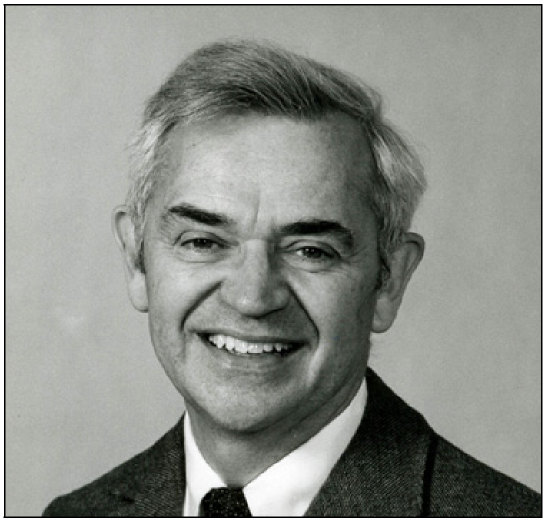

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FRANK E. MARBLE
(1918–2014)

INTERVIEWED BY
SHIRLEY K. COHEN

January–March, 1994, April 21, 1995

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CALIFORNIA INSTITUTE OF TECHNOLOGY
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Subject area
Engineering, aeronautical engineering

Abstract
An interview in seven sessions, January 1994-April 1995, with Frank E. Marble, Richard L. and Dorothy M. Hayman Professor of Mechanical Engineering and Professor of Jet Propulsion, Emeritus. Marble discusses his undergraduate and early graduate study at Case School of Applied Science, his work at the NACA Engine Research Lab in Cleveland, where he was in charge of large-engine research project for B-26 bomber, and his arrival at Caltech in 1946 to complete his doctoral degree in 1948. He discusses his graduate students, including Benoit Mandelbrot and Chuang Feng-kan, his colleagues Clark Millikan, Hans Liepmann, Duncan Rannie, and Arthur Erdélyi; and the beginning of his close and enduring friendship with Theodore von Kármán. Recalls his first visit to Europe in 1949, his meeting with Moe Berg in Switzerland, and his appointment that same year as the first new faculty member of Caltech's Jet Propulsion Center, and the group of courses in jet propulsion he developed for the Center. Besides his discussion of his work in combustion in jet-propulsion systems, flame stabilization, and propagation of acoustic waves, the interview contains his recollections of Tsien Hsue-shen and McCarthy-era politics, the army's refusal to renew Tsien's security clearance in 1950 and Dan Kimball's role in the Tsien case, and Tsien's deportation five years later. Recalls his visiting professorship at

Cornell University, spring and summer 1956; his involvement with the Advisory Group for Aeronautical Research and Development of NATO; the development of engineering at Caltech; influence of Felix Klein; and Robert Knapp and the Hydrodynamics Lab. Comments on the GALCIT complex; Homer Joe Stewart; Ed Zukoski; and Ann Karagozian, his only female PhD student. Concludes the interview with his work on compressors; development of supersonic transport and jet noise; turbulent flow; vortex-combustion theory; work in the 1980s; "The Marble Problem;" very-high-speed flight; invitation to teach in China (1982) and seeing Tsien and his family again; Lee DuBridge; and the Caltech Flying Club.

Administrative information

Access

The interview is unrestricted.

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CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH FRANK E. MARBLE

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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California Institute of Technology
Oral History Project

Interview with Frank E. Marble **by Shirley K. Cohen**

Pasadena, California

Session 1	January 26, 1994
Session 2	February 9, 1994
Session 3	February 22, 1994
Session 4	March 4, 1994
Session 5	March 10, 1994
Session 6	March 22, 1994
Session 7	April 21, 1995

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COHEN: Just give us a little résumé of your growing up in Cleveland.

MARBLE: Well, I can say, for one thing, that at the time I was there, Cleveland was perhaps the finest city to grow up in that I know of. It had so many advantages and so few of the drawbacks that it seems to exhibit now. But before I go into the details of my life in Cleveland, I should say a bit about my parents.

My father, Louis C. Marble, was born in Marcellus, New York, which was a little town then, and it's still a little town, between Ithaca and Syracuse. And his father—my grandfather, whom I was named after, eventually—was Frank E. Marble, who was a captain in the Civil War, married after the Civil War, and subsequently moved to Swampscott, Massachusetts just north of Boston. They had quite a fine house there, because my grandfather became associate commissioner of pensions for the State of Massachusetts after the war. And because they lived there, eventually my father went to what is now known as MIT but at that time was Boston Tech—it was on the Boston side of the Charles River. Word has it that Boston Tech was where Filene's Store is now. Some people at MIT like to dispute that, but I think it's true. He studied civil engineering there, and left school after his father died. And it was some years hence that, as a civil engineer on building railways, he made his way to a little town south of Cleveland called London—near Columbus, actually; about twenty miles from Columbus. And while he was there, he met Blanche Rayburn, whom he was to later marry and who was my mother.

Their backgrounds were very different. My mother was a farm girl. I think her father left home, or was encouraged to leave home, when my mother was fairly young—that is, in her teens. My mother was academically very good. About the only thing she could do as a girl then was to go to what they called "normal school" and then to teach. And for some years she taught. And she was teaching in London, Ohio, when she and my father met. My father's work eventually brought him to Cleveland. My sister, Rosalind, was born first. She was born in 1911, and I was born seven years later, in 1918, just before the end of World War I. And that's how we made our way to Cleveland.

Now, the gifts I inherited from my parents are really quite interesting. My father, I think it's fair to say, was not a scholar. He was a very practical man, a very good engineer. Eventually, he made his living—until the Depression did us all in—he made his living as what we'd now call a technical representative of several organizations, which sold everything from railway equipment to conveyors. So he traveled quite a bit. But as I say, he was a practical man, and he had great capability at building things. He did very good woodwork, and he taught me the trade, more or less. My mother, on the other hand, was a potential scholar. She never had the real opportunity to educate herself, but she was mathematically very capable. Her arithmetic skills were unusual. So I believe I got that from my mother. My interest in mechanical and engineering things I believe I got from my father.

Now, as a young boy, I was not big, I was not strong. Although I could hold my own, I guess, with the kids in the neighborhood, I was not of athletic quality at school. And actually, I was not of high academic quality at school either. My performance in Rosedale Elementary School…

COHEN: This is on the East Side of Cleveland.

MARBLE: I'm sorry, I should have said that we lived at 1322 East 112th Street in Cleveland, which is south of Superior Avenue. The area was really very fascinating. The Rosedale School bordered on the large Italian section, which you may remember is to the south and east of Cleveland. And that, first of all, set to a certain extent the population of the school; and secondly, it set a cultural attitude, a cultural tone. After I left Rosedale School, I went to Patrick Henry Junior High School, which was farther north—I guess almost to St. Clair. There, of course, the entire population group changed. That was predominantly a Jewish area. And I've always credited my immersion to the Jewish culture as a very important part of my education. You may remember, on 123rd Street there was a delicatessen called Solomon's. That was a favorite place for the junior-high-school students to hang out. Two years ago, my wife and I drove through there, and we drove down 123rd Street, and there was Solomon's. But it wasn't a delicatessen anymore. The sign was there, but the store had changed.

Well, to get back to my childhood, I was a good bit younger, seven years younger than my sister. We had relatively little contact. I mean, she went her way and I went mine. It was clear that she favored her mother and I favored my father, as far as activities went. My father was very attentive to me, as far as mechanical work, shop work, looking at books was concerned. He loved to sit down with me and go through books that showed machinery and things of this sort. And I was fascinated by that. Also, he was very interested in wildlife. He had marvelous books and pictures of wild animals, which I enjoyed a great deal. He had been a considerable sportsman. He was a fly fisherman; and fly fishermen are a peculiar breed. They think there's nothing else like it. And one of the low points in my father's life was that I never really took to fly fishing at all. I could catch a lot of branches and leaves, but I could very seldom get the fly out on the lake, where it should be. I was about ten years old at that time.

Another thing that came from nowhere was music. It became clear to a lot of people that I had musical ability of one sort or another. My father was not very supportive of it. My mother thought that music was a terrible way to make a living, and I shouldn't follow it. Whether that had an influence on me or not, I don't know. But I became very interested in music nevertheless; studied, learned on my own a bit about it. And eventually, when I was about thirteen, just about the time I was either leaving elementary school or starting in junior high school, I bought a trombone. And in many ways, music and the trombone changed my life. Because as I said, I was never strong. I was never intellectually at the top of the class at all. In fact, I was sort of lazy about studying. I didn't study well. I would go through the motions, but I didn't study well.

So, the situation was that I advanced as a trombonist, and in music, very well. Incidentally, when I graduated from high school, the citation on my diploma was for excellence and a great future in music. Well, as I've said, I think this lack of eminence either in scholarship or sports worked on me to make me a little bit shy. I was not big, strong, or popular, or anything of this sort. But the trombone, and being able to play, first in Patrick Henry under Ralph Rush, who in my opinion was one of the finest musical leaders that Cleveland had and eventually became director of the Music Department at USC. He was very good to me, brought me along. And I became a first-class trombonist. In fact, I won the state championship in trombone solo twice—1935 and 1936—I'm jumping to high school now—and placed second in the nationals that same year. My point in this was that I feel that the music program in the public school was excellent in this. And I should say that in elementary school we had trips to hear the Cleveland Orchestra. Rudolf Ringwall was just very, very good. And the Metropolitan Opera loved to come there for two weeks, because it could go to a public auditorium, that great nightmare of a hall, which they could fill with thousands of people. [Laughter] I can remember making two or three trips there, both to the Metropolitan and the San Carlo Opera Company which played there too. So this was a very significant, very important part of my youth, because it brought me into a place where I had something that was my own. And I think it went a long way toward overcoming my tendency to hold back, to not take part in things. I always liked to play in the orchestra at a dance, but getting out to dance was a different matter. [Laughter]

I think I should say a little bit, too, about the neighborhood. A lot of people now don't realize—or maybe had never experienced—quite what these neighborhoods are. We were neighborly. We knew everybody. In severe times, like sickness, people would help each other. We weren't apart; we always knew everybody else's activities, or business. It bordered, of course, on being a little bit nosy about your neighbors. So we lived with them, we fought with them, we enjoyed each other. And of course, there was a group of children there that I was raised with, that I grew up with. And you would have a hard time now finding a neighborhood that had the variety of people in it that ours did. And I think, in a way that was very important to me, because even though I'm a white Protestant, I never grew up with the feelings that are attributed to people who have a background like that. I think the schools, the people, had a great deal to do with that.

Now, along with the music activity, which was my main activity all the way into high school and through high school, I began to show a significant ability in mathematics and in science—physics. Chemistry I never showed ability in, because it really wasn't until I came to Caltech and listened to Linus Pauling that I found that chemistry was not just a black art but had some fundamental principles associated with it. So, physics and mathematics in high school, although these courses were not the kind of thing students get now. I mean, at a good high school they get a very rich mathematical background in physics and chemistry. We didn't get that, but we got a significant amount, and the teachers were attentive. They recognized that I had some mathematical ability and some ability in physics. And they recognized that I didn't work hard—that I did it in spite of the fact that I didn't work very hard. Because the only thing I worked on, as I say, was music. By that time, I was writing a little bit of music, doing orchestration—and things of that type.

My last year in high school, I auditioned for a man, Mr. Gerhardt, who was bass trombonist with the Philadelphia orchestra and on the faculty of the Curtis Institute of Music. They invited me to come there, but they didn't offer to pay my room and board. So the financial situation in the family was such that I didn't go to Curtis Institute. This was'36, and that was not the best year.

But I had shown an interest in mathematics and science. And I can remember that you'd get on the streetcar on either St. Clair or Superior. And you'd ride downtown to the main library. And there was this wonderful library, where you could go around in the stacks, pick up books, look at them. I went there originally to get scores of the operas that were being broadcast on Saturday afternoons. And I would get these scores and take them home and follow the operas. I was enchanted by that. But at the same time, I went around and looked at books in various scientific fields.

Now, let me go back a little bit. Way back when I was in elementary school, for one reason or another, I became extremely interested in airplanes. The Cleveland Airport, which is now called the Hopkins Airport, was just beginning to function. And it was a long streetcar ride over there; it would take the better part of an hour and a half to get there. But the whole thing was informal. I could wander into the hangars, look at the airplanes, talk to the people who were working on them—something now that security wouldn't let you do at all. And in 1929, Cleveland hosted the National Air Races. And the National Air Races brought a bunch of somewhat insane racing pilots and their airplanes to Cleveland. And they used the part of the airport which is now the NASA Lewis Flight Propulsion Lab. They had a great show of airplanes, and I could climb around and see them; and there were these crazy people flying and the Thompson Trophy race on Labor Day. And all that really turned me on. That got my interest going in airplanes. Consequently, I got into the model-airplane game. My father helped me with that, because it had to do with building things with my hands, and working, and mechanical things. I got very avid about building model airplanes. I pursued that along with music, but I didn't see the relationship between that and academic work.

But to get back to the library, the reason I brought that up was my interest in airplanes and participating in contests, in which I was pretty good, really. I did reasonably well in the model-airplane contests held in connection with the National Air Races. I began to pick up books on the design of airplanes, aerodynamics, books on fluid mechanics which were way over my head. I can remember that so well, looking at these pictures, and just being charmed by the whole thing. So I began to look at these books, both in physics and in aeronautics, that were way over my head. I remember a partial differential written down there, and I had not the remotest idea what it was, but I was charmed by the symbols. [Laughter] That gradually led to an academic interest in the underlying sciences of flight.

COHEN: Was the word "aeronautics" used then?

MARBLE: "Aeronautics" was used. It was not a widely used term, but "aeronautical engineering" was a common term.

Then I graduated from high school; that was in May or June of 1936. It wasn't quite clear what I was going to do. Money was not easy to come by. I didn't exactly have an outstanding record in high school. It was out of the question for me to go to the music institute; it just wasn't reasonable, because they wouldn't pay for room and board. So it turned out that I had an aunt, my mother's sister Fern, who lived in Columbus, who was single and had a bit of money—not a great deal but enough that she suggested that maybe I ought to go to Case. Fern offered to pay at least part of my tuition to Case School of Applied Science [now Case-Western Reserve]. This didn't get settled in time for me to start college in the fall of '36. And as a consequence, I worked in an automobile shop until the first of the year, the beginning of 1937. And then started a term late at Case as a freshman; went all summer and made up that time. So by the fall of ’37, I was up with the sophomore class. That opened up an entirely new world to me. I was excited about everything, even mechanical drawing, design. At that time, the freshmen had a review class in trigonometry and algebra before they went on to analytic geometry and calculus. So I went through that, and I did reasonably well. For the first time in my life, I really worked.

At Case there was an aeronautics group—at least, a professor of aeronautics. His name was John R. Weske. So I thought, well, if I’m interested in this, I ought to go up and see Professor Weske, and see if he has a job for me, or something. I knew they had a wind tunnel; I knew they had a reasonably good lab. And I thought this would be fine.

Well, John turned out to be one of the most important people in my life. There have been several along the way, whom I’ll mention—aside from my parents. But John was an extraordinarily strong influence on me. His background is interesting. He came from Germany, was educated at the University of Hanover. He was fourteen years old when Germany was in World War I. And they took him out of his home, and because he could swim, the Army took him out and made him swim with ropes across rivers, so they could establish a bridge. And he said, “Oh, it was terribly cold.” And after they did that, they let him go back home, and dragged him out when they needed something else done. Well, he became a Quaker. What his religious background was before that, I don’t quite know. But he became a Quaker. And after he finished at the University of Hanover, the Quakers helped him emigrate to the US. He became a student at Harvard, got his doctorate at Harvard. His supervisor was a very famous man, Lionel Marks, who was famous as the author of a handbook, *Marks Standard Handbook for Mechanical Engineers*, which engineers now look down upon as the lower part of engineering. Anyway, John was his student, got his doctor of science there, and then came as professor of aeronautics to Case. And that’s where I got to know him.

I think his general nature, his easy way with people, his warmth, gave me a sort of confidence that I had extracted from my trombone before that. I was still not the best student in my class at Case, by any means. But John gave me the confidence to go ahead and work hard.

He gave me work in the lab. I did some very interesting things, and I did whatever John wanted me to do. I capitalized on the fact that I could do the metalworking shop and the wood shop, and I could do the kinds of things other students couldn’t do, simply because of my father’s instruction when I was a youngster. Then John used to loan me his books, which were probably for postgraduate students, not for me. But I would take them and read them and I'd work at them. And this contributed enormously to bringing me up to speed in the field of fluid mechanics, aeronautics, way beyond my years, way beyond where I should have been at that time. He knew it. I became arrogant as far as classroom work goes; I would always answer questions that other people couldn't answer. And John got sort of annoyed at me, because I would blurt the answer out when somebody else was fumbling around with it. But he brought me to that stage in a variety of ways.

Also, another man, Richard Burington, who was a professor of mathematics, took a liking to me when I was a sophomore and did an enormous amount of good for my mathematics. He steered me in the right ways; he gave me reading courses. He'd have me come to his office, even as an undergraduate.

COHEN: Were classes so small at Case that a professor could really pinpoint and help an undergraduate to this point?

MARBLE: I think our classes were not over twenty-two. We were split up into sections, and they tried to keep the sections as unified as they could. It didn't always work, particularly when it got into the later years and I elected to take things other students didn't. But certainly during the freshman, sophomore, and part of the junior year, we more or less were with the same group of twenty or twenty-four students, which constituted that section. And that was generally the class size. In some of the advanced mathematics, the classes might have been seven or eight. It's not unusual for them to be of that size here, either.

COHEN: Sounds like Case was a very good school.

MARBLE: Case was excellent at that time, I think, simply because at this time of recovery from the Depression years there wasn't as much outside influence to distract the faculty. The faculty didn't do consulting; they didn't go to meetings. There wasn't the kind of money around to do that. They stayed at home and worked.

Now, I should say that as a human being, Richard Burington was not admirable at all. I had never really in my life before known or met an anti-Semite. He was one of the most anti-

Semitic people I've ever known in my life. And the subject of [Theodore] von Kármán comes up here, because I early recognized, as I read these books, that von Kármán was just a genius in the field of fluid mechanics and aeronautics. And as far as the United States was concerned, he was the accessible one. Ludwig Prandtl was in Germany; G. I. Taylor was in England. But von Kármán was at Caltech. [Laughter]

Now I let this be known. And it turned out that Burington thought that von Kármán was a very important person, too. But he was absolutely sure he wasn't Jewish. [Laughter] That will sort of give you an idea.

COHEN: Did he just pull that out of nowhere? I mean, why should he tell that to you?

MARBLE: His conversation when we were together, particularly in these reading courses—he would never miss an opportunity to make an anti-Semitic comment. I know it seems ridiculous now. I would say he was an isolated person there. And it was rather interesting because one of his colleagues, Max Morris, with whom I studied too, was a Jew and a highly competent mathematician. And Burington would scarcely give him the time of day. [Laughter] Anyway, I wanted to make it clear that Burington, although he did an enormous amount for me, was character wise, maybe a good lesson for me too, because I got a good idea of what racial prejudice and anti-Semitism can be and can do. It was very clear at that time that it was a wrong thing.

Now, let me go back a little bit. There was a period in my life, when I was about thirteen or fourteen, when I was involved in music, I was involved in model-airplane work, I was involved in making things. And in all of these, in my own mind, I was way ahead of other people of my age. I said at that time to myself, "Frank, you're a little bit different from these other folks. Let's recognize that now, and keep that in mind, because later on, you'll want to look back at the point when you did recognize you were a little different." And that became clearer and clearer as I went through Case. I got my bachelor's from Case, not at the top of the class but close enough to be within a shot of it. So I'd come a long way. And that provided the confidence and the interest and the enthusiasm I needed to go on from that point.

There's one other man I should mention, because there's some implications with him here at Caltech. And that's Robert Shankland. He was a physicist, originally a student of

Dayton Miller. Now, Dayton Miller was professor emeritus of physics at Case. And he was the one who carried out an extensive set of measurements on the propagation of light through the so-called ether—ether drift experiments. There was a big shed out in front of Case, toward Euclid Avenue, a big round thing, which held the interferometer; they would measure the speed of light using the movement of the fringes to measure the lengths. And the apparatus was oriented in different ways, and the experiments appeared to show that there *was* an ether—that there was an absolute frame in which you measured the speed of light, which was quite in contrast to Einstein's ideas about this.

Now, Dayton Miller died in 1941, and this data had never been thoroughly analyzed. And it fell to Shankland to unravel all this and make clear why the data showed what it did, when, by that time, it was well established that the ether idea was wrong. I had very close contact with Robert Shankland during this time. He was very, very good to me when I was learning physics, getting advanced physics. He was a good friend of William Houston, who was the chairman of the Division of Physics here when I came, in '46. He introduced Houston's little book, the *Principles of Mathematical Physics*, at Case and brought it around when it was still in note form. And I got to have a look at those notes and work with them when I was a first-year graduate student at Case.

But back when I was finishing my bachelor's degree at Case, I wanted very much to go to Caltech. I graduated from Case in June of '40. But as it worked out, John Weske got me a summer job at Harvard. He and his wife and son were going back to Cambridge for the summer, and they enticed me to come along with them. They didn't have to try very hard. So I went to Cambridge, lived there—partly with them and partly in a little room of my own—and worked in the wind tunnel at Harvard for the summer.

Now, I became very, very fond of Cambridge. It was my first experience living alone, for one thing. I'd always lived at home even when I went to Case, because it was only about a two-mile walk from home to Case. But I became very attached to Harvard. I didn't have anything to do with MIT, for one reason or another. I was at Harvard, I worked there. I got to know many of the people there. It didn't shake my interest in going to Caltech, because the young professor of aeronautics there was a fellow named William Bollay, who had been one of von Kármán's students. He said, "Frank, you're a very bright boy, and you should go to Caltech. We'd love to have you here, but you should go out to Caltech and work with Kármán."

Well, history wasn't quite like that, because it was clear that the war was rearing its head. Eventually what I did was to go back to Case for my master's. It took two years. I had an assistantship, worked half-time, got my master's, and finished up there in the spring of '42.

December '41, of course, was Pearl Harbor. I was in Hudson, Ohio, with Dr. Weske at the time that that happened. And I can remember the feeling he had—that Germany after all was his home. His mother was there. And now this would certainly bring the United States into war with Germany. He didn't know quite what that was going to mean as far as he was concerned. Actually, it didn't mean very much, but he was a little concerned at that time.

So the war happened. And I didn't go to Caltech for that reason. Finished up at Case. And the war was sufficiently under way by the first of the year of 1942 that I decided this was no time to go on for my doctorate. So I didn't. I terminated it at that point. Fortunately for me, the National Advisory Committee for Aeronautics [NACA], which is now NASA, decided to establish another research center. They had two; they had the original Langley Memorial Lab, and the Ames Aeronautical Lab, in Sunnyvale, outside Palo Alto. But they decided to establish an engine and propulsion laboratory. And by my very good fate, Cleveland got that. The new lab was in the area where the old air races had been. So I went out there as soon as I heard they were hiring people. And I was "employee No. 67" for the NACA Engine Research Lab—ERL, that's what it was known as then. So I started there as soon as my classes were over at Case. I was there until the fall of 1946, when I came here to Caltech.

COHEN: How did the draft work for people like you?

MARBLE: That's a big story. I should go back. All this time, Ora Lee and I had known each other. We'd known each other since we were fairly young. Ora Lee is four years younger than I am. So when we were in high school, this appeared as a big age gap. I knew her through church, but sometime when she was about twelve, her family moved into the same apartment building we lived in. So we turned out to be neighbors. Well, because of the age difference, our contact was not what it might have been if we'd been closer in age. But when she finished high school in 1940, the same year I got my bachelor's degree at Case, she couldn't wait to get away from home. She was admitted to the nursing school at the county hospital in Cleveland, on the West Side, just on the other side of town. [Tape ends]

Begin Tape 1, Side 2

MARBLE: When Ora Lee finally went to the nursing school, and lived over there, it became very clear to me that I was interested in her. [Laughter] So we dated and got to know each other real well, and we were pretty serious about each other. The consequence of this was that after I had been at NACA for about year, we married. We married May 29, 1943, and that was a great day.

We had long wanted to go off on a honeymoon. And I'd worked so hard at the laboratory that my supervisor there—a man to whom I will come back later, another very important man—worked it out so I could get two weeks off, which was unheard of at that time. So we got into my 1936 Plymouth, with many thousands of miles on it, and drove to a place called Cook's Forest in Pennsylvania. It wasn't very far, maybe 120 miles. It was just right. It was a nice, wooded park. We could climb hills, scurry around in what they call mountains in that part of the country, and we had a marvelous time.

