

CHEMICAL HERITAGE FOUNDATION

CHARLES W. TOBIAS

Transcript of an Interview
Conducted by

James J. Bohning

in

Orinda, California

on

15 and 16 May 1995

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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This project is made possible through the generosity of The Electrochemical Society.

Charles W. Tobias

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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CHARLES W. TOBIAS

1920 Born in Budapest, Hungary, on 2 November
1996 Died in Orinda, California, on 3 March

Education

University of Technical Sciences, Budapest, Hungary
1942 Diploma of Chemical Engineering
1946 Ph.D., chemical engineering
1947-1948 Postdoctoral studies, University of California, Berkeley

Professional Experience

1942-1947 Development Engineer, United Incandescent Lamp and Electrical Company,
Ltd., Budapest, Hungary
1945-1946 Instructor of Physical Chemistry, University of Technical Sciences,
Budapest, Hungary
University of California, Berkeley
1947-1960 Instructor of Chemical Engineering
1960-1991 Professor of Chemical Engineering
1967-1972 Chair, Chemical Engineering Department
1991-1996 Professor Emeritus
Lawrence Berkeley Laboratory, University of California, Berkeley
1954-1978 Principal Investigator
1978-1991 Faculty Senior Scientist
1991-1995 Senior Faculty Scientist/Chemical Engineer

Honors

1965 Fellow, American Association for the Advancement of Science
1972 Acheson Award, Electrochemical Society
1977 Honorary Member, Electrochemical Society
1982 Henry B. Linford Award, Electrochemical Society
1983 Alpha Chi Sigma Award, American Institute of Chemical Engineers
1990 Vittorio De Nora Diamond-Shamrock Award, Electrochemical Society
1991 Berkeley Citation for Distinguished Achievement, University of California,
Berkeley
1991 Honorary Member, Hungarian Chemical Society
1991 Founders Award, American Institute of Chemical Engineers
1992 Golden Diploma, Technical University of Budapest
1992 Honorary Doctors Degree, Technical University of Budapest
1993 Honorary Member, Hungarian Academy of Sciences

ABSTRACT

Charles Tobias begins this interview with a description of his extended family in Hungary and their interest in engineering. He remembers his early childhood and education in Hungary and the influence of his family and high school chemistry teacher in his selection of chemical engineering as a career. Next he discusses his education at the University of Technical Sciences in Budapest. Throughout this section he points out the strengths and weaknesses of his education and compares the U.S. and Hungarian systems. Tobias continues by recalling his initial desire to join his brother in graduate research in the U.S. and the intermediary time spent in wartime Hungary as a chemical engineer and later as a researcher. Next he describes the legal and logistical problems he faced in leaving post-war Hungary to join his brother at Berkeley. In remembering his initial visits to Berkeley, he fondly remembers the help of John Lawrence, W.M. Latimer and others. He discusses his early research interests and contact with students as a teacher and research advisor. He finishes the first day of interviewing with an overview of the changes within his department during the 1960s.

On the second day of interviewing, Tobias starts by telling of his initial attraction to The ECS through student readings of the society's journal. He recalls his interest in reviving the local Berkeley section and meeting colleagues who would play a role throughout his career. As he describes his leadership in reorganizing the tone and structure of The ECS and the Theoretical Division, he emphasizes the roles played by others who joined with him. Moving on to his presidential activities, he touches on several changes within the society and the emphasis he placed on both professional conduct and attracting and supporting young society members. He also discusses the development of electrochemical engineering as a field, and the roles played by him, his students, and the society within that development. He finishes the interview with a brief comment on the role of intuition in science.

INTERVIEWER

James J. Bohning is Professor of Chemistry Emeritus at Wilkes University, where he was a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. He was chair of the American Chemical Society's Division of the History of Chemistry in 1986, received the Division's outstanding paper award in 1989, and presented more than twenty-five papers before the Division at national meetings of the Society. He has been on the advisory committee of the Society's National Historic Chemical Landmarks committee since its inception in 1992. He developed the oral history program of the Chemical Heritage Foundation beginning in 1985, and was the Foundation's Director of Oral History from 1990 to 1995. He currently writes for the American Chemical Society News Service.

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INTERVIEWEE: Charles W. Tobias

INTERVIEWER: James J. Bohning

LOCATION: Orinda, California

DATE: 15 May 1995

BOHNING: Dr. Tobias, I know that you were born on the second of November in 1920 in Budapest. Could you tell me something about your father and mother and your family background?

TOBIAS: Well, I was born into what might be called an engineering family. My family on my father's side originated from a unique little town in Transylvania called Torocko. The town was entirely a one-trade town. They mined iron ore from the hills around. They smelted the iron and also fashioned the ultimate article and sold it all over Transylvania, a unique economy cottage industry, if I might say. Anyway, in the middle of last century, with the advent of large-scale iron smelting, suddenly the economic basis of this industry collapsed. That is the time when my grandfather moved on to a Southern Hungarian town, maybe one hundred or a hundred and fifty miles away, called Szeged. He opened a smith shop where he fashioned iron articles for buildings. Since there was a major flood there in 1877, there was great need for this, and he was able to maintain a shop in which there were as many as twelve people employed—but that didn't mean wealth at all.

He was a very modest man. There were eight children, actually seven sons, and five of them eventually became engineers. Another taught Descriptive Geometry in the high school, and still another became a master mechanic. On my mother's side, I come from a physician's family. My grandfather was a district physician in southern Hungary in Obecse. This was part of Hungary until the Versaille treaty. The current name is Stari Becej. I don't know the exact spelling.

After high school, my father enrolled in the only engineering university in Hungary, in Budapest, with no help from the family. They didn't really have money to send children to college, but the brothers were outstanding students and won numerous prizes and scholarships. When the next brother came up to Budapest, he moved in with my father. The Tobias family, so they say, mowed down the prizes at the end of the freshman years—only for this to repeat in my generation, and although I didn't get the prize given to the best freshman, my brother did.

So I come from a family in which science and engineering are both regarded as a most noble, most worthwhile way in which to spend your life. From an early age on, I could not

have been more than four or five years old—I knew of course I shall be an engineer. There was none of this floundering of today's youth who at age thirty wonder what they ought to do. I knew it by age four. [laughter] My father also inoculated us with a healthy dose of prejudice against the legal profession, politics—as a matter of fact, maybe too much so. Politics was regarded as a dishonest profession and not worthy of a Tobias. [laughter]

We had a very comfortable existence by Hungarian standards. We were up around the top of the middle class. We were not aristocrats; we didn't have the title of landed gentry, but my father was the technical head of the transportation system in Budapest, which was the third largest enterprise in all of Hungary. He had a very high salary, multiples of that of the prime minister, and we had a very nice comfortable home with values that I cherish and try to carry on here in our home. If you have a little time, you can look around and see what my home environment was like in Hungary.

BOHNING: Okay. What was it like growing up as a child there?

TOBIAS: Well, we had an enormous feeling of security because of this knowledge of what we would be doing. My father had a very secure position, strong ethical and moral values. School was very serious. I must say that having brought up three children of my own, and two later with my second wife, the primary and secondary schooling in America makes me cringe. I think it is really terrible. Something really should be done about it.

Anyway, school was very serious and segregated according to sexes, which I thought was, in retrospect, a very good thing, because there was none of this showing off and none of those disturbing influences of the presence of the opposite sex, which after all is normal for a young person. [short break]

I started musical studies at the same time that I started grammar school. The city of Budapest had an elaborate system of introduction to music, which was very inexpensive and involved highly competent teachers. That was done in the afternoon. School, even throughout high school, was only to twelve noon or 1 p.m. I never went to school in the afternoon, except for musical studies. I kept on with my musical studies until I graduated from high school. In fact, I obtained an academic certification, the highest one could get as an instrument player, a violinist. Music was a great enjoyment in my life. For a while I thought of becoming a professional, but I wouldn't have wanted to play in an orchestra, and to make a concertizing career, the chances looked extremely bad.

Anyway, grammar school went by really fast, four years, and then came high school, eight years of high school at that time in Hungary. I have to remind you that only roughly ten percent of each age group went to high school, what you might call academic high school. A large majority of rural youngsters went for two more years in grammar school, and in urban areas we could go four more years, for a total of eight, into what was an intermediate

school, and either stop then or continue in trade schools, which were really quite excellent. I wish we would have them here. We could specialize in chemistry, indeed, and in physics and electricity, and also in very useful areas, building trades. They carried on the lower functions of engineers.

Anyway, the academic high school I went to, Eotvos Real, was a rather unusual one. It was called a Real school, corresponding to the English word real or realistic school if you considered direct translation. It implied that the school was oriented to educate young people who really chose to be scientists or engineers. Instead of Latin or Greek, we had two modern languages for eight years, German and French. English I might say was hardly taught at all in Hungary at that time because Britain was so far away and Britain was not really Europe, you know. French was a civilized language and I loved it. It was a gorgeous language.

Anyway, for eight years we had quite a bit of science, descriptive geometry, and of course mathematics, which was taught extremely well. I hate to tell you, but that's what I made my living on in my life, the high school math I learned. Our teachers included people who were lecturers in the University who taught in high schools also, because the money was good, so they had income from two sources. This was very good for the high school students, because they had very highly scientifically competent people. There were no problems with discipline; if you didn't behave, you were thrown out. I am grateful for the primary and secondary education I received in Hungary.

I will sneak in a derogatory comment here. Very unfortunately, a large fraction in America of high-school science teachers are there by default. Quite a few are people who couldn't make it otherwise. That especially includes biology majors, because you know it's difficult to make a career with a B.S. degree, be in an analytical laboratory and survive with a position as a B.S., but you can teach high school. Biology, being the most complicated area of knowledge, would require ultra Ph.D.s, [laughter] but you know what happens. This is what happened to my children in high school. I was watching that. Teachers were not duly prepared to teach the subjects, and that of course included math. I carried with me gratitude toward the primary and secondary schooling in Hungary.

BOHNING: Were there any athletics?

TOBIAS: Well, yes. The schools in the city, of course, didn't have grounds around. They were wretched—a school was just like an apartment house. We did have gym, three times a week. We had athletic competitions we participated in. Once in a while I went to swimming competitions, and in my last year in high school I was member of the crew. That was on the Danube, and of course it was not during school hours. But no schools had athletic fields. Zero. No such thing.

We had, however, extracurricular activities available, a large variety of them, including a really quite good school orchestra with a superb conductor, again, in a second job, a professor at the state conservatory, Eugene Adam. He was a very well known man who took this job because it was a second income for him. It was a fantastic thing to have a high school orchestra conductor who could have been conducting any of the major orchestras in the world. I was very pleased.

I would say of the high schools, as much as I knew about them in Hungary, generally the level was very good. You may hear in America about the Hungarian physicists and Nobel Prize winners who came from two specific schools in Budapest, but there are many other schools in Budapest and elsewhere that are also very good. [short break]

Very good. I would like to mention, with regard to my high school education, why I became a chemical engineer, since all my relatives, cousins, and of course my uncles, were mechanical engineers or civil engineers or architects. I was to be the first one to be a chemical engineer, largely because my high school chemistry professor had a decisive influence on me. He was a fantastically inspiring teacher. He could dramatize chemistry. I still remember some of his experiments. He could keep our attention and then hold onto it.

