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HOMER J. STEWART
(1915 - 2007)

INTERVIEWED BY
JOHN L. GREENBERG

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INTERVIEWED BY
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Abstract

Two interviews with Homer J. Stewart, aeronautical engineer and Caltech Professor of Aeronautics, 1942-1980, and Caltech alumnus (PhD, 1940). The interview by John L. Greenburg is in four sessions in October and November of 1982. A supplemental interview was conducted by Shirley K. Cohen in November 1993.

The first interview covers Stewart's youth and education (B.Aero.E., University of Minnesota, 1936) and his early interest in aeronautic technology. Comes to Caltech for graduate study in aeronautics, 1936-1940 (PhD, 1940); courses with faculty members W. Smythe, R. C. Tolman, E. T. Bell, M. Ward, H. Bateman. Comments on critical roles of Theodore von Kármán and Clark Millikan in establishment of graduate program known as GALCIT [Guggenheim Aeronautical Laboratory at the California Institute of Technology]; creation of GALCIT wind tunnel for testing; advancement of aeronautical engineering education; and linking of GALCIT to burgeoning California aerospace industry. Von Kármán's identification of new technologies; his bridging of industry and

academe; similar integrating approach applied to founding of Jet Propulsion Laboratory [JPL]. Discusses GALCIT's role in the development of commercial aviation in the 1930s. Appointment to professorial rank (1942) and wartime teaching and research on meteorology; comments on Irving Krick at Caltech. Discusses beginnings of rocketry at Caltech and his own pioneering contributions; work of Frank Malina and H. S. Tsien. Postwar separation of Caltech and JPL and formation of NASA [National Aeronautics and Space Administration]; takes half-time position at JPL. 1950s top secret work for government on guided missile and satellite programs, including Atlas, Polaris, Jupiter; various controversies over competing missile designs, especially Vanguard, during space race with Soviets. Advising on Apollo lunar program. His work on windmill technology.

The 1993 interview captures in fuller detail Stewart's memories of his service to government agencies and congressional committees during World War II and the years of the Cold War. It includes further reminiscences of Von Kármán and Clark Millikan, and other Caltech colleagues Maurice Biot, Fritz Zwicky, and Howard McCoy; the Caltech wind tunnel; details of airplane design; and observations on the establishment and growth of California's aerospace industry. A list of Stewart's government and industry affiliations is included as an appendix.

Administrative information

Access

The interview is unrestricted.

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Stewart, Homer J. Interview by John L. Greenberg. Pasadena, California, October-November 1982. Interview by Shirley K. Cohen. Altadena, California, November 3, 1993. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Stewart_H

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Four distinguished engineers at Caltech commencement, 1947. (From L-R:) Clark B. Millikan, professor of aeronautics and successor to Theodore von Kármán in 1949 as the second director of GALCIT; Louis G. Dunn, professor of aeronautics and first director of the Jet Propulsion Laboratory; Frederick C. Lindvall, professor of electrical and mechanical engineering and chairman of Caltech's division of engineering; and Homer J. Stewart, professor of aeronautics. Caltech Archives, Lee A . DuBridge Papers.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH HOMER J. STEWART

BY JOHN L. GREENBERG

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES
ORAL HISTORY PROJECT

Interview with Homer J. Stewart **by John L. Greenberg**
Pasadena, California

Session 1	October 13, 1982
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GREENBERG: Let's begin with some questions about your background and your family. Were you born in Minnesota?

STEWART: No, I was born in Michigan. In fact, I was born on the farm where my father was born, and about a quarter mile from the little log cabin where he was born. You see, my father came out of a farm family. He was one of Robert Millikan's students at Chicago; I think he graduated about 1904. As a matter of fact, in going over some of my dad's old things, I found a copy of the page proofs for Millikan and Gale with the corrections made in my dad's handwriting. He apparently had been allowed to keep the proofs as a souvenir. Millikan invited my dad to go on for graduate work, but Dad felt he needed to pay off his brothers and sisters who had loaned him money to go to college, so he got a job as a high school teacher. During the 1915 period, he was teaching high school in the Iron Range, in northern Minnesota, in the little town of Gilbert. When it came time for my birth, he took my mother back to the family household in Michigan; so I was born there.

We lived in Gilbert for a while. As the war came on, Dad got a job as assistant professor at Kansas State Agriculture College. He taught physics and mathematics there. He taught Signal Corps people about new things like vacuum tubes and how to use them for communication systems during the war. He started a broadcast from the campus there, where they would broadcast the weather report to the farmers using Morse code, and then later on he put in a voice system.

Just after World War I, he got an appointment as assistant professor up at the University of Minnesota Agriculture campus. So we went up there. Most of my younger days were spent in that area, but we lived for two years in Iowa. Dad left the university in 1929 and went into the electric power business. And a little later, my parents were divorced. I graduated from high school at Dubuque Senior High, took one year of college at the University of Dubuque, which is a small church school, even smaller than Caltech. In fact, it's so small that even I played football. [Laughter] That was something that I certainly didn't do at the University of Minnesota when I went there the next year.

GREENBERG: You became interested in science at home?

STEWART: Oh, yes. Dad was interested in research, too. At the University of Minnesota, for example, one of the most important things he did, as private consulting, was to help set up some of the radio stations in the early days. He decided that with his farm background, and with electricity coming along as it was, the appropriate thing for academic research was to focus on what use electricity could be on a farm. And he talked a couple of the electric power companies into providing some research funds so he could put short power lines into some rural areas. He equipped these farms with electric motors and meters, and kept economic records. He wrote some of the earliest good economic engineering reports on the application of electrical power on the farm. Of course, that really got him started in the electric power business, and he finally left the university and went into that. He stayed with the Interstate Power Company in Minnesota, Iowa, and Illinois, for the rest of his life until he finally retired.

GREENBERG: Was your mother also well educated?

STEWART: She'd gone through high school. She had no college. In fact, my father met her while he was teaching at the high school. They publicly announced their engagement the day after she graduated. That was in Wichita. I have lots of family relations. My father grew up in Michigan. They're mostly still farm people there, although some of the youngsters grew up and went to town. I have two sisters who still live in Minnesota and two brothers—one lives in

Texas and one here in California. There are very few living relatives in Kansas from my mother's side. It was an educated family, academically oriented. Dad, I must say, was absolutely ecstatic when he found I was coming out to Robert Millikan's establishment. [Laughter]

GREENBERG: When and how did you get interested in aeronautics?

STEWART: Well, it started really when I was in grade school. I started making little model airplanes. The first thing I ever did was in the seventh grade woodshop. They had a little project where you could make a model airplane with rubber-band power. I made one and it worked beautifully, and it just fascinated me. I also carried papers. I remember carrying papers the day that Lindbergh landed in Paris, and that was a big deal. He was a local boy, of course. When I was in high school, we learned to fly a glider. Several of my friends and I first bought time on a glider that somebody else owned. And later on, we saved up our lunch money to build a glider. During high school, I guess I made most of my spending money by building model airplanes and selling them to other kids. I remember one summer there, I kept proper books of account on the enterprise. By the time I finished, I found I'd made a net of fifteen cents an hour for myself. [Laughter]

GREENBERG: When you went to the university, did you know that aeronautical engineering was what you wanted to do?

STEWART: I wasn't certain. I was also interested in radio—I mentioned my dad's interest in that. I remember when I was a senior in high school, I found in some of Dad's things an old Signal Corps kit, a one-tube, battery-powered radio, with a 2,000-ohm earphone. So I put that together and it worked; and I had some friends who were interested in ham radio. Then I built a 56-megacycle, push-pull transmitter, plate modulated on the oscillator; and it worked. As a matter of fact, my first really professional job occurred when I was a junior at the University of Minnesota because of that interest.

You may remember the Piccard brothers, the balloonists, and the first stratosphere balloon. One of them, August Piccard I think it was, came to the University of Minnesota in '34 and was one of our professors there. He had a project under way to build a big plastic balloon; it was one of the first ideas for high altitude science research. One of the professors in electrical engineering had gotten a contract with the weather bureau to make an instrumentation package to go along with it. This was one of a two-stage procurement that the weather bureau had for what later became in World War II the standard radio sonde. The other contract came here to Caltech, where Glen Peterson was working on it when I came out in 1936.

The professor at Minnesota who was going to do the work left and couldn't continue with it; so they were sort of stuck and they found me in there and hired me to do it. I got paid the highest pay rate of any undergraduate at the university; I got paid forty cents an hour. So I built the instrumentation, and I built the airborne transmitter, and the ground receiver, and set up the recording equipment.

GREENBERG: So your interest in meteorology had already begun there?

STEWART: Yes. Now, actually the radio sonde that came out of the project is like what Glen Peterson did here at Caltech, and not like what I built there because my background in radio engineering was just ham radio. I had to build a system that was within my capacity to design and build. I couldn't build a really good quality analog instrumentation system, so I built a kind of primitive digital system. It gave a lower amount of quantitative data, but it did give us good accuracy. On the other hand, Glen Peterson's project was essentially what was used in World War II, the radio sondes. So I was interested in that all along.

When I was a freshman in college at the University of Dubuque, I guess I was kind of lucky; it was so small, their teaching staff was very small. They had one good science professor. He taught chemistry, and most of my work there was for him. I took a course in organic chemistry, as well as the standard first-year chemistry course that he gave. My mathematics course was a funny one. It was taught by an English professor who knew nothing at all about mathematics, but he'd had the good fortune to choose a pretty good text. By the end of the first week, it was clear that there were half a dozen of us students who knew more than he ever would. So we really took care of the course from then on, and it worked out fine. I got a lot out of that first-year math course. I think I learned the calculus there better than most freshmen do.

Then I went to the University of Minnesota because the University of Dubuque was very limited. I got most of what I could get out of it that first year, and came to Minnesota. By that time, I had decided that aero engineering was what I wanted to go into.

GREENBERG: Did you go to Minnesota in part because aeronautics there was in a relatively advanced state?

STEWART: It was one of the more advanced schools in the country, one of the biggest in this area. In fact, when World War II started, if I remember correctly, one of the first scientific manpower studies came up with the result that about 10 percent of the professional people in aeronautics in the country had come from the University of Minnesota. So it had started early. I should say a few words about the staff. John Ackerman was the head of the department; he'd been in aeronautics before World War I, worked with Sikorsky in Russia in building the big bombers that the Russians built—with not too much effect in that war, but at least they built them and he was interested in them. He came over with that background. Howard Barlow, who was the professor I was closest to, was an energetic young fellow who'd done consulting and private engineering work as well as academic work. I don't remember his background at all. But he was knowledgeable about the state of affairs in aeronautical technology. For projects for the students, he brought in practical engineering things on a consulting basis. With a group of about a dozen students, he would do the job. One of the students, Bob Gilruth, who was a year ahead of me, Pater ran the Houston NASA Lab in the Apollo days. His wife, Jean, was also an aero student at Minnesota; she was a Pasadena girl.

We designed Roscoe Turner's racing plane; you may remember the lion cub, Gilmore, the advertising symbol for the oil company that sponsored it. We did another airplane design there, a small two-place. I think it was called Taylor Craft. An airplane design in those days was fairly simple, maybe twenty to thirty man-years of effort. On that airplane, I did wing design, for example, and I wrote up the performance report for the government licensing procedure and part of the stress analysis. On the racing plane, which was the first one for me, I did part of the fuselage design analysis, stress analysis. My principal job was change artist, to keep all the drawings up-to-date. That process got so out of hand in recent years that the most practical way now is to put it into big computers; this data processing is called "configuration control." But in those days, it was a one-man junior job.

GREENBERG: I guess Bill Sears was also at Minnesota.

STEWART: He was two years ahead of me.

GREENBERG: Did you know him?

STEWART: I didn't know him; I knew of him. He was still there during my first year at the University; then he came out here. Actually, I hadn't really thought very much about coming out here until the fall of my senior year. Barlow came by and said, "You ought to think about graduate work. You ought to think about going to Caltech." I really didn't know anything about Caltech, except that he told me that Bill Sears had gone there. Anyway, I got sensitized to the name and ended up by writing out here and got a letter back which offered me an assistantship at the wind tunnel and admission, so I came.

GREENBERG: So, at that time, you really weren't aware of the school as being distinguished among schools of aeronautics.

STEWART: No. What I really knew was that there were half a dozen places—here and NYU, and Georgia Tech and the University of Michigan, and Minnesota. And those were the ones whose names I recognized at the time. It turned out that Stanford had a pretty good establishment out here, too. I didn't know about that before I came out here.

GREENBERG: Once you did get here, did you find anything particularly different? Was what you learned here a continuation of what you'd already been doing?

STEWART: It was in a sense, but it was a real eye-opener, too. For example, at the University of Minnesota, the standard curriculum went only through two years of mathematics, and all the stress analysis and dynamics and everything else was at that level. You had a certain number of electives. I was interested in both physics and math. And when I looked at my elective allocation, I finally decided I couldn't take the physics courses so I took math. And in my sophomore year, I took a course in differential equations which used Piaggio, a normal European text. And in my junior year, I took a course that's like what AM 95 here was intended to be. We used a text by the Sokolnikovs of Wisconsin, which wasn't really a very good text. When [Theodore von] Kármán and [Maurice] Biot put together their book, which was really designed for the same purpose, it was a much better treatment of the subject. Their examples were much better. The trouble with the Sokolnikov text was really that the book was written by two mathematicians, and the examples they chose were mostly too elementary to be of much interest to real engineers. You needed to have a more advanced type of application. The book was perfectly good, but the examples were poorly chosen.

GREENBERG: They weren't real engineering problems.

STEWART: They were engineering problems of the kind that the boys with just calculus could do. You wanted to see if there was some advantage to the higher mathematics. In my differential equations course there, we learned the usual linear equation of the second order, learned something about partial differential equations, and the Ampère-Monge method of solution, which we usually call different names today. Not much of the sort of Riemannian geometry kind of thing; and almost no complex variables. In my senior year, the math courses that I took were vector and tensor analysis. The first text we used was a little text by Coffin on vector analysis, the notation, and algebra. Then we used Gibbs's old book that he wrote back in the 1890s or whenever it was, when he introduced dyadics, which was the second order of tensors, applying them properly. And the third book we used was Will's *Vector and Tensor Analysis*, which I think was quite a satisfactory book. It covered differential geometry, and it gave enough so that I could take [Richard Chase] Tolman's course on relativity and cosmology.

GREENBERG: Well, it sounds like it wasn't a bad undergraduate mathematics education for that time.

STEWART: Yes, it was quite good.

GREENBERG: Were you ready for Caltech when you got here?

STEWART: Yes, I had no real difficulties here. I fitted in very easily. As a matter of fact, there's one funny story I remember regarding this. I did take the graduate courses in physics, so I had a minor in physics when I finished. And one of these courses was Smythe's E&M course. Now, most of the physics students who were trying to take it had quite a problem because they had to learn the mathematics as well as the physics. And I learned the mathematics earlier and learned one application in aeronautics, and so really, all I had to do was translate into the physics; it was fairly straightforward. And one quarter after I had taken the course, I had some reason to look up Smythe. I looked into his office and he was grading papers from the exam. He was looking unhappy, and he looked at me and said, "Why is it, in my course, the only people I can give A's to are aeronautics students or juniors?" One of the juniors who was also there was Panofsky, who runs the Stanford linear accelerator now. Panofsky was a junior taking Smythe's course, and Smythe always had to give him A's; and I was taking it at the same time, and he gave me As. And in that year, the best any of his graduate students did was a B. Another reason was simply that I'd had this material in the mathematical sense before, and Panofsky was an extraordinary student, even for Caltech.

GREENBERG: It had a reputation as a very tough course.

STEWART: It was if you had to learn the mathematics, too. But by the time I took it, I'd also gone through Bell's and Morgan Ward's courses in mathematics here. We used Whittaker and Watson, and Copson, and worked through the analytic theory of all the special functions, processes of analysis, and so on. Now, I had read some things in Whittaker and Watson back at Minnesota. I knew the book. But at that time I really couldn't construct the details offhand as an exercise for the reader.

GREENBERG: Did you take anything from Harry Bateman?

STEWART: I took two courses of Bateman's. He gave a course in compressible fluid theory, and then I took a course a year later that he gave in potential theory.

GREENBERG: Were you familiar at all with the work that Bateman and A. A. Merrill had done here in the 1920s?

STEWART: Not really. I did meet Merrill and knew him. I knew Bateman reasonably well; he was around a lot and I took these two courses from him. He generally attended seminars. I knew him quite well. My impression of Merrill—and this is not necessarily a reliable one—is that he was more the inventor-mechanic type person. And, of course, Bateman is probably as extreme a contrast to that as you can find. I wondered if they ever really did communicate with one another. I don't know if they even tried. They were very different people. Merrill did a good job of building a little wind tunnel, and the thing worked.

GREENBERG: Did Bateman do anything noteworthy in aerodynamics in those early years?

STEWART: Well, he did some things like the original development of thin airfoil theory, also done by Max Munk, who used a linear superposition of two-line element hinges. He'd superpose those things. Now, that's a very messy way of doing the thing. There were several people who gradually improved and simplified ways of developing the theory. I think the last significant contribution was by Maurice Biot and myself. And Clark Millikan did one in between, and Bateman did one of the first of the improvement steps on how to look at it.

I think if you look at the gradual evolution of the applications of the method of characteristics to fluid mechanics, the basic mathematical theory was put together back in 1870 or so by Riemann, following the early work by Ampère and Monge. Then Bateman pulled it together and covered it in his course in a nice, logical way. There were lots of funny misunderstandings about this; it was really very poorly understood. Prior to 1939, compressible fluid flow was considered a very exotic topic. The Volta Congress in 1932 was, I think, the first attempt to pull it together as a branch of applied mathematics, more advanced than the linear acoustic theory.

I think the funniest story I can remember has to do with some of the things that came out of von Kármán's Tech intelligence teams that went into Germany at the end of World War II. The people at Peenemünde, for example, obviously were working with supersonic flows, and they had interest in the calculation of supersonic flows and mathematical theory. They were kind of hyped on security, so while they got the help of some of their theoretical specialists—Sauer and Tollmien, for example, in this particular story—they did it in a sort of disguised way so that Sauer and Tollmien never really knew what they were doing. For example, in this question of learning how to compute things, they were given a synthetic problem, the flow in a divergent nozzle. They gave two contracts, one to Sauer and one to Tollmien.

As I recall, what really happened was that Sauer used a covariant vector decomposition and Tollmien a contra-variant vector decomposition in developing the characteristic theory. This point was entirely overlooked by most of the people who were examining this at work. Also, incidentally, one had used an area method, as they call it, where you laid out panels and took as the computation variable the value of the quantity at the middle of the panel, and the panel is bounded by characteristic lines. And the other, Tollmien took the point method and used the corners of the intersections as the computation variable. So there developed a big controversy in American circles here at the end of World War II on the area method versus the point method. Is one right and the other wrong? The numbers they had were very different, because they were so crude that a 10 percent error, which was what they got, was not at all surprising. And the real difference was that one had used a contra-variant vector and the other a covariant vector, and they were *both* right. It's just that the crudity of the calculation permitted 10 percent of errors. Now that problem, of course, was no problem at all to anybody who had taken Bateman's courses, for example, or Tolman's courses. It was no problem to Puckett or Liepmann, who were here doing the same kind of thing at the same time.

GREENBERG: So you would classify Bateman as having been a bona fide aerodynamicist?

STEWART: Yes, I think so. And he was kind of proud of the claim. There were some problems which he was interested in, phenomena like flutter, for example, and the stability problems. But offhand, I don't recall any publications of this work. He did do some things that you might call eigen-value (characteristic) problems, which are closely related.

GREENBERG: I guess he was concerned about this question of whether or not the Merrill "Dill Pickle" was stable, which I guess it wasn't.

STEWART: It almost surely wasn't. The aero engineers back in pre-World War I days had developed some funny ways of discussing the stability problem. I learned those first at the University of Minnesota with John Ackerman.

GREENBERG: And you say there were funny ways of looking at it?

STEWART: Well, they were the sort of developments that came out of the naval architecture, concerning ideas of metacentric height. They were developed as a parallel to the naval architecture approach to the stability of ships. The first text which worried through the approach that's more customary now, namely, the eigen values of a small disturbance perturbation analysis, was—let's see, Bairstow wrote his book I think in 1910. So the idea of doing it this way was around quite early, but it hadn't penetrated until after World War I.

GREENBERG: Did the English and Germans have a better idea about stability problems at this time than we did?

STEWART: I really don't know. You're talking about the pre-World War I days. Well, I would say that the initial idea of induced drag was really done by Britain, and that was about 1890.

GREENBERG: You mean Lanchester's—?

STEWART: Lanchester. Prandtl idealized it greatly, made it in a form where we could calculate some numbers, and that was a big contribution.

GREENBERG: Were those things known here?

STEWART: Oh, sure, yes. Now, the logical problem to come out of the induced drag theory was, "What's the way to get an efficient design with the minimum drag?" That was done by Max Munk, an American born in Germany, who went over there as a student of Prandtl. That was the first important application, I think. And again, Max Munk was known here; I think he spent most of his life at NYU. This was in 1918, when his thesis was written. My professors at Minnesota knew about all these things; the books I had contained references and so on.

No, the real difference when I came out here was that the staff here, such as Kármán and Bateman and Clark Millikan, had two things that were very important. They had a higher personal capacity to handle mathematics and had an interest in applying it, and had no fear in applying it in any area. And secondly, they were all—except for Bateman, who was sort of an ivory-tower type; he was happy to live within the academic community and publish academic papers—but Kármán and Millikan, and I think Millikan learned it from Kármán, were outgoing people who wanted to live in the whole world. They wanted to influence what happened; they wanted to know all of the people outside.

Kármán invited the Douglas people to come in and give lectures here. He knew all of them. He wanted our air force and our navy to be better technically because their plans were going to be important; so he got to know the military people and got them to send students here. We had lots of military students here in those days. And, of course, this broader social group went outside the school with something which was very strong here. I think that was what made Caltech and the aeronautics department a very special area. That was an important thing.

I remember running into this line of thought about 1950. I happened to be thinking about my undergraduate days, and I suddenly realized that I could only remember about five or six people from the University of Minnesota by name. And I said, "Now, why are they different from the rest?" Well, it turns out that they were the ones who were interested in going out and doing things, getting hot contacts outside. And then I realized that that had been a very powerful part of Kármán's influence here. He involved the school with all kinds of things.

The first fall I was here, I think I bailed Kármán out of what might have been an embarrassing situation, which came about in this way. Do you remember hearing of the Akron and Macon, the big dirigibles? And the Akron accident, off the coast of California here, when it finally went down? Well, Congress had decided to hold a formal investigation of the Macon disaster, and they appointed [W. J.] Durand, who headed the department up at Stanford as a professional consultant to the committee to carry out the technical aspects of the investigation. And Durand had recruited von Kármán to do some of the dynamical analysis. The ship had a ship's log that was kept up all through the accident; the process of going to pot took about five hours, so there was lots of time and lots of notes. The question was, could you make any sense out of this recorded data? Something happened, and they smelled gas and they dropped motors, and all this stuff. Could you do a dynamical analysis correlating all this information and see if it makes any sense?

This had been set up about '35, I guess. During the year 1935-'36, one of our naval officers here did a thesis, which was a numerical, step by step integration of paths using this log data. And the results looked as though they had some interpretation.

Well, when I came out here in the summer of '36, Kármán asked me if I would take that over and finish it up, because the congressional report had to be in the following January. I worked at it for a few days, and something bothered me; I couldn't figure out what it was. I wasn’t real easy with the concept of apparent mass; I can remember going to Francis Clauser, a graduate student here at the time. I talked to him about it and all the apparent mass was in there in the proper ways. Finally, I found that the trouble was with the aerodynamic lift and drag data that they got from tests at the wind tunnel back at Goodyear. They were using wind axes lift and drag data in equations which called for body-reference coordinate systems. So, they should have had normal and chord-wise force components instead of lift and drag. So, in some sense, all the calculations of the previous year and a half were wrong, and Congress was expecting the report in only three months.

Begin Tape 1, Side 2

STEWART: Well, by working fourteen hours a day and taking Incomplete for all my courses the first quarter, I did get through the analysis and Kármán got his report out on schedule. And it did have some interesting results. Now, that was the kind of thing we’d done at Minnesota with the little two-place airplane design and the racing plane design with outside contact. But this was outside contact on a national scale, involving Congress and Durand. I found my first year here was really an eye-opener, because you could see there were so many things that could be done when you started thinking in broader terms.

GREENBERG: Do you think that Caltech was unique at that time as far as this aspect of aeronautics goes? There were other places that received Guggenheim money to build up their programs.

STEWART: There are other places with Guggenheim money, but I think the thing that made this almost unique was that Kármán really pushed hard at this business of outside interactions.

GREENBERG: Was anybody doing aerodynamics in this country—I mean theory now—during the 1920s?

STEWART: Oh, sure!

GREENBERG: You’ve mentioned [Max M.] Munk, for example. And then there are other people with the NACA.

STEWART: The NACA, yes.

GREENBERG: I think [Theodore] Theodorsen was one.

STEWART: Theodorsen, [I. E.] Garrick, [Ira H.] Abbott, [Albert E.] von Doenhoff, and [Charles Stark] Draper at MIT, for example. I don't remember who else at MIT.

GREENBERG: The others were with the NACA?

STEWART: The other group was the NACA group. At the other universities I don't remember the staff particularly. I remember one of my friends spent most of his life working at NYU. He went there in 1938, I guess, and stayed there.

GREENBERG: Where did people like [Jerome C.] Hunsaker and [Alexander] Klemen fit into the picture?

STEWART: Well, Hunsaker was the leader of the MIT group. Klemen was the leader at NYU. Hunsaker was there very early. He was at MIT, I think, by 1920. In '38, when I was first back there—more with a meteorology group rather than with the aeronautics group—there were good people there. They wrote papers in the twenties, I would guess. One of your questions was about publication potentials. In the years before I came out here, in the twenties, I wouldn't be surprised if they were somewhat scant. I noticed, for example, Bateman's papers, and von Kármán had a lot of papers, published in the *Proceedings of the National Academy*. In fact, I had a couple of papers that were published there; Kármán thought that was the best place. By the early thirties, in '32 I guess it was, the Institute of Aeronautical Sciences had been formed. And the *Journal of Aeronautical Sciences* was a very good publication resource, and for many years was the principal one.

GREENBERG: Kármán complained that there were so many developments in aeronautics going on around this time, that the one journal was not big enough to accommodate long papers.

STEWART: That's true. But now that there are ten journals, it's still true. Long papers are always a problem. In a few cases where there were long papers, the NACA would publish them as a NACA report. But by and large, NACA reports were mostly NACA research. And the NACA really wasn't a very big place. It's hard to realize how small the whole aeronautical business was in those days. It really was an amateur business in the twenties. There was almost no government money at all. A typical government order for new airplanes for the next year would be twenty or thirty airplanes for the military forces. Another example: we had to stop flying our glider at the Minneapolis airport in 1930 when the first airline service started—one a day from Chicago! Even the year after the war opened in Europe, the budget that Roosevelt put in, in March of 1940, called for I think seventy-eight airplanes for the Army, the Navy and Marines. Of course, six weeks later, they were hollering for 50,000. [Laughter]

Yes, it was a very small business. I doubt that there were as many as a thousand people who had bachelor's degrees in engineering in the aeronautical specialty by the time World War II came. It might have been more than a thousand, but certainly not ten thousand. Here at Caltech we had eight or nine graduate degrees a year in the thirties. At the University of Minnesota—which was one of the major ones—there were a few more who got bachelor's degree level—maybe twelve to twenty. At places like Georgia Tech there'd be a half dozen or so. It was a very small business. And it wasn't a commercial business. When I got out here, I'd received letters from most of the companies saying, "Come and work for us. If we make money, the engineering crew will get part of it." [Laughter] Their chief engineers were paid and a few of their lead people were, but junior engineers were frequently just amateurs putting in their time. Even in the shops, a lot of the shop work was done by high school kids who came in to plane a beam or weld a fuselage. It wasn't until just the year after I got here that it started to convert to a professional engineering business—what you might think of as conventional business with commercial arrangements. It became customary to pay all engineers that first year.

