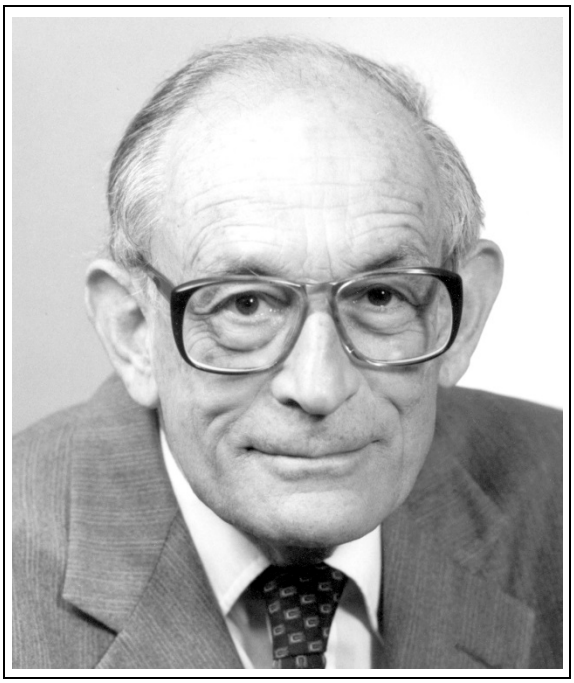

Hans Liepmann, 1986

HANS W. LIEPMANN
(1914 – 2009)

INTERVIEWED BY
JOHN L. GREENBERG

March 10, 12 & 30, 1983

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CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

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Abstract

An interview in three sessions, March 10 and 12, 1982, and March 30, 1983, with Hans W. Liepmann, director (1970-1985) of Caltech's Graduate Aeronautical Laboratories (GALCIT), in the Division of Engineering and Applied Science. Dr. Liepmann received his PhD from the University of Zürich in 1938 and came to Caltech the following year as a research fellow to work with Theodore von Kármán, director of the Guggenheim Aeronautical Laboratory, as GALCIT was then known.

He recalls his early education in Berlin during World War I, postwar inflation, and the rise of the Nazis; his family's move to Istanbul in 1933; his studies at the University of Istanbul with Richard von Mises and Harry Dember; Prague's German University; and Zürich with Edgar Meyer, Gregor Wentzel, and Richard Bär. Recalls his arrival at Caltech and his various GALCIT colleagues, particularly von Kármán and successor Clark Millikan. Comments on GALCIT's relationship with U.S. aircraft industry during World War II; on Robert Millikan;

on his work on turbulence, transonic flow, shockwave boundary interaction; on changes in GALCIT over the years; on teaching at Caltech and the difference between science education in the U.S. and Western Europe. Recalls the controversial deportation of Hsue-shen Tsien. Comments on consulting with Douglas Aircraft Company, on founding of the applied mathematics department, and on his ongoing unhappiness with Caltech's direction, particularly its move toward the social sciences.

Administrative information

Access

The interview is unrestricted.

Preferred citation

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ORAL HISTORY PROJECT

INTERVIEW WITH HANS W. LIEPMANN

BY JOHN L. GREENBERG

PASADENA, CALIFORNIA

http://resolver.caltech.edu/CaltechOH:OH_Liepmann_H

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

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Pasadena, California

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Begin Tape 1, Side 1

GREENBERG: I know you were born and grew up in Berlin. Why don't we begin with your family. What were they like?

LIEPMANN: My father was professor of gynecology and obstetrics and the director of the Women's Hospital of the Social Services of Germany, in Berlin. He was a pious Protestant from a Jewish family.[1] My mother came from one of the oldest families in Germany. In fact, she's a direct descendant of Lucas Cranach, the painter, and I'm related, through him, to Goethe and Kleist. So it was a very interesting combination. My father was a particularly good surgeon, extremely excitable, and known in his hospital as Wilhelm III, which he was. So we had the corresponding fights at home, but nothing particularly serious. The only thing he did which caused me a certain amount of trouble is that due to family tradition I was put in a classical school. So I learned nine years of Latin, six years of old Greek, practically no mathematics, and practically no physics, in spite of the fact that I was decided about my career before I entered school. I never had any doubt about going into things related to natural science. When I was very small, because I didn't know about physics, I oscillated between engineering and astronomy

[1] This fact was not at all as unusual as it may sound in this country. The vast majority of German Jews and certainly the group from which my father's family originated considered themselves Germans of Jewish faith, not Jews with German citizenship. –H. L.

and, at times, mathematics, but there was never any doubt that I was going to stay in that group.

GREENBERG: You had this interest very early?

LIEPMANN: Oh, I was five or six. I was interested in spite of the fact that such a career was not in my family. I mean, my father was a physician, and one of my grandfathers was a professor of surgery. My mother was good at mathematics, but of course at that time she didn't study at all. I just was quite determined—never had any doubts about it.

GREENBERG: It sounds as if you came from a home that was stimulating.

LIEPMANN: Oh, yes. In fact, my father was completely unrestrained. Our conversation went over all phases of surgery; some people fainted at our dinner table. In this respect, there was a stimulus, and the idea that people do research was quite natural. But you see, that can go in both directions. My younger brother went in the opposite direction, really. He became a professional in the U.S. Army and just retired after thirty-one years. So it didn't work the same on all three of us children.

GREENBERG: Was your family well off?

LIEPMANN: Yes. I mean not really rich. My father was completely incompetent with money anyway. We never owned our own house, for example. But we never had any real financial worries.

GREENBERG: How did World War I affect your family?

LIEPMANN: That is very hard for me to tell you, because I was born in 1914—July 3rd, just before the war began. My father was in the German Army, of course, and was for a while in Belgium. He ran a hospital close to the front, until he got a heavy infection from operating on somebody who had blood poisoning, and he nearly died. Then he was transferred to the rear, where he headed one of the army hospitals in Berlin, I think. My

mother took care of us, but when he was ill, she went to Belgium and left us with an aunt, I think. My parents were very strange. They were very close. As I recollect, my father traveled in his life only three times by himself, the rest always with my mother.

The war affected us like everybody else. There was little food, and I remember people giving me glowing accounts of whipped cream, which I didn't taste until I was, I think, four or five. The first whipped cream was made with some stuff which was really condensed cream from Denmark. When I finally got some, it shook me up. I couldn't understand why people made so much fuss about whipped cream—it tasted awful. [Laughter] I also remember people coming home from the war. We lived in an apartment house; you heard about people coming back and about those who didn't come back. So, that's what I remember, but not much more.

GREENBERG: Did the war affect your family financially?

LIEPMANN: Yes, my family had some problems. The main financial difficulty came during the inflation after the war—that I remember. They lost all their holdings, because they had invested everything in German war bonds. So what they had in capital, they lost. The main difficulty was the inflation later on, which I don't think many people in this country realize. Eventually the value of the German mark was stabilized at one new mark for a billion old ones. And the change was such that I still remember seeing German money printed on one side only, because it didn't last long enough. I know from my father that at that time doctors still had their offices in their homes. Payment was immediate. And my mother was waiting to buy groceries immediately. I mean, the inflation was from day to day; you couldn't delay buying for days. That I remember, and that must have been some strain on my family, in spite of the fact that money was strictly not discussed with the children.

GREENBERG: You had no formal exposure to science up through the *Gymnasium*?

LIEPMANN: No, practically none. Even in the last class, the class before you do your *Abitur,* the final examination, I had one hour of mathematics. I think [I had] eight hours of Greek a week, plus six hours of Latin. And in addition, in the German tradition, we

had religion. I was Protestant, so we read the Gospel according to John in the original Greek. There was so much Greek, it came out of my ears. And for that reason I flunked out twice. I flunked out when I was eleven or twelve, and I would have probably flunked a second time if I hadn't changed schools. I had a terribly tough time in school. I didn't like it. I only kept going because I always thought, "Boy, when I get out of here—I have to get out—I have to go to the university, and then I will do physics."

GREENBERG: You say your mother did have a mathematics aptitude. Did she work with you at all at home?

LIEPMANN: She helped me with my schoolwork, yes. In respect to school, my mother did everything. She even learned Greek. We were very open at home. I would tell my parents all my troubles in school, to the amazement of my teachers. But it was all handled by my mother. My father didn't even know which school I went to—really, he didn't know where it was. I know that for sure, because once one of the teachers said to me he felt sorry for my poor parents. I promptly told this at home, and my father said he was not going to accept such an idiotic statement from any teacher and would make him apologize. But in order to go to there, he had to find out where the school was! [Laughter]

GREENBERG: Why did your family leave Germany in 1933?

LIEPMANN: It was not so much because my father came from a Jewish family. He was, after all, Protestant—and, being married to a more-than-utterly Aryan wife, there would have been no reason to leave at the time he left. But he had a big mouth. So when he came into his hospital the day after the fire in the Reichstag, one of the assistants asked him what he thought about it, and he said, "Well, that's obvious. The Nazis did it themselves." In the afternoon, our apartment was searched and my father was jailed. We got him out because we had some pull; he had treated some of the wives of the men in the Cabinet. Then he said, "In a country which has gotten to the point where you can search a house and put people in jail without court order, I will not remain." They left the same evening, against all advice. The whole family was standing around, all his brothers and

so on, claiming he was crazy as usual, which he was. And he left immediately, without money and without the offer of a job.

We had a very tough time then. The Reichstag fire occurred, I think, in February [February 27, 1933]. About four or five months later, he got an offer of a professorship in Istanbul, and that's why we went to Istanbul. You see, Istanbul was at the time under Kemal Pasha, or Atatürk—at that time we all called him Kemal Pasha. He decided to reform the university. In fact, he decided deliberately to take the opportunity to attract people out of Germany, and probably also out of Western European countries, to rebuild the university. And my father was one of the professors who went to teach and do research. And he became the director of the only public women's hospital in Istanbul. He held the position until he died. I remained behind in Germany in rather unpleasant conditions, because we had no money at all then.

Hitler came to power in January '33, and my parents left. I stayed behind. I had just finished school, and I had to stay with my sister and her husband, who happened to be Nazis, with a big swastika on their balcony. I worked at Siemens & Halske, which is the German equivalent of GE. Originally, I thought I'd do this in order to be able to become an engineer. In the German engineering tradition, you have to go first into a company and learn the techniques—shop techniques and so on. But at the time I went there, I had really no hope of being able to study, because my parents were out of the country and there was no money. So for the time being, I didn't know whether I could study or not. I talked to the people at Siemens, and they changed my job, so for about nine months I worked in the shop. I learned to use all sorts of machine tools and, essentially, toolmaking.

GREENBERG: While you were still in Germany, did you know about the Göttingen tradition—Aachen, Berlin, places like that?

LIEPMANN: No, nothing! Not about the mathematics and physics at all. Also, at the time, the primary concern was really the political situation. And that was brought home to me very clearly. As I was then one of a very large number of employees at Siemens, I belonged to what was known by the Nazis as "the working population of Berlin." So I,

with the others, was marched to the Tempelhofer Feld, which later on became the airport; at that time, it was still the parade grounds of the old Empire. There we had the First of May, which they changed to Day of the German Worker. I remember that distinctly, because unfortunately I happened to be in the first row, and I didn't give the "Heil, Hitler" salute, and I saw about ten other guys who didn't either. I remember standing there expecting that somebody would hit me over the head from behind, but it didn't happen.