We had an optional chemistry laboratory in the afternoon which I took, which was a dedicated laboratory in the school. We also had a dedicated physics laboratory. My physics teacher was outstanding also, and we put together a primitive television circuit in 1937.

BOHNING: Hmm.

TOBIAS: It was very primitive. A rotating perforated disk served as a mechanical barrier to the transmission of light. Light signals were detected through photocells that had different spacial definition. In fact, I wrote my baccalaureate exam, designed to test my writing skills, on television. I was told later that I was the first man who would dare to do this, because we were supposed to choose always the literature option. I didn't know this. [laughter]

Anyway, the math, physics, and chemistry were very inspiring.

BOHNING: What was the name of your chemistry teacher?

TOBIAS: Vilmos Kraus. K-R-A-U-S. It also happened that he played violin bass in the orchestra. I already played in the orchestra at age 11 or 10, and the next youngest fellow was 15, but I was a very good fiddler already at age 10, so he paid attention to me, and this relationship grew then. When I took chemistry at age 14, he had a decisive influence on me.

BOHNING: What kind of experiments did you do in his class?

TOBIAS: Oh, I remember the clock reaction, still today. But he didn't give us just fun things. It was a reasonably physical chemistry oriented presentation. He was able to hold on to our attention and he had a sense of humor, which is very important; it's difficult, but it's important.

So, at age seventeen and a half, I went through the baccalaureate exam. Fortunately, I had one B, and the rest were As, which was summa cum laude, they called it in Latin. That was essential to be considered for admission to the chemical engineering course in the University of Technical Sciences, because there were unfortunately three hundred applicants there, and about eighty were admitted. Those eighty were the best high-school students from all over the country. There was only one institution, [laughs] so I was very lucky that I got in. A chemistry professor whose mentality and science were strictly 19th century inspired us on the first occasion, a general chemistry lecture, when he said to us, "Gentlemen, look around. Look at each other carefully. I call to your attention that in the second-year lab there are only sixty spaces. Be prepared." He said, "I also tell you that in the third-year lab there are only forty spaces, so you have to earn my approval." [laughter] Actually, if I would try to characterize my experience at the chemical engineering course in Budapest, the major thing that I would say ultimately was learning about the art of survival. The expectations and the demands on us were totally unreasonable. There was too much rules learning. In general and organic chemistry we were supposed to memorize endless, and I mean endless, lists of stoichiometric equations, and during exams, which were oral, if any hesitation was detected on your part, that is to say when you tried to figure out stoichiometric coefficients, that was regarded as a lack of knowledge, and they flunked you.

I carried a bitterness with me. I didn't get very good grades in these introductory courses. The first time I had a really motivating course was in my second year in physical chemistry where suddenly I started to see that chemistry is a science; there is science behind chemistry. The teacher, the professor, was Istvan Naray-Szabo, the man with whom I later did my doctoral work. Naray-Szabo did several years of post-doc work with [W. H.] Bragg in Manchester, and also I think a year or two with Herzog in Berlin. He spoke several languages and of course was an excellent crystallographer, but he was a chemist first and a crystallographer second. He used crystallography as a tool to investigate chemistry, not as a goal in itself, which I think made him a very good chemist. In fact he later on wrote a whole series of books with the loosely translated title of Crystal Chemistry (1) published in German by Springer Verlag. [short break]

Well, I recall we had an outstanding professor/teacher in organic chemistry, Geza Zemplen, a sugar chemist who was internationally well known. He was a brilliant lecturer who drank all the time, but he was giving better lectures drunk than others sober. He always told us that he wouldn't teach us anything except what he had done personally in the

laboratory. Well, I believed him. The curriculum unfortunately was, as I mentioned, based on 19th century work. We had to read many descriptive subjects in chemical technology, which at that time I didn't appreciate because it was too much description and very little reasoning. It turned out that later on that helped me a great deal, because in the United States it was the inverse thing. They learn much reasoning, but they learn very little of chemical technology, so I was weird when I started out in this area in Berkeley.

We spent untold hours in analytical labs. We analyzed, every weekday, four years, every afternoon; fall semester, spring semester, four hours in a lab we analyzed. The exception was the phys chem lab, where we did of course 19th-century experiments demanding extensive use of analytical balances. We learned the theory of analytical balance, which I never used in my life, but this endless analytical laboratory was a real turn off. Of course in Hungary, don't forget, you might say in all of Europe, most of the major need for chemists in the 19th century was to do analysis, even for chemical engineers. Well, I survived analyzing clay and stainless steel with a large number of components in them—vanadium and tungsten and silicon and whatever—with very simple tools, gravimetry and titrations and alike, with everything by 19th-century methods. I did survive. We had oral exams where your style of presentation and sangfroid was very important. Some people whose knowledge was not very good but who could pretend knowledge well got very good grades.

[END OF TAPE, SIDE ONE]

TOBIAS: In my years in college I must say one of the tragically bad omissions was that we only were given half a year of college math, five lectures a week plus one recitation section, but that of course was not enough. We couldn't take elective math because there was not time for it. In fact, there were very few elective opportunities. I used them. In my upper years, I took polymer chemistry. I took hormones and vitamins and I forget, something else, but that was not enough. I must say the mathematical background that we received in college was very poor.

At the end of second, third and fourth years we had comprehensive exams—in Latin, *rigorozum*—oral, in front of the committee. You were examined not about the subject that you had just heard about, but subjects a year before. This was a little bit of a sham, because of course being an oral exam in front of the committee, and this being such an unreasonable way of testing knowledge, the questions were helpfully stated. Although I must say in organic chemistry I got a question I couldn't open my mouth about. It was not something the professor had talked about. Incidentally, textbooks were generally not available, and we were not encouraged to get German or French or English textbooks. We were expected to take very good lecture notes and regurgitate, so if we got a question that was not in the lecture, that was not a nice question. I remember the question that I didn't know the answer to. The professor wanted to know the structure of *cardiazol*. That happens to be also a drug

for certain heart condition, but that I remember that is interesting. He gave me an A minus, so my answer was not so bad. Well, I passed his comprehensive exams and I got eventually a diploma with a grade of five. Six was the best. I thought it was fair; I didn't deserve a six. I don't think I ever could have gotten a six because it would have required a soul that is geared to repetition and not contemplation.

BOHNING: Was this in chemistry or chemical engineering?

TOBIAS: Chemical engineering. But don't forget you couldn't major in chemistry at this university.

BOHNING: Okay, because it doesn't sound like you had much engineering experience, or did you?

TOBIAS: Well, we had, as I mentioned, technology. We had descriptive courses of technology. Not design courses.

BOHNING: Okay.

TOBIAS: Here, in the U.S., you need to learn about unit-operations. We give massive doses of thermodynamics. In Hungary, we only had half a year of physical chem. Our students at Berkeley here get three semesters of physical chem, and two more thermo courses. Our students here get a separate course in reactor design and kinetics. None of this, but we had massive descriptive courses in general chemical technology, and food and agricultural chemistry. So, I could give long lectures on how things were made, but I couldn't have designed a plant, because we couldn't ask what did it mean to design a plant. We had to be resourceful and we had to know our way. We learned how to do that, because to survive these kinds of analytic labs, how to contrive performing analyses—you know, we were graded on how accurate we were.

I still marvel at how I was able to pass these courses. As I said, I think I learned how to survive. Well, at the end of these four years it was 1942. We were in the war Hungary didn't yet declare. It was not one of the official warring parties, but of course we were under German influence and dark clouds were with us. Of course, my whole family abhorred the Germans and what went on, and we had the darkest foreboding about what was going to happen.

I knew one thing at that time, and that was that I wanted to study more. I thought, "After the war, I will want to go to the United States." There I already had a brother who in 1939 earned a state exchange scholarship. He was lucky enough, because a month or two after he left Hungary to come to America this program was kaput and the war started. My brother was ultra lucky. He was accepted as a graduate student. He was a fifth-year electrical engineering student, and he was accepted in pure physics as a graduate student of Ernest Orlando Lawrence.

BOHNING: Yes.

TOBIAS: Incidentally, just for your information, when the letter came about Lawrence and Berkeley, we were disappointed because we didn't know who Lawrence was and certainly not what Berkeley was. We knew about Stanford and Cal Tech. We knew about Cal Tech because Theodore von Kármán was Hungarian, and Stanford also had two Hungarians, Polya and Gero. Berkeley, we'd never heard of. My brother originally applied to go to Arthur Compton at Chicago, but he couldn't accept any more students. Compton called up Lawrence, and my brother got an invitation from the latter which was not received with the greatest joy. He was really disappointed. Well, I tell you, my brother was the luckiest man in the world, as it turned out later.

To get back to myself in 1942, I knew I didn't want to be in the traditional chemical industry. The danger was also very great that I would have to do military service, which was compulsory in Hungary. I could get a one-year deferral on the basis of working in an industry that was important to the war effort. In any case, the only company that suited me was United Incandescent Lamp and Electrical Company Ltd., which had the brand name Tungram. It was the most westernized and most modern industrial enterprise in Hungary, a member of the world cartel in radio tubes and incandescent lamps, a thoroughly modern company. Of course, it was not a chemical industry, but nevertheless I perceived that it would be an interesting place to be because already then I'd felt an affinity towards physical chemical things.

Well, after a little bit of help from some acquaintances who knew the right people, I got a job offer from there and spent one year there before I was called on by the military. I might mention here that without what we called then in Hungary "protekció," a friendly referral, you would never dare to look for a job, because they would have thought you were a totally idiotic person who doesn't know the rules of society. Some cousin of an uncle of yours had a friend in such and such industry, who called up his friend at this company, who put in a good word for you, and then they looked you over.

It was very different when I came to America, where this was unheard of. Here, you were supposed to present yourself and tell who you were. I mention this because this still exists in Europe. The connections and the referrals are very important. [short break]

BOHNING: Certainly. There's one question that goes back a little bit, and that is, how did your family react when you said you were going to be a chemical engineer?

TOBIAS: My father accepted it. He didn't object.

BOHNING: Okay.

TOBIAS: Maybe they regarded this as something different; he even would have accepted if I had become a violinist, but he did ask me, "How many happy and satisfied concert violinists do you know?" I said, "Oh well. Probably fifteen or twenty in the world." I probably over-estimated. Then he said, "What would be your guess on how many happy and satisfied chemical engineers exist in the world?" I said, "About ten thousand." He looked at me and said, "Well, just think about this. Think about probabilities." [laughter]

BOHNING: You had military service then?

TOBIAS: Well, yes, at the end of a year's work in the radio-tube division of Tungstam. In fact, they didn't quite know what a chemical engineer should do, but I was given little tasks that really physical chemists would have appreciated, and I learned a great deal. It was a thoroughly modern company in close connection with RCA, the giant American radio tube, et cetera manufacturer. Anyway, that was a good year. Then in 1943 I was called out and much later I discovered that with great luck I was put in the railway construction regiment of the army. But I was a chemical engineer, I knew nothing about it. Well, another option was the chemical warfare battalion, but it was located in an impossible place. It had a very bad reputation.

