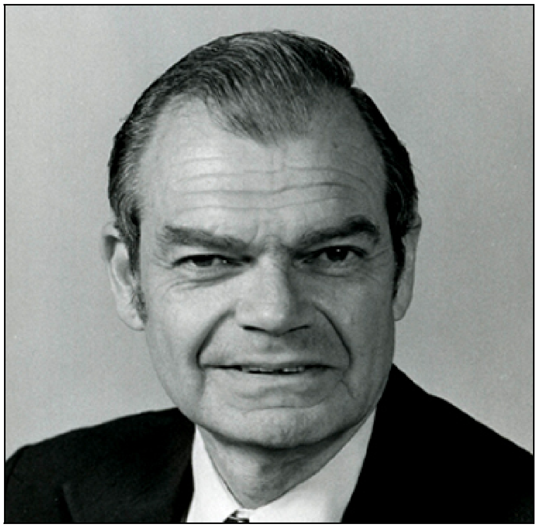

By Floyd Clark, 1977. Courtesy CIT Public Relations.

ROLF H. SABERSKY
(1920 –)

INTERVIEWED BY
SHELLEY ERWIN

April 3 and 12, 1990

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area
Mechanical engineering

Abstract
An interview in two sessions, in April 1990, with Rolf Heinrich Sabersky, professor of mechanical engineering, emeritus, in the Division of Engineering and Applied Science. Dr. Sabersky received his undergraduate and graduate education at Caltech (BS, 1942; MS, 1943; PhD, 1949). He joined the Caltech faculty in 1949 and became a full professor in 1961 and emeritus professor in 1988.

In this interview, he discusses his early education in Berlin and his family's flight from Germany in 1938 to Switzerland and thence to Los Angeles. He entered Caltech in 1939 as a sophomore; recalls his professors there: Donald S. Clark, Frederic W. Hinrichs, Robert L. Daugherty, Robert T. Knapp, Franklin Thomas, William H. Pickering, Romeo R. Martel, William B. Munro, and James W. Daily. Recollections of Thomas Mann. Pearl Harbor and Caltech campus in wartime; restrictions applying to him as an "enemy alien."

He discusses his work on the Southern California Cooperative Wind Tunnel under Mark Serrurier; recalls visits to that project by Arthur (Maj.) Klein. Becomes a graduate student; lives in the Old Dorm and joins the campus fire brigade. Courses from Donald E. Hudson, Robert C. Bromfield, Peter Kyropoulos. After the MS degree, he goes to work at Aerojet Engineering Corp.

at invitation of A.M.O. Smith; works with Martin Summerfield on sustained-duration liquid rocket engines.

Recollections of Theodore von Kármán, Clark B. Millikan. Guggenheim Jet Propulsion Center and H. S. Tsien. Recollections of Fritz Zwicky in his Aerojet days. The influence of Aerojet's William E. Zisch. Becomes acquainted with James Van Allen in early 1946, at Applied Physics Lab, Johns Hopkins; their work together on the Aerobee rocket.

Back to Caltech for the PhD; comments on advent of Frederick C. Lindvall and changes in the engineering division. Recalls his work with Duncan Rannie on axial flow compressors. Courses with Carl Anderson, H. Victor Neher, Charles Lauritsen. Receives his PhD, joins the faculty; consults for Aerojet. Comments on changes in engineering curriculum, drop in engineering enrollment in the late 1950s, the rise of environmental engineering. He discusses division problems with accreditation; assesses student quality and effect of the admission of women. Comments on increasingly cumbersome process of faculty recruitment.

He discusses his work on boiling heat transfer, on fluids near the critical point, on fluid flow in rough tubes, on polymer solutions and non-Newtonian fluids, on flowing granular material. Talks about his "extracurricular" research on indoor pollution with Frederic Shair. He concludes with an assessment of current prospects facing graduating engineers.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

Sabersky, Rolf H. Interview by Shelley Erwin. Pasadena, California, April 3 and 12, 1990. Oral History Project, California Institute of Technology Archives. Retrieved [supply date of retrieval] from the World Wide Web: http://resolver.caltech.edu/CaltechOH:OH_Sabersky_R

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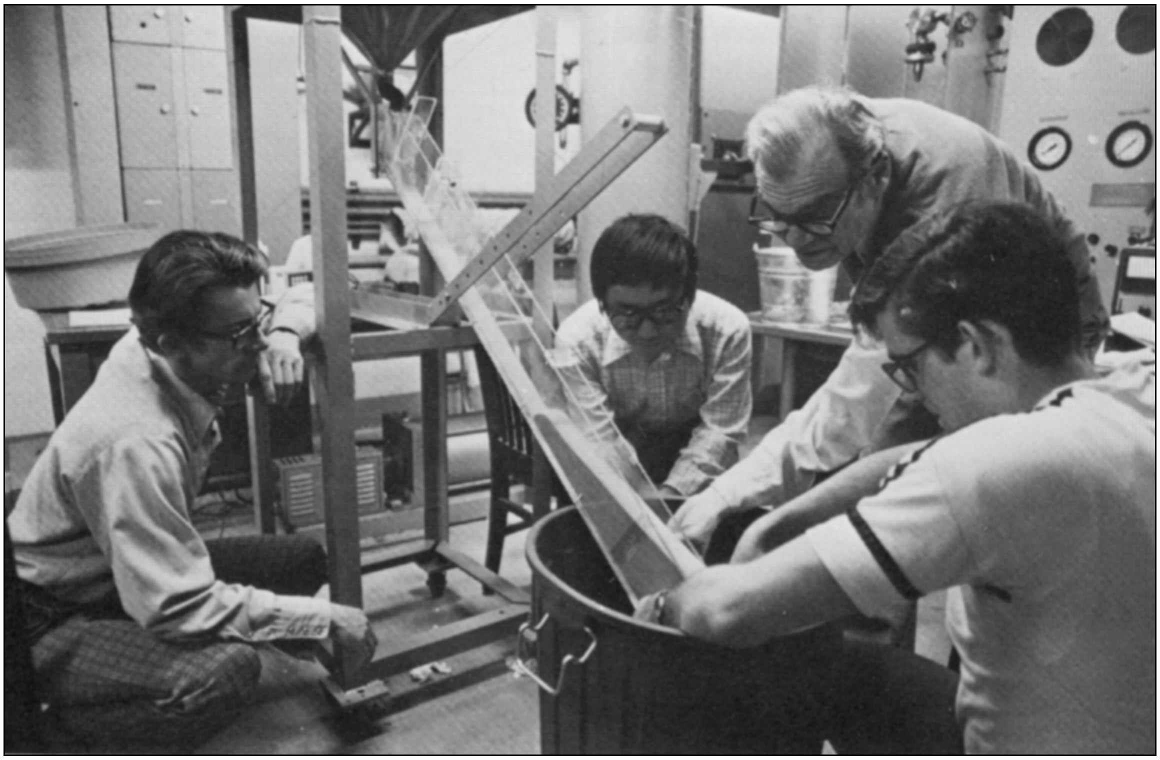

Rolf Sabersky (top right) demonstrates elements of granular flow to Caltech students in 1981 in a new attempt to understand the flow of non-fluid materials. His faculty colleague Christopher Brennen looks on (left). As sand flows down the chute, it is partially backed up behind a gate, while a layer of sand continues flowing over the top—a phenomenon known as hydraulic jump when it occurs in water. Photo courtesy of *Engineering & Science*, April 1981.

CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

INTERVIEW WITH ROLF H. SABERSKY

BY SHELLEY ERWIN

PASADENA, CALIFORNIA

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CALIFORNIA INSTITUTE OF TECHNOLOGY ARCHIVES

ORAL HISTORY PROJECT

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Pasadena, California

Session 1	April 3, 1990
Session 2	April 12, 1990

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SABERSKY: I was born in Berlin, and I went to high school there, that is, kind of an unusual high school. It still exists. It was founded in 1685. I remember the year because they just had their 300th birthday. I have to correct that: Actually it was three years later, 1688; 1685 is when the Huguenots had to leave France, and the Grand Elector, the *Grosse Kurfurst* in Germany, accepted them with open arms. A lot of institutions were founded by the Huguenots, and one of them was this school, the so-called *Franzosisches Gymnasium*, the French High School. It kept up the tradition that practically all the courses were taught in French, and that is still true today. Actually, it turns out that it's even more traditional, in the sense of teaching things in French, now than it has been before, because there's a rather large group of French so-called occupation forces still in Berlin. This is probably going to be changed any day now, because of the unification. Up to now a number of French officers and troops are stationed in Berlin, and their children go to this school. So even more than it was at my time, the curriculum is designed to teach French and to actually give the French students the opportunity to take the French high school exam, the baccalauréat.

ERWIN: How did you come to go to that school?

SABERSKY: Well, that was a school that was, at my time, open to everybody. It was considered a good school, so quite a few people from the center of Berlin went there. It's well known but actually quite small. The present director happens to be interested in the history of the school, and he prepared a thick book on it. He has shown that up to 1945, right after the Second World War, the total number of graduates was two thousand—only two tousand, in all those years, from 1688 to 1945. Because of the situation with the French students that I mentioned, there are some more now; I think another thousand has been added since 1945.

The school was really not at all designed for people interested in technical things. But it seemed to me, from a later perspective, that that didn't really matter so much. What counted was that you learned to work systematically. It didn't really matter whether you studied Latin or Greek or mathematics. The most important thing was to learn how to get things organized, how to work hard, and how to study something systematically, and that served me very well.

ERWIN: When you say high school, what year was that?

SABERSKY: From ten years old to seventeen years old. And I still finished there, despite all the happenings. I graduated with the so-called *Abitur* in 1938. We didn't realize how late it was. I left Germany on July 3, 1938, and my parents left a couple of months later. Soon thereafter, in November '38, the so-called *Kristallnacht* took place.

ERWIN: Your family didn't suffer?

SABERSKY: We all were able to leave, and we all were able to come here together: that is, my parents and a brother and a sister. We all came, after a short stay in New York, to Los Angeles, and I started at Caltech in September '39.

ERWIN: So you lost a year of schooling.

SABERSKY: Yes. I'll come back to that. I did want to mention something about the high school. There's something that's kind of interesting, because it has a connection to the honor system here. The situation there was in some sense quite different. There was close fellowship between the students, and you would do pretty much anything for each other. It was you against the teachers, essentially. [Laughter] And in the cases of exams, it was a matter of honor that whatever you knew you would pass on to your fellow students. So, in that sense, it was quite different from the honor system here.

ERWIN: Was this common in German schools?

SABERSKY: Probably. My experience is limited to this one school, but I think this was a common thing. This attitude really made a rather close-knit group out of the students in the class and close friendships resulted from it.

