on that here last year, but he was not himself. He stumbled through his talk; he read it—he did not read it well. [My second wife] Arrola and I went to the talk. She hadn’t met Beadle before. She said, “He’s kind of showing signs of age, isn’t he?” And it was true. He just didn’t have the zip, fluency, clarity, that he once had, and he stumbled through the paper. It was a little sad, really. Well, that’s the last I’ve seen of him. He used to be such a fine speaker, you know, and he would never read a speech.

GOODSTEIN: It is not characteristic of a scientist to read a talk.

DUBRIDGE: Not a scientific talk. A nonscientific one on science and public affairs, yes. But, no, a scientific talk, you’re more likely to speak from notes and slides.

GOODSTEIN: Was Beadle responsible for bringing Delbrück here after the war?

DUBRIDGE: Yes, and many others. Everybody that came in those years was his. That was a great thing, to bring Delbrück. Delbrück was already noted as a physicist, and he’d only begun, really, his career in biology. But Beadle saw the promise of what he was doing and the ideas that he had. So this was a great thing for the division, to have Delbrück there. Much of the present stature of the Biology Division can be traced back to Beadle’s appointments, his initiation of new work, and the way in which he broadened the department and brought it in touch with physics and chemistry.

GOODSTEIN: That raises the question, What about Pauling's appointments in chemistry, and the stature of chemistry?

DUBRIDGE: Ah, well, I cannot fault Pauling on that, because he brought in some good people, too. Pauling was quite critical, except maybe for these "serfs" who worked for him in his later days. But during the time before and just after the war, when he was chairman of the division [the Division of Chemistry and Chemical Engineering, 1937-1958], I think he built a strong chemistry department. [Arthur Amos] Noyes had started it.

GOODSTEIN: The emphasis under Noyes was physical chemistry?

DUBRIDGE: Physical and organic chemistry. But Pauling put heavy emphasis on physical theories—you know, the quantum theory of molecules. Without the quantum theory, they could not have done what they did, because that gave the key to what kinds of chemical structures were stable and possible, what the binding forces would be, and all the rest of it. That department, that division, became noted as one of the leading chemistry divisions of the country.

GOODSTEIN: Was Pauling a strong divisional leader?

DUBRIDGE: I think so, yes. His style was different from Beadle's.

GOODSTEIN: Did Pauling brook opposition if you came in and disagreed with him? I suppose what comes across with Beadle is that he would listen to other people.

DUBRIDGE: Well, Pauling was much more egotistical. He always knew that he was right. Of course, I was unable to get into any scientific arguments with him. I didn't know the field well enough to talk to him about his scientific work, except to ask him in general what was going on. It was fascinating to hear him talk about it, but I couldn't quiz him on it.

GOODSTEIN: In your autobiography, you mentioned that [Robert A.] Millikan used to go around to the various laboratories and chat with the researchers.[6] Did you ever do that when you came here?

DUBRIDGE: Not very much, I'm afraid. I did some. I remember I had been hearing about Gerald Wasserburg's work on radioactive dating. Well, that was something I thought I could understand. So I called Gerry one time and said, "Gerry, can I come over and see you for a little while?" And he said, "Sure." And we set a date. I found out later that Gerry was a little scared. He thought I was going to scold him about something or other. But I just wanted to see what he was doing and hear about his work. And he gave me a marvelous two-hour talk. By the way, I used to drop over to the Kellogg Laboratory [W. K. Kellogg Radiation Laboratory] to chat with Charles and Tom [Thomas] Lauritsen, Willy [William A.] Fowler, and the others working there. I followed that work with some interest. I would go over and see Carl Anderson's work. On a number of occasions, various people around the campus would say, "Wouldn't you like to come to our laboratory and see what we're doing?" And I always said, "Sure, I'll come over." I usually went by invitation. I made a number of visits to laboratories. And I was delighted. I hated to barge in, and even when I did invite myself I made an appointment first, to make sure they wouldn't be too entangled in something else to be bothered by my intrusion. I didn't do what Millikan did. I don't know how much Millikan visited, outside of physics laboratories.

GOODSTEIN: Physics is where he obviously felt comfortable.

DUBRIDGE: Yes. He was working there in physics when I was a research fellow—and he and [Ira S.] Bowen were still very active in the spectroscopic work. So he'd be in his own lab, and then he'd just wander down the hall to see what was going on. Under the same conditions I probably would have done the same thing.

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[6] Lee A. DuBridge, *Memories* (Pasadena, CA: Caltech Archives, 1979).

Begin Tape 4

GOODSTEIN: I would like to turn to Theodore von Kármán now. My impression from looking at correspondence between von Kármán and yourself is that there was a certain coolness on your part toward him. He obviously enjoyed a very good relationship with Millikan. I think he was very independent—he went his own way. By the end of the war, he had lots of obligations in Washington and seemed to spend less time here. How did that sit with you? Because at a certain point he just took what we would call today early retirement.

DUBRIDGE: Well, I had a great admiration for von Kármán. I found it hard to understand him and talk with him. He had this tremendous Hungarian accent and he was hard of hearing. You had to speak very slowly, and he didn't. So I found it difficult to sit down and have a chat with him. If we were talking about some business thing, why, we would just talk about it. I'm surprised that you thought there was a coolness—I don't know what that arose from. Except that I never could feel very close to him. I was on the Air Force Science Advisory Board when he was chairman of it. Everybody thought he was doing great. But when I sat at these meetings I was a little puzzled, because his hearing was so bad—and apparently he had a hearing aid in one ear but not in the other, so his stereophonic sense was bad. Somebody over to his left would say, "Mr. Chairman," and he would look over to his right. He didn't know where it was coming from. Then he would say "What was that again?" Then he would speak, and I had a hard time understanding what he said. I actually was a little puzzled at the enthusiasm for him that everybody showed. Now, I was judging on superficial things, obviously, and they were judging him on his grasp of the technology. That, you know, was absolutely superb, and he was one of the greater aeronautical engineers of his day. He built a great aeronautics department. I certainly did not encourage him to retire early. Just why he did, I don't know. He was spending a lot of time in Washington. And I don't think I objected to that, because I was, too. And he was always involved in important and influential decisions on the Washington scene, and was highly regarded.