In contrast, my regiment was located in Szentendre on the shore of the Danube about fifteen miles north of Budapest. It was an ideal spot. A good percentage of our officers were aristocrats who had chosen to join this branch because of the proximity to Budapest, so I had a relatively easy time. Well, I could linger on this topic. I was saved from going to the front and also from being taken to Germany, because my entire company then was moved to Germany, but myself and maybe fifteen others were left behind in Budapest in July of 1944 on an official task. We had a bridge repair vessel that we designed and built earlier to repair bridges damaged in air raids. The water filled vessel was towed under and lifted the bridge by pumping the water out. Anyway, before we could finish such a repair job, we ended up being caught by the Russians surrounding Budapest. This was for us happiness because we didn't want to go to Germany. Of course, also, my family was in Budapest, and at that time

we didn't know yet what the blessings were of the Russian soldiers raping and looting, or what the communist regime would be like. We were just happy that the war was over.

Well, in January of that year, after I was successful in evading capture as a soldier, I changed into civilian clothes and could go home to Budapest. I had to provide for my family—father, who had severe emphysema, and mother, and a sister who had rheumatoid arthritis and was completely crippled, couldn't walk. I had to provide them with food. There were no stores, and fortunately, I was clever enough to scrounge around on the black market a little bit. I went back to the Tungstam company and I got some food there.

After a few months the Russians suddenly appeared and dismantled this huge factory on the basis that it was a German property. In fact, a majority ownership was American, GE [General Electric] and RCA, but the Germans of course had appropriated it, and the Russians said, "Oh, it is German property." The Russians dismantled everything, including the toilet seats. I have reason to believe that nothing was ever used again. This was a modern huge facility making lightbulbs and radio tubes. We had exported all over the map and to the troops.

Anyway, it was at that time, around June of 1945, that I returned to the University and went to see my former professor Naray-Szabo. I offered my services as an assistant. He was happy to see me. The university itself suffered bomb damage, and he was happy to have a pair of hands. I got an appointment as an assistant, and of course I expected to do doctoral work there, which I might say was not as necessary there as is a Ph.D. here. Under the circumstances then prevailing in Hungary, there for a while we didn't have even running water or gas or electricity. It was not easy.

So I taught the lab course in the fall of 1945. The sophomore physical chemistry lab was working. In the fall of 1945 and spring of 1946, I also engaged in lab research. Naray-Szabo wanted us to try to prepare pure boron and determine its crystal structure. It turns out that was an unsolved problem at that time. I was able to lay my hands on some boron tribromide and I prepared some boron by thermal dissociation. Needless to say, all this had to be prepared in suitable glass and quartz apparatus. To obtain these in a war ravaged place was just very difficult. My connection with Tungstam helped.

Anyway, I was able to deposit boron on platinum and tungsten filaments, and we got some powder diagrams. We couldn't grow crystals. It was a nice deposit. Since the two coats of tungsten and platinum were identical, we said that if it's identical it has to be the pure material. Well, this is rather weak reasoning, but nevertheless, as soon as I got to the United States a year later, I published in the JACS my first publication (2). It was a modest piece of work.

BOHNING: That was 1949 when that was published?

TOBIAS: Yes.

BOHNING: You had one paper earlier, which we'll get to, from Berkeley in 1948 (3).

TOBIAS: Yes.

BOHNING: I wasn't sure when I saw that if that was work out of your electric light Tungstram company.

TOBIAS: Well, it was to the degree that I conducted experiments at Tungstram as well, all through my assistantship. As soon as the Russians dismantled Tungstram—and they left an empty shell—that company was built back, because workers were hiding equipment and materials here and there, and because Tungstram had foreign holdings and currencies. They were able to build back some of the operations, including the laboratory where I worked before. As a result, I could get my glass apparatus and quartz apparatus fabricated at Tungstram. That was a great help in my doctoral work. But in the late fall of 1946, I had to go back full time to Tungstram because they said, "It's enough. You have to come back because we are building back to full stream." I worked under the leadership of Erno Winter, a very ingenious and clever man, a chemical engineer originally, who had some quite important patents, including the barium tube. He invented the barium getter for getting rid of gasses in the evacuation process of radio tubes.

Anyway, the task I was working on was developing a process for the continuous electrophoretic deposition of earth-alkali carbonates on tungsten filaments which were to be used as cathodes in miniature tubes; that's what we used to call them. They were roughly the size of a cigarette, not as long but a diameter flat. They were the kind of tubes used in proximity fuses. This was 1946, and by this time we did this for the Russians, who took all our output.

Anyway, I learned something about electrophoresis then. This was quite important, it turned out later on, because this was the basis on which they may have hired me at Berkeley. They said, "Well, here is an electrochemist." [laughter]

Anyway, by this time we were in correspondence with my brother, and I told him that I would like very much to come to the U.S., at least to take some courses in nuclear chemistry and other modern subjects. I said that I viewed the three or four years that had passed as lost to the war, and I really needed some further education. I asked could he help. Well, he did.

He arranged it with his boss at that time, E.O. Lawrence's brother, John Lawrence, who was a medical doctor who did some very important work in radiation medicine. It turns out that my brother was the physicist who along with John Lawrence laid the foundation for what today you might call radiation medicine. They were the only ones with access to the cyclotron and when the isotopes started to pour off the cyclotron, they could go to town. Anyway, John Lawrence was very kind. He wrote a letter and offered me a fellowship. I needed such a letter to try to get a passport, which needed Russian approval. In addition, I needed an affidavit because they wouldn't let you come to the U.S. unless somebody would vouch to support you if you needed money.

I got those letters, and then I applied for a passport with not very high hopes that I would get it. By this time the entire ministry was in the hands of the Communist party, and I needed a Russian permit to leave the country. I still have this Russian permit. This was not a permit you applied for in person. When you applied for your passport, you never talked to anybody. You just handed in your request and then three or four weeks later you learned whether you'd gotten it. You learned this by every day going to the huge door, two stories high, of the ministry. On the door was a typed list of people who had gotten their passports, and they could go on such-and-such a day to pick them up. I went every day, and one day my name was there. I also got a Russian permit. It was fantastic because I'd had no assurance whatever. Just to give you an idea, my professor Naray-Szabo didn't get his. A year later he was arrested. He spent eight years in jail.

[END OF TAPE, SIDE 2]

TOBIAS: That fate could have reached me as well, because the charges were totally invented. [short break]

TOBIAS: Yes. First of all I had to get on a ship, but there were no ships going. By sheer accident, I learned in Budapest—which was of course Russian territory, so even getting such news was difficult—that the restored French boat DeGrasse was going at the end of June on its maiden voyage from Le Havre to New York. Now, for this you had to get a ticket, for which you had to pay dollars. Let me not get into details about how that was arranged.

Then you had to get a visa through the Soviet/French/U.S. Sections of Austria, through Switzerland and through France, and they would only give the visas if you had the visas from the next country, because they wanted to be sure that after you entered you also left. The last country would give it to you only if you had a ticket. Let me tell you, it was a nightmarish experience, but I did get out. So at the end of June, I left Hungary, which was hard because I left my parents and sister there who of course needed me badly, but my consolation was that I was hoping to help them after I got here. I went to Paris, which was unbelievable after war-ravaged, bombed-out Budapest, where our beautiful home was in

ruins and at least half of the city's buildings were demolished. After that, Paris was unbelievable, and on the boat I had the most luxurious service and outstanding food, the kind of stuff that I had only heard of in theater or in novels.

It took this full ship eleven days to get to the U.S., but when I saw the Statue of Liberty I felt really pretty damn good. In New York, my first experience was that the taxi cheated me. I went to the Taft hotel from the pier, practically the same side of Manhattan, and he charged me an exorbitant sum, and ever since then I had a fear of taxis. Taxis all over the world have cheated me ever since then. Anyway, I was in New York for three or four days. It was an unbelievable experience. Among other things, I saw *Oklahoma*, the theater show, and of course I went to the museums.

Then I took a flight, United Airlines, a DC4, which is not a pressurized plane. It took a hell of a long time because we couldn't fly over the Rockies. We had to go up to Cheyenne and around. I arrived in San Francisco somewhere around 9 p.m. and my brother was waiting for me. The first impression I got was from the old jalopy that he was driving. The seats were torn, and there was old newspaper all over. I say this because I discovered then that not everybody in the United States is wealthy. Believe me, to us in Europe, America was a land of millionaires, and to discover that there are people who don't live that well was a surprise. My brother was an assistant professor and had just had a second child. You know, it was modest living.

BOHNING: Yes.

TOBIAS: The day after I arrived, my brother took me to see John Lawrence and he was very kind. It was arranged that I should work there although he immediately informed me that he really didn't have more than one or two months of money in this fellowship fund, which of course shocked me. He said, "But don't worry. I will make a few phone calls and you will go." That is when my brother told me, "Charles, you have to learn to accept that here in America things generally turn out the right way. Your experience in life until now was that you always had to cringe because you were worried about what the next thing will be. Here you should be optimistic. It will work out."

The next day I trotted over to the College of Chemistry and introduced myself to [Wendell Mitchell] Latimer. The informality of the situation was amazing. He was quite relaxed; his feet were up on the table. He told me, "Sit down," and "How are you," and "What would you like?" In some pretty bad English I told him that I would like to make up for lost time. I gave him a summary of my background and an abstract of my university certificates. It was a doctor's degree and my bachelor's degree and course grades, et cetera, so he could see what background I had. I said, "I don't know what I could do, but I would maybe like to study nuclear chemistry." He looked at me and he said, "Well, you should talk to Seaborg. His office is here on the third floor. Give him a call. You should talk to him

and maybe also to Perlman." I was taken up there, and it was not easy to get in because on the third floor was the Manhattan Project. There were policemen at both ends of the floor and I was an Hungarian subject.

Anyway, they eventually admitted me, and I talked to Seaborg and Perlman. They would have been willing to accept to me, but I had a feeling that because of this lack of clearance there would have been some sort of difficulty. I went back to Latimer the next day and he said, "How would you like to teach some chemical engineering?" I looked at him and said, "What do you mean, teach?" He says, "Well, I looked at your background and you did actually quite a bit of electrochemistry." Maybe I didn't mention it so far, but my bachelor's degree subject was plating nickel on aluminum.

I tell you right now that that was not easy, because plating anything on aluminum is not easy. Anyway, I worked on that; coincidentally, it was not just nickel, but black nickel-black nickel on aluminum. This was one experiment; the second was my experience with Tungsram and electrophoresis. The third one was that I helped a friend who had a small shop where he did cadmium plating. He needed some advice so I helped him. Anyway, these small items on my curriculum looked good compared to a fresh-out-of-school fellow from an American chemical engineering curriculum who knew nothing about technology but could calculate the bejesus out of anything. [laughter]

So, he asked me, "Would you like to teach?" I said, "Teach?" He said, "We'll be forming a chemical engineering department, and we already have four people on the staff. I noticed that you had only a student visa, so I couldn't offer you anything but a part-time, half-time instructorship." I looked at him. I didn't even know what instructor meant, but he explained it. I was very hesitant because I was overwhelmed. He told me that it would pay eighteen hundred dollars a year. Mind you, I still have the appointment letter. He said, "I'll take you to Bill Gwinn," who was an assistant professor in a lab at the end of the corridor. "You talk to him." Bill Gwinn's field was microwave spectra. He was very kind. He talked to me, and it turned out later on that he liked me because I was a violinist. He learned that about me, and he loved violin himself. Anyway, he said, "Just accept it. You will get it." So, the next day I came back and I told Latimer, "I will be happy to try this." Latimer said, in no uncertain terms, "I expect you to build up engineering electrochemistry here, and it should be the best in the world." My God, those are big words for somebody who had some difficulty speaking English. I could talk, but it was loaded with errors.