Another time, we were all marched over to one of the big halls where they assemble the big generators, to listen to Hitler's address to the working population of Berlin. This time I was a little more clever; I climbed up one of these steel towers, so that I couldn't possibly give the Nazi salute without falling down. I had to listen to that guy. Of course, there was a lot of talk among the workers. It was immediately evident that there were quite a few people who felt like I did—some of the old guys, in particular—and so there was a continuously tense atmosphere. We knew who was anti, which was the larger part of the group. But we had weekly or biweekly raids by the SS on the toilets, because that's where people usually discussed, smoked, and wrote slogans on the walls.

So at the time, my main driving force was an unbelievable hate of the Nazis, trying to get through the time and eventually get even with them. But, you know, it didn't depress me at all; I was just mad. And I found quite a few people in the company—mostly the toolmakers and old unionists among the workers—who were of my opinion. We used to talk relatively freely. In the student group that worked there, there were more Nazis, so you had to be careful. You got used to always talking in riddles. In many cases, you could distinguish Nazis by their vocabulary; they used certain words that a normal German wouldn't use—and avoided words that came from foreign languages and so on. You could get by—it was not that serious—but you had to be damn careful. I had an intense dislike of the people I lived with—my brother-in-law and my sister—because of their Nazi leanings.

GREENBERG: You left Germany in 1934 and joined your parents in Istanbul?

LIEPMANN: Yes.

GREENBERG: Your sister stayed behind?

LIEPMANN: Yes, my sister stayed behind all through the war.

GREENBERG: And in Istanbul, you went to the university?

LIEPMANN: Yes.

GREENBERG: Did you know Richard von Mises and Willy Prager?

LIEPMANN: Yes. Very much so. I took my first mathematics from von Mises. Prager I had very little to do with, but I knew him. He was, at the time, a very young guy. We met him socially. In fact, my parents' house became one of the centers where everybody would come and express their woes. So I knew them all—all fifty Germans, or however many there were.

GREENBERG: Were von Mises and Prager forces in your getting into physics?

LIEPMANN: No, not really. I was extremely interested in mathematics, and I enjoyed von Mises very much, except I had a terrible time following him, because my background was nil. Von Mises, in the German tradition, did not pay any attention to the students. He gave an excellent lecture, but whether you got any of it or not was up to you. There were no problem sets. All he was going to do was suggest a book, which he did. But he didn't tell me in what way to read the book, so I got promptly stuck on page five—I still remember that. But I learned a hell of a lot. And I learned even more from the physicist Harry Dember, also a German. I became a helper in the experimental physics course, because I was very good with my hands and experimentation was always very easy for me. So there I learned a lot, in spite of the fact that the equipment and facilities were unbelievable when it first started.

The lectures were held in a huge hall, which originated when the Ottoman princesses rode there because they were not permitted out in the open. There were 800 students in a class crowding this hall, so you sat practically at the feet of the professor. But I found that very stimulating, and I enjoyed, in particular, the experimental physics courses. So I learned a lot. And of course, I felt I had now finally gotten where I wanted to be. My only trouble was that my background was so bad that for a while I thought I couldn't make it; I thought maybe I was too stupid for physics and mathematics. In fact, I was at one time ready to quit, and I went to my father and told him I was too stupid for physics and I probably had to take up medicine like him. And he looked at me and he said, "Well, that is no problem. Any idiot can become a physician, even late in life. So why don't you try another year?" That was typical for my father and the only good advice I could have gotten. After a year, I didn't have any problems anymore.

GREENBERG: What kept you going?

LIEPMANN: This really sounds perfectly silly, but many nights I would go out and sit on top of an old tower, in Rumelihisari—the old castle, which Mohammed the Conqueror had built on the shore of the Bosphorus before he took Constantinople—and meditate, and worry, try to get myself together to go on. But there were no external stimuli; I was internally motivated completely. In fact, at that time, I didn't know that von Mises thought I was OK. He never said that to me; he was very, very stiff.

GREENBERG: I've heard he was rather authoritarian.

LIEPMANN: Oh, very. Also, very remote.

GREENBERG: And you did your first degree at Istanbul.

LIEPMANN: Well, there is no first degree. In the way in which I was raised, still in the physics curriculum in the German tradition—which is, of course, central Europe—the only exam I ever had was the PhD exam.

GREENBERG: From Istanbul you went to Prague. Why?

LIEPMANN: Because Prague was the cheapest, best European university. It's very simple. My father made a pretty good salary in Istanbul, but to have a son study outside was really very difficult for them. Since the son of my physics professor [Dember] was also in Prague, they decided I could go there. That was a mistake. You see, in Prague they used to have a German university and a Czech university, because the Germans were treated as minorities. I went to the German university. The professors were OK, but the whole setup was very poor. I must say, I am still fascinated with the authoritarian behavior. Philipp Frank, who was a theoretical physicist later on at Harvard, would sometimes not come to the lectures or would come a half an hour late or so. This sort of thing annoyed me—at the time not as much as later on, because at the time I took this more or less for granted. I learned a lot, again. I did a lot in the experimental labs.

I was very sensitive to noise then, and strict. I had had a very, very rigid Prussian education. So I made myself sit in a café with music and learn mathematics. You see, in Prague the living conditions were such that most students sat in the cafés anyway, in the old Austrian tradition, and worked there. I even picked a café with music in order to get over being so sensitive, which worked for a while, but today I still am sensitive to noise. [Laughter]

GREENBERG: Was there any particular individual in physics at Prague who took you under his wing?

LIEPMANN: No. I was very much on my own, as a matter of fact. I met Peter Gabriel Bergmann, now professor at Syracuse, who went as assistant to Einstein at the Institute for Advanced Study. He mainly contributed to a large set of inferiority complexes with me, because he was [a year or so] younger and much farther along than I was—a very bright theoretical guy. Other than that, there was no particularly strong influence.

GREENBERG: At that point, were you sorry you had left Istanbul?

LIEPMANN: No. I never thought that way, I must say. I really enjoyed it, in spite of the fact that I saw the limitations. No, I must say it didn't bother me at all.

GREENBERG: From Prague you went to Zürich.

LIEPMANN: Yes. I went home for the vacation, and then we decided I could go to Zürich, which was plainly more expensive, but we thought would be better. The way I went to Zürich was a mistake. I went because my professor, an experimental physicist—Dember in Istanbul—had recommended that I go to the University of Zürich, to the professor there, a man named Edgar Meyer. But that place was run down. It had had its great times in experimental physics—you see, I went into experimental physics anyway—but it wasn't particularly hot. I noticed this almost immediately. But I had such a loyalty that it would never have occurred to me to move to the ETH [Eidgenössische Technische Hochschule], which by far had a more lively physics department. And I can't complain about the university, because it helped me also to finish fast and to become independent.

GREENBERG: It never would have occurred to you to study at the ETH?

LIEPMANN: No, because when I was accepted at the university, I found I had a loyalty to the place. I took classes at the ETH, and I had a particularly good theoretical physicist. There were two theoretical physicists in Zürich at that time, [Wolfgang] Pauli at the ETH, who was a super great man, and [Gregor] Wentzel of WKB [Wentzel-Kramers-Brillouin] fame—at the university. And Wentzel was an unbelievably good lecturer. In fact, I once compared notes with Murph [Marvin L. Goldberger, Caltech president 1978-87], because Murph had had Wentzel in Chicago, and he had the same opinion. We went to hear Pauli, but he was an unbelievably bad lecturer. I once went back ten years after I had left, thinking nobody could be that bad, but by then it was worse. So he was really very bad. I did a thesis at the university with a Professor [Richard] Bär, on ultrasonics. By that time [1938], I didn't have any technical difficulties anymore, so I felt really quite good. My main preoccupations at that time were, again, political. I was absolutely certain that war was coming. I didn't want to get out of Europe; I was scared stiff of America. My parents told me to go to America immediately, but the U.S. had a very bad

image in Europe. My own opinion, gained from some Americans I met in Zürich, plus the movies, was that this country consisted of very rich people, very poor people, and gangsters. It was, of course, pure nonsense. So I tried to avoid coming here.

GREENBERG: Why did your parents want you to come to America?

LIEPMANN: My parents wanted me out of Europe. They thought that was the only reasonable thing to do. But I first made some attempts to go to England and then to France. But after Munich, I saw that the whole thing in Europe was cracking—mainly in France. So I decided to come to this country. I knew my parents wouldn't be able to support me—for any length of time, anyway—so I had to finish my PhD. So I tried very hard to finish fast. Actually, I did a PhD in four years of total study from beginning to end.

GREENBERG: Hence, the university rather than the ETH.

LIEPMANN: It may have been harder at the ETH. The thing is, I finished in 1938 so that I could at least look for a job rather than study. Then I worked on a prize competition in Zürich. In the meantime, I got in contact with Caltech. That you won't believe!

GREENBERG: Your PhD work was in ultrasonics. Is that the origin of some of the work you've done here?

LIEPMANN: I don't think so. I got into aeronautics literally by being drunk at my PhD party. That's why I say, "You won't believe it." At the time, I was fascinated with what I read in theoretical physics about continuum mechanics. I actually was very worried about having to go into nuclear physics and getting a small part of a large accelerator for a small time. I like to do my experiments myself and to be in charge of them. But at my PhD festivities, which in Zürich they do in a restaurant, they gave me too much beer—I was strictly a nondrinker at the time. At the height of the festivities, Edgar Meyer, the head of the department, asked me across the table, "What do you want to do now?" And I said, "Hydrodynamics." So far as I remember; I don't remember why I said it. And he

wrote to [Theodore] von Kármán [director of GALCIT, then known as the Guggenheim Aeronautical Laboratory at the California Institute of Technology]. Von Kármán asked me whether I wanted to come. I wrote him and said I'd be happy to come. And I told him exactly—which I don't think he believed—that I didn't have any money. I would be supported by my father, who got sick at this time. I thought I could probably come in the summer of 1939 and that I would be supported until the spring of 1940, but that was it. I didn't have any background money or anything. I did not know, but found out later, that von Mises had written von Kármán that he should take me. That I found out through Istanbul gossip from one of the professors' wives, many, many years later.

GREENBERG: This is something von Mises did on his own initiative?

LIEPMANN: Yes. My parents must have told him that I wanted to come here. My parents didn't ask him to write, I know that. [Laughter] I had already had a big fight with my father and told him he should keep his fingers out of my profession.

GREENBERG: Did you have any background at all in the kind of work that von Kármán was doing?

LIEPMANN: No. I had once seen von Kármán in Zürich, giving a lecture. I had also not known that Edgar Meyer wrote to von Kármán. They had been together in Aachen—that I didn't know either. So when von Kármán asked me, it came pretty much out of the blue. I wrote him, and then I didn't hear anything. Then I went back to Istanbul because my father died, and I asked von Mises, "Should I write again? I feel impolite." "Oh," he said, "he probably lost it." So I wrote and I promptly got an answer, and so von Mises had been right. Whether that is what induced von Mises to write or not, I don't know.

GREENBERG: Do you regard this move from physics into aeronautics as a big change?

LIEPMANN: That is very difficult to answer. When I first came here [as a research fellow, in 1939—ed.], I thought I had made a big mistake. Not serious; it never bothered me—I can get interested in practically anything as long as it's related to physics. But the things

I thought would be interesting were exceedingly boring and the things I didn't know became interesting. If I would consider it again, I probably would have gone into cryogenics; I had worked a little with cryogenics. That would have been a better field, I think, for me. But this way has worked out very well. I can't complain at all.