I accepted, with pleasure, the invitation from Jerome Wiesner to come to the White House when von Kármán was awarded the first National Medal of Science [1963]. They had the ceremony out in the Rose Garden, and a lot of military officers were around. There was a great salute to von Kármán as President Kennedy presented the medal, and picture taking. Jerry Wiesner grabbed me and told me to come up and join the picture with von Kármán, and Jerry, and the President, and some of the top air force people. I was very proud of being a part of that, and proud that von Kármán had received the first and, that year, the only Science Medal. I remember I was still on the National Science Board at that time. The Science Board was supposed to make recommendations for the medal. They came to the conclusion that instead of choosing six or eight people they would choose just von Kármán, because he stood out so much above the others they were thinking about. That was a great honor, and I delighted in it. If there was any coolness, as I say, it was simply because I never felt comfortable talking with him—language and hearing. I didn't understand his theoretical ideas very well either.

GOODSTEIN: So, to answer the question of why he retired somewhat early—do you think it was because of his obligations in Washington?

DUBRIDGE: It was his decision. Certainly there was no conscious action on our part that would have led him to it. We had no difference of opinion about the aeronautics department or what it was doing.

GOODSTEIN: Had you run across him during the war?

DUBRIDGE: No, I don't think I did. He was purely in the aeronautical and jet propulsion fields, and I had no contact with that. I was in radar. I'm not sure that I ever met him until I came here in '46.

GOODSTEIN: How did he run his meetings in Washington?

Fig 2. Von Kármán receiving the National Medal of Science from President John F. Kennedy, February 1963. At the far right is Jerome Wiesner, and behind him is Lee A. DuBridge. Caltech Archives, Theodore von Kármán Collection, Box 165.

DUBRIDGE: Well, as I said, to me it was sometimes confusion. The Air Force Science Advisory Board was a fairly large one—it's very much larger now than it was then—but I thought it was awfully big. I would guess there were fifteen or twenty people on it. It was bigger than the President's Science Advisory Committee. Well, he was treated with tremendous respect by everybody on the board. They respected his knowledge, his leadership, his scientific and engineering abilities, his contributions, and so on. So he was listened to with very great respect and when he spoke there wasn't a murmur around the room. And he handled the meetings pretty efficiently. They were orderly and substantive and very good. The only thing was the difficulty in understanding him and his difficulty in hearing what people were saying.

GOODSTEIN: Let's turn to Robert Oppenheimer.

DUBRIDGE: Well, I think I met Robert first when I was here in '28. [He was here on a fellowship and then the following year he became] an assistant professor at Berkeley and came down here for part of the academic year. But I only met him somewhat casually, because he was a theoretical physicist and I was doing other things. Charlie Lauritsen, I know, talked with him, because Charlie was doing experiments on the extraction of electrons from metals by high electric fields. And it was somewhat puzzling as to the results that were coming out. Well, I was working on extraction of electrons from metal by ultraviolet light. Though it seemed straightforward, there were still some quantitative things about that that no theory could cope with. And then Oppenheimer came across with the tunneling idea[7]—that a strong electric field, for example, would narrow the potential barrier, which was preventing the electrons from leaking out. But the electrons could leak through the barrier if it was narrow enough. We talked about that, because it was an important new idea in electron emission, although it wasn't fully understood quantitatively until the Fermi-Dirac statistics came out, as did the Sommerfeld theory of the energy levels of electrons in metals.

That cleared up many things. But it was applied to the photoelectric effect only after I went to Washington University [in the fall of 1928]. There was a visitor at Wisconsin by the name of R. H. Fowler, from Cambridge University. They were still working on the photoelectric effect [at Wisconsin] after I left. I had started the photoelectric experiments, at Professor C. E. Mendenhall's suggestion, in 1923 as a grad student, and quite a number of other graduate students did their theses on it. When R. H. Fowler came, in 1930, he got interested in this problem. He was thoroughly familiar with the new theory of metals. He started to do some figuring and came up with a theory that explained beautifully and precisely just how much the photoelectric current from a metal would vary with the wavelength of the illuminating light and with the temperature of the metal. Well, I had results on both of these things, which I'd been getting at Washington University. Mendenhall sent me an advance copy of Dr. Fowler's paper,

[7] J. R. Oppenheimer, "On the Quantum Theory of Field Currents," *Phys. Rev.* **31** (1931), 914.

and I immediately drew up the charts, and my data fitted the Fowler curve beautifully[8] Then I went on, expanded his theory, and made further tests on it.