I met my new colleagues to be. They were awfully nice people. I was fantastically lucky. All of these personal situations where I met the right people, people of good character and good motivation of collegial attitude, very good stuff. Well, that allowed me immediately to settle in the International House. From there on I had an appointment as an instructor, but it was still summer so I still had my fellowship money from the Donner laboratory, which was the name of the laboratory where my brother worked. It was Donner Laboratory of Medical Physics.

I am leading up now to that first paper. There I met Dr. [Robert] Rosenthal, and he was a very nice fellow. It turned out he was a son of the renowned hematologist, by the same name, from New York. He himself was also a hematologist, and he was studying the effects of nuclear radiation on blood and on physical methods to detect such effects.

This was not very long after the atom bomb, when nuclear disasters were introduced, and nobody knew exactly what happened. One of the things that came into play was what properties would we measure. One thing that was easy to measure was conductivity. I've forgotten who brought this in. It could have been me or it could have been him, or still somebody else, but conductivity is an easy measurement. I quickly adjourned to the library, and I said, "Gee whiz, it should be easy, because the hematocrit, which is the volume of red blood cells in the plasma, should be simply related to conductivity, and you measure that." It wasn't clear to me why it should change upon radiation, but maybe the red blood cells shrink or expand. Anyway, he told me that the experience is that irradiated blood—I forget now if it shrinks; I think the clot shrinks differently from irradiated blood. The question was, "How do we measure this?"

Well, we didn't hesitate very long. We designed a pair of electrodes, and lo and behold, we could measure the conductivity with standard methods: audio-frequency oscillator, Wheatstone bridge and an oscilloscope for the null detector. It turned out that we could measure the hematocrit very accurately. The conductivity of the suspension before and after clotting gave us the volume of nonconducting particles. Until then, physicians determined the hematocrit by just looking at the volume of settled red blood cells. This was very inaccurate. I returned to the library and discovered that because the blood was not settling solid but was composed of sort of disc shaped objects—the blood cells—we had rather complex behavior in the electric field. They affected the conductivity in a complicated manner. As a matter of fact, even if they were spherical, the relationship between the effect of the suspension of spherical non-conductors in a continuum is not a trivial problem, it turns out. It's in fact a problem that Maxwell considered; that bastard considered everything and did it well, I tell you. [laughter] Maxwell considered it sixty or seventy years before. I discovered this by reading. Of course, biologists and medical people never read about Maxwell and only rarely opened physics journals. Neither did electrochemists.

Well, I started to read about the underlying theory, and it sounded very fascinating. I mention this parenthetically to you, that this general problem, namely the conductivity of a suspension of odd-shaped particles, became a life-long engagement with me, because even with just spherical particles the problem is not trivial. Whether you have uniform size or a size distribution, the dependence of conductivity on concentration is non-linear. If it is not evenly distributed, then it becomes a very complex problem. Even in my last Ph.D. student's dissertation research this problem re-surfaced.

BOHNING: I see.

TOBIAS: Anyway, we were successful in correlating the hematocrit to conductivity, quite accurately, and Rosenthal discovered that as the clot forms on the two electrodes and it shrinks, the shrinking clot forms a unique imprint of conductivity. It gives a very good index of the radiation effect, by sheer luck. Anyway, within days, somebody from the Office of Naval Research appeared. Apparently they gave the money for this research and they wanted to patent this device. [laughter] I thought it was funny. Anyway, we wrote the paper (3), and it was, I think, a scientifically valid effort. It started me on a good track because I became familiar with some really first-rate work by such people as Bruggeman from Holland, and Fricke, who was, I think, American. These were good physicists and the mathematics background was solid, as was the physics.

BOHNING: Why do you think Latimer felt that electrochemistry should be an important part of chemical engineering at that early stage?

TOBIAS: Well, that is a very pertinent question. Latimer's major literary work was his monograph text, *Oxidation Potentials*, which turned out to be a classic (4). I wish somebody could have revised it, and included results obtained in the last 40-45 years. A multi-authored compendium was eventually published, but there is no guiding, single, incisive judgment. He had a real overview about electrochemical thermodynamics, and in his reading for background material he became aware of the widespread nature and great economic importance of electrolytic industries. In my recent publication (5), which I'm not sure you have seen, I give him credit for this insight.

BOHNING: Yes. I think you sent it.

TOBIAS: It is correct to say that this has not emanated from the chemical engineering community but from the outside. Ever since then I hammered it back to my colleagues, over forty-four years of service, that periodically we should hire department people with different backgrounds to bring in new ideas and new fields. Don't forget that chemical engineering is an American science. True chemical engineering is an American discovery and contribution—a very respectable one I might add, quantitative, highly successful—but it was generated to satisfy the needs of large-scale petroleum processing and the large-scale process industries. It did very little for small-scale processing, pharmaceuticals and specialty chemicals, and nothing for the myriad of products that do not require huge distillation columns, absorbers, and all that. Of course this, in my opinion, is too bad. I'm not saying that what they created is not good—it's excellent—but chemical engineering, even today, is not a missionary field. It is not a field that tries to embrace all things chemical.

BOHNING: Yes.

TOBIAS: We are actually generating fields for others. The whole field of materials science came into being because chemical engineers did not concern themselves with a lot of things. The old metallurgy departments transformed themselves into materials science, and it was just in time because otherwise they would have had nothing. Chemical engineering is, unfortunately, not a missionary field, and is not sufficiently concerned with the vast spectrum of chemical technology, which is way, way beyond petroleum processing and beyond the major products of the chemical process industries.

I never found that so interesting. I think I would have done anything but work for an oil company. I realize they are important, but I would not like to do that.

BOHNING: I was struck by the fact that when you came to the United States it was exactly fifty years after Herbert Dow started his company in Midland.

TOBIAS: Yes.

BOHNING: Here is a company built on electrochemical processes, but as you pointed out in that article, the cells never changed.

TOBIAS: The cells reflected a great deal of ingenuity. Many of the non-scientifically created chemical industry apparatuses show a great deal of ingenuity, but you can't pass ingenuity on to future generations. Dow himself was an unusually brilliant man, and he had co-workers who were very good, but they could never describe their results in scientific terms. They could never generalize principles of design. I was amazed. I was a consultant to Dow but very late and mostly only about batteries. The Midland laboratories were thoroughly based on empiricism, and what I regret is that the very fine people there assumed defensive attitudes, and looked at people like me with a great deal of suspicion. I always felt in their presence that there were too many secrets. I must have hurt some feelings, but I don't believe in secrecy. Only ignorance needs to be kept a secret, knowledge does not.

[END OF TAPE, SIDE 3]

TOBIAS: Incidentally, I will tell you that this is a golden truth. Did you ask me what my credo is? This is it. I don't believe that one can or should keep scientific knowledge secret. It's a totally self-defeating and demeaning position. However, the chemical industry was not

terribly interested in the kind of stuff that Tobias was doing. They would ask me, "What process did you develop? Show me the balance sheet."

Actually, they never asked me this, but this is what I attribute to them. However, I also benefitted from this lack of interest, because the internal resources at that time—the early 1950s—of the college of chemistry were sufficient to support a modest level of research without needing outside support. Interestingly enough, one of the first topics came about because I developed an interest in hydrogen peroxide in Hungary. When the Russians occupied the factory where I worked, Tungsram, the translator for the Russian commandant came to the laboratory and said, "Tobias, the commandant's blonde girlfriend needs to dye her hair, and she needs the job done soon. Find out how to do it, and I will make it worthwhile for you." I said, "My dear friend, there is absolutely nothing here. This city is as dull as it is ravaged. There is not one building standing; what do you expect me to do?" He said, "Tobias, you will not regret it," so I went to the library right away, to the German compendium recipe books, and I found out that you can blond hair with the help of hydrogen peroxide, and if you add a little ammonia then it will be reddish hued. I didn't waste time. I made a small portion for his friend; told him how difficult it was for me to get this; and I was suitably rewarded with many loaves of bread and sausage. This was an almost incredible thing.

When I started my work at Berkeley, hydrogen peroxide was in my mind and I became interested in how it was made. It turns out that the Germans made a huge amount of it for these buzz-bombs, the V2, and they made it by electrolysis. They actually made peroxy-sulfuric acid, which upon hydrolysis gives you sulfuric acid. It used Fort Knox's platinum, figuratively speaking. It used a huge amount of platinum, because you needed platinum anodes. I will just add that platinum doesn't stay put; it erodes and some part of it gets lost permanently. I was interested in why it must be platinum, and then I saw that even if it is platinum, it should use only a thin surface layer. I had some platinum clad tantalum sheets made and I built an apparatus to continuously produce peroxy-sulfuric acid. It was a very successful apparatus with pumps I designed which operated on pressure and suction and that had no metal parts. It was all glass and looked very nice. Nowadays, you would put this in a museum of modern art. People from all over the building came to look at the fancy stuff Tobias put together.

It was good stuff, and as I was doing this hydrogen peroxide thing, I bumped into the ozone issue in my reading—actually you can make ozone in very high concentrations by electrolysis. There were some papers in the literature about perchloric acid at minus 50, 60 or 70 degrees giving as much as 30 percent ozone in oxygen. Good stuff, compared to the silent discharge method which gives you half a percent. Then I said it was too much to work with concentrated perchloric acid. That was a time when they had this huge explosion at UCLA.

BOHNING: Hmm.

TOBIAS: Remember, if you get some organic matter into this acid, it takes off. I said, "What is the reason? What is the miraculous thing about that? Why should you use perchloric acid?" I started to read the German literature, because by the second half of the 19th century, the Germans did an enormous amount of preparative chemistry, and it paid to look up whether they did it or not. As it turns out, ozone was discovered by Schonbein, anodically in sulfuric acid electrolysis.

Then my question was, why was it not as efficient as this method that employs HClO_4 ? It turns out that Schonbein didn't lower the temperature enough. Then I said, "Why didn't he lower the temperature enough?" I found, of course, that there are eutectic mixtures of H_2SO_4 - H_2O that allow reaching low enough temperatures, but only in certain narrow ranges of composition. The world's greatest expert on sulfuric acid thermodynamics was 50 yards from me, William Giauque, the Nobel winner, and so I went to him.

He was a rather formidable presence, very intimidating. He didn't suffer fools easily, so I just said, "Do you have a phase diagram of sulfuric acid, and could you lend me one?" He murmured something—why don't I go to the library and find one—but he gave me one, and right away I saw that there were several ranges of composition which allowed us to go down. I had my first master's student, with utterly no fellowship. I think he was a teaching assistant. Junior Devere Seader eventually ended up as a professor of chemical engineering, and chairman of the department, University of Utah, Salt Lake City. I was enormously lucky, because he was a brilliant and hardworking student. We tried my ideas and it worked. We produced very high concentrations of ozone.