ERWIN: Just boys, by the way?

SABERSKY: Well, there were two girls. And, of course, the class wasn't large; the graduating class was eleven. And by then, there was one girl in that class. I am still in contact with quite a number of the students. Quite a few are here, including [Alexander] Ringer, now professor of music at the University of Illinois, and there are some in Germany. I still have some old class pictures, and now that they all are coming up for their seventieth birthdays, I send them copies of these pictures, which make a good birthday card. [Laughter]

This way of looking at things and helping each other and having these close-knit friendships—that was kind of ingrained. Fortunately, I never came up against the problem here to have to report somebody in class who may have cheated. But I don't think I would have been able to do that. And that's something in the honor system that I find difficult to stomach, to actually report somebody in your class for having done something. It does happen here, and I see that it is part of the mechanism that makes this honor system work. But it is kind of a difficult thing for me to accept. Otherwise, the honor system here works fine, of course, and the fact that you have exams which are not proctored, that's very nice. We certainly didn't have that. There were always one or two teachers who would monitor exams.

As I already mentioned, I left in July of '38 and then went to Zurich and spent a couple months going to a school which prepared you for the entrance exam at the ETH [Swiss Federal Institute of Technology]. I had to make up for the math and physics and so on that I didn't have in high school. I subsequently passed that entrance exam and then actually attended the Swiss Federal Institute for about a month or so.

ERWIN: That's where Einstein had been.

SABERSKY: Yes, he wasn't there then, but that's the same institute. Now, [Hans W.] Liepmann [Theodore von Kármán Professor of Aeronautics, emeritus] was also in Zurich, but he was at the university there, not at the Swiss Federal Institute. They are within a few blocks of each other, but they're different organizations. I passed the entrance exam, which was a difficult one. I mention it here because among the examiners was one man who also shortly afterwards left Europe and came to Stanford, George Polya, and he was quite a famous mathematician who also wrote several popular books, and I think he made quite an impact at Stanford. He was my examiner for mathematics.

Then I came to New York first.

ERWIN: What was the reason for leaving Switzerland?

SABERSKY: The stay in Switzerland was just a transition period while we were waiting for the visa to come to the United States. We couldn't have stayed in Switzerland; they wouldn't have given us permission to stay, let alone work. So coming to the U.S. certainly was the plan. We arrived in the U.S. in December '38, the day before Christmas Eve, the 23rd. I guess during this part of your life you have to take lots of exams, and so the next thing was to get the freshman entrance exam for Caltech done.

ERWIN: How did you know about Caltech?

SABERSKY: Well, we really didn't know very much at all about the American system or the American universities. The only thing we knew was that MIT and CIT were two of the technical universities, and we didn't want to stay on the East Coast. So CIT was the one. [Laughter] And I must admit, I didn't really know much more than that. So it was a selection that wasn't really based on very much information.

I passed the entrance exam. I think I still did that in New York. Caltech had the system where you could take the exam anywhere in the country. And then we came out here. I studied during the summer, and then it must have been during the summer of '39 that I took the entrance exam to go into the sophomore year. So I started here as a sophomore. By now we are at September '39, and from then on, not much happens. From '39 on, I've been here ever since, every day. [Laughter] Pretty much every day of my life, I've been on the campus since that day.

ERWIN: It's slightly ironic that you say that from September '39 nothing happened. That was the beginning of World War II.

SABERSKY: I came here and started to study here. I lived in Dabney; I was a member of Dabney House. I still have a membership card. That was a very orderly house. At the time, Caltech had resident associates, and these resident associates had a lot of influence and power. Our house was run by Dr. Donald S. Clark.

ERWIN: He was an engineer.

SABERSKY: Yes, in material science—metallurgy, as it was called at that time. He was a very strong, authoritarian personality, and he ran that house in a very orderly fashion.

ERWIN: Where were your parents at this time?

SABERSKY: They were here, on the west end of town, in Brentwood. So I stayed here. I would go there maybe during the weekend, but I lived in Dabney. Over the years, I had three different rooms. Just as now, there was room choice at the beginning of each year.

I might mention some of the teachers I had at the time. There was Dr. Clark, of course, and he had his famous course, ME3 [Mechanical Engineering 3]. You had to know exactly what was in his book [*Physical Metallurgy for Engineers*, Pasadena, CIT, 1935]. Then there was [Frederic W.] Hinrichs, the one the prize is named after. Incidentally, I notice every once in a while, at commencement, that it is called the Heinrichs Prize, which would have upset Colonel Hinrichs greatly as he was very particular about the pronunciation of his name. In fact when meeting a new class, he would introduce himself with the statement, "My name is Hinrichs, not Henricks, not Heinrichs, but Hinrichs. If I mispronounce your name, I wish you would let me know." He had been a colonel before he came to Caltech, and he served as dean of students for a portion of the time, as I remember. I always found it interesting that his son later on became a general.

So there was Hinrichs; and Professor [Robert L.] Daugherty, of course, who was the senior professor in mechanical engineering for many years. He had written a book on hydraulics that is still being used; I think it's the sixth edition now. One of his students, who is now a professor at Stanford, Joseph Franzini, is the latest one who coauthored the new edition with him. It is still quite popular.

Professor [Robert T.] Knapp [professor of hydraulics, d. 1957], I don't know if you've heard much about him. A very fine engineer and excellent experimenter; he founded, or was one of the principal founders and designers of, the pump lab, which later became the Hydro Lab [Hydrodynamics Laboratory], which is still over there, in the basement of the Guggenheim-Kármán complex.

I took a course from [Franklin] Thomas. And [Frederick J.] Converse [professor of soil mechanics, 1933-1962, d. 1987]. Converse was one of the few who had a company on the side. That wasn't all that usual at that time. He had a soil foundation company, which is still in existence. He is not living anymore, but he got to be well over ninety. He taught a course in what would be called applied mechanics now.

I took a course from [William H.] Pickering [professor of electrical engineering, d. 2004] on electricity and magnetism, which included several lectures on the design of electric motors. Charlie [Charles C.] Lauritsen [professor of physics & director of Kellogg Radiation Laboratory, 1930-1962, d. 1968] was my instructor in a physics course when I was a sophomore. He taught one of the sections; these were sections of about twenty. Later I had another course from him, when I came back to school for the master's degree; it was a first-year graduate course in physics.

ERWIN: What was Franklin Thomas like?

SABERSKY: I had only one course from him. He was a very low-key person. He taught a highly organized course on civil engineering, with emphasis on the design of structures. A very friendly, low-key, reserved person. I think he was the one who appointed me to my first assistantship. I vaguely remember I was supposed to help in the drafting course, or descriptive geometry course. I mentioned to him that I really knew nothing about it and was ill equipped to assist, let alone teach, such a course. He brushed away my objections by saying something like, "Well, you don't know anything about it. Neither do they. You go and teach the course and if you're honest, you'll learn a lot." [Laughter]

ERWIN: Was he the chairman of the division then?

SABERSKY: Well, I can't answer that. I don't know if we had a formal chairman or not. [At that time, Franklin Thomas was chairman of what was called the Division of Civil and Mechanical Engineering, Aeronautics, and Meteorology.—ed.] I was not really aware of it, if there was. The structure was not quite clear—to me, anyway. He certainly was one of the senior people. But of course there was also [Romeo R.] Martel; and, in the mechanical engineering area, Daugherty. Thomas is now thought of as the chairman during that period, and this is why this building [Franklin Thomas Laboratory of Engineering] is named after him. If he had that title formally, I don't know.

ERWIN: It's interesting that you should say that, because it seems to indicate that there wasn't the same sense of the presence of an administrator.

SABERSKY: Right. And, of course, the administration still maintains a relatively low profile, but it was lower still then. Our senior man—by "our" I mean the mechanical engineering area—was Daugherty, and I knew he didn't formally have the title of chairman, but he was considered to be the chairman for all practical purposes. Also, I don't think anybody was called executive officer, either.

ERWIN: Things seemed to get done in a pretty collegial manner?

SABERSKY: Yes. And, you know, it's not all that different now. It varies a little from division to division, but things are fortunately still fairly informal, although there's certainly a little more structure now.

ERWIN: Did you know that you were going to start off in mechanical engineering when you came?

SABERSKY: Pretty much, yes. I had started in mechanical engineering in Zurich already. I have to admit, however, that I really didn't know what it was, and I was very pleased, in later years, to find out that the kind of thing that was called mechanical engineering here was exactly what I enjoyed doing. Particularly in Europe and in some schools here, it was a much narrower, more limited type of engineering—much more handbook engineering, which I wouldn't have liked at all. So it was a selection that turned out well.

Of course, mechanical engineering underwent changes here too, and I'll try to indicate that a little bit as we go along.

I might mention a couple of other things. We had public physics lectures. We had a physics course, similar to Ph1 or Ph2, and the students were grouped into sections. We would have these sections and we would learn everything we needed to learn in those sections. But then there was kind of a general physics lecture once a week, intended more for general culture rather than for learning a specific subject.

ERWIN: Were these the demonstration lectures—what became the Friday evening lectures?

SABERSKY: No, it was selected to fit in with the course material a little more, but not too closely. It was a lecture that had, in general, something to do with the area we might study, but it was not something where you would take notes and then be asked questions. I remember one lecture where one of the well-known physicists, and I don't remember exactly who it was, explained in detail that atomic fission as a sustained process for energy release wouldn't be possible. You would always have to put in more energy than you get out. Of course, it was only a few years later that the first atomic bomb went off. But that was the attitude at the time, of at least some of the physics staff.

I want to mention a few other people I had contact with during my undergraduate years. There was [William B.] Munro [professor of history, d. 1957]. "Three-button Benny," he was called, because he was very formal and always had three buttons of his coat buttoned. He taught history, and one term was devoted particularly to the Constitution of the United States. That was a required course for graduation. For some unknown reason, that course was given in the last term of the senior year, and if you didn't pass it, you couldn't graduate. [Laughter] It was never clear to me why they waited that long and why there was all this dramatic kind of a thing that somebody hadn't passed and had to be called back and redo the exam just in time to still be able to graduate.

For lectures in economics, they would often ask some prominent member of the financial community to teach. I remember particularly a very fine man, Cosgrove, who came and taught a course. Another teacher from whom I learned a lot was Jim [James W.] Daily, who now lives here in Pasadena. He was interested in thermodynamics and fluid mechanics, and he was working with Knapp and Daugherty in designing and operating the pump lab.

ERWIN: Was he a staff member?

SABERSKY: I don't think he had a permanent appointment. He was maybe a postdoc or lecturer, or something of this nature.

ERWIN: [Reading from Caltech bulletin] James Daily, teaching fellow in 1938.