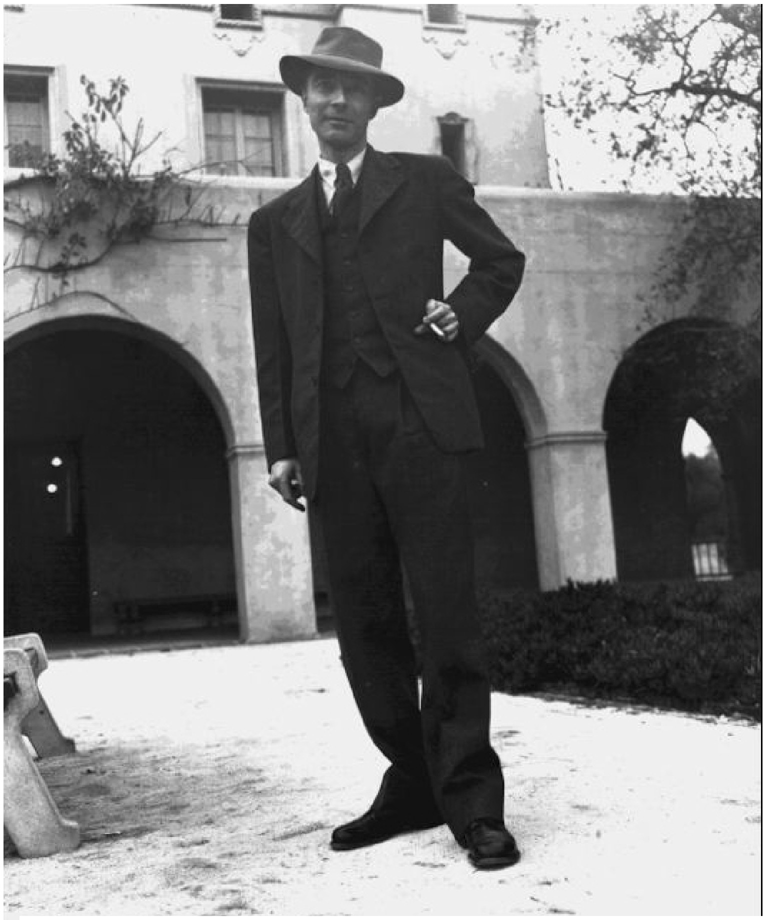

Fig 3. J. Robert Oppenheimer on the Caltech campus, 1930s. Caltech Archives**.**

I [may have seen Oppenheimer] in 1929, when I went to Ann Arbor for the summer, where I first met Bob Bacher. Oppenheimer was there for a while. He was there in 1934, when I went back on my way from Washington University to [the University of] Rochester, and Oppenheimer and I lived in the same house. They rented a fraternity house and all the unmarried men attending this symposium lived together there and we had a jolly, wonderful, wonderful time. And there I really got to know him much better. He talked above my head on his theoretical physics, because I was still trying to grasp what quantum mechanics was all about. But we were well acquainted by the end of the summer. I followed his work and I would see him at Physical Society meetings and chat with him. We went to dinners together. We all knew each other and exchanged ideas, especially at the meetings.

I spent one summer at Berkeley, about 1936 or ’37, at Ernest Lawrence’s invitation. Ernest had suggested during a visit to Rochester that we build a cyclotron, and said he would give us all the help we needed. I decided I would go out at Ernest’s invitation and work with the boys on the cyclotron to see just how they did their experiments and how you ran a cyclotron—to become more familiar with the technology of cyclotron experiments. It was a great summer.

[8] R. H. Fowler, “The Analysis of Photoelectric Sensitivity Curves for Clean Metals at Various Temperatures,” *Phys. Rev*. **38** (1931), 45-56.

Doris and I had a wonderful time. We had our two kids with us and we were able to rent a nice apartment not far from the Berkeley campus. And Ernest said, “I want you to give a couple of lectures while you’re here, on your work,” which was still in the photoelectric field. He said, “If you give a couple of lectures, then I can pay your expenses out here” [laughter]. Of course, Oppenheimer was around and I saw somewhat more of him then. I began really to feel much closer to him in 1945, when he invited me and Jerrold Zacharias to come out to Los Alamos and see what they were doing. He had apparently cleared this with General [Leslie R.] Groves, because they were running into some electronic problems, and also into some problems of contracts with manufacturing companies to make bits and pieces of parts that they wanted. We had done a tremendous amount of that at the Radiation Lab; we had companies all over the country working on radar parts or complete sets. Oppenheimer welcomed us with open arms. He was so friendly and showed us everything that they were doing—he and Bacher. Bacher had gone to Los Alamos from the Radiation Lab in ’43. And Bacher had asked Oppenheimer to visit the Radiation Lab sometime in ’43-’44, just to see the kinds of things going on there. Oppie invited us to come out there, and we spent several days at Los Alamos. There was nothing they did not show us. We saw the uranium bomb—it was in the final stages of completion—and the plutonium bomb, which was still in the process of assembly. This was only a few weeks before the Trinity test. And Oppie and Kitty, his wife, were very cordial and hospitable. The Bachers gave a big party at their house for us, and all of my friends were there.

Of course, immediately after the war, Oppie and I were brought together on many occasions in committee meetings—first in drawing up the act creating the Atomic Energy Commission [1947], on which [Arthur] Compton and Ernest Lawrence and Oppenheimer and others were very active. They called me in on it. The next step was in 1951. Truman appointed the Science Advisory Committee to the Office of Defense Mobilization [O.D.M.], of which Oliver Buckley [president of Bell Laboratories] was named chairman. James R. Killian’s book, *Sputnik, Scientists, and Eisenhower*, gives the whole history of the Science Advisory Committee.[9] It really focuses on Killian’s own relations with Eisenhower. Killian was on our Science Advisory Committee. We met with Truman and we met with Eisenhower and we had no problem in getting our voices heard. Well, Oliver Buckley’s health was failing, and he did

[9] James R. Killian, *Sputnik, Scientists, and Eisenhower: A Memoir of the First Special Assistant to the President for Science and Technology* (Cambridge, MA: MIT Press, 1977).

not want this committee to be a very active one. He said, "We're a standby committee to prepare for the next major emergency, and we ought to be thinking about what we would do then about a new O.S.R.D. [Office of Scientific Research and Development] and who would run it"—a vague sort of thing. That was what he understood the charter to be, and he was right. But we felt that there were much more urgent problems coming up, with the Russian acquisition of the atomic bomb, and the long-range bomber, and the beginnings of the missile program. So we had a three-day meeting at the Institute for Advanced Study, where Oppenheimer was our host—the whole O.D.M. Science Advisory Committee. Well, that was an extremely important meeting. Oppenheimer was a very valuable and cordial, as well as perceptive and scientifically imaginative, member of the committee, and deeply concerned about the whole business. We valued him very highly as a member of that committee. We had many sessions over the years with Oppie on that committee, and I visited Oppie's home at his invitation. [Isidor I.] Rabi and I, I remember, spent a couple of days with Oppie at his home, and Oppie visited out here.

GOODSTEIN: Was there any serious move to keep him at Caltech after World War II, after you became president?