We arrived there on Sunday. The Monday of the following week, I got a telegram from my father, saying that my induction notice had arrived, which he proceeded to send to me. So the Tuesday of that week we cut our honeymoon short and went home. Now, there had been a policy of getting a deferment for each of the people that worked at the laboratory. In my case, this time, somebody had slipped up. The paperwork hadn't been done, and my name was on there to be inducted on June 16th. I went down on June 16th, had my physical examination, just as reluctant and worried as could be. Then later came the call to go off to boot camp. Now I'd continued to work at the lab, of course. But then the day I was supposed to report to go off to boot camp, a man named John Victory, who was the executive secretary of NACA, called me from Washington and said, "Don't go. Just stay in your house. Don't go out; pull your shades down. Stay there until this is all over with. We'll have it settled by tomorrow morning." Well, the head of the draft board fortunately didn't know where I lived, because I hadn't given them a change of address. They went to my old home, and my father was there. And the head of the draft board told my father, "Your son is breaking the law by not showing up, and he'll be dealt with in a severe way. Where can we find him?" And my father said, "I don't know where he is. And if I knew, I wouldn't tell you." [Laughter] Eventually, it all worked out, and I got a deferment. They came in three-month periods; I got a deferment for three months, and then another one for three months. And that lasted until the end of the war. But there was that little hitch along the way, where Ora Lee and I didn't know whether we were still going to be together or not. I sat the war out at the Engine Research Laboratory. I was in the Army Air Forces enlisted reserve, a private. That's what happened to me.

COHEN: Did you have to go for any kind of training, or was this just on paper?

MARBLE: It was just a paper thing. I had no active service at all. When I fill that out: "Are you a veteran?" "Yes." "Discharge?" "Honorable." "Time of active service." "Zero." [Laughter]

COHEN: Were you eligible for any of the benefits, then, after the war?

MARBLE: No, none.

COHEN: It wasn't considered national service that you were working for a government lab?

MARBLE: It was. It was considered important, but no more important than a manufacturing industry. The various big companies in Cleveland—Warner and Swazey, which was making machine tools for production—organizations of that sort had an equal right to defer folks. So NACA was treated just like an industry. And each individual case had to be made. It wasn't because we were civil servants that we had an automatic deferment, or anything of that sort.

COHEN: And did Ora Lee continue her education?

MARBLE: No. I had finished my master's. And Ora Lee decided that when we were married—she had essentially finished her nursing work—she decided, well, she wouldn't practice nursing, although she did work in doctor's office for a while. And I said, "Look, we're making enough money now. My salary's enough. You ought to go to college." We had moved to the West Side. We were living probably around West 100th Street, something of this sort. It was called Andrews Avenue. A very nice little apartment there. So she enrolled in Flora Stone Mather College of Western Reserve University. That meant, at that time, a big trip every day on the streetcar. And she did her first two years there. But she didn't complete her education, actually, until we came out here. I rode a bike to Caltech, and she took the old car and went over to

UCLA every day—always on the other side of town. [Laughter] But she finished her education there. She finished in 1947.

COHEN: What sort of thing did you work on at the NACA Lab?

MARBLE: That's an extremely interesting part of my life. And I want to expand on the lab a bit, because it had as large an influence, I think, as John Weske did.

I got there, clearly, at a time when the laboratory was growing. I clearly had certain capabilities that other people they hired didn't. The result of that was that I got responsibilities and possibilities which would never have happened at any other time but wartime, and I got responsibilities of unusual magnitude. I was put in charge of a large engine-research project, a cooling project for the B-26 bomber. Now, the B-26 was called the Martin Marauder, and it had certain cooling problems. And I was asked to do experiments on that and find out what it was, suggest modifications to the engine company, which was Pratt & Whitney. We did ground tests—wind-tunnel tests—and we did flight tests. We flew the airplane, which was dangerous enough. So I had the opportunity to do that.

It was an interesting issue. It was clear, even at the time I went there, that the field of aircraft propulsion was in the process of changing. Jet propulsion was a recognized possibility. It wasn't efficient; it wasn't serviceable. But it was clearly coming. The question was, how soon?

Well, the man after whom the laboratory is now named, ironically, [George W.] Lewis, came to the laboratory and gave a speech to all of us. He got very irate about all of this, because he knew that we were sneakily, on the side, doing some work on jet propulsion and rockets and whatnot. He said that there would be "absolutely no work on jet propulsion carried out at this laboratory. Reciprocating engines are going to win the war, and we're going to confine our activities to reciprocating engines." Period. That was it. And I always found it ironic that the laboratory that became the center of the jet-propulsion activity for NACA (and later, NASA) should be named after a man who forbade us to do anything in jet propulsion. [Laughter] He certainly was a very important person, as far as NACA was concerned.

COHEN: Was he at the lab?

MARBLE: No, he was part of the headquarters office, in Washington. Victory and Lewis were probably the most influential people in what we now know as the Washington office of NASA. And he came and really dressed us down on that.

Now, because of the growth of the laboratory, and because of the fact that I was very fortunate and got very good pieces of work to do, and was fortunate enough to do them well, I gradually went up the ladder, and I became a section chief after a year, which meant I had some ten or twelve people under me. And then, my last year there, I was the branch chief. At that time, the war was over; we had reorganized. Jet propulsion was in. And I was made the chief of the fundamental turbine-compressor research branch there. The next level up was division chief; they were essentially the ones that ran the lab. So I was promoted rapidly, and because other people came in later, they were naturally under me. So that's the way it worked out.

I think probably the most interesting thing, the most striking thing I did there was on the Boeing B-29. Now the Boeing B-29 was the bomber that was destined to do the bombing of the islands in the Far East. Everything, of course, was hastened to get done at that time. The airplane was brought out early. But the engine—the R-3350 engine, which was built by the Wright Aeronautical Corporation—was in a less than perfectly developed state at the time we were going to fly it. It had a cooling problem. As a consequence, when they tried to ferry the B-29s over the ocean to the Far East, they would lose a significant fraction of them. Either the planes would have to come back to the airport they had started from, or they would have to be ditched in the ocean, so they were lost.

I was put in charge of the cooling problem on that engine. Later, when the B-29 flight tests started, there were two people brought up from Langley. One was Abe Silverstein, who was an utter joy to work with. He later became director of the lab—a marvelous guy, full of spirit, full of ideas, full of Abe Silverstein. [Laughter] The other man was Eastman Jacobs. He had a black goatee, and he looked for all the world like he was trying to imitate Mephistopheles. He was an aerodynamicist of some knowledge. He was very, very good; opinionated, also. He and Abe would fight a great deal, and I would get the work done; that's the way it worked out. The way it went, we developed some new components, a new place to inject fuel, new baffles for cooling the engine, and a new schedule of cowl flap openings for the airplane. And the result was that the problem was essentially cured. I shouldn't take all the credit for that for our lab, because parallel work, based somewhat on what we found, was done at Wright Aeronautical

Corporation, and work on the aerodynamics of it was done at Boeing. So between the three of us, we contributed to solving that, and got the B-29 to work right. That was a real accomplishment. I felt at that time that I had done something.

COHEN: How long a period did you work on that?

MARBLE: About nine months. We were certainly finished with it by the fall of ’43; everything was wrapped up by that time. In those days, we worked every day of the week. That was just routine. They didn’t pay us for it, but they gave us uncompensated overtime. What they did was to give you leave. You could take it in leave sometime. And it turned out that I used all the leave after I came to Caltech. [Laughter] I was on leave from the Lewis Laboratory for six months, which helped a lot.

We worked long days, weekends. And I can remember one summer Sunday afternoon—it must have been the late summer of ’43—I got a call from the chief mechanic in the flight laboratory out at the airport. He said, “Frank, you’ve got to come out here. Jake [Eastman Jacobs] is dead.” I said, “No! What happened?” He said, “He’s just out, lying under the airplane, dead. He must have had a heart attack. Come out, please. I don’t want to have anything to do with it.” So I came out there. And here was Jacobs, lying down under the airplane, under the wing, right under the engines. Well, he was a very intuitive sort of fellow. He felt that by looking at the problem, you begin to understand it. And he’d crawled under there. And in the summer heat, he’d fallen asleep. [Laughter] And I called him, “Jake,” and I rattled his foot. He woke up. [Laughter] But I think he was embarrassed by it, and he never wanted me to tell anybody about his falling asleep under the airplane. After the war, he left NACA and came out and bought a ranch up the coast here.

I should say a little bit at this time about another man very important to my life—Oscar Schey. His reputation at Langley Field was as a rough man, difficult, a fighter. Then he was transferred up to Cleveland, and he became my division chief. He was chief originally of superchargers. And then, as he came up, he was put in charge of heat transfer, compressors, and turbines, and such. So I worked for Oscar. Well, Oscar liked to get work done. He hated things that didn’t get done. And he found out that I got them done, so he became a great fan of mine. He helped me a great deal. He was the man who arranged for the two weeks off for our honeymoon. So everything went along just great between us. He would confide in me and take my advice. He promoted me, probably in a little preferential way, maybe because he thought I'd do the job better. Anyway, he did. We remained friends. He died in '93. He stayed in Cleveland up until about four years ago, and Ora Lee and I would stop around to see him. Gradually, his eyes got bad, but he recognized us, and we had our chats about the old times at NACA. He was a very important influence on my life—because in a civil-service organization like that you could get lost. You can get into a position where nobody knows you're there, nobody cares. But Oscar saw to it that that didn't happen to me, that I had what I needed to get things done.

So in spite of the fact that those years delayed my going back and getting my PhD, they colored the rest of my life. They colored it in the sense that I changed from aeronautics, as such, to gas turbines, propulsion, rocketry—that became the thing in which I had great expertise. And that served me well when I came here, both at the Jet Propulsion Lab and at Caltech itself.

I should say that my father died rather suddenly and unexpectedly in the spring of 1944. And that was a considerable blow to me.

My mother was alone. She was working. She was marvelous; she just kept on working. I think that's where I got the urge to keep on working after retirement. She kept working and took care of herself, and maintained the little place she lived in. Eventually, she worked for Western Reserve University, in the cafeteria in their downtown branch—the branch that used to be called Fenn College.

Then, when it became clear—I guess it was in the summer of '45—that the war was over, the National Research Council came out with the predoctoral fellowships. And I applied for an NRC predoctoral fellowship, and I got one, which had to do with people like Weske and Oscar Schey and Richard Burington and Bob Shankland and all these guys writing nice letters for me. So I got this, and I left Cleveland in August of 1946.

COHEN: Now, did you come out to Caltech before then, to visit?

MARBLE: No. People didn't travel that much at that time. Also, I don't remember when I got discharged from the military, but it must have been early '46. And in those times, you just didn't travel; travel was hard. That, plus the money and everything, we didn't do it. But I did correspond with Caltech. I had a lengthy correspondence with both Clark Millikan, who was sort of running things in the Guggenheim Aeronautical Lab then, since Kármán was away, and with William Houston, who was the division chairman of physics.

Now, my wish was to do my PhD in physics and mathematics. I thought I knew as much about aeronautics as I need know. I was in pretty good shape on that. But I saw physics and chemistry as being the sciences that would mold a new aerodynamics, fluid mechanics, jet propulsion. And, of course, nuclear processes, nuclear energy, nuclear physics was vital at that time. And I wanted to get in on that.

I had my application, so it could be either aeronautics or physics. And I got a marvelous letter from Bill Houston saying that he was perfectly willing to admit me in physics and that I could take all the physics I wanted. But he thought it would be perhaps more efficient for me to stay in aeronautics. So I did; I registered and came here as a student in aeronautics.

That was settled somewhere along February, March of ’46. Ora Lee was not excited about coming out here. She knew I had an equal shot at going to Harvard. I applied to them both, and they were both possible. But I really wanted to go where Kármán was, and Ora Lee gradually relented. And we made a pact then. We said we’d go out there for two years. I’d work hard, get my PhD over with, and we’d come back to Cleveland after that. That was the way it was.

Well, my correspondents had indicated that housing was very difficult to come by, and very expensive. So I did probably the next most idiotic thing; we bought a house trailer. It was a modest size, modest length, modest weight. We started out from Cleveland in the middle of August. Well, I had had great visions of this being a marvelous trip, until we went over to pick the house trailer up. They put the hitch on the car; they put the trailer on it, and the car went down at the rear end. I was going to take it over to my parents’ home on the East Side. So I started out. Cleveland is very flat, except when you’re driving a house trailer, and then minor hills become major hills. And I found myself going up little grades, shifting from drive to second, down to first. Here I was, with no hill at all, in first gear! And my Plymouth was now ten years old; and it had a little over 100,000 miles on it.

I got a man who was a specialist in transmissions on Plymouths. And I said, “Tell me, how long can I pull full power in low gear with this car?” He said, “Oh, about an hour and a half.” [Laughter] I said, “It better be a short trip.”

Well, we made it. But it was an unbelievable experience. We blew out tires. We almost had wheels fall off. We got stuck on mountain roads. But it was an experience we'll never forget.

COHEN: Not one to repeat.

MARBLE: We wouldn't repeat it, but we're so thankful we did it. We look back on it with a mixture of fright and very fond memories.

COHEN: How long did it take?

MARBLE: A little over two weeks. But we took our time along the way. By the time we got into New Mexico and Arizona, we'd stop and park the house trailer and drive off to see the Carlsbad Caverns.

COHEN: You picked the flattest route, of course.

MARBLE: We picked the flattest route. But when you get out here, it's not that flat.

We eventually got here. We parked the trailer, interestingly enough, on Foothill, out where the Avon plant is now, almost to Rosemead. At that time, there were only three things there. There was this trailer park; there was the fire station, which is there now, on Halstead, and we were just to the west of Halstead; and then there was the Seafood Tavern, down on the corner of Rosemead and Foothill, which was a marvelous place, and we enjoyed that a great deal.

COHEN: Before you go further, is there anything else you want to say about the Lewis Laboratory?

MARBLE: Well, I think it was a time when I made a group of very lasting friendships. These are people that, by virtue of the position I had, either as their section chief or their branch chief, I was able to help in a technical way, help them technically and administratively. And they became very loyal friends. One or two of them went off to industry, and I've followed them all the way through their industrial careers. Others waited a couple of years until I was on the faculty here, then decided they'd come to Caltech to get a PhD. And they became some of my students. [Laughter] That was an interesting consequence of it.

And all this time, I kept close association with John Weske. His nature, his manner of life, his Quakerism—all this had a very big influence on both Ora Lee and me. And I'm happy to say that he's alive now still. He lives in a place called Sandy Spring, Maryland, which is a little Quaker community. And we see him whenever we go to Washington.

I don't have anything else to say about the Engine Research Lab, except maybe one important thing. In the wartime, there was an urgency, a sacrifice among the people there. They would work; they'd do something. They were interested in getting the job done, not so much in jockeying for a position in the organization where they could relax for the rest of their lives. It was the spirit of the war. And you know, this was my first real job. So I didn't see that as an unusual thing in a civil-service organization. The minute the war was over, and we reorganized as a jet-propulsion organization, everybody changed. The whole thing was different. Then it was: "Find me a position. I want a position where I can spend the rest of my career peacefully, doing what I want to do. Let's not cooperate. Let's not work overtime. Let's not work too hard." The whole attitude changed. People developed conflicts who'd never had them before. Never, during those years when I was there, did these people argue or fight. But the urge to cooperate, the urge to work together, vanished. And these petty things came to the surface. And I'll say that it developed the characteristic ailments of a civil- service organization, which continue to this day.

FRANK E. MARBLE

SESSION 2

February 9, 1994

Begin Tape 2, Side 1

COHEN: I think we should pick up this interview with you arriving in Pasadena. Tell us something about what happened then, and what year that was.

MARBLE: Well, the war had ended. I had received, fortunately, a National Research Council pre-doctoral fellowship. After my wife and I discussed where we would go, we decided we would come to California for two years and see what happened after that. I think I discussed last time our trip out here in the house trailer and the various adventures we had.

We got settled down in our house trailer, which was parked on Foothill, right near the fire station by Halstead and Foothill. Across the way, where all the building is now, were open fields. The first building you could see was the Passionist Fathers monastery, up in Sierra Madre; that was the only building between us and the mountains.

Ora Lee went to UCLA to finish her education. Consequently, she became the owner of the automobile. I bought a bike and pedaled my way into Caltech every day.

Caltech turned out to be everything I had hoped for. I had become quite relaxed and moved toward my scientific interest during the drive out. I got myself adjusted to that. But I can remember the first day I walked into the Guggenheim Aeronautical Lab, and I was told to go see Ernest Sechler. Ernie Sechler was professor of structures and elasticity. He was handling the incoming students. I remember meeting there a young lady named Geraldine Ellis—that was her name at that time, before she was married. She was so sweet and so nice, I felt at home immediately. She was Ernie's secretary. She had actually written me to tell me where I might park the house trailer, because I had said I needed help and I didn't know anything about the area. And she was very good. We did stay at one of the trailer parks she had mentioned to us. She made me feel perfectly at home. And everything worked out fine.

I was told by her to go see the chairman of the Division of Engineering. I remembered in my letter from the National Research Council, they said, "Make an appointment and go see Professor Franklin Thomas, who is chairman of the Division of Engineering." Well, I looked up

Franklin Thomas, and he was very cordial, very pleasant—inquired all about our trip and everything else. And then he said, "But you don't want to see me. I'm not the chairman anymore. Fred Lindvall is the chairman now." And he had a little office in Throop Hall. I wandered over there, and had a pleasant chat with Fred.

The question was then going back and getting acquainted in the Guggenheim Aeronautical Lab. I first wandered around and found myself a place to sit, took the books I had brought from Cleveland, and put them in a long, thin office on the second floor, which was occupied by a young lady named Alberta Panpeyan. She was the person in charge of the Durand reprinting group. There had been a series of books published in the mid-1930s and edited by Professor William Frederick Durand, called *Aerodynamic Theory*. I was pretty well acquainted with them, and I was very impressed that she was handling the reprinting of these and selling of them out of that office. So I was offered a desk there. I moved in. There was one other desk, which was occupied by another student, Harold Martin, who'd been there for some time. So for the next year, the three of us shared that office.

Now, it happened that that office was right across the hall from Dr. Clark Millikan, who, at that time, because of von Kármán's absence from the institute, was acting as the head of aeronautics. I don't think he was called the acting director, but he was certainly acting as the director. All the decisions as to finances, students, who went where, and whatnot, Clark Millikan took care of—and I should say, most effectively.

I had planned, or hoped, of course, to see von Kármán. But it became obvious, even before I was there, that he was not spending that much time at the institute. He was still involved in his wartime activity in Washington. But he did stop back at the institute from time to time, and I'll get to that later.

The one other person I really wanted to make contact with and try to do some work with was Hans Liepmann. Now really, there wasn't that much difference in Liepmann's and my age. I think he's about five years older than I am—four years, perhaps, no more than that. But he had come through the European system, of course, and came over as a research fellow to work under Kármán. The consequence is that the fact that I was off involved in war work from '42 to '46 meant that he was more advanced in his career. So, in a sense, he had about ten years effective seniority on me. I found Liepmann utterly charming, enthusiastic. He welcomed me with such warmth that I really felt completely at home there. In fact, my whole impression of

Guggenheim—and, consequently, the institute—was one of a great collection of scholarly folks interested in many topics and also taking a great personal interest in students. And warmth, I felt that very, very much—in contrast, I think, to what I had felt at NACA.

COHEN: Now how many graduate students would there have been?

MARBLE: Oh, I think at that time the total complement of graduate students in Guggenheim was probably not more than twenty-five. They included people who had had a couple of years of graduate work and those who were just starting off. So I think there was a spread there.

The war had actually done some interesting things. There were several people who came there after the war who had had a similar experience to mine. Just two come to mind—Milton Van Dyke and Dean Chapman, who were at NACA's Ames Laboratory during the war. They came back to Caltech to pursue their PhDs after that. These two were students who were perhaps not as old as I was, but who had spent part of their career at NACA. Most of the people I knew were from the Ames Laboratory. And we had a little bit of special treatment, because I think the faculty knew we were more advanced, more experienced, than a "green" student who had just finished his bachelor's. So they treated us with a little more thought, a little more concern.

COHEN: You may have been equivalent to what's now a postdoc student.

MARBLE: Well, not that far. I had done two graduate years, and I had had a lot of experience during the war—some of it relevant. I had taught every year during the war, part-time at Case and part-time at classes they had at the NACA laboratory. So I was somewhat experienced as far as the lecturing and the teaching was concerned.

But I want to go back to Hans Liepmann. He was trying to re-form his research group after the war. He had got acquainted with a Swedish mathematician, Paco Lagerstrom. And he and Paco, and Anatol Roshko, Julian Cole, and I were sort of the core of the group that Liepmann worked with at that time. Now, I think everyone knows that Liepmann has great accomplishments as far as scientific and technological work goes. But I think, when all is said and done, his greatest contribution to Caltech and to the scientific area was the enthusiasm he brought to his work, the confidence he gave to younger folks—convincing them how important they were and what their work meant in the context of the aeronautical world. And it's hard to overstate this. It was a very, very important point of view that he had. He kept us stirred up. We spent evenings together, weekends together. And in a way, he and his first wife, Katya, established sort of a family of us students. That is, unfortunately, not quite the spirit at the present time. But at that time it was very important, and for Ora Lee and me—because it included wives and girlfriends—it made us feel we had found a home here. I mean, this was not something we were going to stick our toes in and get a PhD and run back to the East. Because even though we came here—and Ora Lee particularly—with great concern about whether we should or shouldn't stay, after six months the issue never came up again. [Laughter] So we never even discussed the possibility of going back.

As far as the work goes, I had a topic, I had a piece of work I had started to do—or had at least conceived—while I was at NACA. And in the last six or eight months there I was pretty much involved in the administrative activity of reorganizing the lab, and I had sort of laid the work aside. One of the joys of coming to Caltech was that I could pick that up again and begin to complete it. Without going into details, it was one of the first pieces of work in a very important area of the gas-turbine engine. And I wanted very much to complete that.

Now, that work was completely foreign to Liepmann. He had no more idea of it, and only casual interest. His interest was simply that it be a good piece of work. So I pursued that as my PhD thesis, and continued to work on it and write it.

COHEN: So you didn't really have much scientific advice from people?

MARBLE: Well, now, the other thing was that Liepmann was very active in two fields—transonic flow and turbulence, both of which I was very much interested in. So I carried that on in parallel. I worked with him, particularly on the transonic-flow work, and contributed quite a few things to that during my first two years here. But that was never part of my thesis. My thesis consisted of this other piece of work.

And that's sort of interesting, because there were people at the institute who knew a little bit about it. And in that connection I should mention two—Duncan Rannie, and the other one is Homer Joe Stewart. Duncan Rannie died some years back. But Homer Joe Stewart is still active and healthy, and we see each other from time to time.

Clark Millikan just wanted to make sure that as a newcomer I was aimed in the right way. So he scheduled me to give a seminar on it, which I did. And I think the general feeling amongst those who understood the issues was that it was an excellent piece of work and I should go ahead and finish that for my PhD.

Regarding Duncan Rannie: in the spring of 1946, after I'd gotten the NRC pre-doctoral fellowship, I had written to Clark Millikan and said there was no reason that I saw why I couldn't leave Cleveland in June and spend the summer at Caltech getting my research started and doing anything I needed to do there. Well, he turned this letter over to Duncan Rannie. And Duncan Rannie had an interesting background of his own, but I won't go into that. He was at that time chief of the ramjet research section of JPL. He had spent part of the war years at the Northrop Aircraft Company—at the Turbodyne Division of Northrop, which was trying to build an air-breathing, gas-turbine jet engine. So he had that interest. And he was trying to get a program in turbo machinery research work started in mechanical engineering, on campus here.

Eventually, he was appointed an assistant professor of mechanical engineering. But that was after I got here; that appointment had not been consummated at the time I wrote him. He wrote me, but unfortunately Duncan didn't always answer his mail soon after it came; he got this from Millikan in the middle of June and he answered it in about the early part of August. And I had to write back that by then it was probably not worth my while to make a special trip there. I'd just wait and come for the start of the fall term, which I did do.

Well, Duncan was one of the people who was asked to make some judgment of my work. It turned out that he had been interested in this problem also, and had made some partial efforts at it. But I had gotten a lot farther along than he had. And I think in a way he was a little bit uneasy about this student who came in with a background in this field and was sort of plodding over territory he had cut out for himself. We eventually took care of that. But he was not any technical help to me.

Homer Joe Stewart, on the other hand, is one of the brightest people I've ever known. He's very, very clever. I'd known of him, but I'd never met him. When I explained to him what I was doing, he understood it immediately, thought it was a fine idea, a great piece of work, and encouraged Clark Millikan to help it go on. And Clark actually went to NACA, in Washington, and got money to support the computations that I needed done, which at that time were more of a chore than they are now, because they all had to be either hand done or done on one of these cranky calculators. So the $5000 he got to help that along was a great asset.

COHEN: Did people actually do these calculations?