We learned a little about the special problems of iodometry, when you determine ozone, because it's not trivial. Unfortunately, life isn't simple. We were successful and I gave my first research seminar to the college on ozone. It was lucky because a chemist said, "Finally, here is a chemical engineer who is talking about chemistry." [laughter] Ozone remained an interesting subject for me, and I returned to it twenty years later. By that time, we became more immodest and worked with superacids, hexafluorophosphoric acid and antimononic acid. These give very high yields of ozone and the temperature doesn't have to be so low. In fact, even at room temperature, it gives you maybe five percent yield. I actually got a little bit of patent money from the University just last year. Of course, what they give you is a laughable sum, about \$5,000. It was good to feel that by doing good science, you could actually make something useful. It was ICI, incidentally, who picked up the patent. Can I stop now a little bit?

BOHNING: Certainly, certainly. [short break]

TOBIAS: Of course necessity is the best teacher. I took private English lessons in Hungary, and when I was in college I did a great deal of reading and my vocabulary was good. I

learned English by reading Somerset Maugham and Aldous Huxley. Huxley's language is very good. The first real experience with English was on the boat in which I came over. Our table was a nice, big, round table with some American couples and some British. The British I didn't understand at all and to date I don't. I think that so many of them speak with what I would call affectation, even pretense. It indicates a certain social class if you speak Oxford or Cambridge English. I found that American English was a very good language, very practical and simple. Early in my work at Berkeley I learned to write out lectures. The experience of writing and seeing what I was going to say was important, and I quickly learned to present acceptable lectures. The European tradition is oral, and you are asked to present stuff early in high school and grammar school, but in America, to preserve the poor child, to keep him from being hurt, they don't ask him to speak. I think that's a ridiculous position. We were called upon to speak in front of the class and recite hundreds of times. Yes, we laughed at students who didn't do it well, but that made us learn. So the oral tradition is there. Europeans also have inflection in their voice. It's more interesting. The ultimate illustration, to me, of bad English presentation is Eisenhower. Totally one level, one color, and you never knew where you were in the sentence. He never raised or lowered his voice but would cackle and rasp his throat periodically.

We had to write a great deal, and I also think French and German helped. I always loved English. English is a very good language from my point of view. It is especially good for matters scientific. Short declarative sentences are accepted as good English, and it is clear. The clarity is easily achieved.

BOHNING: It was always a problem with German.

TOBIAS: Oh. It is very complex, and let me tell you that good German is very complicated.

BOHNING: Yes.

TOBIAS: German that makes you feel that the speaker is well educated is a very complicated language.

BOHNING: Your first assignment was a graduate course, a seminar course.

TOBIAS: I didn't have to lecture so much, and I only had four or five graduate students who were probably told by their advisor to take the class. I handed them some papers that I found. As I wrote in my article, I had a formidable audience. [laughter] If I had fully appreciated the intellectual power of the people who were sitting there, I would have been frightened.

BOHNING: That included Pitzer and Brewer.

TOBIAS: Giauque and Latimer and people who hardly showed interest, but Latimer came all the time. The other thing that I found at the beginning that was very important, was the enormous good will I encountered. I say that in a very generalized way. America didn't have the terrible experiences of Europe—many wars, one after the other—that caused people not to trust each other because they had to climb on somebody's back to get where they wanted. Here I found that people were very helpful, and very kind and very open. There was no conniving. Things were very simple, and that was a wonderful thing for me. It didn't take much at all to get used to it.

BOHNING: You've indicated, on a number of occasions, that Latimer was the person who had the greatest influence on you.

TOBIAS: At that time, yes. I saw him every day and he spoke to me. I went to his office, we sat down and we talked about scientific things. At that time, people had more time for that. I shudder at the thought of how little time we spend, nowadays, talking to our young colleagues, compared to when I came. Maybe Latimer was a very unusual case. I am pretty sure he was. He was a man beyond personal ambition; I think he lost a son a few years back. He was very friendly, helpful and interested, and we had very good rapport.

He also played Hearts at the faculty club. I started to watch, soon after I joined, and became a lifelong Hearts player. Literally every day at noon, I would adjourn to the faculty club to play Hearts. We had a terribly interesting crew of people from many different departments, and I'm grateful to Latimer for this, because this was the way I met people from all across campus. It was a time of relaxation and good cheer, and we laughed a lot. Nowadays, many of my young colleagues never meet anybody outside their department. Of course the pressures are different. The need to chase money is an evil influence on life and the quality of work. We didn't have to do that, it was a very special situation at Berkeley. Instrumentation was simple, and students did teaching assistantships. Then in the early 1950s, the Lawrence laboratory started a program in chemical engineering, asking for very little in exchange. They didn't tell us what we must do. We didn't have to write reports, and they didn't ask for justification. That was fantastic because I was able to formulate questions without worrying about who was going to pay for it. Nowadays, it's a whole different ballgame.

A number of coincidences in the 1950s allowed me to formulate a certain direction for myself. We had very few graduate students. At that time, most of us had one, or at most two at any given time.

BOHNING: And most of these were master's students rather than Ph.D.s?

TOBIAS: Yes, about an even plate. Numbers were very small. Then in the 1960s, there started to be an ever increasing stream of students, and there are many factors responsible for it.

First of all, money became easily available. There was what then was called the Inorganic Materials Research Division of Lawrence Berkeley Laboratory. I became a member of that. They asked me how much money I needed, and even though they didn't quite give me what I said I needed, it was enough to have a decent program. I didn't have to justify what I wanted to do. Later on, this transformed in a drastic way, and year after year I was made to feel by Washington, by the DOE [Department of Energy] people, that they were not convinced that all this electrochemical stuff was that important. But they kept on giving me the money anyway, although I certainly didn't feel that they were enthusiastic about it.

In the 1960s I started to get—the whole department started to attract—a lot of very good students, and I started to get co-workers who were absolutely first rate.

BOHNING: I would like to go back to the experience of putting together this first seminar course. You found that the available text really didn't measure up. I was sort of getting at this earlier, when I mentioned that it was fifty years after Herbert Dow, yet the industrial aspect of electrochemistry, or the engineering aspect, still hadn't been formulated in a more concrete sense.

TOBIAS: The available books, those dealing with electrochemical technology, were descriptive, and in parts, very weak scientifically, with major conceptual errors. I couldn't accept these as text books. Partly because students would have revolted if I gave them 500 pages describing process after process without a reason as to why. The books had very good picture material and were valuable, but this is not engineering.

[END OF TAPE, SIDE 4]

TOBIAS: I mean these books like Mantell's *Industrial Electrochemistry* (6) that was then renamed by McGraw-Hill *Electrochemical Engineering* (7). This prompted me to write a nasty letter to them. I said that the publisher who had the good fortune and honor to publish Lewis & Randall's *Thermodynamics* (8) should not engage in such games, like re-christening old and invalid technological texts.

But the real thing I based my teaching of graduate students on—and in years following also—was putting in their hands a variable mixture of original papers, papers which I regarded, in my education, as very important. There was not a huge number. There were papers by a Russian, Levich, a physico-chemical hydrodynamicist, by Karl Wagner, a distinguished German physical chemist, and a British chemist, [John] Agar. The papers proved to be very useful and I felt very good about them, because a student could fully follow the logic of an approach to a problem and not just be given the answer. Many times, I felt that one of the problems in teaching is that you don't have time to show how the question was posed, why the question was posed, and how did this particular author approach it. There is a lot to learn from that.

I found this very useful and none of my students ever complained later on. Essentially, they went through the same set of papers as I did. Now, you might ask the question, why didn't I ever write a book? I was urged many times, but besides being somewhat intimidated by such a huge task, by necessity I would have had to write a lot about things that were distant from me, and I would not have done this well. Only part of this book would have been based on material with which I am intimately familiar. I was intimidated by this, and I never wanted to write a book. Fortunately, one of my former students, John Newman, (9) wrote a book, which is very popular. Being a fine mathematician, he was not bothered by the same thoughts as I was.

My undergraduate course concerned with electrochemical matters, offered annually since 1949, first also served as an "inorganic chemistry elective," a required component of our chemical engineering curriculum. Over the years I had developed a set of brief notes, concerned with thermodynamics, kinetics, and electrochemical transport phenomena. These were to be read as a collateral to my lectures. In addition I assigned weekly readings for the whole semester: 15-30 pages from a well written, illustrated chapter concerned with key products. Our library had six to eight loan copies of each book, or article, which were on two hour reserve during the day, and could be checked out for overnight use. I gave half hour quizzes in two week intervals, emphasizing conceptual understanding, not regurgitation of equations.

However, quite a good fraction of my students' work was not published, not because it was not good, but because the student didn't write the paper. I don't know where I acquired this position; I am not a lazy person. Part of a student's doctoral work is to not only write a thesis, but write a paper and put it in the literature. Not only that, but present it at scientific meetings. All throughout my career, I made it possible for students, finishing students, to go to meetings. I put up money from my grants, because I consider it a very important experience—sort of their crowning achievement. I never wanted to pretend that it was my work. I had a part in that work, but it wasn't my property. Now you could ask, should I have done the writing when the student couldn't do it? Yes, I should have. There is a good deal of good work that I didn't publish. It's not totally lost because almost everything is published

in the form of Lawrence Berkeley Laboratory reports. A good many of my students became teachers themselves.

Also, don't forget that the pressure to publish—especially in large quantity—became stronger and stronger these passing years. At the beginning of my career, it wasn't all that strong. I think they read your work more carefully; they were not looking for huge numbers of papers. Nowadays, I am staggered when I look at the number of papers somebody up for tenure has already published. I read that some of my senior colleagues, by the time they are fifty, have published a thousand papers. I don't even know that they read all of them.
[laughter]

BOHNING: When you first started at Berkeley you were only able to teach part-time, because of your visa situation. How and when did you get that corrected so you could stay at Berkeley on a full time basis?

TOBIAS: In the third year, I got what you might call today a green card, under the displaced persons act. That's how the circumstances changed. Then in 1950 I was promoted to assistant professor, in 1955 to associate professor, and in 1960 to full professor.

By the time I was an assistant professor, I felt very comfortable. I wasn't very worried before that either because my colleagues were generally very supportive, and my standards and achievements were not that different from theirs. I didn't feel outclassed, and I also had a fairly strong inner conviction that what I was doing was really quite good stuff and quite important and that the industry was not there yet. This was all right with me. This will lead into interesting discussions about The Electrochemical Society, where I first started to go to the meetings of the local section.

BOHNING: I was wondering if we could save the electrochemical society until tomorrow.

TOBIAS: Okay, good.

BOHNING: We could concentrate on a lot of your activities there tomorrow afternoon.

TOBIAS: Yes. Eventually I produced something like 40 Ph.D.s and I still have two in the pipeline, and some 35 master's degrees. Some of these master's students went on to get Ph.D.s. There were not many failures. If there was a failure, I always regarded myself as part of the failure, but I think I am not deluding myself when I say that I was not the major reason for the failure. Usually it's not a scientific reason.

