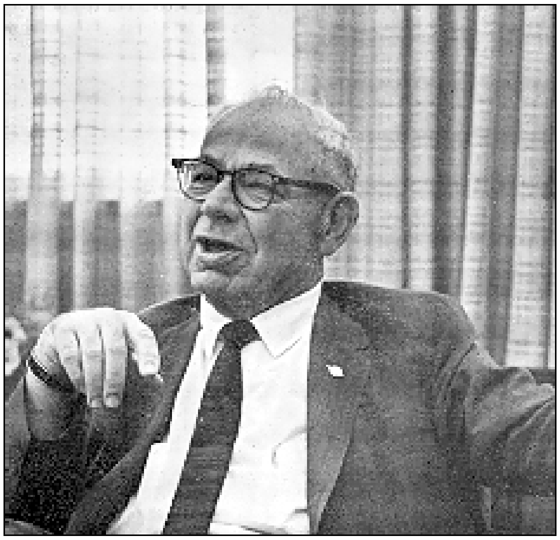

ARTHUR L. KLEIN
(1898–1983)

INTERVIEWED BY
HARRIETT LYLE, JOHN GREENBERG

February 1979, April 1982

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Pasadena, California

Subject area

Engineering; aeronautics

Abstract

Interview in four sessions, February 1979 and April 1982, with Arthur L. ("Maj") Klein, who entered Throop College, the predecessor of the California Institute of Technology, in 1916. When R. A. Millikan arrived as the institute's head, Klein decided to change his major from mechanical engineering to physics in order to work with him, earning his bachelor's degree in 1921 and a PhD in 1925. He stayed on as a research fellow in physics and soon become involved in the activities of the new Guggenheim Aeronautical Laboratory at Caltech (GALCIT), along with Clark B. Millikan and the aircraft designer Arthur E. Raymond. He became an assistant professor of aeronautics in 1929. Klein designed much of GALCIT's 10-foot-diameter wind tunnel, which went into operation in 1929, and he later helped design the Southern California Cooperative Wind Tunnel (1945), which was financed by five Southern California aircraft companies and operated by Caltech. Klein was also responsible for many aspects of the design and testing of important aircraft, including Douglas Aircraft's DC series. He had begun consulting for Douglas Aircraft in 1932; by 1937, he was spending half his time there and half at Caltech, and this arrangement continued until his 1968 retirement from Caltech as a full professor in the Division of Engineering and Applied Science.

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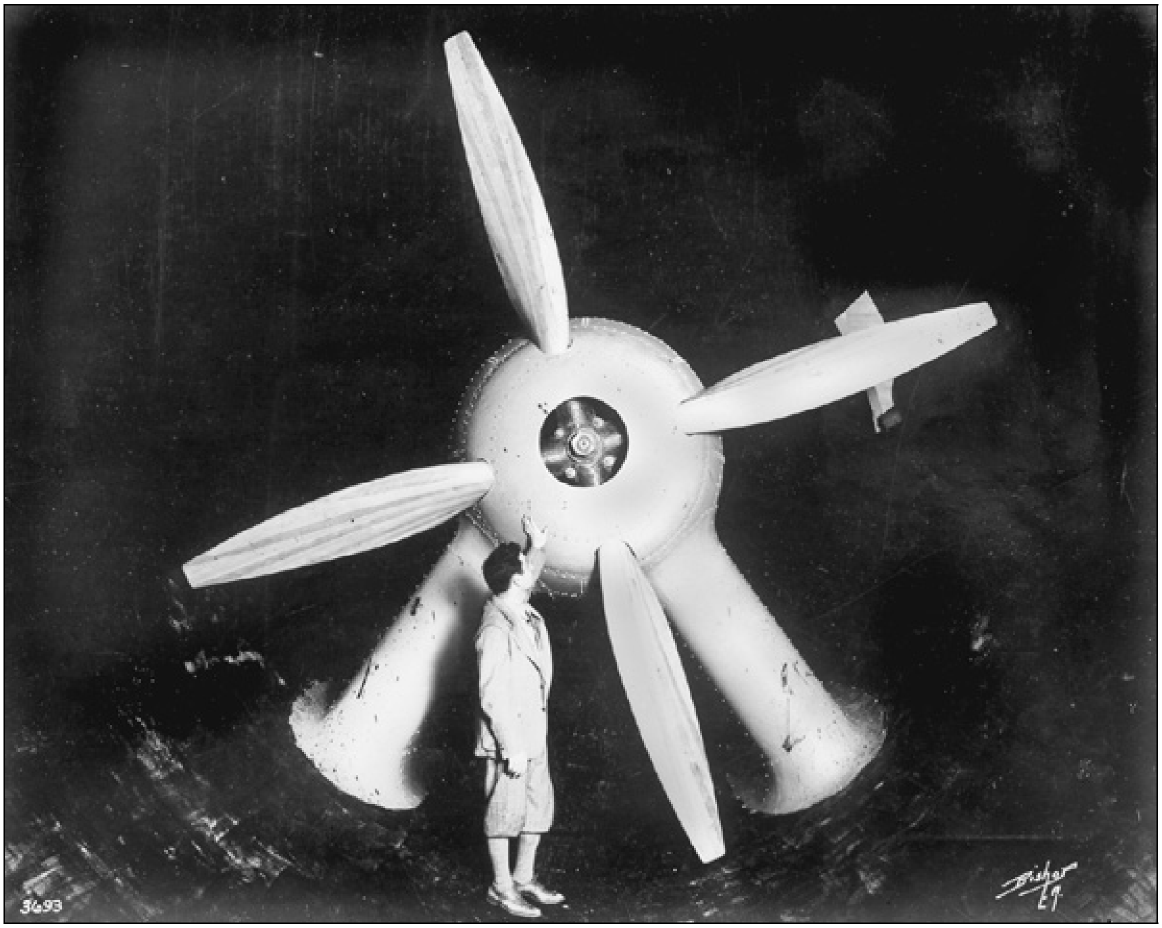

Arthur Klein in Caltech's 10-foot wind tunnel, ca 1929. Caltech Archives

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH ARTHUR L. KLEIN

BY HARRIETT LYLE AND JOHN L. GREENBERG

PASADENA, CALIFORNIA

PALOS VERDES ESTATES, CALIFORNIA

Caltech Archives, 1984

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LYLE: I'd like to ask you about your family background and about growing up in Los Angeles.

KLEIN: My family were merchants in Los Angeles. My grandfather on my mother's side came to California in1850, and my father's family came in the early eighties. They had started separate businesses, which were later unified into a family business under both of their names—Klein-Norton, a men's-wear merchandising company. When I first knew it, they were still operating the original business, which was a retail men's furnishings store. They also had a fairly large—for the time—wholesale establishment.

LYLE: Was that in downtown Los Angeles?

KLEIN: Yes. The businesses were on Los Angeles Street, somewhat north of First Street. The wholesale business carried a general line of merchandise, which was in those days almost everything that a man or a woman would wear, except outer clothing. Every year either my father or my grandfather, Mr. Norton, went to New York and bought the merchandise. It took three weeks each way. Communications were so bad at that time that it took two people to run a business. I think the wholesale business grew up out of this fact, because they could buy for others at the same time they were buying for themselves.

LYLE: Did you go on one of those trips?

KLEIN: Oh, no. No, this was before 1900. When I was a very small boy, they sold the retail store. I have a very dim recollection of it. I lived in the western part of Los Angeles and went to Los Angeles city schools. I ended up going to Los Angeles High School, which my mother had attended twenty or twenty-five years earlier. This high school was then in downtown Los Angeles.

LYLE: Did you have any brothers and sisters?

KLEIN: I had no brothers and sisters. My father came to California because his first wife was ill and needed a better climate than where he was, which was West Virginia. I had a half-sister, the daughter of my father and his first wife. After his first wife died—I think it was from tuberculosis, but I'm not sure—my father married his partner's eldest daughter, so the business became a one-family business.

After I graduated from Los Angeles High School, my mother wanted me to go to Stanford, in medicine, but I decided that medicine did not appeal to me. I was more interested in engineering and science, which was strange, because to my knowledge no one else in my family was educated in these fields or had even been to college.

LYLE: Do you remember when you became interested in science and engineering?

KLEIN: I was interested in technology and science at a very early age. Fortunately, I was exposed to theatrical productions when very young, and what I remember is wondering about the technical aspects of them. Marjorie Rambeau swinging on the clapper of the bell in *The Heart of Maryland*, and why the scenery was so curiously made in the first act of *Peter Pan*—which was resolved when the children flew out the window—and how they kept the alarm clock wound up in the belly of the crocodile. These were the things that impressed me, so they are the clearest things in my mind.

LYLE: Do you remember having any friends who were interested in science, too?

KLEIN: Well, I had one friend who was interested in zoology and biology, but we were not really close. The boys around me were all what you might call just average citizens. One of them ended up as a professor of philosophy at Columbia University, but the others I don't remember ever doing anything outstanding.

LYLE: Do you remember any teachers that were particularly good in science?

KLEIN: At L.A. High School, I discovered an excellent library. I had always been a large reader. My parents had to call up the public library and tell them to send me home. I was able to get a great deal out of this library, and my teacher in mathematics, D. S. Swan, helped a great deal. I also was a laboratory assistant in chemistry when I was a senior. This was not wholly successful—I had a fire. In the library, though, there was a complete collection of the bound volumes of *Scientific American* way back to the middle of the nineteenth century, which I proceeded to read in the study period.

LYLE: You must have had a lot of time to read?

KLEIN: I was a fast reader at a very early age. I learned things quickly, so I was able to accumulate all sorts of miscellaneous information, most of which was of no particular use. Occasionally something would stand out.

LYLE: Did you find that your parents encouraged you at all in this field?

KLEIN: My parents did not really understand me. My father wanted me to go into the business; my mother wanted me to become a physician. But my instincts were not in that direction. They were not difficult parents, though. They let me come to Caltech when it was a very small school, with no graduate school. [Then called Throop College of Technology; name changed to California Institute of Technology in 1921—ed.] Swan called it to my attention; he had gone out there to an open house. I entered in 1916, after graduating from high school.

LYLE: Do you remember what it looked like and what you thought about it when you came?

KLEIN: When I came here as a freshman, the only permanent building that was in use was Throop Hall. In the middle of my freshman year, we moved into Gates Laboratory, the chemistry laboratories having been on the top floor in Throop Hall, under the roof. There were some wooden temporary buildings, including the power plant and a hydromechanical laboratory, which were later burnt down or destroyed. But Throop Hall was the sole permanent building at that time. It lasted until the 1971 Sylmar earthquake.

LYLE: Do you remember the feeling of the campus?

KLEIN: Well, the campus was wild. I have always been a bad draftsman, and I had to make up my freshman engineering drafting during the summer. I was working in an unused office in Throop Hall on the second floor. I had borrowed a .22 automatic rifle from a friend of mine, a classmate, and when I got tired of making drawings I went out on the balcony at the end of Throop Hall and shot ground squirrels. I was never rebuked or had any remarks made about this occupation [laughter]. This gives you an idea what the campus was like. It was completely unimproved, except for the part that was used. Part of the campus behind Throop Hall was still an orange grove, which had pretty much gone to seed. The campus did not go clear through to Hill Avenue at that time.

LYLE: Were you a little worried about coming here, whether you would do all right?

KLEIN: Well, I don't know. I never seemed to have much trouble with classwork. I'd been getting fairly decent grades—not top grades, just average grades.

LYLE: Did you live at home?

KLEIN: I lived on the campus in the Old Dorm. My family was living at Sixth and Vermont in Los Angeles, and I could take the Sixth Street car downtown, right by the Pacific Electric

Building, and then take the Oak Knoll car and come up to California and Lake. I went home on Friday or Saturday noon, and came back Sunday night.

LYLE: What was the dorm like at that time?

KLEIN: Oh, it was a two-story building that had been up at Throop Polytechnic Institute on North Raymond. It was moved down here in sections. It consisted of about thirty rooms, which were occupied by two students each, with common sanitary facilities. There were no individual sanitary facilities in those days. Downstairs there was a dining room, where you ate your meals, and a lounge; I have pictures of it.

LYLE: Did you plan to study mechanical engineering, then?

KLEIN: Yes, I did study mechanical engineering.

LYLE: And you got your degree in that?

KLEIN: No, I didn't. In my senior year I had too much spare time, and along with two other people, I got into trouble. As a result of this, we were expelled. This was for general hell-raising around the campus. Two of us later became faculty members—Professor Robert T. Knapp and myself. That's why it took me five years to get my bachelor's degree [1921].

LYLE: Would you describe why you were expelled, or don't you want to discuss it?

KLEIN: Oh, I don't want to go into that. It was a lot of trivial things, like rewiring some of the electrical laboratories so it confused people, fixing up the little wind tunnel so it blew the wrong way—more or less technological pranks. So, as Dr. Millikan was coming, I changed to physics, because if I'd graduated I would have had to go work in my family's business, and I didn't want to sell socks, shirts, and underwear. In fact, I've always been a bad salesman.