SABERSKY: Now, a few words about the curriculum. The curriculum here was very structured. By that, I mean most of the courses were required and there really wasn't much leeway, but neither I nor my classmates objected to that. We had full faith in the wisdom of the faculty. And things did work out pretty well that way, but there wasn't much flexibility in what you could take. That is quite different from what it is now. Also, there was emphasis on breadth. For example, students in mechanical engineering would have to take a course in electrical engineering, a basic course in civil engineering, and so on. That's why I took a course in electrical engineering from Pickering, and the electrical engineers in turn had to take a course in thermodynamics. That requirement persisted throughout the war and up to the fifties. I still remember that Carver Mead [Gordon and Betty Moore Professor of Engineering and Applied Science, emeritus], who was also a Caltech undergraduate—he was in electrical engineering—had to take a course in thermo. I was teaching at the time, and he is very kind and generous enough to occasionally mention this fact. Again, to repeat, there was this requirement of breadth, and everybody in a particular engineering area had to take courses in other engineering disciplines.

ERWIN: There was breadth, in the sense that you were taking courses in the humanities.

SABERSKY: That was always true at Caltech, and a fairly large percentage, I think it was almost always something like a quarter of the curriculum, had to be devoted to humanities.

ERWIN: Did people balk at that?

SABERSKY: Not really. It was very surprising to me, because certainly in Zurich there wasn't any requirement like that. It was assumed that you had had plenty of humanities in high school and that when you were finished with that, you would specialize. But there wasn't any major objection [here] to the high percentage of humanities courses.

My second summer, I think it was between sophomore and junior year, I worked in the pump lab, particularly for Daily. That was a very fine experience and an exposure to real engineering. I enjoyed that very much and learned a lot. I also got paid and, you know, pay is important. But we very often remember much more something unexpected, and I remember once we were working through the lunch hour, and it got late, because we tried to finish something; I think it was calibrating a Venturi meter. Suddenly Jim Daily came with hamburgers for everybody, which he himself had bought for us. That did a lot for our morale, and I remember that more than how much I got paid for the summer work. [Laughter]

That's the undergraduate part. You mentioned something about the Westside [émigré] community, and I tried to think about something, but I didn't have an awful lot of contact there. A lot of famous people had accumulated there, but I really didn't have much contact with anybody. I do remember two talks that were given, one by Thomas Mann and one by Sholem Asch. There was a little synagogue on Fairfax that I didn't belong to but I went there at times, and they had, every so often, an evening lecture. They would draw on speakers from the community, and I remember very well Thomas Mann reading one of his short stories. That lecture made a real impression on me, and ever since then, when I read something by him, I can almost hear his voice reading the lines.

ERWIN: Was the idea to give a rather formal presentation?

SABERSKY: Well, he just read one of the things that he had written. In fact, it got translated into English; I think it's called "The Law." [The story is titled "The Tables of the Law"—ed.] It's a very nice little essay on Moses and is very charmingly written. I also mentioned Sholem Asch because he was a very strong, impressive personality. He wrote several novels, which are very readable, but he's hardly known anymore. I don't know if he was born in New York or not, but he lived most of his life in New York. He wrote, among other books, *Three Cities*, and *The Nazarene*, and *The Apostle.* Very readable, very fine books.

Pearl Harbor occurred during my senior year. Of course, you would certainly remember that. There was a Professor [W. Howard] Clapp [professor of mechanism and machine design, 1918-1943]; I haven't mentioned him yet. He was a machine-design professor, a very good designer and a fine person. He would invite small groups of students to his house. It was a Sunday evening, and we were at his house when the news came through. As is often the case, you are kind of thunderstruck but don't quite grasp the full meaning of the event.

For the first few days, things on campus didn't change very much. But pretty soon we tried to guard the campus. There was this group of students who were organized to guard the campus.

ERWIN: What were you guarding the campus from?

SABERSKY: There was defense work going on in various buildings. There was the Hydro Lab, for one thing; they were testing torpedoes. And in [Charles C.] Lauritsen's lab they worked on several war-related projects. The students organized some kind of a guard. I was still an enemy alien, so I couldn't be a guard, but I was the secretary for the outfit. We had an office in lower Throop. I kept records on who was standing where and how long, and so on. [Laughter]

ERWIN: Was that just because you weren't a U.S. citizen at that time?

SABERSKY: I wasn't a U.S. citizen, and, of course, the last citizenship that I had had was that of an enemy country. So that made me an enemy alien.

ERWIN: And you were legally of that status?

SABERSKY: Yes. Well, the guards were soon replaced by professional guards. We continued our studies fairly normally. Because of being an enemy alien, there were certain restrictions about moving around. At night, I wasn't supposed to leave the place where I lived. The restrictions were all very manageable, and the government was certainly very reasonable.

ERWIN: You didn't feel in any kind of personal jeopardy or danger?

SABERSKY: No, not at all. There were some of these general rules, and they were enforced in a very reasonable way. But you were supposed to be at home after dark. Also, in order to go to West Lost Angeles, where my parents lived, I had to have a special permit. But as I say, that was all very minor.

ERWIN: Would that have applied to your parents as well? Were their movements restricted?

SABERSKY: Yes. I think it was ten miles from the house and, again, to be at home at night.

ERWIN: What were your parents doing at this point?

SABERSKY: Well, my father was essentially retired by then. He got involved in a little business here and there but did not really have a regular job.

After my senior year, I got a job with the group that was designing the Co-op Tunnel [Southern California Cooperative Wind Tunnel]. The design office was over here in Guggenheim. I got a little exposure to mechanical design. The man in charge of it was [Mark] Serrurier; he became a distinguished alumnus, a few years ago [1981]. He was in charge of the overall design, and another Caltech graduate, Hap Richards, headed a subsection. Maj. [Arthur L.] Klein [professor of aeronautics, 1929-1968, d. 1974] would come by every once in a while and consult on the design. He was professor of aeronautics and one of the very bright people on campus.

ERWIN: What did you call him, the first name?

SABERSKY: Well, people always called him "Major." And I don't know where it came from. He had been a major somewhere. [Laughter] His true name was Arthur, but "Maj. Klein" is how he was referred to here.

There was always concern about the draft, and people started to register for the draft. The studies, however, went on more or less normally.

Then after the summer, I started graduate work, and I lived in the Old Dorm. There was really quite a change between being an undergraduate and a graduate student. The undergraduates were still pretty much treated like high school students, and in the graduate work, you really felt that you were more or less a professional, and you studied, because that's what you needed in order to advance your knowledge. You became much more in charge of your own life.

The campus, of course, was very much aware of the war. We added a civil defense unit, among other things. I was a member of the campus fire brigade. One of my fellow members was [J. E.] Wallace Sterling, who later became the director of the Huntington and then president of Stanford. The member of the brigade who knew most about fire-fighting, however, was the head of our mechanical-engineering machine shop, [Ray] Kingan. He had been a regular fireman before he came to Caltech.

ERWIN: Did you maintain your own equipment and keep special hours?

SABERSKY: No. We were mainly on alert. There was a little rig, and we knew where to go and how to pull off the hose and put on the nozzle. Sterling was very good at that, and he and I often pulled the hose off the truck. The fire chief at that time, incidentally, was Edmundson. You may have seen, around town, tire stores called Stanyer and Edmundson Goodyear tire stores. Well, that was a brother of the fire chief of Pasadena.

As to the academic part, it was mainly a matter of taking courses. The teachers from that time I remember very well. Dr. [Donald E.] Hudson [professor of mechanical engineering and applied mechanics, emeritus], in particular. He was one of the first that I felt taught a really sound, scientifically based course. It was a course in vibrations. That was kind of an eye-opener. He made things very clear; he was an excellent lecturer. We got to know each other, and we are still close friends. Dr. Clark also taught courses. Then I took one course from [Robert C.] Bromfield. He was a Caltech graduate, and he later founded various companies. He is a very enterprising kind of a fellow; he comes and visits once in a while. I also took a math course from [Abe M.] Zarem. He founded Electro-Optical Systems in Pasadena, which he then sold to Xerox. He's involved in all sorts of things. He's a Caltech Associate, and he comes to Caltech once in a while. Another teacher I remember well is [Peter] Kyropoulos. Kyropoulos was a very interesting man; I learned a lot from him. He had close contacts with General Motors and was devoted to engines. I learned a lot from him about real engineering problems as well as about a proper general professional attitude. He later went to General Motors and worked there for the rest of his life.

Begin Tape 1, Side 2

SABERSKY: After one year, then, we all got the MS degree [1943]. I was particularly close to one fellow student, Jack [John T.] Bowen. Bowen had taken a job at Aerojet [Engineering Corporation] at the time. Now, Aerojet was located at [285 W.] Colorado Boulevard, exactly where the Rusnak automobile agency is now. It was an automobile agency at the time also; of course there were no automobiles being sold during the war, so it was empty space, and that's what Aerojet rented. Bowen worked there. I only had the vaguest notion what the company did, because their work was all confidential and restricted. Then one day I got a call from A. M. O. Smith, who was chief engineer at Aerojet, asking me if I wanted to work there. I had gone to various interviews but I hadn't made up my mind yet, so I did accept the offer. A. M. O. Smith, let me point out, still lives around here. He was the first chief engineer at Aerojet. He then went back to Douglas Aircraft and retired from [McDonnell] Douglas maybe five years ago or so [A. M. O. Smith retired in 1975—ed.], an outstanding man in fluid mechanics. He did some very fascinating work in that field. But at that time, he was asked by [Theodore von] Kármán [director of the Guggenheim Aeronautical Laboratory at the California Institute of Technology (GALCIT), 1930-1949, d. 1963] to come to Aerojet. And you know the history of Aerojet; it was founded by Kármán, and some of the key people there were Kármán's associates.

ERWIN: Had you known Kármán much prior to that?

SABERSKY: No, not really. I once got his signature when I was made a member of Tau Beta Pi. That's an engineering fraternity organization of undergraduates. One of the things you had to do as a pledge was get the signature of every other Tau Beta Pi member on the campus, and Kármán was an honorary member, so I got his signature. And I'm glad I did; it's kind of a nice collection of signatures of people; a lot of them became famous later on.

So I went to Aerojet, and that was a very, very fortunate thing for me to have done. It started an association with Aerojet which essentially lasted from then through 1970.

ERWIN: So you consulted for them.

SABERSKY: Yes, later on. But at first I worked there full time, from 1943 to 1946. In fact, I started July 4th; there were no holidays then. A. M. O. Smith is the one who had called me and hired me, but it turned out that I was going to work for Martin Summerfield, who is a Caltech graduate also. He got his PhD in physics, and he got to be close to Kármán. He was in charge of a particular development project. I got to work for him, and this was a very lucky thing. He was really a wonderful kind of a guy. He was not only very intelligent and very smart and very knowledgeable, but he had a large amount of enthusiasm, enough to get everybody else inspired. It introduced me to industrial engineering in the most favorable way possible. I had a couple of associates with whom I became close friends—Chan [Chandler C.] Ross and Marvin Stary, both of whom are around here. We really had a wonderful time doing engineering work.