DUBRIDGE: Oh, yes. We would have liked to have him stay in that position. I don't recall any specific attempts to persuade him, because I think by the time I heard about it, it had already been decided. We thought that [the chairmanship of the A.E.C.'s General Advisory Committee] was a very important position and we couldn't really argue against it. We told him we would miss him and hoped he would come back, which he did on a number of occasions to give talks. Then when we organized the Vista Project, Oppie spent a good deal of time here with us on that.

GOODSTEIN: Was he here as a consultant on that project?

DUBRIDGE: Yes, he was a member of our group. I think he was chairman of the committee on nuclear weapons, which was a part of the Vista Project. And so he spent a great deal of time with us. And during that period, Oppie and myself and Willy Fowler and Charlie Lauritsen took a trip to Europe. We were working jointly with the army and the air force on tactical warfare. We worked with the army on tanks and guns and that sort of thing; and with the air force on tactical support of ground battlefront operations. So with the consent and at the invitation of proper people, we went over just to see what they were doing in Europe—what the air support problem was, what their attitude toward nuclear weapons for the battlefront was. In order to make our Vista Project focused, we said we were going to set up an imaginary scenario of a Russian attack against West Germany—what would happen, how we would prepare for it, how we would meet it, and so on. We talked at great length with the people in Europe; this was a very important session for us over there, and a very illuminating one. Already Oppie's name was so famous that when our group went in to any office or to any meeting with the military people, Oppie was the first one to be greeted. I was chairman of the group, but Oppie was treated as chairman, because they knew him. They didn't know anything about me. That was all right. I was glad, because it created a rapport with the military people, since they respected what Oppie had done.

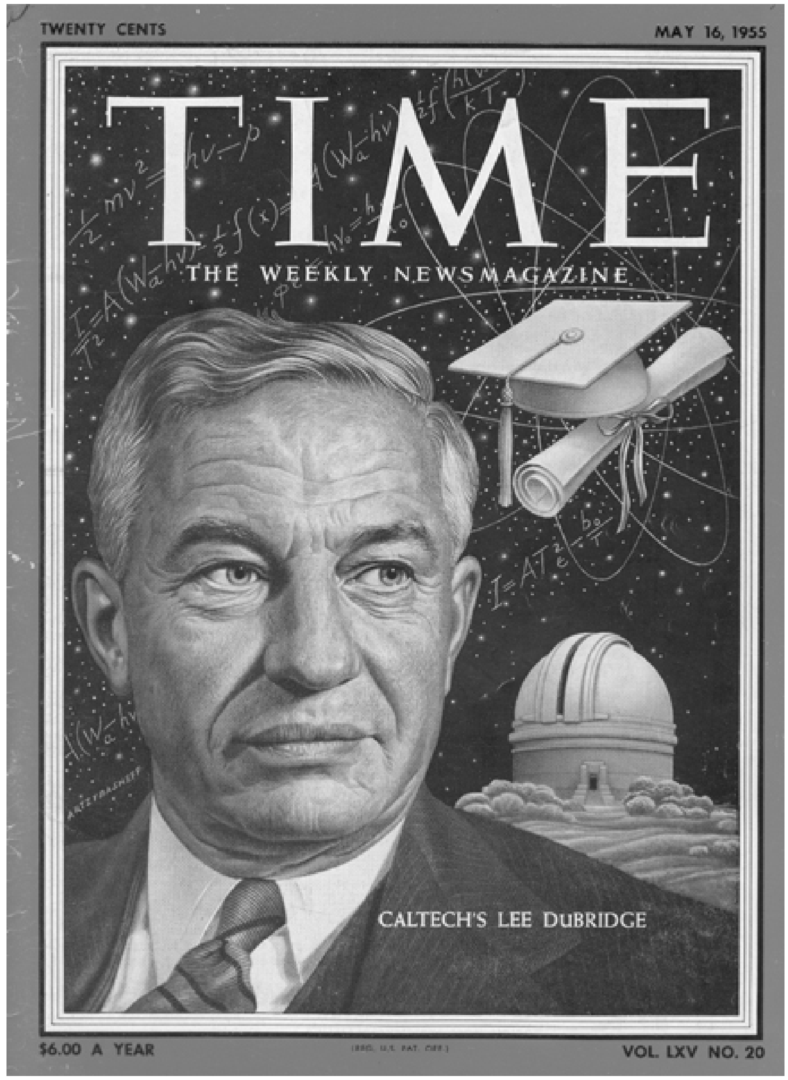

Fig 4. In its cover story on Caltech president Lee A. DuBridge in 1955, *Time* magazine dubbed him "Senior Statesman of Science." Credit: © 1955 Time Inc. Reprinted by Permission.

GOODSTEIN: Was this before his difficulties?

DUBRIDGE: Yes. In fact, at his hearing the Vista Project was brought up against him.

GOODSTEIN: Why was it brought up against him?

DUBRIDGE: This is a very interesting sidelight. I was testifying for Oppie and told [the A.E.C. Personnel Security Board] what a fine job he had done during the war, what a fine job he'd been doing on the General Advisory Committee, and how we respected his loyalty and his enthusiasm and his help and his value to the country, and so on. The A.E.C.'s very rude and nasty lawyer [Roger Robb] quizzed me. He said, "Dr. Oppenheimer wrote the Vista report on nuclear weapons, didn't he?" I said, "No, he was chairman of the committee, but we all joined in writing that report." And he took out a copy of this report and pointed to a place. He said, "Read that sentence." And the sentence read somewhat as follows: "We see no tactical application of the hydrogen bomb." Now, this guy didn't know the difference between "tactical" and "strategic." And our group was very specifically assigned to tactical warfare. And as a matter of fact we had been given very strong statements, from the secretary of the air force and others: "You stay out of the strategic warfare business." We were concerned with the battlefield, and we had no relation to strategic matters—atomic bombing of cities, or anything like that. But this sentence—and I didn't realize at the time—said we see no *tactical* use of the hydrogen bomb. And nobody else ever has since. But he took it as meaning that I didn't see any *military* use for the hydrogen bomb. And he turned this against Oppenheimer and said, "Well, you see, Oppenheimer isn't in favor of defending our country," and that sort of thing. Incidentally, have you seen Leona Marshall Libby's book?[10]

GOODSTEIN: No. I've heard that it's out.