LYLE: You knew he was coming and you switched to physics because of that?

KLEIN: Yes, he came in 1921 and that was the year I graduated. He came between ’20 and ’21. When I came back, I changed to physics.

LYLE: Could you feel a big difference when he came, in the way the institute was run?

KLEIN: Oh, yes. The preceding president was Dr. James A. B. Scherer, who was a minister. The institute was run with a high moral tone. He had compulsory chapel every Monday, with prayers and things, which eventually disappeared. It was also, you might say, post-Victorian in general outlook.

After I graduated, I entered the physics department. I had no appointment—I had asked for an appointment but I didn’t get it. But Jesse DuMond had an appointment, and he was in Europe and was unable to return and they needed an extra teacher. So I got his teaching fellowship and taught freshman physics and freshman mathematics for two or three years. Apparently I was satisfactory as a teacher.

LYLE: How did you feel about it? Did you like it?

KLEIN: I got bored with it.

LYLE: Did you get to know Dr. Robert Millikan when you were doing this teaching?

KLEIN: I got to know him fairly well. When he received his Nobel Prize [1925], I was a member of the University Club in Pasadena here, a very small university club. I organized a banquet for Dr. Millikan for the physics department. Afterward, about the middle twenties, the institute purchased the Neimeyer place, which was the remainder of this block to Hill Avenue; with it they got the old home. I moved in there [the old faculty club] when it first opened—I’d been boarding in Pasadena. Being a young, energetic person, I became chairman of the house committee and put on various events, like garden parties and things, for the faculty. This was a big break, because Professor [Richard Chace] Tolman, who was still unmarried, lived there. Sir Charles—or Dr. Charles—Galton Darwin, later knighted and director of the National Physical

Laboratory in England, lived there. And Professor [Venkata] Raman from India, later a Nobel laureate, lived there when he visited Caltech.

LYLE: They were all living in that house?

KLEIN: Yes. It was a very good atmosphere for a young scientist. I enjoyed that sort of thing.

LYLE: Did you all eat there, too?

KLEIN: Oh, yes.

LYLE: Did you know [Arthur Amos] Noyes at all during this time?

KLEIN: No, I knew him just casually. Of course, he was taking care of the chemistry department, and some of my best friends were in the chemistry department, and they all thought he was very wonderful. One of my best friends turned out to be one of the country's leading pharmaceutical chemists, and he always thought Noyes was just great. But I didn't know him well.

I got to know a good deal about the Mount Wilson Observatory, because one of my other classmates worked there. My first year in graduate school, I lived at his house up north of Colorado Street, with his mother and his young sister. I rented a room from them. Mount Wilson offices, shops, and labs were in Pasadena. My friend worked there.

LYLE: How did he get up to Mount Wilson?

KLEIN: He drove up. You went up the old road then, which was fairly steep.

Norman Bridge Lab [Norman Bridge Laboratory of Physics] had sort of a seething atmosphere, because the quantum theory was new and relativity was coming in. There was an awful lot going on and it was all very radical. At that time, almost all the problems in physics at Caltech had to do with the validity of the quantum theory. Mine was one of the few exceptions. The others were all directly on that.

LYLE: Well, you switched from mechanical engineering into physics. Was part of the reason you switched because of the excitement in physics then?

KLEIN: Yes, it was more adventurous.

LYLE: Did you take the course that Ira [Sprague] Bowen taught in physics?

KLEIN: No.

LYLE: I was talking to Carl Anderson and he said that that course was one reason he switched into physics.

KLEIN: Well, Ira Bowen was a graduate student when I was. Carl, who had room 23 across from mine, is about six or seven years younger. I've always teased Carl, because I did some things for him and we never got any credit. I supplied him with direct-current power for his positive-electron experiments, performed in Guggenheim Lab [Guggenheim Aeronautical Laboratory]. Every time I sit next to him in the Athenaeum, I tell him we never got any credit for anything we did [laughter]. He knew I wasn't terribly serious about it.

So I got my PhD in 1925, after completing my thesis on "Secondary emission from a nickel surface due to positive-ion bombardment," which was a very difficult problem. It took four years to get the apparatus to work; finally I got some real nice results out of it. Fortunately, I didn't try to explain them, because the things came out in what are now called energy levels, and I knew of no reason why these curves should go along on a plateau, suddenly jump to another plateau, and suddenly jump to another one. I had so many points there wasn't any argument about it. I had a hundred some points describing this thing. There was no fancy mathematics I was trying to fit a curve to, but I couldn't figure out what would do that. These things later came out of the quantum theory and surface chemistry. But I was rather proud that I didn't put in a half-baked explanation that I didn't really understand.

Well, about this time, when I was cleaning up on that experiment after my PhD, Clark Millikan and Hervey Hicks were working across the hall on the wind tunnel and aircraft equipment. And they were hopeless, mechanically. I started helping them. They were building an airplane, and they were having troubles with that—simple mechanical problems they didn't know what to do about. They did the stress analysis and aerodynamics very well, but just how you put things together, they didn't know.

LYLE: Where had you learned how to do that?

KLEIN: I think it was just sort of built in; I just picked it up. Well, as a boy, I had worried about things like how did an automobile differential work. I heard the name, and I knew why they had to have it, and then I figured out how to make it. Then I went down on Flower Street in Los Angeles, which was then automobile row, and looked around there until I found one that was cut open, which they usually had somewhere around the showroom, because people didn't understand what was inside it. I found one that was just the way I had figured out to make it.

LYLE: Now, would that be in high school when you did that?

KLEIN: I was in high school, yes. I had a great curiosity about things, and I'd read all of these *Scientific Americans*—oh, some thirty or forty bound volumes of them.

LYLE: So they would come over and say, "We have this idea about how to do something, but how do we actually do it?"

KLEIN: They'd say, "Well, you can't read this thing," and I'd go over and show them how to read it. I told them that the thing they had wasn't going to work, on account of vibration. Finally it ended up by my getting the job of making things work for the aeronautics group. Before my job was formalized, everyone in the [aeronautics] department was away for the summer and I was building their airplanes for them and the Guggenheim Laboratory, and I guess someone finally told Millikan that he'd better give me a title so I could be authorized to do this and that. So I got into the department that way. And then, arrangements with Douglas [Aircraft] went through, which resulted in Arthur E. Raymond, who was then assistant chief engineer of Douglas, coming up here [1926] to teach a course in airplane design on Saturday mornings.

LYLE: Had Theodore von Kármán come yet?

KLEIN: Von Kármán visited; he was not here permanently at that time. He didn't become director [of the Guggenheim Aeronautical Laboratory] until later [1930]. So I attended these lectures, and I got an awful lot out of them, because Arthur Raymond was and is a first-class airplane designer—probably the best one we've produced in this country. He is responsible for the DC-series airplanes and numerous others that the public doesn't even know about.

LYLE: So he gave a series of Saturday morning lectures?

KLEIN: Yes, on airplane design. Aircraft performance was done by graphical methods which were tedious. He wanted a fast method of doing airplane performance, so he had [W. Bailey] Oswald, who was one of the graduate students, tackle it. Oswald came up with a method which was published as a paper by NASA—it was NACA [National Advisory Committee for Aeronautics] then—which revolutionized airplane performance, reduced it to a series of charts. [Oswald, W. Bailey, "General formulas and charts for the calculation of airplane performance," NACA report 408, 1933] Oswald went to work for Douglas Aircraft with the DC-1, for a few days. He retired about thirty-five years later as head of an aerodynamics group in the Santa Monica plant, with about 300 people in it. Ernest Sechler was in that group. He got steered in the direction of sheet-metal structure, and Douglas supplied us with test samples.

LYLE: Did you think about joining that group, too, at that time?

KLEIN: Well, I'd been working for the previous year or two for Jack [John K.] Northrop. Just before the DC-1 was started—about a year before—Jack Northrop, after many vicissitudes, was set up as a subsidiary of Douglas. I wrote up a piece of poop on this for Professor Hopf, because he was all mixed up. He couldn't understand, when the DC-1 was ordered, why they'd specified Northrop construction. He didn't understand the position of Jack Northrop in reference to Douglas. Jack Northrop was a subsidiary. I had been working for Northrop for about two years at the time this happened. So Raymond said he thought I'd better come and be a consultant for

Douglas proper, too, because of this business. There are certain parts of the DC-1 that I designed, and I proposed the wing joint and the flap structure and its driving systems. I started working directly for Raymond as the chief engineer. Shortly thereafter [1934], [James Howard "Dutch"] Kindelberger left Douglas and founded North American Aviation, and Raymond became vice-president of engineering. I worked for Douglas for thirty-nine-and-a-half years, and I had worked for Northrop before that, along with being here and designing apparatus. I designed the wind-tunnel balances that are still up on the third floor and are used when they need an airplane balance, the rigging that supports the model, and various other things. I designed practically all of the older equipment in the laboratory. In '37, Douglas Aircraft was booming, and it was fairly dull over here, and I proposed to Clark Millikan that I go half-time. So I went on half-time at Caltech until I retired [1968] and worked the rest of the time for Douglas.

LYLE: How did that work out? Did you like that arrangement?

KLEIN: It worked out very well for me. It was strenuous, though. I had to drive 18,000 to 20,000 miles a year. Douglas was at Santa Monica, Long Beach, El Segundo, and Huntington Beach. So there was no place for me to live. In '37 I was living in Los Feliz Heights in Los Angeles, which is near Griffith Park. I was divorced from my first wife; I remarried and bought a house in the Sunset Plaza district, which is up above the Sunset Strip. Then my wife was ill and wanted to be near the ocean, and we sold the house and moved down to the beach, where we rented for a year or so, and then bought a house in Palos Verdes—that's where I live now. In the meantime, I've remarried. But you might as well live where you want to, with that schedule, because there isn't any place you can live comfortably. I had to do a lot of automobile driving, which was pretty bad. I got into trouble with it; I had nervous indigestion. Well, that brings us approximately up to date.

LYLE: Did you go to any of the lectures when Albert Einstein was here?

KLEIN: No, I was very busy over here in aeronautics. And he gave them in German, and my German was not good enough.

LYLE: Another thing I was curious about, they used to have special talks once a week, I think it was on Friday afternoon, that Millikan set up. Do you remember anything about those?

KLEIN: Well, the physics department had two physics seminars a week, and there was a third lecture, generally on Friday, of what was called the Astronomy and Physics Club, which was run jointly by the physics department and Mount Wilson Observatory. And all the time I was in physics, I attended these seminars. They were very informative. What are now called the Watson lectures were the Monday lectures given to the physics undergraduate students. They were given here, and then they were given again in the evening for the public. And those survive in some form, but I haven't been following what the physics department is doing, because they change it around all the time. Their subject has proliferated, like everything else, and it's going off in all sorts of subdivisions.

LYLE: Do you remember anything about the Biology Division?

KLEIN: Oh, yes. Well, I knew [Thomas Hunt] Morgan and [Calvin] Bridges and those people when they came out here, and I got to know some of them pretty well. Morgan was a brilliant man.

LYLE: Did you ever take any biology classes or go to seminars?

Klein; No, he came after I was already on the faculty, and I was already up to my ears in things to do, too. But one of the interesting things that happened—my half-niece, who was the daughter of my half-sister, was a very remarkable woman. She was married to Lewis Browne, who wrote books on religious items. They lived in Uplifters Ranch, in Santa Monica. They ran a sort of a salon. I used to go down there before I was married the first time and got to know an awful lot of people, particularly literary types like—I didn't know them well—H. G. Wells and Sinclair Lewis and all those people. I had introduced them out here to Caltech people, and they got to be sort of members of a certain group of Caltech people. They gave a party, and Thomas Hunt Morgan was there and R. A. Millikan and a lot of other people. And the star of the evening was Thomas Hunt Morgan. You know, literary people are funny. Just for your information, I think the dullest lot of people that belong to the Athenaeum are the Huntington Library people. They are perfect examples of C. P. Snow's two cultures. But the astronomers and the people from Caltech are much broader than these literary people are, because they can talk a little bit about other people's business, but the others can't talk about anything else at all.

I lived for two years at the Athenaeum when it was first built, in 1930. And then my father died, and I moved back into the family home and lived there full-time with my mother until she died two years later. But Morgan and those fellows were very, very able people. I knew [George] Beadle slightly, but I didn't know him as well as I knew Morgan and his original associates.

ARTHUR L. KLEIN

SESSION 2

February 18, 1979

Begin Tape 2, Side 1

LYLE: Would you like to tell me how you got into working in the Guggenheim Laboratory?

KLEIN: Yes. As I indicated, I was a mechanical engineering student, and then I had some experience in machine shop as an apprentice machinist. I also was working in experimental physics in the Norman Bridge Laboratory of Physics, under Dr. R. A. Millikan. I was working on a problem which was a rather tough one—it took me four years to develop the apparatus. The apparatus itself was not terribly complex. I had received my doctor's degree in 1925 and I had no regular job. I was what is known as a research associate; that is, I was working there, but once in a while somebody would want a class taught or something, and I would get a temporary job.