The government put out its first big money in 1936, I think it was—put out a million dollars for the B-17, the first big money they'd put out since World War I. I don't know whether this was still true by 1940, but it was certainly true a couple of years earlier. There was no point in anybody spending any money on building airplane engines, because you could buy an OX-5—it's a good 90-horsepower water-cooled engine—for eighty dollars, out of World War I surplus. It would be delivered to you in the packing case that it had been put in back in 1917 or '18 when it was delivered to the government. And there was a bigger water-cooled engine, and I've forgotten what that was—Curtis Conqueror, I think it was called, which also was available from World War I surplus.

There were new developments of those things made for a few special things, like Snyder Cup racers, for example, if you wanted a little more. And the only real engine development during that period was the radial engine—the Kinner, the Curtis-Wright, and the Pratt and Whitney radial engines, they were new. But the numbers were very small. I remember in 1939, Pratt and Whitney was just at the point of shutting down, because they didn't have an order for a single engine. That winter, the French minister of air, Pierre Cot, came over to the United States and gave an address to Congress, screaming for help. He mentioned that their industry had been so disrupted by the Fifth Column that they hadn't delivered a single airplane since the war had started. And that was true. What he didn't say was that it was his unions; he was a communist leader of the unions. [Laughter] Hitler and the communists were all allied then, and it was his unions that had shut them down. And it was true. They hadn't delivered a single plane since the war opened. After Stalin died, I remember going over to Dabney and digging out a copy of *Pravda*, and here was the telegram of condolences from this French fellow. I was sure it would be there. [Laughter] While he was here asking for help, they put in an order for two hundred engines. That's what kept Pratt and Whitney open so that they still had an engine production line going when we got into the war.

GREENBERG: Your colleague, A. L. Klein, told me that Donald Douglas was one of the first trained engineers to get into commercial aircraft, to found a company.

STEWART: I think that's a fair statement. And they [Douglas] were also the first to hire an aerodynamic engineering specialist. W. Bailey Oswald was the first aerodynamicist hired.

GREENBERG: Were the key personnel in companies like Douglas—Bailey Oswald for example, and the chief engineer, the man who came and taught the classes in design.

STEWART: Raymond.

GREENBERG: They were paid, I assume?

STEWART: Oh, yes.

GREENBERG: But nobody else?

STEWART: Well, it depends. By the time I was here, the DC-3 project was already started; and that was starting to be a professional business. They probably had amateurs working for them, but I don't think they had amateurs working on the DC-3.

GREENBERG: But ten years earlier?

STEWART: Ten years earlier, yes. Yes, 1936, '37, was about the period when things started to change—the summer of '36, in aerodynamics, for example. Douglas by that time had two; they had Oswald and Gene Root. Lockheed decided they'd better pay attention to this, and they hired one. They hired Kelly Johnson.

This is a funny story. There was a Navy competition coming up. They wanted a four-engine flying boat—PB-4Y it was called. Harry Sutton was the chief engineer with Consolidated down in San Diego. He hired K. D. Wood, who was professor at the University of Colorado; he hired him as an aerodynamics consultant to come down and help them out with the PB-4Y. And K. D. convinced them that they should make some wind tunnel models and test them in the GALCIT wind tunnel. So we went through this and got some reports as we did in those days, because in general, nobody had aerodynamics specialists. Bill Sears and I, in this particular case, ran off the performance analysis and the stability and control analysis and sent them in. Harry Sutton got this report and he looked at it and he said, "Well, here's this report and here are all of these numbers; what are they good for?" And then he had a good idea. They were just about to start flying the PB-Y, their two-engine flying boat. And he said, "If I make a model of the PB-Y and send it up there, then they can turn out their numbers. Then I can compare them with our flight test data which we are about to have, and I'll know if these numbers are worth anything or not." So we ran through the things. I remember one Saturday morning, Bill Sears and I finished off the analysis. Bill typed off the report—he could type and I couldn't—and mailed it off to Harry Sutton. The following Monday he was on the phone and he said, "Where did you get our flight test data?" We missed the top speed estimate by one mile an hour! [Laughter] Probably never did as good before or since.

At any rate, they ended up by hiring an aerodynamics specialist permanently, too. So it was just that period when the industry was becoming mature. The universities had made their attempts at it earlier. I think the Guggenheim people had a big role in helping it. And Caltech, with Kármán being here, had pushed contacts into all of the industry groups, including Boeing.

GREENBERG: Clark Millikan, the son of Caltech's Robert A. Millikan, was one of the early aeronauticists. Do you feel he played any particular role in getting his father to go into aeronautics in a bigger way in the mid 1920s?

STEWART: I just don't know. He could hardly have helped being involved in it. It would have been logical to expect Clark and Bateman and people here interested in aeronautics to know of the Guggenheim possibility first and to bring it to the attention of the old man. But whether it actually went that way or not, I have no idea.

I think Clark has been more important than most people give him credit for. In many respects, I saw Kármán being a seminal influence on a lot of things, and Clark was the actual builder who made them go. At the time that I came out, Clark had taken the wind tunnel and made that a real operating, going thing. And Maj Klein had helped a lot with the instrumentation and mechanical ideas. But Clark made it a going thing, you might say from a business standpoint, and made it a resource that became fashionable with the industries. They found it really paid them to run their stuff through the analysis here.

GREENBERG: But I guess he did have a low profile.

STEWART: He had a low profile. But the same thing happened again at the Jet Lab. Kármán was completely responsible for starting that. Did you ever hear the story of his discussion at the National Research Council regarding new ideas that ought to get some government support? During the thirties the National Research Council was asked to look at the question of modern technology in areas that perhaps needed some further help in getting started. So they set up a small committee and Kármán was one of the members. And Hunsaker at MIT was another. They came up with several areas where they thought it looked as though there was need for some seed money to start people thinking about new problems. One of them was in icing, another was rockets and jet propulsion for aeronautical applications. It was decided that the NRC would put out ten thousand dollars in each area just so you could afford to have a couple of staff people thinking about it. Hunsaker decided he wanted the icing funds, so Kármán, who was the junior member, was left with the rockets, which he was perfectly happy with. That ten thousand was the first money for rockets out here. The following year the Air Corps put in some money. This was about 1938. I think it was ten thousand, and the following year the Air Corps sent us a hundred thousand to continue. Now, the icing was surely important. On the other hand, the rockets were an idea that had a broader potential for the long run.

As that work got under way, Clark really followed it closely. I remember all the years, toward the end of the war, every week Clark was out at JPL for the Research Council discussion. He played a big role in making it really a going business and not just an amateur's business.

To see the opposite side, you have to think of what happened to Robert Goddard. Goddard was a fellow who always insisted on keeping things close under his own control. And even after World War II started and the Navy brought him back there and set him up with a little shop, he never was able to operate a group bigger than one where he could handle all of the administrative details himself. He'd handle a maximum number of people; I guess there were more than a dozen but probably not as many as two dozen, peak size, in that group there. And it was just his personality; he couldn't think in terms of an organization that could grow.

GREENBERG: You know, Frank Malina had personal dealings with him and found him very uncooperative. He just couldn't get anything out of him.

STEWART: Oh, yes. He'd never say a word even if you had authorization from his employers. [Laughter] He was a funny fellow that way. But it was sad, too, because he had done some of the early work. And it would have been nice if he could have grown with the concept, too. At any rate, Kármán brought the rocket idea here. There were a number of graduate students who were interested in it as well. So there was fertile ground to recruit some help. But I think it was Clark who helped to push it so it could become big. Now Kármán helped in the outside relationship. For example, I'm sure the Army Ordnance came to us in 1943, with the sort of things that led to the ORDCIT contract and JPL being formally organized, largely as a result of von Kármán's discussions with high-ranking people in Washington, and the relations with Aberdeen [Maryland], with Puckett and the supersonic wind tunnel and these kinds of things, and with the Wright Field people. Kármán was sort of the seminal influence, and Clark was the builder. That's the way it appeared to me.

GREENBERG: You were von Kármán's student. What was he like? How did he relate to people? What was the atmosphere among the Kármán entourage? Did he deal with Americans the same way he dealt with Europeans?

STEWART: In the first place, you've got to remember that Kármán was hard of hearing, and he spoke his own version of language. So anybody who was working with him spent the first few weeks learning how to accommodate to his version of the English language. I often thought that he probably spoke Hungarian just as badly as he spoke English. I remember one story that maybe supports that. Somehow the subject of Esperanto came up, an international language. And Kármán said, "Well, there already is an international language; it's called bad English." [Laughter]

And this was especially the problem with his seminars. In his seminars there were always people who came in and couldn't understand a word he was saying. I remember Clark Millikan telling a story of one of Kármán's seminars when he first came to Caltech. It was reputed to be a seminar on turbulence. And Clark said that he started out in his introduction and kept talking about cows—and what the heck does that have to do with it. But it turned out to be "cows" spelled "chaos." [Laughter] So I'm sure with any strangers, this was a serious problem. After a month as a student, I remember never even noticing it at all; I communicated perfectly well with him. I didn't even talk the special language with him; I used perfectly standard language with him. His problem wasn't in understanding standard language; his problem was in speaking it. And it wasn't a vocabulary problem with him either; it was just sounds. But he was hard of hearing, and I expect that had a lot to do with his speech patterns.

In the first years when I came here, he was remarkably patient with the students. And that was kind of a contrast with some of the others. H. S. [Hsue-shen] Tsien, who was a year or so ahead of me in these affairs, really wasn't a very good instructor because he was so impatient with students and he was often cool with them. Now, Kármán in his later days occasionally was cool to people, too. He lost some of his patience. Again, this is a hearing problem. I remember seeing him in a conference outside Caltech and some things were going on that he thought were nonsense. We saw him push up to the table and put his amplifier for his hearing aid on the table, and then he turned it off and leaned back and shut his eyes. That was kind of cruel. But when Kármán turned off his amplifier, it was a pretty good signal that whoever was speaking had better come to a conclusion fast because he wasn't getting anywhere after that.

GREENBERG: Who were the people he was particularly close to here?

STEWART: There's a cartoon up on the wall you might want to look at that gives a clue to this. That was a cartoon that Frank Malina, I think, gave Kármán on his birthday in 1940. You see, that is presumably Kármán's conference table, and here I am, Duncan Rannie, Tsien, Biot, and Kármán, Malina, Sears, Bollay, and [A. E.] Lombard. And here are the Saints Michael and [Sydney] Goldstein, and some of the angels. [Laughter]

You mention aerodynamics quite often in connection with Kármán. I think you should remember that the structures and the structural dynamics were at least as important, and I suspect he probably spent more time on those than he did on aerodynamics.

GREENBERG: His earliest work was in structures; his PhD is in structures.

STEWART: Yes. One of the good reasons for my approval of Kármán and Biot as a text was that they had the patience to go through the longitudinal stability of an airplane, which is a fifth order linear system that you worry about in detail. And they also worried through some pretty good little, simplified, flutter models. In the picture there—for example, Biot, I think, and Lombard—Lombard was a hundred percent flutter problem. By the time that cartoon was made, [E. E.] Sechler was on the staff, and his interests were completely in structures. I guess his thesis work had been the test data and correlation of theory on the panel-buckling problem, and he put that in a form that would be useful for design purposes.

GREENBERG: Making fuselages out of sheet metal, is that it?

STEWART: Yes, and wing structures. If DC-3s fly in rough weather, you can see the wings deflect and you can see the little panel bucklings move in and out the wings. On a small airplane like that, the buckles are more visible; but nowadays, most airplanes are bigger, and the panels have thicker metal and you don't see the thing the same way. Anyway, on the relatively small airplanes like the DC-3, panel buckling was a very obvious phenomenon. And if you wanted to make them efficient airplanes and light enough to carry a useful payload, you had to operate in that range of parameters.

Kármán had a very good, you might say, social aspect, not only with outside people, but also in the social context here. If you were at his house for a seminar and he had coffee or something, that was a social occasion; it wasn't a business occasion until you sat down for the seminar. He separated these. His social relations with almost anybody were impeccably courteous. And almost everyone found it possible to get along with him in some way or other, even if they didn't agree with the things he was doing. Until, as I say, in his last years, he did get kind of impatient with incompetence. But in his earlier years, he was remarkably patient with students. He'd go a long way to get a good student. I have the impression that he went a long way to find Allen Puckett back at Harvard. Incidentally, in talking about other schools, I didn't mention Harvard. By 1940, Harvard was a significant place also. In 1930, it was just in the general engineering sense—mechanical engineering and also science.

GREENBERG: Yes, Bollay went to Harvard from here and helped found the department. And then they picked up Richard von Mises.

STEWART: Anyway, Allen Puckett was one of Bollay's first students, and Kármán got him to come back out here because he needed somebody to work on the supersonic wind tunnel design that he thought the Army ought to put in at Aberdeen in order to make their ballistic missiles more understandable, to raise the general engineering level of design. Puckett was one of my students. During the war Puckett was off busy doing other things, and so his degree work just stopped. Then in 1946, we convinced him he ought to clean it up and get his degree. By that time, Kármán was gone; so I was formally Puckett's chairman. And that, incidentally, is another story that illustrates, I think, the wisdom of these outside contacts that are so important in interpreting Kármán.

During the war, all the services tried to make a guided missile or two, but they never could do it. The interaction problems were always too severe, and they never really worked. And by the end of the war, when we got into it, the problem we were working on was how can you make systems that are this complex and make them work? The Air Force had half a dozen projects going on. In ’47, I think it was, the Air Force leadership asked JPL—and I’m sure Kármán was involved in some way—to form a small committee to go around and make technical reviews of each of these projects and to comment on them back to the Air Force offices at Wright Field. I’ve forgotten who was involved with this; I was, and I remember Paul Meeks, and I think Bob Parks. At any rate, we went through and did our reviews and wrote our confidential comments back to the sponsor.

One of these programs was the Hughes IR missile. By that time, as I recall, Hughes had a pretty good electrical engineering staff. I’m not sure whether [Simon] Ramo and [Dean] Wooldridge were both there at the time, but I think they were. The electrical engineering aspects of the thing were pretty good, but they didn’t understand ordinary f=ma in this broader context. One of my duties in this review we’d done was to look at auto pilots and communications. I remember looking at their auto pilot design. At one point, in the linear control matrix, there should have been a fairly large term which represented the Coriolis-force influence on the dynamics. They didn’t have a Coriolis force in there; they’d made an assumption that the matrix was symmetrical, so they put in a term from the corresponding symmetric point, which was too small by a factor of about 10^4. [Laughter] Anyway, it was clear that they needed to have people who understood ordinary mechanics in a broader sense, and not just electronics, in the organization.

The Air Force bought the recommendation. In fact, one of the first things they did was to offer me a job over there at three times what I was making here. I wasn’t interested; I liked the way things were. But Allen had come to the point where he really was feeling a little crowded here. By that time, he had acquired a worldwide reputation as a designer of supersonic wind tunnels, and he said he thought he had more to him than that. He was feeling squeezed by the academic situation here. So, when the Air Force got Hughes to come to him, he took it. In a year and a half’s time he’d straightened them out, and they’re a multi-billion-dollar-a-year business now. They were falling flat on their faces before he went there. Well, again, it was really Kármán’s doing. The fact that Allen has made that such a good thing can’t help but redound in the favor of Caltech.

GREENBERG: Is there any special reason why you prefer academic work to industry?

STEWART: Yes. At Caltech I found a kind of top-level view—you had government connections and top industry connections. In industry only the half-dozen very top people have that kind of a view. Most of the people involved in engineering specialties are restricted pretty much to a company view. Now, I've had a large number of industrial relations, but it's mostly been as a consultant to top management in some form, where I get the same kind of view. It's just that I enjoyed the top-side view that Kármán had teased me with.

GREENBERG: So, what you had to do, if you wanted to go into industry was do what Puckett eventually did—take over the operation.

STEWART: Take it over and then it's fun! Of course, I never took the entrepreneurial step. The Caltech position was even better, from my viewpoint.

HOMER J. STEWART

SESSION 2

October 19, 1982

Begin Tape 2, Side 1

STEWART: After our discussion last time, I thought of two or three items that I wanted to mention. We talked quite a bit about Kármán's role in making this place somewhat unique, and my impression that the really big thing was that Kármán pursued and developed all kinds of external relations. I looked through my drawer and found a set of class notes that Clark Millikan had put together in 1935, I think; a little later I can show you some of the references he used which might be interesting in defining the state of the business at that time.

There are a couple of other aspects of Kármán's era here that I thought would be interesting to talk about a little bit. There was an inside analog of that drive of his to touch base with all kinds of people who had any connection with aeronautics. This illustrates a characteristic problem in engineering schools in general and engineering firms in general. That is, it's very straightforward to think of organizing a school along discipline lines—physics, mathematics, civil engineering, mechanical engineering, electrical engineering, and so on. In the same way, it's straightforward to organize an engineering firm on these lines, but an engineering firm doesn't work this way. When you've got a project to do, you have to organize a project management office; you have to pay attention to the project needs; and in general you reassign your people from the discipline organizations to a project organization of some kind, whether you actually do it legally or just in fact. So almost all engineering firms really are matrix organizations with some project structure, which is aimed at getting a product out the door; and it's a discipline structure, which is, I guess, a means of preserving the ability to handle the discipline in the low times when that discipline isn't being called on heavily for a project.
Well, the same thing happens in universities. Some organizations are really oriented toward the disciplines, including the majority of university organizations, and some are organized more broadly. The aero department here, as Kármán organized it, was not a department of aeronautical engineering, but a department of aeronautics—in other words, with the freedom to look at anything that bore on the field. He felt perfectly happy putting Irv Krick in here to

develop a meteorology sub-department, because that was a very important piece of aeronautics. They felt perfectly happy in encouraging the development of rockets here, because that would very likely become important.

An important part of this kind of approach which, in this case, was intended to match an industry rather than a discipline, is that it does give a broader viewpoint. This gives you a better chance of finding new technology areas that ought to be of interest and that may have application and may develop into new disciplines as far as that's concerned. In turn, this helps to suggest new research projects that will be timely and productive.

Now, at the time when I came here, there had been some small things in the engineering department which had the same kind of function, including the work of Bobby Knapp and the big water turbines, and the high voltage lab over here, for power generation. But by the end of the thirties, neither of these was really working that way anymore. But in the thirties and into the forties, the aero department and the wind tunnel with its relations through outside really worked as an integrating theme that brought practically all of the problems of the industry into the school. Now, in the forties and fifties, the industry changed. Practically all of the big aero companies, in the first place, got specialist engineers, and they got specialist equipment. Most of them got their own wind tunnels. So the wind tunnel didn't carry that same function. On the other hand, the GALCIT Project No. 1, which later became the Jet Propulsion Laboratory, carried out the same kind of an integrating function very well and very effectively for twenty years. After 1960, when it went to NASA, it didn't perform that function as well because it was just a different game. But up until 1960, the Jet Lab performed that very well. However, after 1960, JPL developed strong contacts with the physics and geology groups at Caltech that I am sure helped with their contacts in the outside world.

Now, at the present time, it looks to me like the function on the campus which is closest to doing the same kind of thing, is computer research. They've gone out and made connections with industry groups, for example, that have been brought in and are working; and that seems to me a very similar theme. Now, I'm not directly involved in that so I can't say.

GREENBERG: You're talking about aeronautics now or more broadly?

STEWART: Oh, no, this last remark was the campus as a whole—the group that seems to be carrying out that kind of a function, in the engineering part of the campus anyway—is the computer group. The applied science activities are sort of a halfway step in that direction, although it tends to stay closer to the mathematics than it does to the industry. So we do have some of that matrix activity going on which tends to produce a cross-fertilization of ideas. And I think it's an important function not only in an engineering firm but in any engineering campus.

GREENBERG: This brings us to a problem that I wanted to get back to. In the thirties, it was the wind tunnel that was a key in the cross-fertilization process.

STEWART: That was the closest connection with industry that we had, yes.

GREENBERG: There seems to be a notion that the work of GALCIT's theoreticians in the 1930s was not applied to aviation at that time. Is that true? Commercial aviation took off during the thirties in Southern California.

STEWART: Well, I don't know that I would say that. By 1938, Douglas was designing their own air foils, using largely the standard links which we had a strong effect on, in the sense of educating people. The thin-sheets structural techniques that were exemplified by Ernie Sechler's thesis, for example, became universal.

GREENBERG: Beginning about when?

STEWART: Well, I think the first airplane that was built with thin-sheet technology, which was about the time that Ernie was doing the work, must have been the Northrop Delta. But the DC-3, for example, used the technology—100 percent thin-sheet structure with ribs and stringers. I think I would say that the structural ideas penetrated just like greased lightning. The aerodynamic air-foil design ideas were a little slower because the NACA had developed quite a backlog of air foils where they had useful data, and they weren't bad air foils. In some applications, it was pretty much a question of designing something for which you had good experimental data. However, Francis Clauser at Douglas was tailoring the air-foil design so that it really fitted the application. Then you had to get the last step of accuracy in your knowledge by doing wind tunnel testing. In most of the small airplanes built around the country they really didn't have the resources, so that there was never any intent of doing wind tunnel testing. There, the engineering design philosophy was: let's design this close enough to what's been done before so that we can use the cummulated data base and avoid the expensive wind tunnel test process.

Incidentally, I think it was just before World War II that Douglas decided to try to design an airplane without doing wind tunnel testing. I think it was called the DC-7—there was another DC-7 later—and it was a dog. I think they modified it during the war and made a light military general purpose plane and sold it to the Marine Corps. But it was a dog.

For the larger airplanes, the economic competition was really a dollars-and-cents competition. There, the value of a few percent in aerodynamic efficiency was quite large. But as I said, the Douglas company very quickly developed a capacity to design air foils to tailor the work to their special needs, and they've done it ever since.

You get some interesting stories. For example, you may have heard here quite an uproar and a lot of favorable comment in the last few years on NASA's work on trans-sonic air foil sections and the flattop air foils. The interesting thing is that if you look at the root section of a DC-10, which was built many years before this, you find that it was about that way, because the Douglas people were tailoring their root to fit the same need. Their tailoring came out looking quite similar to these. The difference was that the Douglas people did it as a station-by-station tailoring to fit the local needs. And the NASA people interpreted it as an easily understood concept, which was an important thing, and then collected some appropriate data which could be used by a lot of people with less special needs. Francis Clauser was over at Douglas when they first started doing things like that, back in the period just before World War II. But really, the structural ideas just interpenetrated as fast as they were written, because they were timed right. You know the idea of designing an air foil so that it has an extended laminar boundary layer. It is a pretty obvious idea, and you saw people thinking about it in the thirties. That was one of the bases for the boundary-layer suction idea, for example. Now, as the war came, at the NACA and in particular in England, the theoretical aerodynamicists did quite a bit of work on designing air foils that should have extended laminar flow areas. Some were built, and in the wind tunnel it seemed to work.

The Air Corps in this country built some flight test wing panels to test such things and they looked pretty promising. The British did a similar thing, and it just didn't work at all. It took a couple of years of detective work before they finally realized that the trouble was that the

American tests were done in the desert where there is almost no insect life; but in England they were flying on air fields where, for the first hundred feet of altitude, you've lots of little insects. And as they'd fly along, every time they'd hit an insect, it would splash and then you'd have a turbulence-tripping device and a non-laminar flow. We saw it in the wind tunnel here in a similar way. If you had a nicely polished wing, and a little grain of dust, too small to really see, would hit on this polished surface and stick in the wax or oil there, by looking at it in the light properly, you could see the transition turbulent flow pattern on the polished surface after that. Anyway, they finally cleared up the British experience. The British said, "Well, if it's bugs, you've got to be able to get around it." So they put a cellophane glove over the wing, took off with the glove on, and after they got up to altitude, they just pulled a zipper string and zipped off the cellophane so their wings worked, too. The reason why these haven't been widely used is basically that they're just so ticklish.

GREENBERG: So in effect, some progress was made on this problem.

STEWART: It never became a really important technical application. The problem with the boundary-layer suction is an example. In the first place, there's a heck of a lot of energy wasted in the flow through the pipes of the boundary layer area to suck the air out. But probably the more important one was that the channels were so small and then so easily messed up with dirt or rain and corrosion. Nobody as yet has really used it.

GREENBERG: You mentioned that eventually people were able to build low-turbulence wind tunnels. Is there any connection between them and the theoretical work that was done on turbulence during the thirties?

STEWART: Oh, very much. Yes, in fact, I think the most extreme thing of that sort was done by Klein. I think Millikan was involved, too, where they used a blimp as a test platform. They figured that if you got up in the atmosphere above the thousand foot level that marks the turbulent zone of the surface, right in the middle of a big clear air mass you ought to have a real low turbulence level in the ambient atmosphere. So if you put a model down well below a blimp and pulled it through the air, it ought to give measurements which were characteristic of really low turbulence. And they did that. Incidentally, in recent years, the same thing has been reproduced in the deep ocean. You take a streamline shape and drop it into the deep ocean and that's another way of getting very low-turbulence environment.

GREENBERG: Did the ability to test in low-turbulent conditions result in progress in aviation design?

STEWART: Yes. Really, what it showed was that there was a collection of problems, including not only drag and the economy of flight, but also the maximum lift characteristic, stall characteristics—a whole collection of problems where the aerodynamic data you wanted was for a low-turbulence medium. If you really wanted to reproduce those kinds of data, you had to have low-turbulence test data in the wind tunnel. People knew how to do that in principle. The fact that it was important wasn't really recognized until the early thirties. It was before I came out here in '36 that this was first recognized as a big problem. This was kind of disappointing to the fellows at the NACA because they'd just spent a lot of time and effort doing the work with their various air foil series, testing them in their compressed air, high-Reynolds-number wind tunnel, where they got the high Reynolds number all right, but they also did it in a way that they got data in high-turbulence level.

GREENBERG: Kármán claimed that at NACA they didn't understand that turbulence was the issue behind it.

STEWART: Well, I think that may well have been true in maybe '31 or '32 or '33. But by the time I came here, that problem had been wrestled through. Now, it happened the wind tunnel here, in comparison to the NACA compressed-air wind tunnel, was a very low-turbulence wind tunnel. But at various times during the thirties, for some special reason, for example, when Kármán finished his isotropic turbulence experimental test work, we wanted to decrease the turbulence level even further. It was straightforward to do. We did it with screens in the forward section. So, the ideas about what to do to make the thing into a low-turbulence tunnel had been around a long time. What changed in the early thirties was recognition of the fact that it was important for some practical design purposes. I think the work that Klein and Millikan did was pretty key in this recognition.

GREENBERG: You were on the wind tunnel crew in 1936 and the years following. How did the wind tunnel work? Did you use it primarily to obtain data? Did you use it to test theories?

