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

INTERVIEWED BY
JUDITH R. GOODSTEIN

February 20, 1981

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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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

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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.

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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

[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.

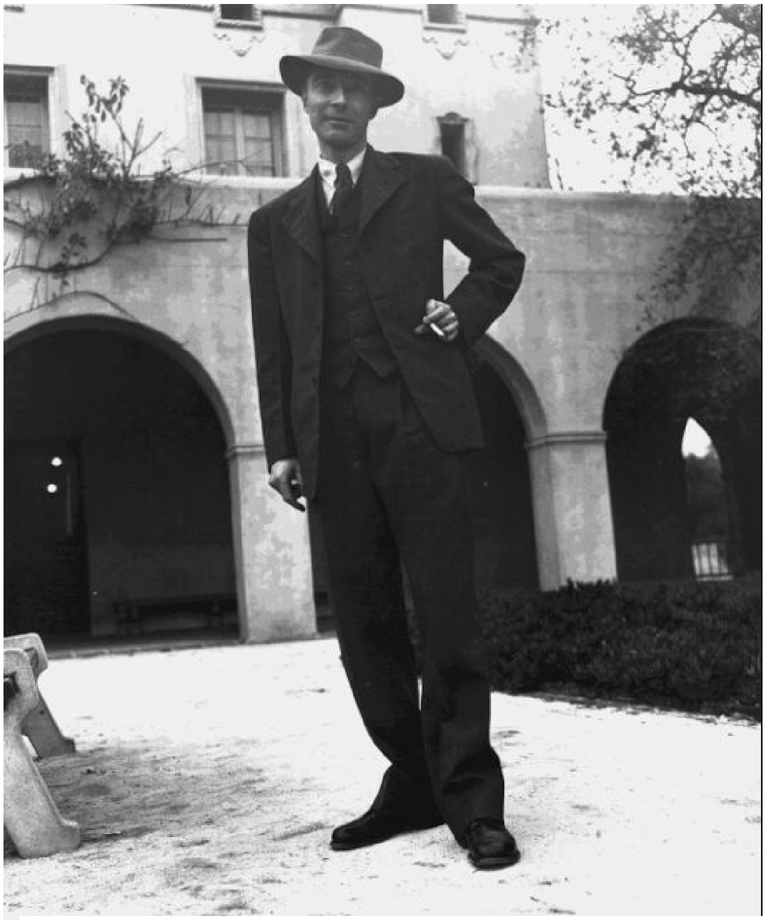

Fig 3. J. Robert Oppenheimer on the Caltech campus, 1930s. Caltech Archives**.**

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