MARBLE: What happened at that time was that the 10-foot wind tunnel had a group that reduced the data. They were competent at computational work. It was right after the war, and they weren't doing really as many tests as they ought to. So they were very happy to use the money up and do all the calculations for me—which they did do—and it helped a great deal.

Liepmann and Millikan both used me to teach sometimes in the courses when they were gone. I remember teaching, for about a month or six weeks, the first portion of a gas dynamics course that Liepmann was supposed to teach. He had to go off to Europe. So I taught that. I remember with great joy now that one of the people who came in to audit the class was Jesse Greenstein, who had just come to Caltech. He introduced himself and wondered if I would mind his sitting in on the course. And I said, "Not only am I honored, but I'm frightened." [Laughter] But he was very cordial always about that.

COHEN: What was the name of the course?

MARBLE: It was called "The Theory of Compressible Fluids." Liepmann had written a book that partially covered the course. Revisions of that book are now the Liepmann and Roshko book [*Elements of Gasdynamics*]. But at that time, the course was not in book form, it was mostly in note form. And I knew about the subject, so I could sort of improvise along the way, and I think that went over well, too.

So I did have my course work. I did my oral examinations, got my thesis done, and completed my doctorate in time to get my degree in June of '48. Two years. That was sort of the minimum, but I had had such a thorough background that they felt that taking longer would not help me a great deal.

During that time, of course, as I mentioned earlier, I had been undecided, or I had thought I wanted to do graduate work in physics and get my doctorate in physics. And William Houston, who was chairman of physics at that time, had said that with my background it would be more expeditious for me to get it in aeronautics, but I could take as many physics courses as I wanted to, and that would be all right. And that's what I did. I actually took more physics and mathematics than aero, because I had experienced much of the aero things. And in the process, I got to be good friends with Willy Fowler, who's from Ohio too. And then in mathematics, Morgan Ward and I had a very good relationship. I can remember the first examination I took under Morgan Ward, in the fall of '46. It was his "Mathematical Analysis" course, which was Math 114 at that time. It was a course he had inherited from Harry Bateman, who died unexpectedly in January or February of 1946. It was a course given in the English style, with lots of problems—difficult problems to work, difficult mathematical-analysis problems to work. I remember going into the first final examination, which was in early December of 1946. I looked at the examination, I started at the top, and started to work my way down. I wrote furiously. And by the end of the three hours I had finished about two and three quarters of the problems, of which there were eight. I remember pedaling back home and telling Ora Lee, "Pack up your things; I'm going back to Cleveland. I've blown this completely." I was very nervous and upset about this, because I had thought I was competent in mathematics, perhaps even bright in it, and experienced enough to be able to work these problems rapidly.

So after a couple of days, I stopped around and hesitantly knocked on Morgan Ward's door and introduced myself, reminded him who I was. I said, "I was worried about my final examination." And he said, "Oh, Marble, you did a beautiful job!" I said, "But I only did two and three quarter problems." He said, "Most people didn't even get one." [Laughter] And that, apparently, was the sort of attitude that Harry Bateman had set. You just give people a lot of problems and see what they can do—if they can do any ones they want to. If I'd known I'd had that option, I wouldn't have started from the top of the exam; I would have picked specific ones out. Anyway, that cemented the friendship with Morgan Ward.

During my second graduate year, Morgan told me that there was a mathematician from Edinburgh coming. That was Arthur Erdélyi. Erdélyi was a Hungarian refugee, whom the mathematics people at Edinburgh had rescued from Europe to come to the Mathematics Department in Edinburgh. He spent the war years there and working in London.

Erdélyi and his wife were just the most marvelous people! We enjoyed them thoroughly. We developed a relationship with them that was of the same order as that we developed with the Liepmanns. We'd go camping with them on weekends, and spend holidays with them, and go to concerts. That was a friendship that always meant a great deal to me. Erdélyi came here on a project, which was that he was supposed to go over the voluminous files that Harry Bateman had. Bateman was known as a real scholar, and he had shoeboxes full of large cards, on which he had written all kinds of things—special functions, integrals, and whatnot—that he had worked on. And this was supposed to form the basis for a great book. Harry Bateman had written two or three books at that time, and this was supposed to be the culmination of them, or an update of a book on partial differential equations that he had written.

So Arthur Erdélyi was asked to come and look in detail at Bateman's notes. And after about six months of study, his conclusion was that there was nothing to be done with them, but that maybe what should be done was to make a contemporary compilation of transcendental functions and transformation theory. And that he did. And these came out as the Bateman Roman Manuscript Project. Professor Erdélyi hired three other European mathematicians, Magnus, Obermettinger, and Tricomi. The navy—the Office of Naval Research—supported the work and quite a few students. And they compiled three books, which now exist as highly prized reference books in transcendental functions, integral transforms, and things of that sort.

COHEN: And all these people who worked on it, they would be in the book?

MARBLE: They are there, and all of them are listed as contributors. So that was how he came here. And it was through that that I got acquainted with him. He was a very important factor in my life also.

COHEN: Did he stay on after?

MARBLE: Yes, he did. He stayed until the position of the chairmanship of the Department of Mathematics at Edinburgh was open. He had been a protégé, in a way, of Professor E. T. Whittaker, of Whittaker and Watson fame, who was chairman of the department at Edinburgh. And there was another gentleman in field of number theory, who followed Whittaker. And under him the department—they'd had a marvelous history—sort of dried up a bit. It didn't thrive. And when this replacement had to step down, due to physical ailments, Arthur felt morally obligated to leave here and go back to Edinburgh and assume the chairmanship of that department, which he did; and stayed there, retired, and he passed away in Scotland about ten years ago.

That was the situation here, roughly, at the time I finished my PhD. I had traveled around in the spring quite a bit to see where I should go. It just turned out that that wasn't quite what the aero department had in mind. They needed someone to teach—particularly the applied-mathematics courses they had, which were under the aero name at that time. Those courses have since moved into applied mathematics or applied mechanics. But they needed somebody to teach that. And Clark Millikan persuaded me to stay on as an instructor in aeronautics. So in June of 1948, I signed up as an instructor for a year in aeronautics.

The situation I was in then was a little different than it had been. My feeling was that, here I was, I was going to be a faculty member someplace or other. And consequently I should move out on my own, instead of being occupied with other people's research activities, as I had with Liepmann. I should probably move out into interests of my own. And I did that. And I say this because it had a big influence on that year.

I think that led to a difference of opinion between Liepmann and myself. Because by the end of 1948—I was an instructor for '48-'49—at the end of '48, it was quite clear that there had been a definite cooling off in the great personal warmth between the Liepmanns and the Marbles. So Ora Lee and I decided, well, we're probably not going to stay here, then. The situation had changed a little bit, and we felt it quite thoroughly. So we decided we'd pick up in the spring of 1949; I'd find myself a job and we'd move.

Now, before this—I would say it was probably in the fall of 1946 or the early spring of 1947—von Kármán had come back to Caltech, and had moved back into his office, and was "holding court" with people who were allowed to come see him. Liepmann had arranged for me to see him. I took thorough advantage of it, and had a great time. I can remember showing von Kármán some of my work. And he said to me, "Young man, are you a mathematician?" I said, "No, Professor von Kármán, I'm an engineer. But I like mathematics." He says, "Oh, good."

Well, I didn't know it at the time, but mathematicians usually occupied a level lower than engineers in von Kármán's mind. He thought they were not aware of what the real physical issues or physical problems were. So he was a little bit afraid that I was too attracted to mathematics. Well, if I had been, that certainly changed my attitude—that reaction of Kármán's. [Laughter] But that was the start of a very close association and friendship with Kármán. Every time he came back, he knew he could turn things over to me if he wanted them done; they would get done. He showed me a piece of work he'd done that had to do with turbulence. And he said, "I would like you to check this over, and maybe see if you can solve this or that." Which I did; I took the thing home and worked on it and brought it back in a couple of days, and he was enchanted. And that led to von Kármán's great warm heart embracing Ora Lee and me both. So that sort of compensated for the other thing that happened.

COHEN: Did this have anything to do with Liepmann not being so friendly?

MARBLE: That's a good question to ask. There was a little bit of a block between the Liepmanns and Kármán and his sister [Josephine (Pipö) de Kármán]. I would say that his relationship with Liepmann and his relationship with Bill Sears were like day and night. Sears was welcome, open to everything. Sears was here before me. He got his doctorate in 1938, went to Northrop during the war, and then founded the Cornell School of Aeronautical Engineering in 1946, the same time I came here. I had met him, but that's all.

Anyway, that was a very close relationship, between Kármán and Bill Sears. But I felt that Kármán's relationship with Liepmann was not that warm. Whether it was Liepmann's wife, whether it was Kármán's sister, I have no clear idea. Pipö herself had a very dim view of Germans—so much so that she stopped calling herself "von Kármán" and called herself "de Kármán." So which of these it was, I don't know. But the relationship was a strained one. It wasn't open and warm. And Liepmann felt that. He felt he was outside the Kármán group. And he felt I was in, or close to it. And I think that may have contributed somewhat. It may have been that he felt some shifting loyalty, I don't know. But whatever it was, the warmth cooled. And in spite of the Kármán presence, I knew he was not at Caltech enough to make it interesting here. So I decided to take a long trip of interviews, in the spring of 1949.

Now, in the meantime, Kármán did one of his usual wonderful things. He had been working on a turbulence problem, and there was to be, in Paris in August of 1949, the first International Congress of Astrophysics and Gas Dynamics. And Kármán was, of course, going to be there. He was going to give one of the lectures. And about two weeks or ten days before the meeting, I got this cable from Washington, from the Air Force, and it said that travel orders were being cut for me to go under the sponsorship of the Air Force to this congress in Paris.

And I would report to Professor von Kármán. So he had asked them to do it. He hadn't mentioned it to me, but he asked the Air Force. So I got this sheaf of orders, and I was supposed to fly on military air transport. And I went.

Now there's an interesting point here, which I should probably make. I had to get a passport in a hurry. So they said, "Well, through the State Department it's possible to work these things, and the military can do it pretty rapidly." So they did, they got it for me. They had to go back to my birth certificate, in Cleveland. Now, it turns out that when I was born, I was not Frank Earl Marble. I was Henry Rawling Gilbert Marble. Now, the reason for that is that my mother—I think because her estranged father's name was Frank—took a very dim view of Frank as a name, although my grandfather—my father's father—was Frank Earl Marble, and my father favored that name. And the cousins of the Marble family were Gilberts. Henry Rawling Gilbert was one of the prominent members, so they had decided to name me Henry Rawling Gilbert Marble. My God, what a name! Apparently, after about three days, even my mother decided that that was nothing to call a child. I guess they decided I was going to live, so they might as well look for a better name. So they went back to my grandfather's name, Frank Earl Marble, and it was changed at that time. However, on my passport—I picked it up in Washington, on my way to the military transport—it said, "Henry Rawling Gilbert Marble, also known as Frank E. Marble." [Laughter]

Anyway, this trip went off, and I don't think I've ever had a more illuminating experience than my first trip to Europe. The war was still evident. The destruction and the problems in Paris itself were evident. My pleasure at finding my way around there was just something memorable.

And then Kármán decided that since I had to fly out of Frankfurt, I should travel with him by auto to Heidelberg, because he was driving to Heidelberg. So we took a marvelous trip. Oh, I'm sorry, not first. The first leg of the trip was to Switzerland, to Bern. He was at that time attempting to extract his brother from Hungary, which was still an iffy thing, because it was under Soviet domination. He had hired a lawyer to take care of this, and the lawyer worked out of this Swiss area. So he went to do that, and I stayed at a hotel, and then we met after that meeting and went on to Heidelberg.

I should mention that the lawyer he had—he introduced me to him—his name was Moe Berg. Now, if you're a baseball fan, you know the name "Moe Berg." He was a catcher for the

Cleveland Indians.

COHEN: It wasn't this Moe Berg?

MARBLE: It was this Moe Berg. I had heard or seen a little note in the paper that even though Moe Berg wasn't the greatest catcher in the American League, he was a lawyer. And evidently he was a very brilliant lawyer, and he had a great facility with languages. He spoke all languages that he came in touch with easily. And of course Kármán in this great warmth that he had, got hold of Moe Berg, because Moe Berg had a Hungarian background and he knew how to do things. And Kármán hired him as his lawyer, and Berg did a very good job. Got his brother out, and the brother lived in Switzerland after that.

Begin Tape 2, Side 2

MARBLE: After leaving Switzerland, we drove down along the Rhine, entered Germany, crossed the Rhine at Freiburg, which is the place where Patton and the tank corps crossed in the defeat of Germany. And it was the first time I had seen the kind of wholesale destruction that had been inflicted on the area. It was a real eye-opener for me. I really became mature, as far as war was concerned, at that moment. This was '49. There hadn't been much rebuilding at that time. At least you could cross the Rhine there, the bridge was open. Well, after that, it was an uneventful trip. We drove along the Rhine, and he went to Heidelberg, I went to Frankfurt, and then flew to Andrews Air Force Base, in Washington, and then got a plane back.

COHEN: Just the two of you were on this trip?

MARBLE: I was going to tell you about the automobile drive. It would take a long time to explain what the presence of Pipö—von Kármán's sister, Josephine—meant. But Pipö had a loud, penetrating voice, and she spoke very rapidly and in a very domineering way. I stayed at the same hotel they did.

COHEN: So she was on this trip also?

MARBLE: Oh, she always traveled with him, yes. She really took good care of him. And they stayed at a place called the Hotel California, which is off the Champs Élyseés, close to the Arc de Triomphe. And I made my way there, and met them there. Then we dined together once, and then started off on this trip, after the meeting.

Kármán had a chauffeur—who I gather was paid for by the military—and he was a French communist. And Kármán's sister could not stand that. She had only one name for him, which was "Damn-communist." It was all one word. And Pipö and I rode in the backseat. She would shout up to her brother, "Theodore, tell that damn-communist he should not go this way." [Laughter] This persisted all the way through. It was just something. But that was my first opportunity to get to know her well, and she got to know me well. But we hit it off all right, finally.

Now, to go back, in January of 1949, I had decided I would make an interview trip. There were three places I was interested in. One was Johns Hopkins, where Francis Clauser was the chairman of aeronautics. Francis Clauser had been a student of Bateman and von Kármán, and he eventually left Johns Hopkins and went to Santa Cruz, and then, of course, he left Santa Cruz and came here; he took Fred Lindvall's job when Fred retired. So it was Johns Hopkins. Or it was Cornell, with Sears. Or it was at MIT, with Tsien Hsue-shen. Tsien I had met. He was undoubtedly the greatest of von Kármán's collaborators. Tsien was brilliant. His demands and his standards were very, very high. He was a brilliant man and one of the few people I really wanted to work with.

So I interviewed at Johns Hopkins, gave my talk there. Went to Cornell, gave my talk there; enjoyed, of course, Bill Sears and [Arthur] Kantrowitz and all those characters there. And then I went to MIT. Well, this was early February. I did this because I had free transportation to the Institute of Aeronautical Sciences meeting in New York. Caltech had paid my way there, because I was giving a paper at the meeting. So I just tacked this interview tour on the end of it. So I went to MIT last. It was early February, and it was cold. And Ora Lee and I wound up at a hotel in Boston. And I said, "Dear, all we have to do is walk across the Mass. Avenue Bridge, and we'll be right at MIT." Well, the bridge, as you know, is a long cold bridge, and it's exposed to the wind and whatnot. And that came about as close to being the end of our marriage as anything that had ever happened. By the time we got there, we stood in the big rotunda at MIT sort of thawing out. She said, "I'll never do that again. Don't ever ask me, Frank, to do that again."

Anyway, I went to see Tsien. He was cordial. He was just marvelous. I gave a talk there, met Tsien and C. C. Lin, who was also a former student of Kármán's here. Ora Lee and I had dinner at Tsien's house that night with the Lins and with Tsien and his lovely wife and child—they had a three-month-old boy. And that was just fine. I thought, well, I think I've done all right at each of these places; I should hear from each of them.

Well, I got an offer—or at least an indication of an offer—from Johns Hopkins. And, of course, I got real enthusiasm from Bill Sears, because Bill and I obviously tended to see things the same way. But nothing from MIT—absolute, utter silence! So I told Ora Lee, "It's between Cornell or Johns Hopkins, or I can go back to NACA if I want to." So we were mulling over that. And I went and talked to Fred Lindvall. And he said, "Don't do anything hasty! I think maybe other things are going to happen." So I held off for a while. And I know Sears called me up once, because he thought he'd made a quite nice proposition to me and why wasn't I at least giving him an answer? He wanted to know something.

Well, it turned out that—unbeknownst to me and anybody else at Caltech except Lindvall—the Guggenheim Foundation had decided to finance a couple of Jet Propulsion Centers—one at Princeton and one at Caltech. This was supposed to be a group to develop courses in the propulsion sciences, particularly, since they involved chemistry, gas dynamics and materials. Guggenheim and his group were convinced that a new curriculum of courses was required to address in a unified way all these fields. He was right, and I have reason to believe it was Kármán who convinced him of this. And he had chosen Luigi Crocco, a notable Italian gas dynamicist, to accept the Goddard Chair at Princeton; and he had convinced Tsien to come back to Caltech and accept the Goddard Chair here. So the reason I hadn't heard from MIT was that Tsien wasn't going to be there; he was coming here. And he had, in turn, written to Lindvall and said, "By all means, keep Frank Marble there, because I want him to be the first new faculty member at the JP Center." So Fred Lindvall was a bit over a barrel, and he had to tell me what was happening. So, somewhat sadly in a way, I wrote back to Bill and told him I wasn't coming—that I was very grateful, but I wasn't coming— because, as you know, Bill's personal warmth is something that's hard to overlook. It was a little less troublesome for me to turn down Johns Hopkins. So that was when Caltech offered me the assistant professorship of jet propulsion and mechanical engineering. So I started off with Tsien here. I wrote to him, we corresponded about the program, and I started teaching the jet propulsion courses, which I developed extensively over the next five or six years.

COHEN: Who else was part of this organization?

MARBLE: Well, there was Tsien and me at the outset, the two of us. Now, I should say a little bit about what the idea of this Jet Propulsion Center was. Tsien did not want it to be part of aeronautics. Precisely what the roots of this were, I don't know, but I think it had to do with a little bit of lack of enthusiasm he had for Clark Millikan. Now, Clark was a very open, marvelous administrator and scientist, and would have done anything for Tsien. But Tsien didn't want to. So even though we moved into the building where aero was, we were not part of the aeronautics structure. What Tsien's point of view was, or what he claimed, was that this center combined things that were part of mechanical engineering, part of aeronautics, and part of chemical engineering. What we wanted was not to be a separate study course but to accept students from aeronautics, from chemical engineering, from mechanical engineering; and they would take a group of courses that we designated. In other words, we'd be an educational center but not a degree-granting group. And that's the way it worked out, and it is that way to this day. I think it was a very wise move, because otherwise it would have grown into a place where eminent people in gas dynamics would have come instead perhaps of going to aero[nautics], and some of the more active people in mechanical engineering, or who were hired by ME, would have opted to go with the JP Center. And the center would have become a larger group, to the detriment of both mechanical engineering and aeronautics. We had some special courses, of course—the propulsion courses. I gave a course in aircraft gas turbines and one in rockets, along with some applied mathematics courses.

COHEN: So what department did you consider yourself in, even though you were part of this special group?

MARBLE: I was assistant professor of jet propulsion and mechanical engineering. I was invited to faculty meetings of both aeronautics and ME, and being a new faculty member I didn't shun them; I went to both of them. In later years, I would avoid both of them. [Laughter] But at that time I was welcome at both. So that was the structure we worked under. Later that year, there was a chemist at JPL, Sol Penner, who had gotten his degree at the University of Wisconsin in chemistry, and spent part of the war years there, came to JPL after that, and was a very bright physical chemist, who worked on propellant problems—the structure of solid propellants, combustion problems, and things of this sort. Tsien hired him. He left JPL and came down to the campus.

COHEN: What would his position have been?

MARBLE: He was assistant professor of jet propulsion.

Now, I mentioned Duncan Rannie's name before. Let me go back to Duncan now. Duncan was the head of what they called the ramjet section at the Jet Propulsion Lab. And as such, he spent almost all his time there. He was made assistant professor of mechanical engineering in September of 1946. So he taught and he worked at JPL both.

Now, it turned out that Rannie did not have his PhD. And he was getting in the position where he ought to be considered for an associate professorship—and tenure perhaps. The rule here, though, is that if you're promoted to associate professor you can no longer get a degree from Caltech. So Tsien came in one day and said, "Frank, I want you to go take Duncan's job at JPL, so that Duncan can come down here and finish his PhD." [Laughter] So I said, "You want me to take that section at JPL?" He said, "Yes. You do that. Keep your work up here, but just move your research up to JPL and take care of that." So, reluctantly, I did that.

The upshot of that was that in the period between 1949 and 1958, when I left JPL, I changed that section over into a combustion research group.

COHEN: So you were up there for quite a few years.

MARBLE: Well, I was half-time there. I had an office here; I had an office there. And I commuted back and forth. I had a position; I had a section of about eighteen folks up there to deal with. Fortunately, I'd had experience in this kind of low level administrative activity at NACA, so it wasn't a great burden or a great chore. In fact, the whole laboratory was a very informal thing at that time, so the administrative chore was minimal. But the important thing was that it did move me into a new area. I got out of the purely gas dynamics, aerodynamics area into the combustion area. And that has been a large portion of my work since that time. Tsien was exactly right; there was a lot more innovative work to be done in combustion in jet propulsion systems than there was in the gas dynamics of it. So I moved into that—developed that at JPL—and even when I came back full-time to the campus here, I maintained the same work.

COHEN: But Rannie still didn't get his PhD?

MARBLE: Yes, Rannie did get his PhD. He did a problem. Tsien took him on as a student, humorously enough, and he got his PhD under Tsien. He must have gotten it in June of 1951.

COHEN: And Kármán was not here anymore?

MARBLE: Well, I'll go back and deal with the visits of Kármán, which were great explosions in our lives, every time he came back to town.

FRANK E. MARBLE

SESSION 3

February 22, 1994

Begin Tape 3, Side 1

COHEN: I think a good place for us to start would be the beginning of your professional career at Caltech with Dr. Tsien.

MARBLE: The background of that, of course, we covered last time. I was in the position of looking for a permanent job. And after having gone to several places where I thought it would be interesting to work, I wound up, as a result of some circumstances I described last time, back at Caltech here, working under one of the people that I had hoped to work with at MIT—namely—Tsien Hsue-shen. And the thing that brought him here was the grant from the Guggenheim Foundation for a Guggenheim Jet Propulsion Center.

Now, there was one of these established at Princeton and one at Caltech. And the pattern of this was set, I believe, by Tsien, with some cooperation from Clark Millikan in aero. It would seem to many that the natural place for the Jet Propulsion Center to be situated would be in aeronautics. Tsien, however, realized that the propulsion field encompassed not only the gas-dynamic and fluid-mechanic aspects of aeronautics but also certain material and design problems that were more at home in mechanical engineering. And also, the chemistry problems that were more in the field of chemical engineering. His picture was that the Jet Propulsion Center would function as an independent entity but not give degrees of its own. We never gave a degree in jet propulsion. There was a jet propulsion option, but the degree was always either in mechanical engineering, aeronautics, or in chemical engineering, with a preponderant number of them being in mechanical engineering and aeronautics.

I think this worked very, very well. I think my aim was to cover the gas-dynamics, fluid-mechanics area, and the general concept of jet-propulsion engines. The next faculty member who was hired directly into the center was Sol Penner, who was in the Division of Chemistry at JPL. He came to the campus full-time and taught the courses in chemistry, propellant chemistry, and propellant properties. The materials portion of it was handled by a Professor Pol Duwez, who was professor of mechanical engineering, a brilliant Belgian in the field of materials, and made many individual contributions to it. Pol had been at Caltech, and subsequently at JPL, during the war. He had been a close worker with Kármán. So it was natural that he would be involved in the Guggenheim Jet Propulsion Center. This, together with David Altman and Howard Seifert from JPL who essentially handled the laboratory work, constituted the Jet Propulsion Center.

We had, I believe, at that time, about half a dozen courses, which we gave over a two-year period to students who were interested in the propulsion field. And also a laboratory course, which was handled at JPL. Now this was a very important thing, because the Jet Propulsion Center at that time had no laboratories of its own. And although we had access to labs in mechanical engineering and some in aero, we really had none in which we could carry out combustion work and propellant work and rocket firings and things of that sort. And those, at that time, we were allowed to do at the Jet Propulsion Lab—not now, but during that time we were able to do that. [Laughter] So this was a very successful operation, and it was a popular field. It was timely, and we had very many students coming at that period.

COHEN: When you say very many, what kind of numbers are we talking about?