STEWART: The use of the wind tunnel changed over the years. I think I mentioned last time that when I came out here to California, it was just about the time that the industry started to recognize the idea that an aerodynamics specialist was a useful adjunct to the engineering crews. Also, it was just about that time that it was generally recognized that wind tunnel testing as part of a development process was useful. It made a significant part of the margin which made the DC-3 such a fantastic commercial development. It was several percent better than it would have been otherwise. And that was a fantastic commercial margin. In fact, I remember in the fall of 1938, the Maritime Commission came out with a report that in essence said, "From now on, nobody will ever make a ship for passenger transport again on the basis of economics; it could never compete with an airplane. And you'll make ships for military purposes or for good military backup for military purposes." The government decided it was worthwhile to keep subsidizing the big passenger ships, but they've even given that up now. But it was a vast difference there.

For a few years, there was an early period where most of the work going on was academically oriented toward showing the usefulness of wind tunnels. Then as that gained recognition, the war came and the demand for use for industrial development purposes came on—because at that time the industry didn't have wind tunnels—suddenly it was 100 percent development testing. By the end of the war, the major companies had their own facilities or quickly built them after World War II was over. For the wind tunnel here, it relapsed back toward a mixed mode of operation.

In fact, in the sixties I would guess that one of its more interesting functions was teaching the automobile industry how to use wind tunnels for useful purposes. And that started to change just about a year or two years ago; GM finally got their big wind tunnel going. It must have been close to 1960 when they first put out a little research contract here to investigate the utility of a wind tunnel for automotive and truck design. Bill Bettes carried out the tests. It was kind of interesting that one of the first things we discovered here was that if you really wanted to learn how to design the wind tunnel for this purpose, their road-testing instrumentation and techniques had to be upgraded to get better quality data so that you'd have something to compare with. But once they did that, the question of the utility of the wind tunnel could be thought about rationally. And obviously it did turn out that it had quite significant applications.

GREENBERG: You seem to be both theoretician and experimentalist. Von Kármán once said you were a great hot-wire anemometer manufacturer for the turbulence experiments. Were you unique in the GALCIT or were there other people who could do both? Von Kármán, for example, does not have much of a reputation as an experimentalist.

STEWART: Yes, that's true. As I said the last time, he was usually the seminal influence that started something going and somebody else carried it out.

At any rate, I was different than the other students here in that I had had an interest in electrical engineering and electronics in my earlier years. So I was able to do these things. From the electronics standpoint, I had a fairly good theoretical understanding. I could analyze any kind of an electronics circuit. My experience level was not very large, but at least I had an exposure to these things and that was different from most of the others. We did have an electronics technician here, Carl Thiel, who was around for a number of years and then spent most of his life up at JPL after that. He built a lot of the electronics equipment in the early days. It just happened that I did the particular boxes that were needed for Kármán's isotropic turbulence work. I would say I'm really not an experimentalist in fact, although I am in spirit because I believe in the role of these things. I can do some things, but I'm really not awfully good with my hands. If I pick up a tool, I'm more apt to use it in a suicidal wounding. [Laughter] But I have done some things, built lots of model airplanes and radios and things of that sort in my younger days.

I remember doing one thing of this sort even much later, about 1947 when we were starting to build the Corporal rockets. One thing we wanted to do was to improve their controllability, and that meant we wanted to go to a Delta-wing kind of tail on it. After we'd come to this point on paper, I was at home charged with entertaining my two year old son who was sick with the flu one weekend. So I decided to entertain him with a practical thing. I got out some balsa wood and made a model of what this new configuration we'd been calculating might look like; I got out some BB shots so I could weight it and get the CG in the right place. I took it and the boy outside. I spent a couple of hours throwing it into the bushes where it wouldn't get damaged and changing the center of gravity and throwing it again, and finally narrowing and finding where the neutral point was between stability and instability. The interesting thing was that with that very crude test I came up with an answer. And about a year later we finally got a proper wind tunnel test. The difference was 3 percent of a diameter, which is about the accuracy of the wind tunnel test.

GREENBERG: So you can learn a lot just by going out and tossing model airplanes.

STEWART: [Laughter] Well, the facts were that I did get the right answer that way. I didn't have any assurance of how good the answer was. So it wasn't a waste of government money to go ahead with it. [Laughter] But what tickled me was that it did come out to such a close coincidence

GREENBERG: Were you surprised?

STEWART: Not particularly, because that particular question is a low-Reynolds-number, low-speed problem, so I wasn't really surprised. But I've always had a good respect for the experimental process and the limitations of the mathematical processes. I think in many ways people try to use mathematical simulation without properly recognizing its limitations and inaccuracies. I don't know if Hans Liepmann told you this story, but there's a marvelous story of the abuse of this perfectly modern computer by all kinds of people. By 1960—this is a story of about fifteen years ago—practically every engineering firm and every academic institution in the country had computer programs by which they in principle could compute supersonic flows over a curved surface. One of the people back at the Naval Research Lab saw all these computations, and he wondered just how good they were. He finally had an idea, and he put out a little RFP for computation. It was a very simple problem, just a blunt-nosed, axially symmetric body; because it was blunt-nosed, the supersonic flow would have a detached shock wave, and you'd have an expansion after that into a supersonic flow again. Anyway, he wanted the calculation of the pressure distribution over the thing. He made a model, and then they tested it and got a good experimental test number. So, he had a good criterion for evaluating all of these things. He procured several dozens of computations. The answers that came in varied all over the map, with the pressure perturbation from the ambient between the various answers varying by factors of three- and four-to-one. So, obviously, some of the programs were pretty bad. There were probably programs that were useful for some other application but in this particular one it didn't apply very well. So I guess I've always been interested in the numerical substitution for experiment but, more than most people, interested in the question of how you validate the program for the experiment you're trying to replace.

GREENBERG: I gather from people in aeronautics that these computer experiments or computer simulations are becoming more and more a fashionable thing.

STEWART: It goes through cycles, and at the moment there's a very fashionable cycle in trying to compute turbulent flows with computers. But again, there are problems here, because if you go back to what Kármán and Chapman and all the rest of them did with the turbulence flow problem—if you try to take the mathematics and carry out logically in the way that Reynolds started to do back in 1870, by going to the second-order tensor and the third-order tensor, then it's not a closed system. Nobody ever solved it, even in isotropic turbulence, which is the simplest form. After Reynolds in 1870, people said, "Well, we'll close the system at the Reynolds stress level by assuming some kind of a second-order closure assumption. The German's call it an Austruch coefficient, or the effective viscosity coefficient. Or you could use a Kármán mixing-length hypothesis. They haven't really gotten beyond that. They're still closing them artificially at that level. So to the extent you're doing the calculation with a problem which had been done before where you could make an empirical fit of some sort, then you can get quite good answers out of it. If you try to do it in a place where the second-order fit doesn't work too well, you may not even get the right general phenomenon. For example, in meteorology, when Helmholtz, I guess it was, was the first one to try to apply modern ideas of fluid mechanics to meteorology, he put in vortex sheets and things of that sort. He found he couldn't even make a model that would bring the Westerlies down to the surface. The Westerlies are a big important phenomenon in the atmosphere. In the thirties when Rossby and some others were working on this, they finally found that you could make—

Begin Tape 2, Side 2

STEWART: [Continuing] You had to make the turbulence non-isotropic by factors of 10^5 or 10^7, in order to reproduce the phenomena even. In other words, the turbulent mixing scale in the horizontal was 10^7 larger than the scale in the vertical. And the same thing apparently holds in the deep ocean and in the upper stratosphere. The motion is heavily non-isotropic. In fact, it's enough non-isotropic that some of the meteorologists are doing theories nowadays on two-dimensional turbulence [laughter], where they completely neglect the out-of-plane turbulence.

We had a lecture here a couple of times over the last ten years by a fellow from NCAR—to my mind, their best theoretician. He was speaking of the various numerical computing programs they have for computing the weather, where they do it as an initial value problem. They start out with an assumed initial state, and then integrate it using the computer and the little blocks of atmosphere. Their closure assumptions at the second order are good enough so that they do show the right kind of phenomena; they do show storms progressing from west to east, and they do show the Westerlies at the surface in the mid latitudes. But they're not very good quantitatively. In other words, the quality of the integration is very poor. His conclusion in the last seminar he gave was that if your prognostication time was as much as seventy-two hours, you were better off using the climatological average than you were using the computer result. For eight or ten hours, they hold together pretty well. And even the things you see in the newspaper are usually about thirty hours by the time you see them. They're starting to get pretty fuzzy by that time. Now this is strictly a closure problem. What they do is exactly the same thing that the modern aerodynamic fellows are so encouraged by. The aerodynamicists are working with problems generally where the turbulence field is much more nearly isotropic. They have experimental data which is not too far off so they can make a closure assumption of some kind. But that is not, to my mind, a fundamental solution of the turbulence problem. I think a fundamental solution still requires that the mathematics go from the basic physics. There is still no reason to believe that anything other than the obvious Navier-Stokes equations are necessary to govern any low-speed phenomenon. What you don't know is the statistics with which to smooth the system—because the mathematics automatically generates an infinity of non-stationary solutions—and how to smooth the multitude of solutions to get an average, which is what you really observe. And I don't think that problem has really progressed since the thirties, except in the meteorological sense where the recognition of the non-isotropy has permitted models to work, to some extent. Non-isotropy is probably also very important around the jet mixing at the edges of jet engine exhaust flows. But I don't think they've made any fundamental step beyond that.

GREENBERG: Back to the team in the thirties. Can you make any generalizations about the kind of GALCIT graduates who went into industry in the 1930s versus the kind who went into academia? For example, what about differences in areas of specialty and the kinds of work each did while they were at the GALCIT? Were scarcity of academic jobs and low salaries serious factors in making such decisions?

STEWART: They were. The scarcity of industrial jobs was, too. As I mentioned, when I got here there were two aerodynamicists in industry, and both of them at Douglas. And in that same year, Kelly Johnson got hired from the University of Michigan. Of the group that was here in the thirties, there were an awful lot who went into industry and then went into academics after the war. For example, Bill Bollay left here; he was the wind tunnel crew chief when I came here. He got his degree in '37, and then he went off to Harvard and stayed there a couple of years. He was in the Navy during the war; then he went into industry. The Clauser brothers were the next year in seniority in the graduate group. Francis Clauser, as I recall, left here to go to Douglas and was there through the war, then went to Johns Hopkins and went into an academic career. Bill Sears went similarly. He was on staff here for a year after he got his degree; by that time the war was practically on us and he worked with Northrop. When the war ended, he went to Cornell.

GREENBERG: And there were many people, including Lipp and Root, who went into the aircraft industries and stayed there. I'm wondering if there were differences so that you could tell who was best suited for what kind of work.

STEWART: I wouldn't have guessed right. For example, of the ones we talked about, I would have thought that Bollay would have been very likely to end up in academic work. That was the first thing he did and then he left it and made himself a fortune—died a few months ago, too. It's hard to tell what the war experience did. He was a naval officer for a number of years in the war; he ran research programs for the Navy. It could be he just ended up with an experience there that was really much more appropriate to him.

Incidentally, there was a funny problem of that sort here. You know, as the war came on, there were drafts. Here we had the reverse problem. For example, there were only about five thousand physically able persons in the whole United States of that age group who were deferred, and I was one. Caltech, as long as the military would request it, would turn in request deferment forms. For a few people, that was kind of difficult—for Clark Millikan, for example, and Allen Puckett. Both of them had reserve officers' commissions. The people in Washington and the Army and Navy came to them and told them they didn't want them to be called up. If they didn't resign their reserve commissions, they would have to call them up. So they asked them to resign. That was a traumatic experience for both of them, but they did resign their commissions.

The war did really heavily influence all of us. In fact, the relation of Kármán with the school here was extraordinarily strongly influenced by the war. It always seemed to me that it made Kármán kind of unhappy, too, in the process. I came out here in '36, and I was almost the last student to actually take any of his regular class courses. I took his course in elasticity theory. By '38, the war was obviously coming on, and Kármán got more and more involved with the War Department and the Navy Department people. By the time the war opened in Europe, I don't think Kármán ever gave anything but a seminar class. He had several students who were doing research work leading toward a degree. He just moved out; he was so busy trying to help the people at Wright Field to organize their affairs in a more effective way. He got Allen Puckett to come out here because he also thought that the people in Army Ordnance needed help to improve their capacity. This idea of the supersonic wind tunnel was one important way of doing that.

GREENBERG: I was wondering about the work done on boundary layer and turbulence. They don't sound like the kind of problems that the aircraft companies would have gotten terribly excited about.

STEWART: There were some other things. For example, one important thing was done by Clark Millikan—it was a degree thesis topic—and two naval officers [Russell and McCoy]. They did a project where they investigated what would happen if you could get a high-powered, small enough electric motor into a model so that you could actually test the slipstream effects. That was really quite important because it turned out the slipstream effects were very significant. That was one of the things that rapidly became a part of the development testing procedure. Malina and Jenney did another important work on propeller effects, windmilling, and braked effects.

GREENBERG: A few years earlier, would the people who had been doing the academic research have been less inclined to go out to work in the industries?

STEWART: As I said, there really weren't jobs necessarily available in industry, unless you had some other job to feed you. It's hard for you to visualize that, I'm sure, but it really wasn't a professional area in the twenties and even in the early thirties. It wasn't until 1936 or '37, as I said before, that it became customary to pay all engineers. In a certain sense, in the earlier part of this period, there were more jobs available in academia than there were in industry. By the end of the thirties, it had reversed. In practice, I think the fraction of our students who got academic appointments in those days and the fraction who have done it in recent years is really just about the same; maybe 20 or 25 percent go into academic careers, and another 10 or 15 percent go into government or JPL, nonprofit places. Somewhat over half of them go into industrial connections. I think that kind of mix has not really changed over the years.

Now the availability of jobs, of course, fluctuates. This year, it's been fashionable for people to weep and wail because academic pay levels are so low that everybody's leaving. They can't get graduate students to stay on. As a matter of sport, when I ran across an old speech of [Lee A.] DuBridge's from 1958, I took it in and gave it to Hans; I figured he could use it somewhere. The speech sounded exactly the same. It was exactly the same. The academic career is a little more stable, and that means that when the highs of engineering come along, you lag behind, and when the lows come along, the academic career is more protected.

GREENBERG: What was the situation like for GALCIT people in the 1930s where publishing was concerned? Were the outlets in this country satisfactory?

STEWART: I thought they were quite good by the time I got here. The Institute of Aeronautical Sciences and the *Journal of Aeronautical Sciences* were, I think, very important contributions. I mentioned that Kármán and Bateman had published a number of papers in the *Proceedings of the National Academy*, and I had a couple of papers published there, too. That seemed to have been more important in the earlier years before I came here. I think Clark Millikan wrote a little paper, an abstract of his thesis work, which was published that way, too. The NACA, in general, had a policy of publishing only their own things. On the other hand, if there was some extensive document that you couldn't very well get published, they would sometimes give a little research support, which really was putting the work into a format that would fit their requirements and then they would publish it. For example, as I recall, Oswald's performance stuff was published that way. That was a route that was used mostly for things that were fairly extensive in their space requirements.

GREENBERG: What about the *Journal of Applied Mechanics*? What was your opinion of that?

STEWART: It was not widely used in the thirties. There were a few papers published in the ASME [American Society of Mechanical Engineers] journal, too.

GREENBERG: Yes, *Transactions of the ASME*. The *Journal of Applied Mechanics*, which was kind of the sequel to that, I think, grew out of it.

STEWART: Yes, there were a few papers published there. And I have a vague memory of Maj Klein publishing something in an SAE [Society of Automotive Engineers] journal, but that was pretty rare, too. By the time I came along and was sensitive to these problems, the *Journal of Aero Sciences* was the standard outlet.

GREENBERG: Kármán at various times seemed to complain during the thirties about the lack of outlets.

STEWART: I'm sure he did in the early part, because Kármán and Clark were part of the moving organization that started the IAS going. I was still at the University of Minnesota in those days. While I subscribed to the journal quite early, I still wasn't in on the active group developing it. By the time I was here, practically every month you'd find something from somebody at GALCIT in it. We were a major part of its clientele.

GREENBERG: Can you summarize your PhD work at Caltech?

STEWART: Yes. I guess I need an introduction for it. In the summer of 1937, Howard Hughes decided he wanted to fly a plane around the world and make a new world record. W. C. Rockefeller, who was here at the school and was teaching a course in dynamic meteorology, was a man Hughes knew. He had used his advisory help as a meteorologist in some other circumstances. So Hughes approached him again, and asked him to come East with him. Hughes wanted to start from Long Island and he wanted Rockefeller to help him out with this round-the-world flight exercise. So Rocky went off to Long Island. September came and school was due to start. The weather had not been favorable, and Howard Hughes still hadn't done it. He wanted to hold on a little longer so he dragged on. The class went to pot here. One of the students took it over and did what he could with what references and materials were available. This situation dragged on and on. It was obviously unsatisfactory to Kármán.

I remember the first day after New Year's of 1938. I was up in the wind tunnel working on something. Kármán had called a staff meeting in his office down here with Clark and Maj and Ernie. So about ten o'clock I got a message asking me to come down to the staff meeting. It turned out that about that time they got a telegram of resignation from W. C. Rockefeller. When I came in, Kármán asked me if I thought I could take over the class in dynamic meteorology. I didn't know what dynamic meteorology was, but I figured if Kármán thought I could do it enough to ask me, I probably could. So I said sure.

Kármán did it properly; he'd called up MIT and got Rossby, who was probably the world's outstanding name in that field at that time, to come out. Rossby spent the first six weeks of the spring quarter here, giving the class. He taught the class for those days and then spent the afternoons working with me to get me organized to carry on. He got me texts and books and things I should read. It really worked out pretty well. At any rate, this started me in the line of special interest in meteorology.

GREENBERG: In fact, even back in Minnesota, you had fooled around with it?

STEWART: Yes. It was more the electronics that was really my interest at that time. My interest in meteorology per se started with Rossby. We tried to develop it in the class. [Laughter] Now, I mentioned that Rossby had done some work with the general circulation of the atmosphere and finally found turbulence models that you could make which would at least reproduce the phenomena of the Westerlies. This general problem of the nature of the atmospheric turbulence effects, which were clearly very different from the kinds of things we were used to in ordinary technical turbulence, was an area of interest at the time. What happened as a result of this connection with Rossby was that first Rossby got a part-time job for me with the Weather Bureau—this was in 1938. It was a grant [laughter] to do research. Rossby was the person who was supposed to work out the subject matter. This finally developed into my thesis. In fact, Rossby was really my thesis advisor, while Kármán was my committee leader. Of course, Rossby and Kármán were friends, and it was a good arrangement—the way Kármán wanted it, anyway.

What I actually did primarily was to investigate the question of whether the same kind of phenomena which in a low-speed two-dimensional flow leads to the Kármán Vortex Street—whether there is an analogy in that to the gross features of the atmosphere. In fact, it looked pretty reasonable that there should be. I ended up by making it on the scale of the horizontal distances in the atmosphere. The atmosphere is really very, very thin. So I made a very thin one-layer mathematical approximation to an atmosphere and took into account the Coriolis accelerations and these things which are different from the ordinary, two-dimensional case. It turned out kind of interesting. The structures which appear to be the most likely to show up at long distances from this thin atmosphere were the high-pressure zones, like the Azores high and the Pacific high. For the theoretical result where I did the mathematics on a rotating disc, not on a rotating sphere, I tried to put in scaling that corresponded as closely as it could to the atmosphere. The result said that there was indeed a stability analog to the Kármán Vortex Street and that a ring of three, four, five, or six vortices, like the highs, would be stable. Small integral numbers provide a stable configuration and the same kind of a causation argument could be made. If you look at the atmosphere, the Azores and the Pacific highs are typically about 120 degrees apart. The third of the member is off where the Himalayan mountains are. [Laughter] I've never seen any data that bears on whether there's any corresponding high-level collection of vorticity there. The basic argument sounded reasonably good.

I think it's reasonable that such a factor is a significant factor in the earth's atmosphere. Long-distance couplings of this sort are, to the extent that they are significant, probably due mostly to these high-pressure zones. It gave long-period things—for example, the natural periods for this three-cell model, which looked like the one that probably fitted the earth best. The shortest period was on the order of, but not a rational fraction of, a year. The next frequency was eight or ten years. I guess what I found there from another standpoint was a purely terrestrial mechanism which shows good logical reasons why the earth's weather should not reproduce strongly on an annual cycle. There should be significant variations from a pure annual cycle.

After I got my degree in 1940, I worked up some of this periodicity data a little bit more. I was asked to give a seminar up at Stanford at the mathematical seminar. I was going to go through this thing, and the paper would be published in the *Bulletin of the American Mathematical Society*. I was a little slow getting the text put together into what appeared to me an appropriate text for publication in the *Bulletin*. By the time I finally sent it to them, it was the fall of '41. They scheduled it for publication in February of '42, and then the war started. Anyway, in that paper, I said that this might have application for long-range forecasting. Someone in Washington decided that was grounds for censorship, so they censored it. The *Bulletin* had already gotten far enough so that they had printed the reprints they were going to send me, and I couldn't even get a copy of my own reprint. By the time the war was over and this was out, they had lost them, so they had to set the whole thing up again. Finally, it did get published in '46, as I recall. [Laughter]

GREENBERG: I hear that sun spots are supposed to be able to produce variations in weather in a periodic way over a long term basis. Have you ever gone back and tried to actually see if you could, over a long period, account for certain things?

STEWART: No. I don't think so, because the model I had was the model where the actual timing that appeared was a function of the intensity of the structures. I know that the structures don't have an intensity; they have a fluctuating intensity, and they have a mean. I think it would be unreasonable to expect the kind of phenomena that I was thinking of there, to have a real period. What you would expect were motions which would be pseudo-periodic, and if you wanted to put down zero crossings, you'd expect it to show periods that flop around in a certain range, perhaps. But you wouldn't expect a real period.

GREENBERG: What were the circumstances behind your being kept on here after you finished your PhD?

STEWART: By 1940, clearly the war was almost on us; it was already going in Europe. It was only a question of time. There was a significant need for additional personnel here. I think by that time Kármán had talked of keeping me, and felt bad that they'd let Bill Sears get away. On the other hand, the choice of academic appointments is always kind of a strange thing; so there had to be at least one strange aspect. Kármán had introduced me to the people back at the University of Pennsylvania. I did receive an offer as an instructor, or assistant professor, from them. Kármán used that offer internally here as the basis for justifying my appointment as an assistant professor, which I guess, is not an uncommon way of doing things. [Laughter] Remember, in those days the academic appointments were nominally nine month appointments, and summers were something else. Even in the summer of 1940, we already had a fairly large class of people that the military wanted to send us for meteorology. It started building up that summer, when the buildup for war training started. Frieda and I had three days off over a weekend for our honeymoon.

GREENBERG: Was applied mathematics an issue around the time of the Second World War?

STEWART: I do remember some little things that might be described that way. For example, back at NYU, I remember going to NACA committee meetings and hearing them mostly talk over beer—between the meetings, not at the meetings per se. I never did quite understand what the problem was. I got the impression that the motivation was sort of like the motivation that I mentioned Kármán had had for thinking of this as a department of aeronautics rather than a department of aero engineering. That is, applied mathematics had a broader application than any one discipline. Felix Klein had started out with that idea. So I thought this meant that there ought to be a cross-disciplinary function, which could serve some of the same kinds of things as a matrix organization. I was never involved in the discussion.

GREENBERG: The war effort gave rise to the Richardson-Prager program at Brown, the Brown applied mathematics department, and eventually the Courant Institute came out of that. The war seems to have had some role in all of this. The department of aeronautics had been going on at Caltech for at least ten years at that point. Did you have any opinion about these other developments?

STEWART: Well, not strong. As I said, looking at it from my standpoint, they were trying to do sort of the same things that we had done here.

GREENBERG: Was there anything novel about it all?

STEWART: It looked to me very much like Kármán could give a seminar one week on turbulence and the next week he'd be giving a class on theory of elasticity. Those are very different disciplinary subjects. But, what the heck, from an applied mathematics standpoint, they're not much different.

GREENBERG: Did the people who organized these other programs out East understand how things went on here—what this place was like and what the GALCIT was like?

STEWART: I really don't know, because I didn't know any of them at NYU, or—you mentioned Prager—I didn't know any of them that well. The few times I crossed them at meetings, they seemed to be talking what seemed to be sensible. There should be a broader way of looking at the problem than the way it came in some institutions. I guess I thought that it was what I hoped would be a successful reaction against the ivory tower where you put the elasticity man in one box and you put the fluid mechanics man in another box, and then build a wall between them. I didn't think of that as a healthy thing, but you saw it nevertheless in a few places.

GREENBERG: Ivan Sokolnikov, who wrote the text that you used as an undergraduate, was a faculty member at that first Brown University summer session. I gather from a number of sources that his summer course was about as inspiring as that book that you used. In other words, his book really didn't get to the heart of certain problems—and his course didn't either.

STEWART: My thought was that he and his wife, who wrote the book with him, were really interested as pure mathematicians who thought that mathematics ought to be useful. They were lacking in the experience level with the utility. That was my interpretation after using the book. I thought it was a good attempt, and it was the best one around at the time. But it wasn't very successful.

GREENBERG: In fact, I think the Brown program was criticized for not really having the kind of industrial connections the aeronautics department here had during the thirties.

STEWART: Yes. I think we all knew the story of Felix Klein. We all felt that what we were doing here was somehow a modern interpretation of Klein's 1892 thinking. I saw these symptoms of the applied mathematics activity in the East as a move in the same direction, but a little closer to Klein's original thought.

HOMER J. STEWART

SESSION 3

November 2, 1982

Begin Tape 3, Side 1

STEWART: There are more ASME things here that I remember [*Transactions of American Society of Mechanical Engineers*]. There's a marvelous one that reminds me of something by Alcock and [Charles] Sadron, that was published in the journal *Physics*. For about twenty years after that, we had about fifty gallons of salad oil down in the basement, because they had made a salad oil tunnel. Salad oil has a double refraction characteristic; when you put it under shear, it changes its optical properties. By using polarized light, you could show changes of polarization and velocity gradients.

GREENBERG: Do you still conclude that there were problems in getting things published in those days?

STEWART: In the twenties there were problems mostly because the industry was so tiny. I don't think there were any real problems after the IAS got going. I remember that in every year's issue there were several papers from people here. Incidentally, have you ever run into the name Sadron before?

GREENBERG: No.

STEWART: Charles Sadron was a French student who came over here in 1932-33 just before my time—I never knew him personally. He stayed here for about three years and then went back to France. After the German conquest of France—about 1942 or '43—he was grabbed by the Germans and thrown into one of their death camps, Nordhausen, the one where they were manufacturing V-2 bombs. He wrote a little book on his experiences, which is quite interesting.

To keep it brief, the problem with the V-2 manufacturing was that it was not coming up to what it should have been—they should have been turning out about eight hundred a month and they were only turning out about four hundred. The SS came down to the management and said, "Get it up to eight hundred, by God, or you can join the workers." The problem was that they didn't have enough lead personnel to keep things flowing, so they went to the workers and made a deal with the ones who had the right background: in return for becoming foremen, they'd feed them enough to live. Sadron was put in charge of inspections. He said that after a while, as near as he could tell, there was no German above him inspecting his inspections, and so after that, he did what he could. I don't know if you remember the V-2 history, but they were more dangerous in the launching area than they were anywhere else because so many of them exploded right away.

A little later, the SS came in again and said, "Now you've got to double it again." This time it was physically impossible. Again, the management had to do something about it because being an ordinary worker was pretty unpalatable. They organized a system where every afternoon they would close up, and check off all the V-2s that were almost finished. They'd trundle them off into the depot in the forest nearby. Then at night, with the help of some of the slave labor there who had been given survival privileges, they'd steal them back and put them in the plant. The next day they'd finish up the V-2s again. Each one got checked off twice. Things were so disorganized in Germany in the last year that this went on for over six months before the final collapse. Nobody was ever caught.