BOHNING: Yes.

TOBIAS: Not a failure to meet scientific criteria. It's a question of character, or a question of personality. Difficulties in life et cetera, et cetera, complications. Falling in love, falling out of love, et cetera. Not many of those. A few cases were motivational. One brilliant student could not decide, after very patient waiting on my part, whether he really wanted to remain in this field at all. Eventually he left, and it turns out he didn't want to be a chemical engineer. He had his B.S. from MIT. He was brilliant. I gave him a long time to flounder and not produce, but periodically he showed that he was able.

I consider my work with graduate students the key element in my service, and I enjoyed it immensely. I think it's a great privilege to work with talented young people. No where else in life do you have this idealistic condition where there is no conflict built into your relationship with people. If you work in industry there is a conflict. Your ambition might hurt somebody else. At school, that's not true. We are on the same wavelengths. I found that very, very pleasant.

I enjoyed teaching undergraduates very much. I think I was a good teacher, and I always got good classroom evaluations. The classroom evaluation was introduced in early 1970s or late 1960s, and although I never believed that these classroom evaluations had full validity, it was something. I liked classroom work, and in fact I always remained an undergraduate advisor. Every one of the members of the faculty has to do advising work. Some do graduate advising, others undergraduate, and some do a mixture. I always wanted to do undergraduate advising, and I developed an electrochemistry course at the undergraduate level and one at the graduate level. I also developed a freshman course, "Modern Chemical Technology," which was a motivational course that I taught in conjunction with Professor Michael C. Williams, who is now at the University of Alberta at Edmonton. In that course we confronted freshmen with real problems, not watered-down problems. We gave them the same problem that a senior would get, and there was a lot of hand holding, but we carried them through and thereby illustrated what chemical engineering is about. It was successful and motivational.

I developed another course, or contributed to its development. It was originally started by my colleague Judd King, who is now vice-president of the University, and Professor Scott Lynn. The course concerned judgmental aspects of chemical engineering. Professor Eugene Peterson also participated when Professors King or Lynn were not available. In this class, "Process Synthesis," we analyzed real situations in industry that might include an existing process, the invention of a new process or a failure analysis. This class was offered at both the undergraduate and graduate level. I liked to teach that class very much, and I found it very interesting. At the undergraduate level students are used to knowing everything and being given well-formulated problems that have a singular answer.

In our course, the questions were not well formulated, there were no singular answers, and it was very interesting how students showed up in this light. Some of them shied away and were very upset by this atmosphere, while others thrived in it by using judgment and intuition, et cetera. That was a lot of fun.

We had case studies that we developed over the years. The electric car was one of the topics I always did.

BOHNING: I think at this point we should come to a close. I appreciate your spending this time with me today.

TOBIAS: It's not easy for me.

BOHNING: I understand that.

TOBIAS: Physical energy takes quite a lot.

BOHNING: No, I understand that.

[END OF TAPE, SIDE 5]

INTERVIEWEE: Charles W. Tobias

INTERVIEWER: James J. Bohning

LOCATION: Orinda, California

DATE: 16 May 1995

BOHNING: Today we would like to focus on your activities with The Electrochemical Society.

TOBIAS: Yes

BOHNING: We've already discussed the origins of your interest in electrochemistry. You joined the society in 1948?

TOBIAS: Yes. The story of when I first became aware of the society is very interesting. It emphasizes what I believe is the key role of the Society in scientific life. My bachelor's thesis in Budapest was on black nickel plating of aluminum, and I was doing library research when I discovered the *Transactions of The Electrochemical Society*. I started to run through these and became fascinated. Unlike other journal publications, at the end of each chapter there was a very lively discussion session which helped to illuminate the subject of the chapter. I liked that a great deal, and it made an impression on me. Later on when I came to the United States, I remembered this very well.

My membership with the society started here in the local section. There was a local section in the 1930s, maybe even 1920s, but that went defunct. Somebody from Dow chemical, I think it may have been either Richmond or Richard Bechtold, contacted our department to see whether there was an interest in reviving the section. We said yes, and my colleague Theodore Vermeulen and I—he was then the head of our group—went to the first meeting. It was in the city at a wonderful French restaurant, St. Julien's. I remember this very well because I didn't have enough money to join them for dinner. My salary was very modest and I just watched them eating an awfully good French dinner. It was a small group, maybe ten or twelve people, and many of them were in technical sales rather than scientific pursuits. Dick Bechtold was the assistant superintendent of the Dow chlorine factory in Pittsburgh, California. He was a very attractive, very bright young engineer, and we hit it off right away. Incidentally, that plant was originally with something like Western Electrochemical. Dr. Hirschkind was there earlier.

BOHNING: Yes.

TOBIAS: How come you know that?

BOHNING: Well, I've been doing a lot of Dow interviews (10).

TOBIAS: Oh.

BOHNING: Did you interact with Hirschkind at all?

TOBIAS: Later on I did. That was a very interesting interlude, but if I start to talk about that, it will be a half hour. [laughter]

He made me aware of very interesting historic aspects of, of all people, Fritz Haber. He was the last doctoral candidate of Fritz Haber. He told me that Haber was such a sabre-rattling German patriot that when they demoted him in the thirties because he was a Jew, it was a blow that he could never survive.

This contact with Dick Bechtold kept my interest, and our small group met maybe three or four times a year in the city of San Francisco. We didn't have terribly technical talks, because there were not many of us to give those, but we did have a few each year. Eventually this local section became much more technical, and quite a thriving little enterprise, especially when Kaiser Aluminum and Chemical Company started a pilot scale aluminum operation in the Bay Area. Chemical engineers who were in that group, including Ted Beck, caught wind of this local section; they became involved, and we had quite a growing concern there. I became chairman of this group in the early 1950s. First it was Bechtold and then I took over. We were a lively group.

As to the national Society, I joined early in part because of this local connection and because of my early experience in Hungary with the Society Transactions. I thought it was a good thing. I forget the first meeting I went to, but it must have been in the very early 1950s. I don't have the records here but I could dig it up somewhere. It could have been 1950 or 1951. I remember that it was in Philadelphia [1952 meeting], and I was very impressed. It was a manageable-sized enterprise, and people were very friendly. The technical sessions attracted comfortable crowds, and there were discussions after each talk. It impressed me as a friendly group and I was very interested in being active in it. I also discovered early on that this was a group that encouraged people to learn things. One didn't feel squashed by a superstructure of powerful individuals who would not let you do what you want to do. Later

on this became very, very important for me, and it was strong motivation for my remaining active in the Society throughout my professional life.

BOHNING: Did you meet some of the old-timers in the Society at any of the early meetings?

TOBIAS: Oh, absolutely. I met some who were very pleasant and encouraging. For instance, Bob [Robert] Burns from Bell Laboratories and Norman Hackerman from the University of Texas. I found Burns a very charming and interesting individual, and later on I learned how important he was in putting the society and its publications on the right track. I also met some of the technical salesmen—people from Union Carbide or representatives of the chlorine industry from the sales end, rather than the scientific end. They were also nice people, but I didn't feel very much kinship with them. Once in a while they took me along to their cocktail hours after the meetings. I didn't know much about drinking scotch, but I joined them. It was good fellowship. It was not very technical, but I cannot throw a stone. They were good decent people. I met Ralph Hunter from Dow Chemical, and while he was friendly and encouraging, I didn't have the feeling that he was a strongly scientifically-oriented individual. I did have the feeling that he was a very successful developer of plant scale processes, and indeed he was. He was the head of the electrochemical laboratory in Midland. I'm sure you are familiar with him.

BOHNING: When did you give your first paper?

TOBIAS: That was in Philadelphia.

BOHNING: Okay.

TOBIAS: The reason why I went was to give the paper, and I believe I gave it on free convection effects in electrolysis—mass transfer in free convection. I think it was a good-quality technical paper. It didn't create great waves because chemists knew nothing about transport, much to my chagrin. They still don't know, even today, why the real process is controlled by transport. But my talk was attended by a reasonable sprinkling of people, and I felt that this was a good place. I thought, "If you become active, you might influence what goes on here." I certainly was encouraged to be active and join. So the question is, what were my pre-presidential activities in the society? I mentioned local sections already, but I should mention the divisions. This is an interesting thing. At this meeting, I believe it was in Philadelphia, I went to the business meeting on Friday. To begin with, the business meetings of divisions were not very businesslike, and on this Friday afternoon there were only eight or ten people in the room. We were supposed to transact the business of the division, and it

turned out that neither the chairman nor the vice chairman nor the secretary of the division were present. Somebody from the National Headquarters came and presented us with the official slate, and I took it upon myself to talk to Ernest Yeager from Western Reserve, whose work I already knew, and Paul Delahay, a very fine scientist at that time from Louisiana State. We put our heads together and said, "This is a division election and nobody is here, and there is no one here with whom we could discuss the program of the division." This was called the Theoretical Division. I have to remind people that the reason why there was a such-named division was that the rest of the divisions had technological names: Electrothermics & Metallurgy, Electroplating, Batteries, et cetera. Supposedly, the science was to be done by the theoretical division. Of course it wasn't very theoretical, but it was scientifically oriented. That's how I went there, instead of the Industrial Electrolytic Division. I went to the Theoretical Division because they were the ones who discussed quantitative aspects of processes and foundations of electrochemical technology—meaning the science of technology. So I got up at this meeting and gave an impassioned speech, with my typical Hungarian temperamental emphasis, and said that we should propose a slate from the floor. I knew nothing about the by-laws or whether this was possible or not, but later I discovered that there were no by-laws for the division, or at least, few were followed. We agreed that we should keep the present chairman, who was L.B. Rogers, a professor at MIT. We decided to leave him in place. He didn't do much, but we left him in place. We elected Paul [Delahay] for Vice Chairman and Yeager for Secretary. I believe this was the order, but it could have been the reverse. Incidentally, they suggested that I should become chairman, which shows how open-minded they were. I said, "I am corrupt, but not that corrupt. Let's leave Rogers in. I will come to this in due time." So we changed the slate, and this was a revolutionary move. There was a great deal of murmuring among the elders of the society, but they didn't do anything about it, and in my opinion it changed the course of the society in a very significant way.

We immediately sat down with these two colleagues and decided that this program needed revitalization. We needed to arrange symposia, do all this through correspondence right away, and see to it that the division has a valid program. I will add here that such things did indeed develop, and the division changed its character from a floundering, sleepy place to a vital and expansive group. We got more and more people joining the activities and beginning in the mid 1950s we had a whole series of excellent symposia. We had to invite a good many people from abroad, because electrochemical science wasn't exactly popular in the United States. There weren't a huge number of people in academia you could invite, and we didn't just want to alternate ourselves as speakers—to speak about what we didn't know or ground that we didn't cover at all. I have some pictures preserved from this time which show really first rate selection of British, French and German scientists whom we invited. We obtained some assistance for their travel from the Society, and we did this for many years.

BOHNING: Were other divisions inviting foreign speakers, or were you unique in doing that at that time?