ERWIN: What were you doing?

SABERSKY: We worked on sustained-duration liquid rocket engines. Aerojet built the first sustained engine, meaning one that in principle could run for a long time. Now, "a long time" always turned out to be five minutes or something like this, but the solid-fuel rockets that Aerojet started to build, the jet-assisted take-off units, had operating times on the order of twenty seconds. The sustained engines had a large storage of liquid propellants, which then were pumped into the thrust chamber. You could run it as long as the fuel would last. So I got a lot of exposure to the design of these combustion chambers and the pumps; I got to work a lot with pumps—pumps for the propellant.

ERWIN: Who was actually using these?

SABERSKY: These were development projects; the applications were not all that definite. One possible application was to high-speed airplanes. There was one developed in Germany about the same time. Here, Northrop had a plane—at least on the drawing board—that might have used one of these engines. The airplane project was called Avion. Mr. Northrop came to see us once. I didn't really meet him, but he was there talking to Summerfield. Later on, it turned out that these sustained liquid rocket engines were the engines needed for our missiles; The Titan engine is an outcome of that. Now, these missiles were not quite visualized at that time, but it turned out that these were the first steps toward that kind of a unit. So this was all new. We really had a chance as the first ones to develop pumps for these propellants and the combustion chambers, the injectors for it, and to learn all about these propulsion engines and particularly about the heat transfer. The combustion temperatures in the rocket chambers are very high—5,000 degrees or so—and it took quite a lot to cool them. In fact, I had always been interested in heat transfer even in my undergraduate years, and the rocket work certainly reinforced my interest. I got a chance to use my heat transfer knowledge, and later I continued research on heat transfer when I came back to Caltech.

At Aerojet, there was cooperation with JPL [Jet Propulsion Laboratory]. JPL also did work on rockets, and they had test facilities, and some of the things we designed were tested there. Cooperation is the right word, although there was a lot of rivalry between the two.

ERWIN: How was JPL set up at that time?

SABERSKY: It also was set up by Kármán, but it was closer to Caltech and of course was not commercial. It was set up as an arm of Caltech, pretty much like now, but at the time I think it was funded by the army and not by NASA. But it was completely supported by the government for development, and Aerojet was supposed to do the more commercial work. Aerojet was started by supplying jet-assisted take-off units to airplanes. Those were solid-fuel rocket motors, kind of little bombs that were attached to the sides of the airplane and were supposed to help them take off, particularly from short runways.

ERWIN: The aeronautics part of the engineering division always seemed to have a kind of independence to it. Is that true?

SABERSKY: You're very observant. And it still does in a way. In a way, it's supposed to be just another group within engineering, and it has an executive officer and so on. But they do have this title of director of GALCIT [Graduate Aeronautical Laboratories, California Institute of Technology]. Although the director reports to the division chairman, he has a somewhat special position.

The aero group was more independent then and right after the war than they are now, I think, partly because [Clark] Millikan was the head of GALCIT [then named the Guggenheim Aeronautical Laboratory of CIT], and Millikan had a lot of personal stature.

ERWIN: Kármán, before him, had the personal stature, too.

SABERSKY: Yes. Now, how it worked in those days I'm not so sure, but it was no doubt similar. I don't know enough about the organization to know if Kármán was supposed to report to [Franklin] Thomas or not. But I think that whatever was done was done directly with [Robert A.] Millikan. I remember one case where Kármán said he told Millikan, "I think I have a solution for this problem." And Millikan replied, "Do you think it is the best solution?" Whereupon Kármán countered with, "*A* solution is better than anything I had hoped for." [Laughter] So he apparently reported directly to Millikan. When the son, Clark Millikan, was in charge of aero, it was a little bit that way, too. Aero was internally certainly much better organized. For example, when the aeronautics group presented a proposal for appointing a faculty member, there was rarely a question. They had enough clout to get it through, which was not at all true for the other groups. But I don't know that this was ever formally acknowledged.

ERWIN: There's something called the Guggenheim Jet Propulsion Center, which came into existence in 1948.

SABERSKY: Yes. I think this was purely to increase confusion. [Laughter] That's facetious. What happened is that [Harry] Guggenheim sponsored jet propulsion centers in several universities; I think there was one in Princeton and one in Purdue and one here.

ERWIN: These were academic entities?

SABERSKY: Yes. And I think he asked that they be named Guggenheim Jet Propulsion Centers. So that's why they're named that way. That group, however, certainly was a part of the engineering division. Unfortunately, the name of the group is similar to "Jet Propulsion Lab," but there is no organizational connection between the two.

Now, the Guggenheim Jet Propulsion Center was not entirely a part of aeronautics but, rather, was connected to both aeronautics and mechanical engineering. The head of the center, I think, was called the Goddard Professor of Jet Propulsion. The first one to hold this position was [Hsue-Shen] Tsien, and Duncan Rannie succeeded him. The center was definitely a part of the engineering division and, again, there was no connection with the Jet Propulsion Laboratory except the kind of contact that any faculty member can have with JPL.

ERWIN: But they were working on the same kinds of things.

SABERSKY: Yes, but the approach [of the center] was broader and directed towards more fundamental questions in the field, such as combustion and fluid mechanics. The center could have been called "Fluid Mechanics Center" or something like this, just as well.

Now, to heighten the confusion, Rannie, as well as [Frank E.] Marble [Richard L. and Dorothy M. Hayman Professor of Mechanical Engineering and professor of jet propulsion, emeritus] had in the past worked at JPL; but, again, there is no formal connection between the center and JPL now.

ERWIN: Do you happen to know how Tsien came to be the first director?

SABERSKY: Yes. He was one of Kármán's outstanding graduate students; he was very close to Kármán, and Kármán thought a lot of him. He was a very exceptional person, and when the war was over he was the logical one to head the center.

ERWIN: So he had done all of his graduate work here.

SABERSKY: His graduate work here, yes. And he wrote papers with Kármán and worked with him closely.

ERWIN: We were talking about Aerojet.

SABERSKY: Now, I pointed out the influence that Summerfield had on me. I'm still in contact with him, just got a letter from him recently. He is still very active, and he runs a laboratory in the Princeton area—a commercial organization, Princeton Combustion Research Laboratories. The company is conducting combustion studies, and Summerfield himself still has this great enthusiasm. It's really a wonderful experience to spend time with him.

Another person with whom I had contact from my Aerojet days was [Fritz] Zwicky [professor of astronomy, d. 1974], because Zwicky was an Aerojet employee then, too. I had the good fortune to see him quite often, for example, at dinner after work. He had dinner in the Athenaeum [Caltech faculty club] or in the Pasadena Athletic Club, and some of us would join him there. So there was a lot of discussions and talk. He was a fascinating person, with all kinds of ideas and a really very original mind and a very forceful personality. I had the good fortune not to work for him. I think it must have been very difficult to work for him. In fact, this good friend of mine, Bowen, found that out. He started out working for him, but that didn't last very long.

ERWIN: When did Zwicky come on the faculty?

SABERSKY: He already was on the faculty, I think [Fritz Zwicky joined the Caltech faculty in 1925—ed.], so he may have been on a leave of absence from Caltech while he was at Aerojet. More and more people recognize Zwicky's stature these days. Somebody from astrophysics here pointed out just the other day that he thought of Zwicky as one of the really outstanding people in astronomy and that Zwicky had done much original work and had started the work on supernovae. I think he was not fully recognized at the time because he himself was always the first to point out his great achievements; that, in turn, always raised a sense of opposition in others. But now that he cannot be his own agent anymore, people are beginning to recognize more and more on their own his stature and the importance of his work. Zwicky was much involved in Aerojet's work. There's a big book on him, a biography that came out recently. It turns out that Zwicky kept very accurate notes, a diary, on what he did day by day, and the book is based on this diary. Aerojet is mentioned on many pages of the diary.

ERWIN: He was Swiss, is that correct?

SABERSKY: Yes. And he never did become a citizen of the U.S., because he felt that the United States wouldn't grant him full citizenship, as there is this restriction that a naturalized citizen cannot become president. He thought that way he would be a second-class citizen, and he didn't want that.

That didn't cause any trouble until much later, in the fifties or so. All during the war, that didn't cause any problem. He was involved in all the high-level conferences and whatnot; he was not restricted in any way. It caused some problems later.

Another person I met there [at Aerojet] and whom I got to know much better later on is Bill [William E.] Zisch, a really fantastic kind of a person, too. I credit him with the great rise of Aerojet that took place in the fifties and early sixties. He also was brought to Aerojet by Kármán. He started at Aerojet in an administrative position and later became involved in top management. His influence was very strongly felt, and I think the growth of Aerojet into a major company in the propulsion field is largely due to him. I got to know him a little bit, and I think he was interested in listening to me—not necessarily that he did what I might suggest, but he was interested in talking to me. And it was due to that that my later connection with Aerojet was maintained for twenty years or so.

ERWIN: Due to your connection with him?

SABERSKY: I think so, yes. Then when his influence decreased, and a new management took over, later on, in the seventies, then my connection also faded away. Of course, Zisch is very active now as a trustee, and he's also vice chairman of Science Applications International, a company with headquarters in the San Diego area.

ERWIN: What is that?

SABERSKY: That's a large consulting company. It's a company which has dozens of offices all throughout the country and does special projects, mainly for the government. Their main office is in La Jolla. Another one is in the Washington area, and there's one in the Newport, Rhode

Island area.

Let me mention one more thing before leaving the subject of Aerojet. At the end of the war, early ’46, I was asked to go to Washington to make some contacts with the navy and other government agencies. In that connection, I had the opportunity to see the people at the Applied Physics Lab [at Johns Hopkins]. There, as a young lieutenant, was Jim Van Allen. He’s about my age, and we were both reasonably young at the time. We were asked to work together, and he and I outlined the design for the Aerobee. The Aerobee was an exploratory rocket for space exploration and atmospheric exploration. It was a very useful unit, and that contract went on for something like thirty-five years at Aerojet. Actually, Van Allen referred to it in a book to which he contributed a chapter on the history of the sounding rocket; that’s what those rocket units were called. [Van Allen, James A., John W. Townsend Jr., and Eleanor C. Pressly, Chap. 4, “The Aerobee Rocket,” *Sounding Rockets*, ed. Homer E. Newell Jr. (New York: McGraw-Hill, 1959)] He mentions these meetings in Washington in early ’46. He had just gotten married. And he was very understanding of my situation, as I was trying to arrange a date with my future wife in New York. I tried hard to make contact with her from his apartment in Washington, and he was helpful in facilitating my telephone calls to New York.