MARBLE: Well, I would say that we probably had fifteen to eighteen students who were taking their option in jet propulsion. The first year or two, the preponderant number of them were from aeronautics. Subsequently, but gradually, the preponderant number were from mechanical engineering and applied mechanics. And I'll treat that a little bit later, so you can see how things changed there.

I've mentioned Duncan Rannie before in connection with my first coming here, my contacts with him—actually, correspondence with him—before I came, and then some contact with him while he was here. He had been appointed an assistant professor of mechanical engineering. And he was at the Jet Propulsion Lab, in charge of what was called the ramjet section. Initially, the Jet Propulsion Laboratory—which was at that time a very small group, of fewer than a hundred and fifty people—was divided up primarily by engine types; that is, solid-propellant rockets, liquid-propellant rockets, ramjet engines.

COHEN: Is “ram” an acronym for something?

MARBLE: No. “Ramjet” means that it compresses the air by the air ramming into the front of the engine and it retards the air. And it’s an engine that works at high speeds only—that is, at supersonic speeds. Subsonic ramjets were never a success at all.

Rannie was the head of the ramjet section, which had about sixteen or eighteen people in it, and quite a bit of work. Everything at the Jet Propulsion Laboratory at that time was sponsored by or funded by the Ordnance Division of the Army. That was the group that Kármán and Frank Malina [when he was director of JPL] originally made contact with, and it continued to fund the work until JPL became a NASA lab.

Now, as I mentioned, the situation with Rannie was that he was an assistant professor of mechanical engineering but had never finished his PhD. He had come to Caltech from the University of Toronto specifically to work with von Kármán. And the story of his progress here was rather interesting.

In the early forties, at the time of the Tacoma Bridge failure, Kármán, while flying from Chicago to Los Angeles, picked up a newspaper, saw about the bridge failure, saw a photograph of it and its violent oscillations, and before the plane landed in Los Angeles he had written a few conjectures on the mechanism on the back of an envelope. When he got back, he gave these to Rannie and said, “Now, Duncan, you will work out the results of these formulas.”

Duncan did. He worked these out, and eventually this became a quantitative theory of aerodynamic oscillations of bridges and other structures, such as the flutter of airplane wings. Duncan, with Kármán’s encouragement, prepared a thesis based on this. It was written up, and Duncan, of course, being a Scotsman, wrote in rather terse English—very good, very well chosen, but terse English. So everything he wrote came out about half the length you expected it to. So his thesis was not very long. But all of the content was in it.

Now, just to go back a moment: Kármán, of course, was known as a master of gas dynamics, fluid mechanics, elasticity, materials. In fact, a lot of people don’t realize that his first work, when he was a student, was in materials. He did work on material properties, material failure, and such things. So this was a natural thing for him. And he did some very outstanding work in elasticity.

Also at some time, the great Russian elastician, Stephen Timoshenko, came over here.

I'm not quite sure whether he went to the Westinghouse Corporation first, but he was there for a while, wrote two or three very significant and important books, and then went to the University of Michigan. And at that time, Kármán got to know him reasonably well. And although Kármán did not have the highest opinion of him, he considered Timoshenko a good friend.

At one time, Kármán made a rather mild approach to Timoshenko to come to work at the California Institute of Technology, which Timoshenko did not do. He went to Stanford instead. So the relationship between Kármán and Timoshenko was that Kármán liked him, saw him as a good friend, but did not think of him as a first-rank engineering scientist.

Now, to get back to Rannie. When Rannie brought his thesis to von Kármán, Kármán looked through it and said, "Oh, Duncan, this is quite good. This would do great credit to a Timoshenko student." As a consequence of that remark, Duncan went back to his office and dropped the thesis into the wastebasket. And as things went, with the war starting, he never did finish his PhD.

All right, here we are, with Duncan not having his PhD and being an assistant professor of mechanical engineering. He was appointed in 1946, and this was now the fall of 1949. And the prospect of his becoming an associate professor was imminent. And rules at Caltech at that time were that anybody who was at the associate-professor level could no longer receive a PhD from the California Institute of Technology.

So, with his work at JPL and his professorship here, his chances to do a thesis seemed a little bit on the skimpy side. So Tsien came around to me one day and said—Tsien did not discuss things; he sort of stated them as facts—"Frank, you will go take Rannie's job at JPL so he can come down here and finish his PhD." With that, I left the work I was doing here and proceeded, reluctantly, up to JPL, took over the ramjet section there. It was an administrative job. It was actually smaller than the group I had had at the NACA laboratory, in Cleveland. So it wasn't any real chore. But it was a new field for me. This was the fall of 1949 and the spring of 1950. So I decided then that I would take the section out of the field of hardware, out of the field of ramjets as such, and have it deal with the combustion problem. I felt that the more fundamental issue of combustion was more suitable for theses for graduate students and such. And over the years, a very healthy, fundamental combustion research group developed, which many, many students were financed by and worked at JPL and got their Caltech PhDs.

COHEN: So, in some sense, JPL was your laboratory.

MARBLE: JPL at that point was the source of my research funds in the laboratory in which my students worked—that's what it really came down to. And I had no comparable laboratory here on campus.

This was just working its way out, and things were fine. And Ora Lee and I felt reasonably settled at that point. So we went off on a month's camping trip into the Canadian Rockies. And when we got back, we found that things had happened that sort of distorted the future of the Jet Propulsion Center.

This was August of 1950. Tsien had gotten in trouble with regard to his security clearance, and he was being held in San Pedro by the Immigration and Naturalization Service on the grounds that when he had re-entered the country a few years earlier, after a visit to China, he had lied by saying that he was not a member of the Communist Party.

Now we have to go back, because this is a complicated and convoluted story. You have to remember that this was the beginning of the McCarthy era, and that communists were being found in every Congressional office in Washington and under every rock you could find. Universities were a great place for finding communists; after all, Linus Pauling was nailed for this one way or another, and some of his students.

The facts are that in the mid-thirties, when Tsien first came here, there was a group of young men who were perhaps a little bit on the radical side. They principally had interests in music, particularly chamber music. But also, at that time, the Spanish Civil War was under way, and there was a great deal of sympathy for the Spanish republicans. It was essentially an anti-fascist feeling, and this, of course, was discussed at the meetings of this group.

Various people wandered in and out of it. Tsien was one who was invited to come by Frank Malina, who was then a graduate student in aeronautics and one of the more active people in the group—numbers of other students. Pol Duwez certainly came to some of their meetings. Although I've not mentioned it, Pol was a cellist of the first magnitude. In fact, when he was sixteen or seventeen, he had to make up his mind whether to become a cello soloist or follow science. And he followed science. But he still played the cello and was very active in chamber music.

This appears to have been the root of Tsien's problem, as far as his security clearance goes—that he was associated with this group. Now, I should say also that Frank Malina had come under some suspicion or some concern. He may have been questioned about this, and I don't know that his security clearance was ever cancelled. But he resigned as the director of JPL in 1946-47, about the time that I came here, and he accepted a position with UNESCO. He worked there for the rest of his scientific career, and he was based in Paris. I had met Frank earlier, but my biggest contacts with him were in Paris, actually.

Now, Tsien had been very much involved in Kármán's classified study for the Army Air Forces during the last years of the war, and he had been a major contributor to a series of books called *Toward New Horizons*, which essentially set the technological plan for the Air Force that was followed for the next ten years. It was a very important set of books. The studies essentially showed, among other things, what rockets could do as far as ordnance was concerned. The ballistic missile was studied and outlined there in great detail, what its capabilities were and what its problems were, as were other jet-propulsion engines. And Tsien essentially wrote these chapters. So he was very high on the list of people who had access to secret information.

Now, in a routine way, apparently, in the spring of 1950, when it came time for his clearance to be renewed, there was a question about it. He understood that he had the option of either just not applying for his clearance or going to Washington and stating his case to the army.

COHEN: So they were already looking at this group.

MARBLE: They had looked at individual members of it, yes. Tsien never let on to me—at that time, we were not actually close enough so that he would have confided in me. I'm sure he confided in Rannie, with whom he was quite close. And I'm sure he confided in Pol Duwez. And he told President DuBridge, Fred Lindvall, and, I presume, Clark Millikan. I found out about it only after Ora Lee and I returned from our elegant camping trip.

The way things developed was the following: Tsien went to Washington at his own expense, stated his case, and pleaded his innocence. And, evidently, while it's possible to prove you're a member of the Communist Party, it's impossible to prove that you're not. So evidently he did not satisfy them. And the authorities with whom he dealt said they could not reinstate his clearance.

Tsien had already made up his mind that if this happened, he could not be of use in the

United States. And it was probably a good time for him to pick up his family and go back to China, at least for a year, until things settled down. While he was in Washington, he decided to pay a call on Dan Kimball. Now, Dan Kimball was the Assistant Secretary of the Navy. Tsien had known Kimball very well, because he had been the president of the Aerojet Corporation. The Aerojet Corporation was founded in 1942 by Malina, Kármán, Millikan, and perhaps another investor or so—they were certainly the major stockholders in it. And Kimball became the president of that organization as it grew to considerable size and involved a considerable amount of money. Kimball was appointed Assistant Secretary of the Navy, and he went to Washington. Tsien felt that while he was there, it would be appropriate for him to call and pay his respects to Dan and tell him what the situation was, since Kimball had used him as a consultant at Aerojet on highly classified things.

Well, Kimball was warm, friendly, and all that, but very firm in his feeling that Tsien should not leave the United States. Tsien explained that his opinion was that without a clearance he was not as useful as he might be. And actually, he took it as something of a personal insult that this sort of thing should happen. He told Kimball that he intended to go back to China. He had already requested a year's leave from Caltech, which was offered to him. He would pack up his family and go off to China for a year. His father, who was ill, had not seen either of Tsien's children. By that time, Tsien and his wife had two children—their son, who was born in '48, and a daughter born in the spring of '50. He told Kimball all of this, and Kimball said, "Tsien, I'd rather see you dead than back in China."

As Tsien left Kimball's office to get back on the airplane, wheels were set in motion, whereby the FBI and the Immigration and Naturalization Service would meet Tsien as he landed at the airport in Los Angeles, arrest him, and take him to the San Pedro INS facilities.

COHEN: You think that Kimball told them to stop Tsien before he left?

MARBLE: Kimball has said that he did. Now, Kimball had no evil in his mind. He merely wanted to detain Tsien and talk him out of this. He thought maybe they could handle it that way. But once the Immigration and Naturalization Service gets hold of something, their clumsy, heavy-handed means go into effect. And this, in a way, proved the undoing of Tsien, and led to his eventual return to China.

The way it developed was the following: He was certainly harassed unbelievably by the FBI. And I should say that a very unfortunate thing happened. The Tsiens had been living in a nice house in Altadena, a house owned by Fred Lindvall's secretary. When she learned of Tsien's problems, she essentially evicted them and told them that her mother was going to come and occupy the house and they would have to find something else. Well, it was impossible for them to find anything, because now he was notorious, his name was in the papers, his pictures were in the paper; he was the subject of rather frightening headlines. So Ora Lee and I took it upon ourselves to try to find him a place to live. We did. We finally found them a house, and told the woman who owned it that this was the man whose picture had been in the paper and who was accused of this and that, but she would find them very good people to rent to.

I may have gotten that a little bit out of order. I should tell a bit about the hearing. The Immigration and Naturalization Service decided that they would have a hearing on whether Tsien had re-entered the country [in 1947] illegally or not—and that is, whether he had lied about his membership in the Communist Party.

The incident was the following: After he went to MIT in 1946, he returned to China in the summer of 1947 and married his childhood fiancée, Tsiang Yin. They were married, and they came back in the fall of 1947. And it was that re-entry that the Immigration and Naturalization Service was concerned with, particularly one Albert Del Guercio, who was the appointed hearing officer. They said, "Because you were a member of that group at the California Institute of Technology, you were a Communist, and you lied about your Communist membership when you came back into the country." And that was it.

Caltech was very reasonable about the whole thing. I would say that the board of trustees was for firing Tsien, at the minimum. Lee DuBridge held his ground and protected him. Not only that, but Caltech hired a lawyer for him, a lawyer named Grant Cooper.

Now, Cooper was essentially a criminal lawyer and a very reputable one. But he had really very little experience with the Immigration and Naturalization Service, which is a whole different kettle of fish. And I think it's fair to say that he was not as effective as he might have been.

Anyway, I took Tsien to the hearings. I should say that Tsien had been released. His bail had been set at $15,000, which was an enormous amount at that time, but it was posted by Polly Mills, the wife of one of his good friends, Mark Mills. She was independently wealthy, and plunked down $15,000, which remained with the INS for more than five years.

COHEN: How long was he held down there?

MARBLE: He was held down there about two weeks, until the bail was posted and everything was straightened out. Then he came back to Pasadena. Ora Lee and I felt sort of responsible for taking him over, because it turned out that a lot of people were a little bit reluctant to take on the care and feeding of someone who is pursued as a Communist. We didn't have any concern about that. So I went with Tsien to every hearing, went through all of this, worked with Grant Cooper, the lawyer, on getting all the information that there could possibly be. The hearing extended over a period of about six weeks, I would say.

One of the key people in it was a former student in chemistry named Richard N. Lewis. Richard Lewis was at that time on the faculty of the University of Delaware. He gave a deposition, which was presented at the hearing, which definitely fingered Tsien as a member of the Communist Party. No other evidence of any significance was ever found.

COHEN: Was this Richard Lewis part of the group in the thirties here?

MARBLE: Well, it was clear that he had had access to the group, was acquainted with them. But what the extent of his involvement was wasn't all that clear. He was a student at Caltech; he got his PhD in chemistry here.

I was making a trip to the East Coast, and I told Grant Cooper that I would be happy to go and speak to Lewis. Because Lewis had been subpoenaed to come to Los Angeles to attend the hearing and give his opinions, or answer questions directly at the hearing in person.

So I made contact with Lewis. He didn't want to meet in his office at school; he didn't want to meet me at his home. He picked some remote coffee shop one afternoon. To make it short: He said, yes, the FBI had approached him; yes, the FBI had threatened him. The FBI had said, "You either give us substantial evidence against Tsien or we'll use what we know about you to discredit you at the university here, and you won't be welcome." And he told me, "Look, I've got a wife and two children myself. I can't do anything else." And I said, "But you really don't have anything substantial to say about Tsien, do you?" He said, "I've talked to the FBI, and I'll say what I say."

So he came. He gave testimony, which was damaging, certainly, but would never stand up in a court of law. Nevertheless, the hearing went against Tsien. It was decided that he had lied when he came into the country in 1947 with his new wife. And he was, therefore, issued a deportation order. He was to be deported. And the deportation was to be stayed for five years. Hence, he was not allowed to go more than twenty-five miles from his home. So he was essentially under house arrest for a period of five years.

COHEN: Why do you think they did that?

MARBLE: Well, the reason for that is clear. He was probably the most highly informed aerodynamicist, gas dynamicist, propulsion expert, in the classified and secret literature of the United States. And the feeling was that in five years what he knew would be sufficiently out of date so that they could get rid of him.

Now, this put Tsien in a sort of unusual spot. He was not able to partake of industrial work or classified work. And at that time, a great deal of the propulsion and rocketry work was classified. He *was* able to teach, and he was able to pursue his own research work, which remarkably, he did.

COHEN: Of course, it was DuBridge who was protecting him.

MARBLE: His professorship and his chair as a Goddard Professor were still intact, and he was a highly respected member of the faculty. He did not travel, of course. He did not leave Caltech. On several Sundays, Ora Lee and I would take our son and their two children and Yin and go off into the mountains some place. We made a lot of trips out into the desert and the mountains when the weather was nice.

Tsien carved a remarkable record for himself during those years. One reason I'm stressing this is that those years are very important to me, because I saw Tsien work at his ultimate. The range of problems he worked on, the rapidity with which he saw a problem, encapsulated it, solved it, published a paper on it, was truly remarkable. And the guidance he gave to students and to both Penner and myself—and to a certain extent, Rannie—while he was still here, was just a remarkable thing.

To finish that story up, in 1955 he told me, “Frank, I’ve gotten my orders to leave the country now. So Yin and I and the children will be leaving in September of 1955.” Well, it was a sad day for us. And Tsien had had to move out of his house. He and his family had actually come and lived with us for the last two or three months. We had a marvelous time with them. Ora Lee learned how to cook Chinese. [Laughter] And then in September of ’55, we accompanied them down to the boat and they sailed for China.

COHEN: Now these five years had been already preset, that he would stay five years.

MARBLE: Yes, that was part of it—that the deportation order would be postponed for five years; and at the end of those five years it triggered and off he went.

COHEN: Were there any appeals during this five-year time?

MARBLE: No. I should say, not that I know of. I’m not really certain about that. What Caltech did I’m not quite sure. But I don’t think there were any. I have the feeling that I would have been privy to them if there had been. So that closed the chapter of my direct work with Tsien, which was very important.

Now, I should go back and tell a little bit about the students I had during this time. While I was still instructor in aeronautics, three students come to my mind who were important. One was my first PhD student. His name was Gerald Monroe. He was a navy student and a brilliant young man, and completed his work in 1951. Another one was, interestingly enough, Benoit Mandelbrot. Now, Mandelbrot is the initiator of fractal theory, as everybody knows. He came into aeronautics. He was handed to me as a research student, and he worked with me for two years. He was a marvelous fellow; I just enjoyed Benoit thoroughly. He was utterly French, and was an enormous help to me when I was making my first trip to France. He essentially drew a map of Paris on the blackboard, and told me I should go from here to there and that and that, and then go to his uncle’s house. He was very helpful in that.

COHEN: Were you handed these students from JPL?

MARBLE: No, no. This was late 1948, early 1949. I got these students while I was still an instructor in aeronautics.

Just to follow Mandelbrot along—after about eighteen months of our work, Mandelbrot wrote a very nice thesis, but it was an engineer's-degree thesis, not a PhD. He had decided that he would leave engineering science and go back to mathematics. He had studied mathematics, and of course his uncle was a famous mathematician. So he felt that he'd go back to pure mathematics.

So as Benoit became well known as a mathematician, and then famous—or perhaps infamous—as far as fractal theory is concerned. And every chance I've gotten, I've told him, "You know, Benoit, you were my greatest contribution to pure mathematics; because without me, you might have fallen into technology." [Laughter] He is greatly filled with himself—very, very proud, somewhat overbearing, almost to the point of being rude. He has changed a great deal. When he was just a kid, this was not so. It developed as his fame developed. But I still find him a very enjoyable person to have at a party. I don't enjoy his lectures, but I do enjoy him at a party.

Another one was Chuang Feng-kan. He was the first mainland Chinese that I had as a student. He came right after the Liberation—or the Revolution, depending on whose side you see it from [1949]. And he came as a student, and stayed with me as a student for a year, until I moved out of aeronautics into the JP Center, at which time he moved from me to pursuing his research with Liepmann. Feng-kan and I have been very good friends ever since. And I see him in China when I'm there. He went back to China after he finished his PhD. [Tape ends]

Begin Tape 3, Side 2

MARBLE: Chuang Feng-kan sent both his daughter and his son here to Caltech. His daughter finished her PhD in aeronautics, and is now, I believe, at the University of Michigan in Ann Arbor. His son was in mechanical engineering here, and then moved to the University of Southern California. He finished his PhD at the University of Southern California. And I've lost track of him right now. I don't think he's back in China. I believe he's working in the United States.

From this time on, the students that I had came to the Jet Propulsion Center. Now, three of these are interesting. Two of them sort of followed me from the Cleveland Laboratory of NACA. One of them was Artur Mager. Art Mager was working at the Engine Research Lab in 1950, came to Caltech as a student in aeronautics, and came to work with me as a research student. He'd known of me from his time in Cleveland, and we had known each other only slightly, but he came particularly to work with me. And, of course, he did a beautiful job—did a thesis that was extremely interesting, on turbo machinery flow, a notable piece of work. Eventually, after several jobs, he became the technical vice-president of the Aerospace Corporation, and he retired from that about five, six years back, as a very notable person. He was elected to the Academy of Engineering.

Another one was Jack Kerrebrock. Kerrebrock has been a very important influence in my life. He was a very good student, did outstanding work here; completed his PhD. And after working at several places, he wound up at the Gas Turbine Laboratory at MIT as a faculty member, and eventually became the director of the Gas Turbine Lab, then head of the aeronautics/astronautics department, and after that the associate dean of engineering. The Kerrebrocks and the Marbles are still very good friends and see each other as frequently as we have the opportunity. He still is a very important senior faculty member at MIT.

Kerrebrock was actually born in California, did his undergraduate work in Oregon, wound up working in Cleveland at the Engine Research Lab. Met Vicky, his wife, there. And then in 1953, he left there and came here. He was not there at the same time I was. I left in '46, and he did not come to the laboratory until '50 or so. But we knew each other, got to know each other before he came here.

Now there are two others worth mentioning. One of them is Thomas Adamson. He was one of the first people I hired on my JPL contract. He worked with me at JPL, did analytical work. He and I did what turned out to be a quite important piece of work, which is known in the trade as the Marble-Adamson problem. It's still called that after all these years. He finished his PhD in '54. I tried to keep him at JPL, and he did stay for a little while. But then he got an assistant professorship at Ann Arbor, and within fifteen years wound up as department head of aeronautics there, and retired from that only two years ago. So he has had a very distinguished career at the University of Michigan.

And then the other one is Ed Zukoski. Ed is a very interesting fellow. He comes from a distinguished family, all of whom were from the north, but for business reasons—his father was a trust banker—went to Birmingham, Alabama. They were sort of the local radicals in Birmingham. Ed grew up there. It was an affluent family; he went to private schools; went to Harvard; and came to Caltech after Harvard. He came to me for research work, and we went to JPL. And Ed essentially became my experimental right hand. He was an unusual person; he still is. He's on the faculty here. He's been a professor of jet propulsion and mechanical engineering for many years now. He's within about seven years of my own age, so he's crawling up on the retirement ladder also. Ed was probably the most interesting experimentalist I've ever worked with. He avoided, for some reason or other, the ailment that so many experimentalists have. That is, they become so entranced with their instrumentation and the details of the techniques they're using that they forget about the physics, or the situation they're trying to explore. Ed was not this way at all, and is still not this way. The physics of the problem are still his focus. And the instrumentation, although very important, always occupies the second tier of importance. He's a wonderful experimentalist to work with.

He did some experiments for me which, in the end, were instrumental in solving one of the very difficult problems of combustion-chamber design and behavior for air-breathing engines —both for gas turbines and ramjet engines. I think by 1955 we had taken the issue, which was then very, very cloudy, and had made it into a perfectly clear point of view, which is accepted. The results we obtained at that time are still being used.

My students have always been important to us. And Ora Lee and I have always treated them, or thought of them, as an extended part of our family. When we had our big house up on Mountain Street, we always had them over for parties on Christmas afternoon. And we've maintained contact with each since. And that's one of the rewarding issues for having had such a nice group of students. I'll deal with others; but those were the ones from the early times.

FRANK E. MARBLE

SESSION 4

March 4, 1994

Begin Tape 4, Side 1

COHEN: Last time we ended at 1955, and you said goodbye to Tsien.

MARBLE: Yes, Tsien received the order to carry out his deportation in September of 1955. And he and his family sailed on September 17, 1955. They had been living with us. We saw them to the boat, and packed them onto the boat, with a bit of sadness. And as I left, Tsien said to me, "Frank, I'll see you in twenty years." That shook me up a little bit, but I didn't know the realities of the situation quite so well at that time. But it actually turned out that I didn't see him for twenty-seven years. And when I finally saw him, I said, "Tsien, that was better than engineering accuracy." [Laughter]

Anyway, the departure of Tsien changed quite a few things here at Caltech. He was, after all, the Goddard Professor and head of the Jet Propulsion Center. And this left the professorship open, and it left the directorship of the center open. Of those of us who remained on the faculty, the two senior people were Sol Penner and myself. We were roughly of the same stature. And I think, without coming right out and saying so, neither would have stayed if the other had been put in the Goddard Chair and given the directorship. And I think we understood that. So Sol and I sat down one day and chose a substitute candidate. We said, "Well, Rannie is a jet-propulsion fellow, too. And he's professor of mechanical engineering. Good friend of Tsien's, and a good friend of each of us. He doesn't manage anything with an iron hand. So why don't we go to Lindvall and suggest making Rannie the Goddard Professor and head of the Jet Propulsion Center?" So we marched about thirty feet to Fred Lindvall's office and told him what we had in mind, and he said, "Great! That's fine. It's done."

So the way it was, Duncan Rannie then became the Goddard Professor of Jet Propulsion and the new director of the Center, which was a position he held until his retirement in the mid-eighties.

At that time, the situation with regard to my staying here at Caltech opened again. I would not have left during the time that Tsien was in limbo here, for two reasons: One is that he needed whatever support he had; and the second thing is that I really prized the opportunity of working with him, which was a great advantage for a young faculty member.