HANS W. LIEPMANN

SESSION 2

March 12, 1982

Begin Tape 2, Side 1

GREENBERG: Though you had no formal exposure to mathematics, science, engineering, or astronomy, you had heard enough to know that you could go to the university to study these disciplines.

LIEPMANN: Yes, they were discussed at home. My father at one time had a patient who was a female astronomer, and I'd talk to her.

GREENBERG: You mentioned that you didn't know about mathematics at Göttingen, Aachen, and so forth, as a youngster. But I gather that you knew that such institutions existed.

LIEPMANN: Oh, yes, certainly. I should add one thing, which is interesting. I grew up at a time when there were public arguments about relativity. And believe it or not, there were fistfights about relativity between people who didn't understand it, from either side. So in this respect, I was aware of the Einstein business. It was brought, together with politics, into the school classes.

GREENBERG: What was the subject of your prize-winning work in Zürich? Was it connected with your PhD thesis?

LIEPMANN: Yes, it was essentially a takeoff from my PhD thesis.

GREENBERG: While you were in Zürich, did you know people like Ernst Meissner or Heinz Hopf or J. J. Stoker?

LIEPMANN: No, not really. Meissner I didn't know. You mean the mechanics fellow?

GREENBERG: Elasticity.

LIEPMANN: Yes. He was only known for being as unreasonable in exams as possible. Heinz Hopf—I knew the name. But I was at the university; they were all at the ETH. Of course, my main contact was with the physicists and mathematicians I took courses from, who were [Rudolf] Fueter; [Andreas] Speiser, who was a very well known group theorist; and [Paul] Finsler, of Finsler Spaces. So actually, the level of people was very good. I can't complain about that. And experimental physics was superb at the ETH.

GREENBERG: How were you able to obtain a passport to come to the United States?

LIEPMANN: Oh, I had a passport, a German passport, which was valid. I could postpone my military service for two years, and I think I postponed it until the fall of '39. In the meantime, I had made it to this country. To keep my passport I had to appear at the German Consulate periodically and report.

GREENBERG: But I gather you came close to being conscripted into the German Army.

LIEPMANN: I was. I was called but put back for two years. I was in the first year—1914 was the first year they called up.

GREENBERG: Is there anything else to be said about life and education in Europe, or does that pretty much cover it?

LIEPMANN: Well, the only thing I would say is that I can compare it now with education in this country. We can talk about that later.

GREENBERG: Then let's return to your immigration to the United States. At that time, did you know anything of von Kármán's reputation?

LIEPMANN: Yes. I knew about von Kármán's reputation, but not in aeronautics. I knew von Kármán from the Born-Von Kármán work on specific heats. I had heard von

Kármán lecture on turbulence, and you know he *was* one of the great fluid mechanicists of his time. But I had no idea of his input in actual aviation and aeronautics and military affairs.

GREENBERG: Was Caltech known in Europe?

LIEPMANN: Yes. After I heard about Caltech, I went immediately and got myself a catalog, and I looked at people who were here. Of course, there were people here who were very well known. But I had no idea about the size. It never would have occurred to me that it was not coeducational. And I was mainly concerned about its being private, because I had the standard European prejudice that a private school was one in which the people who gave the money would have a big influence, et cetera. I changed my mind once I was here. But yes, it was known scientifically. For me, it was mainly that I found names like Millikan, Tolman, Epstein, Morgan, and so on, and von Kármán, of course. The names made the difference.

GREENBERG: You said that you thought you might have made a mistake in coming. Why?

LIEPMANN: Occasionally when I found that I was supposed to know about how to design a propeller—that came up during the building of one of the wind tunnels—I thought, "What the heck?" But it wasn't a strong feeling. I was happy that I could work on something I found interesting.

GREENBERG: What was your reaction to the American style of doing science and to science education?

LIEPMANN: In the beginning, I really had very little feel for it. You see, I didn't teach, so I had very little contact with undergraduate students. And as far as research goes, I was simply put to work on a particular problem, which I did very cheerfully. I thought it was interesting, so I worked on it. So, really, my ideas about education and the way research

was done, comparing Europe and America, were formed much later, when I was more or less in charge of my own research programs in around 1945.

GREENBERG: How did people react to you when you arrived? Did you get the enemy-alien treatment?

LIEPMANN: No! In fact, whenever some people complained about that happening, I always thought they were crazy. No, I had absolutely no problem in this respect at all. In fact, I think everybody was most generous. And considering that I could hardly speak English, I found the treatment was perfect.

GREENBERG: You came in 1939. You were here for the last six years of the [Robert A.] Millikan [Caltech's head, 1921-45] era. Did the people already in aeronautics when you arrived—Clark Millikan, von Kármán, "Maj" [Arthur L.] Klein, Ernie [Ernest Edwin] Sechler, people like that—talk much about aeronautics at Caltech before GALCIT and during the very early GALCIT years? Did they talk about Harry Bateman [professor of mathematics, physics, & aeronautics, 1917-46] and Albert A. Merrill [instructor in aeronautics, 1918-30] at all?

LIEPMANN: Bateman and Merrill I met. Bateman I didn't get to know that well, but Merrill I knew fairly well. I didn't have much of an historical sense at the time. You see, right after I came, the war broke out, and things became rather frantic in the research business. So I don't think I ever questioned much of the history. I would look back later, but not at the time.

GREENBERG: Do you know if Clark Millikan played any special role in convincing his father to go into aeronautics in a bigger way?

LIEPMANN: I really don't know. I mean, the way R. A. Millikan got von Kármán is completely astonishing to me. He picked three guys—[Ludwig] Prandtl, [G. I.] Taylor, and von Kármán as possible directors, and he couldn't have done any better. I am told that one influence was [Paul Sophus] Epstein [professor of theoretical physics 1921-

1953], who knew von Kármán well, which is probably correct. But how he came to Taylor, I don't know. That may have been Bateman or Clark Millikan. Clark Millikan was a Bateman pupil, essentially; he did his PhD [1928] with Bateman.[2] Also, the choice was right; of the three, von Kármán was certainly the appropriate man to bring here.

GREENBERG: You've said elsewhere that in one of his early letters to [Daniel] Guggenheim appealing for money, Robert Millikan once stated that the time of the inventor had passed and the future belonged to scientific engineers. Do you think Millikan regarded Merrill at that point, as passé?

LIEPMANN: I would think so. I think he probably thought that Merrill would stay here, or stay around; later on, he went to Pasadena Junior College, if I remember correctly. But Millikan wanted to go into aeronautics with a far more scientific foundation.

I think Merrill had an influence on education at Caltech, and he'd run the early experiments. But he was already getting old at the time. I don't think he would have had much more of an impact.

GREENBERG: How would you describe your relationship with von Kármán?

LIEPMANN: That is a difficult and a rather tricky problem. I came here overwhelmed with the idea of working with von Kármán on experiments. I had an extremely high attachment and love for him, and appreciation, of course. But we had difficulties. Eventually we didn't get along as well as we should have. To a certain extent there was something which I didn't realize at the time. You see, the people who were very close to von Kármán at the time were Bill [William Rees] Sears [instructor, assistant professor, aeronautics, 1937-1941] and H. S. Tsien [Tsien Hsue-shen (Qian Xuesen[3]), then a research fellow at GALCIT], and, to a certain extent, Frank Malina [research fellow,

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[2] Millikan, Clark Blanchard, "Some Problems in the Steady Motion of Viscous Incompressible Fluids," PhD dissertation, Caltech.

[3] The Wade-Giles spelling of Chinese names, the standard before 1998, will be used throughout the interview, and followed only in the first instance with the pinyin conversion.

assistant professor, aeronautics, 1940-1946]. Now, Sears told me many years later that he always felt that von Kármán made a strict distinction between Europeans and Americans. With the Americans he was chummy, and with the Europeans he was the "Herr Professor." Whether that is true, I don't know. But I do not think we hit it off as well as we could have. It was certainly not only his fault but also partly mine. Looking back now, I realize that von Kármán's attitude toward experiment was quite different from mine. For von Kármán, theory came first, and an experimentalist was expected to check theory. For me, experiments are done for discovery first and for checking theory second.

Then there was an additional difficulty, which I, being naïve, didn't realize. That was von Kármán's sister [Josephine (Pipö) de Karman]. She was a very peculiar woman, as you may have gathered—highly neurotic, in my opinion. She had an overwhelming antipathy for Germans on the one hand, but on the other hand she had in her house a German nurse, who, to my mind, was Nazi. So it was a very peculiar combination. The first thing that happened in von Kármán's house was very bad. My wife [Dietlind] and I went to see him and we were waiting for him together with [associate professor of physics Jesse W. M.] DuMond. And Pipö, von Kármán's sister, called DuMond out and asked him, in a voice which she must have known we would overhear, whether it was all right for him to sit in the same room with "these Boches." My wife got up and said she was not going to stay, and I said, "Look, this is plain silly," so we stayed. So there were several incidents like this that didn't help the situation. For a while, we had a seminar in von Kármán's garage apartment, and after the seminar Pipö would invite everybody in but us. That was so bad that at one time Sears refused to go. But we all knew that Pipö was peculiar. I never held this against von Kármán, but it certainly caused some difficulty.

GREENBERG: What about the work relationship?

LIEPMANN: We never had a very close working relationship, for the simple reason that right after I came his interests were already very much centered on Washington, so he wasn't here very much. We discussed things, but it was never really intimate. To a

certain extent, I feel now that von Kármán was really a man who wanted more hands; he wanted people to work out his ideas. And I wasn't very comfortable with that. So actually, I was much closer to Clark Millikan than to von Kármán. But that had nothing to do with my admiration for von Kármán and what I learned from him—from his attitude, from the way he looked at problems. In this respect, he had a very large influence on me.

GREENBERG: After von Kármán's arrival [1926, as a visitor, to advise on wind tunnel design—ed.], how rapidly was aeronautics research at Caltech actually applied to the design and construction of airplanes?

LIEPMANN: That was only indirectly through von Kármán. That was mainly due to the wind tunnel. You see, we had the wind tunnel here and the wind tunnel operated very closely with Douglas [Aircraft Company]. And then, of course, von Kármán always had connections in the aircraft industry. He contributed a lot of the basic scientific stuff in wing theory and so on. But the direct influence on airplane design I think was largely through the wind tunnel operation and the resulting problems, and, of course, was through the training of students. One of the first PhDs who worked with von Kármán, I think, and Clark Millikan was [W.] Bailey Oswald, who became the chief aerodynamicist for Douglas, lasting until the end of the DC era.

GREENBERG: This may help explain a statement that you made elsewhere that the original GALCIT 10-foot tunnel of the von Kármán days had a beneficial influence on research and instruction that is difficult to overestimate.

LIEPMANN: That was part of it. But also, one of the most important parts is that it was the only thing, practically, in the early days which offered technical employment to incoming students, so that they didn't have to support themselves. Because, you see, the way we operate now, with research contracts and fellowships and so on, did not exist. Most of the people, when they had to work, worked as gas station attendants. The wind tunnel could employ them. Through the wind tunnel work, close contact with day-to-day

problems in the industry was kept, and this benefited the research at GALCIT and vice versa. In this respect, it was, and is, unbelievably effective.

GREENBERG: How did the relationship between the GALCIT and the American aircraft industry differ from that between the aeronautics research at Aachen and Göttingen, and the German aircraft industry?