DuBridge: It is out, and I got a copy of it. I knew Leona because she married one of my graduate students, John Marshall, who got his Ph.D. with me at Rochester. She brings up this particular point, and she misunderstands it, too—saying that Oppenheimer was charged, among other things, with saying that the hydrogen bomb had no tactical application.

GOODSTEIN: You think she doesn't understand the difference between military and tactical?

[10] Leona Marshall Libby, *The Uranium People* (New York: Crane, Russak & Company; Charles Scribner's Sons, 1979).

DUBRIDGE: Well, she didn't. At least she used the same interpretation that the lawyer implied—that this meant that the hydrogen bomb had no *military* use. All we meant was that it was a strategic and not a tactical weapon. Now I thought it was obvious, but, you know, people misunderstood it, because "tactical" doesn't carry the same technical meaning to most people.

GOODSTEIN: What's your perception of the book, other than that?

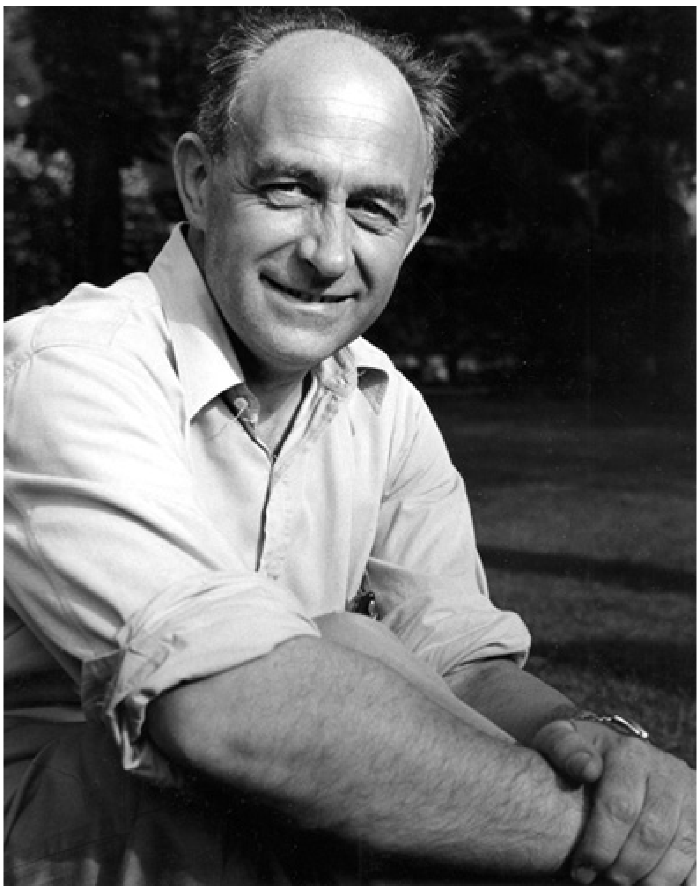

Fig 5. Enrico Fermi (1901-1954) ca 1945. Caltech Archives.

DUBRIDGE: It's a badly written book. It jumps from one thing to another. It repeats itself. I found one paragraph in one chapter that was word for word the same paragraph in a previous chapter, except for the first line. Badly edited. But it has some fascinating sidelights on some of the principal people—especially [Enrico] Fermi. She was very close to Fermi. She was at the assembly of the first atomic pile in Chicago with Fermi. She went to Hanford with Fermi to supervise the construction of plutonium-producing reactors. So she was very close to these people. She has some very illuminating and interesting sidelights and incidents, amusing and otherwise, about Fermi, about Compton, about [Leo] Szilard, and many other people. Some of these, I think, are good statements of the characteristics and achievements, weaknesses, if any, of these people. I found it fascinating reading, although it's badly written.

Anyway, Oppie was quite close to all of us—Lauritsen, Fowler, and of course Bacher. Well, we had quite a group on the Vista Project. And this time in Europe was really a very heart-warming time for personal reasons as well as a very productive time for learning what battlefield problems were. The military officers there were very cordial to us and put on demonstrations and took us out to all their installations and their bases.

GOODSTEIN: Whose idea was it to bring the Vista Project to Caltech?

DUBRIDGE: It was brought to us by the military. We didn't want to do it at first, but they brought very strong pressure for Caltech to undertake it. There was a general [General Gordon Saville]—his name escapes me—whom I'd had a good deal to do with during the war, because he was interested in radar for tactical aircraft use. He was a controversial and lively fellow. I admired him because he was quite a good guy.

GOODSTEIN: He brought Vista to Caltech?

DUBRIDGE: He and two or three of his associates came to my office. I guess he'd warned me, and I had Charlie Lauritsen and Bob Bacher and some others in there. He said, "We just need help on the technology of the battlefield. Our tanks are obsolete, our guns are obsolete, our tactical air support is in a mess. If you would just work with us and develop recommendations, we'd greatly appreciate it." Now, I think they brought pressure on the trustees to do it. So we just plunged in and worked like hell on it for many months. We took the full summer off. We were given quarters over in the old Vista del Arroyo Hotel [in Pasadena]—that's the reason we called it the Vista Project.

Well, getting back to Oppenheimer, when I first heard that there was to be any serious challenge to his clearance, I dismissed it. But at this meeting at Princeton of the Office of Defense Mobilization Science Advisory Committee, Killian came to me. Killian had a remarkable way of getting information from many, many sources. He said that the air force people were after Robert and there was going to be trouble. And we'd better know about it. I think he told Oppie the same thing—that charges were going to be made against him by some of the people in the air force and by Lewis Strauss in the Atomic Energy Commission. They seriously wanted him kicked out of government service. Well, this was an appalling piece of news. And even then I thought, Oh well, it'll blow over. But within a short time the thing was made public and the rat race began. During the hearings, of course, I had long conversations with Oppie along with Robert Bacher and Rabi and other people who were testifying on his behalf. And with Oppie's lawyer [Lloyd K. Garrison], as to how we should approach this.