TOBIAS: I cannot speak for them. I don't know the record. I know that we did it right away. We had the first symposium on electrochemical engineering in 1960, and that was co-sponsored by the Theoretical Division, by the Industrial Electrolytic Division, and I believe the Battery Division. It was very successful; we had a lively crew in attendance, and it reflected how little people knew about the quantitative aspects of electrochemical technology.

I got a few speakers from abroad, notably Norbert Ibl from Switzerland, whose work I was familiar with and for whom I developed a great liking. There was a series of papers which more or less mapped out the field. It was almost on the level of a tutorial because the contributions you could get from American industrial people—there was nobody in academia except myself—were only descriptive or semi-quantitative empirical papers. Industry was terribly secretive. Ralph Hunter was the absolute worst. He pulled you into a corner and told you, "I am unable to talk about this, but we have an excellent development in this area." He wouldn't tell me what development. I would ask, "Could you suggest something for me to become involved with that would be of interest to industry." He wouldn't even tell me that because that would reveal their problems or their involvements. I thought it was terrible. That is when I developed the philosophy that secrecy covers ignorance. If you have knowledge, you don't want secrecy. You want openness.

I mentioned pre-presidential activities, and I mentioned local sections. Of course I became involved with the Theoretical Division, and that remained my main affiliation. I became its chairman, of course, in due time. In the 1970s, by which time the Theoretical Division was very strong, I switched and I started to attend more of the Industrial Electrolytic Division's activities. I saw that it needed to be brought more in line with modern engineering.

I became involved with publications already in the fifties. I sent my papers to the Journal [of the ECS] to be published—you will notice in the 1950s I have a few papers—and lo and behold in the mid 1950s, Carl Wagner, who was a divisional editor at the time, resigned and recommended my appointment as divisional editor. I was awfully green compared to Carl Wagner, who was one of the greatest scientists of the century in my book at least, but I think in other books too.

I took over, and at that time there were only five or six divisional editors. I was pretty busy, because I would get all the so-called theoretical papers in the journal. Needless to say, during this time the solid-state side of the society emerged. Of course I was not qualified to handle that, and there were competent people to do that side. Whenever a paper had, shall we say, fundamental or qualitative aspects, it was sent to me. This was quite a task for me at that time. I had to quickly develop keener judgment and a knowledge of who the people were that I could send papers to for reviews. I quickly became acquainted with an entire landscape of scientists and technologies, and I certainly had to learn very fast.

BOHNING: Were most of those people industrial people as opposed to academic people?

TOBIAS: I would say that the reviewers were split. There weren't many academics and virtually no engineers, but there were a few respected scientists in academia. I had to become acquainted with them because I needed them. Necessity is a good teacher you know. [laughter]

Coming to awards and prizes, through these activities I became known as one of the active younger members. I was in my mid-thirties at that time and I joined various working committees. I recall one in particular. This was before I became president. I think it was a Palladium Medal committee, and much to my amazement it was run as a fiefdom by Herbert Uhlig, later on the president of the Society and a respected corrosion expert from MIT. This was really a divisional award, but since the society only had one award, the Acheson award, this Palladium Medal was sort of a semi-divisional, semi-society award. I was on the committee and I discovered that we never met in person. Voting was done by phone or by writing, and there was a tremendous amount of vote-splitting. I protested against that because eventually there was at least one case in which somebody was nominated for the Board's approval whose name I'd never heard before, and that looked bad. I was already the division editor for many years, and I didn't even know the nominee. It turned out he was a friend of Herbert Uhlig, or at least Uhlig knew him well. I might add that the board did not accept the nomination, which is a very unusual step. The reason I was upset was because there were some other excellent nominees who didn't make it due to Uhlig's engineering. During my presidency the bylaws were changed so that award committees had to meet in person at least once. I insisted on that and it is still run that way today. You can't just vote by mail or telephone.

[END OF TAPE, SIDE 6]

TOBIAS: Among my pre-presidential activities I should mention, there was a highly successful meeting of the society in San Francisco—the first such meeting. We had it in the Mark Hopkins Hotel, which at that time was the best hotel in the city. [laughter] I never stopped hearing criticisms from the old timers for spending money like crazy. We went out to local industry and got some money. We hired a complete dance orchestra and put on a show with Broadway singers and actors, et cetera. There was a fantastic buffet and dinner, but no beer drinking. I hate the smell of beer; it is such low-class stuff; I would outlaw it. In the two meetings we had in San Francisco while I was active, there was no beer. In fact, we got wineries to contribute for wine tasting.

I became quite intensely active in the Society. I attended every meeting in the 1960s, and was asked to serve on various committees. In due time I was asked to run for vice president—as you know, one first serves three years as vice president and then is

automatically made president. An interesting feature was that at that time they put up three candidates for election. The electorate had to choose between three, not two. On the official slate they put me up along with Paul Delahay, who was also a professor, against this third person, who was from industry. It was very clear that this was vote splitting. [laughter]

Neither of us got elected although together we got many more votes than the industrial fellow. During my presidency this was changed so that henceforth the official slate had only two members for the electorate to choose from, and they should be of reasonably equal strengths and represent the different areas of the society. Hopefully, people from solid state and wet chemistry would alternate, as they should between academia and industry. None of this three people standing for election, because that way you don't necessarily elect the most desirable candidate. Anyway, in two more years I was put up again as a candidate and was elected. I served as vice-president for three years and one year as president, and it teaches you a lot. It also allows you to follow through on some of your ideas and things you want to accomplish.

If I talk about my presidency you have to realize that it covers more like six years, rather than one, because it includes three years as vice-president, one year as president and then two more years on the board. Past presidents were on the board for two years. I certainly don't wish to cover in any ordinary manner or enumerative manner all the things I tried to do and succeeded or didn't succeed in. There is a record of what business the Society transacted during this year and anybody who's interested in that can follow the meeting minutes of various meetings: the Ways and Means committee, the Technical Committee and of course meetings of the Board.

But there are certain things that I recall as having been important to me, things that I was somewhat passionate about, although I won't make the claim that I did them single-handedly—by no means. In a democracy, unless you have your colleagues' support, you won't accomplish a thing. I like the way that this is a democratic institution and not one-man rule. I have to remind you that for a very long time the Society was run out of an office at Columbia University. Colin G. Fink and then later on his student who became professor, [Henry] Linford, had the Society office in their academic office, and even though they were benevolent autocrats, they were running the Society. Well, a secretary is not supposed to run a society. The Board is supposed to run the Society, and the Board assigns executive powers to the president.

One of the important things that we achieved in this time span that I mentioned was that gradually the transactional business of the Society became more and more business-like and tightly organized, and therefore became valid. Earlier, the board used to meet on Sunday, and then the committees would meet afterwards. Well, that made it meaningless, because whatever the committees did would cover for action only half a year later, by which time most people forgot what this whole thing was about. So we changed that.

We changed it so that the Board met Thursday and all the committees met before, starting Saturday—the Executive Committee, when I was president, met Saturday, and then various committees met. Many of them met Sundays because people would want to attend technical sessions, and some committees still met during the week. This changed the complexion of the Society in a very serious way. When I first attended board meetings as a divisional officer, chairman of the Theoretical Division, I was aghast. Our meetings were on Sundays, and frankly by the time we sat down around a huge table, board members, including non-members, were soused. We had a number of drinks and there was no serious business done. There was no need to do serious business because of the very strong power of the secretary—both the executive secretary and the so-called Secretary of the Society. I remember very well the names of some colleagues who were supposed to contribute to serious things but were somewhat drunk.

Well, this was completely out of the question to run it this way, and it was Ivor Campbell—he was president for two years before I was really instrumental—who with our help changed the working methods of the assembly. We introduced a rigid structure concerning what various committees had to transact and when they had to transact it. The Board was to meet last in the week, meaning Thursday, and the business detail was performed by the Technical Committee, the Ways and Means Committee, a Publication Committee, awards committees and a number of other committees concerned with finances. This changed the Society's complexion in a very significant way, I assure you. I already mentioned that the Honors and Awards Committee was required to meet in person, as were the subcommittees concerned with the Acheson and the Palladium medals.

I personally crusaded during my presidency, and even before it, for the introduction of more awards, because I knew how important it was to stimulate activity, especially by younger, technically-keen members in whose careers awards can be very significant. The Society had only one major award, the Acheson Medal, and that was usually given to no one younger than fifty. Furthermore, the medal was very often given for merits in serving the Society; it was not only given for scientific merit.

The other award, the Palladium Medal, was a divisional award—corrosion—only later on was it broadened to include fundamental electrochemistry. I was pushing for more awards and I note with a certain pleasure that nowadays the society has at least three more Society awards: the Electrochemical Engineering Award, the Solid State Science and Engineering Award, the Wagner Award for mid-career people, and of course the Palladium Award which became a Society award instead of a divisional award. In addition to this, at least six, maybe more, divisional awards were created, and this has become a very important activity in the Society. It has attracted more technically keen and ambitious young people into our ranks, and that was important for me.

BOHNING: As you reorganized this whole meeting arrangement that you just described, did you have any opposition?

TOBIAS: Yes, there was. Not as much opposition as, "Oh, we don't have enough candidates," and, "Where is the money going to come from," et cetera, et cetera. I said, "We shall get sponsors," and of course we did. There were enough candidates, but an honest awards committee has to work hard to develop a valid list of candidates. It takes effort, and we have to be reminded of that.

BOHNING: What about changing the meeting dates and putting the Board last instead of first?

TOBIAS: I don't remember any huge opposition. Of course there is opposition to everything that you want to change, but that was accepted. Don't forget that by this time, when this came up, there were several of us in this group of vice president and president who were attuned to such changes. We bulldozed a lot of things through; we pushed enough. We knew it was correct. Don't forget that a lot of these actions required change in by-laws, not everything, but a lot of them, so there was a long lead time. However, Ivor Campbell had a law degree in addition to a scientific degree, and he was very helpful in getting these things accomplished.

Another area that I had to deal with, maybe not a popular one but one that coincided with my presidency, was the student activists on the various campuses in the United States. There were also activists within our Society, and a rather conservative membership didn't like this. The activists wanted discussion of current social issues and wanted to shake up this group, and there were strong voices to the effect that we should just exclude them and close the doors. I didn't do that. I didn't like to be disturbed, but I thought that one way to deal with this is to let them talk. Do not allow them to talk in technical sessions, because that would disturb the normal flow and manner in which we run a meeting, but allocate special places and times for such discussions to take place. We would advertise this on big billboards and during several of our meetings we had special meetings for what I would call activists. Actually, we overwhelmed the activists with our participation, because the whole board came and a lot of people who opposed this came; we had more defenders than activists. We also had a nice spread there, coffee, tea and cookies, and we acted friendly and engaged in discussions; every topic was acceptable. It is terribly interesting to me that 25 years later a professor in electrical engineering told me, a Berkeley professor actually, that he developed a great admiration for me because we were willing to listen and we didn't turn away from them. He said, "I understand that we were obnoxious," [laughter] "but you listened and you didn't close the doors. That was a great thing to do." Well, I didn't do it because it was a great thing.