LIEPMANN: I can't tell. I didn't know the German industry at all. But I'm quite sure that von Kármán's personality played an enormous role. You see, von Kármán was quite different from Prandtl and even more different from Taylor. He had the technical ability equal to both. But he had a much greater interest in having his fingers in the pies, both politically and in the industry. He was much more interested in political decisions, industrial decisions, and so on, so he had a much more intimate connection, let's say, with General [Henry H. "Hap"] Arnold from the military, with Jack Northrop, and other leaders in industry. I don't believe that Prandtl ever had that. In Prandtl's case, this connection with German industry must have been a little more on the professorial and committee side, not on this personal, day-by-day basis that von Kármán had. Taylor, of course, is quite different. Taylor never had a large establishment or a large school. He was more a loner, in the sense of Lord Kelvin or Lord Rayleigh. His influence was, I think, far more indirect.

GREENBERG: What about the scientists at Caltech under von Kármán, or the industrial engineers with the aircraft companies. Did they communicate with each other?

LIEPMANN: We used to have a class taught by Douglas engineers for many years. In addition, they would come with problems—mostly to Clark Millikan, I think. Clark Millikan, Sechler, and Klein were really the key figures in this. And von Kármán got it on a higher level. They would discuss and deliberate on problems, and sometimes PhD theses would come out of these problems. Then, of course, when the war broke out, this place was practically the only one which could handle compressible fluid dynamics, gas dynamics. The flight Mach numbers increased, and we went along into transonics, and so on.

GREENBERG: Why was this the only place?

LIEPMANN: Nobody had taken that seriously, you see. Well, Göttingen had done it even earlier. But von Kármán was clear that this was the coming field. Also, the mathematics and physical basis was much better here than in most places. There is a famous reported exchange between von Kármán and [Jerome] Hunsaker, when von Kármán wanted to go into the stuff which later on became the Jet Propulsion Laboratory, which involved supersonic flow and jet propulsion. Hunsaker, who was heading the MIT aero department at the time, said, "Well, you can have the Buck Rogers stuff, and we'll stick to de-icing." There was a little of this attitude. You see, the danger in engineering schools and in technical applications is that very often you get overwhelmed by the day-by-day problems and do not prepare even for a few years ahead. In this respect, this place was unusual.

GREENBERG: Were you aware of the applied mathematics controversy—if you can call it that—which seemed to be brewing around 1940?

LIEPMANN: No. Not really aware. At the time, certainly not.

GREENBERG: Well, out of this came institutes like [Richard] Courant's and the Richardson/Prager Division of Applied Mathematics at Brown. I'm wondering if you were in on any of the background to all of that.

LIEPMANN: No. But I knew, of course, von Mises. In fact, when I came to this country, von Mises had just come over and we had lunch together. He invited me in New York. Basically, of course, I think von Mises was the first one who pushed applied mathematics in Germany. So, I was aware that this was there, but I had very little to do with it. I knew that von Kármán considered von Mises very highly.

GREENBERG: I believe they even thought about putting out a journal here.

LIEPMANN: Yes. They had done that, of course, in Germany. I think the *ZAMM [Zeitschrift für Angewandte Mathematik und Mechanik]*—that was von Mises' business, where von Kármán strongly cooperated.

GREENBERG: It didn't materialize, but they talked about it.

LIEPMANN: That I think is very likely.

GREENBERG: What about the University of Michigan's summer school in applied mechanics—[Stephen] Timoshenko's operation?

LIEPMANN: I had nothing to do with it, because I was strictly on the physics-of-fluids side at the time.

GREENBERG: Would aeronautics at Caltech, after 1939, have been a lot different without the Second World War?

LIEPMANN: Yes. I think everything was accelerated. I, certainly, was suddenly drafted out of working on transition and turbulence to do compressible fluids and high-speed flows. I was asked because, basically, that was very closely connected with the difficulty with the P-38 [aircraft]. So I started teaching compressible fluids and fluid flows when I knew very little about them. [Aeronautics lecturer Allen E.] Puckett and Clark Millikan and I taught war-training courses. So that was certainly the result of an accelerated development due to the war.

GREENBERG: So your early work, as a research fellow at Caltech, was in turbulence.

LIEPMANN: The very early work I just took over from the [postdocs Francis and Milton] Clausers and [Alex] Charters, on boundary-layer transition, not on turbulence.

GREENBERG: Can you talk more about how you got into this?

LIEPMANN: Into turbulence? They just asked me whether I wanted to, so I did.

GREENBERG: You followed the orders.

LIEPMANN: Well, that never bothered me, because, as a matter of fact, I would feel the same way today. If I get into a technical field, I find something interesting.

GREENBERG: You were still a research fellow when you switched into transonic flow. Can you summarize the work that you did during your years as a research fellow?

LIEPMANN: Yes. As a matter of fact, quite a number of foundations are still valid today. One was the laminar boundary oscillations and transition, which was the first field I worked on here. That never got enough publicity, because it was all confidential. Furthermore, I don't like to publish, so I didn't push publication after declassification. The other one was the early work on turbulent mixing, which also came in the war; and some of the papers, which were done by me or by my students, or both, are very much cited even now. Then the transonic flow. The first thing is, I built a transonic tunnel. Tsien had worked on the supersonic tunnel, and then I built a transonic tunnel. But that was getting toward the end of the war. I would have to check whether that was during the war or not, because the publications were obviously very much delayed anyway. But I certainly made the move into transonic flow and shockwave boundary interaction at the time.

GREENBERG: You were a research fellow at Caltech for six years. How did you finally succeed in getting a promotion?

LIEPMANN: By getting a full professor's offer! No, that's exaggerated. Part of it was the interregnum. You see, von Kármán was the director, and von Kármán wasn't here very much. Clark Millikan could not make any decisions. And I must say, I never pushed. What then happened is, first of all, I got an offer, I think from Purdue, to be associate professor with tenure and with almost immediate possibility of full professor. Right afterwards, Sears went to Cornell and made all efforts to get me there as full professor. I

decided to stay here, and I didn't play academic blackmail at all. But it then went very fast, with Clark Millikan becoming director [of GALCIT, 1949]. So, in 1949 I was made full professor.

GREENBERG: The book by von Kármán and Maurice Biot, *Mathematical Methods in Engineering*, appeared in 1940. Was it a milestone in engineering mechanics in this country?

LIEPMANN: It's very difficult for me to judge. It's a damn good book, but I didn't know at the time enough of the status of mechanics in this country to judge. Like anything von Kármán did, it was very well done. So I don't know whether it was a milestone, but it was certainly a contribution.

GREENBERG: At the time of the GALCIT fiftieth anniversary, you mentioned that GALCIT began with an unusual emphasis on the scientific foundation of fluid and solid mechanics. Why unusual?

LIEPMANN: Because in the ordinary aeronautical engineering school there is a much greater emphasis on, say, performance, control, engines, actual design—structures of airplanes and so on—and much less emphasis on basic fluid mechanics, boundary-layer theory, fatigue, crack propagation, et cetera. There was a much heavier emphasis here always on fundamentals.

GREENBERG: Caltech was unique in this regard.

LIEPMANN: In engineering, probably it was unique. I have always maintained that aeronautics and aeronautical engineering are different from any other field in engineering, for the simple reason that in most of the classical engineering fields you can make up for ignorance by safety factors. In aeronautics you can't. Because if you slap a factor of 10 on an airplane, it stays on the ground. So you were forced to know more, and this forced you into understanding the mechanics better. And then, of course, the tendency of von Kármán was very much in this line. When von Kármán wrote me first

that I should come, I remember he said that we are working on the borderline between aeronautics and physics.

GREENBERG: From the moment that von Kármán came, did GALCIT begin to train aeronautical engineers who went out into industry?

LIEPMANN: That's a complex problem. I looked over all the statistics once. On the average, between 50 and 55 percent of all graduates went into industry. However, we have never trained what are known as "line" engineers. We have never trained a majority of the people who go on the drafting boards in the companies. The people we trained usually ended up in either high managerial posts, research, or advanced design. So we did train them, but not in the standard sense. You see, that's an issue which comes up and up: Do we really train engineers? The answer is definitely yes. But it's usually those engineers who shape the future more than those who produce for today.

GREENBERG: Does this come up as an issue?

LIEPMANN: Oh yes, that comes up as an issue, in particular since I took over as GALCIT director [1972]. I get accused in both ways—that we are not doing enough practical things on one side, and that we do too many practical things on the other. I'm convinced that we have no business trying to teach ordinary aeronautical engineers for the drafting board. This is done by the state colleges and the large engineering schools perfectly adequately. What we have always done is train people who can move with the field, people who are not specialized but can specialize in anything they want. And that is, I think, what the whole aim in GALCIT has been, and I hope is going to be.

GREENBERG: Sears said that von Kármán brought two things to Caltech: the emphasis on original research and publication, which was a carryover from his Göttingen-Prandtl background, and informality. Is that entirely correct?

LIEPMANN: That is very largely correct. It was, however, a very funny informality. There is no doubt that the institute was informal, very nice. And, of course, I took to the

first-name basis as easily as anybody; I liked that really very much. But around von Kármán there was, at the time, something like a court atmosphere, which I didn't like very much, and which Sears didn't like either, very much. But this was more von Kármán's sister than anything else. You see, von Kármán's father had been minister of education, and she was still convinced that there were certain class distinctions. That's the way it looked to me. She was very peculiar, but Sears knew that. I don't want this to get too much publicized, but there was—and when you ask Sears privately, he'll admit it—a court atmosphere around von Kármán to a large extent.

GREENBERG: Can you imagine von Kármán mingling with his students in the bierstubes of Aachen?

LIEPMANN: Yes. In this respect, there was no doubt. But when it came right down to it, he wanted to make the decisions, and he also very much wanted acknowledgment. You see, von Kármán was quite vain. In the early years, this didn't matter at all, because it was overshadowed by an enormous amount of spirit and, well, genius. When he became reasonably old, the vanity stuck out so much that people who knew him in his later years got the wrong impression. The vanity was only a small part, but it was there; he wanted to be acknowledged. He could deal with students in the bierstube, but he still wanted to be the great "Herr Professor" when it came down to meetings and so on. But that's perfectly natural. After all, he was leading in this country.

GREENBERG: He saw himself, I think, as Prandtl's successor at Göttingen.

LIEPMANN: That is absolutely correct.

GREENBERG: And he didn't get there.

LIEPMANN: Prandtl's successorship to Göttingen wasn't handled very well. Actually, Prandtl's best pupil was definitely von Kármán, and the next best I think was [Adolf] Busemann. And then comes [Dietrich] Küchemann. And neither one of them became the successor. [Hermann] Schlichting is very solid but less inspired, relatively minor, and

[Walter] Tollmien is ingenious but also not on the level of the others, and certainly not that stable. So, they didn't handle the successorship very well.

GREENBERG: Clark Millikan took down von Kármán's courses by hand, even the most elementary ones. I'm wondering if that's because von Kármán was so charismatic, or because Millikan didn't know mechanics at that point.

LIEPMANN: I don't know. I think that Clark, first of all, had an enormous appreciation of von Kármán. And I think that von Kármán brought a fresh point of view for Clark, too. So I'm quite sure that's why Clark recorded the courses. Von Kármán's lectures were not particularly systematic, but when he really wanted to, boy, could he turn it on! I took one class from him, and it was excellent.

GREENBERG: During the so-called von Kármán years, up to 1941, how much of von Kármán's efforts were devoted to research and how much to getting others to do research?