Across the hall from me were two people who had a little influence on my career. One was Don [Donald H.] Loughridge, who got a PhD [1927] and was succeeded by Carl Anderson, who later became a Nobel laureate. Next door to him was Clark Millikan. Millikan was working on apparatus for the forthcoming wind tunnel, which he had already started on. This was in 1926. They were getting ready to start building the wind tunnel, and they were building a small airplane in some temporary buildings along San Pasqual Street on the old campus. They were always in mechanical difficulties, because all the members of the department at that time were theoretical types. Professor von Kármán was not yet here; he would shortly come in as a visiting professor and give us some lectures. I would go over and help Millikan and a man named Hicks who was a graduate student, and work on these things and try to straighten out their efforts. I continued to do this on the airplane, the apparatus for the wind tunnel, and on the wind tunnel itself. Came the summer, and everyone who was officially in the aeronautics department went on vacation. I was the only person who was around, and the mechanics on the airplane and the building people would come around and ask questions and I would answer them, seeing as there was nobody else to answer them. And my decisions seemed to stick and work. So as time went on, I became more and more involved in these things. Pretty soon I was building the

Guggenheim Laboratory wind tunnel and the airplane, and I wasn't even in the aeronautics department—I was still in physics. I guess the department decided they'd better regularize it. And so I was given an appointment in aeronautics, and I took over these jobs officially. As a consequence, I designed a good part of the wind tunnel, all of the mechanical features, the wind-tunnel rigging, the balances, and so forth. And the wind tunnel turned out to be extremely successful—extraordinarily successful, in fact—which was largely due to the fact that I was too lazy to make it as complicated as some of the wind tunnels were. I simplified the design; I added one or two unusual features—namely, water-cooled vanes for removing the heat from the wind tunnel. The wind tunnel had 700 horsepower, and the energy appears only as noise and heat. It is necessary to cool wind tunnels in order to operate them continuously, because otherwise they will overheat and ruin various pieces of machinery. I designed the rigging, and as we gathered more and more experience it was redesigned twice, ending up in 1941 with the system that is presently in Guggenheim Laboratory. This system seems to be fairly satisfactory, though modern standards would lead to a much simpler system.

LYLE: When you started working with Douglas Aircraft, or with this other company, did you do similar kinds of work?

KLEIN: No, I was working building airplanes. I'm a general-purpose engineer. I did all sorts of odds and ends. For instance, the flap gear on the DC-1-SBD—I hold a patent on the SBD dive brake flap system. I did the braking system on the DC-1, which was an attempt to extend the manual braking system to too heavy an airplane, which was not successful and was replaced on the DC-3 by a power braking system. I did miscellaneous things. As a matter of fact, the TWA rder for the DC-1 and -2 called for the Northrop type of wing construction, and I had been in that from the beginning with Jack Northrop.

LYLE: You liked him a lot, I read somewhere.

KLEIN: Well, I got along with him; he was a difficult person to get along with, very difficult. I designed the wing joint and one of very similar design was used by [John Leland "Lee"] Atwood on the DC-1, and continued and improved on the DC-2 and -3. That same type of system was used on many other airplanes.

LYLE: How did you like working at Douglas, say, compared to teaching and doing research at Caltech?

KLEIN: Well, they were equally satisfactory. In all the time I worked for Douglas, which was about forty years, I never worked for anybody of lower rank than a vice-president. I always was up in the front, in the center of things. I was not down being an ordinary consultant at all. In fact, I was sort of an integrated consultant.

LYLE: So what would you do? How did you work?

KLEIN: Well, in the first place, I'm a mechanic. I'm a good machine designer—everybody says that, Raymond says that, other people have said it in public. I redesigned the mechanical pulley brackets and stuff in the control system because I didn't like the way it was done. It was too cumbersome, too heavy, and too expensive in man hours, so I simplified it.

LYLE: So did you see the problems, or did somebody call you in?

KLEIN: Yes, I saw them. I was self-loading, you might say. I had the approach of a physicist to the problems. At that time, aeronautical engineering was in the process of a revolution. They were going from the old wooden wings, and welded steel tubing and wire and cloth-covered wings, to sheet metal. This was a case of understanding the radical distinction between these things. Consequently, I was in there, because I was educated as a physicist and was used to analyzing my problem even more deeply than they were. Most aeronautical engineers were designing by analogy, as most engineers do now. With the first sheet-metal hull that Douglas built, they had built a truss and then they covered it with sheet metal, and that was nonsense. After the prototype, they decided they'd just leave out all the diagonal members, which they did. And the hull was very satisfactory without them. But they were thinking in terms of bridge trusses, because the designers were all ex-civil engineers at that time. So, it was a revolutionary epoch. If you look at the production of airplanes in 1930, you will see biplanes, fixed external landing gear, exposed engines. And in 1940, the same manufacturers will be making cantilevered low-winged monoplanes, with retractable landing gear, with cowled engines, and a completely different, modern-looking airplane. In fact, there are only two things; many of them had nose wheels, and the others had tailskids. Now, I was mixed up in all of these changes—the nose wheel and the other changes.

LYLE: So you would suggest a change, after having analyzed it, and then they would go out and build it?

KLEIN: Oh no, you can't do that. You have to argue with people. Nobody's going to do what you tell them to do, especially if you're a consultant, because a consultant hasn't any authority. I was asked a couple of times whether I wanted more authority during the war, and I said I didn't, because I wasn't there [at Douglas] all the time. And if my suggestions didn't work, I didn't want them to waste time working on it, because I wasn't there to cancel it. But I was in a very strange position.

LYLE: Did you try to go in regularly?

KLEIN: Oh, yes, I went three times a week to Douglas, after '37. I went in and I'd visit all three Southern California plants every week, and especially the ones that were designing a new airplane. When they were designing new airplanes, I was there.

LYLE: And then at the same time you were teaching a class.

KLEIN: I was teaching here, and I was designing the Southern California Cooperative Wind Tunnel, too. I was real busy. Fortunately, I did my work almost entirely mentally, with very few pieces of paper, so I wasn't losing things all the time.

LYLE: When they were just beginning to really work on airplanes, in the early thirties, was that the most exciting time in aeronautics for you?

KLEIN: Well, I guess through the DC-1 was very interesting. Of course, I'd been through the Northrop Alpha already, and that was a stupendous success. In fact, when Jack Northrop was folded up by the United Aircraft & Transport in 1932, he had cash on hand for the building of more airplanes. He had deposits on more airplanes that had been ordered, and he had to refund the deposits, because United Aircraft & Transport put him out of business.

LYLE: How did they do that?

KLEIN: They had bought out the Northrop Aircraft Company, and he was operating as a subsidiary. He built an airplane which outperformed the Boeing airplane, and it was used by TWA to take the mail away from United Aircraft & Transport. UA was a unit of United Aircraft & Transport that included Boeing, Northrop, Pratt-Whitney, and a lot of other companies. And so it was within their power. It was their company; they could do anything they wanted with it.

LYLE: And they didn't want him?

KLEIN: They didn't want him. He was taking business away from Boeing. Now, in 1934, there was a new Air Mail Act passed, and this act changed things rather radically. It prevented this sort of thing from happening. Ever since then, an airline cannot have an interest in aircraft manufacturing. It was part of their consideration for the charters they received and the subsidies they were receiving at that time; they could not be a part of a manufacturing organization. Before that time, United Aircraft was a part of this big consolidation. And Eastern Airlines was controlled by General Motors; American Fokker was in some other things, and eventually became North American Aviation. But North American Aviation separated itself from Eastern Airlines on account of the Air Mail Act, too. So that was the thing, and when the DC-1 was ordered by TWA, they wanted the Northrop kind of construction. Now, I knew Raymond quite well—the man I worked for was a consultant for Jack Northrop, who was then running the Northrop division of Douglas, down at what is now LAX [Los Angeles International Airport]. Raymond said I'd better come over and help him with the structure of the DC-1, because I was familiar with the Northrop Alpha.

LYLE: Did they follow the Alpha design in setting up the DC-1?

KLEIN: In general, as it was suitable. I think what they meant was a metal airplane that was reasonably simple and well thought out, in which the parts were accessible and repairable. Because the only metal airplane they had was the Stout-Ford design, and that was a corrugated thing and was pretty difficult to repair, apparently. The Fokker had wooden wings, and they were just incredibly bad. You see, the older airplanes they had to overhaul every 1,000 hours. That means, roughly, less than a half year's use for an airline. With the sheet-metal airplanes, they started overhauling them every 1,000 hours and couldn't find anything to do, so they went to 2,000, then to 3,000, and finally got up to 5,000. Now, they have no regular overhaul period. They have what they call on-condition maintenance—when the airplane needs it, they fix it. They wear parts out taking them off and putting them back.

LYLE: When von Kármán set up the Jet Propulsion Laboratory, were you interested at all in getting into that?

KLEIN: Well, I was part of the committee that picked out the site. What happened was that we had them down here, and they were shooting off rockets in the building. And this caused corrosion of the electrical machinery—we had over 1,000 horsepower in electrical machinery down there in the sub-basement. So we said they had to shoot off on a terrace, which is where this building [Firestone Flight Sciences Laboratory] is now. So they started off there, and this caused a lot of smoke and big bangs. Then they were told that they had to move the rockets off the campus, because the neighbors and the administration didn't like it—R. A. Millikan's office was in Throop Hall, just across the alley from Guggenheim Laboratory. So we went around looking for a place which was government land, where there was no nearby population, so you could shoot something and not get any complaints. We found this place on the side of a hill that was reasonably accessible, and they started shooting rockets there. And that's where JPL is. And it's the worst site in the world.

LYLE: Why?

KLEIN: Well, it's like this [gesturing]. You get out of a building on the second floor and then you go in the first floor of the next building after walking up some more steps. It's built on the side of a hill, like a mining town. They've got 5,000 people up there, living in a place where circulation is extremely difficult.

LYLE: Did you work there at all?

KLEIN: Yes, I worked there later on *Mariner 4*. The other thing I did was helping Douglas on a different organization of the engineering departments. I didn't like the way it worked. I pointed out that they had specialists, but they weren't using them properly. Most of the aircraft engineering departments had the idea that an engineer is an engineer and he could do anything. Well, this isn't true. Some of them had had experience. Generally, experience means that they've had troubles and they've learned not to have those troubles anymore. So I said, "You've got to set up specialists; you've got to have people that know how to put an engine in an airplane, and other people who know how to put the landing gear on an airplane," and other people who were structural designers, and so forth. So eventually they ended up with what is now very popular, called a matrix organization, which is really a three-way organization. I copied it, more or less, after the military. In a military organization, for instance, there's a chief engineering officer, but technically he's responsible to the chief of engineers, who is in Washington or wherever the headquarters is. But he's also responsible to the commander of the division for doing the jobs that need doing—and for the way he does them, he's responsible to his technical board. Well, in a factory, you have to split the chief engineer's job three ways. He is the chief technical engineer. He's also the chief administrative engineer—that is, he hires and fires, either directly or by delegation, provides library facilities, orders things, and disciplines when that's necessary. And then the third thing—he's dealing with the customers to design things that they want, because the airplanes are capital goods.

You know the difference between capital goods and consumer goods? There's a technical difference. Consumer goods is the stuff you buy in a grocery store that's bought in stock; you buy it off the shelf. Capital goods is something that's made to order, like a big power plant. You don't buy them off a shelf; you tell them you want a power plant to do such and such. You get somebody who knows power plants, and they design the whole thing, usually—the buildings and fuel, handling facilities, and everything. Aircraft are capital goods—except the small ones, the little private airplanes; I was never interested in that class of airplane. But the bigger airplanes are capital goods, either for the government or the airlines. They are generally designed more or less as a tailor-made product; they are modified slightly for each customer's demand. Some of the customers of the DC-10, for instance, have the galley in the basement. That is where the American operators wanted it, and European operators wanted it upstairs, because in the winter they live on their air freight; they don't have any passengers. They'd rather have a bigger cargo hold than a bigger passenger compartment. So you have the same basic airplane, but it's been really put together differently for the different customers.

So the chief engineer has relationships with customers. The Douglas organization is typical—people have different names for these jobs. They have the chief designer, who has the engineering aspect of the chief engineer. And they have the executive engineer, who's the customer aspect of the chief engineer. And an administrative engineer who takes care of all the housework. Of course, we're talking about large engineering departments, up to 4,000 people.

LYLE: In the training here at Caltech, when you go into engineering, do you learn those three capacities?

KLEIN: No, we learn only the professional capacity. Well, we had a man here from Douglas giving a lecture on the financial decisions on a DC-10. He's a man I've known for many years; he'd been an engineer in one of the plants, a really bright guy. He said they were forecasting financially; they gathered up all this information from around the plant and they forecasted that engineering would not have enough to do, that there'd be enough cash flow, that the airplane was needed, and then he went off and he gave us the same lecture he gave to the board of directors from the McDonnell-Douglas Corporation about the DC-10. When he got through, some of the students asked him how he got the people—"Do you get engineers or do you get business people?" He said, "Well, we find it takes about twice as long to take a business type and train him in aircraft as it takes to take an aeronautical engineer and train him as a financial man." So mostly these people came through engineering. In general, strictly commercial executives have not been very successful in aircraft. They don't understand the full complexities of a problem in the way that technical people do.