GOODSTEIN: Where were these meetings held?

DUBRIDGE: Well, I guess usually in Washington, maybe at Oppie's house sometimes, in Princeton. But the critical ones were in Washington shortly before the hearings, when Oppie's lawyer was trying to get familiar with what each of us could contribute. And these were long and intimate and sad discussions of the whole situation.

GOODSTEIN: What made them particularly sad to you?

DUBRIDGE: Well, to think that this great figure, who had done so much for the country during the war, and was such a great physicist, and had done so much with the Science Advisory Committee and other activities—who had been chairman of the General Advisory Committee of the A.E.C. and ran it so beautifully and so perceptively….It was a great experience to be with him, with Fermi and [James B.] Conant, and Rabi, and Glenn Seaborg, and the rest, who were on the General Advisory Committee then, while Robert Bacher was on the Atomic Energy Commission. We felt very close to Oppie and very admiring of him. And I regarded the charges against him as trivial, if not false. And irrelevant, because they'd been looked into by General Groves years before and Groves had decided that there was nothing there to interfere with his clearance. But, you know, [Edward] Teller testified against him, [Luis] Alvarez testified against him. It was a rigged hearing. So it was a very sad business. And then it was sad afterward. Bacher and I visited him at his home several times after it was over, at the Institute for Advanced Study. Though he bore it fairly well, you could tell that it really broke him up.

GOODSTEIN: Let me ask you something about the Vista Project. I want to return to that for a minute. When it was finished [January 1, 1952], was there sentiment on the campus that Caltech should not do that sort of thing again?

DUBRIDGE: Well, there was uneasiness on the campus about doing it at all.

GOODSTEIN: Did some people come to you and express their displeasure?

DUBRIDGE: I think we satisfied the principal objectors about it in two ways: by saying that top people in the military and the government had urgently requested this, and by taking it off the campus. The whole operation was over at Vista, so no classified work was on campus. We had guards and everything over at the Vista Hotel. Only those who wanted to participate on a classified basis had to do it. Nobody *had* to be involved. No secrecy barriers were erected on campus. Of course, we couldn't talk to the other campus people about what we were doing, but that didn't seem to worry anybody. I think at the end of it, we all felt that we hoped we wouldn't have to get into this again.

Begin Tape 5

GOODSTEIN: Is much of it still classified?

DUBRIDGE: I think it is. I remember when Jerry Wiesner went into the White House as Kennedy's science advisor. He said, "You know, I came across a copy of the Vista report. That's a fine report, and it's just as pertinent now as it was then, and it's too bad that the army and the air force haven't adopted many of your recommendations." Some of them were adopted, but a lot of them were not. Now, I think many of the military people did not like the report, and I never found out why. Some military person said that the Vista report was "rubbish." And it wasn't rubbish. It was an earnest attempt to look at what was going on then. We were invited to military installations all over the country, and they put on elaborate demonstrations for us—big parachute drops, you know.

GOODSTEIN: You yourself played an active part.

DUBRIDGE: I was chairman of it. Willy Fowler took on the administrative and technical supervision of it. And Charlie Lauritsen was an important part. Bob Bacher was, too, and others. We had extraordinarily friendly cooperation. If our report had defects in it, it is as much the fault of the military as anybody, because every paragraph in that report was written in collaboration with one or more of the military people who were here. And all the things we set forth in the report we learned from the military people, by visiting their installations, seeing their operations, or talking with them about what they thought. So we were fairly proud of the report when it was done. But not too much happened.

GOODSTEIN: And it's never surfaced since, has it?

DUBRIDGE: No.

GOODSTEIN: Were any other schools invited to prepare a similar report?

DUBRIDGE: Not on that subject, of course, but M.I.T. had a number of studies on various kinds of military problems.

GOODSTEIN: Are those also still classified?

DUBRIDGE: I think so. Some of them were done at M.I.T. under military supervision. Some were done at the request of the President's Science Advisory Committee. They were more heavily involved in various military things like the development of the Distant Early Warning radar network; that was a heavily M.I.T.-directed project, kind of like the Radiation Lab studies at M.I.T. during the war. They were developing military hardware. We didn't do that at Caltech. We just looked at the equipment, its failings, and what new technology could be of use—radar, new types of airplanes, radar equipment for airplanes, detection of troops on the ground by radar and other mechanisms, that sort of thing.

GOODSTEIN: Do you think your findings still would have application today?

DUBRIDGE: Well, I'd have to look at the report again. Times have changed and the nature of ground warfare has changed. We did advocate the use of small-yield atomic weapons in the battlefield situation, to hit supply dumps behind the lines, to hit troop concentrations behind the lines, to destroy various military equipment. And the small fission bomb was then becoming available, and we recommended use of it. Those things have been adopted—whether as a result of our report or not, I don't know. But they do have nuclear artillery and they do have nuclear tactical weapons.

GOODSTEIN: Did Caltech ever undertake a similar project after the Vista Project?

DUBRIDGE: No, not as Caltech. Individuals were involved. For example, going back to this meeting at the Institute for Advanced Study with Oppenheimer, of the Office of Defense Mobilization Science Advisory Committee. We had decided that we ought to recommend to President Truman and O.D.M. that we get engaged in some more active things having to do with national security. Well, at that time Truman was still in the White House but [it was] immediately after the election of Eisenhower, in November 1952. We said that Eisenhower would probably be interested in our getting to work on some of these things, so as soon as he was in office we would make contact with him. At that meeting, they asked me to succeed Buckley as chairman of that committee. So after Eisenhower was inaugurated, we saw him and talked with him about our ideas. He said, "Fine, I need your help." We had very good back and forth conversations with Eisenhower. You probably have that picture of our Science Advisory Committee meeting with Eisenhower—that was only one of many. I chaired this. I asked Killian to head a technical capabilities panel. He did a magnificent job, and he recounts it in full in his book. And I spent one whole summer in Washington working with them on this study. I was there when the panel's report was presented to the full session of the National Security Council. The President, the entire Security Council, and many other people—there was a room full of people. There must have been seventy-five or so people, all connected closely with the Security Council or with the top military people. We had a carefully rehearsed and carefully outlined program of verbal presentation of our conclusions. Killian led off, and each of several of us then added something about our assigned part of this report. Well, Killian's book says that this had a very important impact on the military, that they accepted it with enthusiasm.