LIEPMANN: That's very difficult to say. He certainly inspired a heck of a lot of research. But the main thing he did, which I have never been completely able to tie down, was that he changed the atmosphere in the whole country. Aeronautics before and after von Kármán is two different things. To a certain extent, people—first the people at GALCIT and then the people from the outside—took GALCIT as an example. And in this respect his influence was much larger than can be accounted for by the number of students he had—because he didn't have very many. Also, his influence through this changing of the atmosphere was much larger than people give him credit for. You see, when people want to give von Kármán credit, they always pick something like his papers. This is minor compared to his general influence on this country, as good as the papers are. There are very few papers in the world which you really find are absolutely essential after ten or twenty years. But his influence remains. And the interesting thing is that it is not due to his directing students as much as indirectly, and I find that unusual. It's very difficult to tell people, because they always want to know who was his student. The best student of

his recent years was Sears. He didn't have very many. But the indirect influence, that was really something.

GREENBERG: He had Sears, Tsien, [William] Bollay, [Frank] Wattendorf.

LIEPMANN: Yes, and [Norton] Moore. He had—I don't know how many. I would say the people who had impact were Sears, and in some sense Tsien—but in my opinion not that much scientifically—and Bollay to a certain extent, certainly. And then some people who went into industry. But his influence cannot be explained through the number of his students. It's much greater than that.

GREENBERG: You've said elsewhere that von Kármán regretted that it was impossible at the time to use the name "applied mechanics" in characterizing his operation at Caltech.

LIEPMANN: I hope that's correct. He told me that once—that he would have liked to have had applied mechanics denote GALCIT, because that was the applied mechanics in the sense of Felix Klein. And he felt, of course, that he was a prototype, and he was, of that spirit. Why that didn't go, I don't know, but I would today probably guess that since he came here with the Guggenheim Aeronautics Fund, it would have been technically very difficult to do it. So then he insisted on aeronautics. He did not want aeronautical engineering. Our administration sometimes doesn't know that. When I tell them there is no aeronautical engineering at Caltech, they look in the catalog. But it's true; he made a point out of this. He wanted the field, or his laboratory, broader than just the engineering application of aeronautics—a place where all problems arising or connected to aeronautics could be studied. The question, "Is it very useful or not?" should not be asked. The question was mostly, "Is this an interesting problem?" and then the use would come. In some cases, of course, you want the work to be applied. But he wanted the title as broad as possible.

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GREENBERG: There is a document in the Archives dated 1945 which says that GALCIT was already unique in the broad domain of modern applied mechanics.

LIEPMANN: Yes, that's exactly right.

GREENBERG: And von Kármán would have liked to use the term "applied mechanics" for that reason.

LIEPMANN: Yes. I feel very much the same way. I do not particularly think the name has much influence, but I am certainly happy that we never insisted on "aeronautical engineering." At one time, the administration made some overtures to us to put in the name "aerospace" when the space business broke out. At the time, we didn't want to do this; we wanted to keep the name GALCIT. We proposed that we would call ourselves, if necessary, the "Galactic Laboratories." Mainly Sechler, Clark, and I felt that we wanted to keep the spirit of applied mechanics alive.

GREENBERG: Is this notion of applied mechanics European?

LIEPMANN: It's a Felix Klein notion. It's not around anymore, because if you refer today to "applied mechanics", it's practically all solids—elasticity and structures, and so on. The concept which von Kármán meant, and which came straight out of the Felix Klein school, was broader than that. You must know the story of Klein's revelation at the Chicago World's Fair. I think what you would call it today is probably scientific engineering, and in particular, applied science in the form that you work on and do research and study on all the subjects which have an impact on modern technology.

GREENBERG: You really see this as Klein's legacy. This isn't just hearsay?

LIEPMANN: No. Klein apparently had a very great influence. Look at Prandtl and von Mises and [Arnold] Sommerfeld, who by the way, was a Klein pupil. And I know that

von Kármán felt very strongly that applied mechanics was what they did. Today, you would name it a little differently, because it's not only mechanics; everything from quantum mechanics to electrodynamics plays also a role in what we are doing in engineering.

GREENBERG: Why was JPL [the Jet Propulsion Laboratory] founded in 1944 as a separate entity rather than as a jet propulsion section within Caltech?

LIEPMANN: I don't exactly know. First of all, it was probably expected to grow so large that it was not feasible to establish it at Caltech. I can't judge that. Originally, they were kicked out from here because they had explosions, and it was safer, at the time, in a completely uninhabited area around the Arroyo. I also think they probably felt that it would become a government lab eventually, which it did. But I'm not sure. Because the commercial side, which von Kármán and the others pushed at the time, went into Aerojet.

GREENBERG: Do you know much about the founding of Aerojet?

LIEPMANN: No. I was never in it. That, by the way, is one of the things where differences between me and von Kármán always kept counting. Many of the things I was never a part of, for that reason.

GREENBERG: He kept you out of it deliberately?

LIEPMANN: No. But I was never close enough so that I would automatically be in. I don't think it was anything deliberate.

GREENBERG: Only von Kármán's closest associates were involved.

LIEPMANN: Yes.

GREENBERG: Is there anything else that might be said about your work during the [Robert A.] Millikan era?

LIEPMANN: I liked that era very much, because Caltech was like a private club. So you felt part of a fraternity of scientists. There was a complete informality, in the sense that we agreed that he decided everything. You could get mad at him; you could argue with him. But in many respects I felt just completely at home, in spite of the fact that I didn't agree with him in many things. For example, it took me six months to find out there were no co-eds, for the simple reason that there were enough secretaries that it never struck me, because I thought things like this didn't exist anymore. Also, Millikan had that religious belief that the atomic bomb would never be built, and so on. I didn't agree with that. But he made an atmosphere here which I found extremely appealing. I wish we could reconstruct it.

GREENBERG: That is to say, it no longer exists.

LIEPMANN: It doesn't exist in that sense, no.

GREENBERG: Did you meet a lot of Caltech people outside aeronautics?

LIEPMANN: Oh, yes. You met people outside, in spite of the fact that I'm not very good at it. But we met a lot of people. And the whole atmosphere was different.

GREENBERG: Let's turn to the DuBridge era. [Lee A.Dubridge succeeded Millikan in 1946 and remained Caltech's president until 1969—ed.]. Did GALCIT change in any significant way with DuBridge's arrival?

LIEPMANN: No, I don't think so—not really. DuBridge, of course, from what I heard through the grapevine, insisted that von Kármán make up his mind—and I think von Kármán resented that, but it was correct—either to be here or give up the title of director. He was never here, and it made a lot of difficulties. For example, at that time, [Chia-Chiao] Lin and [Lester] Lees left because there was no way to be able to decide which way to go. So there was a crisis. Tsien went at the time to MIT. He didn't like Clark. The last thing he told me was, "Why don't you leave, too, and then we'll blow up GALCIT." Clark never noticed this, but Tsien didn't like him.

GREENBERG: C. C. Lin also.

LIEPMANN: C. C. Lin was not at all outspoken. C. C. Lin and Tsien couldn't be more different. Tsien is very forceful, a very tough cookie, with almost a malicious streak. And C. C. is quite different. But Tsien was completely set that if von Kármán left, we should blow the whole thing up—at least, that is what he told me at the time.

GREENBERG: What was your relationship with Clark Millikan like?

LIEPMANN: We were very close; I was as close as one could get to Clark. A lot of people thought he was a stuffed shirt, which he wasn't, because he was always meticulously attired—not necessarily formal, but he had style. He had his cigarette holder and so on. But what I admired about him, he was an extremely able guy. He could do first-rate research, no doubt about it. But he was content in letting the people in the institute get the glory. He handled the administrative work; I really appreciated that. In this respect I felt I could rely on him for anything. He would tell me what was necessary, but he would take care of everything and I would rely on him. And I think all of us felt that way about him.

GREENBERG: When it came to research, was he an original thinker? Was he a man with ideas?

LIEPMANN: Yes. In fact, he wrote one basic paper on the turbulent boundary layer which was not appreciated at the time but which is a standard in the literature today. He was original. He suffered, I think, from being the son of his father, but he never pushed. He was almost timid in asking the institute for considerations. But there's no doubt he was original, in my opinion, and I think it's shared by a lot of people. People don't appreciate him as much as I think he should be.

GREENBERG: What was his relationship with von Kármán like?

LIEPMANN: He adored von Kármán. I think on the other side, however, Pipö didn't like Clark. I don't know why, but there was certainly that difference. Also, I think von Kármán was hampered in getting together with or being closer to Clark partly because of his sister.

GREENBERG: Were you satisfied with what you were paid here in those days?

LIEPMANN: I never had any trouble with that. My pay, anyway, is hilarious. My first pay was $90 a month, with a PhD and all. And my first raise was gotten for me through a man who at the time was a secretary here and did the payroll for Clark. He found out that my graduate student, who worked for me—Stanley Corrsin, my first graduate student—was getting paid more than me. So I got raised to, I think, $120. And what really amuses me is that this secretary is now on Caltech's Board of Trustees. He is Bill [William E.] Zisch, who went to Aerojet, became its president, and is now on the Board of Trustees. You know, in forty-two years I never asked for a raise. I always felt I was taken care of pretty well. I have made it a principle never to know what somebody else got paid. And I think I'm being raised fine. But I must say, it never really bothered me.

I had a tough time when I first came. I was married, and on $90 a month we could go once a month to the movies if we worked at it. So it wasn't easy. But I must admit, we felt that the possibility of my working in my chosen field was so astonishing, and that to be able to keep on doing interesting work during the war—of course, a lot of it was war work—was something fantastic. I also don't believe it right now when they say that you can't get engineers because university salaries are so low. I don't think that's true.

GREENBERG: Do you recall a visit to the campus by Courant, [Kurt] Friedrichs, and Mina Rees, during the war, concerning the building of rockets? Was there some problem?

LIEPMANN: That I don't know. But I know that Courant came down to see what I was doing on shockwave boundary-layer interaction. I know he turned around to somebody and said, "Why don't we do this at New York University?" That's the only thing I remember.

GREENBERG: So he was impressed with what he saw going on.

LIEPMANN: Well, sure, because they didn't do any experimentation, and at the time I did quite a bit.

GREENBERG: Do you know much, if anything at all, about the relationship between von Kármán and Courant?

LIEPMANN: No. I really don't know. I only know one remark von Kármán once made—that fundamentally, von Mises was much more gifted, or something like that. That stuck in my mind, because I knew von Mises quite well.

GREENBERG: I've heard other people say the same things about von Mises—that he was a giant.

LIEPMANN: In many respects, yes. I felt the same way, but I couldn't judge it. I think von Mises was somewhat neglected with his contribution to probability, because the Kolmogorov School took over with the set-theoretic approach.

HANS W. LIEPMANN

SESSION 3

March 30, 1982

Begin Tape 3, Side 1

GREENBERG: Let's tie up some loose ends. When did you begin to teach at Caltech?

LIEPMANN: I started during the war, when there was a sudden demand for a class in compressible fluids. Both von Kármán and Tsien were unavailable, so I was asked to teach—it must have been, I think, '43. And I started also teaching war-training courses and so on.

GREENBERG: What was your initial reaction to science education in America?

LIEPMANN: I never really thought about it, I just accepted it. The things that struck me first—and which were completely strange to me—were the continual examinations and problem sets.