LYLE: Have most of the aeronautical engineers been coming from Caltech, or where are they coming from?

KLEIN: All over. Caltech couldn't possibly supply the need.

LYLE: Even in the thirties, was that true?

KLEIN: Well, in the thirties, we were much smaller. When I first worked for Douglas, they had 300 employees in engineering. We produced [at Caltech] ten students a year, something like that. At that time, we were producing three in the first group. One of them [Ernest Sechler] is in this office with me. But people were educated all over, and a lot of them were educated primarily in other industries. Most of the stress men were civil engineers. This was one of my troubles, because they didn't understand sheet-metal construction. I don't fit into any ordinary category, unfortunately, and consequently it's hard for me to describe myself to you. Engineering is a very broad field. My boss told somebody—and I heard about it—he said, "Well, Maj [Klein] is sort of a philosopher of engineering; he tells me what he thinks about the way we should be going." [Laughter]

LYLE: You mentioned you were called "Maj."

KLEIN: That came from the fact that I was a cadet major when I was an undergraduate. We had a cadet corps here then—this was just before and after the First World War. I entered when I was sixteen, you see, and I graduated in '21. So the war period was in the middle.

LYLE: Was the training for entering the service after graduation?

KLEIN: Well, I went to one camp after I graduated and got my commission as a second lieutenant in the reserve, in the Corps of Engineers for the army. And I kept it up for a little while, but when I got into aeronautics I realized that I was in a national industry that was closely related to [national defense] and I decided I shouldn't be a reservist. Now, a lot of them were.

The British discovered at the beginning of World War II that they had everybody in the aircraft industry as a reservist. They all got called up, and the industry stopped dead. Then they had to throw them out again and tell them to go back to their jobs making airplanes. So the U.S. Army had the same problem, but it never called up anybody that worked in aircraft. I came across a document yesterday, which was about a hearing about my being re-evaluated by the Selective Service. I was never called up, of course.

LYLE: I had another question to ask you about the early days here at Caltech. What was it like to live in the dormitory? Did you make very many friends there?

KLEIN: Well, let me tell you something about myself. I am not a person who makes friends easily, unfortunately. I didn't have any real friends until I got into graduate school.

LYLE: Was that partly because in graduate school you had stronger interests, so you made friends through that?

KLEIN: Well, I'd always been an omnivorous reader, and I spent most of my time reading. And I found most of the things that the other boys were doing were not interesting. When I was in the living room of the old faculty club, there were a lot of faculty members there, and I found them more congenial than most of the people in the dormitory. I was, I think, more sophisticated than most of my fellow students. Not in a social sense. Well, I'm a very queer person. I do silly things—people think they're silly. I read all twelve volumes of *The Golden Bough*, for instance, which turned out to be very valuable, because I have never been put off by the size of a book since then. I spent a great deal of my time reading all sorts of periodicals, and it turned out that this was very valuable, because I was a person who knew what was going on someplace else.

LYLE: And you didn't really share that interest with other students when you were in the dormitory?

KLEIN: No. There were very few people who were readers, I discovered. I had one roommate, who was made a professor here, who would read anything I brought in, but he'd never bring any book of his own. He was sort of a passive reader.

LYLE: It sounds like your interest in reading is one of your stronger interests.

KLEIN: Reading is just one way of saying I have an intellectual interest in all sorts of things.

LYLE: Do you still read a lot?

KLEIN: Oh, yes. Well, I don't read as many periodicals as I used to. I read about forty periodicals.

LYLE: I wanted to ask you what people you've known in your life whom you've admired particularly.

KLEIN: Well, I admired R. A. Millikan for several things—and some other things I didn't admire him for. He was an utterly fair person; he was completely fair. Everybody got a square deal, no matter who they were, if they deserved it. He also never turned anybody down who came asking him for help. He always saw that they got as much help as there was available. I was asked this question by a friend of mine—a vice-president of Boeing—a few years ago. The question was, "Why does Caltech have such a strong influence on industry, while the University of Washington doesn't?" So I thought about it for a long time, and finally I told his local representative, who brought this message to me, what I thought. And then I met my friend in New York at an annual meeting of a technical society, and we talked about it some more. But essentially it was the fact that Millikan would help anybody or would direct them to somebody who could help them, and he never turned anybody down. People with the ivory-tower attitude didn't do well here. It was sort of an open-door place, if you know what I mean.

LYLE: Why did that make Caltech have more influence in industry?

KLEIN: Because a man [from industry] comes over, he's in trouble, he wants to get some help, you don't say, "Well, I can't be bothered." This was the attitude that a lot of institutions had.

Caltech is not like that. Sometimes you get very annoyed with a lot of crazy inventors—we used to be pestered by them, but we didn't throw them out.

LYLE: This would be inventors who would come here with….

KLEIN: Yes, with crazy inventions.

LYLE: And they wanted Caltech to check it out?

KLEIN: Yes.

LYLE: That was one thing you liked about Dr. Millikan.

KLEIN: Yes, that's what I liked about him. What I didn't like about him was—well, his son Clark once said, "My father's a minister's son, but I'm not." And that was the thing I didn't particularly like about him. He was sort of a holy man, in a sense. And I'm an agnostic, so I didn't like that particularly. But I never had any discussions with him or anything. I got bawled out by him once or twice, but I had it coming to me. Then R. C. Tolman I admired very much.

LYLE: Can you think of any particular incident or reason why you did?

KLEIN: Oh, I don't know. He had a real nice, healthy attitude towards people. I'm not much of a hero worshiper. I admired Donald Douglas very much; but Donald Douglas made some very serious mistakes.

LYLE: Like what?

KLEIN: Well, the financing for the DC-8, which was the one that wrecked the company. He should not have done it the way he did it. The company was extraordinarily prosperous, and he tried to build it on a cash flow, and other companies moved in; the jets came out and the cash flow stopped, *bang!* and there wasn't any more.

I admired von Kármán very much. Von Kármán was really a great man. I liked his originality and his approaches and his methods—the fact that he was never impeded by the fact that the methods he worked with were not perfect. He could always get results that fit and do something useful. He was very adventurous; he worked in all sorts of fields. He was a very nice fellow to work with. In the thirties, before I was on half-time, I was responsible for building apparatus in the laboratory and running the shop and things like that. He used to try to stop me by asking me questions—"How are you going to do this, how are you going to do that?" Just out of the cold. And I could usually tell him how I would do this.

Begin Tape 2, Side 2

KLEIN: Well, I can't go on; I know too many people. I've never known anybody I thought was perfect, and I know damn well I'm not.

LYLE: What about in the 1930s, with the Depression? Were you aware of people who were interested in Communism or Marxism, because of the Depression?

KLEIN: Oh, yes. Sure, I was aware of it. In fact, I met a couple of them. I thought they were rather silly. I happened to know an awful lot of authors at that time. My half-niece was married to a man who was rather prominent in the literary world, Lewis Browne. They lived in Santa Monica and they had sort of soirées, and I met [Wilhelm] Furtwängler, and all the German authors of the time, and Aldous Huxley, H. G. Wells, Sinclair Lewis—all these people.

LYLE: It must have been exciting.

KLEIN: They were ordinary people, mostly. Charlie Chaplin I met. He was actually fairly stupid; he'd make stupid remarks. He was undoubtedly a genius, I don't want to deny that. But a lot of people hung on his words, and the words didn't make very much sense.

LYLE: So there was a discussion of politics at these parties.

KLEIN: Oh, I remember once he made a remark. He was very indignant about the way "these bastards" were restricting our liberties and not allowing us to say what we wanted, and "By God, we've got to hang them!" Here he was objecting to censorship, and he was willing to go to violent measures to shut them up, and that didn't make any sense at all. There were a lot of these literary people who were not too broad—you'd be surprised. Some of them are very narrow-minded and uninformed in very large areas. H. G. Wells was very shy; you could hardly get a word out of him if there were more than three people in the room. I spoke to my niece about it one time, and she said, "Well, he's very shy. He talks to me and my husband when we're alone."

LYLE: Another time when there may have been political problems was in the fifties, when they were having the McCarthy hearings. Were you in any way involved in those?

KLEIN: Oh, no, not at all. No, I was always too busy to be involved in that stuff. I didn't pay too much attention to it. We didn't have much of it here on the campus. We never had a campus problem of any magnitude.

LYLE: And at Douglas there was never any problem, either?

KLEIN: No. Well, there isn't there, because, you see, they were doing a lot of military business, and a lot of work was classified. If you got anybody that was questionable, the military wouldn't pass them, so you couldn't hire them. Consequently, we didn't have those people around. You didn't have the agitators around; you didn't see them.

LYLE: After the war, Caltech grew a lot. Was that true at Douglas, too?

KLEIN: Well, yes. There was a big growth, and the reason for it was rather obvious. During the war, most sources of goods, particularly luxury goods, supplied to the public were cut down. For instance, you couldn't buy an automobile; you couldn't buy a new radio; you couldn't buy lots of other things. Consequently, people had a lot of money, in spite of the high taxes; they couldn't spend it and they'd put it in government bonds. There was this great pent-up purchasing power, and when the war was over there was a splurge, and this caused a business boom. Now, with the first war, the other thing happened. They had a collapse, and they had a sort of panic after the first war. But the second war produced a boom. We did not have our manufacturing plants damaged in the way they did in Europe, where you couldn't supply things. That was their problem, while our plants were undamaged and very productive. There were an awful lot of people who had gotten used to working and wanted to continue to keep getting income.

LYLE: Did Douglas get money like Caltech did, to do research, from the government?

KLEIN: Well, yes. But also the aircraft business was going great guns during the war. And at the end of the war, the new airplanes—the DC-4s—supplanted the DC-3, and then the DC-6, which was a supercharged airplane, replaced the DC-4. The other manufacturers did the same, and an enormous amount of money was spent in travel that hadn't gone into it before. Another thing happened in Southern California; a lot of people had come through here from the Middle West and the South on their way to the Pacific. Many of them, and I heard this from quite a few people, decided they'd rather live in California than where they'd been living. So the California population boomed, and that set off a boom in building, and then the whole rest of the economy took off.

LYLE: Would you describe the changes you saw in the aeronautics department at Caltech after the war?

KLEIN: The big thing was that we had all sorts of what we called GI students. These were people who had been discharged from the military and they had the GI Bill—the government paid for their education. Our classes, which had been running an average of about twenty students, the way they are now, suddenly had seventy to eighty people in them. These people were the best students we ever had, because we got aircraft people who had been involved in airplanes and taken care of them in the military, and they knew something about them. They were prepared to work hard and really learn how things were going. We had a big boom—in this department, at least, and I think the other departments had, too. It was very refreshing. I enjoyed teaching those classes. I enjoyed everything except correcting seventy-five term papers instead of about eighteen or twenty. And then we seemed to have acquired, although we had it in a small way before the war, an international reputation in aeronautics, and we got all sorts of foreign students. I think we have more foreign students than American students now.

LYLE: Have you gotten to know the students very well, and are you aware of student problems?

KLEIN: No, I'm not really. Before I was married for the first time in '34, I got to know the students pretty well.

LYLE: Before you were married? Just because there was more time?

KLEIN: More time. And then during the war you're too damn busy. You're physically exhausted all the time and frequently you just want to go to sleep on Sunday, which was your only day off.

LYLE: OK, so you did get to know the students, around say 1932, '33, '34. What was it like? Were they happy about being here at school?

KLEIN: Oh, I think so. We had four naval officers and a marine officer who were all friends of mine and my family, and when I got married they gave me a very nice little wedding present. One of them became a four-star admiral later.

LYLE: These were students at Caltech?

KLEIN: These were officer students, yes. This was before the foundation of the Naval Postgraduate School up at Del Monte, and we had about three to six naval officers a year as graduate students. And the first year, in this particular group, there were five officers. They all became admirals—or what would it be for the marines, major general?—except two men: One was killed in an airplane accident, and one was lost at Corregidor, and they don't know what happened to him.

We continued to get a lot of military students for quite a long time. The navy at that time had the policy of sending these officer students. After a year in a graduate course to refresh their mathematics and other things, they'd send them here for a year, and they'd get an advanced degree. Not a doctor's; it was an engineering degree. These were generally very successful officers.

LYLE: And were they successful students?

KLEIN: Oh, yes, they did well. It was pretty important that they do well, and it would have been very bad on their record not to have done well. Early on, we had one or two who were not really up to it, but after the shakedown period we never got a really bad one.

LYLE: What do you mean, the shakedown period?

KLEIN: Well, after a year or two, as soon as the man who was running the courses back there at Annapolis discovered what our requirements were, he never sent us anybody who was a scholastic problem.