But I had tenure; I was an associate professor—I was made an associate professor in 1953, I think. The question of whether I remained here at Caltech was now an open one again. I could think about it.

Before the end of the year—that is, sometime in maybe November, December 1955—Bill Sears, my old friend, called me up from Cornell and said, "Frank, why don't you come here? We have an opportunity to have a visiting professor here, so why don't you and your family plan to come. In fact, you can come right now, if you want to." As I think of it, I'm not sure that Bill didn't call me as soon as he heard that Tsien had his deportation order [August], because in the back of his mind was that I could come for the fall term of '55 and the spring term of '56. As it worked out, we decided we would close our house up and go and spend the spring term of '56 and the summer at Cornell. And we did that. We packed up the car and the kids. We had two children by that time. Our son Stephen was born in '51 and our daughter Patricia was born in '53. So we packed them up and roared off to Cornell, knowing very well—because of our background in Cleveland—what the snow was going to be like when we got into the northeastern part of the country. [Laughter] We left here around the middle of January, and didn't come back until the fall. So I was on leave during that time.

We thoroughly enjoyed our time with the Sears. The Sears have a way of sort of fathering and mothering new faculty members, which is a great advantage when you come into a strange town and don't know the ways around, and the snow is eighteen inches deep all around you. And that spring of 1956 was not a light one. I can remember we had a very heavy snow in May sometime. I was giving an aeronautics seminar before the snow came. And Bill Sears was sitting in the front row. I was about halfway through the talk, and he leaned over and whispered desperately at me, "Frank, cut it short. It's snowing like hell!" [Laughter] And I didn't pay as much attention to him as I should have, and as a consequence the snow was around five inches deep by the time we got out of the seminar and started home. And that was just enough to make a lot of people get stuck on the way home, and enough for me to be unable to find my own driveway—which I wasn't too familiar with. We really had a marvelous time there. We went back to California after an extensive trip on the East Coast and the Gaspé Peninsula, down through Montreal, and back home.

The issue was then whether I would go to Cornell permanently or stay at Caltech. Ora Lee and I did a lot of soul-searching, thinking about the children, thinking about ourselves, thinking about our future. And for a variety of reasons, we decided to stay here.

I never really wanted to leave Caltech. In spite of everything, Caltech was just different from anyplace else. I had no urge to leave. I was a little bit restive about what the future would be with Tsien gone—because he was such a strong person—but I decided to weather that; and I had enough seniority myself to take care of things as they would work out and also to map whatever future I wanted.

At that time, I did make the decision to leave JPL. There were a couple of reasons for that. One was that JPL in 1956 was showing the signs of change that would alter its whole appearance and attitude. By 1958, everything had changed there completely.

COHEN: Dr. William Pickering was director then?

MARBLE: Pickering became the director in that time frame. But the main change was that NASA was formed. NACA, where I had worked, became NASA, and JPL made a strong shift toward space activities. And the kind of thing I had been involved in became somewhat less interesting to me. Also, JPL became larger. I can remember that each time JPL reorganized, changed its line of work, the number of people increased by a factor of ten. There were originally about thirty people, during the rocket experiment days—that was in the thirties, early forties. And then, at the time that the army placed big contracts there and started to develop the Sargent and the Corporal missiles, they increased again by a factor of ten. And about the time I went there, they probably had three hundred or three hundred and fifty people. And then, of course, as the space work came in, and JPL took on a large contracting responsibility, why, the number climbed to about three thousand. So each time, it was a factor of ten. And I can remember pointing out—it's the one time in my life I think I one-upped [Caltech President] Harold Brown, because we were talking about the possibility of reorganizing and shifting JPL over to environmental work, making it the great center of environmental activity. And I said, "The only thing wrong with that, Harold, is the history. You know you never get rid of the old people; you just build an organization so large that you don't notice the old folks. That's what happened when JPL got involved in space work—namely, they hired enough new people so that the old rocket folks there were scarcely noticed." [Laughter] And I said, "With about four thousand there now, you don't really want to wind up with forty thousand people there, do you?" And he said, "That's something I have to think about." [Laughter]

Anyway, this new circumstance—plus the fact that Tsien would have probably taken a dim view of my leaving JPL and coming back to the campus full time, but now he was gone—these reasons all went into it. So I left JPL in the spring of 1957.

That was a hard move. Fred Lindvall, who was the chairman of the division of engineering, didn't mind at all. In fact, he was rather happy about it. I had not realized, however, what a large fraction of my salary JPL had been paying. They had been probably paying two-thirds of my salary, because every time I would go in to negotiate increases in the salary that some of my people up there were getting paid, Bill Pickering would look at what my Caltech salary was, and he'd chuckle heartily—I was getting paid less than any of the other people in the section. He'd say, "Well, your salary is set by Caltech. There's nothing we can do about that, Frank. Ha-ha-ha!" [Laughter] So that was the result of that.

Anyway, this salary problem continued until I got my contractual base at Caltech built up.

COHEN: Did you continue to have your labs up at JPL? Because you said your students trained in labs at JPL.

MARBLE: I tapered off my last students. I think my last student up there was a Canadian student named Grant Brownlee. He was in the Canadian military, and a very good student. He was still doing his work after I'd come down here full time. But Ed Zukoski stayed at JPL, and he sort of acted as a stand-in for me when Brownlee needed something, like money or firing-pit time, or whatever. So we handled it in that way, and gradually—certainly by the end of 1957—I had no students left up there. They were all down on the campus.

Now, I didn't leave there and just leave the job open. I was too proud of the group I had. What I did was to persuade the chief of the combustion chemistry division at the NACA Lab in Cleveland to leave there and come to JPL. His name was Melvin Gerstein.

In order to close the loop on how Mel and I became good friends and very close—and we remain so to the present time—I have to go back to another chapter, which I've purposely left until now. And that is my association with AGARD, which was the Advisory Group for Aeronautical Research and Development of NATO. In 1952, Kármán called me to a meeting at his hotel in New York. There were three or four people there. I remember Jean Fabri from France. There was an English combustion man—Brian Mullins, from Farnborough—and myself. And he outlined for us what his ideas were. Kármán was close to the NATO organization, because he felt the defense of Europe was a desperate thing at that time. And having been born and raised in Central Europe, in Hungary, he knew what the problems were and he knew what the dangers were. And he had a profound lack of enthusiasm for the Russian occupation. He wanted not only to strengthen NATO but to build up an organization consisting of young people from all the NATO countries—England, France, Denmark, Holland, and so forth—primarily to develop common research aims. I think his picture was that they would pursue their research work unclassified, and we would be able to exchange not only the results but also exchange people. He felt very strongly about that, and when he talked to us at that meeting he said, "If the youth of the world cannot cooperate in scientific things, where is our civilization going to be thirty, forty years hence?" And quite clearly, what he saw was that the old feelings of division among the European countries and between Europe and the United States was not as strongly ingrained in people thirty years old or younger. And that by living together and working together in this organization, we would develop friendships and ties which would see us through hard times politically, militarily, which he felt were bound to come in later years. And that was the basis on which he formed AGARD. So we were some of the very earliest members of that. It turned out that the three members of the combustion panel from the United States were Sol Penner, Frank Marble, and Melvin Gerstein from the NACA Lewis Lab. So we traveled together a great deal, to all of the NATO countries over the next several years. In fact, I stayed on the panel from its inception until I think, 1965.

These associations, these travels to Europe, came regularly over the next ten, twelve years. Two to four times a year, we would meet in one of the European countries; we very seldom met here. The reason for that was that the United States Air Force was very happy to transport us over there, but less enthusiastic about bringing Europeans here. It was a long, hard trip, flying 8,000 feet over the ocean. It took a long time to get there.

The first general meeting of AGARD was in Rome, in December 1952. In fact, it was a meeting of NATO and AGARD; it was the start of the AGARD organization. Now Kármán was in charge of the entire thing, with a positive control over it that defies description, really. Without seeming to try, he just orchestrated everything that happened. For example, with regard to the combustion panel, he got hold of me before we sat down in the general assembly to nominate folks, and he said, "Now, Frank, when I ask, or when you are asked, for nominations for the chairman of the combustion panel, you will nominate Dr. Jean Surugue, from France." I said, "Dr. von Kármán, I don't have the remotest idea who Jean Surugue is." He said, "Nevertheless, you will nominate Jean Surugue." [Laughter] So, came time, the question was asked, I got up and nominated Jean Surugue as chairman of the combustion panel. Of course, that cemented the friendship of Jean Surugue and Frank Marble, even though we'd never known each other before that moment; he thought it was a gesture of great confidence and friendship. Surugue was a very fine man and played a significant role in my attachment to AGARD.

Kármán explained his reasons to me somewhat later. He said, "You know, Frank, most of the money and the organization of this entire thing comes from the United States. And the impression the Europeans get is that we are manipulating them. It must be that Europeans are the chairmen of most of the panels. They must have that right; they must have the influence to push things the way they want them." So that was his reasoning. And it worked very, very well. As usual, Kármán was a marvelous judge of what was right and what was wrong in situations of this sort. But I must interpose another story here.

There was still a surprisingly strong militaristic hold over Italy at that time. Military officers in bright uniforms were plentiful.

COHEN: Was this more so than, say, in the other countries you were involved with?

MARBLE: Well, it was more notable in Italy, and it has a little bit to do with the story I'm going to tell. It was certainly more so in Italy than it was in France—considerably more. Anyway, we were all ready to march into the hall, up the stairs into the hall for the general assembly, and the Italians had put a very handsome soldier on each side of each step. So we went through this file of bright-colored military folk—just handsome and tall and strong. I walked with Kármán, as several of us did. And it was clear that he was a little bit ill at ease about this, as we started up the first step. He didn't take stairs all that rapidly in those days, anyway. So he walked up a couple of steps, and he sized the thing up as being a little too stiff, a little too formal. So he walked over to the second soldier and shook hands with him. And that just cracked everybody up—that this big stuffy guard would have this little American come up to him and shake hands with him, while they were trying to put on the biggest show of their lives with all this window-dressing. [Laughter] It just cracked the whole place up. And it really injected a note of levity into the entire general assembly-meeting. He knew exactly how to break the ice, and that was the way he did that.

The reason I bring this AGARD story up is that not only did the organization have a great deal of influence on my life but it was there that Mel Gerstein and I got to know each other. I knew that Cleveland was not his home—Chicago was his home. But he was very fond of Cleveland. They had been very good to him there. And his wife was a Cleveland native. They had one adopted son.

The problem in 1957 was for me to convince Mel to leave NACA [NASA]. He is not one who takes big, bold moves easily. I told him later that I spent my full month's allocation for telephone calls on one long-distance call to him, and he said, "Yes, I know that. I came because I was too tired to continue the telephone conversation." [Laughter] So he came as the chief of the combustion research section at JPL, and stayed there. And he was a part-time faculty member at Caltech until the sixties, at which time he went as professor of engineering to USC, and retired from there about three years ago.

Anyway, that sort of terminated my active association with JPL. And my problem now was to build up a comparable research program, or at least a sizable research program, at the campus.

COHEN: It would be interesting to say something about the organization of the engineering college in general here at Caltech.

MARBLE: Yes, I think that is probably interesting here. If you go back to the prewar years, electrical engineering was part of physics. It was called the Division of Physics, Mathematics, and Electrical Engineering, at that time. I think astronomy was a division of its own—I'm not quite sure. But electrical engineering was part of physics. And Fred Lindvall was one of the people who was in electrical engineering.

In 1946, when Lee DuBridge came, electrical engineering moved into the division of engineering, and Fred became chairman of that division. This changed the complexion of it a bit, and of course brought eventually a considerable addition to the faculty. So now the division of engineering was in several parts. There was mechanical engineering, applied mechanics and earthquake structures, civil engineering and hydraulics, electrical engineering, and aeronautics. Aeronautics was a place of its own. And I think it's important to say that Kármán, when he came, lifted the scientific quality of aeronautics above anything else in the division of engineering, including electrical engineering. He just had that way about him of doing it, and he carried the aura of a great European. He also was extraordinarily good at extracting money from a wide variety of people—even, in some cases, Robert Millikan. So he kept things going very well.

There developed, at that time, a bit of a split between aeronautics and the other portions of engineering, at least civil engineering and mechanical engineering, in the sense that these were old-time engineers—that is, experimentalists, largely empirical by nature. And here was Kármán, with his analytical approach and his scientific understanding of why things happen, not just what happened. And Kármán had introduced a whole new attitude of engineering. He called it engineering, but it was very different from what was practiced by any of the others. And it's one thing that made Caltech unique at that time. The aeronautics school was a unique place. Probably there's only two institutions that had anything like that, that I know of—Brown University was one of them, and that's because it went way out of its way to import—or to give refuge to, I should say—to some of the outstanding scholars that had left Europe before the war, and even during the war. Brown developed a school of applied mathematics that was second to none in the country. And, of course, Richard Courant went to NYU, and established a similar school there. It's interesting to think that all of these people were related, in the sense that their lives were influenced by Felix Klein. Felix Klein was primarily a mathematician at Göttingen, and he influenced this entire generation of people, including Kármán, Courant, and the large group of people that went to Brown. Klein, in spite of his mathematical competence and mathematical understanding, realized the limitations of pure mathematics. He acknowledged the fact that mathematical disciplines originated in physical problems, really, and that you abstracted something from the physical problem, you abstracted a mathematical description of it, and then you studied the mathematics of that. And if the mathematics got too far from the physical issues, it became a little irrelevant, he felt. He was very much interested in real problems—in physical problems, engineering problems, and such—and he instilled this in everybody that came close to him. And even though these people were not formally his students, the influence he had was phenomenal. In a way, he is the godfather of this entire new generation of engineering.

Now, at that time, that made a significant contrast between aeronautics, where this situation sunk in and grew, and the rest of the division. Eventually, aeronautics was populated by Kármán's students—such as Sears, Sechler, Stewart, and Clark Millikan, to a considerable extent, although he was here beforehand. And by other Europeans whom Kármán brought in, such as [Hans] Liepmann, who also either had the same point of view or rapidly took that point of view on when they got to know Kármán. And I don't think it was Kármán's choice, but this sort of isolated aeronautics. And it might have given aeronautics, or the people there, a slightly arrogant attitude toward the rest of engineering. This was certainly felt by the people in mechanical engineering, although they were friendly enough, and the people in civil engineering. And it was certainly the point of view that I was introduced to when I came here in 1946. And it was a considerable factor in Tsien's choice to set the Jet Propulsion Center as sort of a bridge between mechanical engineering and aeronautics. He felt that by putting the center in there, it would somehow loosen this tension, and, in a way, upgrade mechanical engineering. And I think that's true, that's what has happened.

Of course, electrical engineering didn't need any nudging in this direction, because they came out of physics, and consequently they didn't need any borrowed arrogance; they brought their own along with them. [Laughter] During the years I was here, up to the part I'm talking about, before the 1960s, this was the situation that existed. And the more forward-looking people were of the analytical sort—fundamentally scientific people who were interested in engineering.

Therefore, many of them wanted to change the name of the division of engineering. I was a senior faculty member when this issue came up at our faculty meeting; I was a full professor. I had remembered how I liked the old name of Case, which was the Case School of Applied Science. I said, "Why don't we call it the Division of Applied Science?" Well, Lindvall liked that very much, and many of the people liked that. But they didn't want to drop the "engineering" completely. So it assumed its current name which is the Division of Engineering and Applied Science—and that was a transplant from Case. Case has since thought it would be much more clever to be an institute of technology so they became the Case Institute of Technology—which gave them the right to use CIT also. And then they became, of course, Case Western Reserve, which deemphasizes Case's role in the whole thing.

Anyway, the Kármán point of view has broadly influenced the fluid-mechanical, gas-dynamic, and structural portion of engineering, and it certainly has had its influence on the environmental-engineering work. So that, I think, is important.

Now, hooked on to the Guggenheim Aeronautics Lab—to the south of Guggenheim—was the old Hydrodynamics Lab. Robert Knapp was the strong dictator of that area. He was one of the only people to carry on highly classified work associated with torpedoes and submarines and such during the war.

COHEN: Did Caltech have a policy on this after the war?

MARBLE: Whether to be classified or not? There was a policy. These wartime Navy contracts were very, very highly classified—although I should say that, looking back on the wartime years at NACA and other military organizations—the notion of security was very vague. I mean, we had no idea how insecure our security was, and we had no idea whether a given piece of work should be classified or not. So, when in doubt, classify it. And therefore much of the work we looked at in the Hydro Lab was classified, but probably there was no real serious overriding reason for it to be so.

Anyway, during the war the navy was very concerned that this work should not become public knowledge, and that enemy agents should not be able to get at it, or anything of this sort. So they insisted that we have an armed guard. Now this was before my time, so it's hearsay. But I think it's reliable in spirit, if not in detail. [Laughter] Anyone who was around at that time will remember that we had one security guard, Officer Newton. Officer Newton was a jolly fellow. I think he was probably either retired or from the Pasadena Police. And the students loved him, and they loved the opportunity to play pranks on him. Officer Newton was the brunt of many, many student pranks, such as a bag of water dropping on his head from the third story of the mechanical-engineering building—things of that sort, which would irritate him, but he knew he had the students' love.

He was the only guard they had. So the question came up of giving him a gun.

COHEN: Did you have to show identification to go into the building?

MARBLE: Evidently. You had to show identification to go into the Hydro Lab. He was the guard for the whole campus, but in order to make the Hydro Lab legal he had to have this gun. Well, Robert Millikan took a dim view of that, and he said, "Well, he can have a gun, but it can't be loaded." So Officer Newton spent the war years as the armed guard at Caltech with an unloaded six-shooter. [Laughter]

Anyway, the Hydro Lab sort of saw its final years after Bobby Knapp died. He died at a rather early age—he couldn't have been much over fifty—of a massive heart attack. As a consequence of that, the Hydrodynamics Laboratory saw rather poor years. Nobody was hired to replace him. It was run by a committee that consisted of Milt Plesset, Duncan Rannie, and Aladar Hollander. And as you would expect, a divided management didn't work at all, because nobody managed. And the consequence of this was that the lab eventually became not a great influence on the campus, or with the navy, or anything else.

Now, in the 1960s—and this was one thing that had persuaded me to stay at the institute, because in the late 1950s, when I was making up my mind whether to stay or not, it was rumored that Aerojet General Corporation was going to give a laboratory to Caltech in Kármán's name. In the early sixties, this happened, and the laboratory was completed in 1962, I think. And that added a second and third floor over the Hydrodynamics Lab. So that laboratory now consists of a basement, a sub-basement, and three floors. And at this time also, the Firestone Laboratory of Aeronautics was built and occupied. So the aeronautics faculty moved out of the second floor of Guggenheim, and the Jet Propulsion Center got the western portion of the second floor of Guggenheim, and the entire second floor of what is the Kármán Lab. And that's been our home since that time. Although it's part of what they call the GALCIT complex, it's recognized as the Jet Propulsion Center's abode. GALCIT is put together in a very strange way; there's a total of four buildings, four separate structures. There's the Kármán Lab; and the old Guggenheim Lab; and what they call the Guggenheim Annex, which is to the east of that about fifty feet; then the passage over the driveway to the Firestone building, the fourth structure. So when earthquakes happen, all the patches between these various buildings move and all the floor has to be replaced there each time. [Tape ends]

Begin Tape 4, Side 2

COHEN: Who was in charge of aeronautics at that time?

MARBLE: There was sort of an interesting development here, which I was a passive witness to. Von Kármán, who'd been the director of aeronautics, rather hoped that even though he was away from Caltech for long periods of time—first because of business for the Army Air Forces and later because of his AGARD and NATO work—he could remain the director of the Aeronautics Lab—of GALCIT. But when Dr. DuBridge became the president of Caltech in 1946, he was really interested in a tighter organization. You see, at that time, Kármán was the director and Clark Millikan really did all of the work. He was the one who took care of all the paperwork, saw that people were paid, saw that contract reports were done, and all of that. And I think, deep in his heart, Clark Millikan thought that Kármán ought to retire and he ought to be made the director of the lab.

Kármán was very good at avoiding this. And I don't think I mentioned this before, but in 1949, when I was with von Kármán in Paris for this first meeting on astrophysics and gas dynamics, Lee DuBridge was in Europe, too. And he had written Kármán, or had called Kármán, and said he'd like to have lunch with him on a certain day. And Kármán said to me, "Now, Frank, you know what DuBridge wants to do. He wants to retire me. I do not particularly like that idea." So Kármán, with the manner he usually had—something clever—agreed he would have lunch with DuBridge and that they would meet at a certain place at a certain time, which they did. But Kármán proceeded to invite ten other people, including me. So the lunch was at a restaurant with a large table, almost a conference-room table, with DuBridge at one end, Kármán way down at the other end, and five other people on each side who were interested in very different things, talking to Kármán and talking to DuBridge, and talking to each other. So it turned out that Lee had to get a plane out of there before he ever really had a chance to talk to Kármán about retiring. [Laughter] But, of course, eventually it came to pass. Kármán retired, but not enthusiastically.

COHEN: Did he still maintain his home in Pasadena?

MARBLE: He maintained his home in Pasadena—1501 South Marengo. So Clark Millikan became the director of aero. And he died in 1966—I think of a kidney attack or something like that—at what I would consider a very young age. He was around sixty-two or sixty-three. Then aero was without a director.

Now Ernie Sechler had really been taking care of a lot of things. Even when I came here, he was the one who took care of students. He managed all the graduate students, the records, what their status was, what their programs were. He signed off on everything like that. And he had a lot to do with the management of the contracts. So he just sort of automatically took over, in his quiet way. He was not a man of great loud words or anything. He did his job and he did it well.

But a great problem developed. There were three strong people in aeronautics at that time: There was Sechler, there was [Hans] Liepmann, and there was Lester Lees. Tsien had persuaded Lees to leave Princeton in the early 1950s and come here and run the hypersonic wind tunnel, which was a newly completed tunnel, financed by the Air Force.

Now, there's no question in my mind that deep in his heart, Liepmann yearned to be the new head of aero. And the successive two or three or four years, I think, are not years that do credit to aeronautics. This was in the early late sixties. The infighting, the innuendo, the difficulties, the personality conflicts, that took place among the three of them was not a picture that was very happy.

The first thing was that the entire business bothered Ernie Sechler to the point where he retired. He just stepped down. He was a very useful faculty member after he retired, but he decided that the fight to maintain a position he did not yet have wasn't worth it.

Lester Lees and Hans Liepmann, although they had been friends when Lester first came as a student in the forties, developed very different points of view. I wouldn't say that either of them handled this conflict with the kind of rules you would hope would apply in these conflicts. But the upshot of the whole thing came about when Lees decided he would become head of the Environmental Quality Lab. He left aeronautics and became professor of environmental engineering. That was a new area. And that left the way clear for Liepmann to be the director of aero, which was what he desperately wanted. And although things that happened under his directorship were fine, and had great scientific merit, still the breadth of interest in the aeronautical problems was greatly reduced, because Liepmann's horizon was very different from what Kármán's or Millikan's had been.

When Tsien arrived here back in 1949, Kármán's office had been available, and Tsien moved into it. Upon Tsien's leaving, in 1955, Duncan Rannie moved into it. In 1962, we made Clark Millikan's old office the secretary's office, which opened on each of the two adjacent offices, and I had the office to the extreme west, which had been Homer J. Stewart's office until 1962, when he moved into Firestone. So this was the situation there after building construction was complete. And Rannie retained the directorship of the JP Center, which was separate from but intimately associated with aeronautics, and also the Goddard Professorship.

FRANK E. MARBLE

SESSION 5

March 10, 1994

Begin Tape 5, Side 1

COHEN: I think it might be a good idea, as you have suggested, to talk about your work and how that proceeded through the years and formed the framework for what followed.

MARBLE: Yes, I think that's sort of the plan that I would like to follow for a while, because regardless of whether the work was done here or was done at my first institution, Case School of Applied Science, or the NACA Laboratory, or JPL, or in some of the trips I've made away from Caltech for short periods of time, the work pattern has been a consistent one. I think it's probably a good point to try to draw a contrast between the kind of work that somebody in engineering science or scientific engineering does, draw a contrast between that and what somebody in science does. Generally, I think when you are in a basic science, if you are going to accomplish something you have to fasten on to a fundamental problem, a fundamental issue that deals with the foundations of that science. Usually, any scientist follows one of these problems for long periods of time. That is, actually, may follow it for his entire life, or he may have two or three different items he works on during his lifetime. But these problems are to a considerable extent dictated by what is current in the science. When high-energy physics was in its prime, for example, there were certain problems that were generally recognized as being important, and if you were to accomplish something, you took a piece of this and did something innovative, either analytically or experimentally.