But at the time, and still to a certain extent today, I feel that the transition from high school to college in the Western European system—which means Germany, Switzerland, the Scandinavian countries—is too rapid and cannot be absorbed by a lot of students. You see, when I went into the university, I was suddenly completely on my own. Nobody checked up on me; I lived where I wanted to. I had never an exam. I had no problem sets to speak of. So it was up to me to find what I wanted to learn, and learn it. If you are motivated in a certain direction, as I was, I found that wonderful. But in the European system there existed "eternal students," the people who floundered and never finally came up to their final exam, because they didn't really know what they wanted to learn. So I always felt that the combination of the European system for the upper level and graduate school, with the American system for undergraduates, was better—to relax slowly from the school atmosphere to what used to be a completely free scholarly exchange of information.

GREENBERG: So you weren't unhappy with what you found at Caltech?

LIEPMANN: No. At the time, I certainly was happy that I could be in the profession I wanted to be, so that overshadowed everything.

GREENBERG: No, what I mean is, you didn't have a negative reaction to these poorly prepared students by comparison with students of the same age in Europe.

LIEPMANN: No. I don't think they were poorly prepared. The students were eager. And the graduate students I had, starting with Stanley Corrsin, were quite good. In fact, of my first nine PhDs, eight made the Academy [National Academy of Engineering]. So they were good; I had no problem. No, I never thought they were ill prepared.

GREENBERG: But these were graduate students. What about the undergraduate students?

LIEPMANN: Undergraduate students I got to meet much later. I only got into undergraduate teaching in the fifties or sixties. I met undergraduates, but I was not involved in undergraduate teaching until quite late. Because you see, GALCIT was always the graduate school of aeronautics; there was no undergraduate program. The only, in a sense, undergraduates I taught were in the courses I taught in industry, in these war-training courses. And there, of course, the situation was quite different. They were usually older, more experienced, but not specialized people.

GREENBERG: I suppose by the sixties, though, you were so used to the way things are done here that undergraduates were no surprise to you when you encountered them for the first time.

LIEPMANN: Strangely enough, I taught a thermodynamics course, which became APh 17, on a two-week notice, because I complained about the way it was taught. And they told me at the time, "Why in the hell don't you do it yourself?" They thought I would turn it down, of course. But I didn't. Then I discovered that I had overestimated the students. I took a look at them and back-pedaled real fast. I mean, they were always depicted as the

young genii who can do anything—and of course, this is only limited truth. They are extremely good, but not that good. No, I found undergraduate teaching very rewarding, but much more difficult than graduate teaching.

As a matter of fact, I think the undergraduates are really mistreated here. I'm amazed that they don't make more noise, because they are overloaded, and nobody can tell me differently. I think a student should have enough leisure to follow up on a particularly interesting subject he had in class, or an interesting problem, or do something else. Here, they go from one problem set to the next. They are always behind, always overworked, and then you get this famous burnout; they suddenly want to take a leave. I do not think we should cater only to the best-prepared and brightest guys, but take into account the possibly deeper but certainly slower-moving ones. So there I disagree completely with the policy.

GREENBERG: Do you think European education accommodates that kind of a person better than ours?

LIEPMANN: Possibly yes, but on the other hand, they lose a lot of the guys who simply get out of school and take it easy for a while and then can't find the knack again. That's different now, too. The universities have tightened up much more, because schools are now overcrowded. Originally, when I went to school, you could stay in school as long as your money held out—but not anymore. I think here we could accomplish more by relaxing a little. I am afraid that part of our difficulty there is overambitious teachers—ambitious in a funny way—namely, by what I believe is confusing the value of a class with its difficulty. I do not believe that you always overstrain everybody, and that you shouldn't say things twice. I would be for a much more leisurely approach. And I do not believe it would impair our standards at all.

GREENBERG: You did some consulting work at Douglas Aircraft.

LIEPMANN: Yes.

GREENBERG: When was that?

LIEPMANN: I was hired to be a Douglas consultant in a car on Wilshire Boulevard by Francis Clauser [C.B. Millikan Professor of Engineering, emeritus; d. 2013]. And that was, I think, ’44 or ’45. He told me that Douglas was designing a supersonic fighter. This was the X-3. And I thought he had lost his mind; I thought they were crazy. Because that was just when we started understanding supersonic flow. The idea that you already had to start designing something was at the time strange to me. They were completely correct. The next thing—which is, by the way, very interesting—is that we moved in Douglas immediately to the design, and simultaneously, or a little after, we worked on the first satellite. That’s less known. That was in ’45, if I remember correctly—anyway, it was way before anything was ever talked about. It was a satellite report about the feasibility of making a satellite. And out of that study the RAND Corporation was formed, with people who were on that study. I worked on that, and Paco Lagerstrom [professor of applied mathematics, emeritus, d. 1989] was on it, and Francis Clauser was fairly high up in it. It was then shown that this was a real possibility, and completely forgotten is that it was confidential. It’s declassified now.

I was a consultant for Douglas for fifteen years. I stopped when Junior’s [Donald Douglas, Jr.] time started. When [Harold T.] Luskin left the research group, the company didn’t look very good to me anymore.

GREENBERG: Did Caltech aeronautics graduates feed into Douglas in the thirties and forties?

LIEPMANN: Oh, yes. And it is replaced as our closest industry contact only recently by TRW, which is now pretty much what Douglas used to be. But Douglas still has, I think, fifty or sixty of our graduates.

GREENBERG: With that in mind, and the fact that wind tunnel work on the DC planes was done here, was Caltech’s industrial connection in aeronautics peculiar to the institute?

LIEPMANN: Oh, yes. And still is. You see, the institute administration, with a few exceptions, has never completely understood what we did with the wind tunnel, which was essential for the building up of the whole of aeronautics, and a lot of the engineering

part—namely, the ability to work with the daily problems of industry, rather than long-range planning and research. Every single airplane flown in this country, I think, has been in the 10-foot tunnel, with very few exceptions. I can check it out, but I think practically everyone has been in there, until very recently.

GREENBERG: One of the concerns of Caltech's founders—Arthur Noyes, in particular—was to take research out of the hands of industry. They wanted an institute for basic science, whose research would not be constrained by narrow industrial concerns. Did this philosophy enter into the creation of GALCIT in any way, or is the history of GALCIT different from that of other departments?

LIEPMANN: I really don't know. I took GALCIT the way it was. I think von Kármán was quite aware of that concern. A philosophy which I always followed in this respect, and which I'm pretty sure must have been very similar to Millikan's and von Kármán's, is that working closely with industrial problems leads to ideas of a much more general type. You see, the mistake some schools make is to try to cope with day-by-day engineering problems. Industry can do that much better. The point is that industry, on the other hand, wants to fix a problem and not solve it. Solution and understanding, this is our domain. But if you do not have interaction with the day-by-day problems, you don't even know what they are. Many of the important fluid-mechanical problems started out as rather trivial, day-by-day problems in industry.

Basically, what you develop in a school, and what I believe should be developed, is the understanding of fundamentals with an appreciation for applications. There's nothing more challenging than to see an application being told to you by some guy who's involved, say, as a test pilot. It's completely jumbled. And you have to dissect the whole thing, clean it up, and suddenly you have a beautiful problem, and also something which has far-reaching importance.

GREENBERG: At the same time, the basic aspects—the understanding of fundamentals—are tied to practical considerations.

LIEPMANN: Oh, yes. You see, to me, a school is where the long-range, basic, low-level, in the financial sense, research is being carried out on fundamental problems. And then, the researchers are getting a continuous feedback in and out with industry. That looks, to me, as applied science.

GREENBERG: Why is aeronautics different from other engineering disciplines?

LIEPMANN: Well, you see, the point which is so different in aeronautics—which I stress all the time, and why aeronautics developed differently here, in particular, and in most schools when they finally got around to it—is that you're incapable of covering up your ignorance by using safety factors. So you've got to do it right, and you've got to understand better. That's why the mathematics and the physics background was more predominant in aeronautics than in any other discipline. And this is still true. Look, take composites. There are a lot of things you still don't know about composites, but you certainly can't solve the problem if you safeguard yourself by a safety factor of 5. You might as well forget the whole design.

GREENBERG: You said earlier that von Kármán was very important at the beginning in tying aeronautics and aviation together. Now he's been gone a long time. Are they tied together as well today?

LIEPMANN: Yes, but there is one fundamental difference. At the time when von Kármán came—and almost to the time when he left, not quite—the aeronautics staff at Caltech was nearly the same size as the aeronautical staff at Boeing or Douglas. Today, this is a factor of 1 to 100. So it is clear that it must be different. On the other hand, we cannot work only on problems related to aviation, for the simple reason that if we do, first, we are not broad enough to educate our students; and second, we would not be able to survive the ups and downs of the aviation industry; and third, the interest in problems which arise from aeronautics has been increased and broadened. I feel that the general fluid mechanics we are doing now produces students who are much more versatile and who can go into many more fields—including aeronautical engineering but not excluding

others. We have students in fusion research, we have students in laser development and communications, et cetera. Aeronautics, I think, has changed, and it had to change.

When they offered me essentially the successorship to Prandtl, they asked me why the next generation after Prandtl was unsuccessful. I told them that they had worked, in my opinion, on the problems they thought Prandtl would have worked on—meaning the problems Prandtl had worked on ten and twenty years earlier. But he wouldn't have stayed in that field. You see, it is this difficulty, which a lot of pupils of great men have—that they do not realize that however great he was, he lived in a different time. So if you do not move with the technologies, you are out today. And that realization has to come almost instinctively.

GREENBERG: Is Clark Millikan largely forgotten?

LIEPMANN: Certainly not by the people who really knew him. But Clark always had a very low profile. He spent his first years in the shadow of von Kármán. And in this respect, I don't think von Kármán was very generous; he did not push somebody into the limelight easily. But the people around here—you can ask anybody who knew Clark—they are not only fond of him but they admire him. And that goes for a lot of people he dealt with in government and industry. But it is certainly true that he was never such a colorful man as von Kármán—and also, of course, von Kármán was outstanding. There is no doubt that von Kármán had an enormous mathematical and engineering ability. Coupled with that, he had a great deal of showmanship and also an effectiveness in dealing with people in industry and in the military. This is unique.

GREENBERG: It's an impossible act to follow.

LIEPMANN: It's an impossible act to follow. Prandtl was entirely different. And I'll tell you another thing, G. I. Taylor, who scientifically was certainly at the level of von Kármán—in fact, in some respects I would rate him even slightly higher—is certainly not nearly as well known. I wouldn't be surprised if a lot of people in England would not know who G. I. Taylor was. You see, sometimes the limelight works and sometimes it doesn't. If you look at the great scientists of our times or previous times, you see that

Einstein had everything. And in spite of the fact that nobody who knew about him when I was a schoolboy understood the theory of relativity, he just caught the imagination. Freud was another whom everybody knew. But somebody like Planck, or Bohr, or Rutherford—people who have in many respects been as revolutionary as Einstein or Freud—are much less known. Let alone Pauli; most people haven't heard of Pauli, and he was one of the absolute top physicists. So there is a rare combination of ability, and also public image, in von Kármán's case, and it worked like a charm. And also opportunity. I think Caltech being really the only place which was prepared for the demands of war, in aeronautics, made a big difference.