LYLE: Did you notice any changes that [Lee A.] DuBridge [Caltech president 1946-1968] implemented that you thought were particularly good, or disastrous, as the case may be?

KLEIN: Well, you realize that in '37 I went on half-time, and at that time I withdrew from all my faculty committee work. And I didn't see faculty members very much after that, except the people here in this laboratory. After that, I only served on one committee and that was patents. At Douglas, I had a job that involved patents, in a way; I was the editor of all technical papers for Douglas. It wasn't as hard as you might think; I wasn't going to edit them grammatically. I'd call up the man's boss and tell him, "For God's sake, get a professional writer to straighten this fellow out." We had a lot of writers around. Then, if I were uncertain about the technical part of it, why, I'd bounce it back to his boss. Once in a while, somebody would try to bypass his boss. I wouldn't stand for that; that's very bad practice. He wouldn't think that his boss would approve his writing a paper, so he'd write the paper and try to send it to me directly. Anybody who does that is a fool, in an organization, because he's going to get caught up on it sooner or later, and it doesn't do his reputation any good with his boss; and his boss is controlling his future to a certain extent.

I was on the engineering course committee off and on, up to the time I went on half-time. We approved or disapproved of the course arrangements in the various departments and recommended to the faculty as a whole whether to approve or disapprove of them when a department wanted to teach a course in something or other. We might say, "Well, you're infringing on somebody's field in talking on this subject, and you'd better get the consent of these other people so they're happy about it." Then, of course, the committee would act, and they might or might not approve it. If they approved it, it would go on the agenda for a faculty meeting. This was the normal procedure. Then the Faculty Board might act on it, and then it would go to a vote of the faculty. Sometimes you'd get things that were contrary to institute policy. For instance, the Humanities Division always wanted to expand at the expense of the technical departments, and this is a technical school. They wanted to omit technical courses in the curriculum, like elementary physics or chemistry or mathematics, for undergraduates, because if they're going to get a degree in economics or liberal arts they don't need it. But after all, this is a technical school, and the people who come through here in whatever field should have a good rudimentary technical background. Well, this sort of thing happens all the time. It's just a difference of point of view.

ARTHUR L. KLEIN

SESSION 3

February 21, 1979

Begin Tape 3, Side 1

LYLE: You mentioned that you were involved with IBM and the computing business in about 1938, 1940. Could you describe that?

KLEIN: Well, I had gotten involved in the computer business because of a very unsatisfactory situation in the recording of wind-tunnel data. The data came in in massive quantities, and it was mostly hand-inscribed, and frequently there were one or more errors on each page. In order to eliminate the errors in transcription of the data, we were spending roughly double the amount of hours that should have been required to put this data in a form where you could plot the conventional aerodynamic quantities. In looking forward, I tried to think of all the possible computing systems that would fulfill several needs, such as eliminating the human error in transcription of the data, which was probably the most important; and also to have the data in a mechanical form that could be reproduced without introducing further errors—or, at least, to minimize these errors. It's well known that errors in numbers occur on the order of one-half of one percent to one-tenth of one percent of the numbers inscribed by unskilled personnel. These figures do not seem to be bettered by any easy means. There are a few accountants and other professional number handlers that have improved their skills, but they still have many errors in their work. Needless to say, the entire double-entry bookkeeping system has been introduced just to take care of this problem. It was introduced in the Middle Ages, incidentally.

With this idea clearly stated, I investigated all the possible things I could think of over a period of years and finally came to the conclusion that the only ones that seemed reasonably well implemented as far as industry is concerned were the punch-card data systems, used for business applications almost entirely at that time. I, therefore, investigated both the Remington-Rand card system and their machinery and the IBM system and their machinery. I discovered that the IBM system was much more flexible than the Remington-Rand system. Therefore, after some discussion with various people, I decided we had to use the IBM system, if any. The system was developed for the Bureau of the Census. Mr. [Herman] Hollerith built the first equipment for the

1890 census. The IBM card and the Rem-Rand card are physically identical; I mean, the same size card can be fed into either machine, but they can't be interpreted by the [other company's] machine.

LYLE: In 1939, what kind of businesses used them?

KLEIN: Oh, all of the larger businesses used them for bookkeeping and stock-keeping—general accounting. Practically every large company used a punch card as a pay card for attendance and things of this sort. Here in the institute, the first computing group was in the business end. IBM machines were a fairly complete line. They had, in addition to the tabulator, a printer that prints a line clear across the page simultaneously and a whole series of other machines—sorters, interpreters, summary punchers—that are familiar to most accountants. On a little further investigation, I found that most companies even had multiplying machines, which are roughly equivalent to desk calculators. Again, the IBM machine was the more flexible. They had a machine called the 601 that could do simple multiplication what we thought was very rapidly—two numbers in about fifteen seconds.

LYLE: How did you find out about what they could do? Did you go to their offices?

KLEIN: I went down and visited them. I visited the various establishments and talked to the senior people in them. We had an entry, through some mutual friends, into the IBM organization, which should not be thought of as being very much like it is now—this was in the late thirties. Their engineering department was very small, and though they had done some very nice things technically, they were not the massive research and manufacturing organization that they are now.

The Remington-Rand multiplier was a very interesting piece of machinery because it employed a mechanical multiplication table. It actually multiplied two numbers and summed the partial products, more or less like a person does it by hand. This investigation of mine proceeded in a very dilatory manner, because of the fact that I had no direct application for it at that time. But then when the Southern California Cooperative Wind Tunnel got started as a serious project, and I was put in charge of instrumentation and such things, I decided we just had to go to mechanical data handling. At this time, we approached IBM about it and proposed to them that we use their machinery to record the data and later to do the computations involved. The computations on wind-tunnel data are not complex. They consist generally of addition, subtraction, and multiplication of a large group of numbers by more or less common constants. Consequently, they are not nearly as complicated as many of the mathematical problems we think about today. As the design of the Cooperative Wind Tunnel and its sister tunnel for Curtiss-Wright in Buffalo proceeded—we designed both wind tunnels—we came out with a need for six channels of forces and moments; two channels for the angles, the angle of yaw and the angle of pitch; and one for the dynamic pressure, which is the measure of air speed. All together, nine channels. The forces were measured to four digits and the angles, due to the very wide range of angles, to five digits. IBM agreed to make this machinery and to cause their digitizers to read out in such a manner that the data could be recorded in their tabulator and summary punch in the normal way on IBM cards—and also to supply us a number of key stations with pushbuttons so we could put in things that were not in the digitized data. The IBM card has eighty columns, and the data, if you count it up, took about half of the IBM card. The remainder of it could be used for other inputs and also for labeling the test, the date, the number of the points, and other bookkeeping information necessary.

We were happy to get this equipment in running order in 1943. There was a great deal of engineering involved, in both the tunnel itself and this other thing. We set up a group of our own to carry on our end of the work and rented the necessary IBM equipment, including two of the 601 multipliers. The equipment was extremely successful. We were able to gather a lot of data with this equipment, and our error rate was cut way down—just what I was trying to do.

Using the 601 multipliers, we were able to cut our data-calculation time a great deal. At a later time, we got an Electrodata computer to do the data reduction and a plotter to plot the aerodynamic curves, thereby enabling us to turn out finished data in a relatively short time. It's interesting to note that with the development of the computer—the first commercial, sophisticated computer being available in 1950—that the data in most such installations nowadays is recorded directly in memory in the computer, and the whole process goes through, and the curves are generally out within a minute or two from the end of the run. It used to take a week.

LYLE: Was there any resistance to switching to the computers? Did everyone who was working on these wind tunnels want to switch to this kind of machinery?

KLEIN: Well, our system was a very complex and expensive one, because it was a pioneer system, and it wouldn't be justified in a small wind tunnel. You couldn't afford the cost of all this stuff. In this wind tunnel, which was designed as a testing wind tunnel for aircraft companies, the attempt was to do the instrumentation very well and to permit very efficient operations. We achieved the latter aim, as shown by subsequent comparisons of our wind tunnel with other wind tunnels doing the same type of work. The numerous human-factor improvements we made in this wind tunnel permitted us to set up models outside the working section while something else was being tested and do other things of equivalent type. None of these were cheap.

LYLE: On this Cooperative Wind Tunnel, what companies were involved?

KLEIN: Douglas Aircraft, Lockheed, Convair, McDonnell, and North American.

LYLE: They would all use this for experiments and testing?

KLEIN: Yes. Well, they paid for the wind tunnel and retained Caltech to design the wind tunnel. And Curtiss-Wright was going to build a wind tunnel in Buffalo, New York, and agreed to pay half of the engineering cost of that wind tunnel—it was about a million dollars.

LYLE: So did the airline companies pay for a certain amount of time on this?

KLEIN: No. Well, the companies set up an engineering committee to handle that, and agreed on the charges. The charges for wind tunnel use, as in the case of the GALCIT [Guggenheim Aeronautical Laboratory at the California Institute of Technology] wind tunnel, which was on the campus and owned by Caltech, were for tunnel occupancy at so much an hour, for electric power used, and a certain overhead charge per hour was added to take care of repairs—because all wind tunnels have accidents, which can be extremely expensive. One accident we had at the

Co-op cost something over a million dollars.

LYLE: What kind of an accident was it?

KLEIN: Well, the wind tunnel fan came apart, and pieces of blade flew all around. We designed it to contain them, so they didn't get outside of the wind tunnel, but we had to take the motors out and repair them, repair the wind tunnel, get new propellers made, new fans made. So it was an expensive thing. Now, we have the same sort of thing on a much smaller scale in our old wind tunnel. So you have to be prepared, meaning you have to have an insurance policy.

Well, this operation gave me pretty close connections with the senior people in IBM, and I followed actively the use of their machines in the engineering aspects of airplane design. The result was that when they introduced their 701, in 1950, I was on a three-man committee to recommend for or against the buying of these machines for Douglas Aircraft. The committee recommended strongly to do it, because by this time the Caltech multiplying equipment and other things were in pretty high use for them. IBM had in the meantime produced a good electronic multiplier, and the people at Northrop had combined it with some other equipment to make what was known as a card-program calculator. This was a tabulator, summary punch, some additional memory, and a 604 multiplier. In this machine, which was a predecessor of the computer proper, two kinds of cards were put in the card feed. One was an instruction card, which told the machine what to do with the next card that came along—hence its name. And these were used with great success for many jobs in aircraft engineering, such as stress analysis, dynamic analysis for flutter, and other similar jobs, where quantities of mathematics—mostly matrix calculus—were involved. These led naturally into the usage of a real computer. And the 701, being a von Neumann-type machine, was very useful to the aircraft companies. Some twenty-one or twenty-two of these machines were built, of which about fourteen were in Southern California. They were for quite a while the mainstay of the aircraft industry. Later these were, of course, replaced by more advanced machines, and I was in a general sort of advisory capacity on all these problems. In the meantime, I had been chairman of a committee in 1946 for Rand, to try to find out what they should buy, and we found that in '46 there was no commercially available machine they could buy. There were experimental machines being operated successfully by Professor Howard Aiken at Harvard. Bell Telephone Laboratories, under Dr. [George R.] Stibitz, had built some relay calculators which were used for telephone calculations. But the commercial people—namely, IBM, Univac, and the others—had still not produced a saleable machine at that time. Rand made what I thought was the very misguided effort to build a machine of their own. When I went to Bikini at that time, and wasn't around, they spent about a million dollars trying to build a machine, and they didn't have the capabilities. They had a lot of people with a gleam in their eye, but they didn't have the engineering skills to put this thing together, and most of the components weren't in existence.

LYLE: Are there any striking differences in character about, say, Douglas compared to North American compared to Rand?

KLEIN: Well, Rand was founded by Douglas and was a Douglas organization for the first two years of its existence. When it was part of Douglas, I worked down there, and I was working for the same man—namely, the vice-president of engineering in both places—so it was no different. I knew the senior people. Different problems, but the same people. I had no other experience with large aircraft companies. I had this experience of working with IBM, but not for them. I got a pretty good knowledge of how to deal with companies—what to expect and what sort of key questions to ask them.

LYLE: Like what?

KLEIN: Well, I asked one outfit what the patent situation was. Because if they're violating somebody's patents, and they sell you the machine, you're liable, too. I asked this outfit two things, and they didn't give me a satisfactory answer to either one of them, so I couldn't recommend doing business with them.

LYLE: What was the other question?

KLEIN: The other one was, I'd like to see their bank statement, because you don't want to deal with a company that's going bankrupt. This has happened too many times. I heard a case of this just the other day, that Hughes had to subsidize an outfit that was going bankrupt. This happened to Douglas a couple of times. They had people making things for them who were going broke, and they had to bail them out or not build the airplane—one or the other. So, if you're active as part of management and you're there as the highest representative, no matter how low you are in the company, then you've got to look at it as a company matter and ask the questions you know the people will ask you when you get back: Are they reliable? Are we going to end up in a mess of patent suits if we do this? People like to know; they're entitled to this information. You can't operate a big organization without being aware of these things.