Fig 6. Meeting of the Science Advisory Committee of the Office of Defense Mobilization with President Eisenhower and Arthur S. Flemming, Director of the Office of Defense Mobilization, 1953. Standing (left to right): Emanuel R. Piore, Oliver E. Buckley, Alan T. Waterman, James B. Fisk, Detlev W. Bronk, Bruce S. Old, J. R. Killian, Jr., David Z. Beckler, Robert F. Bacher, Jerrold R. Zacharias, Charles C. Lauritsen. Seated (*left to right*): Arthur S. Flemming; President Eisenhower; Lee A. DuBridge, Chairman; I. I. Rabi. Caltech Archives. Photo by Abbie Rowe.

I was on the committee six years, four as chairman. Finally I said, "We ought to have a rotating membership on this committee, and I'm going to be the first to rotate off." I resigned from the committee in 1956. It was not easy to be at Caltech and be there, too, because we met frequently and for long periods. That started the rotation, and there's been pretty regular rotation of membership since. When I resigned in '56, the committee asked Rabi to be chairman. He said, "I'll take it for a while, but I think other things will be coming along." And he and Bill [William T.] Golden and others, for years, had been advocating a full-time science advisor, but none of the presidents would take it. Then came *Sputnik* [November 1957], and the great excitement. Eisenhower was much concerned. So he made two moves, first to appoint Killian as full-time science advisor—Killian had to resign as president of M.I.T. to do that—and second, to make the O.D.M. Science Advisory Committee the *President's* Science Advisory Committee officially. To have it report directly to him, not through O.D.M. any longer. That changed the character of the whole business.

GOODSTEIN: Let me ask you about [Hsue-Shen] Tsien.

DUBRIDGE: Oh, Tsien was such a marvelous guy. He had been here before the war [1938-1946]. I didn't know him then. He had been in great demand during the war on various aeronautic and jet propulsion projects. He was as prominent as von Kármán was. He was a real protégé of von Kármán—mutual admiration as far as I could tell, between them. After the war, he went to M.I.T. as professor of aeronautics for three years. Then Harry F. Guggenheim decided to finance two research centers for aeronautics, one at Princeton and one at Caltech. And for each one there would be a Robert H. Goddard Professorship of Jet Propulsion, and there would be financing for an expanded program in modern aeronautics, jet propulsion, supersonic flight, and all the rest of it. Both Princeton and Caltech wanted Tsien to be the Goddard professor, and we won. He came back here, to our great pleasure. He fitted in so beautifully and was so imaginative and so effective, and so at ease, and respected by the faculty. He was a fine faculty member. And then he came in to see me one day [in 1950] and said, "You know I have some elderly parents in China. I

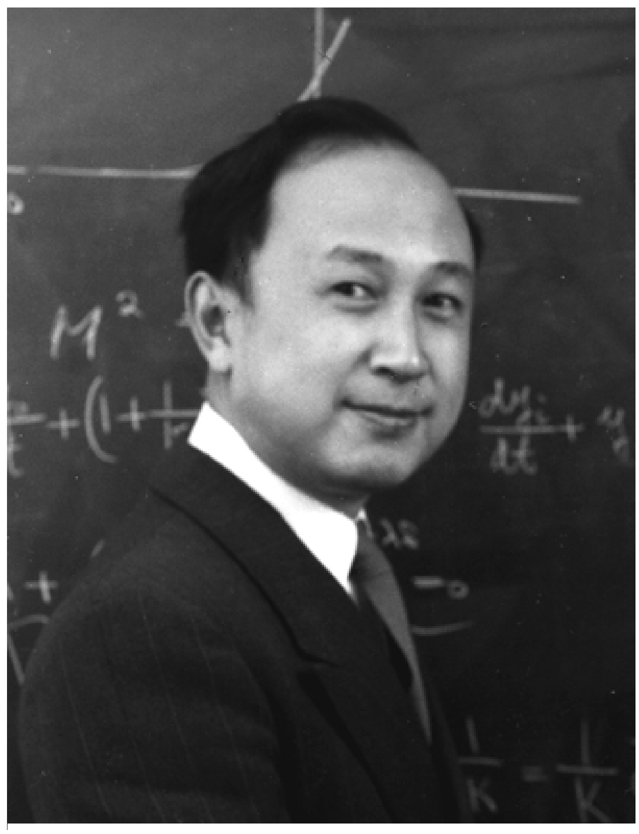

Fig 7. Hsue-Shen Tsien, undated photograph. Caltech Archives.

haven't seen them for a long time. Of course it wasn't possible to go back during the war, but I can probably get back there now. And I would just like to take a leave." I said, "How long?" He said, "Well, I really don't know how long I want to stay with them. It depends on my parents' health. But a few months anyway." And I said, "Of course, you can have a leave of absence." So he made the arrangements perfectly openly and he told us all about it. Well, someone spoke to Dan Kimball, who was then assistant secretary of the navy, saying that Tsien was going to make a visit back to China. Dan said, "Oh, no, we shouldn't let him go to China." You know, China was not our best friend. Tsien obviously knew a lot, and Dan Kimball felt that it was a little risky for him, Tsien, and for the U.S. to have Tsien back in China. Well, I didn't feel that way. I trusted Tsien enough to know that he wasn't going to take along a lot of his papers to work on, some of his aeronautical theoretical work. Well, the trouble is that somebody took Dan Kimball's remark seriously and said, "We've got to stop him." How were they going to stop him? Well, the way they found out to stop him was to charge him with having been a Communist. They found there was a little Communist group here in the thirties.

GOODSTEIN: I've heard that said before.