Anyway, with the various Tech intelligence groups going over there—including Louis Dunn and Clark Millikan—they knew where Sadron was. They wanted to see if he was still alive. Louis Dunn was in the first party to go into that factory campsite. He found that Sadron was still alive, called Clark Millikan back in Paris, and Clark called Sadron's wife and told her he was okay and would be home—a happy ending to a weird tale.

GREENBERG: If we go back to the 1920s, we find theoretical aerodynamicists around at NACA. You mentioned names like Theodorsen, Garrick, Munk; there were some others at NACA whose names I've forgotten.

STEWART: Von Doenhoff, and Abbott, and [Eastman N.] Jacobs.

GREENBERG: You mentioned Draper at MIT. But these were ivory-tower types with little connection to the outside world.

STEWART: They really didn't work in industry. Draper was the only one drawn that way. In fact, of all the people in the country, other than Kármán's direct people here, I would say that Draper was the most like him. Draper was more machinery oriented than Kármán. Draper is responsible to a very large extent for all of the development of precise gyros, precise navigation systems, high-quality auto pilots and the inertia navigators for the ICBM. Draper probably had a bigger role in that than anyone. He was more interested in the guidance and control aspect than the fluid mechanical aspect. But the difference is in the field.

Kármán wasn't interested in just fluid mechanics. He also had an interest in structure and structural dynamics. For example, I was just noticing a couple of papers by Biot on earthquake dynamics and critical torsional oscillations on a rotating accelerated shaft. Here's Kármán, Sechler, and [Lloyd] Donnell, the strength of thin plates under compression. That was a very important paper. Here's Klein on the effect of fillets on wing fuselage interference, published in 1934. The DC-3 came along in design in the first wind tunnel test in 1936 and used these ideas. Of course, they'd made the low-wing monoplane the system.

GREENBERG: Was this paper by Kármán, Sechler and Donnell connected with the problem of metal fuselages?

STEWART: Yes. It's on the use of thin-sheet structures. That was a '32 paper—a very important paper. By the time I came out here in '36, Lockheed and Douglas and Boeing were all using the ideas.

GREENBERG: In fact, if I recall, you were suggesting that it was the work in structures where theory really made the direct impact on practice and on design?

STEWART: Yes. The industry was more educationally prepared to accept the ideas of structures. The new ideas on performance calculation, started by Millikan and extended by Oswald in his thesis work, were also immediately adopted by the industry. But a lot of the aerodynamic ideas were very complex. One of the best pieces of research now in this department involves using heat impulses to modulate and put inputs into a boundary layer. Maybe that's going to be a practical way of controlling and getting laminar boundary layers. They still haven't found one yet, but this is the most promising one, I think, to date. It's a problem where the potential advantage is clear.

GREENBERG: Who's working on this?

STEWART: It really was Hans's original idea, but I think it's Dan Nosenchuck who's carrying out the research.

GREENBERG: What was the relationship between the NACA and the GALCIT during the thirties?

STEWART: There wasn't a great deal of direct contact because the NACA was primarily a government laboratory that was funded independently. NACA did their own thing and published reports. Occasionally, if an appropriate topic came along which required more publication space than any journal could afford to give, they would put out a small contract to get a manuscript written. For example, W. Bailey Oswald's paper on performance calculations, their Publication No. 15, was done in '32. So there was some contact. They had a little bit of money to support outside research; NACA did support several small items here in the 1930s. The biggest budget the NACA ever had was for 1957, just before it became the NASA. That budget was for $70 million. Of that, $10 million was for outside research contracts, and that was a big growth from earlier years.

Hans Liepmann, as I recall, did develop relations with them to get funding support for some of his work, even before World War II, although I'm not absolutely certain of that. Hans is the only one who really had close relationships in a research support sense.

GREENBERG: What about the award NACA received in 1928 or 1929 for the development of cowlings?

STEWART: The NACA cowling was one of their great inventions. It solved the drag problem for radial engines. It was a good idea.

GREENBERG: NACA was the first research organization to be instituted. Were they jealous of their prerogatives? Was there a great deal of competition once the GALCIT got going?

STEWART: I don’t think there were bad feelings. They were very upset when the importance of turbulence level in the wind channel in determining things like maximum lift and drag was first documented. But that was because they’d just put a lot of their resources into this high-Reynolds-number compressed air tunnel, and here one of the areas where they’d hoped to get good information was one that you couldn’t get it out of. As you would expect, in all scientific questions where there are some new ideas, the debate is pretty lively sometimes. They didn’t forever deny the validity of the thesis; they accepted it. It was work done here that really tied it down, because the original design of the wind tunnel, by coincidence, was unusually low turbulence. That was a coincidence of original design [laughter]: the reason for its being low turbulence was that it was designed to be high efficiency. It just happened that they had a lot more turning vanes in the corners than some of the others, and the turning vanes were designed with a little more—coincidental—foresight.

GREENBERG: It was designed in the late twenties with Kármán’s help. Did he design the tunnel as a result of earlier turbulence studies that he’d done?

STEWART: No. I think it was done for reasons of efficiency and energy. They wanted to make efficient turning vanes so that the flow would turn and come out smoothly. You wouldn’t have energy losses from mixing the jets, and impinging and mixing. Of course, you need to do the same thing if you want to make low turbulence without putting in losses. You can have a highly turbulent flow, and then you can damp out the turbulence with screens. In the thirties, we made some super low turbulence setups in the wind tunnel where we had screens in the big section to smooth out the flow by another factor of two or three.

GREENBERG: So this was not really an instance of the application of turbulence studies?

STEWART: The application was to the important problem of maximum lift. And then there’s the problem of extrapolating drags to full scale from wind tunnel tests—another important problem. Bill Sears and I used to do these calculations for the various companies that tested their things. We’d do the performance analysis on the specific configuration in the test model which was most apt to give a good extrapolation to full scale. We also had to consider what we could use for the transition Reynolds number. We had to argue through all these things. We usually did pretty well, so that it was recognized as useful by the industry. They started putting in aerodynamicists. It did have a very direct effect, but not in the sense of boundary layer control [laughter]; that's still floating around loose.

GREENBERG: The Kármán-Biot *Mathematical Methods in Engineering* appeared in 1940. What can you tell us about the book? Was it a milestone in engineering mechanics pedagogy in this country?

STEWART: I think it was. I mentioned earlier that when I'd gone to Minnesota, the best available text there was the Sokolnikov text. They tried to do about the same thing that Kármán and Biot did or that we do now in AM95 at Caltech. The Sokolnikov text had two deficiencies. The first one related to the fact that the Sokolnikovs were really mathematicians and not engineers; their examples were poorly chosen. The second problem was that their treatment of complex variables and two-dimensional potential fields was really too brief. Again, the examples were too narrow. Kármán-Biot, I think, still has the problem with the complex variable. The two-dimensional potential problem isn't handled as completely as it should be. In the areas Kármán and Biot did cover, which included a lot of dynamics, they chose real problems that were technically significant. They talked about the longitudinal dynamics of an airplane. They showed that if you went to one limit you got the pure phugoid, and the other pair of roots is the short period oscillation. They even talked about flutter. The examples were reasonably close to the level of the flutter business at that time, which was mostly two degree of freedom flutter systems. However, today, with big transports, you probably have to go to fifty degrees of freedom flutter analyses. You can't even count rocket engine instabilities because there are so many. The ideas developed by these large-degree-of-freedom systems apply in the rocket engine design, and they're applied more empirically than they are directly and mathematically.

Kármán and Biot were outstanding for their choice of examples—real examples at the professional level, and not just the simplest classroom mathematical exercises to illustrate the mathematics. I don't think it's really obsolete even now; in some ways, the texts they use in AM95 suffer from having examples that are too simple to really show the problems.

GREENBERG: Did you know Timoshenko?

STEWART: No. I just met him at a couple of meetings.

GREENBERG: Was there ever a serious effort to get him to come to Pasadena?

STEWART: I'm not sure. I remember hearing some rumors of this sort in the late thirties. I was interested. Again, when I was an undergraduate, Timoshenko's books weren't out, so my professor used *Drang und Zwang* by Föpple as our text in theoretical elasticity. I bought Timoshenko's books as soon as they came out. It was a familiar name. I met him at the Conference of Applied Mechanics in Boston in 1938. Prandtl was at that meeting, too; and that was the only time I ever met Prandtl.

GREENBERG: You didn't know him personally but you know something about his work?

STEWART: Yes.

GREENBERG: Is there any way that von Kármán and Timoshenko can be compared? How did they influence the course of engineering mechanics in this country?

STEWART: Timoshenko did it in a much more narrow field, of course. I'm not aware of any of Timoshenko's work that wasn't really motivated by structural problems. I think a lot of the things that went on here were strongly influenced by Timoshenko's guidance. I think some of the most useful things that Sechler's students did, for example, were dealing with shell-buckling problems. The problem was that empirically you found the reasons why the simple theories that Timoshenko used for examples didn't work very well. There had to be some explanation for it. They finally got the problem sorted out. His work certainly influenced the later work here.

GREENBERG: If I recall, Donnell was actually a student of his and then came here.

STEWART: I wouldn't be surprised; I didn't know that.

GREENBERG: We talked about how you got into meteorology.

STEWART: Yes. Incidentally, you asked about the question of hard feelings between the NACA and Caltech. Meteorology has a very sad story concerning the relations between Irv Krick and the Weather Bureau. I talked to him in the last couple of days. He had a fire, and all his old records were burned. I found a report that I wanted to send him. He was interested in it, so I will send it to him. Let me talk about the sad story first.

I think I told you that the way I started was when Kármán called me in and asked, "Can you handle the course in dynamic meteorology?" I said yes, because I figured if he thought I could, I could. He got Rossby to come out from MIT to spend six weeks here and work with me. At that time, the relationship between Rossby and Krick was fine. Krick's relation to the whole professional meteorological community was fine. Before the war, Krick developed a very good private forecasting consulting business. For example, the movie industry was a good client of his; if they'd lay down an outdoor shooting scene with ten thousand extras and then it rained, you couldn't do it. As a result, they really paid through the nose. They had a strong bias that if they didn't schedule it and it didn't rain, there wasn't very much loss because there are lots of days when it doesn't rain. The only time they lost heavily was when they'd schedule it and it did rain.

Another client of his, having the opposite bias, was Edison Electric, the hydraulic power people. Their problem was just the reverse. If they used their water power faster than the stream flow coming in, the level of the water in the dam went down and the energy you could get out of each drop of water went down. That was a loss you couldn't make up. On the other hand, if you scheduled a certain rate of use for it, and the water level came in extra, that was fine because your level was a little higher and you were getting more out of the water than you had planned on. They had a heavy bias in the direction they needed to know more about. If they thought it was going to rain and it didn't—that was very bad. If they weren't expecting it to rain and it did rain, that was good.

So Krick had these two prime sets of clients. In one he had to bias his forecasts heavily in one direction with respect to rainfall, and in the other he had to bias in exactly the opposite way. These were special interest groups, and he could talk to them and tell them what he was doing. They understood, and they were happy as clams. They paid him good consulting fees. He was doing five-day forecasts for them that were really very successful. During the war, the Weather Bureau, because of the national knowledge of Krick's success, was talked into doing five-day forecasts. At that time, as I recall, Rossby was on leave from MIT. He was in the Washington

Weather Bureau running their theoretical research, although he'd really been doing it as a professor at MIT. The Weather Bureau didn't have any special customers; they just had to put out a general purpose forecast, a zero-bias forecast. If it hit somebody, all right; but for most people, it just wasn't a good forecasting technique. The Weather Bureau got black eye after black eye.

Some of the headquarters people from the Weather Bureau, I remember, were called into Congress to investigate why they were wasting all this government money on five-day forecasts. It was just a disaster for them. They ended up by being very bitter toward Krick. By their method of testing, his forecasts weren't as good as theirs because his were biased. [Laughter] Here he was making a fortune while they were having to sit in the bad seat in Congressional hearings. A bad feeling developed that just never quit. It's still alive and still poison in the business. In particular, they were convinced that Krick's forecasting techniques were no damn good. Then they'd come back in here and tell R. A. Millikan that he ought to fire Krick. I was told that Rossby came in one day and told Millikan he ought to fire this faker.

As a result of that, Millikan asked Paul Epstein and me to do a little study for him on some of Krick's long term, ninety-day forecasts. We ended up deciding, by every criteria we could find, that the successes he had were so far beyond any kind of random expectation or climatological expectation that you had to conclude there was something usable in the atmospheric dynamics that consistently had longer periods of coherence than the few hours that the Weather Bureau numerical forecasting procedures permitted.

GREENBERG: Did this vindicate Krick at Caltech?

STEWART: Yes. Oh, there was more trouble. During World War II when Krick was in England, he did some five-day forecasts for the landing in North Africa—in the Casablanca area. It was a very dicey thing and they hit it really very well. The D-Day landing was another one. He had excellent success there. Again, this just burned up the Weather Bureau people who'd been clobbered so badly by Congress. They controlled the American Meteorological Society, and among other things, the American Meteorological Society set up an ethical standard for professional meteorologists that was strongly biased against extended-range forecasts and cloud seeding.

GREENBERG: Cloud seeding was Krick's idea?

STEWART: No. He was one of the early proponents of it. He did a lot of the early experimental work. Incidentally, even before he was a graduate student here, Paul MacCready as an undergraduate had a cloud-seeding contract with the Salt River Valley Association in Arizona. I was a member of a government committee that came a few years later looking into cloud-seeding research. We concluded that the one that Paul had set up had the best statistical controls of any of the cloud-seeding experiments done yet. Krick had done some of that, and it was much better than the ones done by Irving Langmuir, who did a lot of work for the Department of Defense.

The reason the committee I was on was set up—the chairman was Sverre Petterssen [(1898-1974), a Norwegiean meteorologist]—was that some of these cloud-seeding experiments had apparently given very dramatic results. It wasn't certain that there was a proper cause-and-effect relation; the results might have been accidental. The people like ONR, who were interested in the question of whether these results could have a military application, found when they started to talk to the meteorologists that the feelings between the Krick and the anti-Krick groups were very strong—so strong that if a meeting was organized and group A was going to be there, group B wouldn't even come. This Petterssen committee included people that had been knowledgeable in the area; we hadn't been committed to either side of this feud. We could talk to both sides, and finally came up with a recommendation to the government that they should do a series of experiments. We managed to get the one that was most likely to have a positive statistical interpretation of significance; we got that assigned to the Weather Bureau, and they did it up in the Seattle area and showed very firm support for the fact that there was an effect. You couldn't make it rain if it wasn't going to rain anyway, but you could influence the amount of the rain. For a few years, the Weather Bureau talked sympathetically toward the idea. However, over a five year period, they gradually drifted back again. They were just as anti-cloud seeding as ever. It's been a real tragedy. The problem was that the Weather Bureau people got hit so badly by doing things that in their view really weren't worse than what Krick was doing. But he was able to bias them because he had customers who understood what he was doing and he understood the bias in their needs. I've always felt badly about that.

To finish that meteorology story: during World War II, Krick was in England a long time. After the war was over, he divorced his first wife and married a British girl. His first wife was very bitter about this. He didn't have any particular capital reserves, but he did have this consulting business that was working quite well. By some peculiar perversion of logic, the judge who was handling the divorce settlement ruled that Krick's consulting prospects were worth a quarter of a million dollars to his wife. As a result, Krick couldn't afford to be a college professor.

In 1946, I had resigned from the meteorological department because I wanted to spend more time with the rockets at JPL. At that time, there were only three of us with tenure—Krick and Paul Ruch and myself. In 1947 Krick decided he had to resign; and Paul Ruch went with him. Krick and Paul left and went off to Denver and set up a commercial consulting business, and made millions.

GREENBERG: Did that effectively end meteorology at Caltech?

Stewart: There was a question that Caltech would have to start over from scratch. There were no tenured staff members left. DuBridge didn't know Krick very well, anyway, and I'm sure he knew the Weather Bureau people better. He probably heaved a sigh of relief at that point, because the attacks from the Weather Bureau people must have been distressing. That's when we just shut down the department.

Begin Tape 3, Side 2

GREENBERG: How did you get into the windmills?

STEWART: That happened through von Kármán very directly. A man named P. C. Putnam talked the S. Morgan Smith Company into carrying out an experiment to see whether by the use of modern aeronautical technology you could increase the cost effectiveness ratio of windmills enough to bring them back as an economic source of energy. The old-fashioned farm windmills clearly became economically competitive with electricity only in very remote areas where you had to put in long electric lines or set up a power plant specifically for very small applications. He'd done some work and was convinced that if you used modern technology—1935 technology, that is—and built the machines much bigger, on the order of one or two megawatts instead of a fractional kilowatt, which the old machines were, then it ought to work out. The S.

Morgan Smith Company decided to spend some stockholders' money to do a full-scale experiment to get more reliable data for answering this question as to whether windmills really could compete with fossil fuels. As part of the project, they brought Putnam in to work with them and organize; Putnam got practically all of the MIT mechanical engineering department involved. He got Hurd Willett from the MIT meteorology department. Then he got von Kármán interested and wanted Kármán to give them some help with the aerodynamic analysis and performance analysis for the rotors. Kármán, in turn, recruited Bill Sears and Duncan Rannie and me to do the dog work, you might say.

GREENBERG: The rotor was your part of the project?

STEWART: Part of the rotor problem was my part, yes. As an aerodynamicist, Kármán's area of influence was expected to be primarily in the rotor area, and possibly in the site selection area, too. Anyway, I was asked to set up the calculation procedures for computing the performance of a given rotor design. We had some guides on how to make it a high efficiency design because [Hermann] Glauert in England had worked through the first step of the optimization problem. So we did have some theoretical guidance from Glauert's work. Kármán worried mostly about that sort of thing. He'd tell me what to compute. I'd compute the designs, step-by-step numerical integration for hours on end. It was the sort of thing that my pocket computer can now do. It's a fairly messy problem; on the pocket computer it takes an hour's time to compute one power output at a given wind speed for a given design to a reasonable precision. It took weeks in those days. [Laughter] [Note added by H. J. S.: In 1985, the same problem takes 170 seconds on my IBM personal computer.]

I also designed a set of wind tunnel models that we could test up in the Stanford wind tunnel, which was set up with the specialized installations for propellers and also could be used for windmills. The general reason for that, of course, was to get a calibration on how good the computing techniques were and to what extent you could use them with the extrapolation from the wind tunnel test data. Of course, there was some problem because their wind tunnel tests had to be on a very small scale. The biggest thing we could test in the Stanford tunnel had a 30-inch diameter, and the things we're talking about were 180 feet. So that's a factor of 70 in scale.

GREENBERG: Why did you use their wind tunnel?

STEWART: They were set up to handle propellers; we didn't have that kind of a mounting system or instrumentation system. The old Stanford propeller wind tunnel is long gone; it was thrown away about twenty years ago. It was a logical thing for them to have a propeller-testing tunnel; everybody should do something a little different from his neighbor.

GREENBERG: Was the rotor of the Smith Putnam windmill designed like a propeller?

STEWART: The aerodynamic theory is much the same. The interesting thing was that in principle, we should expect that the simpler theories, which were all we had the capacity to handle in those days, should apply better to a windmill than to a propeller. What really came out of the wind tunnel tests was that the calculation techniques we used were really quite good. The fit was better than you would have had if you'd been doing a propeller test. Any propeller, as a matter of fact, goes into a windmill mode if you throttle it down too fast and its rpm gets too low for its pitch. Then it goes into a windmill mode and starts slowing it down. In fact, one of the problems in propeller-driven airplanes was that if you had a rotating propeller in the windmill mode, even if you had a clutch to open it up so it could free wheel where it's putting out no power, the windmilling propeller still has a terrible drag. The drag is about the same as if you had a solid plate the size of a disc. That made an incentive for engineering development on engines—to find ways of bringing the rpm to zero, so that you could clamp the propeller and hold it stationary. As a stationary thing, it looks worse, but it really has much less drag than if it's rotating freely. And, of course, if you could fix the propeller and then feather it so it points into the wind, the drag is really very small.

GREENBERG: The Smith-Putnam windmill apparently developed some problems and was eventually abandoned?

STEWART: Yes. I should mention that in addition to the straight applied aerodynamic work that I was doing, which was directed toward performance, and wind tunnel validation of the calculation procedure, Kármán had Duncan Rannie working on the dynamics problem, i.e., on flutter. This thing had variable pitch blades, and the blades had the freedom to cone about an axis so that they would go fore and aft under a wind gust. It's really the mechanical analog of a helicopter, even though the operation is usually with the wind almost on the axis, whereas in a helicopter, it's usually ninety degrees away. So the aerodynamics are quite different. But the general dynamical problem is very much like a helicopter. That's a very difficult problem. Rannie worked on that for about a year.

By that time, it was clear the war was coming on. If they were going to get this test done, they had to get things out early. In 1940, there was heavy pressure to order manufacturing of parts. For example, the blades were made with a heavy root forging which connected to the shaft, about which it could rotate, and also connected to the sheet metal structure of the blade. This heavy forging has long lead times, so they made a first guess and ordered it. By the time it was delivered for installation, Rannie had already done enough work so he was pretty sure that it would be overloaded and would have a poor fatigue life. They figured that they could still do the test program if they didn't run it just for sport.

It also had another problem. During the war, one of the thrust bearings on the shaft wore out and failed, but that wasn't an unusual thing. The only trouble was that you couldn't get it replaced during the war because of priority problems. So for about a year and a half, maybe a little more, the machine just sat there on the mountain, without even turning over and exercising any of the joints. It got damaged from the storms during that period. When they repaired it, there was some damage in this root forging area, and they apparently did a poor job of repair, which made it still worse. At any rate, they did get through the experimental test that they wanted to do, and by '46, when they reviewed it—as you'll see in Putnam's book—they concluded that they couldn't quite meet the fossil fuel competition problem. If I recall, they tried to make economic comparisons, which is kind of difficult because you have to capitalize some costs. Fuel costs, for example, really aren't capital equipment. But as I recall, in their comparison, they estimated the windmill could be built for an effective capital investment of about $150 a kilowatt, whereas the fossil fuel systems had a capital equivalent investment of about $120.

They concluded that the experiment, while interesting, wasn't quite successful, so they decided not to pursue it further. They also decided that as long as they weren't going to pursue it further, they might as well turn the machine on and leave it on the line, feeding power upward into the line to the power grid, and let it go as long as it would last. It lasted just two weeks [laughter], so they barely got through their test program. As you can see, there were some problems. They knew that there was an initial design fault, where it was a little under strength for what it was supposed to do. Then there was some repair damage that was done inadvertently as a result of storm damage while it was sitting unattended during the war. But on the whole, it was a very good experiment.

GREENBERG: There was no immediate follow up?

STEWART: Right.

GREENBERG: You were telling me that improvements in helicopter technology during the Vietnam War have also wrought improvements in windmills.

STEWART: Yes. There is an analogy to large sizes. The improvement of analytical techniques and structural analysis techniques that came about make this problem much better. Of course, procedures are numerically messy enough so that Duncan couldn't have done them in those days anyway, but with computer assistance you can. Even Duncan's level of analysis was enough to show that there was a potential problem. You have to say that it would have been better if the war hadn't been coming on and they hadn't had to jump to get into operation or hold it off for God knows how long.

GREENBERG: Do you think it's just a matter of time before windmills will eventually become accepted?

STEWART: Yes. I think so. I think they'll make it this time. One thing you've got to remember is that a windmill is a capital-intensive piece of equipment; almost all of its costs are capital costs. On the other hand, a fossil fuel system is mostly fuel costs; the capital costs for the equipment to burn oil are pretty small compared to the fuel costs. If you differentiate between the price increases in the fuel due to inflation and the price increases due to supply problems, it's clear that inflation hurts the windmill much more than it does the fossil fuel. If you double your interest rates, you double the cost of the power from a windmill. The environment of the last ten, fifteen years has been very unfavorable to windmills from that standpoint. But I think that all of the countries of the world are finally deciding that inflation isn't really good for the working man. If they do decide, then I think windmills will become an important energy source.

GREENBERG: I think I've read that the Mojave Desert would be a good place to install windmills.

STEWART: There are good sites out there, yes.

GREENBERG: Near the Cajon Pass.

STEWART: Yes. You could probably put in a thousand megawatts of windmills out there on the approach to Cajon Pass. They're first installing them in Banning Pass. Southern California Edison has a test site up there where they've had a couple of windmills going and there's a windmill farm growing in that area. It's at about the 3500 or 4000 foot level. It is an extremely favorable site with nearly an eight meter per second average wind speed there. The old farm rule was that the least usable average wind speed is ten miles an hour. That's about four-and-a-half meters per second. So eight meters per second means that is a very good area indeed.

GREENBERG: I've noticed a farm south of Oakland.

STEWART: Altamont Pass.

GREENBERG: Is that promising?

STEWART: Yes. It looks very promising. Incidentally, there's an article in the latest *Smithsonian Magazine* on windmills. Putnam is involved with this Altamont Pass thing as a consultant.

GREENBERG: Was GALCIT's relationship with industry in the 1930s unique for that time?

STEWART: Yes. It really was unique. It wasn't that the other schools weren't receptive to outside relations. It was just stronger here. Partly, that was the luck of the environment. A lot of the aircraft companies moved to Southern California because it was clear and you could do your flight testing. For example, Consolidated Aircraft moved out from Buffalo. Buffalo is just a terrible place to do flight testing. But whatever the total collection of reasons were, by 1930, there were a lot of aircraft plants here. The concentration was greater here than anywhere else. It was easier for the school to make productive relations, and Kármán worked at it, and that made him happy. Clark Millikan pursued it and continued it.

I remember back in the thirties when I first came here, Clark Millikan signed off on every wind tunnel test that was made. Later on, he did the same thing with JPL when that was getting set up. He was involved in every research conference meeting. Kármán was the seminal influence that started these things, but Clark really worked on making them grow. The wind tunnel was really very central in this relationship because it got all of our young graduate students plus Clark Millikan directly and Klein and Sechler and Kármán indirectly. Knowing what the industry's problems were—what they were interested in working on—helped to find your research topics.

I wrote several little papers on wind tunnel wall corrections, which developed directly out of the industrial needs of the application of the wind tunnel today. I should say that it wasn't only the wind tunnel. We also had some structural testing work going on which was very closely driven by the interests of the local industry. The early work by Sechler and Donnell on thin shells did not only go directly into the industry design procedures; it also fed back the things that led to the next generation. I guess the next one really was the general shell stability problem that Louis Dunn, for example, worked on. There were strong connections that way; and there were connections in meteorology.

GREENBERG: Clark Millikan resigned from the American Society of Mechanical Engineers in 1938. Do you know anything at all about that?

STEWART: No. In fact, I never heard of it.

GREENBERG: Von Kármán thought very highly of Jack Northrop. I think he thought that Northrop was probably the country's best airplane designer.

STEWART: I think he encouraged Bill Sears to go over there, anyway.

GREENBERG: What's your opinion about the whole flying wing business? Is that a missed opportunity?

STEWART: No. I think it was explored as it should have been explored. I think it was an idea that was, if anything, late. Its period when it might have been a potentially useful idea was early. But by the time they started getting flight speeds up into the two- and three-hundred-mile-an-hour range, I think it was too late.