So I kept up with this and encouraged the growth of the computers in the factory and promoted quick design methods based on computer technology, which after many years of effort has produced remarkable results in structure analysis and structural design. At the same time, I was doing many other things.

LYLE: You mentioned the aircraft-control work you did. Would you like to discuss that?

KLEIN: Yes. When I got into the aircraft industry, the aircraft-control system was made of cables, pulleys, and miscellaneous hardware—much of which were army-navy standard parts. These were all listed in the army-navy standard book and could be used with the rating shown without any questions. At this time, I was active in general design at Douglas, and Douglas secured contracts for two very large airplanes. The largest airplane that Douglas had built up to that time was a flying boat weighing about 30,000 pounds. They secured a contract from the airlines to build the DC-4, an airplane that weighed 60,000 pounds, and from the air force a contract for the B-19, which ended up weighing 160,000 pounds. To an aeronautical engineer who views the entire problem, it was pretty obvious that one of the things that wasn't going to enlarge very easily was the manual control system used on small airplanes, where the pilot moves the stick and that moves the surfaces out on the wing and at the back end of the airplane. Either improved surfaces must be designed, or improved systems built, or some method found for adding power to the system to make the airplane, as far as the pilot was concerned, a satisfactory airplane and a safe one and yet enable him to have as much control as necessary to maneuver the airplane. Now, a little thought will indicate that as you take a geometrically enlarged airplane, the hinge moment, which is the effort necessary to move the control surface, will go up not as the first power but as a higher power of the size, generally about the third power. In other words, if you double the size, the forces required to move it are going to go up by a factor of eight. Therefore, it was necessary to design these surfaces, and the system for operating them, with much more care and refinement than had been done heretofore. As a result, a few of us started in on a project to investigate the elements needed in the design surface, and the project divided itself into two separate pieces. One was: What is the optimum shape for the control surface itself so that the aerodynamic balance can be achieved—so that the hinge moment [pilot's effort] of the surface is minimal? And secondly: How can the pilot's effort be conserved and used to the maximum advantage in designing airplanes? The aerodynamic part of the problem was turned over to a very able aerodynamicist named Gene L. E. Root, and he concocted an elaborate set of wind-tunnel tests on control surfaces to be performed in the GALCIT tunnel. At the same time, the rest of us made a detailed investigation of all the control-system parts that occurred in the airplane, to find out what was good and what was bad. Our first conclusion was—after some very elementary testing—that nobody since the Wright brothers had even thought about it. Because it was obvious on a simple inspection that many practices were wrong. We investigated the optimum pulley size, the optimum cable size, the effect of fairleads—fairleads are the things that support the cable, and there's not a pulley when the angle's small—friction in pin ends and in ball-bearing ends, and so forth. And we came out with a series of rules and design charts that forced a complete redesign of the control system. We discovered that under no circumstances should you use roller chains—bicycle chains—because they have very high friction and they wear badly, and as they wear they get worse. We discovered you should not use small pulleys at all, that you should use the largest permissible size pulley and as few of them as possible. We discovered that all cable terminals that rotate, such as those on levers, should have ball bearings. We measured the control effort on many airplanes, including the DC-3, and discovered that the mechanical efficiency of the system was very low, on the order of forty or fifty percent, and that the thermal elongation of the system was not adequately taken care of. We discovered that the army-navy standard on the location of control-surface stops made no sense whatever and something else should be done.

LYLE: So all of this was a response to the problem of increasing the size of the airplane?

KLEIN: Yes. As a result of this, we came out with an entirely new set of components and rules for the design of the control system. We introduced ball-bearing cable terminals, using the standard ball bearings that were already aircraft standard for other things, and many other practices. They resulted in our control system being ninety-three to ninety-five percent efficient for the rudder and elevator, except in the case of the ailerons, where it was generally between eighty-five and eighty-eight percent. This is because there are two ailerons and there's more complicated cabling for them; it goes more places and consequently has more friction. We investigated the effect of temperature and of rigging loads, to avoid slack cables in cold weather and prevent excessive friction. We discovered that in large airplanes you need large cable travel and small cables to come out with a minimum weight and satisfactory rigidity and efficiency. After two or three years of development testing, our control systems achieved these things, and we had no further problems with control systems. And in fact the B-19—where in addition to a high-efficiency system we had power boosts; this was subsequently disconnected because it was unnecessary—had the lowest control forces of any airplane we ever built; this is the 160,000-pounder. So we'd overcome it by technology. I want to emphasize that neither the mechanical nor the aerodynamic approach alone would have solved it. It had to be both.

LYLE: This was about what year? Before the war?

KLEIN: Oh, yes, this was in the middle thirties. From that date forth, Douglas was able to design control surfaces for airplanes which required very little change. The usual things that are noticed in prototypes—that their tail surfaces don't look like the production article—were not true of Douglas airplanes after the DC-3. This was purely because we had achieved a background of knowledge. We weren't following a lot of rules that didn't make any sense. You see, they'd been designing airplanes like the Wright brothers' and they just built them, and they'd build some more like that. And if the loads were bigger, they'd put in bigger cables, and then they got more friction.

I often had to make decisions about overengineering. I did this in the aircraft business, and I did it in the wind-tunnel design. I look at some of my early designs and wonder what I could have been thinking of when I did them, because they're awkward and gadgety and so forth. It's awfully hard for a young designer to feel that adding complexity to a part is not necessarily beneficial. And frequently the best thing you can do is to leave the part out altogether. This, of course, is unrewarding. It doesn't look like an achievement. Well, I designed things that were failures that were not my fault, like the braking system on the DC-2. It was an attempt to extend manual braking to too heavy an airplane. And by taking advantage of all the things you had, you still couldn't do a good braking system using your arms and feet. They had to use power as airplanes got heavier. Also, I did a control column for a prototype airplane which was much too complex; it had no advantages and destabilized the airplane longitudinally. It had to be counterweighted to keep it from destabilizing the airplane. Things like that. They were valuable lessons, that's all I can say.

Begin Tape 3, Side 2

KLEIN: One problem we had to deal with was maneuverability. The English thought they could get the greatest maneuverability by building an unstable airplane, which is easy to do, and then let the pilot correct its instability, and he'd have great maneuverability—provided the instability was not so great that the airplane would take the controls away from the pilot, like a horse running away with its rider. We were able to achieve very low control forces by being very careful with our aerodynamics and our mechanical design. We had eliminated all unnecessary friction and had used a control system that had all of the optimum characteristics it could. The control surfaces are limited by certain factors I have not mentioned. One is icing on the leading edge of the surfaces. You cannot use a large aerodynamic balance in icy conditions, because ice will build up on it and overbalance the surfaces and may take the controls away from the pilot. So the leading edge of the surface must be masked all the time. This is particularly true of transport airplanes, which in their long flights almost always run into bad weather someplace. I just give this as an example. There are also hailstorms and insects and all sorts of other problems that come up that necessitate doing things you would not otherwise have to do in airplanes. Like put little access doors in the windshield, because occasionally an airplane will run into a hailstorm and the windshield will be so fractured by the hail that you can't see through it. The pilot needs a little view forward; they are generally happy if it's three or four inches in diameter, if they can just peer out and see where they're going. The same thing happens if he happens to run into locusts or grasshoppers or something and you get massive insects on the windshield that you can't get off in flight. These things cause deviations from what you'd say was perfect streamlining or ideal design. It's necessary for safety; they're safety features, and there are many of them.

LYLE: But basically this plane you designed was very stable.

KLEIN: Well, you could let go of the controls and it would go back to flying straight ahead. Another thing that had to be determined over a period of years—the rules now are pretty good—the pilot wants a set of controls which to him feels as if he's making the same amount of effort, fore and aft, sideways, and with his feet. As your feet are used to boosting you upstairs, you can push 300 pounds with your feet; even a small woman can do it without even noticing it. The feet are much less sensitive than the hands, and you can easily make the rudder forces too low for the feet in comparison with the aileron and the elevator forces. So you have to adjust these forces so they seem right to the pilot. Now, this is a perfectly arbitrary thing, and it's what is called the human-factor problem. We were unable to solve it for quite a while, because the older pilots were so used to flying what we thought were bad airplanes. They were overly sensitive. I'll give you an example. When Harold Adams, who was a chief designer, designed the first power brake valve, which is like a power brake on an automobile only it's more powerful than that, he went out and tested all the automobiles he could find. He found one model that he thought had the best brake feel of any automobile; this was in the 1930s. It was a particular model of Pierce Arrow. In this valve he uses, what the pilot feels is purely arbitrary. It was a mechanical spring that he put in the valve. The pilots were so used to very heavy brakes—brakes that almost didn't work—that they had to put everything they had into the brakes, and they couldn't use the new brake. They overcontrolled badly. He had to arbitrarily increase the strength of this spring from about 30 pounds per full throw up to about 100 pounds per full throw. Over a period of five or six years, in successive models, he gradually reduced it back to 30 pounds. But by this time the pilots were used to low pedal efforts, and they could accommodate themselves to the low forces and not overcontrol. Now, that's why you can't make a lot of human-factor decisions just arbitrarily, because they depend on habit and custom. Well, we had all sorts of problems of this kind.

LYLE: When you were doing that kind of work, were you teaching here at the same time? Could you come back and discuss these problems with the students?

KLEIN: Yes, surely. Most of them. Some of them I couldn't. Some of them were military matters that had to do with military security. But this brake problem I could discuss. We usually had engineering officers from the air force and the navy in the class anyhow, and they'd appreciate these things, so I would discuss it with them. Most of them appreciated what we were doing. We gradually established a standard of what you should do. And I haven't seen the publications, but I understand that their human-factors publications now contain this sort of relative stiffness information and things of that sort. But this was twenty-five, thirty years ago when we were doing this.

LYLE: Are there any other observations you would like to make?

KLEIN: Well, the only thing I could say was that I've always had a very broad interest in all branches of experimental science and some of the theoretical sciences, particularly in the physical sciences. And due to friends and proximity, I became rather familiar with seismology. I was a member of the Seismological Society for quite a while. As it happened, this turned out to be very useful to the aircraft industry and in the designing of wind tunnels. One time, when I was in my office at Douglas—this was in the thirties—the fellows who were doing vibration problems down there, which was at that time pretty naive, came to see me and asked, "How do you design a vibration pickup so that we know what we're getting? Because we find we're getting all sorts of mixed outputs from our pickup. We can't design a pickup that will give us velocity or acceleration or displacement. We're always getting mixed ones." I told them that I thought I could get the information on this, because I remembered a paper that a friend of mine, Professor [Hugo] Benioff of the Seismology Lab, had written on the modernization of a Russian theory on how you design a seismometer for measuring what you want in an earthquake. So I said, "I have it at home, I will bring down a copy for you. The mathematics, should, with the proper adjustment of constants, satisfy the problem." I never heard any more. This apparently solved the vibration problem of the aircraft industry.

LYLE: You gave them the paper?

KLEIN: Yes. And then they had the equations and they could solve the equations. They were not mathematically inept; they just didn't know anything about vibration theory. And the seismologists had been at it longer than anybody else and had the most advanced theory. So it's just a case of designing for a different frequency range than that used by the seismologists; changing the constants changed the ranges.

Well, I used my seismological information in one place in which I needed it in designing the Co-op Wind Tunnel. I needed tiltmeters that were sensitive to one part in a million, for a reason that I won't try to explain to you. We made them and they worked. And they did what we needed to do. I also became aware of earth tides, and tidal effects, and things like that. And I kept lecturing the tooling people at Douglas till they realized that many of their tooling problems were due to the fact that the tides came in not too far away, and the whole block was tilting. They had been surveying these things with transits and tools, using a vertical gravitational reference, and this, of course, gets you into all sorts of troubles. So finally, when the instrument became available, they shifted over to what is called an optical grid, which is just beams of light—mirrors and things—which isn't particularly tied into a vertical reference. And they built the jig so that they can tilt it over and all the parts built in them will have the correct dimensions. Well, there were three or four other things like that where I was able to use knowledge in other fields of science usefully in the aircraft business.

ARTHUR L. KLEIN

SESSION 4

April 20, 1982

Begin Tape 4, Side 1

GREENBERG: You said you entered Caltech in 1916.

KLEIN: Yes. I went to Los Angeles High, and then to Caltech, and I've been at Caltech ever since. I have very few survivors in my class. I have two friends both with the surname Raymond and no relation to each other at all. Now, one of them, Albert Raymond, was a classmate of mine and a pharmacologist and has been an executive at G. D. Searle and Company in Chicago. The other man is equally strategic—Arthur Raymond, who's an airplane designer and was chief engineer at McDonnell Douglas for many years. I worked with Arthur Raymond a great deal. As a matter of fact, I worked as a consultant from 1947 until I left when I retired—working half-time for McDonnell Douglas as a consultant for the engineering department. And in that, I was active as an airplane designer. Among the airplanes we designed were the DC series and a lot of military ones. I once asked Arthur Raymond whether I hadn't worked on more airplanes than anybody he knew of, and he said, "Yes, probably," because we had three engineering departments that were active in design. I was at the El Segundo one, designing navy airplanes, and at Long Beach the airplanes were the commercial airplanes. I was in the midst of all that.