GREENBERG: Do you have any opinions about how the Tsien affair of the early fifties was handled? [4]

LIEPMANN: It was handled, I would say, very ineptly. But the basis for the handling of it was correct. I think the overall fault lay with Tsien, and partially even with von Kármán. Because Tsien went on the Air Force Scientific Advisory Board, remaining a Chinese citizen, with no intention or declaration of staying in this country. This, I think, was plainly incorrect. It was done probably on von Kármán's urging. Tsien went to Europe on an evaluation team with the rank of an air force colonel and then decided to go home right after the Communist revolution had swept China. This, I think, was completely unrealistic. Now, the reaction of the government, from what I've seen—I've seen it somewhat from the outside and only a little from the inside—was also peculiar. They first, I think, tried to talk to Tsien, and then they weren't sure he would stick to any agreement. So, I've been told, they advised customs to keep track of Tsien's shippings. And Tsien shipped his famous 1,700 pounds of books, including the one confidential report, which he had actually edited himself. And this was given as the reason for

[4] A graduate student under von Kármán, Chinese national Hsue-Shen Tsien earned his PhD from Caltech in 1939 and was appointed to the faculty. After wartime (civilian) service to the United States Air Force in rocket research, he was appointed Goddard Professor of Jet Propulsion and first director of Caltech's Jet Propulsion Center. In 1950, accused of concealing membership in the Communist Party in the late 1930s, Tsien's security clearance was revoked, he was arrested by the FBI, released on bail, and confined to Los Angeles County. A hearing by the Immigration Service resulted in a deportation order (which conflicted with a earlier restraining order by the same service forbidding him to leave the United States). Tsien was permitted to return to China in 1955, where he became the chairman of the Institute of Mechanics of the People's Republic of China's National Academy of Sciences in Beijing. Tsien died in 2009.

restraining him from leaving. But there was no way to restrain him legally. I mean, he was a Chinese citizen, and I presume the [U.S.] government was not willing to admit that he had been on a confidential board. Even that wasn't the worst. What happened then, if I recollect correctly, is that the Immigration Service tried to deport Tsien. And that became a comedy, a very unpleasant comedy for him. At one time they took him out to Terminal Island and locked him up. And then he came back here, and he wasn't supposed to circulate freely or read reports freely. And that was essentially mishandling, but I still think that basically it was Tsien's fault.

GREENBERG: You think that there were sufficient grounds.

LIEPMANN: There were sufficient grounds. Also, I know that Tsien was very left. One should, on the other hand, remember that it was before the Korean War, so it was not that serious. But Tsien was certainly not, as has often been depicted, a completely innocent martyr of the American government. I think that's complete nonsense. Tsien, in a way, had a great deal of conceit. I think he felt that he, with von Kármán's help, was above a lot of things—which, by the way, was also true of von Kármán—that certain rules did not apply to him. And I think that caused a lot of the trouble. That's the way it looked to me. And to me, it was absolutely inconceivable. I told you, he came down the stairs and told me, "Hans, I'm going back to China." And I turned around and said, "Tsien, they can't let you." It was to me obvious that he couldn't leave. The whole thing has been used to demonstrate that the government is always bungling—which to a certain extent, it was. But I don't think that story is quite correct.

GREENBERG: You say von Kármán did hold Tsien in high esteem. How do you rate Tsien as a researcher?

LIEPMANN: Tsien is very competent, but in my opinion, not very original. In fact, I would go further than that. I would say that the combination, Tsien–von Kármán, was unbelievably good. Von Kármán would throw out the ideas; Tsien would scrutinize them. He had a good technique, worked hard, was a very good critic, and so on. The breakoff of that intimate relationship put Tsien on his own. I think the realization that it

was not flowing as much as before may have also caused some of the bitterness he felt. On the other hand, he had one fault, which was that he couldn't admit a mistake. He wrote some papers which were mathematically wrong, and he wouldn't even admit that. This is, I think, part of Tsien's makeup. He was a very difficult case in this respect.

GREENBERG: In effect, he worked out von Kármán's ideas; he solved the problems posed by von Kármán.

LIEPMANN: Well, he did better than that. He also was perfectly capable, for example, of finding a direction, or extending it. For example, in China, he must have been unusually competent, because he noticed when there was something in the air. He noticed when cybernetics was important; he noticed when nuclear engineering was important. But I do not believe that he was a man who had very original ideas. And I do not think that any of the papers not done with von Kármán are really earthshaking. That's one of the hot subjects around here.

GREENBERG: This is a controversy, even now?

LIEPMANN: It's a controversy. And I knew that pretty damn well. People, of course, claim I'm jealous. But I assure you that had nothing to do with it. He was a theoretician. And in fact I never was very hot in the fields he worked in. This is certainly the way I saw it. And I'm pretty sure it's reasonably objective.

GREENBERG: You saw him as recently as last year?

LIEPMANN: Yes. It was very pleasant. He had only minor trouble during the Cultural Revolution, because he was in Beijing. What my friends here tell me is that in Beijing Chou En-lai kept the Red Guards under control, so nothing much happened to him. He is very up. I mean, he appeared with a black limousine, with side curtains and back curtains and all the paraphernalia. And he may be really good at, for example, organizing the buildup of a missile-producing facility or of a whole research project to go into missiles. For that he would be exceedingly good—and exceedingly difficult to deal with. I mean,

he would be the worst dictator you can imagine. In China, I think, he deserves what he got. He is a first-rate engineer; don't misunderstand me on that. It is just that to rate him in the same breath with von Kármán is not right at all. I don't think he went and worked in nuclear energy in China. That was quoted, but I don't believe that. I think he must have done the missiles, for which he was competent, and the wind tunnel, for which he was competent. The only thing about which I would always feel uneasy, if Tsien were to work for our government, is his complete lack of ability to admit mistakes. That would scare me, because, you see, that means he would blame somebody else or cover up. He had not the ability to say, "OK, I was wrong here. I made a mistake"—which von Kármán had, by the way. I think there are no scientists on Earth who haven't done this at one time or another. But I cannot imagine Tsien ever saying, "Boy, here I really made a botch."

GREENBERG: Apparently, there was slower than anticipated progress in laboratory installation of supersonic and hypersonic flow apparatus in 1950. Is there anything to discuss?

LIEPMANN: No. I think it came through pretty well. In 1939, von Kármán wanted a supersonic tunnel—I remember that. It was supposed to be a 15-inch supersonic tunnel, a size which today is peanuts. At the time, the general in charge of procurement told him he would never give him the money and would see that a thing like this would be kept out of the hands of the longhairs. That made von Kármán exceedingly mad. Tsien and I together convinced von Kármán that to build the little 2.5-inch tunnel, which we still have, was worthwhile. He didn't even want to touch it; he said it was too small, it was useless, and so on. We convinced him to do this. And then a peculiar thing happened. I was originally supposed to run that—and suddenly I wasn't. It was never clear why. Today I would say I was too inconvenient for Tsien. Then he did something which turned out to be a mistake. He hired Allen Puckett, who is now running Hughes. Allen Puckett was exceedingly good, exceedingly smart, and with much bigger push than I. And pretty soon Allen Puckett was running the show and Tsien wasn't. Puckett then built up both the supersonic and the hypersonic lab. And I do not see there was much

delay in that. Eventually, it became Lees' lab. In between came [Henry T.] Nagamatsu, who was pretty much inferior. And in between, I built the transonic lab [1946]. But the whole country was behind in high-speed research. Once it got going here, it worked well. I mean, we had to learn how to do it.

GREENBERG: Let's turn to the tail end of the DuBridge era, the years just after Clark Millikan's death. The year 1966-'67 looks as if it might have been a critical year. Clark Millikan died; GALCIT lost two of its members to other institutions; Maj Klein retired; three other people from GALCIT moved to the new applied mathematics group. Around this time, you received an offer from Göttingen, among other places. Could you talk about these developments?

LIEPMANN: Yes. The death of Clark was a tremendous shock. The following year we were not very fortunate. You see, Ernie Sechler became executive officer. Ernie was an excellent engineer, but not, in my opinion, with a very broad scientific view. He was the best man I have ever known in judging students, but not in judging faculty. We got some peculiar birds in there at that time. So that was somewhat trying. And at the time, I got an offer from Göttingen.

GREENBERG: This time was personally trying for you?

LIEPMANN: Oh, yes. I found the whole business very annoying because, you see, you never knew what was going on; the unity of GALCIT wasn't preserved. I had also the feeling—and that feeling is coming to fulfillment now—I thought Caltech was going in the wrong direction. That was the beginning of this idea of "Science for Mankind" [a Caltech Development campaign], which was the beginning of this strong interest in social sciences, which I think is wrong. So, at the time, I was susceptible to going—and the Göttingen offer came out of the blue. I had no idea about that, and I was extremely naïve about it. You see, I'm naïve with finances and so on. I didn't even know that the Göttingen offer was unbelievable. I went over [to Germany] and looked at it, and never found out that I would have retired on full salary—no, I did find that out, finally.

That was by far the most serious consideration I ever gave to leaving Caltech. In fact, it went so far that I took my children out of school and I was ready to go. I'd never said so, but I felt really unappreciated here—there's no doubt about that. And the people who talked me out of it were the group here. They were upset; [Anatol] Roshko [Von Kármán Professor of Aeronautics, d. 2017] was upset, and Roshko got Dick [Richard P.] Feynman [Tolman Professor of Theoretical Physics; d. 1988] to sit on me. So finally I decided not to do it. Not that I didn't want to go back. The people in Germany still say I had hesitation about coming back there. That's baloney; it's not true at all. But there was the consideration that I didn't know how my kids would fit in German schools. But the major one was that I really didn't have the nerve to leave the group here, and that was quite a trauma. In fact, that sometimes bothers me yet. Because I think, as far as my own scientific and technical development is concerned, Göttingen would have been better. I did not find out what they would have paid me—typically bad. But I found out that I would have had a $1.2-million budget; and budget meant budget. I wouldn't have to ask for contracts. And at the time—it's not true anymore in the Max Planck Institute—I would have had a free hand directing the budget and selecting the people. So it was an offer which was very unusual. I felt that Caltech never gave me any credit for staying here. I said to DuBridge at the time that this was one chance in a lifetime. He said they would make it the chance of a lifetime here, but nothing really happened.

GREENBERG: So it still haunts you from time to time.

LIEPMANN: It irritates me, but not in this respect, because in a way it worked out here pretty well. What irritates me is that while people claimed I saw black, [my fears] did come to pass. In particular, in very recent time I am not too happy here. You see, Caltech used to be a fraternity of scholars, where salaries and outside consulting and making your own companies were irrelevant. We came here not because of the salaries but because we liked the surroundings. We had complete trust in the administration. There was nobody checking on the administration from the faculty. It was this mutual trust, and this is not here right now. Whether that's only because of time or due to personalities, I can't judge. But that, at the time, I noticed happening. And that was the

main reason I was not as tightly bound as I used to be. But I turned Harvard down on the phone before, and Stanford the same way. And I got three offers from MIT with salaries almost twice mine, and turned them down. Before, I was completely tightly bound here, because I liked it. It was never a question of salary, it was always a question of the ability to do research, to teach, and to feel that people were here because they were dedicated to the profession and not because they got more salary here, or housing or staff benefits. That, I think, has eroded, and it is eroding. You see, you have to be a tough cookie.

Of course, inroads have been made into academia. And I do hope that we have not come to a point where dedication to teaching and research is considered old-fashioned and people are considered suckers if they do not run a company on the side or have patents and so on. That is, I think, my main concern now. This idea that technology and science is really bad for humanity—the atomic bomb, and so on—that started it, and that was stupid, in my opinion.