Let me tell you another story which will illustrate why. Frank Lehan was one of our fellows in electrical engineering who spent several years at JPL. In 1969, he was asked to become the assistant secretary for research of the Department of Transportation; one of the problems that he didn't know much about was the supersonic transport research activity that had been going on. He asked me to take him around and help him to learn about it. So I volunteered to do that.

The first step of what I found there applies to this flying wing airplane problem. It also displays a weakness in government procurement techniques. The government typically has what they call competitive procurement things where they'll put out an RFP and ask somebody to make a proposal subject to these kinds of ground rules. It's pretty obvious that in such a response to a thing like that, if one competitor makes a mistake—not a number error but a bad choice of concept; it really doesn't matter if the competition makes the same mistake.

Well, in this case, the two main competitors, Lockheed and Boeing, had both assumed that a supersonic transport should be a flying wing design. The earlier work that preceded the B-1 down here at North American looked that way; the European work with the Concorde looked that way. So it was just a passive assumption they made. The B-70, which was the predecessor of the B-1 and the predecessor of the Concorde, could fly from here to Chicago but couldn't fly across the ocean. They thought they could see how to improve it so that they could do that. That illustrates the problem, because when they really got through the Boeing and Lockheed proposals here, at the proposal stage they finally decided to choose Boeing. Both of them had flying wing systems. When they got to about a hundred million dollars more down the engineering design line and found out that the transport would barely go to Chicago, they did a couple of things to improve it. They removed the swing wings and then they put on a tail. The range went up to 5,000 miles, where it should be for a good transport. But of that range increase, almost none of it came from removing the swing wing; practically all of it came from the tail.

The problem is that if you're flying fast, skin friction kills you if you've got too much wing area. If you have to land, on the other hand, you've got to come up to a high altitude. You have big aerodynamic moments where there are changes from your situation in cruise. If you have a little tail, a long way away, it doesn't take much force to trim that moment out. But if you have a flying wing design, you've got to trim that moment out with your control surfaces, which are very close. With the flying wing design, you can never get a high enough CL to make them competitive with tail designs if you're going to make a high-speed airplane. That's my conclusion in this affair. If you were dealing with airplanes that never had to go faster than a hundred and fifty miles an hour, I think you could probably make a competitive flying wing. I don't see those as being of any significance in the modern world.

I seem to remember talking about the Northrop flying wing fighter design either to Kármán or Sears. And the conclusion was that if you worked very hard, you could make it just about as good as a conventional one. Even in 1940, our speeds were already getting too great.

GREENBERG: Let's turn to jet propulsion. Why was JPL founded in 1944 as a separate entity rather than as a jet propulsion section within Caltech?

STEWART: Well, of course, before that it had always been a jet propulsion section as part of the aeronautics department; it was GALCIT Project No. 1.

GREENBERG: Frank Malina was a graduate student in aeronautics.

STEWART: Yes. As a matter of fact, I really should change your question a bit—how did the rocket work start here in this area? It really started out with amateurs. There were lots of people interested in rockets in those days. When I was in high school, I used to manufacture gunpowder and make little rocket motors for my model airplanes and also make bombs. I had a friend who lost a finger by an explosion in some bad nitroglycerin he made. I got through it safely. When I came out to Caltech, Weld Arnold had given a thousand dollars to support ideas and work in this area. Even before that, there was a little bit of amateur activity going on. They built the things with their own resources and their own time. Frank Malina, Apollo M. 0. Smith, Bill Bollay, several others were all interested in these things. I was interested in a peripheral way but not too much.

It converted from this kind of student amateur activity in 1937 when the National Research Council—that was the acting executive wing for the National Academy of Sciences—decided they should organize some discussion on the question of whether there were new areas of technology that ought to be applicable in the aeronautical field. They decided they should spend a few pennies of seed corn to start something going. They had a series of meetings there. Von Kármán was intrigued with the rocket idea, and that was introduced as one of the sub things they discussed. Finally, they decided there were two areas for the NRC to put out a little seed money to start supporting things: MIT took the wing-icing problem and Kármán the rocket problem.

I'm not sure about the numbers, but as I recall, the National Research Council appropriated $100,000 or $10,000, I can't remember which. This was sent to us in June of '38, maybe '39, with the idea that we should look at rocket assistance takeoff problems and see whether this might not develop into something that would be of interest to military services or somebody else. Once this money was actually here, of course, Kármán and Clark had to recruit a staff to work on it. I was one of them. They also recruited Malina and Tsien. Harold Fisher was one of the Navy students here at the time; I think he was one of them. There were still one or two other names.

GREENBERG: The war hadn't started yet?

STEWART: No. This was about '38 or July of '39. At any rate, the first day there was any money to be made on rockets, I was on the payroll. [Laughter] I was doing the sort of thing you would call system engineering in modern language. Some of the others were worrying about chemistry, but I was worrying about whether you could make a machine that was useful out of these kinds of things. I was doing performance calculations on takeoff runs and what happens with a certain amount of rocket assistance. I was also trying to scale the problem and get some ideas about what kind of equipment might be useful.

GREENBERG: Is this what you did during the war?

STEWART: No. This was what I was doing in the first year. It did go on pretty well and the numbers that came out of the applications studies—Clark and I both worked in that area—looked interesting. By the following year—yes, I think it was $10,000 the first year, and the second year the Air Corps came up with $100,000—we formed the GALCIT Project No. 1. Now that, I guess, would be1940. It was an internal, conventional academic-sponsored research program.

The only difference was that by the summer of 1940, I was no longer a student; I was a staff member, an assistant professor—I had been an instructor before that. Tsien also was assistant professor. It was a mixture of academic staff and a few students. Then there were a couple of people who had come in, in the amateur period, who were outsiders.

GREENBERG: Do you mean Parsons and Foreman?

STEWART: Yes, they were involved, too.

GREENBERG: Was there any kind of pecking order at that time? Was Liepmann involved in any of this?

STEWART: No. Liepmann wasn't involved at all. As I recall, Kármán had asked Tsien and Malina to put together a little report for him during this first phase, which was the paper he carried to the Air Corps people and got Air Corps support for the next phase. That also fitted with the chronological pecking order. Tsien must have gotten his degree in '38 and Malina shortly thereafter. I was just brand new to that seniority level. Then there were students. So there was a pecking order; really all you needed to have were names and date of PhD. [Laughter]

Begin Tape 4, Side 1

STEWART: I continued working with the rocket research group through 1940-1941. In that time, they finally got to the point where we could carry out some real flight test experiments at March Field with Homer Boushey [Brig. Gen. U. S. Air Force; d. 2001], who was an Air Corps officer assigned to study here. He was our flight test pilot for that operation. At that point, I dropped out of the rocket work because of the pressures on meteorology with the war coming on. By the time the war actually started, I had to concentrate there. Although I dropped out of the GALCIT Project No. 1 for a while, I kept track of it on the side.

I should say that as this came on and started to be formally organized, Frank Malina was given the assignment to try to handle the operation of the GALCIT Project No. 1. Kármán by that time was an infrequent visitor, although he hadn't formally taken a leave of absence yet.

Clark was really the administrative, academic leader who kept things going. He ran the weekly research conference and the weekly administrative conferences. By the time it had shaken down into a formal organization, the pressure on me from meteorology to drop out was getting to the point where my memories of that period aren't very good. For example, in the summer of '40, I had a special class of meterology students, and the classes just went round the year after that, with no vacation.

In '43, the German work, of course, was known through intelligence activities, and Kármán was asked to put together a proposal to Army Ordnance. He'd been working with the Army Ordnance people for years through the wind tunnel work that Puckett was doing for him. That was finally coming to a head and they were building the tunnel. So Kármán had Frank Malina, I think, put together the paperwork, but he put together some thoughts on what ought to be done to make a guided missile system really work. Everybody had been working on guided missile systems; the first attempts went clear back to World War I. But they never really worked with any real success up to that time, primarily because the engineering problem was a complicated one. It involved many specialty areas, including a mixture of aerodynamics and electronics—that is with two establishments that up to that point were technologically and culturally different. For example, the Navy's program for guided missiles from World War II assumed that you could buy a lot of black boxes. This one had a gyroscope and this one had a computer, and you'd plug them together and it would all work. Of course, they never did, because there were always false grounds they'd forgotten. The Ordnance people had also had a lack of success with these things. So Ordnance decided that they really had to organize this in an intellectually superior mode to what they'd tried before.

In retrospect, it seems to me that high tech engineering grew out of two independent nuclei; one in the airplane industry around Prandtl, Kármán, et al., and the other in communications around Gibbs and Shannon.

Ordnance got Kármán to put together this proposal; at the point that it was undertaken, this was going to be a pretty large scale thing, compared with being an adjunct to a small department of a small school. Even at the earliest, it was conceived as an organization that would grow to hundreds of people, at least. It was decided at that point that it wasn't practical to organize it as GALCIT 1 had been organized. It was getting big enough by this time that it couldn't be a purely academic activity. Incidentally, it was really that feeling that caused it to be separated. Also, it required many disciplines, including aerodynamics, structures, electronics, guidance and control, chemistry, and so on. There were people who thought that the security of the procedures was part of the problem, although it really wasn't.

In that very first work with the rockets in 1939, I remember I'd had a student doing some work where we didn't worry about security. As a result, the thesis came out classified. Then we had to go through the mess of getting it undone, so that if there was anything interesting there, it could be published. It was such a mess that after that, I always went through the security problem first; we had students working on classified work, and we also had students, usually the same ones, working on theses. We always worried about getting the thesis content declassified ahead of time so that we never had any trouble with it. Although JPL was on sight a classified facility, it was much easier to have students do thesis work there than it was later under NASA. I never had a single thesis that was classified, except for that very first one.

GREENBERG: I want to talk a little bit about Malina. He was apparently unhappy about how Caltech treated him. He felt that they should have given him more recognition for his part in founding JPL. Did he get shortchanged?

STEWART: I always thought he got full recognition for various things; in fact, they made him director. Clark Millikan was still on a next echelon of management.

GREENBERG: I guess one problem may have been that Malina and Clark Millikan never really got on that well—from the very beginning. I know that Clark Millikan was one of the skeptics about jet propulsion.

STEWART: I wouldn't say that.

GREENBERG: No? Well, that's the traditional story.

STEWART: That's Malina's story, not anybody else's. I don't know why Malina resented Clark.

GREENBERG: I guess Malina, and Kármán in his autobiography, too, tells the story that Clark, at the outset, was skeptical. Clark told Malina that he shouldn't take that kind of a thing; it wasn't suitable for a doctoral thesis and ought to go out into industry.

STEWART: That's conceivable in '37, '38. By '39, when there was the first sponsored research of any kind, then I think Clark's position was quite different.

GREENBERG: Once JPL got going and Malina was made acting director, and Clark was a member of the board that oversaw the whole thing, I guess there was constant friction between the two.

STEWART: I think there was. On the other hand, my feeling was that Frank just didn't like to report to somebody.

GREENBERG: Were you surprised to learn that Malina lost his passport during the McCarthy period?

STEWART: No. I knew that something had happened to him. I didn't know that he lost his passport. After reading your outline, there's a whole background that I think was most unfortunate. For example, there was some real honest-to-God poison around Caltech; it's still not entirely dead. It hit me the first time in the spring of 1940. We were coming close to the end of the academic year, and I had a large class of military students in meteorology. The senior officer of that group decided to have a pre-graduation get-together for all his fellow military members and invited the staff people like myself there, too, to his house. After we got there, he called me aside and said, "Can I speak to you privately for a bit?"

It turns out that—he didn't go into the details in any great extent—our counterintelligence people had run into some classified information that had been stolen. They thought the theft had occurred at Caltech. It was information that indicated a connection with the wind tunnel. He wanted to talk to me to see how the wind tunnel was organized, how we ran it, and what access was. Of course, I was horrified. I never heard of it later. As far as I know, they never caught anybody that did it.

I had quite a similar experience that happened in 1945. In this case, I know a little more about it. Our counterintelligence people in Paris had intercepted a Russian courier, and found a lot of secret documents that had been stolen from Caltech, including a number that I had written.

That's why they called me in on it. I learned a little bit about it. But again, they were never able to trace where it came from. There was something going on here that was unhealthy. At the [Sidney] Weinbaum trial and the trial proceedings, Dubnoff testified concerning what the government knew about his operations, and said [in effect], "Sure, I was the secretary of the group; sure, I collected dues from them." Of course, it didn't bother him; he never got in any trouble at all because it wasn't a crime to be a communist.

GREENBERG: This was a local branch of the Communist Party?

STEWART: Yes. Weinbaum, who'd been a member, was unfortunate enough to deny that he was a member in making an appeal in a clearance investigation. So they finally tried him on perjury. If you're interested in that, there was a book called *The China Cloud*, by [William L.] Ryan and [Sam] Summerlin. If you really want to know about it, you have to read the legal transcripts of the trial, as well as the book. Frank Malina was friendly with practically all of these people. I'm sure this gave him lots of trouble, whether or not he ever was involved any more deeply.

GREENBERG: He left the country and went to France. Was his leaving connected with this?

STEWART: It may well have been. For example, I remember traveling with him one time. He'd been to Annapolis to visit Fisher and to talk with Goddard. I gather it was a very unsatisfactory discussion. He and I were supposed to go over to the [Johns Hopkins University] APL laboratory to meet somebody there for a discussion. When we got there, things were all loused up; our clearance hadn't been straight. They finally admitted us in some special relation with no talk about classified material. I was told later that the problem was that the Navy had not forwarded Frank's security clearance.

GREENBERG: Did you get along with him well enough?

STEWART: Oh, yes. We shared offices for years. But there was official suspicion of something bad going on at Caltech. I think Frank was hurt by it. Whether he deserved the hurt or not, I really don't know. That's a pretty awful feeling—that someone you know, a friend, may have done something that's really very bad.

HOMER J. STEWART

SESSION 4

November 9, 1982

Begin Tape 5, Side 1

STEWART: Clearly there was something here, as I described, that was poisonous, even if it was only the official suspicion. On the other hand, everybody you know is presumably an honorable gentleman. Neither case was ever closed.

GREENBERG: Do you recall a visit here during the Second World War by Courant and Friedrichs, to solve a rocketry problem?

STEWART: I recall a meeting with them, but the meeting was in Langley, Virginia. I'm not familiar with their visit to the campus. There was another interesting visitor to the campus during the war, referred to in Churchill's memoirs. As he said, we suddenly understood why the V-2 firings against London, which had all been aimed at the big power stations on the Thames, uniformly missed; thousands of rounds had been fired and never hit their targets. When we started out working on the guidance problem, we suddenly realized we understood it. The explanation was so simple that one word would give it away. We didn't want to put it in writing, but we let the people in the British mission in Washington know that we had something that we would be happy to transmit verbally. Churchill's right-hand man for science sent a couple of his people out to us and had a meeting down on the first floor of the Guggenheim. We went over it with them and explained it to them. In his memoirs, Churchill says, "Their chaps were somewhat more knowledgeable in these areas than ours." [Laughter] That one I remember.

GREENBERG: I'm told that there are a lot of good anecdotes involving von Kármán, according to Professor [Rolf H.] Sabersky. Apparently you were involved in some of these. Are there any that you think might be worth talking about?

STEWART: I think I mentioned to you Kármán's habit of disciplining a bad speaker by turning off his hearing aid, leaning back and going to sleep.

GREENBERG: Did he ever make some utterly irrelevant comments to deal with the same problem?

STEWART: Yes. He occasionally made some sour remark, particularly in his later years.

GREENBERG: I guess in public places, like restaurants, he also came off as quite a character sometimes.

STEWART: I do remember one tale which I was almost involved in. Not long after the war, Kármán was in Paris in the California Hotel. I was going to be in Paris for a day and so was Allen Puckett. We thought we'd meet and go over to see Kármán. As it happened, I got to Paris early; neither of them was there the day I was. When I got back here, I saw Allen, and he said that he'd gone over to the California Hotel. Kármán wasn't there, but he was expected in shortly. Allen went into the bar to kill time and met a young lady sitting at the bar waiting for somebody else. After a while, her friend came in; he was an American reporter. The three of them were chatting and passing the time. Kármán then showed up, and made it a foursome cocktail hour. Eventually, Kármán had to leave because he had an appointment. After he left, the young lady turned to Allen and said, "Who is this man? Every day he comes up to me and makes interesting propositions." [Laughter] There are several stories of that general nature.

GREENBERG: You were one of the pioneers in American rocketry—JATO, JPL [Jet Propulsion Laboratory]. Can you summarize your role and contribution?

STEWART: I guess to use modern terminology, what I contributed was mostly in the nature of systems engineering. To use the 1930s terminology, I contributed the most in preliminary planning and preliminary design work. For example, when the first little bit of money from the National Research Council came in, the idea was to look at the rocket business from the standpoint of its potential application for jet-assisted takeoff—JATO. I did most of the work on putting together the various parameters of the problem—to see how much rocket thrust you needed to do some good, and what was the best way to use it if you had rockets. I also studied whether you should turn them on when stopped or whether you should wait until you're part way down the runway and then turn them on for the last part. I worked out the parametric analysis so you can look through to see the best way to use it, and what the design objectives should be for any kind of an experimental development program.

When JPL was organized, the Army and Navy jointly put together a specification for a weather rocket which could carry instrumentation up high with a rocket, and radio data back. They wanted to go to 80,000 feet, which was pretty high for rockets in those days. We had already started doing some work on the general concept of what finally became the Corporal. It seemed that if we were to make a small scale model of the Corporal system as we laid it out, it might be a good approach to the weather sonde thing. I remember Frank Malina, Colonel Skinner—our senior military liaison man at JPL—and I spent most of a weekend together, working through the details of this thing. We looked at the state of the art as to what would be available to use within a year's time. We came up with a design that looked like it would do a little more than what they wanted. They wanted twenty pounds at 80,000 feet. It looked like we'd get a little bit more than that by using the Tiny Tim rocket, from the Navy project out at China Lake, and a scale model of our Corporal plans.

Since it was a small thing, we called it the WAC Corporal, the Corporal in small size. As you undoubtedly know, they did decide to ask us to go ahead with that. We flew a dozen of them. Then the NRL took it and slightly reworked it, and it became the first Aerobee, for which Aerojet got the building contract.

So that's preliminary design. Another example, I remember, was trans-sonic aerodynamics. It was a miserable problem in that 1945 period, and nobody had any good ways of testing. Somebody had made a proposal to make a rocket-propelled sled that you'd run on a track. You'd have an instrument package there, and if you ran it up supersonically, you could pretty easily make the geometry so that there wouldn't be interference from the geometry of the sled; you just had to put your test item forward of the interference zone. That sounded like an interesting idea. I worked up an analysis, again, of what you had to do, and what parameters you needed. We put it into the information system; it was one of the early JPL reports.

The Navy thought it looked good enough, so they decided to go ahead with it. They built the first track out at Inyokern. When they got to the point of making the first demonstration, they invited me to go along with their party to observe the first high speed test run. There were about a dozen of us there, observers up on a hill above the flat valley where a long track was laid out. We had a slit trench behind us for protection in case of trouble. As it happened, just as it got to maximum speed, in the last second of the propelling rockets' propulsion, one of them exploded and sprinkled shrapnel in all directions. We all dived for the slit trench. Only one man was hurt; he was a four star general. He got hurt because he was on the bottom of the trench and we all landed on top of him. [Laughter] So it proves that experience has some survival value, but it's somewhat treacherous even so. That was another example of where we did some preliminary work. In that case, the Navy picked it up and followed through with it.

Actually, when JPL was organized in June of '44, I'd been out of the rocket business for two years because of the press of the meteorology training program. They asked me to come back. The initial staffing, of course, was practically all campus Caltech. In particular, they had one section which was called Section One, Research Analysis. He was asked to work with that. H. S. Tsien, who was still here at the time, was the section chief. In a couple of months, he made the decision to move to MIT. He was section chief for five or six months—it was December of '44 when I became section chief.

Research analysis was a shorthand for system engineering. I guess there is another way of describing it. We were an applied mathematics group, where there were all kinds of problems which were messy mathematically. We would work on them, and typically, if we got something far enough along so that it could be described as routine engineering, then the regular engineering groups took over. It happened in lots of areas. For example, trajectory calculations, trajectory analysis, and all these things were part of our charge. Five to ten years later, when the computer capacity started getting big enough so that you could logically do these things on a computer, the computer operation was largely staffed out of the electrical engineering group at the lab. One of the people from our group still maintained a top level control on the trajectory analysis thing so that we could keep the thing conceptually as a package and wouldn't be doing the same problem over again and wasting energies. Another example is heat transfer in nozzles, a standard elliptic-equation integration problem.

GREENBERG: Did you do anything on the shockwave problem?

STEWART: Yes. We worked on the external aerodynamics all the time. Our group handled the wind tunnel testing with all the model design and external configuration design, and model testing. When Allen Puckett got his supersonic wind tunnel going back at Aberdeen in the fall of

’44, I remember one of the first tests they had; we put together a series of models, covering the sort of configurations we thought might be useful for the Corporal. I remember putting it in a big box that weighed about fifty pounds and carrying it back East. I ran the wind tunnel test there and we brought it back. In general, we handled the problems of aerodynamics, and that included shock waves.

One of our principal outputs, of course, involved the aerodynamic parameters that you need for guidance and auto pilot and stability calculations. Even in that area, we did some little things. For example, one of the standard deficiencies of most of the old auto pilot kind of work was that it was done by electrical engineers who were used to the Laplace transform, with a high capacity to handle any linear equation with constant coefficients. When you’re dealing with things like a rocket that’s taken off at zero speed and pretty soon it’s going Mach 3, you’ve got parameters that are highly variable. In many cases, just making constant parameter fits at various points along the trajectory doesn’t give a useful answer. Sometimes the thing will actually be unstable when the constant coefficient analysis says stable, and sometimes it’ll really be stable and look unstable. For example, you can see a rocket going up in the atmosphere and the fins are damping it and taking energy out of the oscillation. If the density drops fast enough, even though the energy’s going out, the amplitude will go up and the frequency will go down fast. The amplitude actually increases as the energy content goes down. So that may be stable, but it looks unstable. We did some work with the problems that were really nonlinear, too, but that was done mostly numerically. With problems that are linear but not exponential, you can usually do something in an analytic way which gives you a little more insight. If you reduce it to a purely numerical process, then the insight is hard to get. The trajectory problem, incidentally, is a classical nonlinear problem, and the insight was hard to get. [Laughter]

GREENBERG: When JPL was formed, most of the staff was from Caltech. Gradually it became an independent entity and acquired its own staff.

STEWART: Yes. That’s why it was separated. It clearly had to grow; it had to become too large to be just an adjunct to the aeronautics department here. It had to become multidisciplinary, too. The objective was to explore the range of technical problems that have to be pulled together to make guided missiles work; this includes aerodynamics, structures, electronics, auto pilot, stability and guidance, and information processing, as well as propellant chemistry and environmental design conditions.

GREENBERG: Have you divided your time since then?

STEWART: Yes. The arrangement began in June of ’44 and continued until ’73. I spent half my time at JPL, so the campus billed JPL every month for half my salary. I never was on the JPL payroll; I was always on the academic payroll. JPL people were not quite paid on a government level; they were paid at about the average of the industrial level, which was somewhat higher than the academic level. One time during the fifties when I was running one of the divisions there, it turned out that out of the several hundred people in my division, there were two junior engineers who were getting less than I was. [Laughter] It didn’t create a problem. Since I was on the academic payroll, in principle I could do a little bit of consulting. The custom allowed a day a week. I never used that much, but I did use about half a day a week. That half day easily made up more than the difference between academic and industrial pay.

GREENBERG: Did the GALCIT change in any significant ways with DuBridge’ s arrival?

STEWART: No, I don’t think so. There was the other transition that happened when Kármán finally decided to resign. In 1947 Clark Millikan became the director, instead of acting director. DuBridge came out shortly before that. I don’t think DuBridge really knew Kármán in any useful sense, but he did know Clark and they worked well together. Clark provided the main strength of the Institute-JPL connection. DuBridge was very interested in JPL, and he always followed it surprisingly closely.

A highlight of my JPL memories was in 1958 when the NASA formation was in the works. The rumor started that they might be interested in transferring JPL from the Army to NASA. I remember DuBridge organized a meeting in his office with Clark Millikan, and Pickering, who was director of the Lab, of course. There were also three or four of us junior members. I was there along with Eb Rechtin, Jack Froelich, and Bob Parks. It was a small meeting to consider this question of receptivity toward the NASA feelers. We argued it out, and finally came to the agreement that we should be receptive. But we decided that we should ask to be given a mission assignment, namely the deep space and planetary exploration problem.

DuBridge carried it forward. If you remember, the initial memorandum of understanding, which governed our relations in a non-legal sense, did contain this mission assignment. I thought that was very important because there are lots of government laboratories that don't have clear cut mission assignments and they're really not too effective. With a mission assignment, it looked like JPL ought to be in a healthy way. That was one of the basic reasons why things had gone well under the Army contract. Now it's more than twenty years later, and it has held up pretty well.

GREENBERG: Clark Millikan was director of the GALCIT at this time until his death. Is he a forgotten figure?

STEWART: I think he's very much overlooked. I think he had very great significance in the development of this area, and also of the Jet Propulsion Laboratory. Kármán was the seminal influence, but Clark was the builder who made it go. We can think of that in a hundred ways. For example, before the war started, supersonic aerodynamics was a very exotic area. But by '39, we'd had a first P-38 dive come in in a fatal crash out near Palm Springs because of the compressibility phenomenon that hadn't been anticipated and was finally recognized. It was just as startling a discovery as flutter had been eight or ten years earlier. As the shock wave gets stronger on a wing, the pressure distributions are vastly different, and the airplane suddenly tends to tuck under. If it's bad enough, you may not be able to control it and pull it out; that's what happened to the poor guy in the dive. That was really the first demonstration that compressible flow phenomena had some significance other than in compressors and turbines. At any rate, this clearly needed a little different kind of mathematics. Clark, among other things, brought in Paco Lagerstrom, who shifted the applied mathematics interest in the staff toward that direction.

GREENBERG: Tsien left Caltech and went to MIT. Was he unhappy here?

STEWART: I don't know why he left. At that time I think he was an associate professor. I suspect he saw a chance of becoming a full professor, and that was attractive.

GREENBERG: He was a pretty ambitious guy.

STEWART: An ambitious guy. He had lots of capacity and he knew it and let everybody else know it. Incidentally, in discussing this period, I may make a mistake, because there were two Tsiens here at the time. You've got to remember that. They're both in China now.

GREENBERG: There were two Tsiens?

STEWART: Yes. There was H. S. Tsien, who was an assistant and an associate professor here, and was the chief of Section One at JPL for the first few months before he went to MIT.

The other Tsien was W. Z. Tsien, who, I think, got his PhD in Toronto or Montreal, Canada. Then Kármán got him up here in '43 with the idea that he would work on some structural and elastic problems that he thought were important. He was also recruited when JPL was formed to work in Section One along with C. C. Lin, who was one of our students at the time. C. C. Lin and W. Z. Tsien were the two who had carried out this preliminary guidance analysis work which told us why the V-2s weren't hitting their targets. W. Z. was an interesting fellow. He'd been a member of the Communist Party in China and he'd gone on the Long March route, and came here. It never caused any security problems for him; he had clearance in practically every area. In '46 he decided to go back to China. There were no problems. A little later, when H. S. Tsien went back, they both ended up in fairly high positions in modern China. For example, H. S. heads up the applied mathematics, applied mechanics division of their organization, which I guess includes all their rocket and guided missile and every kind of high technology thing. W. Z. was an echelon down from that; he was also a professor at Tsinghua. Five years ago, when he was with the first Chinese mission that came to the United States, he asked the State Department—which was running things—to invite me up to San Francisco; I went up and spent an afternoon with him and had dinner with the group. At that time, he was head of two of their laboratories. He was head of the area where they built their solid state components. He said they really had to start off from scratch. They even had to make equipment for making silicon crystals. The other division was one where they were doing their computer work. So he ended up in a very important place there. He was in the United States within the last year and visited Caltech. We showed him what JPL is today.