I think engineering is a more difficult issue. In pure science, one has not only the obligation but the right to pare the problem down to its fundamental issues. That is, to simplify the problem to where the scientific issues are separate from all extraneous effects. Engineering, on the other hand, is essentially dealing with complexity. It may be complex because the device you're dealing with has many parts. Or it may be that the various factors affecting the problem are many. The chore the engineering scientist has is to—without destroying the effect of the complexity—sort out which of the many factors are the important ones and simplify the problem by ignoring those factors that are not primary influences on it. So, in dealing with a piece of engineering science, you're obligated to deal with a very complex piece of equipment or a very complex problem, which you would in no way have formulated from first principles. You will formulate it to some degree of approximation. There is a common term now—people talk about "the model." You make a model of a certain physical or engineering issue. I don't really like that term, but I find myself using it from time to time, because that has become the accepted word for it.

Now, the roots of my own work, really, were when I did my master's degree at Case School of Applied Science. I've indicated before that John Weske was probably the most important influence on my life, as far as my getting into scientific engineering is concerned. And he has continued to be an influence, even only if I think of him and what his view would be on something I'm doing.

In 1940, I began to do a piece of work with him on axial-flow fans; in this case, it was a wind-tunnel fan. And this was a problem we'll call, in a generic sense, a turbo machinery problem. It deals with the flow through rotating equipment whether it be an axial fan, a centrifugal turbine, or whatever.

COHEN: Does this apply only to air-flow? Or would this be water also?

MARBLE: This would be water also—water pumps, water propellers all fall in this field. The general principles that govern them are the same. Now, I was a bit annoyed at John for having essentially forced me this way. But since he was supporting me while I was getting my master's degree—one doesn't fight these issues. I was interested in airplanes and airplane wings, air-flow sections, boundary-layer separation, that kind of thing, which to me was a marvelous field. But he was interested in fans, in turbo machinery. So turbo machinery it turned out to be. I did my master's thesis by measuring the distribution of pressure on a rotating axial fan, a large one. And actually that was the first time that that particular measurement had ever been made. So John and I published that work after I left Case, in 1943, and after I went to work at the NACA Engine Research Laboratory, in Cleveland.

In Cleveland, I was working on engine cooling—again, because it was necessary at the time—on two of the large airplanes, one of them being the B-29. But that was a diversion, actually. Because as soon as the war ended, and we were allowed to move on to the new area of jet propulsion, the turbo machinery problem became a prime issue: that is, the gas-turbine engine, which consisted of a compressor, a burner, a turbine, and a jet. There the turbo machinery components—the compressor and the turbine—were the most essential issues: the compressor because it had to operate in a very efficient way in order to make the device work, and the turbine because it had to be able to withstand temperatures so high that current metals were unable to sustain it. So these were significant problems.

The aerodynamics of the compressor intrigued me very much, because that was something which, as I said, against my will, John Weske had gotten me into. And because of that background, I had an edge on others. I became a local expert in some features of the axial-flow turbine machinery. And at that time, I started to work—even though I was in charge of a large group of people, about thirty-five to forty at that time—I started to work on a problem of the three-dimensional flow, the complicated flow in a rotating blade row.

Now, up to that time, the calculations of flow in turbo machinery were fairly primitive, actually. I would say one-dimensional or very quasi- and unsatisfactory two-dimensional approximation, which we called the radial equilibrium theory. I was rather fortunate because I was immersed in this, and Ora Lee was very tolerant in letting me spend large amounts of time at home working on it. I developed a technique of dealing analytically with this very complex issue. The end of the war came, and the work was not done. So I took my ideas and my initial analysis and thoughts on the work with me when I came to Caltech. And they formed the basis of my thesis for the degree of aeronautical engineer, and a significant section of my thesis for the PhD also. I published this work, and I gave a talk on it in Cleveland in the spring of 1948.

COHEN: Was there nobody here then who was immersed in that work also?

MARBLE: Well, there were two people here who were at least able to understand the problem I was working on. Duncan Rannie was one of them, because he had had wartime experience in turbo machinery at Northrop. But Duncan had his own fish to fry, really, and I think he saw me when I came here as more of a competitor than a coworker. I was never his student in any way.

The other one who understood quite clearly what I'd done was Homer Joe Stewart. Now Stewart was not active in this area; he was really principally active in rocketry and missile development. But he was a fluid mechanicist of very great insight. He had a very strong and quick mind. Many of us think he probably had the quickest mind of anybody around here. The fact that he didn't utilize it to the highest efficiency is just part of his nature. He was more interested in, I think, a baseball game sometimes than he was in anything else—but a marvelous and highly intelligent man. He understood. And he confirmed my ideas. And his influence on Clark Millikan helped to give Millikan the courage to finance me so I could get some of my calculations done.

And while Hans Liepmann was my thesis advisor, his judgment of my thesis was entirely based on what others—namely Stewart, Millikan, and a couple of people in mathematics—said that it was exactly what it should be. So Liepmann said, "Fine." My aim in working with Liepmann at that time was that I had a great interest in the very basic turbulence problem—fluid turbulence. And I had thought I would try to do parallel work in turbulence theory with Liepmann. The fact that this never worked out completely probably had, in the long run, very little effect on what my research program turned out to be. I continued to develop this turbomachine work after my PhD thesis, until about a year after Tsien arrived here, which was in 1949. So I would say I continued it until about 1951. Two other papers came out at that time. And I started writing a large chapter for the Princeton Series in High Speed Aerodynamics and Jet Propulsion, which was published eventually in a book titled *Aerodynamics of Turbines and Compressors*. And that, more or less, established me as an authority—at least the industry was in such a state that it looked authoritative. [Laughter] It's a little bit like the one-eyed man in the country of the blind, I think. But I think I did gain, from that, a reputation in the field.

I became thoroughly involved in the combustion work and the Jet Propulsion Lab. I've said that when I went to JPL it was not my first choice, but it was Tsien's first choice, and he had the dollars. So I changed my field to combustion, and made it my business to find out what, in this particular field, I could do that would make a mark.

Now, combustion is a very complicated field. It's one that deals with the chemistry of exothermic reactions: that is, for example, the reaction of a fuel with an oxidizer—such as gasoline or kerosene with air—and the release of large amounts of heat, and all of that. And the chemistry was reasonably well understood in principle, although chemical reaction rates were not very well known. The measurements of those were very complicated, and most of the known chemistry of reaction rates was really for those that took place much slower than those in combustion chemistry. So the chemical aspect of it intrigued me a great deal, and I spent a good bit of time learning about that.

Then, I thought I could bring something novel to the gas-dynamic part of it. Combustion also has to do with the flow in gas, which is burning. That is, you may mix the fuel in the air, but it's in a flowing system. It may be flowing at low speeds, it may be flowing at high speeds; the fuel and the air have to be mixed, and the fluid-mechanical mixing process is a complicated one. And then there's the issue of how you ignite the gas. Everybody equates it with putting a match to a gas jet, where gas mixes with air and will start to burn, sometimes enthusiastically. Practically, though, this problem is a very complicated one, because the fuel may be injected into the air as a spray. It has to vaporize, mix with the air, and then in some way or other be ignited.

I felt I could bring something to these gas-dynamic issues, so I started a program that had to do with the stabilization of flames—how you hold flames to make the combustion take place when and where you want it. For any combustion process that's sound in an engineering sense, you have to be able to control where the combustion takes place. This is done primarily by putting blunt objects into the stream, such as a sphere or a flat plate perpendicular to the stream, or a cylinder. And the flow goes around these and separates from the surface, creating a slow-moving wake, and it is this region in which one performs the task of igniting the fresh-mixed gas.

Now, as I've said, combustion problems have not only the usual gas-dynamic variables but also the chemistry issue. I set about in this field to try to separate the two. As I've said, engineering problems generally have great complexity to them, and this was a problem that had great complexity to it. And you could make progress only if you could separate it into issues where the fluid dynamics dominated, issues where chemistry dominated, and then small regions where maybe they were both important.

Now, without going into the details, I first carried out an analytical study with Tom Adamson. Tom Adamson was a student of mine, and he got his degree in aeronautics around 1953. He was supported by the work at JPL. He was my first combustion PhD student; I'd had others in turbo machinery. Tom was very good, and we worked together enthusiastically on this and published it. And it was an important step, because, in a way, it was the first piece of combustion theory that involved enough gas dynamics to qualify as a gas-dynamics study. I used what is commonly known as boundary-layer theory to simplify the problem in the region where both the fluid mechanics and the chemistry were important. And this caused a bit of a stir, and formed a start, a source, for many papers—a few by me, but many by others, which elaborated in great detail over the ensuing years. So, in a way, our work performed the same function that the papers on three-dimensional flow in turbo machines did. I established a reputation in this field; and Tom did, too, because he went to Michigan as an assistant professor subsequently, and he eventually became the head of aeronautics there. It shocks me when I realize that he retired three years ago. [Laughter]

The experimental counterpart of that flame-stabilization issue was done with the great help of Ed Zukoski. Ed, as I've said, was an experimentalist who had great judgment in how far he should go in detailed instrumentation and how far we should go on clever experiments. He and I worked together very, very well. We worked more like two coworkers than like an advisor and a student. Ed subsequently became a faculty member here at Caltech, and he is now thinking he probably ought to retire also. [Laughter] It's always a shock when your students start to retire.

Ed and I published three really important papers on the structure of the flame-stabilization problem on buff objects. Just to describe it in a few words: A buff body supporting a flame in a flowing stream of combustible gas possesses a wake—that is, a region extending downstream of a cylinder, for example—which consists principally of hot combustion products. Now, these hot combustion products, in a very crude sense, perform the same role that the match does when you hold it to a natural-gas stream. They perform both the thermal and the chemical source for the ignition of the fresh gas that flows by. And we were able to separate out the gas-dynamic structure of the wake from the chemical issue of the ignition process, in a way that allowed us to state very clearly the criteria for being able to produce a stable flame on buff bodies. And this eventually had a considerable amount of influence. It allowed us to do what we call "scaling." That is, from an experiment using a given piece of equipment and a given fuel, we were able to say how a different piece of hardware using a different fuel would perform too. So the people—who were designing burners for gas-turbine engines and ramjets, and afterburners for military engines—were able to utilize our work to determine the size of the flame holder and velocity of the air stream in which the flame-stabilization process would take place. So that was a very successful effort. And I would say it went on for about eight years—roughly from 1952 to about 1960, which was after I had left JPL and come back to the campus.

At JPL, the Army Materiel Command was able to support a certain amount of basic research, until JPL became essentially an extension of NASA. Up to that time, a certain fraction of the total money for missile development, such as the Corporal missile or the Sargent missile—I think it was about 20 percent—was set aside for fundamental research.

COHEN: So you didn't have to write up proposals and run around finding money?

MARBLE: No, the funds came to the laboratory, and the decision on what they would spend it for was made within the lab. So I really had to sell it only inside the lab. And we had done good enough work so that this wasn't any problem. I look back with great envy on the ease with which we funded things in those days. [Laughter] So I think the flame stabilization was an important thing, and the scaling of flame stabilizers.

Another issue had to do with instability. It turns out that combustion processes, even though you are very careful in confining them to a certain region, can develop unstable oscillatory properties. These are usually involved with the interaction between strong acoustic fields—strong sound waves—and the combustion process itself.

Now there's a certain interaction between the acoustic waves and the chemistry. Many times, it's minor. The important thing in the cases I became interested in had to do with the interaction between the acoustic waves and the gas-dynamic flow. The problem that attracted me first was called "screech." And if you've ever heard it, why, you'd agree that it was very aptly named. I think it was recognized first by the Pratt & Whitney division of United Technologies and the United Technologies Research Center in Hartford. It was probably recognized in 1950, '51, '52, along in there. And I became aware of it through some contact with them. Now, again, it was a very complicated issue, having many, many variables, very many processes going on at the same time. And I persuaded a young student named Don Rogers to duplicate the piece of equipment in which the United Technologies Research Center had originally observed the "screech" phenomenon. And he spent three or four months trying to get the thing to screech. And when it did screech he came into my office with such a white face, I thought I would never get him into the laboratory again. It scared him almost to death.

The phenomenon is very, very noisy. I think we measured 160 decibels in the test cell, which is enough to do away with one's eardrums in short order. But we were able to determine precisely what the mechanism was. And again, it separated into a gas-dynamic problem and a chemistry problem, so that we did not have to deal with the two of these at the same time. The gas-dynamic problem had to do with the transverse high-amplitude acoustic wave, which moved the gas-flow stream transverse to its direction of flow, creating vortical structures which in turn rolled up combustible gas which burned in the vortex core. And this was something entirely new in the combustion field. It was the first really clear-cut mechanism for supporting combustion instability—high-amplitude, destructive combustion instability—in air-breathing gas-turbine engines.

We were interested not because of the noise but because the acoustic waves reached such a high amplitude that the heat transfer between the gas and the containing vessel, whether it was an afterburner or the engine itself, became so high that the surface would melt and the entire burner would be destroyed. The issue was to preserve the equipment itself.

At the same time, the instability problem in rocket combustion was an important one. And I got involved in that with a student named Grant Brownlee, whom I mentioned before. He was a Canadian, who came and did a very, very fine series of highly controlled experiments on the instability in solid-propellant rockets. This was completed after I had left JPL and was eventually published in about 1960.

In the meantime, I had gone back to my first love, the turbo machinery work. This was around the time I took my first leave of absence, to go to Cornell and work there; that was in '56. So I still had the work at JPL, but I was getting involved in turbo machinery work again. I knew that Tsien was eventually going to leave—and he did leave in September of '55. And I had the feeling that whether I liked it or not, it would be necessary for me to leave JPL and come back and help fill in the gap that would be left when Tsien was gone. So I knew I would be coming back to the campus. I knew it would be very difficult—perhaps impossible—to continue the combustion research work. So I began to work on turbo machinery again.

At that time, it had been noticed, and I believe it was first noticed by Professor Howard Emmons, at Harvard, that when compressors stalled—which means they reach the peak pressure they will pump, and then if you reduce the flow through them, the pressure drops, and this can be accompanied by all sorts of disastrous related effects, such as surge—Howard Emmons found that this process didn't occur in a uniform way. It occurred as little cells, which would stall and propagate around the fan, or the compressor stage. And I think the best analog I can give to it is, we've all probably seen an open window with a Venetian blind over it. And sometimes you see little ripples of variation go up or down the Venetian blind. You open the window, a breeze comes in, and these slats in the Venetian blind deflect. That blind won't stay still; it'll ripple up or down. And exactly the same thing is happening in compressor stall. I saw that, and I discussed it at some length with Tsien before he left. And I began to work on a theory of that, which, while it took very many turns and two or three pieces of work, I think I eventually did it right. And I believe I did it right in about 1955—'56 maybe—along in there. I should say that propagating compressor stall has become one of the most important issues in gas-turbine work, the turbo machinery work in gas turbines, even at the present time. And I believe my work at that time was important in pointing out that regardless of how you treated the complicated flow field, you must treat the blade characteristic, or the actuator-disk characteristic, as a nonlinear function of the flow rate through it. Without the nonlinearity, the flow field is not described adequately or properly. And I was the first one to discover that, and to utilize a very simple but straightforward nonlinear characteristic.

This compressor-stall work continued, then, for some years—both analytically and, to a certain extent, experimentally. Duncan Rannie had built a good-size, three-stage compressor in mechanical engineering, sponsored by the navy. And it was a very good research compressor. The speeds were fairly low; it had good access for instrumentation. And as Duncan began to lose interest, or run out of problems he was interested in, I had access to it, and I developed an experimental stall-and-flow- distortion program in there. This was in the Thomas Laboratory. And I should say that when I first came down from JPL, it was before the Kármán Laboratory was available. So I was in the Thomas building; my office was on the second floor of Thomas at that time.

Then a couple of things happened. One was that the Navy decided they didn't want to fund our compressor work anymore. Personally, I think Rannie didn't fight hard enough for it. But they decided to cut his funding, and I was utilizing some of that at that time. Also, I had separate money from the Air Force Office of Scientific Research to support it, and several very good PhD students. But the navy decided not only to cut the money but to take the equipment. So they took the compressor and shipped it up to the Naval Postgraduate School in Monterey, where it still runs. This left us without the experimental counterpart to the analytical work I was doing, and I felt—and time has proved me right—that there's no point in trying to conduct analytical work on a complicated engineering issue such as that without having the experimental counterpart to it. If you don't have that, why, you may go off in left field before you know it. So I realized I had to change the problem a bit, or else go someplace where they had a good turbo machinery lab.

I'll just mention in passing that that opportunity came up. Dr. C. S. Draper at MIT, who was head of aeronautics there at that time, was very much interested in having me come there and take over the Gas Turbine Lab, which was sort of running out of air because the man who was the head of it—the marvelous Edward Taylor—was not as active as he had been. He was getting older, and he wasn't quite sure what the laboratory ought to do. He had not been completely immersed in the new gas-turbine work. And I think both he and Draper thought somebody ought to take that over. And interestingly, the way it worked out, I did not go to MIT. But about four years later, my student, Jack Kerrebrock went, and he became in two or three years the head of the Gas Turbine Laboratory there and developed it into a place that has excellent experimental capability. And now I spend two or three weeks a year working at the MIT lab. So I go there and sort of talk them into doing the things I want to do. I impose myself on them. And since he's my old student, he's been polite, up to the present time.

Anyway, as far as Caltech and the students here are concerned, a new problem came in. I indicated that in engineering this happens from time to time. What you have to do is to keep your eyes open for problems that occur in a high-technology industry, problems that are unable to be solved in any obvious way. You look at a problem and say, "Now, is that just complicated, and so they'll eventually get it? Or are there fundamental issues there that involve the application of basic scientific principles in a way that hasn't been done before?" And if the answer to that is yes, then it becomes a good subject for an academic research program.

You'll remember at this time, in the late fifties and sixties, there was much enthusiasm for the intercontinental ballistic missile, which meant that propellants to fuel rockets had to be efficient and very high-energy. And this was done, in the later portion of that period, by adding powdered aluminum to the solid propellants, stirring it in, and the aluminum would eventually burn, with very high energy output, to aluminum oxide. It turned out that some of this aluminum oxide, due to the temperature level in the combustion chamber, formed small particles—that is, two-to-five-micron particles—which came out the nozzles as dust. Now, dust isn't a gas. It doesn't behave in quite the same way. So if part of the products of combustion in this thing come out as small particles, they don't interact with the temperature and volume the same way as a gas does, and they don't move exactly with the gas. They tend to lag behind it. And the consequence of that is that the performance of the rocket is significantly less than it theoretically ought to be. And the rocket people at that time were somewhat at a loss to know how they could estimate at the outset what this loss would be.

Well, that got me into a field that scarcely existed at that time. That was multiphase gas dynamics. That is, gas dynamics that involve not only gas but solid particles. I became very intrigued by it, because just looking at this problem opened up an entire new area. [Tape ends]

Begin Tape 5, Side 2

MARBLE: This problem of the gas/particle lag in rocket nozzles opened up a field that was to a large extent virgin, even though solid particles have been transferred by gases always—I mean, like the dust blowing over a field, or the formation of sand dunes. Because the particles themselves have mass, they interact on the gas flow, and change the way the gas moves.

I gave a long talk on this subject, and this was at one of the AGARD meetings—a meeting in Braunschweig in 1962. I gave a talk at that time on the fluid mechanics of gas/particle mixtures. That really was a seminal paper. It was essentially the start of that field. Single studies had appeared here and there, but treating it as another phase of gas dynamics was completely new.

COHEN: You mentioned the fact that you have to watch industry and see what they can't solve. So did you come to this problem by just watching the rocket business?

MARBLE: Well, yes. I was associated with the rocket business through consulting work—for the Thompson Ramo Wooldridge Corporation, but more at the aeroneutronic division of Ford Motor Company, which was an offshoot of Lockheed Missile Systems. They are a large laboratory still working down in Newport Beach—and this issue came up in connection with the work there. There was a great concern about the Minuteman rocket in connection with this. And a lot of work was done in designing the nozzle of the Minuteman rocket in order to minimize the effect of particles. And I had a chance to contribute to that.

I should say, to be completely fair to everybody, that there was an issue of this type in nuclear-weapons work. That is, a nuclear blast wave picks up sand from the ground and transports the sand; this changes the nature of the blast wave. And people who were involved in classified work in nuclear weapons—particularly those at Los Alamos—were very much aware of the importance of the solid phase being transported by gas. I was not aware of that at the time, and I believe there was only one consultant who worked for them who did eventually publish a paper—that was George Carrier. He published a paper on the influence of solid particles on the structure of shock waves. And that was roughly at the same time that I was working on it, although I didn't know about it—nor did he know about my work.

So this set me off on an entirely new track—the dynamics of gas/particle mixtures. And I continued with that for a period of almost eleven years. The thing that kept me on it for so long—and I should say that my contribution was to give, the first time, a complete fundamental formulation of the gas/particle mixture in gas dynamics: that is, the mathematical description of the entire thing. And then to treat about half a dozen typical problems that one could treat, and observe the modification of the gas flow by the particles. This was an important issue.

As I thought I had that done, another issue came up. And I'm hard put to say exactly where it came from, but it had to do with the transport of droplets of liquid, and eventually droplets of liquid which could vaporize or condense. Oh, I know where it was. That had to do with the flow in the low-pressure stages of steam turbines. In the low-pressure stages of steam turbines, water tends to condense and form in droplets; the droplets impinge upon the blades of the turbine and erode them.

In that effort, again, I was able to formulate the entire thing in a coherent mathematical way. And these equations really formed the basis for a lot of subsequent work—some by myself, but very much with others.

One interesting aspect of that had to do with the propagation of acoustic waves. I thought I'd exhausted my enthusiasm for multiphase flow fields. And I thought I'd done what I could do, and anything else I'd be sort of straining at it. Then I was asked to be part of a committee looking at the generation of the noise by supersonic transports. You recall that in the late sixties, early seventies, there was a supersonic transport being developed by Boeing. There were two or three issues. I guess the main one was noise—both from the sonic boom and from the engine at take-off.

Well, the jet noise was very well treated. That had been investigated thoroughly, largely by M. J. Lighthill and his students, in Manchester, England, and then a few others. Lighthill formulated the issue very, very well. And his formulation still holds to the present time.

Several of us went to Boeing and saw the engine, listened to it, and saw what was happening. And there was one thing that troubled me—and troubled them, too. And that was a very-low-frequency noise. Noise which I'd say was of the order of fifty cycles per second. It was one that you wouldn't expect, gas-dynamically, to come out.

I became convinced that the gas turbine itself and the engine nozzle were contributing to it, and that led to a study of acoustic noise generated by entropy waves, or temperature waves, passing through a nozzle. This was a study by me and my wonderful French student Sébastien Candel. And between us, over a period of two or three years, we solved that problem. And it's a very interesting one.

Having had combustion experience, I knew that the gas coming into the engine's nozzle was not of uniform temperature. There were probably gobs of hot gas, followed by gobs of cooler gas, coming down the duct to the nozzle. When they came out the nozzle, these temperature changes changed the rate at which gas mass could flow out of the nozzle. The temperature changed, the pressure changed, and we got fluctuations, or pulsations, in the flow coming out of the nozzle. And if you took the flow, that is, the velocity through the nozzle, and the size of what you thought these gas masses would be—they'd be like spheres, corresponding in dimension to the diameter of the pipe leading to the nozzle—well, you got pulsations in the frequency they were interested in: that is, of the order of 50 to 100 hertz, in along there. So that was the birth of my new interest in acoustics.

Now, the question is, How do you kill something of this sort? How do you deal with troublesome acoustic waves? In transport engines, they were a limiting thing at that time. They still are, in Orange County; at the John Wayne Airport, planes are forbidden to land and take-off at certain times simply because of the noise. Part of this is moderate-frequency noise—500 hertz, 1000 hertz—emitted from the front of the engine. Some of it is emitted from the back of the engine.

I had noticed—with one of my last students in multiphase flow, a fellow named David Wooten—that there was a completely new and a little complicated mechanism of attenuating noise at very low frequencies, which involved the sound wave alternately evaporating and condensing the liquid droplets. That is, the pressure and temperature rise in the wave, and the alternate cooling and expansion of the wave, condensed liquid on the droplets and then vaporized it away from them. And this constituted, in certain cases, a large removal of acoustic energy.

So I worked on this and got support from the Department of Transportation. And I guess it was the first thing that attracted enough attention so that I had to give telephone interviews all over the country—and all over the world, as a matter of fact—on how effective I thought this mechanism would be. Would it really remove the entire problem of acoustic noise from airplanes? And I had to tell them, “No, that was a little bit more enthusiastic than it should be; it wasn’t as easy to do as all that.”

That provided another piece of work, which lasted about eight years. I think those problems—the problem of the thermally-induced low-frequency noise from nozzles and the absorption of acoustic energy by condensation and vaporization of droplets of liquid—occupied the better part of those years.