Then, in ’46, with the war over, there was a question whether I would stay on with Aerojet or not. I did not, and I came back to Caltech. But my ties to Aerojet have always been very strong, and I still know a lot of people there. Apparently, there are many ties between former Aerojet employees, and as a consequence some old Aerojet people still get together for lunch every couple of months. The next luncheon is next Friday, here in the Athenaeum. [Laughter]

In 1946, then, I came back to Caltech. Engineering certainly had a little different atmosphere from before the war.

ERWIN: What made the decision for you?

SABERSKY: Well, I felt that the thing to do was to do some more graduate work. I had a master’s degree, but some advanced graduate work seemed to be necessary for a sound engineering career. It would have been very attractive to stay at Aerojet, and it wasn’t an easy decision. I remember [Dan A.] Kimball, who was the executive vice president and who also had a lot to do with Aerojet's growth, called me in and wanted to know why I wanted to go back. He said, "Well, a doctor's degree. You know you can do it if you want to; you don't really have to prove it." But I'm certainly glad I did go back.

ERWIN: Were you married by that time?

SABERSKY: Yes. So it seemed like an unreasonable time to go back to school, but that's what I did. As I said, the atmosphere at Caltech had changed, partially because of the new people who were now here and who were setting the tone. I guess [Frederick C.] Lindvall [chairman of the engineering division, 1945-1969] was principally responsible for that, as he assembled the faculty team. The person I got to know and worked with was Duncan Rannie, a very outstanding man—very quiet, very low key, but with perfect integrity and a deep understanding of engineering and science. It's really from him that I learned what engineering science and research is about, what engineering is based on, what the basic laws are, and what the assumptions are. I learned from him to think clearly about a problem and to interpret your results objectively.

ROLF H. SABERSKY

SESSION 2

April 12, 1990

Begin Tape 2, Side 1

SABERSKY: I had just said a few words about Rannie. He was going to be my advisor when I returned from Aerojet to continue my studies after my master's degree. I think I may have mentioned that he was really an outstanding personality, a true scholar, who had a deep and clear and rigorous understanding of fundamentals. I learned a lot from him. He was very professional, with perfect professional integrity. He was also very low key and abhorred ostentation. To some degree, that fitted Caltech pretty well. He had worked closely with Kármán. He was one of the real, true students of Kármán, as he got his degree with Kármán as an advisor, and Kármán thought very highly of him. He was out in industry during the war. He also had just come back, and he had received a faculty appointment here. A close friend of mine, Jack Bowen, and I were Rannie's first advisees. We worked together on a project which, of course, was of interest to Rannie. He had worked on axial flow compressors when he was out in industry, and there were many problems to be investigated. He had outlined a project to design and investigate axial flow compressors. We started essentially with nothing, and the first task was to actually build a great big compressor. All of the construction and mechanical design had to be done or organized by us. I think Bowen did most of it; he was an excellent engineer. The test compressor was located in this building [Franklin Thomas Laboratory of Engineering], in the subbasement. The project was done under navy sponsorship, and eventually, when we didn't use the compressor anymore, the navy took it to Monterey, and for all I know, it's still there.

ERWIN: Was the building named Thomas?

SABERSKY: Yes, this building. Well, now, wait a minute, I don't think it had a name then. I think the name Thomas was added later. I should also mention that this building consists of two parts. There's the old part and the new part; we are in the old part, and if you look outside, there's a metal plate there that covers up the dividing line between the two buildings. The new one was added while we were working on the axial flow compressor project [1950]. It may well have been at that time that the building, as a whole, was given Thomas's name.

During that time, after coming back, I also took some courses. Among the faculty who gave the lectures were Carl Anderson [professor of physics, d. 1991] and Vic [Henry Victor] Neher [professor of physics, d. 1999]. Vic Neher comes around and visits every once in a while; he lives up in Watsonville. I also took a course from Charlie Lauritsen, that's the older Lauritsen. I mentioned before that he taught a course when I was a sophomore, but then I took another course with him when I came back to school after the master's degree and being at Aerojet. I also remember very excellent courses that I took from Clark Millikan and [Hans] Liepmann. Those were the people who were very active in teaching at that time.

Eventually, in 1949, I got my [PhD] degree. Then there was a question of what I would do now. I interviewed at a number of industrial places and also applied at Caltech. I did accept the Caltech offer, and I was an instructor first, for a year. That category, I'm not sure if it exists anymore. At the time, new faculty members would frequently start as instructors, so that's what I was for a year.

ERWIN: Was that sort of a probationary category?

SABERSKY: Well, I don't really know. That's an interesting question. You know, tenure wasn't a major issue at that time. None of us worried about tenure at all. It wasn't such a big step, and it kind of occurred in the course of promotions. I'm not sure if the rule was in effect then that the institute has to decide after six years whether to grant tenure or terminate the appointment. I'm not sure that that rule existed. Somehow, tenure wasn't much on the mind of any of us. So just exactly what the position of instructor implied I am not sure, but it was the first step.

I also, at that time, started a rather regular arrangement with Aerojet, by which I would consult there once a week. That met the Caltech regulations, and this arrangement with Aerojet turned out to be really most interesting. The two activities I pursued at the time complemented each other very well, and I benefited a lot from the industrial connection and appreciated it very much. That arrangement lasted for some twenty years.

ERWIN: Who else came onto the engineering faculty about this time?

SABERSKY: The people I recall who joined at about the same time were Dave [David Shotwell] Wood [professor of materials science, d. 1998], who is in materials science and recently retired but, like me, continues to come to the campus pretty much every day. Charlie [Charles H.] Wilts [professor of electrical engineering, d. 1991], Bob [Robert B.] Leighton [Valentine Professor of Physics, d. 1997], and Frank Marble. Of the people already on the faculty for some time and with whom I had the most contact, I recall especially Rannie, Hudson, Kyropoulos, and [Dino A.] Morelli [professor of engineering design, d. 1972]. They were all most capable, very fine, effective and interesting people. Kyropoulous later went to General Motors; his main interest always was cars. Morelli was a very vivacious, outgoing, machine designer, a unique personality. Those persons to a large degree determined the atmosphere in the department.

ERWIN: And Lindvall.

SABERSKY: Lindvall was the division chairman.

ERWIN: You didn’t include him in that group; I just wondered why.

SABERSKY: Lindvall was the division chairman, a level above the regular faculty, and I didn’t have all that much direct contact with him.

ERWIN: I see. Those people you mentioned were the ones you had most contact with.

SABERSKY: Right, whom I would interact with and who had a direct influence on what I would learn and what I would do. Lindvall did that in a much more indirect way. In addition, Lindvall was also very low key. He was rather effective in getting things done, but his way was certainly low key.

I think I started out teaching thermodynamics and then fluid mechanics and later heat transfer. My colleagues and I rotated teaching these courses periodically.

Now, the curriculum at that time was still very rigid. I mentioned before that when I was a student here, it was pretty rigid, in the sense that the courses you had to take were pretty well prescribed and there wasn’t much flexibility that would have allowed you to select other courses. The required curriculum was fairly broad, in the sense that the students in electrical engineering would have to take thermodynamics and the mechanical engineers would have to take electrical engineering courses. That, incidentally, made Carver Mead take thermodynamics, and that's where I met him the first time.

The curriculum remained pretty much unchanged for quite a few years, but then a few things happened, and I'm not exactly sure in what sequence or exactly what year. But around the middle fifties, there began to be a large drop in enrollment in what I call the non-electrical engineering activities: That's the engineering activities excluding computer science and electrical engineering, so it's mechanical, civil, aero, and so on. I've kept charts ever since then, and this chart shows the number of students—or, rather, graduating seniors—in all the areas I mentioned as a function of the years. As you can see, somewhere around '55, '57, there's a large drop in enrollment, and the number changes from around forty to around twenty or so. It's always hard to trace down what causes things like that. But during that time, the emphasis—and perhaps more important, the publicity—was all directed towards science and physics. No matter whether a spectacular project was essentially an engineering project or not, it was credited to physics. This was true for all the nuclear work, for all the space work and satellite work. Students in high school got the impression that these feats were all the work of physicists, and if you wanted to be part of that, you had to study physics. So the young students came here and they all enrolled in physics. As a consequence, the number of students who selected engineering, and particularly the non-electrical type of engineering, decreased very significantly during that time.

ERWIN: Why did you make the distinction between the electrical and the non-electrical?

SABERSKY: The distinction is based on the kind of courses people would take. Those interested in aero and mechanical and civil engineering formed a fairly close-knit group, taking rather similar courses. Those interested in electrical engineering formed a clearly different group, interested in electronics, semiconductors, and so forth. So there were two distinct spheres of interest, among the students as well as the faculty. The two groups also had different histories and traditions.

ERWIN: Yes, I understand electrical engineering had for a long time been in the physics division.

SABERSKY: Yes. I think that may have changed just before or just after the war.

ERWIN: 1949 to ’50, I have here.

SABERSKY: OK, so it was right after the war. Bob Leighton, for example, graduated in electrical engineering when it still was part of physics. And Charlie Wilts, too. They had just graduated before the war and they were in that area. Now, Lindvall also was an electrical engineer, basically. His interests were very broad, but I think technically he was an electrical engineer.

Something else happened with the curriculum. I mentioned that it was pretty structured. Then, in addition, people tried to add more and more courses to it. There’s always this tendency—and I’m sure you’ve run into it in your field, too—that the faculty feels that every engineer should have had a course in materials science and should have had a course in applied mechanics and should have had a course in surveying, and then you can go on—business law, and ethics, and all that. Pretty soon the curriculum tended to become unmanageable; there were many too many courses.

ERWIN: What period are we talking about?

SABERSKY: Again, the late fifties. Realizing all that, then, the faculty made a complete 180-degree turn. And both of these factors may have played a role, the decreasing enrollment as well as the overloading of the curriculum. They sat back and said, “Well, really, it doesn’t make all that much difference exactly what courses everybody took. The important thing is that each student would take some real good courses.” As a result, the requirements became very flexible, and the option “engineering and applied science” was started for undergraduates. We did away with all the special engineering options, such as mechanical and civil engineering. The curriculum was now very flexible. You just had to select, essentially, so many units of engineering courses. There was a realization that engineering was changing very fast and you couldn’t educate people anymore to design a steam turbine or an internal combustion engine or a bridge or a dam or something very specific like that. It was realized that it wasn’t likely that somebody would go out and get a job in which he would be required to design, on his own, something specific, such as an internal combustion engine. Instead it was thought more appropriate to prepare the student for a much broader range of things. The changes, I think, worked out well, and it turned out that students took some good courses—fundamental courses—and learned how to attack a problem. They were then well prepared to make up on their own the detailed knowledge required for a particular specialized field. And after all, you know, if you take a term's course, that's about ninety hours, or two to three weeks, of full-time study. If somebody knows how to attack a new field, it is quite feasible to make up for such a course in a relatively short time. In addition, our industry is quite prepared for that, and the older employees are generally quite willing to teach a newcomer some of the basics of their specialty. This system, fortunately, works very well. Actually, I have had the opportunity to see some of our BS graduates who went into industry, and I could see that they really were able to tackle almost any kind of problem. They're good in the machine shop, and they're good in writing proposals, and they have the necessary mathematical and analytical background. If it's something new, it doesn't take them very long to catch on. So the principle on which our curriculum is based seems to work very well.