GREENBERG: I guess H. S. Tsien is better known, because of the difficulties he encountered.

STEWART: Yes. But W. Z. Tsien worked with me for several years at JPL, so I knew him better. I have a funny story about him. One time we were at a Christmas party at Frank Malina's. W. Z. Tsien and C. C. Lin were there. H. S. Tsien was gone by that time. I remember seeing W. Z. drinking a martini, and I remarked to him that I was somewhat surprised to see him drink a martini. All the other Chinese people we'd had were very modest in their alcoholic tastes, and a glass of wine was as much as I'd seen any of them take before. He said, "Oh, when you're out on the Manchurian Plain, you've got to keep a fire going inside." [Laughter]

At any rate, H. S. Tsien left by December of '44. As far as I know, he left in perfectly friendly circumstances.

GREENBERG: Of course, he eventually came back and got into trouble.

STEWART: Yes. It wasn't entirely his fault. He had been a little careless in taking all these documents which had been secret during the war. He had boxed them up to ship to China—they'd all been declassified but he hadn't gone through the process of re-marking them. When the customs people saw them, of course, everybody went through the roof. That was the way his problem started.

GREENBERG: In fact, they had been declassified?

STEWART: They had in fact been declassified. I think there was a little more than was ever publicly admitted, and there's probably no reason why it ever should be publicly admitted. There is a very strong coincidence that occurred within the next few months. Because of this uproar with Tsien's papers, he was stopped and held. Apparently—and now I'm extrapolating—people in Washington decided this was a good excuse to hold him for a deportation hearing. I really think there were quite a number of Americans who were held in China, and our State Department was setting up to make a trade. At the time when Tsien was finally released, there were seven or eight other Chinese who were suddenly permitted to go back, and there were five or six Americans who showed up in Hong Kong and came back. So I think the deal was a trade. The classified papers became a convenient excuse to hold him for deportation charges until the thing could be arranged. That's my interpretation of what went on. Needless to say, Tsien was very angry about it and felt quite abused, and it's not surprising. I think the Americans who came out of China felt very happy about it.

GREENBERG: In 1957, after the launching of Sputnik, the Senate Preparedness subcommittee appointed you and William D. Houston to investigate U.S. missile and satellite programs. As chief of the research analysis section of JPL, you participated in such projects as the WAC Corporal, which you mentioned, the Corporal, Bumper, Sargent, and the Jupiter C. As chief of the liquid propulsion systems division, you participated in the Explorer series of satellites, which was a joint venture with the Army ballistic missile agency.

STEWART: Jupiter C was, too.

GREENBERG: So you were really in on things.

STEWART: Yes. There is a chain you left out, which preceded the appointment as consultant to the Senate committee. It started out just at the end of World War II.

GREENBERG: Let me preface what you're going to say with the following question. I still have memories of 1957; if I recall correctly, the situation in this country concerning rockets was depicted in the press as dire. Was it dire?

STEWART: There was a problem. I was involved in quite a bit of top secret activity on the side here for a number of years, which started out in '47 with a small contract that JPL did for the Air Force. They asked us to go around to all of their guided missile activities. They had half a dozen programs—I think I told you a little bit about this in connection with Allen Puckett's going to Hughes. At any rate, after we'd finished that problem and had written our final report—
[Tape ends]

Begin Tape 5, Side 2

STEWART: [Continuing]…the Air Force Intelligence people—I think this was before they changed to foreign technology as their nominal title—came to us and asked if we would participate in an experiment. They wanted to see whether by bringing research people in this country as close as possible to the original technical intelligence data—without requiring them to go overseas—you might have an improved capacity to understand these things. There were half a dozen of us who participated in that. We would occasionally go back to Wright Field; they'd send periodic information out, meet with us, and we'd discuss things. In fact, I used to go to the top secret pickup place. I had special permission to carry the material with me up to JPL without carrying a gun, as regulation would have required, because it would be too conspicuous. [Laughter] At any rate, by the first go-round here, it was pretty clear that the Russians had a big rocket engine development underway; it was a hundred metric ton thrust engine. We didn't know exactly what that meant; a hundred metric ton engine was a little over three times the V-2 engine, but it still wasn't an intercontinental missile as we visualized it in those days. As time went on, there were other indications that they were working on an intercontinental weapon. By 1950, it was pretty clear that that's what their problem was. We'd even heard the term "five cluster," which really was what their ICBM did turn out to be. On the other hand, by 1950, our top management in Washington really didn't believe these things.

Incidentally, I have a declassified copy of a lecture I gave in 1946 at the Infantry Conference in Fort Benning; all our top brass was there. They'd asked Clark to do it and he couldn't, so I did it. I have in there the numbers that I thought were appropriate to an ICBM. The Air Force had followed along a little further and they'd had some studies by, for example, Convair, which preceded the Atlas. But by 1950, those things were all on hold, with just some paper analysis going on. Within the Department of Defense, they'd decided that this intelligence was really convincing.

I think it was 1950 when General [Bernard A.] Schriever, who was a colonel at the time, was brought back to the Pentagon. Rand set up an office there to work with him. They were trying to get organized for an ICBM development. Kármán had had me appointed to the SAB, which was organized in 1947, more or less in its modern form. I was put in charge of a three- or four-man committee to look at this ICBM problem.

That committee had a fine future. The year after I had it, Clark Millikan took it over, and the year after that, it became the Teapot Committee. An applied mathematician was chairman, John von Neumann. Later on, they organized Ramo-Wooldridge as a support group as it became a going operation.

In 1950, when I was chairman of the first phase of this, Truman was president, and he just didn't believe these things were possible. I remember another thing that happened about the same time. Truman had an old friend, Ari [Aristide] von Grosse, a professor at one of the eastern colleges who was one of the four or five people who held the original patent for the separation of uranium material. Truman trusted him, and Truman had heard this talk about satellites. He called him in and asked if he'd make a special study for him on this question of satellites, because at that moment, the Department of Defense people were, in essence, forbidden to use the word satellite. Anyway, von Grosse talked to everybody and came to Harry and said, "Well, as near as I can see, the question of satellites is if you want to do it, all you got to do is just do it." Harry thought that was so ridiculous he threw him out of the office. So that was the sticking point.

In 1953 things changed with a new administration. New people looked at things again, and they decided to go ahead. That's when the Teapot Committee really played its role in starting off the Atlas program. You can see from this that the Russians had a head start from '46—which was where the earliest intelligence started to indicate that they really had a program already underway—until '53. It wasn't all lost time because we had some papers and studies. As a matter of fact, I guess even during the year when I was chairman of that SAB committee, we did make one important contribution. We got in conversations with [Edward] Teller. We got the AEC people to decide that you didn't need a seven thousand pound warhead on an ICBM to make it significant. They felt they could probably make a reasonable warhead for fifteen hundred pounds. And that, of course, knocked the gross weight estimates from about a million, million and a half pounds takeoff weight, as it had been with the larger payload, down to—well, essentially the number that we ended up with on the Atlas. So, it wasn't entirely wasted time. They weren't seven years ahead of us.

Nevertheless, by 1953, we had a good deal of work to do to catch up. By '57, when they flew their first satellite, we were still months short of flying our first Atlas. By 1958, we had caught up with them, and by 1960 we were way ahead of them. It's hard to believe that in four years you can make up a six year gap. Our engineering people really did a magnificent job.

GREENBERG: They did it in the years from '53 to '57, essentially?

STEWART: Yes.

GREENBERG: And this is before all of the publicity?

STEWART: Oh, yes. You don't expect the press to understand anything. As a matter of fact, there's a famous incident in my own personal history, in conjunction with the Vanguard. The appointment to the Senate committee had come because in '55 I was asked to serve as chairman of a committee on special capabilities—out of the office of the secretary of defense. This was formed because the IGY committee in Rome had recommended that the nations should, as part of the International Geophysical Year, have a satellite, a scientific experiment phase. The first question was, Should we involve ourselves with it? My committee was organized to look at the launching vehicle problem and make a recommendation to the secretary of defense and he, in turn, would recommend to the president. There was a similar committee organized to look at the scientific experiment side of it, and Bill Pickering was on that committee.

The first decision we came to was, yes, we ought to participate because it looked like there were a number of possibilities for doing it successfully. My committee made a unanimous recommendation that we should, to the secretary of defense, and he in turn to the president. It was announced on the White House steps in August 1955. A committee member who had never attended a meeting was on the White House steps—the only representative from the committee. [Laughter]

The next phase, in the fall of '55, was to recommend what kind of a launching system we should use. The Redstone was in pretty good shape and had gone along pretty well. However, some of the members just didn't like the idea of Redstone doing it. Also, the Redstone had been arbitrarily restricted to a two hundred mile range by the tactical strategic decision of the Army-Air Force division; they built the Redstone with heavy armor plate and about a thousand pounds of concrete along with the warhead to get the weight up so that it wouldn't over-perform accidentally. [Laughter]

Some of the people on my committee didn't really believe that you could take all this stuff out and get the proper performance out of it. They really thought it would be better to start anew, which was the Navy proposal. The Air Force had said they could do it and would do it, if they were asked, with the Atlas. In view of the pressure to catch up with the Russians in this area, they preferred not to be asked. So we thought that was appropriate and concurred. When we came to the decision of the Redstone or this new Vanguard idea, the committee had a flat split. The majority decided to recommend the new development.

GREENBERG: In fact, the Redstone would have been perfectly capable of doing the job?

STEWART: Oh, yes. As a matter of fact, in ’56, we flew the Redstone with the upper stages on it—only three live stages—as a preliminary test for the Jupiter C warhead testing. Without the fourth stage, we threw the thing three thousand miles down the Atlantic. And it was the spare vehicle for that, which was already down at the Cape in storage in ’56, that we finally flew in 1958 when we flew Explorer 1. We brought that out of storage and made a couple of modifications so that it could coast between the first stage burnout and the second stage ignition. For the warhead test, we didn’t have to coast between the two of them for the launching. They put on a small modification kit for that and put a fourth stage on the nose, which was already designed to handle it anyway. We could have done it in ’56 if we’d wanted to.

I think there was a bit of high echelon politics that I wasn’t exposed to, because the Navy had a real problem in ’55; they really had to get into the ballistic missiles business somehow with submarines. There was a strong group in the Navy that didn’t want to do it that way. They wanted to do the sort of thing that we now call a cruise missile, and they had some turbojet-powered cruise missiles. Their performance as vehicles was pretty good, but the guidance system wasn’t good enough in those days to make a cruise missile a sensible weapon. However, there were a lot of strong Navy people who really wanted to keep pushing that. I always had a feeling that somebody had decided if you push the Navy into rockets with the satellite, they’ll get rockets into the submarines. And, of course, they did.

But it was kind of a mess. Explorer 1 was finally done in less than a hundred days—we took the Jupiter C out of storage and made little modifications and flew it. The biggest worry at the time was that the little solid propellant rockets for the upper stages were the oldest high-performance rocket propellants in the country. They were older than any of the military weapon propellant aging test items. They’d been in storage down there since ’55. Our biggest concern was whether the propellant really would hold together under these high acceleration conditions, with no aging experience of that duration.

As a result of the split decision in the committee, there were stories floating around. I remember getting a call from Jack Anderson on the telephone at home one time. He was going to write a column for Drew Pearson. I don't know who he'd been talking to, but it was somebody who wasn't really very closely involved with the affairs because he had the story all loused up. He told me what he was going to print, which really was very derogatory toward me. I just told him I wasn't authorized to speak about these classified things. He went ahead and did it and blamed the unfortunate choice of Vanguard on me, although had supported the Redstone. I was told that some group even bought a full page ad in the *New York Times* recommending that I should be hung. [Laughter] I never checked through the back files to see whether it was true, but it's a nice story. I probably could have stretched security rules a little bit and straightened him out, but I had no obligation to do so. It was his problem to write a responsible story.

GREENBERG: Did they ever fly a Vanguard?

STEWART: Oh, yes. They didn't make the opening of the IGY. There were further tests in 1957; in December, for example, they had an explosion which got an awful lot of publicity. Actually, a little over a year later they finally had a completely successful test which flew the round-ball, twenty-inch sphere, payload number one, that it had been designed for, and it really worked well. As far as I'm concerned, the engineers on the Vanguard did an extraordinary job; they brought through a completely new development from scratch. They didn't have anything when they started. There wasn't a single component—except for one, which was almost like what they finally used, and that was the first-stage, oxygen, jet fuel engine, which had been used on the Viking project. That was pretty close to the Vanguard configuration, but everything else was from scratch.

GREENBERG: They did this in a relatively short time?

STEWART: They didn't settle on their final configuration until January or February of '56. By early '59, they did a full-up flight with full mission. They really did an extraordinary job. It's just that we really had no right to ask them to do that in such a short time.

There were all kinds of underground stories going around at that time about this equipment that was capable of orbiting. The stories were embarrassing to the people in the top echelons of the Pentagon. At one time in '56, the secretary of the Army sent down instructions that all equipment capable of orbiting should be destroyed. He was just tired of being bothered about this. Jack Froehlich out at the Lab was department director, one of the three in that echelon under Pickering. He solved that problem. He assigned the equipment down at the Cape to a terminal test, namely, aging. That's how, when we finally flew them, they were still there, and that's why they were so old.

GREENBERG: How did the applied mathematics department at Caltech come into being? What were the circumstances?

STEWART: I wasn't strongly involved in these discussions, but let me tell you my bystander view.

As it started out, the aero department here was really an applied mathematics department too. The two aspects were indistinguishable; you couldn't do aeronautics without doing applied mathematics. I guess in principle you could do applied mathematics without necessarily doing aeronautics, and that's what led to the final split. The applied mathematicians thought they had a broader mission in life than just things that might be of interest to aeronautics and space. At any rate, as Clark Millikan had grown the department, he brought in Paco [Lagerstrom] and [Philip G.] Saffman, and so on, who were also associated with aeronautics, but they were mathematical specialists. In Manchester, England, I would have been called a mathematician, not an aeronauticist.

GREENBERG: [Gerald B.] Whitham was already here in aeronautics at that time?

STEWART: I was trying to remember when Whitham came; he came a long time ago.

GREENBERG: It seems to me he was here in the aeronautics department and then moved to the applied mathematics department when it was created.

STEWART: Yes. That's right. At any rate, they were here. There was an interest, which I didn't quite understand, in separating out into a different group. I'd hear a little gossip over at the Athenaeum about the problem every once in a while. All I can say is that they finally decided they should make a change; they decided it should not be in the mathematics department per se but should be in both mathematics and engineering. The problem was how to work out some kind of a compromise that would fit these nominally conflicting objectives. When it finally went through, it was separated and stayed adjacent. Still, it tended to cross attendance at seminars, etcetera. It looked to me like it was working perfectly well. There was a similar cycle with the applied physics.

GREENBERG: So the relationship between the two groups actually is pretty close?

STEWART: There's a difference in viewpoint. The people who became applied mathematics people really were more interested in their discipline. My impression was they didn't value the matrix idea of a cross-disciplinary group, which is what the old aeronautics department was. They were looking for a more discipline-oriented organization.

GREENBERG: Do you have any notion as to why that's the case? Is this an identity crisis?

STEWART: I think so. They wanted to view themselves in that light. That's a continuing problem. There are some people like Brad Sturtevant, for example. I think he would be much happier in a discipline-oriented organization where he can call himself a fluid dynamics man. From what I've heard, I think he'd viewed the cross-disciplinary thought as obsolete. In a way, this split off with the applied mathematics and applied physics operation shows that, in this area at least, they thought the cross-disciplinary approach was no longer appropriate.

GREENBERG: Do you personally see any advantage to the non-cross-disciplinary approach?

STEWART: Yes. I do. I don't think that it's bad in itself. I think that completely organizing that way is not wise. I think the cross-disciplinary approach has advantages in bringing to light new areas for research. Conversely, the purely discipline-oriented organization has an easier time of falling into the ivory tower and drawing up the drawbridges, with your outside contacts just professional journals and professional specialist meetings. That's too narrow a world. I think a top grade university needs a broader view than that.

GREENBERG: Applied mathematics has gradually emerged as a discipline since the Second World War. What is its status today? Does it subsist in the shadow of pure mathematics? Is this why people are somewhat self conscious of identity and have a need for a disciplinary approach?

STEWART: I'm the wrong one to ask, because my comment on that would be that, yes, they are in the shadow because they don't get around enough in the real engineering areas.

GREENBERG: This would never have been a problem for von Kármán.

STEWART: No. It isn't only a problem here; it's a problem in the applied mathematics groups nationwide, in that they tend to talk to each other and not to enough other people. For a time, they talked a lot to the nuclear physics people, but that's unfashionable now. I think I see signs of withdrawal.

GREENBERG: Did GALCIT change in any significant way with Harold Brown's arrival?

STEWART: I would say it did. I think the whole school changed, because Brown viewed his role in a different way than DuBridge did. DuBridge was a father/confessor for everybody. He was interested in what everybody was doing and he would give encouragement. I think that Brown felt a little diffident about the faculty. I think in several ways he organized things so that the faculty was supposed to be a self governing, democratic body, which in part it really has to be. I think we also suffered from excesses of democracy. [Laughter] Among other things, the job of department head or division head became an unattractive job because the ability of the head of the engineering division, for example, to grow areas or to influence what went on at the group level was really reduced, because each appointment had to be heavily processed by committees. He couldn't approve it; he could veto it by not forwarding it to the top level committee, where the decision to make an offer would be made. He had a veto power, but he didn't have any firsthand power to improve the health of the organization except through complicated political processes. I think we still suffer from excesses of democracy. And I think the job of division chief, or department head, like Hans Liepmann, is not an awfully attractive job these days. You've got lots of troubles and lots of difficulties and damn few satisfactions.

GREENBERG: Hasn't it gotten better since Goldberger came?

STEWART: It has in one way. Goldberger, at least, went back to the business where he was really interested in what people were doing. For example, I don't recall ever seeing Brown pat somebody on the back and say, "Hey. That was something real good you did." I never saw any attempt by Brown to guide the Institute into various areas, except by going back to the grassroots to see if something would come up from there. If you want to do something different, it doesn't necessarily come from there. New ideas can start anywhere. I have a feeling that Goldberger is much more interested in what individuals are doing and is interested in talking to them.

I think Brown viewed his duties in a more limited way. He did a very good job of handling the central administration. Caltech came out of the sixties much better than a lot of the universities in the country because he'd recognized that this boom for great expansion in all directions was not a good idea. He just didn't let us do it. The boom finally collapsed for economic reasons; inflation was finally getting to us by 1970.

GREENBERG: What about the social science business? Brown was responsible for that, and that's pretty controversial right now. Should the move have been made?

STEWART: Well, I still don't know what to think of it.

GREENBERG: I guess a fair amount of money goes into it.

STEWART: Yes. Let me talk a little bit on general philosophy for a moment. If you go back to the nineteenth century, education was the same almost everywhere. I know that when my father was going to high school, he took courses in ethics and physics and economics—all the classical things. President Jefferson, for example, knew everything about everything—about as well as any professor at Cambridge did. During the nineteenth century, the Renaissance man was the ideal. I remember Poincaré boasting that he knew everything about physics.

About 1890, I think, developments in the application of science and engineering technology had become messy enough that people in the colleges decided that you didn't need to expose everybody to these complicated affairs. They thought you could let some people specialize in science just as some people specialized in medicine. Places like Harvard, for example, had a BS degree and a BA degree, but the BA was the one with importance. Unfortunately, with the BA degree they very rapidly left out science.

By the middle of this century, we found that very few of the people with BA degrees got even as much as one year of honest-to-God modern mathematics or science. I think that's a real tragedy, because you can't understand why the modern world is different from the fifteenth century without understanding science and technology to some degree. At a science school like Caltech, where you also require the students to spend a fair amount of time on some of the classical ideas of antiquity, the old BA degree idea is more nearly followed than it is in most of the colleges that grant a BA. So it's really this thought that gives me a reluctance to come to any conclusion about the social sciences work at Caltech. I think that the basic idea of a little bit more of that in the minds of our people is probably all to the good. I just don't know whether the particular way they're doing it is all to the good.

GREENBERG: I guess it gets back to what the role of this kind of an institute should be. Is Caltech a typical university? Historically, the social sciences at Caltech seem to be an anomaly.

STEWART: It does in a way. On the other hand, Caltech is probably closer to what an 1890 BA did at Harvard than a Harvard BA is now.

GREENBERG: From the beginning, there was a concern that students here receive some background in classical studies. The people here, like [Clinton] Judy, were quite good at that.

STEWART: Yes. We taught our students that they could work hard, and up to the time they came here, most of them hadn't had to learn that. If they have a sufficiently broad viewpoint, a lot of our people are moving up in pretty powerful ways.

GREENBERG: What kinds of things have you been working on most recently? Anything very different?

STEWART: One thing that I wanted to do was to spend a little more time working with the windmill problem. I was a little disappointed at the way some of the windmill work was picked up by the government. They picked up the idea again in the NSF and later in ERDA and the Department of Energy. I sat on some of the NSF proposal review sessions in the early years. Their view of what was appropriate just didn't seem very productive. They ended up spending a lot of money at a lot of universities. None of the programs they approved were programs that I wanted to be associated with. That's the difficulty of the peer review group; if you get a certain kind of activity going, it tends to preempt anything different. The essential problem was to put good engineering on fairly large scale things. It's hard to break that down to a one-man PhD thesis.

Begin Tape 6, Side 1

STEWART: I submitted a proposal to NSF only once. Based on the reaction, I decided there just wasn't any point. The fellow who was head of the DOE windmill section was one of my old friends from JPL; they brought him back to Washington because in the NSF they only had experience in putting out research contracts to individual professors. I used to go back to see him and show him some of my parametric studies.

He also had difficulties. For example, one of the problems I saw there was that they brought in the NASA-Lewis group to handle their big windmill work. NASA, quite properly, started out with their first project, their Mod Zero windmill, in the quickest way that they could to get something going. They bought a paper design from a German who had built a hundred-kilowatt machine. This gave them something that they could build and start going. They wrote a very conservative specification where the thing had to be able to withstand all kinds of bad treatment and all kinds of system failures. It probably was just as well, because when they built the first one, they almost broke it a hundred times with poor operating processes and bad system ideas that didn't work very well. The problem was that when they went on to the next step, after they'd done some paper work to try to decide what kind of windmill might have an economic future, they didn't have the wisdom to change sufficiently that initial specification. So they kept getting relatively rugged but very expensive, very heavy, and very low efficiency machinery.

I would go back and talk with Lou and show him what I had. I guess the problem was what the people in Washington call a sensitivity analysis; they had specifications, and each specification statement had some implication for the final product. If you over-specify a project, the result is completely defined by the specification whether you realize it or not. If your specifications are such that you have to get low efficiency equipment, you're going to get low-efficiency equipment, because every contractor who works for the government today knows that you'd better follow the specification. Otherwise, you're going to lose your shirt. It doesn't pay to tell the customer how to get a better product, unless he asks you to tell him. This business of looking at individual line items in the specification to see what their overall effects are is called a sensitivity analysis. Over a five-year period, Lou was never able to get a sensitivity analysis program out of the NASA-Lewis people. I finally decided that I should do some of these things on my own here. I've been doing some of them and every once in a while I carry them back to the NSF.

I did carry some things back in 1980 to the summer meeting in Cleveland. They were interested enough so that they did follow through. That was just when I was retiring. Since I was on an early retirement program, they couldn't very well take a contract here because the government tax collectors would say that I wasn't retired; that was just an excuse to get out of Social Security payments. It was perfectly all right to do it through any other way, so they finally gave me a subcontract down at the University of Kansas at Wichita. I published a paper showing what the ideas were, but they wanted me to do a design for a fixed pitch windmill with certain other things. They wanted to compare that with some other things that they had.

It was interesting that by the time I finished with this calculation, I compared it with this first Mod Zero they had—a variable pitch design where they could change the pitch at each wind speed to get the best performance. With just a fixed pitch design, I ended up with a calculation that checked pretty well with their experiments. The procedures were pretty well calibrated; they'd done good predictions in the Grandpa's Knob windmill.

At its peak design point where you wanted to get full power, my design showed about 6 percent more power output than their Mod Zero machine; at two-thirds that wind speed, it showed 80 percent more power. At half that wind speed, theirs went to zero power, whereas this was still, in a dimensionless way, at half the peak efficiency. In other words, it had a much higher efficiency curve. Their curve, incidentally, was substantially below the Grandpa's Knob curve; mine was just about at the Grandpa's Knob curve, even though it had a fixed pitch. I finally gave that to them a few months ago, and I haven't heard any strong reaction; on the other hand, I haven't badgered them to see what they think about it either. At any rate, I am working, and I do see some ideas that I think have some real possibilities for improving the commercial product, as commercial products come along. As a byproduct of that I'm also trying to put together my last year's windmill class notes as an integrated manuscript.

GREENBERG: Was this course on windmill technology that you taught in the seventies a novel course?

STEWART: Yes, it was novel. It was not unusual—we've given special courses on special topics at various times.

GREENBERG: Here at Caltech, but is windmill technology unusual as a university course elsewhere in the U.S.?

STEWART: In several universities they have given propeller courses as part of the regular aero-engineering thing. Of course, the aerodynamics of windmills is closely related to either propellers or helicopters; the structure is closely related to airplane wings or helicopter rotors. So the aeronautical analog has existed in a number of places. We've never given a helicopter aerodynamics course here at Caltech; several times we have tried to give it, but for one reason or another it didn't go. But other people have done so.

GREENBERG: You were saying that there were advances in helicopter technology during the Vietnam War.

STEWART: Yes. These were mostly analytical advances that permit you to do your design work better to understand what the problems are. The real advance was in being able to do these messy problems with a computer. In the thirties, you could conceive of doing the problem, but you couldn't carry it out by hand.

GREENBERG: Can this be easily carried over into the windmill problem?

STEWART: Yes. Actually, we started thinking about the windmill idea in '71. I think '73 was when Ernie Sechler and I first actually did it. I don't think any other place in the country actually did a windmill series at quite the level we did, although there were many who did it at a very elementary level. Wilbur Nelson, who was one of our graduates—I guess he got his AE degree here about 1935 and spent most of his life at the University of Michigan—organized a windmill course at a more advanced level there than most. So our attempt was unique. I think we did it more completely from a systems standpoint than most people did. Of course, we had some prior experience with the Grandpa's Knob that helped. We spent a fair amount of time worrying about the aerodynamics and the various system ideas. We also studied why you're probably going to get—if you want a high efficiency system—an old-fashioned, horizontal axis windmill that looks more like a propeller than a windmill. We gave them a pretty good exposure to the literature. There is a collection of references that our library put together and made a big bibliography. We've got a file down in the library with a lot of data, and a lot of the appropriate background materials.

GREENBERG: And now you hope to publish your lectures in the form of a book?

STEWART: I'm making slow progress because I get distracted with many things, like that fixed pitch design I did for the University of Wichita.

GREENBERG: What about the rocket test series that you were involved in?