DUBRIDGE: I don't know who was in it. But there was a little group here, as there were at many universities back in the Depression days. They said that there must be a better economic system, and maybe the Russians had found it. Tsien's version of his association with that group was as follows. When this charge was first brought up, he came in to me and said, "I don't understand this." And I said, "Well, did you have any connections with a Communist group?" And he said, "Well, there was a group of people here that had social gatherings. When I came over to this country, a stranger, two or three of these Caltech people invited me to their house for a little social gathering and I went several times." He said, "I guess there was some talk about politics; but it was mostly just talk about general things, and I regarded them as purely social events. I certainly didn't sign up in any way with any Communist Party. And I didn't even remember the word 'communist' being mentioned at these affairs." But somebody had written down on a piece of paper the names of the people who had attended one of these meetings, and this was later brought into evidence. I think there was a typewritten list, and over at the side was written "Tsien." Well, that killed him. He had been back to China once before [1947] and returned to this country. And apparently the standard procedure when you came back to this country was to answer the question, "Have you ever been or are you now a member of the Communist Party?" And of course, he wrote "No." So the charge was perjury—that he had been a member of this Communist group but when he reentered the country he had said he had not been.

GOODSTEIN: And the evidence was this list?

DUBRIDGE: Yes.

GOODSTEIN: Did you see the list?

DUBRIDGE: I saw a copy of it. It was put in evidence at his hearings. So he was arrested and his baggage was seized, with his papers and everything else. And the customs or immigration officer said that there was a lot of "very highly technical material"—implying that it was secret material.

GOODSTEIN: Was that the first word you had of what was going to happen—when they arrested him?

DUBRIDGE: I had heard that there were some rumblings about Dan Kimball's remark that he ought not to go. But I didn't think they would really move on it. When [he was getting ready to leave], everything seemed to be all right. When we got word of his arrest, that was the first time I heard of it—he called somebody here. They put him in a detention center in San Pedro, where we visited him. He had a little cubicle, a room, that was perfectly comfortable. It wasn't a jail—but it was a detention center. He had a room and a desk and a light and a bed and so on. But for him to be detained that way was a terrible blow to him—to his ego, his self-respect. Here he thought he'd served this country so well, which he had, and then to be treated in this way….It made him, eventually, very bitter. Well, Clark Millikan and I visited him often, talked with all the people we could think of.

GOODSTEIN: Did you talk to Kimball at that time?

DUBRIDGE: Clark certainly did. I don't remember whether I saw Dan or not. Dan was also shocked. He said, "Well, I didn't mean that he should be arrested, you know. That's terrible! He's no Communist. There's no reason for detaining him." He was irate at the action of the Immigration Service—and I think very angry that his passing remark had been taken seriously, and that there hadn't been some other way to persuade Tsien not to go. Maybe Dan thought I should have persuaded him not to go—I don't know. Anyway, it was a sad event. I visited Tsien a couple of times down there and just talked to him, to get his reflections about it. They finally put him on parole and he came back to Pasadena. But he could not leave Los Angeles County without permission. And he was put on parole under the supervision of Clark Millikan, who had to swear that he would report if Tsien left the county. It was a very humiliating experience. But finally the parole was lifted and he went back to China anyway [1955].

Then he became a very bitter anti-U.S. man. Recently Caltech decided to offer Tsien the Alumni Award, and he said he could not come. He wrote to Frank Marble and said, "The reason I can't come is because I'm still under a deportation order. If I were to come back to the United States, I would assume that that deportation order would be brought out, and I would be excluded. So I don't want to get into trouble. I would like to come back. I would like to visit." Marble sent me a copy of the letter and said, "What can we do about this?"[11] This is just a few weeks ago. The only thing I could think to do was to write Frank Press [science advisor to President Carter]. Frank Press then wrote to me in reply, saying he was interested to hear about this and he would get somebody at work on it promptly—but of course he left [the White House] January 1st, and I don't know what's happened since.[12]

GOODSTEIN: Was Tsien framed? Do you think it was a real piece of paper with a real list of people?

[11] Frank E. Marble, now Hayman Professor of Mechanical Engineering and professor of jet propulsion, emeritus, was Tsien's friend and closest associate at Caltech.

[12] The award was hand-carried to China some twenty years later, in December 2001, by Frank Marble, who presented it to Tsien on behalf of Caltech

DUBRIDGE: Yes. I had no reason to doubt it, because we found out during the McCarthy days that two or three Caltech graduate students and others were involved. It was really a small Communist group. And some of the people later admitted it. But I do believe Tsien when he said that he did not regard this as joining the Communist Party. He was a lonesome stranger in this country, and to be welcomed by this nice group of people among the Caltech family and some people around town, I think….He said he had fine times at these nice social gatherings, never dreaming that he was involved in anything improper. “Framed” is not the right word; I think it was just a series of misunderstandings and overreactions.

GOODSTEIN: Did the trustees become very upset about Tsien? They were upset about Pauling. The business with Tsien happened in the same time period.

DUBRIDGE: Yes. I suppose that the ones who didn’t like Pauling also believed the charges against Tsien. I remember telling the trustees about it—that we had looked into it and that we were convinced that he did not belong to this Communist group but was associated with some of them, as a lot of people were. I don’t recall any vigorous remarks among the trustees about it. Some of them, I know, felt that [the way he was treated] was a shame. Some of them cooperated with us in seeing whether we could do something about it. But I suppose some of them thought, “Well, if he’s a Communist, then let’s just put him over in China again.”

GOODSTEIN: Well, they certainly produced a very ardent Chinese Communist as a result.

DUBRIDGE: That’s right.

[End of Part II]

The Scientist’s Job

Lee A. DuBridge declared in 1947:

> The first responsibility of the scientist or engineer is to be a *good* scientist or a *good* engineer… It is not the job of the scientist to be primarily a politician, a sociologist, a military leader or a preacher.

Quoted in Judith R. Goodstein, *Millikan’s School: A History of the California Institute of Technology* (New York: W.W. Norton, 1991), p. 261