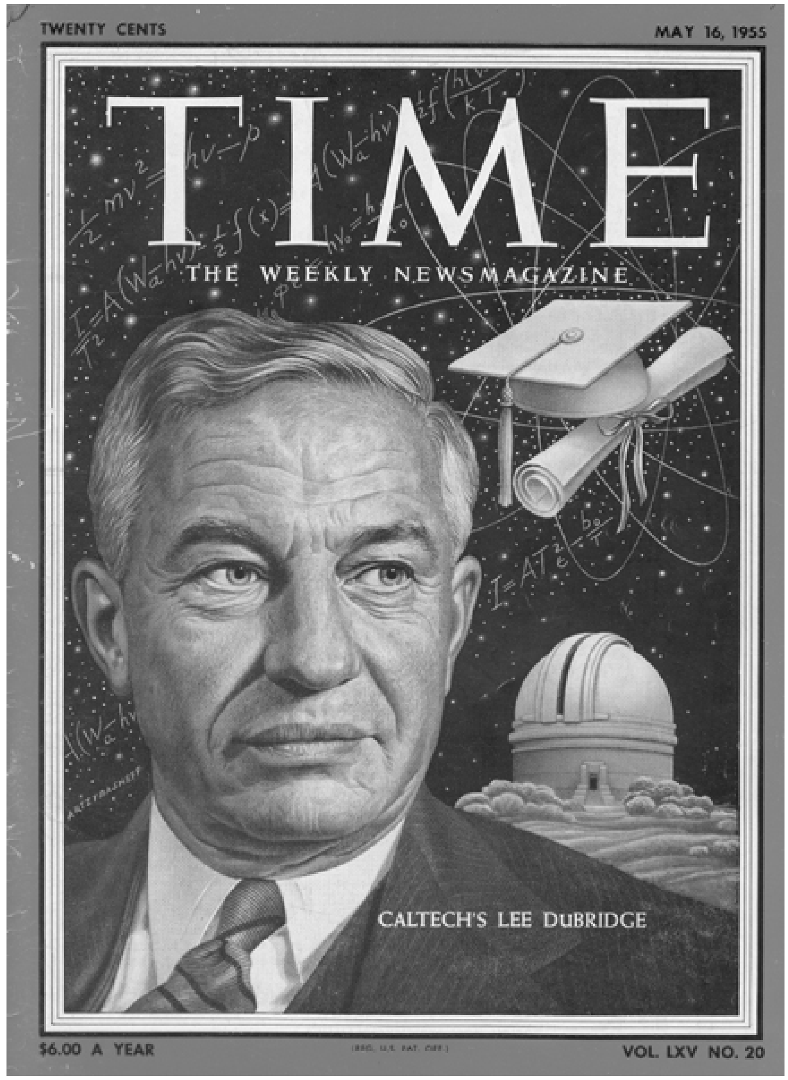

Fig 4. In its cover story on Caltech president Lee A. DuBridge in 1955, *Time* magazine dubbed him "Senior Statesman of Science." Credit: © 1955 Time Inc. Reprinted by Permission.

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.

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?

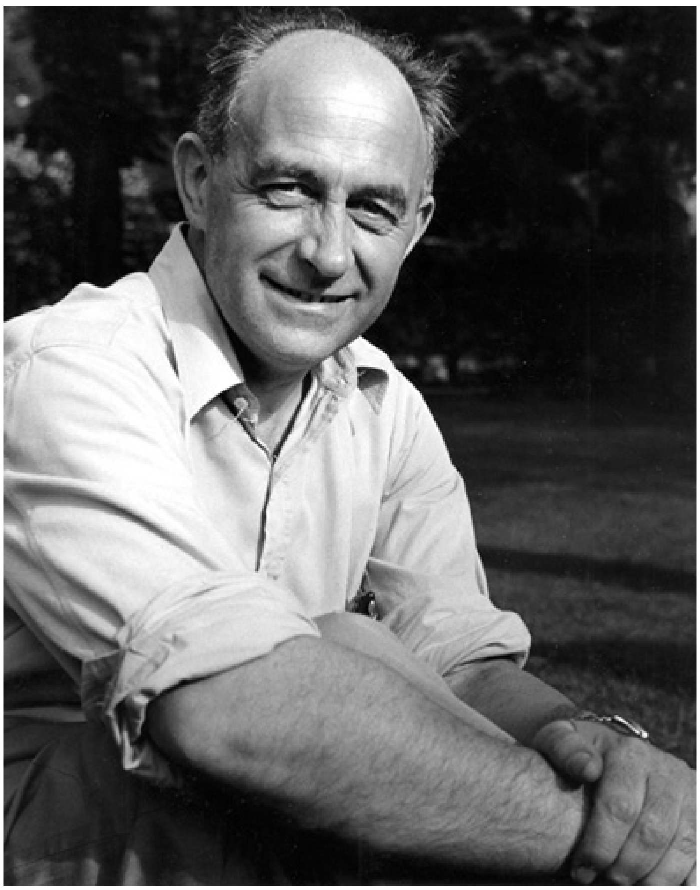

Fig 5. Enrico Fermi (1901-1954) ca 1945. Caltech Archives.

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

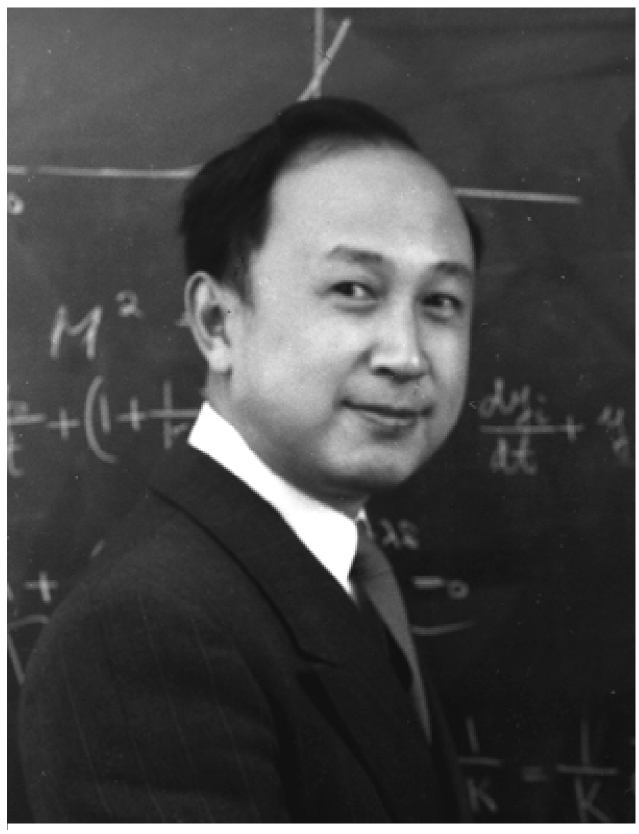

Fig 7. Hsue-Shen Tsien, undated photograph. Caltech Archives.

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?

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[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