STEWART: I mentioned the special capabilities committee where we'd done the advisory work with the secretary of defense on the satellite area that led directly to the Senate appointment. Before that, in '47—before the Air Force was set up under the secretary of defense instead of under the Army—they had what was called the Joint Committee for Research and Development in the Defense grouping. That became the RDB, the Research and Development Board, after '47. The RDB had several subcommittees, one of which was a committee on guided missiles. Clark Millikan was one of the members of that committee, and for some time he was chairman. There were a couple of subcommittees that reported to that group, one of which was called the technical evaluation group. Clark had me appointed to that. For one year, during the 1951 period, I was chairman of that group.

Our duty was to examine all of the guided missile programs going on in the country on an annual review basis, and to prepare a status report on each of them for submission to the main committee. It was actually a technical auditing function, which they set up outside but paralleling the technical command function that went through the services. This was a very instructive and fairly influential activity. I think it helped considerably to speed our rate of development in the guided missile area.

I should mention that this business that I thought we'd made a big jump on in the early fifties where we really caught up fast on the Russians and the big ICBMs, came out of this RDB kind of thing. A number of special programs were set up. For example, each of the services set up a large-weapons program. The Air Force set up one under Ben Schriever, and the Navy set up one under [Rear Admiral] Red Rayburn, and the Army under General [John B.] Medaris. He was the one who headed up the Redstone activity and ran the Redstone development and the Jupiter development. The special programs that were started were the Atlas and the Polaris and the Jupiter; in all three cases, each of the services set them up with special administrative functions which in essence bypassed all of the normal intermediate administrative review echelons of the Pentagon. All three of them worked beautifully and made rapid progress. By '57, when all these things were working, they gave up the special organization and went back to the standard procedures; the rate of progress also went back to a normal slow rate. [Laughter] At least it proved that if we really wanted to do something and were willing to accept the superior organizational mode, we could do it.

GREENBERG: It's interesting that the decision to do it had absolutely nothing to do with the launching of Sputnik.

STEWART: Right. By the time Sputnik was launched, they were almost at the point of giving up the special organization and relapsing into the normal bureaucratic mode.

GREENBERG: That's interesting. The public didn't know anything about this. Is there anything we haven't talked about that you'd like to bring up?

STEWART: From the standpoint of my own personal history, I'd like to say a few things. In 1958, when NASA was formed and JPL was transferred to it, T. Keith Glennan, who was going to be the new administrator of NASA, and his deputy, Hugh Dryden, came out to California. They invited me to meet them in a hotel in Los Angeles one evening in September—which was just a few weeks before NASA was starting operation. They invited me to take a leave of absence from Caltech and go back there to head up a staff office in the headquarters under their office for planning purposes. I accepted it and took a leave of absence here. We left about the first of November. I took the family back East and bought a house just outside the District line.

We kept our house here; I had no intention of staying in Washington. We spent two years there.

It was an interesting exercise, and I think that I stayed long enough to do something useful. I had a fair impact on what went forward. The main thing that Glennan wanted from me and the group I set up there was a draft of a long-range plan. In terms of that, you could then make the short-term, year-by-year budget decisions and program decisions that would keep you moving in the right direction. Most of the things that have been done between then and now were incorporated in that plan in some form. The planetary business went ahead much faster than we visualized in those two editions of the plan that I had anything to do with. The bottom line on the long-range plan that was put together the first year and submitted to Congress had a nonscheduled item, which was the manned landing on the moon—it was there largely as an indication of the general direction that the whole activity was moving toward. We didn't have any formal statement in the plan that went to Congress which even sized that activity.

In the fall of '60, I got a request from the White House scientific staff to brief their people on our thoughts on the manned lunar landing problem. Some of the regular people in the program offices of NASA headquarters were a little reluctant to stick their necks out on such a thing. I did the briefing pretty much myself, although most of the numbers came from the organization. The time schedule we gave them was ten years from whenever you decide to go; we didn't think that it was quite the time to go yet, and we weren't considering that it would be right the following May, when it actually became time to go. I used a twenty- to forty-billion dollar cost estimate, which Kennedy later used in his speech. I think that's probably as good a brief program estimate as anybody's ever made for the government.

The reason the White House had asked for it was the people there wanted to know something about it because they figured it would be coming up again. It was after the election, of course, and Kennedy was going to be the new president, and Eisenhower didn't want to do anything to pre-commit him in any way. He sort of put this in the same category as he did the Cubans who were in Florida. He wasn't going to approve any program for their use of Cubans because it was too important, and he didn't want to pre-commit his successor to a bad plan or to a plan he didn't understand. We briefed the White House people on the Apollo thing. But we understood that they weren't going to do anything about it right away.

GREENBERG: You estimated ten years and it was slightly less than ten years?

STEWART: The first landing was eight and a half years. It was a good estimate. I did it by looking at other big projects. People usually don't realize how big some of the projects were that were done in those days. The biggest one I knew at the time and one that I used most strongly for scaling the costs was the B-52 program. They started building the B-52s about '52, I guess. By 1960, all the structures had been built and they had all been flown and they had all been run back, most of them, through the mod lines to put in the updated instrumentation and guidance equipment and all the rest of this stuff that they needed. I guess they finally shut down the last mod line in '62.

As of 1960, when I put together all of the different cost elements I could think of—which was hard to find in the federal budget because they're never set up to do that; they're set up to decide whether the money is spent legally—the B-52 was up to fifty billion dollars, and that was fifty billion in 1955-56 dollars, which would be about three hundred billion now. That was the biggest thing I looked at to try to get a scale on the cost for the Apollo. I tried to do it by deciding what was the level of effort required, and then I tried to find something comparable. It was a little less than the B-52s, but not much less. Yes, a dollar went a lot further in those days.

GREENBERG: In his autobiography, von Kármán stated that Prandtl's work was of Nobel Prize-winning caliber but that people in mechanics do not win the Nobel Prize because the field "is not as sublime in the twentieth century as the branches of physics." I was wondering if you had any thoughts on that.

STEWART: There's something to that. You know, there were people like Poincaré who could say he thought he understood all of physics. In fact, there's a classical physical problem that had been identified twenty years before Poincaré's remark which still is not solved. That's the problem of turbulence. It has application in all kinds of places, not the least of which is stellar dynamics. When we get Bob Christy to give us a seminar here on stellar dynamics, they find they have to rely on turbulent mixing in order to get the energy out of the star. Of course, they're stuck with the same problem we are with the atmosphere and a lot of other things, where you can use empirical rules that enable you to scale the effect but you can't calculate it *ab initio*. Frankly I would think that if anybody were to solve the turbulence problem—and that's purely applied mathematics now I think—there's reasonable evidence that no additional physics is necessary beyond the Navier-Stokes equations. The problem is that they're so complicated in nonlinearity that nobody knows how to do the statistics of averaging. For example, I think Einstein pretty well solved the Brownian movement problem, but that's a very simple problem in comparison. I think the turbulence problem is a very good problem in mechanics, which would deserve and probably get a Nobel Prize if anybody would actually solve it.

GREENBERG: I guess the problem for von Kármán was he thought that Prandtl had actually done some things that were of Nobel Prize-winning caliber—like, introducing the concept of the boundary layer. He thought that was fundamental and really as good as anything that anybody did in that time, but Prandtl, of course, never got the prize.

STEWART: No, he didn't. I don't disagree with Kármán's assessment that this was a piece of work as good as what many people did, but it was different. It was a simplified view of the physics which turned out to be useful where you neglected the full details; whereas the Nobel Prize went more to people who were finding out more precise details. It was a different line of thought, and as far as I know, it's a line of thought that's appropriate to applied mathematics, but it's not appropriate to physics.

GREENBERG: From reading some Caltech publications about current work in aeronautics, I'd gotten the impression that there was a new line on the turbulence problem, that the old approach of the thirties of statistical averaging was somehow in the process of being modified as a result of some experiments.

STEWART: It really isn't. The idea of a universal turbulence pattern and wall flow, for example—what they've got now is similar to the wall flow idea. This is a pattern that happens in turbulent mixing, the jet interface, for example. They see characteristic structures that come out of that. In boundary layers on wings or other things, the transition zone is characterized by typical structures that are observed—this idea was first noted thirty years ago. The more modern instrumentation of the laser velocity measurements has pretty well identified similar structures in the typical structures in the jet mixing zones.

GREENBERG: That is to say that there's more of an overall pattern than had been noted hitherto?

STEWART: It seemed to be more consistent. In a way, I would interpret this as meaning that this should lead to more reliable empirical rules for closing the mathematical structure than the old empirical rules. But I don't think it gives a clue yet as to the fundamental interpretation of the turbulence problem. It should enable you to do better prognostication of many turbulence problems, while still using empirical data to establish closure problems. It's still a step short of what I would consider a Nobel Prize solution of the turbulence problem.

Begin Tape 6, Side 2

STEWART: I have been doing some work which might sometime lead in that direction. I talked with Hans [Liepmann] long years ago, and I thought that if you could really work out in detail an exact solution of the Blasius boundary layer equation singularity, that might be a clue as to the direction to go for a more general investigation. I did some miscellaneous work with that fifteen years ago and decided I wanted to spend more time on it. That's such a long shot research thing that I couldn't possibly ask anybody to support it, so eight or ten years ago I asked to go on three-quarter time to leave me a little time for some personal research. That was one thing I had my mind on. I learned a lot of odds and ends of nonlinear equations, and a dozen times I thought I'd made a breakthrough to what might be a new understanding. To date, those things have always been aborted. [Laughter] Every few months I'll pick it up and work on it again, and will probably work on it as long as I live, unless I finally get somewhere. It is a possible way of getting at the Navier-Stokes general problem. It's been fun; I know more than I'm interested in about all kinds of classical nonlinear equations; I go from one intractable form to another so far. [Laughter] Of course, the problem from a numerical standpoint is, if you want to get the numbers, just compute them off to any accuracy you want, but the significance of the answer is no more than that of the closure assumptions. I was trying to do the more applied mathematical problem and get a form of solution of the Blasius equation in which the constants of integration appear explicitly, so you can see the functional relationship that's involved there. I have some new clues to pursue. It's an intractable problem, and I'll work on it a little bit, now and then. It's been fun but nonproductive, so far.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

SUPPLEMENTAL INTERVIEW WITH HOMER J. STEWART

BY SHIRLEY K. COHEN

ALTADENA, CALIFORNIA

SUPPLEMENTAL INTERVIEW WITH HOMER J. STEWART
1993

INTRODUCTORY NOTE

The Archives' original interview with Homer J. Stewart was conducted in 1982. Subsequently Dr. Stewart requested a second interview. He wrote to the archivist:

> A part of my life that always seemed important to me and was not explored to any extent in our previous discussions was the many interactions with government agencies that were not directly connected to the academic program at CIT. I know of many similar stories in the lives of other CIT members. The oldest story is of R. A. Millikan's work with the NRC [National Research Council] and his research on underwater sound and the development of SONAR in World War I.
>
> Great emphasis was always placed by von Kármán on the importance of developing a broad range of professional contacts, not only in academic circles but also throughout industry and the government. The energetic propagation of this viewpoint throughout our small group was, in my opinion, largely responsible for the timeliness and productivity of our researches and for the strong role played by GALCIT in developing the modern aerospace industry.
>
> I feel that a collection of this type of information from the senior members of the CIT staff might well suggest reasons why this small institution has had a worldwide reputation.
>
> (February 2, 1985)

The 1993 interview captures in part Dr. Stewart's memories of his service to government agencies and congressional committees during World War II and the years of the Cold War. It also includes reminiscences of Kármán, Clark Millikan, and other Caltech colleagues such as McCoy, Biot and Zwicky; the Caltech wind tunnel; details of airplane design; and observations on the establishment and growth of California's aerospace industry.

Dr. Stewart provided the Archives with a list of his government and industry affiliations. That list is included in an Appendix to this interview.

November 1996

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

Interview with Homer J. Stewart **by Shirley K. Cohen**

Altadena, California

November 3, 1993

Begin Tape 1, Side 1

STEWART: Well, you must have the first word.

COHEN: Okay, Dr. Stewart. We're going to talk a little bit more about your life outside of Caltech, as far as your professional work went—how that began.

STEWART: Well, of course, one thing you've got to remember is that engineering was a very primitive business in the 1930s. It was just starting to become technical, in the sense that physics and science were more important than government rules and regulations. The government rules were nonsense; you were a fool if you only followed them. Well, I was thinking in this line when I came to Caltech. But really I started earlier than that, although I didn't realize it. About 1950, I was thinking about what had gone on during the previous fifteen years, and suddenly realized that I could only remember the names of four or five of my professors back at the University of Minnesota. I wondered about that. Finally I decided that the thing that made this particular group different from the others was that they were the ones who had outside connections: connections with industry, connections with the government science offices. When I came to Caltech, this point was impressed upon me even more strongly. In thinking back on it, I realized my interpretation of what I'd done at Caltech.

COHEN: It just came to you?

STEWART: I hadn't had it as a line of thought, of how to interpret things up to that point. But as you know, [Theodore von] Kármán was a, well, the title of the book up there is *The Universal*

Man, you know. He wanted to know everybody. He knew essentially all of the people in the industry. They were largely concentrated here in California by that time. He had them over for seminars. He knew the military because by that time—by 1936, it was clear the war was coming on. Our poor military couldn't do much about equipment, although they'd already got the first big government money since World War I—they got a whole million dollars for the design of the B-17 back in, what, 1934 or 1935. They couldn't build equipment, but they could at least train their young officers. We had a group of the best young men that the services, both the army and the navy, could pick out and send to us. And they were very good. I remember in the first classes I taught, there were three of them that ended up as lieutenant generals.

COHEN: Now were they special students or were they regular students?

STEWART: Regular students.

COHEN: Sent by the army to Caltech?

STEWART: Yes. There were a couple of naval officers that ended up as vice admirals in that early group. They sent us the best people they could.

COHEN: Who made that arrangement with them?

STEWART: Well, Kármán was largely responsible for that. He talked to all the people back at Wright Field and he talked to our congressional people. He knew people in Congress.

As a matter of fact, in the fall after I'd been here three months, Kármán asked me to take over a job for him which was being done for Congress. This is the story of the Macon and the Akron, if you remember those big dirigibles. The last one to die was the Macon, which went into the ocean off the coast here. The failure took about, oh, a whole afternoon. It was first damaged and then they dropped an engine aft and valved some gas forward. They had a log that kept track of all these things. The congressional committee investigating the thing decided that you ought to have a scientific look at this. They organized a committee headed by [William Frederick] Durand, the head of the [aeronautics] department up at Stanford. Durand incidentally was head of another committee which you probably have heard of. It was a committee set up by the Guggenheim Foundation to put together a good scientific series of books bearing on the field of aeronautics. It's called the Durand Series. Here are the books.

COHEN: That's okay.

STEWART: Well, I wanted to show them to you for a special reason. Here's the first one.

COHEN: Okay, and did many people here work on that?

STEWART: Well, yes, Kármán has a piece in here. And there are people all over the world. And the arrangements were made to publish them in Germany. They were published by Julius Springer. You can see the little paste-on there. Back when World War II came on, we decided they needed lots of copies of these things. We were the people who did all the reprint books. Anyway, they borrowed my set of the volumes, took them apart, and made the corrections. I had a list of corrections that I'd done through them. They did the corrections and reprinted them. After they were reprinted, they rebound my things and gave them back to me.

COHEN: So that's a historical book.

STEWART: Yes. So that's the same Durand. Now, to return to the Macon, what Kármán did for Durand was to arrange with one of the naval officers here [to study the Macon dirigible failure]. He did this as an MS degree-type thesis and set up the equations of motion. He calculated what the airship should have done from the record of the control settings, the speed, the altitude, and so on. To the extent that it didn't follow that, you could infer what the gust structure that hit the thing must have been. And so he carried that through and spent a year working on it. Then he'd gone off—got his degree and gone off. The report was due in Congress in January so Kármán wanted me to sort of finish it up. Well, I got into it about two weeks and there was something I didn't feel right about. And I remember talking to Francis Clauser about the differential equations that were being used. There was one there that I wasn't too certain of. "Yes," he said, "Yes, that one's all right." And suddenly, I realized what the problem was. The problem was that the differential equations were worked out in a coordinate system that was tied to the airship, so as the airship pitched up, the coordinates pitched up. In other words, it was a moving coordinate system. While the aerodynamic coefficients used relative wind coordinates.

COHEN: The wind was horizontal?

STEWART: Yes, but there were gusts and the airship moved and the aerodynamic coordinate system was tied to the relative wind rather than to the airship. So here we were about five months short of being due in Congress and all the calculations that had been done to date were in some sense wrong. So I dived into it. I've still got the paper, the file of this stuff, somewhere in my things. I put in fourteen hours a day from then until it was finished. In the process, I had to take incompletes in every one of my classes.

COHEN: You were a graduate student?

STEWART: I was a first-year graduate student. That's a heck of a way to start a graduate course!

COHEN: Kármán must have had great confidence in you.

STEWART: But I did it and I got it done. And the results were of considerable interest—shed a lot of light on what was going on.

COHEN: Would you say that was your first introduction to working with someone outside the university?

STEWART: No, I had done it before. Go back to 1934. The Weather Bureau had put out two contracts for developing radio equipment that could be carried up on balloons—radiosonde-type equipment.

COHEN: Right, I think we have that. So you don't have to tell that story again. That's a good one. So anyway, you'd had some previous experience.

STEWART: And I'd had previous experience with industrial things through two airplane design projects I'd done.

COHEN: So there always was this liaison between work and study.

STEWART: Well, I had some connection with the real world, yes. At any rate, this business with the Durand committee and Congress was, you know, quite inspiring for a young fellow. All at once, you're messing around with national politics.

COHEN: Did you actually go to Washington?

STEWART: No, I never had to do that.

COHEN: That was something that Kármán did?

STEWART: Kármán and Durand and so on.

COHEN: Well, I hope they gave you credit.

STEWART: Oh, probably. There probably was a list of helpers. But that was quite an eye-opener for me. Kármán also had set up an arrangement so that the chief engineer at Douglas came over and gave a series of lectures to us. In my second year here, that series of lectures was the sort of thing that Maj [Arthur L.] Klein handled and was about various phases of airplane design problems. The next year, that course started out with Maj Klein handing out a little story sheet which was essentially your assignment sheet for the term. This story sheet pointed out that the U.S. Maritime Commission had just put out a report which said that now that the DC-3 was coming along and now that we could see what it was going to be, no nation will ever build a passenger ship to carry passengers across the ocean for purely economic reasons. It can't compete with the airplane. They may build passenger ships for military reasons or for military support reasons—to carry passengers as part of the cost of having them available—but never for purely civilian reasons. That was a very foresighted thing. Most of our politicians didn't understand it for thirty years. And we, all of us who were working on this thing, had a good incentive in the class to see what was going on. And it wasn't only that. Clark Millikan, for example—

Oh, another thing about Kármán. He was interested in all these things and he had lots of good ideas and did a lot of good initial work. But he was also very lucky because every once in a while, when he got to a point, he had a good first-rate youngster to pick it up and carry it on. Now that happened at Caltech with the wind tunnel. In Kármán's first contact with Caltech, they decided that the plans for the wind tunnel that they'd had were not the best. It would be better to do it as a closed wind tunnel and get a higher efficiency. What happened is that in getting a higher efficiency, they also got a lower turbulence. Clark came along, and he took up the wind tunnel. It was his prime responsibility. He carried it on and he made it a world-renowned device.

COHEN: Do you think the idea of the wind tunnel was to get more cooperation with outside people? More than just a teaching device?

STEWART: I don't know to what extent that was a prime incentive in the first place because there were plenty of incentives from any standpoint.

COHEN: I see. Anybody who can design something—

STEWART: Now it was true that there were the government laboratories. The NACA [National Advisory Committee for Aeronautics] had some wind tunnel facilities. And they did put out reports which summarized their experiments. A lot of the simpler parts of the aircraft industry were satisfied with extrapolating or interpolating or whatever you had to do from the NACA record, and accept that as being good enough. The thing that made the DC-3 different was that, if you thought about economic problems, why, a three percent or four percent difference in productivity is a world of difference in the profit line after you take all the costs off. And suddenly this was enough so that the DC-3 could carry passengers without a subsidy. You could carry even if you didn't have the airmail subsidy and still make money.

COHEN: So that was an economic decision.

STEWART: That extra few percent of aerodynamic efficiency they got out of the wind tunnel tests done at Caltech.

COHEN: Is that right? Those were done here? The DC-3?

STEWART: Oh yes. In fact, that was a good part of my work, night work in the tunnel during the 1936-37 period.

COHEN: Now was that part of your studies? Part of your thesis work?

STEWART: Well, I was invited to come here. My professor back in Minnesota said I should go to Caltech. So I wrote and inquired about the support because I was penniless except for what I earned. I got back a letter offering me a wind tunnel fellowship which paid sixty cents an hour. As a matter of fact, the first seven months I was here, I made $500. Golly, I was so rich, I could hardly stand it. [Laughter]

COHEN: It's not the usual thing, you know, paying the students.

STEWART: Yes, but laboratory assistants are normally paid. So I worked on the wind tunnel. I did my classes except that I had to take incompletes that first term. I did go to the classes. I just couldn't do the extra work. I got sidetracked here.

COHEN: What I would like to go on with is the torpedo problems—the 1942 NRC [National Research Council] on the torpedo problems.

STEWART: Okay, we'll get to that. Well, anyway Kármán brought in all of these people. So we did have contacts with everybody. He brought in good seminar speakers. He brought in Sydney Goldstein from Manchester, who was here for a year, or six months maybe. He brought in people that did different things, and just as a general eye-opener. But it wasn't until 1950 that I realized his was kind of a unique viewpoint. There were an awful lot of places that didn't do this.

COHEN: That's correct. I mean it may even have been unique to Caltech.

STEWART: It was more extreme here than at most places.

COHEN: But I think when we were just chatting, you said that this really was always Robert Millikan's idea. That Caltech would provid—

STEWART: That of course was one of the basic reasons why Caltech was put together in 1918, when Millikan agreed to come and take over. It was because of his feeling, which was in agreement with the Board of Trustees group, that California needed a place like MIT that could provide a science center which could provide the support for industry that industry needed to have. Up to that point, California didn't have one. Yes, so the basic idea was wider than just Kármán. But he—

COHEN: But he carried it to an extreme.

STEWART: He carried it, yes, he worked hard at it. And Clark worked hard at it, too. Anyway, I got through that first year. I made up my projects so I could get my grades and continued as a student. But, oh, a whole series of things went on. We started getting secret projects at Caltech. They came in as the war got progressively closer.

The first one we had was a television-guided bomb that was a project of Hugh Dryden at the National Bureau of Standards. And then we got the testing for what became the P-38, our first twin-engine fighter. A good long-range fighter. As the war came on in Europe, the British realized that they needed to have a long-range fighter. The Spitfire was beautiful and wonderful and a lot better than the [German Messerschmitt] Me 109 in several respects, but it couldn't fight a battle more than 100 miles from its air force base, and Berlin was 500 miles from London. One of the first things that happened in 1939 was that the British made arrangements with what was then North American—it is now Rockwell—to do the design for what became the P-51. The P-51 could accompany our bombers clear to Berlin when we got there. So we got a two-year start on actual military weapons for the war.

COHEN: When you say we, whom are you referring to?

STEWART: The United States in this case.

COHEN: I see. Not people just at Caltech.

STEWART: Yes. But Caltech was an essential part of it. And we did the wind tunnel testing on it.

COHEN: I see. So people in aeronautics like you.

STEWART: Yes. One of our young fellows who had just gotten his engineering degree the year before and gone over to North American, he was one of their aerodynamicists put on this special design job.

COHEN: So he came back just to work on that project.

STEWART: We saw him every time we had a wind tunnel test on the P-51. He was there. And incidentally, one of the first tests was very interesting because jet engines later became important. There were theoretical papers on jet engines before that. The design of what later was called a ramjet had been done theoretically, but it was kind of hard to believe. I remember a seminar in 1936 when some of that early work was being discussed. You could see the numbers and you could believe the numbers, but it was still hard to believe that it actually would work. Well, when we put the P-51 in the wind tunnel, one of the things that was set up with it was a special radiator drag investigation because it was a water-cooled engine. Radiator drag was always an important thing with water-cooled engines. The people at North American decided that if they design it with these ramjet principles in mind, maybe the drag won't be so bad. To test it in the GALCIT wind tunnel, we had this radiator installation and we had an electric heater inside the radiator to simulate the heat input. By golly, when we turned the electricity on, the drag went down. So that was one of the things that made it a good long-range fighter. It had a better radiator design than any of the others. [Laughter]

COHEN: Oh, that's interesting.

STEWART: Well, all sorts of things like this went on. And Caltech people in general had a very wide involvement. There were an awful lot of people with an awful lot of interest and interactions on the national scale, particularly in terms of wartime military intelligence activities.

COHEN: But then after the war, when the [aerospace] industry really got established here. Maybe you want to talk about that a little bit, too.

STEWART: Yes, but the big change happened during the war, of course. Before the war, the [airline] industry was really amateur—it had a little support with postal subsidies which permitted it to carry passengers—but the DC-3 changed that.

The earliest analysis of jet engines was done by a Frenchman, Maurice Roi, in 1918. It was published in this country by the NACA. As I recall, it was one of the early reports in their very first NACA report volume of 1918. And Roi did a magnificent analysis there. Even his metallurgy guesses were pretty good as to what you might do with high temperature metals. Unfortunately, as he finished up his discussion, he said, well, it looks as though this kind of a system is of interest, but probably not much interest unless you want to go faster than 350 mph. Well, in 1918, 350 mph sounded utterly ridiculous. What he did was almost completely forgotten. But when we started re-doing the jet during the war… In fact, we had a special class in aeronautics which was only for military students, where we taught them about jet engines of all kinds. By that time, the fighters were up to 350 mph and the question was, what are you going to do to get beyond that? So it suddenly was no longer theoretical. Suddenly it was real.

Well, this thing of theoretical problems turning real, let me tell about two or three other instances. The first one that I recall was in 1932.

COHEN: Now, was this done at Caltech?

STEWART: No. No, this wasn't. This was done primarily in England by the British. I think the DH Moth was the name of the little airplane. It had had a series of accidents in the previous year. Suddenly they realized that these accidents had a common cause. It was the phenomenon of wing flutter, where if the wings were not designed stiff enough and you flew too fast, they could be unstable. If some disturbance caused a small oscillation, the oscillation would grow in amplitude until it would just tear the wing off. That had been thought just an academic curiosity before then, but suddenly, in about a year's time, it turned into a real problem, important all over the world. So that was the first problem I recall of this sort.

The second one happened in 1938. I mentioned that we'd done the early work on the P-38—classified work in the wind tunnel on its design. In one of the very earliest flight tests, out of March Field and over towards Palm Springs, the pilot dived it. The speed went up and up and up. Finally, he decided to pull out and found he wasn't strong enough to pull out. He hollered and yelled on the radio all the way until he crashed. Killed, hit at darn near 400 mph. It didn't take very long before suddenly people realized that this was a compressibility effect. Now, the previous year, I'd taken Harry Bateman's course in compressible fluids. And that was, at that time, considered a very esoteric subject. [Laughter] But suddenly, that was real.

In another year's time, by 1939, Kármán got Hans Liepmann here. Kármán wanted Liepmann to pick up and start working on compressible flow problems. In 1940, he got Allen Puckett here from Harvard. Puckett came in because Kármán felt that the army, not only the air corps but also the army ordnance, needed to have a good wind tunnel so they could understand what their ballistic missiles were doing. It was Puckett's task to develop a practical design for the supersonic wind tunnel. So that was an enormous change.