GREENBERG: Sechler was named executive officer in ’66. And in 1972, you were named director of GALCIT. It seems like that took quite some time.

LIEPMANN: That was a measure of the indecisiveness. They couldn’t decide between me and Lester Lees, I think. And in a certain sense, also, they didn’t know what was going on here. Also, I didn’t push it. I had never thought of becoming director of GALCIT at all. In fact, when they offered me the Max Planck Institute, I told the president at the time that I wasn’t even sure I could do it. I wanted first to think about it. You see, I never had thought about this position business. I was perfectly happy to see Clark run the show. Now, I think that was a mistake. I could have done more for GALCIT if I had taken it right after Clark. Gerry Whitham [Powell Professor of Applied Mathematics, emeritus; d. 2014] tells me now that he wouldn’t have left and gone into applied mathematics. I could have kept the thing together, I think.

GREENBERG: But you weren’t given the opportunity.

LIEPMANN: No. I was never given the opportunity, and I also never pushed for it. But looking back now, it would have been a better solution at the time, I think.

GREENBERG: So Whitham left aeronautics to go to applied mathematics in part because of some of the problems within aeronautics.

LIEPMANN: In part. He could never get along very well with Ernie.

Begin Tape 3, Side 2

LIEPMANN: But I should also say that these things always look simpler from the back than from the front.

GREENBERG: How closely is the creation of the applied mathematics department bound up with the history of the aeronautics department?

LIEPMANN: Very closely. As a matter of fact, the people who started it—Lagerstrom, Whitham, and [Julian] Cole [professor of aeronautics 1959-67; professor of applied mathematics 1967-69]—were originally theoretical aerodynamicists. Whitham had a joint appointment in mathematics and aeronautics. They wanted to broaden out. Then also, they wanted to get out from under Ernie, I think, because they felt that Clark had represented them but Ernie didn't. That was certainly a feature of the situation. And I was perfectly willing to go along, because it looked to me that if they wanted to go to a new applied mathematics group and take root with another field, the contact would remain—and it has remained even today. Not to the same extent, but right now we are debating making a big joint undertaking with industry again. So that contact remained. You see, the ties are still very close. So, in a way, that was not a real loss. The only serious loss was the loss of Julian Cole. But the future will tell how close we can keep them. I mean, of course, the people who come in now, mainly the numerical people who had no part in the history—how that's going to develop depends on mutual understanding and so on. But so far, it has worked very well with [Donald] Cohen [Powell Professor of Applied Mathematics] and [Philip] Saffman [Von Kármán Professor of Applied

Mathematics and Aeronautics; d. 2008], and in particular Whitham and Lagerstrom—and, to a certain extent with [Herbert] Keller [professor of applied mathematics; d. 2008] and [Heinz-Otto] Kreiss [professor of applied mathematics; d. 2015]. But we have to see how that goes.

GREENBERG: There are still joint faculty appointments?

LIEPMANN: No. Right now we don't have a joint faculty appointment. We've been talking about it, but we never did it. But it is certainly so that people go to one another's seminars—that some of our students go over there, and so on. It is not as close as I would like. The fact that we have now the applied mathematicians makes it impractical for us to have theoretical aerodynamicists, because we would get another theoretician in a group which has already many, and that makes it sometimes difficult. We cannot offer all the education to theoretical students we could otherwise. But I think it's a minor question; it's not a major question.

GREENBERG: In the early sixties when all this came about, were the individuals involved reluctant to be referred to as applied mathematicians?

LIEPMANN: No. In fact, that they liked. Most of them considered themselves applied mathematicians.

GREENBERG: Since you became director, has GALCIT changed in any significant ways—new lines of research, new administrative procedures?

LIEPMANN: No. There are continually new lines of research, and I'm not particularly good at administrative procedures. The only thing I did is I eventually asked Bill [William H.] Bettes, who's running the wind tunnels, to be what I call manager. He does the budget and essentially is the direct supervisor for the staff, because I am lousy in doing budgets and memos and so on. The research would have changed in the same way, anyway. I think that develops one out from the other. In fact, when they first told me I should become director— It was [Francis] Clauser who started that chain of events, and

I told him that really it wouldn't make much difference; I would do the same thing anyhow. So it really has made, in this respect, little difference except that I am more or less in charge of some of the budgets. But since Roy Gould [Simon Ramo Professor of Engineering, emeritus] became division chairman, my competence has been quite cut down, because he felt that everybody in engineering ought to be more equal. We used to have quite a few privileges. We had our own instrumentation budget; we had our own travel budget, and so on. Now everything is handled from the division. And that brought me very close to resigning, because I was very dubious whether that made any sense. Well, I didn't—again, really, mostly because of keeping the thing together. But I didn't like it. And at the present time I'm not particularly happy with the way things are.

GREENBERG: What are your current areas of research? Are you doing things that are new since the time you took over GALCIT?

LIEPMANN: Yes. Liquid helium for example, quantum fluids. And then I'm back to something which is new to myself, with a group of students, of course. That is the control of transition. And I hope that the last thing I'll do is to try for the control of turbulence. That we are actively doing now. There's a lot developed, due to Roshko—and [Garry] Brown [professor of aeronautics, 1978-1981], when he was here—in the development of chemical reactions in turbulent flows, now handled by [Paul] Dimotakis [Northrop Professor of Aeronautics and professor of applied physics]. That's new. Then there is something that's just starting now which is going to be exciting: [Bradford] Sturtevant [Liepmann Professor of Aeronautics; d. 2000] has been working on two-phase fluids. And he has now a cooperation with [visiting professor] Susan Kieffer, who calls herself a geochemist. She is really involved in geophysics, working on Mount St. Helens and the geysers. They have gotten some startling results applying modern gas dynamics to volcanic explosions and geysers. In the structures area, we are relatively weak. We have a first-rate guy in Chuck [Charles D.] Babcock [professor of aeronautics and applied mechanics; d. 1987] and, to a certain extent, [Wolfgang] Knauss [Von Kármán Professor of Aeronautics and Applied Mechanics, emeritus] is OK. They are moving with the

times. They are going to move into composites, and they have been working on crack propagation.

So, things have changed. But I don't think I had very much to do with that. I think my main contribution here has been to keep this thing together and keep it alive—sort of a spark plug. A lot of the things which have been worked on in fluid mechanics were originally my ideas, that's correct. But not all of them, by a long shot.

GREENBERG: One of your more recent awards was in teaching. How have you managed to be both a distinguished researcher and a distinguished teacher?

LIEPMANN: I have no idea, but I must say the following. I consider teaching more important. I always have. I think you have to do research. First of all, you're driven toward it; and second, you can't teach on the level of Caltech without doing research. But I think that teaching is basically more important. That's really our main goal in life, if we take the professorship seriously. And also, I think, it has the more lasting influence. Whether you like it or not, most of your startling papers are going to be footnotes in handbooks in the not-too-distant future, and that goes for everybody. Even today, I would find it very difficult to say that one single paper of von Kármán couldn't be dispensed with. But the teaching, the passing on of a certain style and approach to science, and also to knowledge, in a sense—that is, in my opinion, a more challenging and also more rewarding business. And that's why I put a lot of effort into teaching.

GREENBERG: Do you have a sense that this can be connected back to not very satisfactory teaching when you were in school?

LIEPMANN: No, I would say the opposite. I had an outstanding theoretical physics teacher—Gregor Wentzel. And that really struck me, what really good teaching could be. So that made more impact. I once talked with Dick Feynman about that, too. You see, with really good teaching, it is the same sort of thing—if you see a particularly nice mountain range or a particularly good sunset, you want to show somebody else. It's nicer to show somebody how beautiful something is. And that, I think, is a very large part of

wanting to teach—and to a certain extent, I think, a somewhat more mature and realistic attitude toward what your own research is going to be.

GREENBERG: Is there any particular award or honor that you've received which you regard as the most satisfying?

LIEPMANN: Well, I think what I really got a kick about was to get the Sigma Xi, the Monie A. Ferst Award, because that was the closest to what was real. Namely, for the bringing up of a set of graduate students and the inspiration of graduate students—or something like that. That I felt was the closest to where the citation was correct. This I really consider my main contribution: to bring up a bunch of students who might not have gotten as good without me, that's all.

GREENBERG: Would you care to pursue this business about research in the social sciences that goes back to the sixties? I gather you're not very happy with it.

LIEPMANN: No. I don't have anything against it in general. I just think that Caltech is a technological university; it caters to a specific group of people who are, in many respects, oddballs. Somebody mentioned the other day we should be the combination of an institute of technology and a college of humanities. Namely, in humanities we would find teachers and professors who would enjoy teaching, in the same sense I just told you—showing the beauty of humanities to a bunch of very intelligent and very technically minded people. I think that the development in economics and social sciences is wrong; that it drains our resources; that it has, in many respects, brought a split between the administration and a large part of the faculty; that many of us, including me, feel that it was introduced through the back door and not quite openly. There was a long story at the time that they would be self-supporting and there would be no drain on Caltech's resources, which turned out to be, of course, not true. It's partially the attempt of two of our presidential candidates, at least, to, as they say so nicely, leave a mark. And I have a very low opinion of people leaving a mark. Either you leave a mark naturally or you're better off not trying. I feel that any attempt for us to become another kind of university, with the size and the resources we have, is going to make us second-

rate. If I want to go do social science, goddamn it, I don't go to Caltech! Actually, if you talk to people who used to be students here in the old days, the humanities were highly esteemed. There is so much—history, the history of science, English, literature, and so on. And it ought to be a challenging task without having to go into graduate programs.

GREENBERG: What about the whole attempt to formulate the social sciences analytically, which is essentially the orientation of people here?

LIEPMANN: I wonder whether that is possible. And, in particular, I wonder why we should do that. The much advertised connection with the sciences just didn't happen. They said they were going to talk to the applied scientists; I think they never did. And I don't think it's our pot of tea. The people I know at MIT say they shouldn't have done it, and MIT did it more successfully. Also, MIT happens to be five or six times larger. But to see the humanities division now, which has twenty-one permanent students, and then the engineering division, which has 500 and is understaffed, is ridiculous in my opinion.

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 Nobel Prizes because the field is not as sublime in the twentieth century as the branches of physics. What's your opinion about that?

LIEPMANN: Well, that is partially true. I think in Prandtl's and Taylor's cases, a Nobel Prize would be easier than with von Kármán, because von Kármán's was much more subtle. In Prandtl's case, the concept of the boundary layer was certainly of Nobel Prize character. And in Taylor's case, his dislocation theory was, too. Nobel Prizes some people get and some people don't. It is part of the scientific establishment that the overwhelming part of Nobel prizes in physics, in modern times, went to atomic physicists. The ones which were given for mechanics were given indirectly. For example, [Donald] Glaser got it for the bubble chamber. That's a mechanical problem, and I don't think it's even very high-level mechanics. [Hannes] Alfvén's work on magnetohydrodynamic waves is certainly not on the level of some of the research of Prandtl, Taylor, or von Kármán, but it worked into different directions. So I think von

Kármán was bitter about that, to a certain extent, and in a certain way correctly. On the other hand, if one remembers that Sommerfeld never got a Nobel Prize, he shouldn't complain. Prizes are funny things. You have to reach a certain level to be even in the view. But afterwards, it's often a pure matter of being at the right point in a discussion at the right time.