ERWIN: It's not entirely clear to me whether this loosening up of the curriculum at this time came about mostly as a result of external pressures.

SABERSKY: I'm not so sure, either. I mentioned two possible influences, but I wouldn't be able to prove that those were the only ones. There was this decrease in enrollment, and that was certainly an outside factor.

ERWIN: You implied it was based on a misapprehension of what engineering really was.

SABERSKY: That's the way I felt. Public perception depends a lot on the press and the media, and also on the job market. I think what finally made it turn around, in the eighties, was the job market. It turned out that physicists didn't get any jobs anymore and engineers got pretty good jobs, so that's what turned it around. But then, at the same time, we realized that you can't—at least, not at Caltech—you can't have these narrow specialties anymore.

ERWIN: What about the influence of *Sputnik*?

SABERSKY: *Sputnik* occurred around that time, in '57. And it looks like that's just when the engineering enrollment dropped. I used that argument, but it was shown to me that it was not a very good argument, because those people who graduated in '57 had come to school four years earlier! So it may have been the atmosphere surrounding space travel and satellites that brought this about, more than the actual *Sputnik* event. Of course, the nuclear power development had a lot to do with it, too. Nuclear power was considered very high tech, and it was considered the domain of physicists, no matter what, even though all the problems were engineering problems.

This flexible curriculum, by the way, served very well, although I think we may have gone a little too far; it may be a little too flexible now. Over the last few years, a number of the faculty in Engineering and Applied Science have been pushing for something a little more structured. The reason I say that it's a little too loose is that although most of the students here will select a good set of courses and get a good education, there are always two or three each year who get by with a set of courses that don't hang together very well and don't make too much sense. I think, therefore, that it would be better to offer well-thought-out groups of courses and then let the student select one of these packages. Somebody interested in mechanical design would take a certain package, and somebody who's interested in civil engineering would take a somewhat different one, and so on. I think the faculty may be going that way.

ERWIN: When the curriculum loosened up, it became important for an advisor to step in and offer some guidance.

SABERSKY: Yes, and that worked well in most of the cases. But the advisor in our present arrangement has no power. So if a student wants to take the advice, and most of them do, it works fine. But if the student really doesn't want to, he or she can usually get around it. The catalog is law, and nowhere does it say anything in the catalog that there is an advisor or that the advisor has certain powers. Without changing the advisor system, however, you could list these course packages in the catalog and require the student to pick one. Even then, at Caltech you can always petition to get a modification. These modifications could then be considered carefully before being approved.

I was quite concerned at the time when there was this big drop in enrollment. It's really not that there weren't good students around; there were plenty of good students. But we had, at

Caltech, limited enrollment like we have now, and the freshmen who come here don't apply for a particular division but just to Caltech. So if all the physicists wanted to come to Caltech, well, they were taken in, and then there wasn't any room for engineers. Actually, at that time I even went to Dr. [Lee A.] DuBridge [Caltech president 1946-1968] once and suggested that we maybe accept students directly into the divisions. That suggestion, however, didn't find much backing. I guess in twenty years' time or so, the situation turned around by itself and now there are plenty of students in the engineering options. But I was a little unhappy at the time that we didn't have control over the enrollments in our division.

The flexibility of the organizational structure in the engineering division worked out pretty well, too. As an example, I might mention the way nuclear engineering was accommodated. There was a lot of pressure—I don't know if you remember that—for nuclear engineering in the late fifties. This was the thing of the future, and there was a lot of pressure on schools to establish nuclear engineering departments. A lot of schools did. It was important for students to be able to get the degree that emphasized the fact that they had studied in this area. It was also important for such reasons as getting fellowships and grants for nuclear engineering, which were available at that time. What Caltech did was to create an option in nuclear engineering. Now, what an option is isn't all that clear. It is a somewhat informal subdivision of the division. It's very easy to create an option as well as to abandon it. It doesn't take much red tape to do either. So we created an option in nuclear engineering. Later, when nuclear engineering became less important, the option just quietly disappeared, without affecting the faculty members, their courses, or their research. We had the same faculty before and after the nuclear engineering option was abandoned; just the title of some of the faculty members was changed from professor of nuclear engineering to, say, professor of applied mechanics.

In this connection, let me point out something else interesting. We had, over the years, emphasis on nuclear power, as I said, and emphasis on various other topics, such as energy and pollution and all kinds of things like this. As the topics changed, we could very easily establish groups that would work on those problems and make proposals and obtain fellowships, and so forth. It turns out that the basic science and engineering for all of these topics is the same. For any one of them, you still need thermodynamics and heat transference, fluid mechanics, and so on. This experience made it very clear that it's really the fundamentals that count, and the emphasis in fundamentals has served Caltech pretty well.

ERWIN: It seems, though, that in the late sixties there was a lot of emphasis on environmental engineering. But if, as you say, the fundamental work was largely the same as for other subjects, then it's a little bit misleading to see these new options coming and going.

SABERSKY: Somewhat. But the faculty certainly did address environmental problems vigorously, and the only point I wanted to make was that the research in this new field required the same fundamentals that were applied to the research on energy and nuclear power. The interests of our faculty and the curriculum we offered were entirely suitable for this new area. I might add that the environmental problems also involved a lot of chemical engineering. The chemical processes in the atmosphere and the study of very small particles as carried out by Professor [Richard C.] Flagan [Irma and Ross McCollum-William H. Corcoran Professor of Chemical Engineering and professor of environmental science & engineering] are examples. This development will lead to new research projects and eventually to new graduate courses as a natural development of the mechanical engineering field.

ERWIN: Well, there's social pressure—I guess that's what I'm driving at—social pressure to put labels on things.

SABERSKY: Yes, and let me say that the establishment of an option advertises quite correctly the fact that we are able to work in the new area and that we are interested in doing so. It advertises this fact to both graduate and undergraduate students, as well as to possible donors, which could be the agencies of the federal government or private people. So it's important to use these labels, like nuclear engineering or environmental engineering, to make clear to the outside that, yes, we are interested and prepared to work in these areas.

ERWIN: Did you ever reach the point where you got so many new labels on things that it began to be too diffuse?

SABERSKY: No, somehow that didn't really happen. For example, the environmental engineering option is well established, but in a way it is still a part of the civil engineering group—in particular, the group that was interested in hydraulics, the people in Keck [W. M.

Keck Engineering Laboratories]. The new areas are tackled by persons already on the faculty, and it's not that you have to get a different crew of people for each new area.

I might say a word about accreditation, which bothers us periodically. There exists professional accreditation for engineers. You have that, too, for lawyers and other professionals. The course program for these professions gets accredited. In this case, the accreditation is done by a group that was created by various professional engineering societies. We always have problems with them, because they like a course program that is very structured and written down and where the courses are exactly specified. Although we usually meet their requirements in spirit, it's very hard to prove that we do. So they come every six years, or if we are not behaving well, they come every three years. Each time, we have long discussions on whether we are accreditable or not. They like to specify so many hours of this subject and so many hours of that subject and a certain amount of engineering design. It becomes a little awkward. We have tried to remain accredited without really compromising our own ideas too much. So far we have been successful. But each time, it's a question of whether we should ask for accreditation or not. So that's something always hanging over us a little bit. For us, however, it is not a major problem, and there are people on the faculty who feel, quite correctly so, that if it becomes too onerous, we should just forget it.

ERWIN: That's an interesting point, though, because it points up the difference between Caltech and other schools.

SABERSKY: We have a little more freedom there. In a way, however, we feel a responsibility to the other schools to speak out on the subject and hopefully to have some influence on the accreditation process.

I should say a word about the student quality. It's a little hard to assess that, but I have had the opportunity to watch students for forty or fifty years now. I think the quality of students is really getting better every year; they're extremely capable. And it's kind of interesting: Even though there are a lot of problems in education in the country, the top people are still outstanding, and Caltech has been fortunate to get the top. I think that each year the students are a little better. The overall trend, I'm sure, is up. The students are really outstanding. The students who graduate with a BS—which, after all, is four years of study, which these days is not considered very much—are very capable professionals. As I mentioned before, I've seen them in companies and they do very, very well. They are able to learn, and learn on their own, what they need to know beyond the material they studied at Caltech.

ERWIN: Has there been any appreciable difference with the admission of women?

SABERSKY: That's interesting. There were, of course, lots of discussions before on what the admission of women would do and would not do to the Caltech campus. In fact, there was really very little effect that I could see—certainly from the point of view of the faculty. I think what happens is that the women who are admitted are just like the men: They are very hard-working, very intelligent, highly motivated toward technical studies of one kind or another. They are no different in character or behavior from the men. So it certainly hasn't made any difference as far as the academic work is concerned. At times, we had undergraduates over to our house, and the women weren't any different from the men.

ERWIN: And you've had your share in engineering?

SABERSKY: Yes. Well, of course, there were never very many anywhere. In general, I could never see any problem brought about by the admission of women. They seem to get along with the men in the class without any problem, and women and men work together, just like any other group of students. In short, the admission of women had much less effect on the character of the campus than one might have expected. That was interesting and surprising.

You asked me earlier about the effect of presidents and provosts on the department. Of course, on the working level you don't feel the effect very directly. There could, of course, be a change in atmosphere if the president is very different from the previous one. But so far, the divisions have really had enough autonomy—in fact, the individual faculty members have had enough autonomy—so that the effects are not felt very directly. Where it does count is in the ease with which you can get academic appointments. The number of appointments that a division gets is somehow allotted, and the president and the provost have a lot of influence on that. Now, that is a very important effect. In that connection, I feel that the process of appointing new faculty members has become awfully cumbersome. In Lindvall's time, he would listen at length to the discussions of the faculty concerning a proposed appointment and then, in his inimitable way, he would say, “Yes, I think I get the drift.” Then he would do what he thought ought to be done, and that was it.

ERWIN: And then you would be presented with the new faculty member?