GREENBERG: This began when you started working half-time?

KLEIN: Oh, no, it was before that. Generally, in [a faculty position in] most places, you can use about twenty percent of your time without question; that's just one day out of five. I was working that way for Jack Northrop and for Douglas. And when the thing got pretty thick for us in '37, I suggested to Raymond that I go on half-time and become a half-time professor. So I did, and I was half-time at Caltech until my retirement.

Though I was a consultant for Douglas, I was more than a normal consultant. I was what you might call an integrated consultant, because I was there. I got into some very strange jobs, which you would never expect a consultant to get into. For instance, I ended up editing all the Douglas technical publications because I was working for Raymond. Raymond said, “You’d be the logical person to do this. You’ve got the best background in physics and engineering.” So I had to set up a system. During the time I was there, some twenty years or so, I issued and approved over 2,000 papers.

GREENBERG: You and Arthur Raymond are the links to the Caltech industrial connection. How unique was such a connection among academic institutions, and exactly what was the nature of the connection?

KLEIN: Well, I could tell you about that by hearsay. I haven’t really a direct connection to what happened, but I know what was going on. In the first place, Clark Millikan, who was R. A. Millikan’s oldest son, was nutty about aeronautics. He was getting his degree at Caltech [and] got his father enthusiastic about aerodynamics. His father, R. A. Millikan, had been a lieutenant colonel in the Signal Corps during the First World War, and one of the aircraft men in the group was a junior officer whose name was [Henry H. “Hap”] Arnold. And Arnold was a go-getter. He then got to be a very good friend of R. A. Millikan. [In 1931, he became] commanding officer of March Field. He used to come out to Pasadena and to other places in Southern California and visit Millikan. He’d always drop in to look at what aeronautics was doing, so I got to meet him. He was one of the people that we kept seeing all the time. In the meantime, Millikan had persuaded Donald Douglas to loan an engineer here to train his people, which included me and Clark Millikan. I already had a PhD, but it was in physics. And Raymond came over every week, and taught a graduate course in airplane design.

GREENBERG: All this took place after you had begun to do some of the nuts and bolts work for the aeronautics group?

KLEIN: Yes. The little experimental airplane—I’ve got a model of it here—I designed that damn thing structurally and designed all the hardware on it. That model was stillborn, because the old man working on that didn’t understand aeronautics well enough.

GREENBERG: This was [Albert A.] Merrill?

KLEIN: Merrill, yes. If Clark Millikan hadn't insisted on putting an elevator on it, which Merrill was very much opposed to but I wanted, the thing would have crashed. And without the elevator, which could lift the wheel, the airplane would go this way and that way [gestures].

GREENBERG: I gather that Merrill ideally had a kind of tail-less plane in mind?

KLEIN: Yes, he did. But, you see, Merrill apparently wasn't an engineer in the ordinary sense. And the problem there is that the airplane has three to six degrees of freedom. And three degrees of freedom are all carefully taken care of. But the dynamic modes aren't. So, if you were riding in the Merrill plane, and the power should go off, it would go like this, and then it would go like that [gestures], and it wouldn't come out, it would just keep on going.

GREENBERG: The one you designed the hardware on, that's the one that crashed?

KLEIN: Yes. Well, it didn't crash in flight. It ground-looped and broke the wing, and that was the end of it.

GREENBERG: I gather that it did make some successful flights.

KLEIN: Oh, yes. If you used the elevator, you could fly it quite successfully. I flew in it as a copilot. We used to fly around at the Dominguez Air Meet with the thing.

GREENBERG: Was this with M. F. Kelley as the pilot?

KLEIN: Yes. But the thing was not a satisfactory airplane as it was, without using an elevator. See, what happened was that without the elevator, the thing would go this way [gestures] and then go through a series of undulations of violent amplitude. If you got ahold of the stick and you could stop it, then it would fly perfectly. You had the stagger-decalage effect that Merrill was so proud of, which gave it what is called a direct stability, but it didn't have any dynamic stability. It became dynamically, violently unstable with very large amplitude. So the thing was a failure. I was not very fond of flying in it. Clark Millikan did a good deal of flying in it—with the elevator, fortunately. And that was that. But that, of course, was like a lot of the early aeronautics; people didn't understand all the things that were going on. Now, I didn't understand them either, but I had been a physicist. Then I took these courses and I started reading things about what was going on, so I rapidly became cognizant of static and dynamic stability and the other things you needed to know.

GREENBERG: Before taking Raymond's courses, you had helped with the construction of the Dill Pickle.

KLEIN: Yes. You see, at that time I had gotten my [bachelor's] degree in mechanical engineering. I was a fairly satisfactory designer. I would not say I was a good designer. I've seen, later, things that I designed, and I realize that I was pretty crude and inexperienced. At Douglas and with Jack Northrop, I was able to become an experienced designer of airplanes.

GREENBERG: When you found yourself thrown in there with Clark Millikan and [Harry] Bateman at the beginning, what was your reaction?

KLEIN: In the first place, I was in a peculiar position, which I don't like to say too much about. At that time, my family was reasonably well-to-do. I didn't have to find a job. I finished my bachelor's degree, and in the last year of it I converted to physics because [R. A.] Millikan came. Then I went into Norman Bridge Laboratory, and I was four years in Norman Bridge Lab and did my PhD in physics under Millikan. Immediately after this time, about two-thirds of the time I had a job at Caltech, and the other time I didn't. Generally, I'd get an assignment from somebody like Jesse DuMond, who had a fellowship and was in Europe and didn't have anybody to do his teaching. So I taught Jesse DuMond's elementary physics course. So, in spite of the fact that I didn't have a job most of the time, I usually had jobs more than half of the time doing something for somebody. Incidentally, I eventually became, I think, a pretty damn good mechanical engineer. It was a fascinating business. You see, the trouble with the aircraft industry before we got into McDonnell Douglas and the DC-1 was that outside of a very small number of people, like Donald Douglas and Arthur Raymond, they were not educated engineers. Most of the people in the aircraft industry were promoters; they were athletes, they were doing everything but engineering. And they were all dashing around, setting world records and getting into trouble, and the aircraft industry wasn't getting anyplace. Raymond and Douglas were different. They were both engineers; neither one of them was patents conscious. They were interested in doing things properly and getting them done so they would work, not doing something to prove something. Like Jack Northrop's flying wing, for instance. This is not a typical Douglas sort of deal. I knew Jack Northrop. Jack Northrop was a very able guy; he was a damn good designer in some ways and he had no sense in others. I worked for him for about five years, long enough to know. He designed some of the goddamnedest gadgets you ever saw. He had a very nice eye for lines—the Vega was Jack's. He made airplanes look a lot prettier. There was a competing airplane for the DC-1, built by a company in the East, and it was never finished, because the DC-1 was way ahead of it. It came out, and much to our joy, it behaved much better than we expected. The DC-1 was guaranteed—if I remember right—182 miles [per hour] cruising speed. When the airplane was flown, it went 212, and that's fantastic. We didn't realize how dirty airplanes were until we built a really clean one.

Then we had the DC-2. When we got this airplane out, we couldn't believe our performance. Company pilots were having a wonderful time, because they had DC-2s flying around this country. The pilots would find a military formation, and the DC-2 flew faster, and of course this just burned up the military. And that was the end of the bi-motor as a military plane. The bi-motor has too much turbulence, drag, and friction.

Well, anyhow, we get this airplane [the DC-2] out, and we build it all out of sheet metal, because we hoped that it would be a lot better. In addition to the fact that we had an airplane that was extremely above and beyond anything that existed, we also discovered to our pleasure that our maintenance costs were about one-third of the previous planes. We reviewed it after 1,000 flying hours and couldn't find anything to do with it. We got up to 3,000 hours before they had to do any reviewing and overhauling, which is about one full year of service. And this, of course, made the airplane ever so much more efficient than anything that was available before. And consequently, all these things like the Curtiss Condor and the rest of the military airplanes dropped by the wayside. Now, when the DC-2 was replaced by the DC-3, it included ten feet more span and had a more or less circular fuselage instead of a flat side. At the beginning of the war, the American domestic fleet was just under 400 airplanes—about 380—of which, all but eighteen were DC-3s. We had inherited the entire air travel industry by making a really good airplane from as many ways as we could think of. And so we were very happy, of course.

GREENBERG: Did a fair amount of that work actually go on at the Caltech premises?

KLEIN: Well, we did wind-tunnel work in the Caltech wind tunnel. If you want, you can go look up the old wind-tunnel reports. There's a report there on my designs for the wing-fuselage fillet, which improved the aerodynamics of the airplane. I feel very happy about the wind tunnel, because I designed a good part of it, all the mechanisms.

GREENBERG: This is the original, ten-foot [GALCIT] tunnel?

KLEIN: Yes. Of course, the ten-foot tunnel itself was a piece of machinery, and there were a lot of people there. I designed the force equipment and the weighing machinery and that sort of thing. They have been used up to today, fifty years later. The only trouble we have had with them has been in the self-balancing beams. They age and crack, and you get into trouble and you can't get any spares for it. But we have used these things for fifty years with satisfaction.

GREENBERG: Let's go back to the beginning, because this brings us to von Kármán's first visit to the Caltech campus. You and Clark Millikan had designed a certain kind of wind tunnel—an open-return tunnel.

KLEIN: Yes. Clark Millikan did; I wasn't really tuned in on it at that time. I was just getting on board, and Clark was doing his thing, and then von Kármán came in. His proposal made much more sense than the other ones, because the open-stroke wind tunnel was very large. You'd have a building somewhat bigger than the aeronautics building was, without anything in it, because you had air packages in it. Von Kármán pointed out that the Göttingen type of wind tunnel was much more efficient, and much more compact, too. So it went that way. At the beginning, I was not active in this. I really didn't have the judgment yet as to what should be done, and I knew it, so I was trying to listen to everybody. A few years later, I got to be sophisticated enough to know what was going on and why.

GREENBERG: So, from your standpoint, von Kármán's tunnel made more sense.

KLEIN: Yes. Because we had enough space in the wind-tunnel building for our classes, which we wouldn't have had in the other structures. I will say that Clark Millikan wasn't sticky about this. He knew that von Kármán knew more about it than he did.

GREENBERG: I noted in your previous interview that you associated the Caltech industrial connection with Robert A. Millikan, and that he was very important in maintaining it.

KLEIN: Yes. R. A. Millikan was very, very anxious to be useful in the broadest sense of the word and to do the things that needed to be done. This introduced a very fine atmosphere at Caltech, because the people like me who were in the lower levels of it, all of us had the feeling that part of your job was to keep the public happy. You didn't do any ivory tower-isms and operate from sort of a remote space. Now, there have been two instances in universities on the Pacific Coast with this sort of thing—Stanford went completely to pot on that same ground. The old people who were there—[Everett Parker] Lesley and the older ones, way back—were very able people and were helpful; and in fact Lesley designed the propeller for our wind tunnel. Then, after they left, a bunch of people came in and I don't know what happened to them, but they didn't have any friends. Their student population went down to two students in the graduate school in aeronautics. A lot of people who had gone through Stanford in the older times were very unhappy over it. A group of them had gotten together and had gotten money from the aircraft industry and the factories. I was asked what I recommended, and I recommended they put some money into it, too. The man in charge, the man who became dean of engineering there, an electrical engineer, had not been from the ivory tower. They got very good people. They have more graduate students than we have.

GREENBERG: Was [William F.] Durand gone?

KLEIN: Yes. Well, Durand retired about 1924. He did a lot of good work on propellers and other kinds of things. I knew him; he was a very fine gentleman. He retired, and I heard von Kármán persuade him to do this encyclopedia, and it became a standard book [William F. Durand, ed., *Aerodynamic Theory*, 6 vols. Julius Springer: 1934-1936]. It's obsolete now, but that's not his fault. But the people who came there later—[Alfred Salem] Niles and the rest of those guys—I don't know what they were doing. They didn't operate the way Stanford had; they didn't operate the way we did.

Now, the other [West Coast instance]: I heard from people I knew at Boeing. They couldn't get anything done at the University of Washington in the aeronautics department. They could get nothing to come out at all; they didn't cooperate with Boeing at all. A man asked me what the difference between Caltech and these other universities was. I thought about it, and I was pretty firmly convinced that it was the senior Millikan. He was the person who was responsible for making Caltech everybody's friend. I know when I was a graduate [student] there, the one thing you could not do is turn anybody down. If somebody needed some information, you gave it to them if you could. The atmosphere was that there you're operating as part of the United States, and you're helping people. We were always being as gentle as we could with people, and we didn't turn people down just because we were too busy.

GREENBERG: I'm wondering if the connections between Caltech and industry were such that without Caltech the development of the aircraft industries in Southern California—Douglas and Boeing—would have been a lot different.