Just an interesting side issue here: David Wooten, who is an extremely personable fellow and a real gentleman, stayed in the research field for some time, did some consulting for some time. And now he has taken on a job I would never have figured him for; and that is as the manager of the Balboa Bay Club, in Newport Beach. In fact, he’s the manager of a lot of bay clubs, because I found out, after we went down to see him when he assumed this august role, that they had clubs in other parts of the world, too, which they managed out of the same area. He says, “Oh, I’m keeping my hand in. I do a little bit of consulting.” But I said, “This consumes most of your time.” “Yes, it does,” he said. “It’s a pretty good life.” [Laughter]

And a couple of other people were involved in this acoustics work, too. One of them I mentioned—Sébastien Candel. He and I were the ones that first laid to rest this issue of the thermally-generated noise in choked nozzles. Sébastien Candel was a French student. He came here and did his PhD. And he was noted, in France, as one of their brightest students. The French became very annoyed when Sébastien came here to get his PhD. We had a marvelous time. He and I got along very, very well; we had a great time here. He went back to France after he finished. I wanted him to stay here, but he wanted to go back to France. It turned out that they refused to recognize the PhD from Caltech, because to get into the academic system there, you had to pass the so-called *Docteur d’Etat*—that’s a restriction which has been removed since.

So two things happened to him: They were so mad at him they wouldn't excuse him from his military service. Usually, they allow good students to go to work at a manufacturing company or a university, or something like that, and this is counted as alternate service. But they were annoyed enough at poor Sébastien that he was sent into the tank corps and spent eighteen months chugging around in a tank close to Orléans. I guess his compensation for that was that he got married during that period. And then he had to write a thesis again, for the *Docteur d'Etat*. So he took his PhD thesis, reworked some of it, elaborated on it, and submitted that, and they accepted it. Now he has risen meteorically in the academic and industrial framework of France. He is one of the most important people in aerodynamics, particularly combustion and acoustics. He's head of the aerodynamics laboratory and the curriculum of aerodynamics at École Centrale des Arts et Manufactures, which is outside Paris. It's down in a little place called Chatenay-Malabry. He's there, and I've spent time with him there. He actually spent time here this summer. But he cut his teeth on that problem, I should say, and I think between the two of us we established thermally-generated noise as a new source of sound.

COHEN: This would have been in the early seventies?

MARBLE: This was in the early seventies. My venture into acoustics was from 1970 to 1978.

I went to the University of Cambridge for a year in 1972-73, and I met a young man there named Nicholas Cumpsty. He was at their turbo machinery laboratory, and I spent some time at that laboratory, which is now called the Whittle Laboratory. He was interested in noise generated in turbine compressor stages. And I suggested that some of it could be due to the non-uniform temperature in the turbine coming out of the main burner.

Well, there was a big argument about whether the temperature coming out of the main burner of a gas turbine engine was non-uniform or uniform. And I said, "I know enough about combustion, Nick, that I'll say it's non-uniform." But he didn't have much confidence in that, and he went around and asked the people at Rolls Royce, and they said, "Oh, perfectly uniform, just a few degrees of fluctuation." I said, "That's just not so; they just don't measure it properly."

Well, there was finally a man named Ray Dills, who was at Pratt & Whitney. He was really an unusual person, because he was entirely his own person. And between Nick Cumpsty and myself, we got out of Dills a high-frequency-response record of the temperature coming out of the main burner, and the fluctuations were about the size that I'd said. I had said that it fluctuates almost from the temperature of the unburned gas to flame-temperature gas, or almost a factor of two in the temperature. Since the temperature going into these choked turbine blades was fluctuating, the flow rate through them would change; consequently, they would generate noise. And Nick and I—he bore the brunt of the numerical calculations—between the two of us, we took this mechanism and developed essentially a theory for what is called the internal noise of gas-turbine engines. It's a component of noise which is not from the jet and not radiated out of the front of the engine. It's gurgling around on the inside. And Nick is one of my close friends and spends a lot of time in the US. He was here at Caltech for a year, also. He got his doctorate from the University of Cambridge.

That takes me up to about 1978. In that period, I traveled a lot, lecturing; I had all this background of stuff, which people wanted me to talk about. And I'm afraid it eroded my creativity for two or three years. I had a few students, but they were involved in problems that were essentially left over from the fields of acoustics and two-phase flow.

COHEN: What was happening at Caltech at this time? Were there new people coming into the department?

MARBLE: Faculty, no. But what I mean is that the students were working on problems that had been generated in the years from 1970 to 1978. And there was a period of about three years there in which I didn't start anything new, partly because I was away on these trips, regurgitating, if you will, a lot of the things we'd been doing for the last fifteen years. [Laughter]

But I realized that had to change. And a very important thing happened. In aeronautics, [Anatol] Roshko and [Gary] Brown—I can't remember really whether Gary Brown was a PhD student or a postdoctoral fellow—they began to recognize that the turbulent flow in a mixing layer, which we hadn't thought of in great structural detail over the years, contained in it large structures that maintained themselves. That is, vortical structures that maintained themselves. And the unique thing Roshko and Brown did was to follow the flow at a speed different from either of the streams in the mixing layer; they followed the flow, and in that way identified the fact that these vortex structures maintained their lifetime for a long while. They were essential to the structure of the flow.

Once Roshko and Brown observed this, it became clear to me that the turbulent mixing regions in combustion problems were of that sort. What was happening in these mixing regions—which I had worked on at JPL in the early part of my life—was that a vortex mixed, or tended to roll up and mix, a fuel and an oxidizer, or a fuel and oxidizer on one side and a hot gas stream on the opposite side; this was the mechanism. And if I did an analysis of the combustion in that vortex structure, I would have the mechanism of one of the essential components in turbulent burning.

So I started in 1981 to work on this. And I was very, very fortunate, because I had chosen just the right thing. It developed quickly and almost abruptly into a field. It was done in 1982. This was due to a number of crazy events. I made the mistake of agreeing to publish it in a book honoring Luigi Crocco. I mention him because he was Tsien's counterpart at Princeton. Crocco was a dear friend of mine. We had many wonderful times together in Italy and France, as well as in the US, but mostly in Europe. And when they wanted to write a book and dedicate it in his honor, I said, "All right, I'll submit this." Well, delays and confusion kept the book from coming out until 1985—whereas if I'd published the work in a respectable journal here, it would have been out in 1982 probably, or 1983.

Be that as it may, this problem had very much the effect that the so-called Marble-Adamson problem had—that is, this combustion in the mixing layer. It started an avalanche of other people's work in the same field. In fact, in 1988 there was a whole session in one of the combustion symposiums given over to studies on this particular problem. So it not only contributed the scaling laws, which I developed and which have held up over the years, but it also formed an inspirational nucleus for other people to elaborate on and extend. It was just my great good fortune to put my finger on the right thing, and that was it. It was inspired by a lot of things that developed in the fluid-mechanics and gas-dynamics area at that time.

That, and the extensions to that, kept me occupied for probably the next four or five years. I'd say between '81 and '86, I was primarily involved in elaborations and extensions of this vortex-combustion theory. And several of my students worked on it. One of the notable ones—actually, it's unfortunate to have to say it, but this was my only female PhD student—was Ann Karagozian. She worked on this, and did a beautiful job. She was a magnificent student.

And then she got a faculty position at UCLA, and she's now a full professor of aeronautical and mechanical engineering there and really establishing herself in the country as a highly organized researcher and academician, and I'm very, very proud of her. I go over and see her from time to time, and spend a little time at UCLA with her.

This next thing will take me up to the present time. And it occurred in the following way: Over the years, there have been propositions made for high-speed, hypersonic flight with air-breathing engines. Air-breathing engines are engines that get their oxidizer from the air. They carry the fuel, but the oxidizer is the air. A rocket, on the other hand, contains both its fuel and its oxidizer. You need that for getting out of the atmosphere, where you don't have any air to work on.

But there have been proposals—and the first one that I know of in detail was about 1953 or 1955, along in there—for building very-high-speed airplanes—that is, airplanes that would eventually fly into orbit. The last go-around on that was the National Aerospace Plane—NASP. This was conceived in the early eighties. And it's the third time that the proposal came up. It came up first in the late fifties, from Tony Ferri. Ferri was essentially an Italian refugee from World War II. He was brought here by NACA and worked at Langley Field. After that, he went as a faculty member to the Brooklyn Polytechnic Institute. Ferri was one of these impossibly energetic, inventive, intuitive folks. And I'll say also, he was utterly fearless. The fact that something was impossible didn't stop him at all. He was just a marvelous, enthusiastic, invigorating person to work close to. He was the first one, I think, who proposed this in a practical way. He did work on combustion processes and mixing processes that would have been appropriate for that kind airplane, which would fly up to speeds that would get it into orbit or close to being in orbit. And then that proposal eventually died—I think partly because Tony's health became poor. He had a number of reverses in his research program on it. And the Wright Aeronautical Corporation, which had been egging him on, folded up. They went out of existence around that time.

It wasn't until the mid-sixties that another crack at this came about. And I believe this was probably fostered by the Wright Patterson Lab at the Air Force base in Dayton. They are the ones who funded it. They did a lot of in-house work. And Westinghouse and General Electric were big contractors of theirs. And they did some fairly substantial work in that field.

Usually what the idea came up against was the block due to materials. That is, the temperature that the materials would have to withstand and the cooling capabilities were such, that it just wasn't a practical thing.

Well, in the early eighties, there was a group of people who became convinced, for one reason or another, that the materials were there and all that was needed was a good enthusiastic effort. This took the form eventually of the National Aerospace Plane. It was promoted by Tony Dupont, aided and abetted by a man at DARPA—the Defense Advanced Research Project Agency—Bob Williams. Williams was a visionary and an organizer. And while his technical judgment was not always sound, he listened to people whose judgment was reasonably sound, even if overenthusiastic. And he mustered the means to get something done. So the effort really was pretty much spearheaded by Williams. And subsequently he's been damned by people who felt that the money spent on it was wasted. But I think they miss the point, in a sense, even though the National Aerospace Plane is now held in abeyance. The general idea, and the work that was done on it, advanced the technology to a point where in later times, if it proves reasonable, I think a version of this can be made and be very, very useful. It was a great technological accomplishment.

Now, one of its problems had to do with the engine. It's a ramjet engine—it was called in the parlance of the times the scramjet engine. I should say that a ramjet engine is one that works without any turbo machinery. The compression process is entirely done by slowing the air down from the stream. If you're going at high enough speed, you can get very high compression ratios just by taking the air from the stream relative to the engine and retarding it, slowing it down with respect to the engine. The pressure will go up a great deal, and it accomplishes everything that a mechanical compression process would.

The ramjets themselves have never been the most successful of engines; eventually a gas-turbine engine came along, and that did the job much better—or at least as well—and it was preferred.

The scramjet engine is a ramjet engine in which the combustion process is carried out at supersonic speed. So the acronym—Supersonic Combustion Ramjet—led to "scramjet." The problem here turned out to be one of mixing the fuel in the air rapidly enough. Without going into the sordid details of the whole thing, the gas becomes very hot when you slow it down. And it's still moving very, very fast. So with the length of engine that one could possibly keep cool at Mach numbers of 16 or 18, you would have only about one millisecond to mix and burn the fuel with the air. And the fuel was hydrogen. Hydrogen fuel was chosen because it was the best coolant. Storing it as liquid hydrogen gave it great capacity for cooling the surface of the airplane and the inner workings of the engine, and compensated for the fact that the materials we had were certainly not able to withstand temperatures of the gas going through them. So that consideration dictated the choice of fuel.

But how to mix this gas rapidly with the air? A lot of work had been done on injection, the mixing of hydrogen. But primarily it had been done by things that stuck out into the stream. And these injectors, or struts, that stuck out into the stream suffered from the same problem the engine did. That is, they couldn't withstand the temperatures—even with the liquid hydrogen flowing through them. So something was needed which, first, would mix very rapidly, and second, could be injected from the side of the engine through the cooling wall.

Well, again, there'd been a piece of basic research done many years before on the passage of shock waves through regions where the temperature was non-uniform. This was work that had to do with the attenuation, or the action of sonic booms coming through the atmosphere, where the temperature isn't uniform. There was a hope, you see, that if the temperature in the atmosphere was sufficiently muddled up or spotty, then the sonic boom from the supersonic airplane would be attenuated and wouldn't trouble people very much. So they could fly supersonically over the land, not just over the sea. And consequently, there were some detailed studies of the interaction of shock waves with regions where the temperature was not uniform, or the gas was not uniform.

Well, the mechanism of the interaction of acoustic waves or shock waves with non-uniform gas regions where the density changes over the interface between the two media is a mechanism for generating vorticity, and very strong vorticity, at the interface between the two media. This is relatively well understood but never used. It was never part of any technological process.

Well, when you have hydrogen and air, you have about as big a density difference between two things as you can, unless you have an artificially heavier medium, a heavier oxidizer. So it occurred to me that we must use the difference between the density of the hydrogen in the air and a weak shock wave passing through it to generate the vorticity and mixing rate which would mix this hydrogen rapidly. And this took the form of an injector and generator impinged upon by a fairly weak shock wave which generated strong streamwise vorticity and mixed the gas very, very rapidly.

This was at a time when the university research initiatives were in the process of being formed. So, in 1984, I wrote up a big proposal on this and submitted it to the Air Force Office of Scientific Research. I got word from the contracting officer there, whom I knew. He said, “You know, we’re going to give out two of these things. And we have almost a hundred and twenty applicants for it. So don’t hold your breath, if you think you’re going to get some.”

Well, I did. It came out. And eventually the judgment throughout the military industry was that this was a good thing. So we got a very highly funded research program. It started off at around $800,000 a year, which for us was a good-size program to manage, and grew a little bit over the three-year period. It was eventually renewed for another three years. So that covered a six-year period starting in 1985, which brings us up almost to the present time.

At first, people were very skeptical of it. I was clear at the outset, when I gave my first public talk on this in 1986, that there was great skepticism, particularly among people at NASA/Langley, who had been working for years and years on this scramjet problem. So we got a lot of bad mouth about it. But then, as other people began to say, “Oh, no, I thought of that first,” I realized we were doing real well. [Laughter] And over the years, as our work developed, we were recognized sufficiently that I was given additional money to go to Langley Field and do experiments in one of their tunnels. And that was the time I got acquainted with my last PhD student, Ian Waitz.

COHEN: Somewhere in here, did you retire?

MARBLE: Yes, I retired—legally—in 1989. But Waitz didn’t get his PhD until 1991. So we stretched that a little bit. That work was done… [Tape ends]

FRANK E. MARBLE

SESSION 6

March 22, 1994

Begin Tape 6, Side 1

COHEN: Let's backtrack a little bit. We were talking about your vast scientific efforts through the years. Perhaps you'd like to pickup where we left off last time by saying, again, what you were doing just recently.

MARBLE: In the early 1980s, I became interested in the combustion process in vortex structures, which involved several students of mine, including my student Ann Karagozian, whom I mentioned at the end of the last session. That work came to an end, as far as I was concerned. But it established a problem, which now goes, evidently, by my name, as the Marble problem, which people have expanded into a rather sizable literature, and it continues to motivate and stimulate a lot of numerical computation at the present time. When it gets to that point, I realize it is time for me to bow out of it and get started on something else.

And in the middle 1980s, the concept of the aerospace plane, which was an airplane that would use air-breathing propulsion to orbital Mach numbers, came up. And I think I described before that this was a device that has been reincarnated twice. It was originally proposed in the early fifties. Its first rebirth was in the 1960s; and its second—and perhaps final—was in the late 1970s, early 1980s. As I moved out of the vortex-combustion work, I looked for another area in which I thought I could do some work that was novel. I'll come back to that—the aspect of novelty in my work. But I felt I could do something novel regarding the injection and combustion problem in the National Aerospace Plane.

At the very-high-speed flight—we're talking about flight speeds almost up to that required to put the airplane into orbit, so we're talking about flight speeds of, let's say, Mach 18, or 18 times the speed of sound, which is very, very fast. The problem of injecting the hydrogen fuel into the air and burning it in a relatively short time became an essential issue, because the temperatures were so high that the cooling capacity of the hydrogen limited the acceptable length of the engine. So the problem was to inject and mix hydrogen with air in one millisecond.

What I did at that time was to draw on some work that had been done in connection with the attenuation of the sonic boom—that is, the scattering of shock waves by non-uniform temperature spots in the atmosphere. And it turned out that if you looked at that another way, the interaction of weak shock waves with non-uniform density gases made it possible to induce a vortical motion at the interface between the heavy gas—air—and the light gas—hydrogen. And this vortical motion induced very strong and very rapid mixing.

I proposed this as a university research initiative in the early 1980s; won that, even though the Air Force itself thought I wouldn't. I did win that, and it had one year left on it when I was retiring in 1989. So this was the work, a very exciting and very novel piece of work, which I pursued over that period of six years.

After about the first two years, the results were so promising, and I'd made enough noise about it at meetings and such that I got a call one day from a man named Dennis Bushnell at Langley Field—an interesting fellow. If you go to Hartford, you'll find Bushnell Park, Bushnell Museum; that's the family he came from. [Laughter] Anyway, I got the call at about closing time at Langley Field, which was sometime around 1:00 or 2:00 o'clock in the afternoon. Dennis called and said, "Frank, we're having an important meeting on how to fund work on this mixing problem for hypersonic ramjets. And you just have to be here." And I said, "But, Dennis, that's tomorrow morning! When does the meeting start?" He said, "Well, we should start about 8:30 our time." I said, "But I'm not in Washington or even on the East Coast; I'm here." [Laughter]

Well, the upshot of it was that I took the red-eye—and I had sworn at that time of my life I wouldn't fly the red-eye anymore—wound up at Langley Field on time, without any sleep. We had the meeting. I knew what he wanted. I was there in case I had some idea he could put some money into. So during that night on the airplane, I sketched out what I thought was a practical implementation of the shock-enhanced mixing process I'd investigated. And I talked about it at the meeting that morning. There were some people at Langley Field who took an extremely dim view of it. But Dennis Bushnell was enthusiastic and wide-eyed; he liked it. So he said, "Frank, I'd like to give you $250,000 a year to build one of these things, and we'll put it in our Mach 6 wind tunnel here at Langley and do the experiments on it." I said, "Well, I'll consider it, if you'll let us run the experiments here." And he said, "I'd be overjoyed if you and your students or postdocs would run the experiments here."

So I got on the phone—that was about 11:00 o'clock, East Coast time—and called Ed Zukoski. And, fortunately, Ed always comes in early, and he was there by 8:15. And I said, "Ed, we have a chance to do some experiments that will require some design and some experimentation here. Will you go along with me if I accept this?" He said, "Sure!" So by noon of that day, it was a done deal. And the rest of it was just the formality of getting a contract that was acceptable to NASA and acceptable to Caltech.

That work went on over a period of two and a half years. And the device was designed, built, shipped to Langley Field, put in the tunnel, and tested. And it was quite successful. I'd say it did roughly 80 percent of what we had hoped it would. Or rather, I should say, of what I had sold. Of course, my selling price was probably a little bit high, but nevertheless, we considered that a very exciting and rather successful foray into a completely new field.

COHEN: You and Professor Zukoski did that. Did anybody else work on it?

MARBLE: One student in particular. And that was Ian Waitz. He did a very good job. Actually, Bushnell was the one who introduced me to Waitz, because I didn't have a student at that time who was quite the right fellow for the job. But he said, "You look at this fellow, Ian Waitz." Waitz had worked with Bushnell at Langley Field, and the great advantage was that he knew his way around Langley Field. He knew how to coax and cajole the people into getting done what he wanted them to do. So when the experiments were to be done there, he just went there and stayed for three or four months, saw that they got done. He was our liaison man, and he did a very good job. He is now assistant professor of aeronautics at MIT.

That was the last big piece of work I chose to get into. I thought it was probably a pretty good time to end, because it extended actually about a year or so past my retirement. And it was probably time to taper off. At that time, the rules were that I couldn't be a principal investigator and have students after retirement. That's changed now, but at that time, that was it; so I sort of pulled out.

That, in a sense, winds up the things I've done here. Other small items, I think, will emerge; but they will be primarily theoretical, analytical things that I do alone. And those will emerge over time.

The entire line of work that I've done has been to try to pick up pieces of fundamental work that had some novelty to them—to try to find an area that was untouched, or that needed to be worked on but hadn't been. And I think in each of the cases I've done something worthwhile, by bringing in a novel twist. One way or another, that's been the contribution my mind has made. Only once can I remember getting into a field that was highly developed. There were many, many people working on it, and much work going on. And we did make an important contribution there. But in general, I've tried to move into something where I was establishing novelty as well as research.

COHEN: I'd like you to talk about things you've done concurrently. You mentioned leaves, but you didn't talk about your long visits, such as going to China.

MARBLE: That was a very important thing. I mentioned, when I was talking about Professor Tsien leaving, that he said, "Frank, I'll see you in twenty years." Well, as it turned out, it was not quite that. He left in 1955. And then, in 1982, the Academy of Sciences of China and their Graduate School of Science and Technology in Beijing, invited me to come and teach. And in order to accommodate us, they invited Ora Lee to teach conversational English to the senior staff in the Graduate School of Science and Technology.

We left here in early August of 1982, and we were in China for three and a half months. I lectured almost all of that time. They were very, very anxious to have lectures in propulsion, combustion, gas dynamics—on things that were current here in the US. But of course this was the kind of thing you weren't supposed to talk about in China at that time. But I think the lectures were completely successful.

COHEN: Did you have anything to do with the State Department before you left?

MARBLE: No, I didn't have any trouble with the State Department. By 1982, things had eased quite a bit with regard to China. The Academy of Sciences here had a regular relationship with the Academy of Sciences in China. So the travel and the clearances and things like that were, more or less, routine. Of course, when you travel to a country like that, you have to let the various military organizations for which you work, or have classified clearances with—you have to let them know you're going. And that's just about the only aspect of it. But as far as my lectures went, they really contained only things on which I was lecturing here. So I didn't have to do too much new preparation—except that they really wanted to have view-graphs, because the English translation to Chinese was terrible. And eventually, after a rather short time, the translator just threw up his hands and quit. So I lectured to them in English, which they appreciated, because they said the translator was terrible. By having view-graphs, which covered almost all of the lectures in detail, I was able to supply, really, everything they needed. And my big chore was getting new view-graphs made for all these lectures, because I hadn't really expected to do that.

COHEN: Where did they put you up?

MARBLE: We were in the Friendship Hotel in Beijing, which, at that time, was a much more primitive place than it is now. But while we were there, one of the high points, of course, was getting to see Tsien and his family again. It was an extremely interesting thing. Tsien was a member of the—well, there were two hundred members of the Communist Party which made up essentially a decision-making group. He was one of them. The Twelfth Party Congress was on at the time we arrived, and Tsien was sequestered. He was put up in a hotel and he was not allowed to get out until after the Congress was over. So after the Congress was over, Tsien and Yin, his wife, came one evening to our hotel.

Now this is a complicated thing. Here were two people who had been close friends in the US more than twenty-five years ago, had diverged in their ways, in their thinking. Tsien had been immersed in the big days of the post-Revolution era. He was a great close friend and admirer of Chou En-lai, and mourned greatly at Chou's death, which happened before we arrived there.

COHEN: He had nothing to do with the Cultural Revolution?

MARBLE: Well, the Cultural Revolution came; he suffered, and his wife suffered quite a bit. But not to the extent that they would have if he hadn't been so enmeshed in the military work. At one time or another, Tsien decided to accept the post of vice chairman of the Chinese Committee for Defense Science, which essentially corresponds to the Department of Defense here. And the vice chairman corresponds to the Secretary of Defense. So it was one of the very highest jobs, one of the most closely watched jobs in China. It's important to know that on these committees the vice chairman is the one who does the work and the chairman is usually quite elderly and honorary and performs certain functions like proposing toasts for visitors, things of this sort. So Tsien was really carrying that load.

As a consequence of this position, he had to make a couple of public confessions, I think, but he was not sent off to the farm to work or anything of this sort.

His wife, however, was the head of the department of vocal music at the Beijing Conservatory—I probably never mentioned that she was educated in Europe in vocal music. She was a classmate of Elizabeth Schwarzkopf, the very notable German soprano. She and Elizabeth Schwarzkopf studied at the same time with the same teachers in Switzerland during World War II. Anyway, she was very hard put to it. And the consequence was that she lived in the conservatory and never went home for a period of more than a year. And her students brought food to her at the conservatory.