SABERSKY: Yes, he would appoint one. So, summarizing, the process would be something like this: A group would make a proposal for a new appointment, and he would look at it. Then he would present it to the faculty of the division. He would then listen to the long discussion. After that, he would make up his own mind and handle it accordingly.

Now the process has become very formal—not only at our school, of course; it’s nationwide—and it involves outside letters and comments and long discussions. Then the proposed appointment has to be defended to the satisfaction of the other division chairmen and to the Institute Administrative Council. It becomes very difficult. And now, even before you can start the appointment process, you have to be permitted to form a search committee. It has become very cumbersome.

ERWIN: Is that as a result of tenure?

SABERSKY: No, I don’t think so. I don’t know what it’s a result of. It started here quite some time ago, but I’m not sure that it didn’t start at every other school about the same time. I don’t know if it had anything to do with the fairness rules and the ability to prove that there wasn’t any favoritism. I don’t know if it had to do with that, or if schools themselves thought that it ought to be done more formally. Of course the schools want to be sure they get absolutely the best and don’t make a mistake. But if you don’t make any mistakes, often you don’t do anything. This is purely my personal view, but I think it’s much too cumbersome.

The other thing I worry about for the development of the division is this tenure business. The tenure rules, the way I understand it, were formulated to protect young faculty members. But I think the way it turns out, it just puts more and more pressure on them. There is this rule that you have to decide after six years, or some such time, whether to allow the new faculty member to stay or not, and if not, you have to fire that faculty member. During that time, then, a young faculty member has to make his or her name in teaching and in research and in getting support and has to become internationally known. All this probably at a time when many of them are getting married, are forming a family and having children. I think that's just a tremendous pressure on a person. Then, if tenure is not granted, the rest of the world concludes that the person was fired, which will make it so much the harder for the person to get another job. I think the young faculty members would almost be better off without tenure. Then, you know, if somebody decides to leave—and that can be, more or less, by mutual agreement—they can just say, "I didn't like it there, and I'll try to go somewhere else." Presently, the pressure on the young faculty members doesn't make for very nice working conditions.

ERWIN: I know that in some areas it makes for a lot of very rushed and somewhat flimsy contributions to the literature.

SABERSKY: Sometimes, but that doesn't often occur, because here those contributions are looked at very carefully. It's not just the number of publications. It's interesting, in the case of new appointments at Caltech, that not only the professors in the area and the division chairman but even the provost will frequently read the papers of the candidate in detail and discuss them with the others. In short, I think these hurdles you have to overcome to appoint a new faculty member present a bit of a problem. People here are very much aware of it, but I don't know if it's going to change.

ERWIN: We haven't talked very much about your own research work.

SABERSKY: Yes, I want to summarize that now. I was always interested in heat transfer, even when I was an undergraduate here, but my interest was certainly strengthened when I worked at Aerojet, where one of my jobs was to cool rocket thrust chambers. The combustion takes place in these chambers. The pressure is generally quite high, and the heat transfer rates are extremely high, way beyond anything you usually encounter in any other application. Just to give you an example, in current rocket designs the heat-transfer rates in the nozzle, like in the thrust chamber for the space shuttle, are about twice that at the surface of the sun. I was very much interested in these cooling problems. After Aerojet, in 1946, I came back to Caltech as a graduate student.

When you are a graduate student, it's difficult enough to find a good advisor, and once you do that, you work on a project that's of interest to him. For this reason I worked on axial flow compressors, as I mentioned before, until I had my degree. But then, when I was appointed to the faculty, I did go back to heat transfer. I did work on boiling heat transfer first. To explain what that is, think of a surface cooled by water, which is flowing over the surface. The problem becomes particularly interesting when the temperature at the surface becomes so high that the water starts to boil. Then you have little bubbles forming, and the question then is, Is that good or is that bad for cooling the surface? How much does the heat transfer improve, and are there limits to it? And so on. This is the subject I worked on first. Now, research here—probably everywhere, but certainly at Caltech—involves very much the graduate students; in fact, they usually do most of the work, and that was certainly the case for me. The first student I had, and who worked on boiling heat transfer, was Ed [Max Edmund] Ellion. After working several places, he went to Hughes [Aircraft Company] and became director of research or development of a particular area. He recently retired from Hughes, but I think he formed his own company. He did some very fine work on boiling heat transfer. Most of his experimental work was carried out at JPL at the time.

Begin Tape 2, Side 2

SABERSKY: I continued the work on boiling but also worked on liquids near the critical point. The critical point occurs when pressure on the liquid becomes so high that there no longer exists any difference between liquid and vapor. So you don't have the two phases anymore, like liquid and vapor, but there's just a continuous change from a dense to a less dense fluid as the temperature is raised. These changes, of course, have an influence on the heat-transfer characteristics. We did work in this area, and the two people who come to mind there are Karl Knapp and Ed Hauptmann. Ed Hauptmann remained a very good friend of mine, and just recently we came out with a book by Acosta, Hauptmann, and myself. [*Fluid Flow: A First Course in Fluid Mechanics*, by Rolf H. Sabersky, A. J. Acosta, and Edward G. Hauptmann (Englewood Cliffs, N.J.: Prentice-Hall, 3rd ed., 1989)]

Certain problems of free convection were directly related to the heat transfer, to fluids near the critical point, and for this reason we investigated certain aspects of free convection in

Benar cells. The person who worked on that problem was Dick [Richard C.] Nielsen. He did a very careful thesis, which involved both experimental and analytical work. After graduation, he went to work for J T Thorpe, a company here in the Los Angeles area that is a manufacturer of big industrial furnaces. He's president of that company now. He very quickly got used to doing all of the work necessary to run a company, from union negotiations to counting bricks to analytical work, quite independent of what he did at Caltech for his research, a good illustration of the flexibility of Caltech graduates.

In parallel, I had started another line of research, and that was heat transfer to fluids that flow in rough tubes. Flow over rough surfaces is an important subject, both for predicting friction in tubes as well as heat transfer. Very little was known at that time about the behavior of fluids in rough tubes and their effect on heat transfer. So we started that as a project, and the man who did most of the work on that, and did very excellent work, was Duane Dipprey, who is currently the associate director of JPL. One of the real difficulties was to build a tube with a controlled roughness, and he did some beautiful engineering work on that.

ERWIN: What would be an application of the rough tubes?

SABERSKY: Many tubes are rough to start with, whether you want them to be or not. Then, when using them, you have to know to what degree roughness improves the heat transfer and increases the friction. You could also intentionally roughen tubes, in order to increase the heat transfer rate, which would allow you to transfer a given amount of heat in a shorter tube. In such a case, you would also have to overcome a larger pressure drop. So there are always trade-offs, but it's for applications like this that you have to know what the effect of roughness is. That was recognized long ago for the friction. There are classical papers on the friction drop in tubes, written a long time ago, and the results are quoted in all the textbooks. But there was very little—almost nothing—on the effect on heat transfer. Dipprey provided the information on heat transfer, and he also proposed an analytical model that turned out to be very good and is generally accepted now. He really did a beautiful job. He and I are still in close contact.

I continued work on heat transfer in tubes, which is one of the basic heat transfer problems. This time an interest developed in the flow of dilute polymer solutions in tubes. It had been shown that a little bit of a polymer—which is a long chain molecule manufactured by the plastic industry—when added to water, would change both the friction and heat transfer characteristics. And here, again, a lot of people had done work on friction but not on heat transfer. Nevertheless, engineers also need the information on heat transfer. That was a niche we could fill very nicely. It was very thoughtful of other people not to work on the heat transfer and leave that for us!

ERWIN: It seems that you were pretty consistent; your research spanned a long period of time.

SABERSKY: Yes. So we investigated various polymer solutions and the results were interesting. The person who worked on that was Paul Debrule. He is now a vice president at SAIC—Science Applications International Corporation.

We later on continued that work and studied polymer solutions of a different kind and at different concentrations. It so happened that there was an application that came along which could possibly benefit from our work. People were trying to develop a non-misting kerosene for use in aircraft. A fuel was to be developed that, in case of a crash, would form not a mist but bigger droplets, which are less likely to lead to an explosion. You may remember that just a few years ago, they actually took a plane and intentionally crashed it close to here, in Palmdale. This plane was using such a non-misting fuel, and the test was to demonstrate its effectiveness. JPL was involved in that, too. In connection with this program, we conducted a series of heat transfer and friction tests on this fuel.

At this time, I had a general interest in investigating a number of fluids that were not "well behaved," like water or air; in the technical jargon, those fluids are called non-Newtonian fluids. Aside from the polymer solutions, I also was interested in naturally occurring fluids that are handled commercially and behave differently from Newtonian fluids. One of these was tomato juice. We worked on that, and the experiments were actually performed at JPL. We got a hold of a lot of tomato juice, which was donated by Carnation. At my insistence, we measured the friction and heat transfer characteristics of this fluid. I must admit, however, that the student who carried out the experiments, Eric Matthys, was very embarrassed about the whole thing! He did all the work in one day, I think, to get it over with. [Laughter]

ERWIN: Well, can it be told simply what the non-Newtonian characteristics of tomato juice are?

SABERSKY: The tomato fibers influence the heat transfer, and for the same flow rates, you get a different heat transfer rate for tomato juice than for pure water. You've got to have this information in order to design a good heat exchanger. Now, tomato juice, that wasn't just picked out of the air facetiously. Tomatoes are a big crop in California. Tomato sales are about $400 million each year. The tomatoes are used mainly for juice and ketchup and paste. All of the crop comes in over a short period during the year, and it has to be quickly processed to avoid spoilage. This involves heating the substance and a lot of heat exchange. We wanted to start looking at some of the problems in that industry, being quite aware that we are by no means ready to solve any of their problems. But we wanted to get at least a little bit of an idea on how a fluid of this sort behaves. In this connection, we had previously visited Hunt Foods and Carnation to find out what they do, in order to get some feel for their operation. They have large pieces of equipment to process this tomato product, and it is really interesting to see it work. If you now think of Campbell's Soups, in addition to Hunt and Carnation, you realize that there are a lot of tomatoes to be heated. So I thought we shouldn't be aloof, and [we should] get involved in this kind of industrial operation.

As I mentioned earlier, the person who was involved in this work was Eric Matthys, and he is a professor at U.C. Santa Barbara now and doing very well and working very hard.