A key first step in upgrading the aeronautical design practice to handle the flutter problem came from GALCIT in 1934. If you look back at [Ernest] Sechler's thesis, it involved the buckling strength of thin sheet structures which were reinforced with ribs and stringers. Airplane wings, by the time of the DC-3, were largely designed that way. The fuselages were also designed that way. One of the problems was that the basic theoretical analysis had been developed by [Stepan] Timoshenko and others, but the experiments didn't agree with it very well. So what Sechler did was to put it together. He did enough testing and got enough data so that you could understand why the simpler theoretical analysis was incomplete. It was this structural development that made the DC-3 and the previous DC-2, the Boeing 247 and the Northrop Delta. In fact I think Northrop was probably the first plane to actually be built using Sechler's ideas. But this was so much better that it made aluminum the structural material for airplanes.

COHEN: So it was directly out of Sechler's thesis?

STEWART: Quite directly. One or two other people worked on it, including Kármán of course. It was Sechler that made the engineering of a high quality. So that you could use these things and understand what you were doing.

I remember many times sitting in a DC-3 as we were flying along in rough weather. I'd sit and look out the window, and I would see little waves running out the wing and running in the wing as the wing was flexing. [Laughter]

COHEN: Makes me nervous.

STEWART: Well, modern planes are bigger and the panels have thicker metal. You don't see the waves anymore. [Laughter] I miss them. Well, that was an intermediate big change.

The next big change, and almost the last one in this story I want to tell, happened during the war. Our intelligence was good, we knew what was going on in Germany—we knew about the V-2. Charlie Lauritsen was head of the team that went over to Sweden to examine the V-2 wreckage. I'd done a bit of military intelligence the same way. A German was picked up who had done work on one of their guided missile projects. They brought him over to the United States and I was called in to help with the debriefing process—to find out what he knew and what was going on. I think I have mentioned that Louis Dunn and I had done the debriefing for Ernst Stuhlinger and Von Braun. Von Braun was not a highly educated engineer, but he was a good manager. Ernst was, you might say, his prime technical assistant.

COHEN: And they both went to work here?

STEWART: Yes, they both came over at the same time. The army brought them over and put them in the hospital at Fort Bliss as a holding station for them. And Louis Dunn and I went down and spent a week with them.

COHEN: Now, did they speak English or did you speak German?

STEWART: Louis spoke better German than I did. My German was very bad. I could read it moderately well, but I never learned their vowel sounds.

COHEN: Did you understand spoken German?

STEWART: A little bit. But they knew Pidgin English too.

COHEN: You didn't have an interpreter?

STEWART: Their Pidgin English was better than our pidgin German.

COHEN: Were they forthcoming?

STEWART: Yes, they wanted to come here.

COHEN: So they wanted to cooperate?

STEWART: Yes. What we found was that our understanding of the V-2 was pretty good. The problem with the V-2, from a military standpoint, was that it had been a sad waste of funds for the Germans. What they had done was to learn how to make a big rocket engine. Then they made a rocket to go with it. But their engineering processes were not very good at all. For example, it took them 700 flights before they had an auto pilot that wouldn't destroy the missile. They had a guidance system on top of the auto pilot in order to get a little more accuracy. But the first guidance missile test flight was the one that went off to Sweden. After a second flight which also ended up a failure, they simply gave up the guidance system. They didn't even try it.

COHEN: Well, they managed to get plenty of missiles to London.

STEWART: Yes, they did. But London was about the smallest size target they could hit, and the missiles were more dangerous in the launching area than in the target area. The effort they put into this project was tremendous. That was one of the things that we did worry about. We wanted to get a better feeling for the overall effort. It was like ten or twenty times what the British and ourselves together had done on all the jet propulsion projects. They put in a tremendous effort and got very little out of it. It scared the people a lot, but they never hit their targets. All the flights to London were aimed at the big power station on the Thames. They never hit it once, not out of all those thousands—more than two thousand.

COHEN: Unfortunately, they hit a lot of people.

STEWART: Oh, well, yes, of course. But after all, the British people still remembered Ypres in the First World War when the casualties were much, much worse. That was something we didn't understand very well. You must keep things in an overall context. While the V-2 put a little over 1,000 tons of HE [high explosive] into London, there were many nights when our bombers put 10,000 tons of HE into German cities.

COHEN: So another big change is—

STEWART: Yes, the understanding was that a heck of a lot of engineering had to be done to make guided missiles that worked. Well, it had come to us in the military during the war, a couple of years before the interview with Von Braun. That was really the reason why the Jet Propulsion Laboratory was set up. It was set up with the army, and Caltech felt it had to be separate from the Aeronautics Department because it involved chemistry, metallurgy, and especially electronics and guidance instruments. The conclusion was that it had to be set up as a separate agency, an agency associated with Caltech but not part of any one department. Now of course, the people in Washington who recognized this need were in the circles of Robert Millikan, Clark Millikan and the NSF [National Science Foundation] people. Kármán also, although his interest was more through the military end of the communication link. You see, by 1943 Kármán had already taken his leave of absence and was gone. He never came back really, although he did visit us several times over the years.

COHEN: But he was on leave all those years?

STEWART: Yes. By 1942, he decided he just wasn't going to get around so he formally took leave. He was a special assistant to the air force—the air corps in those days. Now they did set up another project just like JPL. It was the Bumblebee Project at Johns Hopkins which was set up under the navy leadership as part of the same deal. The decision was made to look at things in a more comprehensive manner in order to find out why it was that from the first guided missile attempts back in 1919, all through the war, none of them had really worked.

COHEN: Now that is interesting because in those days military projects were set up with universities in mind. And all of a sudden, in the 1970s, we have the national laboratories with no affiliation.

STEWART: Well, I think we have a lot of socialist trends going on in this country. The Russians have learned after seventy years that it's a bad way to go, but I'm not sure the Americans have. [Laughter]

COHEN: You do not think the establishment of the national laboratories was a good idea?

STEWART: Well, I think when they kept JPL, Caltech was associated with it. Cal Berkeley associated through Los Alamos and Lawrence Lab. One of the worst things that happened to MIT was when some of the MIT staff essentially chased Draper out. MIT never was as significant after that as it was before. So I think all these things that isolate the universities, that make them more pure, may have been fine for some of the professors in the universities, but it was very bad for the national interest. I think it was good for the students when the professors knew what was going on. When they're reading the newspapers they are more apt to say something that's meaningful than just the logic that was formally validated 300 years ago.

COHEN: Yes, that is certainly true. It has been in aeronautics and engineering where people spent a fair amount of their time doing consulting. And it was encouraged in some sense.

STEWART: Yes, well, it's always been encouraged. The formal rules here were that you were encouraged to do up to one day a week of consulting, provided it was of a professional quality and not just drafting work for somebody. That kind of a rule is not uncommon in academia. That was the rule up at Berkeley. Now it's true that a lot of Berkeley professors violated this rule and put a lot more time than that into their consulting work, so it became a kind of a scandal up there. But that was because the professors violated it, not because the principle was bad. Well, I've never known anyone here to really violate it, although during the war, you remember I told you that Kármán was gone full time for two years before they finally gave up and recognized reality.

COHEN: Yes, those were extraordinary times.

STEWART: Now, I spent maybe as much as a tenth of my time on government interactions of one sort or another.

COHEN: Was that mainly advice to the government?

STEWART: Yes, trying to help some government office that needed highly specialized help.

COHEN: Could you think of one particular instance just to give an example of this sort of thing you are talking about?

STEWART: Well, there was one that I spent the most effort on. Did I ever tell you about the Senate Preparedness Investigating Subcommittee? I don't think that's in my other memoirs.

COHEN: That's a good representative—

STEWART: It's a good representative one. There was a big problem that came up in the mid-1950s and broke into the press with the first Russian satellite launching. This was called the missile gap problem. Now, it was true that there was a gap. It was revived again in the 1960s with Kennedy, but that was just because they were trying to dig up something that was a democratic scandal.

The scandal was back in the Truman days. In fact I was one of the first people to get hit by it. At JPL at the end of the war, 1945, 1946, and 1947, my group had a little contract. It was called JPL 8. We wrote a series of reports. What we were doing was examining the requirements for ICBMs [Intercontinental Ballistic Missiles] and satellite launchings for the navy. It was really satellite launchings, but of course the two were the same. So we were also doing ICBM launchings. As a matter of fact, at the end of the war, the army had their big conference at Fort Benning. Eisenhower was a visitor for the evening cocktail hour. The next day, every four-star general in the army was there. As part of the program for that first day I was asked to give a lecture on guided missiles and what they were going to do to the future of weaponry. Clark Millikan had been asked to do it first at Fort Sill. He asked me to take it over for him because he had other things to do. So I'd done it. And then, when the follow-up at Benning came, Clark asked me to do it again. So I gave them a lecture and told them about the state of affairs and talked about surface-to-air missiles and surface-to-surface missiles—ICBMs. I even ended up with a little bit about flight to Venus.

So everybody was busy thinking in these terms. Now the real problem of missiles, of course, was that guided missiles didn't work. That's why JPL and the Bumblebee Project had been set up. They were to try to find out what you had to do in order to make these damn things work. Well, we were pretty sure we knew what the problems were. It was just that it was a much more complex interaction problem than a standard airplane, even a military airplane. You had to learn all the interactions. You had to understand the interaction of the flutter problem on the guidance problem. [Laughter] So we set out with the objective in mind, to find out why the projects were not successful. The high tech idea had originated in two quite different lines of endeavor. One was in the airplane business where you sort of dated it back to Felix Klein who visited the 1892 Exposition from Germany and then went back to Germany. He said after what he saw in the United States, with all its resources, that by golly, by the time we grew up and had filled out our land a bit, why Europe was going to get smashed in the competition unless they did better. So his question was, how can we do better? He thought we can do better by putting more science into industry. This was the source of getting an aeronautical department where [Ludwig] Prandtl was. I saw Prandtl before he died. Just once in 1938, he visited the United States. I met him at Harvard. Of course Kármán went through that in his education period. He was in a sense Prandtl's prodigy. This idea was floating around in aeronautics in all kinds of ways. The whole NACA was an example of this. There were half a dozen universities that were trying to make the conversion. But the 1930s was when that conversion started actually working.

COHEN: Let's get back to your briefing at Fort Benning.

STEWART: Well, the briefing went off fine, but two years later, the navy suddenly cancelled our contract. Well, what had happened, and this was too bad, was that Truman's strongest interaction in scientific and engineering affairs was with Vannevar Bush. I remember Clark Millikan working in those circles. He and Vannevar Bush were together quite often. I remember having lunch with them several times after the war. Van Bush was convinced that these complicated things like ICBMs and satellites were just never going to happen. Just too many things. Here was a man who was exposed to the best possible quality of information that you can carry across the continent and was acquainted with the people who were intimately involved with what was going on, like Clark Millikan. Still he didn't understand it. The first Corporal, the first thing that JPL flew which simulated a full missile configuration except that it didn't have an active warhead in it, we launched at White Sands. My duty in that launching was range safety officer. I sat there with my hand on the little throttle that could maneuver the airplane by the command circuits that were involved in the guidance system, and I could push the destroy button. I had nightmares about that. I remember a nightmare where this thing was falling, flaming out of the sky onto the city of Las Cruces down below. Well, anyway when it came time to fly it, why it went beautifully. It took off. It went through the speed of sound so smoothly there wasn't even a wiggle in the telemetry trace. And it went up. Its maximum altitude was 110,000 or 120,000 feet. As it was coming in at about Mach 3, at about 100,000 feet, I maneuvered it a little to the right, a little to the left, so that we could check out our maneuver calculations. It came on in and hit the ground and made a big hole in the ground, even with no warhead. Just the residual fuel in the tanks made quite an explosion. Well, when we got the ground crew out there to check the coordinates, it was quite a bit further from the desired impact point than what we thought it was, based on the guidance information. It took us four years before we could really prove it. The problem was that our guidance system was ten times as accurate as the mapping system. And that had been only one problem in these long-range missiles all this time. The maps were not good enough to use the guidance system. It is interesting to note that the key element in the Corporal's accuracy was the use of the SCR-584. The fire control system was developed at MIT during the war by DuBridge's Rad Lab.

COHEN: So anyway, they cancelled your contract.

STEWART: Yes, but that was only part of the difficulty. At that time, the military intelligence rules said that they didn't bother the president about intelligence unless there was unanimity among the agencies responsible for that area. They didn't want to get involved in inter-agency squabbles and so on. Unfortunately, the State Department people simply refused to believe all of our military intelligence. So the president never heard of this, but he did hear Van Bush's stories about how these things were never going to happen. Now Stu [Stuart] Symington—who became a senator from Missouri in 1950, by which time our information was essentially quite complete on the Russians—was Secretary of the Air Force. He tried his best to get this information to the president. As a matter of fact, during that time I was a member of the Scientific Advisory Board, and under Symington's leadership I helped Rand to set up a little office in the Pentagon where they could start doing things to prepare for the fact that sooner or later, they were going to have to do something about this. So Symington had done everything he could. By the time this scandal broke out, after the first Russian satellite flew, Stu Symington was a senator. If you wanted a scapegoat, why Symington was the logical man, but it would have been very unjust [Laughter] because he'd done his best. It was those stupid rules.

COHEN: So is that when you went to the senate committees and the State Department?

STEWART: Yes. As a matter of fact, I got this message from Lyndon Johnson's office. They wanted me to sign on for part-time work with this special Senate Investigating Subcommittee. In the latter part of 1957 and in 1958 was when the hearings went on. In a later phase when I took two years off and went to NASA I was one of the people being interviewed. But I must say that it's much more comfortable sitting on the side with the senators than it is sitting on the other side.

COHEN: Okay, so that's a good example of your going and testifying to Congress. How much of your time did you spend doing that? Was that your one day a week?

STEWART: That was one day a week for a good part of that year.

COHEN: One doesn't get rich on that, though.

STEWART: Oh no, especially if it's the government.

COHEN: Right. But that's a very good example of the sort of thing that took up a lot of your time.

STEWART: Lyndon Johnson did that absolutely honorably. He invited me in. The first day I was there, I was invited to lunch. He and Stiles Bridges, who was the senior Republican member on the committee, had lunch with [William V.] Houston and me. Do you remember Professor Houston who later on was at Rice University? Well, Houston was a physics professor here who became president of Rice. Anyway, Houston was also going to do some consulting for them. So the two of us were there at lunch with the senators. And both of the senators swore up and down that they weren't looking for skeletons in the closet, they weren't looking for scapegoats. They wanted to do something for the republic, and they did. Frequently it got a bit nervous and you would sometimes see Stu Symington over on the side, pacing up and down. But everybody on the committee, Republican and Democrat alike, leaned over backwards to avoid doing anything that would louse us all up with this ancient history.

COHEN: They just let it go.

STEWART: Of course, yes. And it was sad. During the war we had an army and we had a navy, and we had research projects on both. One of the agencies set up during the war was the Joint Research and Development Board—JRDB—which helped to form the JPL and Bumblebee Projects. Clark Millikan had things doing with them all the time. As a matter of fact, I remember when we were setting up White Sands, I was in Washington on something else, and Clark asked me to join him. We flew west and stopped at Biggs Field in El Paso, got on a little airplane, flew up around and through the Tularosa Valley, and looked at these beautiful mountains there. They had decided quite properly that the tech area should be down at the bottom of the valley. We chose a site up at the foot of the Oregon Mountains to be the site for the camp and the living quarters. And we went back to El Paso. That noon, Clark went over to the army—the big military base there, where the Artillery Board 4 was meeting that day. Clark gave this report on the site at White Sands. So I always had a paternal feeling when I went out to White Sands. After the meeting, Clark and I went over to Juarez and had dinner and saw a bullfight—my first bullfight.

COHEN: So that's a good example of interaction.

STEWART: Yes. And that continued, you see.

COHEN: Up until when?

STEWART: Well, the JRDB continued after the war until the reorganization of the Defense Department, at which time it was changed to the RDB and was put under the Secretary of Defense. Clark had dealings with this group all through the years. As a matter of fact, for a time he was chairman of the Guided Missile Committee.

Now one of the things that the Guided Missile Committee obviously needed was intelligence. And they did one thing. Now I'm going to extrapolate. This I don't know to be the truth. This was my conclusion afterwards. But in 1946 or 1947, the air force, air corps at that time, gave JPL a small contract to go around on all of their guided missile contracts and do what you might call a technical audit, and make a report. This was in conjunction with their RDB/JRDB needs. Well, there were two outcomes, one, indirect to me, but one very direct. The indirect one was that the Guided Missile Committee where Clark Millikan and Pat Hyland—from Hughes, he was chairman for a time—well, they set up this group called the Technical Evaluation Group. And in, oh, 1948 or 1949, I was asked to be a member. Later on, I was the chairman for a year. So that was one way it happened.

There was another thing which I think was part of this same packet, though no one ever told me so. Just after we finished this technical audit for the air corps, the air corps asked me to participate in a special top secret intelligence experiment, which wouldn't require my travelling or anything, except back to Wright Field maybe. I did that a couple of times. Mostly the idea was to try to expose somebody who was at the forefront of the business to technical intelligence in its most crude form, without requiring them to go overseas and sneak around through back alleys.

COHEN: Now this was all the Russian stuff that you were analyzing?

STEWART: Yes. And of course I knew what the Russians had done with the Germans that they'd taken in there, how they'd insulated them from their real projects. So the Germans had no idea what was going on. But we also knew what was going on with the Russians. We knew about their big engine contract. We knew that they had a five engine configuration they were working with—and that's what they finally flew, you know. So by 1948, the basic information was all here. What wasn't all here was their progress towards achieving the result. That came along gradually later. But even by 1948, we knew that. And of course Van Bush had all that kind of information available to him, in his position, but still he just couldn't bring himself to believe it.

COHEN: Pity.

STEWART: Yes. A shame.

COHEN: So that's very interesting.

STEWART: That's the most dramatic story of this sort.

COHEN: Now your years of dealing with these members of industry, you haven't said anything about that. I was going to ask you about a few of these people. I have here [Howard] McCoy, [Maurice A.] Biot, [Fritz] Zwicky. Would you care to say something about some of these people?

STEWART: Sure.

COHEN: Okay. Why don't we start with Biot.

STEWART: Well, Maurice Biot was a student here at Caltech ahead of me.

COHEN: Was he a Frenchman or was he an American?

STEWART: He was Belgian-born, but he'd come to the United States and had become an American citizen.

COHEN: So he was an American.

STEWART: So he was an American. But part of his story which I can't leave out is that, while he was growing up in Belgium, he'd done his military duty and was put on a reserve listing. When the Germans attacked, they called him up for service, although he was an American citizen. When he didn't report, he was listed as a deserter. So he felt bad about that. Mostly, he hated the Germans for making it happen, but he also hated the Germans because of what they'd done to his family. Biot spent the war as a naval officer. After Caltech here, he went back to Columbia.

COHEN: So he got his Ph.D.

STEWART: He got his PhD here at Caltech. And he did a lot of work; he was Kármán's primary assistant on the production of the book, *Mathematical Methods in Engineering*. Then he went back to Columbia. I should say he spent the war years as a naval officer in technical areas. In that connection, he went to Paris towards the end of the war with the tech intelligence teams that they were organizing. On the particular team that he was responsible for was a non-military technical assistant who was Fritz Zwicky.

COHEN: Oh, that's this Zwicky.

STEWART: Fritz Zwicky of Caltech, professor of physics. Yes. He's the only physics professor whose course I never took. And the reason I didn't was that I had so many night hours on the wind tunnel. And Fritz Zwicky always gave his class at eight o'clock in the morning, and frequently I hadn't got to sleep before three o'clock. [Laughter] So I never signed up for Fritz's class, although I must say I've got Whittaker's *Dynamics* over there. I read the book anyway.

COHEN: Okay. Good enough. So let's come back to Biot. He was a naval officer after the war?

STEWART: No, during the war. He was in Paris as I say, operating like Clark Millikan as part of a naval intelligence team. We knew that the German V-2 people had moved their wind tunnels and a number of their operations a year earlier from the exposed area of Peenemünde, north of Berlin, down to the south of Germany to the little lake called the Kochelsee. We knew their wind tunnel activities for example had all been moved down there. So Biot's assignment at this time was to get down to Kochelsee before our GIs had destroyed everything of interest—which unfortunately happened at Aachen; there was a lot of interesting lab equipment that just broke up. Although the sign on what had been Kármán's office at Aachen still said "Th. von Kármán." [Laughter] That was still there. Anyway, Biot's orders were to get to Kochelsee in time to recover some things. The battle line was about, oh, fifty miles or so from Kochel. Biot went out in his nice navy uniform with his civilian assistant, Professor Zwicky, commandeered an army tank company, went roaring through the battle lines and captured Kochel. [Laughter] Zwicky was a pretty formidable fellow—probably at least as formidable as the SS troopers. They lined up all the German people at the lab, and Fritz walked down the line. In front of the fattest senior he pushed a gun in his middle and, in thirty seconds, he had them all broken down and they were hustling in all directions, recovering all the stuff they'd been hiding.

COHEN: Where did Biot go after the war?

STEWART: He was already at Columbia before the war.

COHEN: I see. He stayed there?

STEWART: Yes, he went back to Columbia.

COHEN: Did he have anything else to do with Caltech after that?

STEWART: Nothing directly, although we had personal communications occasionally—rarely, but occasionally. His main business was at Columbia after that.

COHEN: He was a professor of aeronautics there?

STEWART: Yes. He had a distinguished career. But I always thought his military career was the most interesting one.

COHEN: Now who was this McCoy you have here?

STEWART: Howard McCoy. Well, McCoy was one of those bright young officers that the air corps sent to us in the days before we were in World War II. Now McCoy was here in 1941 or 1940. I think he came here in 1938, and he got two years of graduate work. He had courses in aeronautics, and I think he was probably in the first special courses in jet propulsion. Afterwards he went back to Wright Field and had a distinguished career. In the last year of the war—this would be probably in the fall of 1944—the Russians had pushed in through a good part of

Poland. Now in the last development phase of the V-2, the Germans at Peenemünde had decided they really needed to have some tests on warhead effectiveness. In order to do this, they adopted a very simple test procedure. Instead of firing west against normal shorter range targets as they normally had, they fired them into Polish villages. Then they could go in and see what had happened. Well, now the Russian battle line had overridden the area in Poland where they had set up a little headquarters to handle this operation. Somebody made the decision that we should send a mission in there to see if they had abandoned any records, papers and so on, to see what we could find.

Now we were very leery of this because of course the Russians have a very primitive sense of lots of things, you know, the ancient aristocratic principle of winner take all. As far as they were concerned, prisoners are slaves and slaves are slaves. So when they overran a German prison camp with our people in it, why they just incorporated our people into the slave content. Some of them we recovered and some of them we didn't, you know. And here we were asking a crew of our people to go back into Russian-controlled territory.

Of course this whole business is part of the story of why this MIA [Missing in Action] thing has been such a problem ever since. As a matter of fact, the brother of one of my friends died in a Siberian gulag. He was one of our officers who wasn't returned at the end of the Korean War. The night the Korean War ended General Vandenburg said on the radio that of the officers that were known to have been alive and well in prison camps a few months earlier, precisely half of them had been returned. So the other half—well, as I say, I heard about one. In those days, the CIA [Central Intelligence Agency] could operate on the ground, so they did. They did get some information of course. We haven't had that capacity for twenty or thirty years now, since the senators started exposing all the names they could.

At any rate, everybody was kind of leery about sending anybody in there at all, but they finally decided to do it. McCoy was given command of the operation, and the Russians gave us permission to go in. We flew a crew into the area, let them off, and our planes came back out. Then the only question was, could we get back in to get them out? Well, they started their work. The first thing they found out was that there really weren't many records. Part of the basic problem was that the Germans were short of toilet paper, so that they used all their old records for toilet paper. All these old records that they were supposed to examine were at the bottom of the slit trenches, the latrines.

COHEN: Maybe that's all they were worth.

STEWART: It turned out that was all they were worth. There was no useful information there, which is too bad. But the interesting thing was that we did fly the whole crew out. Everybody got flown out through the southern route in degrees.

COHEN: Well, do you think there's anybody else?

STEWART: Well, there was one name that I mentioned today that you ought to think about, and that was Allen Puckett.

COHEN: Puckett. Have you mentioned him in your original—

STEWART: No, but I mentioned him today. Puckett was brought by Kármán as a student. Bill Bollay had set up the aeronautics group back at Harvard and Allen Puckett got a master's degree under Bollay there. Then Kármán got Allen to come out here to do graduate work because Kármán needed somebody to take over development of a supersonic wind tunnel. Bill Bollay's brother Gene also went through Caltech and was a meteorologist and was one of my students. So there were Bollay connections. Well, Allen Puckett carried out the design work for the Aberdeen wind tunnel which was the general project that Kármán was pushing. He did this by setting up a small supersonic wind tunnel about ten square inches across with adjustable shapes so that you could get the flow you wanted. You could make the measurements and decide how to design the big one. That was Allen's main job when he came here, and he carried that out. Coincidentally, at the same time, von Kármán had decided that [Hans] Liepmann should pick up the compressible fluid business. The two of them—Liepmann and Puckett—put together one of the first very good books on the subject which came out as a GALCIT series. I suppose you're familiar with that.

Incidentally, I had a great problem with that because when I learned compressible fluids, I learned it with the old English tradition where the Laplace transform operator is a P and not an S, which is the way all of communications people talk it. Also the method of characteristics, well, I had to learn, that was a newfangled word.

But the big problem about the guided missiles was that you're combining the electronics people and the aeronautical people. I talked about Prandtl and so on, but I forgot to mention that the other group was the electronics group. In my mind, it sort of started out with the information theory in communications, and in practice was best represented by the people at MIT. And of course, that's the sort of thing that DuBridge was much involved with. But the main center of this in my mind in those days was at the Bell Labs. And these two cultures had never had any important cross-communication. They'd grown up quite independently from the primitive handcrafted worker stage to suddenly working on a basis where scientific principles were not only followed but were even respected.

COHEN: So it's a matter of putting together these different people.

STEWART: Yes and that was the main problem. When we did put them together, things did work, but you see the Germans hadn't recognized it, so their things didn't work.

COHEN: Well, I mean that's supposed to be the strength of Caltech. It's a small place where people talk to each other.

STEWART: Yes and it's been a wonderful place to live.

APPENDIX

The following is Homer J. Stewart's list of professional affiliations in chronological order, from 1936 through 1979.

1936	Durand Committee, U.S. Congress, on Macon and Akron accidents.
1940	Work for S. Morgan Smith Company on Smith-Putnam wind turbine.
1942	National Research Council, work on torpedo problems.
1944-45	Technical intelligence work for Army Ordnance, including debriefing of Wernher von Braun and Ernst Stuhlinger (with Louis Dunn).
1947-49	Technical intelligence work for Air Force through the Jet Propulsion Laboratory [JPL].
1948-51	Research and Development Board (of the Armed Services), Guided Missile Committee; served as chairman of Technical Evaluation Group (1951).
1949-53	Member, U.S. Air Force Scientific Advisory Board; second term of service, 1961-65.
1955	Chairman, Office of the Secretary of Defense, Advisory Group on Special Capabilities.
1957	Consultant, Senate Preparedness Investigation Subcommittee.
1958	Consultant, Advanced Research Projects Agency [ARPA].
1958-60	Director, Office of Program Planning and Evaluation, National Aeronautics and Space Administration [NASA] (Two-year leave of absence from Caltech).
1965-77	Member, Army Scientific Advisory Committee, Ballistics Research Laboratory [BRL, Aberdeen, MD]
1965-79	Member, Board of Directors, Sargent Industries, Inc.