Well, when I realized the difference in Tsien's and my experience in the previous twenty-five years, I was very concerned—and he was too—that we wouldn't see eye-to-eye on anything, and that this great warmth that had developed while we were here—being interested in the same problems, fighting the same political issues—wouldn't exist anymore. And when we met, we probably had a slightly tense half hour, while we sparred with each other. We tested each other. And at the end of the half hour, we realized we could talk to each other just as openly and freely as ever. Since I had no Chinese to speak of, Tsien was using English. The amazing thing was that his English had not deteriorated very much; it came back so rapidly in the next two or three months that we saw each other, that the subtle nuances, including contemporary terms that hadn't existed at the time he was in the US, made me realize that they had been using English a lot more than they wanted to admit. And Tsien had always claimed that he hadn't used English in China, that they never spoke English, that his children didn't speak English. Well, it turned out he was right about his children, but less than accurate about himself and his wife. But it probably was not politically popular for them to admit that they spoke English. So it was a great renewal, a great reunion for us. And the Tsiens and the Marbles would take off for some place, outside of Beijing, and have a picnic.

To get back to the educational part of it, I had a class of about sixty people. They were very faithful; they came to every lecture and took voluminous notes and studied. They were divided into two parts: The students over forty, who were essentially pre-Cultural Revolution, and the younger ones, twenty-two years or younger, who were post-Cultural Revolution. And that whole middle piece, there were ten or twelve years just missing. There were no educated, scientific people to speak of from that period. And the behavior of these two groups was fascinating. Every lecture was scheduled for three hours. And a bell would ring in the middle of it, and we would have half an hour for them to do their compulsory exercises. And the situation was that during this period the older students would get together in the back of the room someplace and talk about lectures and they would try to figure out with each other what had gone on. The younger students, on the other hand, would come up and besiege me with questions about this or that. There was one fellow who came up and said very pointedly, "This equation wrong!" And it took me a half hour to get him straight on that—that it wasn't wrong, that it was right—and he was very grateful. But they were very different in their attitudes. At the end, I didn't want to give an examination, but I did give an oral examination. I spoke to each of them, so that they could at least have the feeling that they'd completed something and were examined by this foreigner who had lectured to them all this time. Even though the pseudo-examination didn't mean very much, it was still satisfying to them, and I was happy to do that.

COHEN: It sounds like you worked very hard.

MARBLE: It was a very hard set of lectures. It was essentially lecturing six days a week, from 9:00 until 12:00, with that half hour out when the bell rang for them to do their calisthenics—which, of course, they didn't do, though some people did. That trip to China was a very eventful one. I made contact with all my old friends who had been here and gone back, and made many new friends, whom I still write to and hear from and see on subsequent trips.

We were back there in 1991 for a shorter period of time—for only a month. In the ten years that had intervened, it had changed a great deal. Tsien's health was not all that good. He had definitely aged. And the pity is that both he and his wife suffered from deteriorated hearing. So talking with them was not quite as fulfilling as it had been earlier. I was very glad that I got there as soon as I did after the end of the Cultural Revolution.

There was another trip that I had—to France, in 1984. This, again, was due to the courtesy of one of my students, Sébastien Candel. And I and Ora Lee spent two months there, living in Châtenay. And I lectured at École Centrale for that period. There, the lecturing schedule was much more casual, because the French I don't think would put up with three hours of lecture, six days a week. I was lucky to get two lectures in a week in that period—sometimes three, but usually two at best. And I can remember specifically that the month of May was so perforated with religious holidays, state holidays, student ski trips to the Alps, that trying to schedule lectures was more than either I or Sebéstien Candel could do. But that was a marvelous visit; I enjoyed it. I was a visiting professor.

So the four really major trips away from Caltech were the one to Cornell in '56, to Cambridge, England, for the year '72-'73, the one to China in '82, and then to École Centrale in '84.

There was another one, which was long, but not quite of the same sort. And that was in 1981. I was appointed the Hunsacker Professor of Aeronautics at MIT, which involved being there in residence for half a year, and giving the Minta Martin Lecture. Minta Martin was the mother of Glenn Martin, who was the founder of Martin Aircraft. She was a very domineering woman who influenced Glenn's life all during his founding of his organization, his moving around the country from California through Ohio to Maryland. At the time she died, friends of Glenn Martin established a lectureship in her name. So I gave that, in the spring of 1981, and Ora Lee and I were there for a period of about five months. It was a very nice trip; but not in the same vein as long stays in foreign countries.

Incidentally, I will be back in China at the end of this summer, probably in September. And that's for the purpose of getting Tsien's papers straightened out.

COHEN: He's still alive, isn't he?

MARBLE: He's still alive, but when he was deported, he left all of his papers, his notes, his documents here. And over the years, I collected these and organized them. And only last year I had the courage to get them sent back to China. They have established a library of Tsien's archives, if you will, of his American period. Aside from Tsien himself, I'm the only one who really knows enough about it to be able to straighten them out. So they have asked me to come over for a period of two or three weeks—I'll probably go for a month—and just get these detailed notes in order so that they're accessible to students. They must understand the relationship between them and the various papers he wrote.

COHEN: Where were these papers during this time?

MARBLE: I had them. I had four four-drawer file cabinets full of his stuff. So that's now in China. There may, at some time, be some interest in having Tsien in the Archives here, but his ten years at Caltech were—well, they were not that long—and he was not too much part of the history of Caltech. But he was so influential in Kármán's work during the late 1930s and the early 1940s that I'm sure he appears in the Kármán files again and again. And Kármán himself was, of course, very involved in the time that Tsien was being deported, although he never saw Tsien again.

COHEN: Let's go further. And talk about some of the honors you've had.

MARBLE: First, I should say I've never been very much of an organization man. I have never been a great society person—always avoided society participation where I was able to. And the only one I've been consistent with is what is now called the American Institute of Aeronautics and Astronautics—the AIAA. And the consequence is that societies have not showered me with awards. Although the AIAA did give me their annual Combustion and Propellants Award in 1991, which was very nice of them—one of their two or three annual awards to people who have lived long enough to get them. [Laughter] And as far as the others go, I think really the honors that I've felt most deeply about were three.

One was the Richard and Dorothy Hayman Professorship. An endowed chair, by itself, of course is a great thing, but the thing about it that was so great was the relationship that the Haymans and the Marbles developed. Sometimes people get an endowed chair and they never know who the donor is; or maybe they meet the donor at one time or another; or the donor's dead. The Haymans were not at all dead, and they were not at all separate from us. We had very many excellent social encounters, and we became very deep friends—Dick Hayman, particularly, who was a student here. He wanted to know what was happening. He prized the relationship with a faculty member. And we had very, very great and warm times. It was the end of an era when both Dick and Dotty died. And that was a tragic thing, because neither of them was really elderly.

The other two honors I've had that I prize very highly were the election to the Academy of Engineering, which I think was in 1974. That I consider a great honor, and have enjoyed participation in that. And, of course, that opens the door to participation in the National Research Council, which has occupied a great deal of my time in the last three years.

And, of course, less expected was the election to the National Academy of Sciences in 1989. The engineering group in the Academy of Sciences is very small. And in fact, the fact that it was small, or almost nonexistent, led in one way or another to the formation of the Academy of Engineering. But anyway, there are very few people—maybe a couple of dozen people—in the country who are members of both of them. So I was extraordinarily honored by being elected to the Academy of Sciences. It was just a very gracious group of people here who were not only willing to support me in that but some of them had to sit down and do an awful lot of paperwork to get it done. So those, I think, are the things I prize the highest.

In my undergraduate years, there was the election to Tau Beta Pi and Sigma Xi. I was greatly honored at that time. But these later ones—based on, I guess, things I had done over the years—mean a great deal to me.

COHEN: In winding up, is there any observation you'd like to make about your life at Caltech, and your colleagues and friends here?

MARBLE: It's hard for me to think back to exactly when I first realized that I wanted to come to the California Institute of Technology. It was certainly before 1940. When I finished my bachelor's degree at Case, I knew definitely that I wanted to come to Caltech. I'd studied the catalog; I knew everything that was involved in it. And in the end, it was a mixture of war and finances that encouraged me to stay at Case for my master's, and then go to work after that and postpone my trip to Caltech.

When I came here, I think Caltech represented what seemed to me a step into the ultimate good, if you like. It was just a marvelous place. The people I dealt with talked a language that I like to hear. They were warm and pleasant to Ora Lee and me. We enjoyed it thoroughly. And I think, as I've said before, the atmosphere at Caltech here and our life here at Caltech was such that even though we had promised each other to go back to Cleveland two years after we came, after six months the question never was raised again. And when all these opportunities came up from a variety of schools and positions that had some interest, some challenge, we said No, we'd be idiots to leave Caltech. Caltech is part of our life, part of our life blood, and we're going to stay here.

And the same thing was reflected in retirement. There's always the question then of what you do. Well, my effort was to change my life as little as possible. In fact, two years ago—to put it in perspective—we sold a house that was remote from Caltech and bought one within easy walking distance of Caltech. The most humorous comment came from the Ansons, who said, "Oh, we think that all couples who've reached their fiftieth wedding anniversary should buy a new house, instead of going to the old folks' home." [Laughter]

But there has been a bit of change at Caltech, and I don't think it's my aging. I think the situation has changed. At the time I came, the faculty—and the student body too—was more intimate. I was as welcome in physics, mathematics, and chemistry as I was in engineering. In fact, my graduate work was more in mathematics and physics than it was in engineering. Interaction and cooperation and friendliness existed at that time, more so than it does now. Camping trips, for example. We'd always have Arthur Erdélyi and Morgan Ward from mathematics, and Charlie De Prima. One of the people from mathematics would probably go with us; and maybe somebody from physics, depending on who it was.

COHEN: Now, was it your work that brought that about; or…perhaps being friendlier with those people?

MARBLE: I think that it was just in the course of seminars, work, and interests. It was mutual interests.

COHEN: Do you think people are much more parochial now?

MARBLE: I think there's been a separation out. For one thing, when I was first advising students—first getting students to take the graduate courses I thought they ought to—I always had them take a course or two in mathematics. The courses changed, but they started off with the famous Math 114, which Morgan Ward—and eventually Arthur Erdélyi—taught. Also, I always had them take the course in atomic and nuclear physics, which Charles Lauritsen and Willy Fowler taught at one time, and Bob Leighton after that. [Tape ends]

Begin Tape 6, Side 2

MARBLE: I think it was that I considered a graduate student in engineering educated only if he had had this course of mathematics from mathematicians and courses in physics from physicists, rather than from people in engineering. I always claimed that a PhD in applied science had to be able to change his field every five years. This was just the type of life that he would lead; he'd have to change the things he worked on. And only with a good, basic education in the sciences and in mathematics would he be able to do that—not with a specialized education in engineering.

Gradually this became impossible, due to changes in the curriculum in mathematics—and, as a matter of fact, a certain lack of enthusiasm about having large groups of engineers coming into classes in mathematics. The mathematicians took a dim view of that. The people in physics did also, because gradually some of these courses had come to have more engineers in them than they did people from physics. So the physicists felt like they were giving the courses for engineers, and they didn't like that. So now it's more complicated for somebody in engineering to go to a physicist for work or go to a mathematician for work. In chemistry, it may be a bit easier, because of chemical engineering. But there has been a compartmentalization of interests, which I think is not as healthy as it was at the time I came here.

COHEN: Are there more people involved? The faculty isn't that much bigger.

MARBLE: The faculty isn't that much larger, no. It's always hovered around two hundred and sixty. The staff, and the postdoctoral population, has gone way up. This has changed. And it's rather hard to say why that is. But certainly I had very good friends in physics, mathematics, chemistry—not only academic friends, not only scientific friends, but social friends. That was a very important part of our lives. And we've tried to keep that, not as successfully as we might, of course, but we've made some effort to keep that.

COHEN: So you feel the institute has changed in that regard?

MARBLE: I think the institute has changed in that. It has not changed from being the most attractive scientific, applied scientific institution in the world—it's still that. And anybody who comes and spends any time here, unless he has a mental block against Caltech for reasons other than its scientific and cultural wonders, anybody who spends any time here admits that also.

I think I've picked on one example there, where I think Caltech has changed. But really, if I look at the changes in other academic institutions—MIT, Michigan, the University of California, Stanford—if I look at these, the fact that Caltech has maintained its size and its scientific and technological integrity to the degree that it has is remarkable. I think it's changed remarkably less than the others.

COHEN: Do you think that's been the stability of, say, DuBridge being here so many years?

MARBLE: It's hard to say, but I think that every new president who has come—certainly it was a picture that DuBridge had, and I was not well enough aware of Robert Millikan and his group to know what their idea was, but I presume that they did not have the urge to become just an enormous institution. But DuBridge certainly didn't. DuBridge did, however, have a very great fondness for physics as such. And I think Caltech to him was physics and a little bit else. I say that in all respect and kindness to him, because I can remember two speeches he made in faculty meetings. And he said, "My aim is to make Caltech the greatest school of physics in the world." [Laughter] Deep in his heart he meant it, but he knew he shouldn't have said it. But I think DuBridge did maintain the scientific integrity; he fought for it, he maintained the size.

I would think that the only question that I have about that is, he made the important decision with regard to JPL. And JPL is now a permanent adjunct of the California Institute of Technology. And whether this is good or bad, one can't quite ask. It's like what Stephen Vincent Benét wrote regarding the results of the Civil War. He said, "Say neither 'it is accursed' nor 'it is blest' but only 'it is here'." [Laughter] And I think that's the situation here.

And then I think subsequent people as they came, recognized the uniqueness of Caltech. I remember asking Harold Brown, before he came here, about this. I asked him why somebody with the background and extensive organization that he had would really want to come to a much, much smaller organization such as Caltech. And it was a very straight answer. He said it had to do with the excellence, the uniqueness of the school, and the importance of maintaining that.

So I think that the presidents, for one reason or another, and perhaps the board of trustees, have fostered this idea—the idea of the size and strength and excellence has maintained itself. And the urge to get the very best people they can. And I think that's happened. And I look at the difference between Caltech post-World War II and now, and I look at other institutions—including my own undergraduate institution, Case—the differences are just unbelievable. But not for Caltech; Caltech has maintained itself. I hope that this healthy attitude will continue.

FRANK E. MARBLE

SESSION 7

April 21, 1995

Begin Tape 7, Side 1

MARBLE: After I prepared the oral history that you've written up so nicely now, as my wife and I were going over it, she noticed—or she said—"But Frank, you didn't say anything about your personal flying. And that must have something to do with your life in aeronautics and whatnot." And she was absolutely right.

I have said in the oral history that very early, from 1926 or '27 on—when I was eight or nine years old—I was fascinated by flying. And that the presence of the National Air Races in Cleveland in 1929 was a very important step in my interest and enthusiasm in aeronautics, because that involved not only the flying races at Cleveland Airport, but also a marvelous show of airplanes in the Cleveland Public Auditorium, which was an enormous place. It was just full of airplanes. I had never been that close to airplanes. Here I could climb around and sit in the pilot seat. That was great!

The next manifestation of that interest was that I became involved in model airplanes. I had a pretty good success in model airplane building, model airplane contests, and things of that sort. And that went on; really, it dominated my activity. And, of course, it was much more important to me than any of my academic work at that period of life. This was true up to the second year of junior high school, I would say.

The other thing that happened was that, as a consequence, I built up a pretty significant woodworking and, partially, metalworking shop in my home and was able to do quite a few things there that other people couldn't do. The experience I had there served me very well when I got into Case.

COHEN: So this was already in high school, you were doing all of these things.

MARBLE: I wasn't in high school yet. This was junior high school. I had the bug from the middle of elementary school up through the second year of junior high school. In the third year of junior high school, as I have indicated, music became the dominant thing. It sort of outdid the airplane model making at that time.

I guess my single entry into entrepreneurial activity happened at that time. At that period of history in the Cleveland area—and probably elsewhere, too—pictures, movies of gangsters and gang wars and Chicago and all this was very important. We could see these, and the kids were very excited about it. I developed quite a profitable little enterprise making wooden guns. I made wooden hand guns; I made automatics, revolvers. I even made a couple of Thompson machine guns out of wood, painted them, loaded them with lead so they felt heavy, and sold these to kids. These were very good replicas of the real thing. I know I'd get put in jail if I did that now. [Laughter] I have a couple of those tucked away in a trunk someplace. But this was just one of the byproducts of that shop and hand-working activity.

All that was sort of put into the background when I was involved with music in high school. When I got to college, I was interested in aeronautics very, very much. It wasn't until my sophomore year that I got in contact with John Weske—as I've said—in the aero group at Case and worked from there on.

Now, at that time, it just happened that Case had a glider club. We had a primary trainer—a simple glider that you pulled a little bit and it flew a little bit and came down.

COHEN: What year are we talking about?

MARBLE: We're talking about 1938—late '37, '38. I worked with that group for quite some time. We'd have accidents; we'd crack the thing up and have to rebuild it. So I learned quite a bit about maintaining simple structures. And these were very simple structures—wooden ribs, wooden parts, a few metal pieces here and there, but primarily a wood and fabric unit.

In late 1938, early '39, the government established the Civilian Pilot Training Program—the CPTP—which was sort of an -echo of the war in Europe. The idea there was to train pilots, to train people who were physically able, to be able to fly in case fliers were needed, in case we got into the war. It was really a fairly farsighted thing. So I couldn't wait to get involved in that. And I did, and took my primary pilot training.

COHEN: This would have been your first experience with flying?

MARBLE: This was my first experience at flying. I'd actually never been in the air before, I'd never flown before. Travel by air was not very extensive at that time. Also, it was not only not extensive, it was very expensive. This was my first chance.

I did the ground school course, and then I began to fly. And I flew at a place called the Lost Nations Airport, which was east of Willoughby, outside of Cleveland. The Lost Nations Airport was run by a man named Dewitt Eldred—Dewey Eldred—and he was a big, tall fellow, and he was the local agent for the Taylorcraft airplane, which was a not very powerful 65-horsepower monoplane. It was a fairly popular sport plane at that time among the few people who did fly. I learned to fly in that, and did very, very well in it. Everybody encouraged me to keep flying. I did get my pilot's license then in, I think, April of 1940.

COHEN: You must have been part of a very small group.

MARBLE: There was a small group. I think there were about twelve of us from Case, and about ten or twelve from Lake Erie College, which was a women's school, which was very active in sports and such things.

COHEN: You mean women pilots?

MARBLE: Yes. They taught as many women as they did men. And we both trained at the same place. It turned out that two of the women I knew became ferry pilots with the WAFS, taking airplanes from the US to England—flew them over there—and they spent the war flying across the ocean. I've always claimed that—it's a fascinating thing that I got my pilot's license before I learned how to drive an automobile. [Laughter] I flew before I drove, at least. So that was the start of my flying. And it was a very important thing, because I think in the same way that my music and trombone playing helped get over a bit of shyness, which I had always had, this capability of flying, and the confidence of controlling yourself in a three-dimensional space rather than on a two-dimensional surface, was a big step for me—a big psychological step—and I felt it was an important thing. So I really felt part of the aeronautics field, the aeronautics business at that time.

Then, of course, the war came. And I did no flying of my own at that time. I was too busy at the laboratory where I worked—at the NACA [National Advisory Committee for Aeronautics] in the Cleveland Lab. The only flying I did was as a flight test engineer on the B-26 and the B-29 airplanes, which I've talked about in my oral history.

So my flying sort of lagged. I did very little for several years. And then in the late 1950s, here at Caltech, the Caltech Flying Club started up again with a few people. There were several people from JPL and several people from Caltech who invested in a small airplane. It's interesting to see how these small airplanes started off. Really, the birth of the small airplane was the Taylorcraft. And that's the one that I learned to fly in. One of the people, who had worked with Taylor, the man who started that, broke off and started the Piper Aircraft Company, which succeeded much better than the Taylorcraft did. So there were two small airplanes at that time which people learned to fly in—the Piper Cub and the Taylorcraft.

Well, at the time that the Caltech Flying Club started, the popular airplane—the one most easily available—was the Cessna. The Cessna line still exists, and some of the nicer ones—like the Cessna 182—are very popular and very sturdy private planes.

So I got my rating back. I did what I needed to do to pass the test again. It turned out that the kinds of flight tests that you needed to do were certainly watered down from the one that I had to take in 1939. At that time, you had to do all kinds of complicated stalls, tailspins—show your ability to get out of a tailspin both to the right and to the left—and things of that type.

COHEN: Were planes better now, or was it just that people were used to flying?

MARBLE: Oh, the airplanes are much better now than they were then. In fact, in a current airplane now—or in the 1950s—you had a very difficult time to put it into a tailspin. It would take quite a bit of experience and effort to put one into a tailspin. The Taylorcrafts and the Pipers of the 1939, '40, '41 era you could still put into a tailspin very, very easily. So the chances of going out of a stall into a tailspin were non-negligible. What is important is that you know how to extract yourself. So we had to do that.

Well, I just loved that. I loved to go up and practice tailspins. And the people who ran the airport knew it. So I was going out to practice tailspins one day, and Dewey Eldred said to me, "Now, Frank, you go out over Lake Erie. Go out about five miles over Lake Erie and do your tailspins out there." That was fine. The thing I didn't realize—or didn't pay any attention to—was that there was an onshore breeze. There was a breeze from the Canada side down to the US side. And gradually, as I was enjoying myself with the acrobatics, I noticed I was right over the middle of a town down there. I got back to the airport, landed, taxied in. Dewey Eldred met me; his face was red; he was mad as could be. He said, "I've got all kinds of phone calls from the town down there saying that there's some idiot pilot up there doing acrobatics over the middle of town. Get him out of there." [Laughter] So I was confined to the ground for a couple of weeks after that.

To come back to the late fifties: When I was getting my license back, the FAA [Federal Aviation Agency] tests that they gave you were very simple compared to what I had to do at that time. So it was no trouble at all.

Then I continued to fly fairly extensively, and in the late sixties, I decided I'd get my instrument rating, which allows you to fly in on the instrument-controlled landings and such, which gives you a lot more freedom of flying in weather that isn't as clear as is required when you fly visual flight rules. I feel that that's very important in the Southern California area—partly because the traffic is so bad, and also because if you come in, in the late afternoon, there's quite a haze if you're flying into the west.

So I continued to fly extensively. When I did some consulting work, I would fly to San Jose or Albuquerque, or other such places. Even though it was not efficient, so far as time was concerned, it was a lot of fun.

COHEN: Would you rent an airplane?

MARBLE: No, I leased an airplane, which means there are about four or five people that take care of all the service. It belonged to a flying service called the Sky Roamers at Burbank Airport. Four of us leased a Cessna 182. I kept this arrangement for some seven or eight years, until it began to be a little more expensive than the fun I was getting out of it.

One of my friends said to me once, "You know, one measure of a good pilot is that he knows when to quit." [Laughter] So about the time I turned seventy, I decided, "Well, this is the time I ought to quit. I just ought to stop flying now." So I packed it away, and that was the end of it.

But certainly the whole experience of flying myself was a vital and important part of my getting in and staying in aeronautics. It probably has a little bit to do with my being in the aeronautical side more than I am in the space side. If I didn't have this personal love for flying, I imagine I would have devoted more time to rockets and satellites and things of this type.

Anyway, I just wanted to bring up that portion of my life, involving personal flying, because it did have a big influence.

COHEN: Did you take your family occasionally?

MARBLE: Oh, yes. I'd take the family. Ora Lee would go with me. My daughter Patricia flew several times with me, my son Stephen a time or two. My daughter enjoyed flying a lot more than Ora Lee, as a matter of fact. And she was a very good navigator. I remember one Tucson trip we took, to see our friends the Sears—Bill and Mabel Sears. We just had a lovely time—ran into some rain, some clouds, and had bad weather coming home—and that all made it exciting.

COHEN: How common do you think this would be for professional people like you also to be involved in flying?

MARBLE: It's not very common. There are a number of people who do it. I know the number of people here at Caltech who really flew seriously. Seriously means that you don't just go out on a Sunday and fly around a little bit, but you do it as part of your daily activity. I know four or five who did at one time—I don't know what they're doing now. But in the seventies, when I was still flying quite a bit, there were five or six people who did similar things.

COHEN: It was really recreation as well as getting places.

MARBLE: It was, yes. And I guess the fact that Ora Lee was not exactly attuned to it made me ease off on it a little bit, as far as my recreation went. Because I didn't want to do something that she wasn't very enchanted with.

There's another very interesting thing. About that time, the board of trustees at Caltech decided that they would not allow faculty to fly and compensate them for their flight time as if they were flying on a regular airline. There hadn't been any particular accidents due to private flying, but maybe somebody on the board got a bug someplace or other and thought that they shouldn't be financing this dangerous activity. It may have had something to do with insurance; I'm not quite sure. I don't know of any of the Caltech faculty who have had a private plane accident where they have hurt themselves. I know a couple have bent the landing gear or a nose wheel, or something like that. But none have hurt themselves—whereas we've had two or three faculty die in commercial airplane accidents. So it's not entirely logical. [Laughter] But it did put a big crimp in faculty flying; it sort of ended it. What I did—what they did, too—was to utilize this to get your necessary flying time in to keep current. And in a way, you were being subsidized by the school, because your contract was paying for your flying time, instead of going on a commercial airline.