In a parallel effort, we also studied the flow and heat transfer characteristics of flowing granular material. That subject fitted into the general category that although very relevant and important, had not been worked on by a thousand and one persons over a hundred years. I got exposed to the problem in an interesting way. I was making a visit to Procter & Gamble at the suggestion of our development office. The engineering group of Procter & Gamble was concerned with soap granules. Granules hot from the processing area were to be cooled before being packaged into bags or cartons. They were going to build a heat exchanger for this purpose in which the granules would flow over horizontally arranged, cooled pipes. It quickly became quite clear that neither they nor we knew anything about either the flow or the heat transfer to a flowing granular material. So I thought that would be a nice topic to work on. We came back to Caltech and set up a little apparatus that would allow us to watch granules flowing over a cylinder. Then we started to make more serious experiments and got a little bit of support from the National Science Foundation.

ERWIN: Did you get any from Procter & Gamble?

SABERSKY: They helped here and there a little bit. There was a Mr. [Robert L.] Kramer, a Caltech alumnus, who was very interested in Caltech and he gave us some money for our project out of his personal funds, which we really appreciated. But over the years we also got NSF [National Science Foundation] grants, the first of which was for $16,000. At the time, that carried us very nicely for a year or even more. We set up a small hopper and supply system and let granules flow over a flat plate and made heat transfer experiments. And this involves an interesting point. As I said before, many people do friction experiments, but relatively few perform the corresponding heat transfer experiments. Furthermore, the heat transfer aspect is not only important for industrial use, it also gives you quite a lot of insight into the mechanics of the flow, because the heat transfer is very sensitive to changes and densities and velocities and therefore tells you a lot of what's going on inside that fluid.

We started the work on granular materials in the early seventies or so, almost twenty years ago. The more we studied this problem, the more we realized we didn't understand much of what was going on. The first student who worked on granular materials was Bill [William Noel] Sullivan; he did a beautiful job. He is now at Sandia [National Laboratories] and has worked on all kinds of different problems. The Caltech program fortunately gives students a sufficiently broad basis that enables them to work in many engineering fields. Quite a number of students worked on the granular material. We were fortunate that, a few years after the program was started, Dr. [Christopher E.] Brennen [Richard L. and Dorothy M. Hayman Professor of Mechanical Engineering] also developed an interest in granular flow. We then worked jointly on this program and got joint grants. Some of the students he supervised and some of them I did, although, with most of them, we both were involved in their work. The program is still continuing now, under the direction of Dr. [Melany L.] Hunt [professor of mechanical engineering] and Dr. Brennen. It is now realized that the applications are more plentiful than once thought. For example, the geologists are interested in rock slides, and it turned out that the same kind of mechanism governs these slides. This seems to be true also for some of the snow avalanches. In that case, the snow is in the form of small crystals, and it's cold enough so that the liquid phase isn't important. In addition, granular material, such as coal and grain, is transported in huge amounts everywhere.

At times, we also got support from Union Carbide, whose engineers were interested in our project because they had to transport plastic beads; the plastic that they make—polyethylene, or whatever—comes out in small beads. The equipment for this transport—chutes and hoppers—was large and costly and had to be properly designed. So there's a fair amount of interest in the flow of granular material, and I was really pleased to see the other day that Dr. Hunt got a request for a proposal for work in this area, which is unheard of—not just an unsolicited proposal but a request for proposal from the Department of Energy.

ERWIN: Are you still working on that now?

SABERSKY: No, I'm not working on anything, but I'm here and interested in seeing what my younger colleagues do. I'm an observer now, and I follow their work.

ERWIN: You didn't mention the pollution work.

SABERSKY: Yes. Well, that was kind of an extracurricular activity.

ERWIN: Now, this was during the late sixties, early seventies, during Harold Brown's administration [Caltech president 1969-1977].

SABERSKY: Probably. I was just interested in smog, and the ozone component in particular; and I talked to Dr. [Fredrick H.] Shair about it. Then we got a hold of an ozone detector and certain other instruments, and Shair and I performed a set of experiments indoors. I guess that was one of the first times somebody measured anything indoors. This has become very popular since then, in connection not only with smog but also with the kinds of things that give off vapors in the house—plastic furniture and things like that. But at the time, we were mainly interested in smog and ozone and how that would behave indoors. Our results were quite interesting, because we found out—I'm not at all sure if we were the first ones, but we found out that if you have a house or a room or any closed space, the ozone disappears very quickly. Ozone is very reactive, and there's enough stuff around in a room that reacts with ozone, and consequently the ozone level decreases to one-tenth or so of the outside level in a very short time.

ERWIN: So on a smoggy day, you should close up your house.

SABERSKY: Exactly. And reduce the ventilation, unless you need it for other reasons. You don't have to worry about any lack of oxygen, because California residential houses are leaky enough. So, just close windows and doors up as well as you can, and you will have no ozone to speak of inside.

I might also mention that one summer I made an arrangement with a student, Gordon Petersen, to drive a car all over Los Angeles all along the freeways and measure the ozone concentration inside the car. It turns out that even in a car, the ozone level goes down to about one-third of what it is outside, and that's usually below the level at which it is really disagreeable. Again, the result showed that if you keep a space pretty well enclosed, the ozone goes away. So that is the story of our involvement in the pollution business. As I mentioned before, this was not really a research project but more of an extracurricular activity. Nevertheless, we did publish our results. [G. A. Petersen, R. H. Sabersky, "Measurements of Pollutants Inside an Automobile," *Jour. Air Pollution Control Assn.*, 25(10): 1028-1032, 1975]

ERWIN: In 1975 to '76, in the annual report of the engineering division, you turn up as an investigator in at least six projects.

SABERSKY: Maybe that's advertising! [Looking over list] Some of these overlap.

ERWIN: Did that make your life more complicated?

SABERSKY: No, I don't know that it was much different from any other time. I really never had an awful lot of students. You can get a pretty good indication of the work load by the number of PhD students, and I hardly ever had more than two PhD students at the same time. It is true, however, that in addition I had at times a number of students who worked for the so-called professional degree, the ME degree—mechanical engineering degree. They were often graduates from the navy, from the Monterey Post-Graduate School. I enjoyed working with them, and we conducted some interesting research. But I rarely had more than two PhD students at the same time.

ERWIN: Can you explain just a little bit about the engineering degree, what that is?

SABERSKY: As you can see from the records, it's hardly ever used. It's a degree that involves some research, and usually takes an additional year after the master's degree. I don't know why it was originally introduced; maybe it was thought that there was a demand for it. When I was involved with it, it was used mainly for students who came from the navy or the air force who were assigned to an additional one or two years of graduate work. There was an arrangement that allowed them to take it at Caltech. That professional degree then was more than a master's degree and included some research. Sometimes we also had students from other countries who had a limited amount of time they could spend here. I remember in particular one from South Africa and one from Kenya; they were able to spend an additional year after the master's degree, and in that time they could get the professional degree, which gave some formal recognition to the end of their studies. The student from Kenya, by the way, is now what we would call president of one of their universities. Then, just recently, just last year, a man from the U.S. Army was here for two years. We were able to complete a little research project, which led to a nice publication, and he got an ME degree.

ERWIN: Does it correspond to any European degree?

SABERSKY: I don't know that it directly corresponds to a particular degree. The professional degree shows that the recipient did some research. It is really used only in those special situations.

ERWIN: I want to talk just a little bit about your 1968 article in *Engineering and Science*, "Who Will Take the Lead in Engineering Education?" [vol. xxxi, no. 7, April 1968]. It seemed to me you made some very interesting statements there, about what the definition of an engineer was. Do you recall that?

SABERSKY: Well, this had to do with the push towards physics and the overemphasis on pure physics and mathematics. Our admissions policy would weigh performance in math and physics very heavily, perhaps too heavily. I was concerned that we would have too few people studying engineering because of admissions policies that were too restrictive. Now, years later, I think the problem has more or less solved itself.

ERWIN: There was a shortage, though, of engineers.

SABERSKY: Well, let me say a word about shortage. I made statements about that, and I think they still apply. But there are two sides to shortage. I think usually when people talk about shortage it is because they feel there ought to be more engineers employed in various industries, particularly in our civilian industry.

But there's another angle, which became clearer to me later on. Having the engineers available doesn't necessarily mean that they get employed by the industry that needs them. And I think really what is required more than just educating more engineers is to see to it that the engineers permeate the industry, as well as the appropriate government organizations, more heavily. In other words, the demand for more engineers has to be there. There are a lot of places where we think there ought to be engineers, but just having them wouldn't be enough; we have to have the desire from the industry and government organizations to employ them. Just producing more engineers without having that desire will just depress the engineering profession more, status-wise and financially.

ERWIN: How do you think the profession is right now?

SABERSKY: Well, I don't know. We may well have too many engineers graduating for the jobs that I see.

ERWIN: As a whole, has engineering lost its identity?

SABERSKY: No, it hasn't lost it, in the sense that it wasn't any better before. Engineers don't have a very strong identity or organization, unfortunately.

ERWIN: Is that because they do so many things?

SABERSKY: Perhaps. Also the concept "engineer" includes an awful lot of people with BS degrees, MS degrees, and PhD degrees. It's not as select a group, I guess, as a group of medical doctors would be. I don't know exactly what the problems are, but engineers don't have a lot of clout. They are also a strange kind of group. They don't support one another as much as other groups. When the physicists want a project, they all endorse it and they're all for it, and their efforts are pretty well coordinated when they make their presentations to the government. If an engineering group wants a project, well, ten percent will be for the project and ninety percent will tell you why that project shouldn't be done. [Laughter] And you can see it also in proposals to the NSF, the way the peer groups behave. In astronomy, you will find that most of the proposals are rated "excellent" by the peer reviewers. In engineering, most of the proposals are labeled "good," or "fair" at best. Engineers are very critical of each other and very vocal with their criticism.

ERWIN: There must be something inherent here.

SABERSKY: I don't know if there is anything deeper. Of course, engineers are brought up to be very frank and open and tell things exactly the way they see them. It may be that, and it may be, as you said before, that it's a diverse group with different backgrounds. It's funny—or unfortunate, rather—but it is true that engineers, as a group, just don't have much clout.

At the end, I might mention a few people who had strong influences on me over the years and during the Aerojet time. I mentioned these people before, but let me summarize. First of all there were Dr. Summerfield and Dr. [Bernhardt L.] Dorman, whom I am still very much in contact with. Next let me mention Mr. Zisch. I didn't ever work closely with Mr. Zisch, but by observing and seeing him in meetings and similar situations, I learned a lot from the way he handled problems. Furthermore, here at Caltech, during my student years and early years on the faculty, Drs. Hudson and Rannie and Kyropoulos were people whose work and conduct I admired, and in that way they influenced me. In the later years, I worked closely with Dr. [Allan J.] Acosta [Richard L. and Dorothy M. Hayman Professor of Mechanical Engineering, emeritus], whom I first met when he was in the V-12 navy program. That must have been 1942 or '43, and we have been close friends, essentially, from that time on. The friendship with Dr. Hudson fortunately has also continued, and although we worked in different fields the contact continues.