KLEIN: I'm sure it would have been somewhat different. Let me tell you something. This is directly from the horse's mouth. When we got the [Guggenheim] laboratory built, and there were the four or five of us in there, rattling around in this new building, with part of a wind tunnel operating, Harry Guggenheim came over with a small contingent of his own. And I remember, I was walking across the third floor, right next to Harry Guggenheim, and Clark Millikan was a little farther on. And he [Harry Guggenheim] said, "You know, we're expecting you to train all the teachers. We're the only school that is a graduate school at the start, and it's up to you to teach them." And he sort of shook me. I didn't know whether I was capable of doing it or not. I was not the kind of academic person whom you'd expect to do that. Though I used mathematics as tools, I was not primarily a mathematician. But that's the way the

Guggenheims felt about it, and that's what we were trying to do. And if you look through our product, something over 1,000 graduate students, you see we have an enormous number of high-grade technicians.

GREENBERG: Did you know Alexander Klemin and Jerome C. Hunsaker?

KLEIN: Both of them. I knew Hunsaker, I think, a little better than I knew Klemin.

GREENBERG: Neither was able to get quite the enterprise going that Caltech did.

KLEIN: Well, I'll tell you, Klemin I always thought had false ideas. They seemed to be more interested in getting a wind-tunnel job financed than in how good it was. We didn't like to do that. We started in working on it [to make it] as useful as possible. And strangely enough, as soon as we got going, we were overloaded. I remember we had the B-17 in the wind tunnel. It was there for quite a while. We did a lot of airship work, which probably wasn't terribly useful.

GREENBERG: You did all that stuff in Akron, at the Guggenheim Airship Institute?

KLEIN: I designed the wind tunnel. The wind-tunnel work was perfectly good. And [Theodor] Troller is a good man, and he did a lot of very valuable work. But the trouble was that the airship business wasn't any good. It always crashed. [tape ends]

Begin Tape 4, Side 2

KLEIN: One of the strangest things is that I was in on the design of the last navy airship, because the Bureau of Aeronautics had gotten in a beef with Goodyear, and they wanted an airship that was designed by somebody other than Goodyear. And seeing that [Edward H.] Heinemann was the fair-haired child of the Bureau of Aeronautics, and I was one of his consultants, we all got in there. And so, when it came down to designing this airship, I did several things. We had run quite a few tests on pieces of various ships—outboard propellers, for example. First, I laid out the power plant. Heinemann lived in Washington, and he didn't know what to ask them. He said, "You've got to help us. We don't know anything about airships." I said, "How about the bag?" He said, "You'll have to get somebody to get us out of this." He did design the cab and the carpack.

GREENBERG: It seems people are talking about bringing airships back in today.

KLEIN: They're still around making all these studies, and I don't believe a word of them. Every one that I've had anything to do with crashed. You see, it's not a good transportation vehicle. The point is, you could [defeat it] on the simplest grounds—utilization of manpower. Now, you take an airship, it's going to cross the Atlantic in maybe three days and it has to have a fairly large crew, because it's [not] a one-shift operation. And when you get through with that, you find that you've got, let's say, three days, or six man-days, of crew to cart back and forth. And an airplane will do the same number of passengers in one shift, and your crew costs are just fantastically different. And because they are so different, you just can't make a better ship come out. I have used, for years, the Santa Fe Superchief as an example of a high-speed machine. But you can't come out with that either. It's forty-four hours from Los Angeles to Chicago, which means it's on six shifts, and it's got an onboard crew of somewhere between forty-five and fifty, and you're hauling them for the three full days. You can put people in a big airplane and take them out there in less than one shift, about two-thirds of the shift. And the operating cost is very, very much less, just on the basis of man-hours. And I never even bothered with the other costs. This is the whole point; they haven't got the money to carry all these people back and forth. That's the whole business of economics. If you take a modern high-speed airplane, like the DC-10, you will find that the cost per passenger mile is about the same as a Volkswagen. You're going such a short time, and you're going a long distance. And when you've been able to get your airplane to operate the distance, which we have now achieved, it's just an incredibly satisfactory gadget. This is the whole trouble with these high-speed trains and things—they cost too much money. It's much more expensive than an airplane.

GREENBERG: You knew von Kármán quite well. Did you work with him or for him?

KLEIN: Both.

GREENBERG: What can you tell us about von Kármán? Did he play a particular role in maintaining this industrial connection?

KLEIN: He had a flair for it. And of course, he pushed us in the direction of JPL [Jet Propulsion Laboratory], first with the Ordnance Department and then when he finally took over JPL for NASA. And von Kármán was the godfather in that.

GREENBERG: Well, let's put it this way—is von Kármán really the godfather of aeronautics in Southern California?

KLEIN: Well, I don't think that's quite a fair way of putting it, because of the fact that von Kármán came into California rather late.

GREENBERG: Things had already begun developing before von Kármán got here?

KLEIN: Yes. Well, you know how they got von Kármán, don't you? There was a committee, which was the senior Millikan and a couple of other people, and they were considering three people for the directorship of the aeronautics department. Clark Millikan, me, and all the rest of us—we were all too junior to qualify. The great people were the Göttingen man [Ludwig Prandtl]; G. I. Taylor, the Englishman; and von Kármán. Von Kármán, of course, was a very able, original, and astonishing person; he was always doing something that wasn't expected. My relationship with him in almost all cases was very good. I saw him for the last time in New York, about six weeks before he died [May 7, 1963]. He was going to Europe, to the Von Kármán Institute in Brussels.

GREENBERG: Did he make his own rules? I gather that after he was gone, the number of theoretical aerodynamicists produced in the aeronautics department at Caltech dropped considerably. And that this is connected in some way ultimately with the birth of this applied mathematics group.

KLEIN: Von Kármán was always a person to start things up, and he started a lot of different things. When he wasn't there to keep them going, they didn't stay that way. However, I wouldn't blame von Kármán for that; he was just a superior promoter. The only person to compare him with, maybe, is George Ellery Hale. He was always an amusing person. His social manners with women were like the Marx Brothers. Well, we were a rowdy department, I guess.

GREENBERG: I gather he was certainly a hard act to follow. And Clark Millikan ended up having to follow him. How did he do?

KLEIN: Fairly well. We didn't have anybody to replace von Kármán, in a sense. The present director [Hans Liepmann] is certainly not as brilliant as von Kármán. But you can't expect that sort of man to show up very often. We have had troubles like that at Caltech. There isn't the brilliance that we had. But I don't think you could expect it.

GREENBERG: Clark Millikan you knew very well, also. You were contemporaries.

KLEIN: Oh, yes, I knew him very well. I was about four or five years older than he was. I was finishing my academic work before he had started. But he had this thing that I didn't, because I wasn't crazy about aeronautics, and Clark Millikan was crazy about aeronautics. And that pushed me off into the position that I ended up in. I was very happy ending up with a major aircraft company, which I could listen to, and they'd listen to me.

GREENBERG: Was von Kármán, though he was an aeronauticist, more sympathetic, more knowledgeable, easier for the people in the aircraft industry to deal with than Clark Millikan?

KLEIN: No, I think Clark Millikan would be easier to deal with than von Kármán, because Clark Millikan was more in the regular stream of things and von Kármán was always going off and coming up with some brilliant thing that caused people to rethink what they were doing. He used to have new gadgets. For quite a few years I used to go to seminars, and I was running the shops—this was before I was a part-timer. And it was up to me to provide the experimental equipment and means for doing this and that and the other thing. I would have to design stuff for him. Fortunately, I was able to do it.

GREENBERG: And you did the aeronautics laboratory.

KLEIN: Oh, yes.

GREENBERG: I was reading one of the talks given by [William R.] Sears at the GALCIT Fiftieth Anniversary. He was needling you good naturedly and saying something about how the aeronautics laboratory was too small and there weren't enough classrooms in there. I guess they hadn't planned a very big aeronautics department at the outset.

KLEIN: No, they hadn't. In fact, the building as built had three little offices, and that was all. And we had a lot of so-called laboratory space, which wasn't very useful. We just didn't know what we were doing. We made a lot of mistakes in our building. We could have saved quite a lot of money by not putting in so much plumbing, because it wasn't used. It came out pretty good, about as good as any laboratory. You can make a general statement about the design of a scientific laboratory: In ten years, you'll use every room for something different from what you intended to. I got indignant with the wind-tunnel people continually, because I would build facilities according to what they wanted to do, and then when it would get built, they discovered that that wasn't what they wanted to do at all. Later work would be second-guessing. Well, for instance, on the Southern California Cooperative Wind Tunnel, the thing was planned to get full span and a large flap across the wind tunnel, and fly maximum lift, and all that. When they got the thing going, they didn't have enough power to drive it, and we never did reach the lift in forces we were supposed to, because the specifications didn't give us enough power. This is one of the complaints of a wind-tunnel physicist. That's where I was all the time, designing something that couldn't be used, because somebody had forgotten something.

GREENBERG: How did things like that happen—that somebody would forget something? A breakdown in communication?

KLEIN: Well, in the first place, nobody's ever built this thing that you're talking about. And so, when you build it the first time, you're finding what had happened, and if you make a mistake in the loads on it, why, you're in trouble because it's too late to recover. I'll give you two or three examples of what happens to a wind tunnel. During the midst of the war, we had the A-26, which was an advanced light bomber, and we had a power section, power nacelle, for it that had a 200-horsepower electric motor in it, and a piece of wing. And we had just built a brand new force-measuring system in the wind tunnel to support these things. The first job we were to put in the wind tunnel was this gadget, which was across from the wind tunnel, weighing with this 200-horsepower motor, and I had designed the force-measuring equipment. And my force-measuring equipment was limited to about 2,000 pounds active load. And the other thing weighed 6,000 pounds. So I had to do something about that. I had to put in counterbalances to balance out the extra weight. I was routing a thing, which should have had a 6,000-pound loading system on it, and I had to use a 2,000-pound weight, because that's what it had. Now, this was my bellyache. This was nobody's fault, because the reason for this nacelle being so large was because it had a very powerful engine in a real airplane. And to get good results, they'd like to have it that way. So we had it that way, but I had to go around and put in auxiliary supports and things to do it. Now, that was one case. Now, the Co-op Wind Tunnel, I did another one like it. They had a post tunnel model, large piece of wing, set up 12,000 horsepower. You have this big wing with big flap on it, put it in the model, test it, they can't get it up to full speed, there's too high a resistance. So they never do get the airload that they were supposed to have, because the thing was being overloaded. And I mention this, that every time I've designed something like that and they give me an outside load, something always happens that you can't use it. I used to get frustrated over these things. Usually, we had equal or better facilities than anybody else had. So, it wasn't too bad, just too many people who would want these specifications but didn't really know what they wanted. If you want to go into that kind of business, why, you're going to be frustrated.

GREENBERG: One last question, about Merrill. You got in on the nuts and bolts of his airplanes. And Arthur Raymond came and taught people real designing.

KLEIN: Yes, we did some biplanes that Douglas had designed—the XL-14 was a small bi-motor trainer. And then there were some others, and these were all bi-motors. Raymond was able to do a great deal of that. He taught us aeronautical design.

GREENBERG: Did all this have an adverse effect on Merrill?

KLEIN: I haven't any idea. I don't think so. Merrill was a crazy inventor type, and I don't think anything would have impressed him. I don't think he believed that his airplane was unstable.

GREENBERG: But he became extremely paranoid toward the end.

KLEIN: That's very common in the aircraft industry. Jack Northrop has gotten that way, too.

GREENBERG: And he thought that the airplane was being taken out of his hands, everybody else was getting in on the design, and so forth?

KLEIN: Yes, sure. I think this is a regular inventor's attitude. I tried not to be an inventor.

GREENBERG: [Robert H.] Goddard was another inventor, right?

KLEIN: Oh, sure. I almost got a job from him. The Mount Wilson people wanted me to take over during the First World War and shoot some rockets for them. Nothing ever happened. R. A. Millikan wasn't in favor of it, so nothing ever came of it. I do really fancy gadgets. But I do them mostly in my head, so you don't see things partially built, piece of machinery. Then, when you can get this thing all designed so that you can see what you're going to do, you can go through and do the design of it very quickly. And that's what's happened with the Co-op Wind Tunnel. You see this thing, you wonder, How in the hell do you do that in time? It was ten years doing. I had figured out how I was going to do all these digital things before I did them. So when I had a chance to put them together, I put them together.

But I've designed all sorts of other things before wind tunnels, which would surprise you. For instance, I built some very sensitive tilt indicators, to one part in a million—two of them—for measuring the forces in the wind tunnel when the Santa Fe Railway train went by. And other things like that. That was easy to do; I'd go over and talk to Benioff in the Seismo Lab on how to do it, and then I'd design something that would fit my case. I had these tilt things, and various other gadgets. I wasn't the kind of designer that built a lot of machinery, and who would design it in junk.

GREENBERG: You had it completely worked out in your head, from the start.

KLEIN: Yes. It takes a lot of time to do that.