During the same time I was also chairman of my academic department here at Berkeley, and let me tell you, all my time went into dealing with students. I used the same

technique. I never threw them out or chased them away. I told them that normal lecture and laboratory periods are sacred; they are not to disturb that; I don't tolerate that. "On the other hand," I said, "I am open. My colleagues and I are open for discussions every morning at 6:30 a.m. in the student cafeteria; we have a room reserved. You get free breakfast if you come." This went on for several weeks, and eventually I wore them down. [laughter] At first several of them came, maybe a dozen. By the time two weeks had passed, they stopped coming, but they couldn't accuse me of chasing them away. This happened in a different form in the Society and so I thought of that, and that was the correct approach.

BOHNING: Well, Berkeley was certainly one of the hotbeds of student unrest at that time.

TOBIAS: Oh yes. Let me assure you it was not a time I recall with pleasure. No matter what these people claim, I don't think they accomplished a thing.

Another achievement on the social side was that we always had a banquet after the Acheson medal. There was always a Tuesday evening banquet where we put on dinner jackets—there was a cocktail hour back then—and afterwards the cream of the Society, about fifty or sixty people, gathered in the president's private suite. The president was in a suite that the Society provided, and there were drinks of various sorts. I remember there was never a good French cognac. [laughter] But there was scotch and gin and bourbon. There was a total absence of young people and the general membership was of course not included. I was instrumental in changing this completely. First of all, I said, "Who the hell wants to get together to drink after you already had a cocktail hour and the dinner. You don't want to drink again. Let's have it the next day, Wednesday evening, and it should be to honor all the people who worked for the good of the Society. That means all divisional officers, all members of standing committees, and of course the Board and officers." I'm talking about standing committees, and this is a pretty good crowd. We are talking about maybe a hundred people, but I thought, "These are the people you want to invite to have a drink." I also demanded that there should always be a French cognac offered. I must say, much to my chagrin, although this custom was continued for a number of years, I don't think they still have it. This I thought was a change in relating to membership and relating to people who do the work for the Society. It also helped for people to realize that what they do is appreciated. It is noticed, and I think that's the way it should be. On one hand I insisted that the Society be run in a more business-like way, and on the other hand, we wanted to make sure that people's services were recognized. I think that's good stuff. I will repeat again and again that all this was not done by me alone, but these are issues I feel very strongly about and I was heavily involved. Whether I suggested it first or not is really not critical. If somebody claims otherwise, I shall be happy to yield. But I was a heavy factor for them; I won't yield that.

Another area that I was very strong about was attracting students to the Society. I forget when we introduced the student membership. It involved payment of a modest fee which I insisted on. They should pay, but in a very modest amount, maybe ten dollars. This

is a way you recruit future members, and it has always been a way that operated for the Society. All my graduate students were always members. Advanced graduate students presented their thesis work at meetings, and many of them became very active in the Society. Two of my former students were presidents, and at least twenty were officers of various sorts at the divisional level. I still think the Society offers an excellent forum for young people, because it is small enough to respond to individual contributions. You don't feel overwhelmed by the superstructure, by layers of experienced and older people who won't let you speak or won't let you do what you would love to do.

Of course I mentioned by the time I was president, the Society had its own separate offices. This was accomplished before I became president and eventually they purchased a house in Pennington, New Jersey, and it is a very efficient and productive operation. I think that if you consider the publication activities of the Society, it's amazing what they put out with the number of staff they have.

Why did I want to be president? Well, maybe I don't have to say that. After all, being a president is not such a great prominence. You work hard and the recognition you get is very localized, but when one becomes involved, it is a normal thing to happen. Problems—well, I mentioned the problem of the activists. Towards the end of my service on the Board—don't forget that I had a total of eight years of service on the Board: two years as divisional officer and four years as vice-president and president, and then two years after I was president; I became quite experienced. Running the affairs of the Society through the Technical Committee, which is responsible for meetings and coordination of affairs between divisions, served to identify and to solve problems. It served to compare agendas and timing. If somebody wanted to generate a symposium at the same time as somebody else with the same topic, they should get together and offer it jointly, et cetera, et cetera. Plus, the final body before the vote was the Ways and Means committee, and that was a very well-functioning body under the chairmanship of the senior vice-president. Some members of the Ways and Means Committee were officers, others were named from the general membership.

[END OF TAPE, SIDE 7]

TOBIAS: Speaking of relationships of the Society to other organizations, we were never deeply involved with others. We maintained correct relationships but never anything intense. Of course everybody belonged to the American Chemical Society, I did and still do, and the American Institute of Chemical Engineers, but my major activity was always in the Electrochemical Society. I can't say anything special on this.

BOHNING: The Electrochemical Society grew out of people who were dissatisfied with the ACS.

TOBIAS: I wasn't around [laughs] but that could very well be true. Of course, the ACS meetings are enormous, and it's really many meetings running in parallel; it's overwhelming. The nice thing about the Electrochemical Society is that even today, it's a manageable size.

As far as our relationship with industrial sponsors, I don't know anything very special about sponsors. Industry had a very good standing in the Society, and a good technical journal was published. They had sustaining memberships who were sponsors who provided up to a thousand dollars a year—I didn't think that was so great—but industrial sponsorship came more through allowing members to spend significant amounts of their professional time on Society affairs. This may be the correct place to mention the significance of Bell Laboratories in the history of the Society.

It was a highly positive role. Bell provided key people in the modern history of the Society, perhaps starting with Robert Burns and [N.] Bruce Hannay, who was one of the directors of chemical research at Bell. Not only did they contribute massively themselves, but Hannay was also president and Burns was involved with changing from the *Transactions* to the *Journal*. He encouraged the younger members of the staff to be involved. Such people as [Paul] Milner and [Dennis] Turner have done tremendous service to the Society. I would say that the reason the Society has a first-rate program in electronics and dielectrics is largely due to the massive participation by the staff of Bell Laboratories, which after all used to be, without any question, the greatest scientific laboratory in the world. I think the Society gained enormously by their massive participation, and I am very grateful for that.

Concerning the relationship between the academic and industrial contingents, speaking about the "wet side" now, of course there was a Theoretical Division where supposedly the eggheads participated. [laughter]

Then there were the divisions that were technological, and that was more widely industrial, originally more people involved with sales than with technology, but that changed gradually. During the 1960s and 1970s there was a significant shift in all these divisions, towards doing their own so-called theory, their own research reports, and their own science. I had a major role in changing the name of what is today the Physical Electrochemical Division. I was one of those who agitated for changing the name from Theoretical Division to something else. Some of the old-timers, like Walter Hamer, absolutely didn't want that, because it was a sacred name—it was a name from their youth—but I said, "It's not honest." First, most of the work presented in the Theoretical Division is not theoretical; it's experimental. Second, they don't cover the ground. Divisions have changed; they do their own theory. Then, during one of these lunch-business meetings, a few years back, there were suggestions from the floor about changing the name and I suggested the name Physical Electrochemistry. It's an honest name; it's exactly what the division does. Well, lo and behold, they adopted the name. [laughter] Can you stop for a moment?

BOHNING: Certainly. [short break]

TOBIAS: Relations between divisions through this committee structure were very favorably influenced by the establishment and hard work of technical committees which coordinated the programs between divisions so that conflicts were resolved early, before they had a chance to develop. Also, in more recent years it has become easier to create new working groups and new divisions. Earlier, the attitude was that the divisions of the Society were cast in iron and there was no need to change anything, but now that is not so.

How has the Society contributed or reacted to significant scientific or technological events in electrochemistry and related fields? Well, the Society provides a forum for the presentation and exchange of scientific ideas. People engaged in the pursuit of knowledge are able to meet each other, and this personal contact is indeed a very critical item in my eyes. Providing a scientific forum and opportunity for human contact is very important.

Secondly, through the publication of a first-rate journal; it influences the development of science and technology, and the publication of a series of monographs and the proceedings of symposia serves a very key purpose. I would say that these are the key reasons that justify the existence of the Society.

There are not many extraneous secondary goals or pursuits that I see, and the Society satisfies the most puritanical judgment in this regard.

BOHNING: You mentioned the Journal, and there was a question I wanted to ask you about it earlier. You were the editor, one of the editors there, for many many years.

TOBIAS: Yes.

BOHNING: Forty years?

TOBIAS: Thirty-four years.

BOHNING: What kind of rejection rate did you have? What was the quality of the papers that you were getting?

TOBIAS: Our rejection rate was not huge. I can't give you a quantitative figure. The way we worked was that papers were rejected through Norman Hackerman. First of all, if a paper

was clearly out of line and hopeless, I sent it to Hackerman, telling him to reject it. It was good to have his prestige and to avoid the hassle from the divisional editors. If a paper was approved I would deal directly with the author, and in most cases they would cooperate. If they didn't then I threw up my arms and sent the paper to Hackerman and said, "My feelings would not be hurt if you publish this, but I don't think it should be published. I can't give you a firm figure of the rejection rate, but it wasn't huge. I don't believe that it is an index by which you can judge a journal, because there is also the question of who are the type of people submitting papers. The *Journal of the American Chemical Society* traditionally has a huge flow of papers, quite a large fraction of which should not be published, but I would not make a judgment based on that.

I think the Society is responsive because its size is manageable. Whether we deserve it or not, we have an excellent professional crew in Pennington, people who served us with distinction and are very productive. We get an awful lot out of them for the money we pay them. I have always been impressed with them, and I think they deserve our gratitude. The Society is responsive to the members' needs. The road from conception to achievement is not very long, and it is not full of hindrances. If you want to do something, you will do it sooner than you hope; we will just start to do it. I think the Society fulfills its obligation to members and to the scientific public by publishing a very good journal and by arranging scientific meetings as a service to the community.

The positive changes I think we have covered. I am not aware of negative changes. Yes, we are somewhat larger than we used to be, but not that much larger. I am not sure how big the Society was when I was president, but I don't think we were so much smaller, maybe twenty or thirty percent smaller.

BOHNING: Did you travel a lot when you were president?

TOBIAS: Our agreement was to visit local sections. The local section programs are, you might say, questionable in effectiveness. On the other hand, as long as members want to maintain them, why not? For some people who don't get to travel a lot it is still a scientific forum, an opportunity to meet people involved in the same technology, and I think as long as they want to do it, it should be encouraged.

Question 15 (11) how were my career and contributions to science influenced by meetings of the Society and my contacts with its members? Well, it was a major forum for the interface between me and the scientific and technological fields. I got to know a lot of people, and for most of the people I had very high regard. Even if they were not the greatest scientists they were certainly intelligent, interesting, and capable people, and I didn't meet a lot of people whom I disliked. Maybe if I worked hard at it I could discover some whom I disliked.

BOHNING: From the very early days, you used the Society as a forum for promoting electrochemical engineering. Could you comment on how you were first assigned to the Friday afternoon sessions and then moved your way up?

TOBIAS: Oh, yes. They didn't know what to do with the scheduled topic and even today you will find that unusual topics which don't seem to fit with anything dealt with before are pushed to the later part of the week. By Friday afternoons people are usually already gone. So for a few years my papers were scheduled for late in the week but it didn't bother me. Many times only ten or twelve people were there, but my feelings were not hurt. Later on this changed, especially when the papers we gave represented the various divisions which were dealing more with technology. I didn't see any personal angle or neglect in scheduling these topics for late in the week. I didn't think, "They don't want me or what not." I always tell this to my students, "Don't assume that people are out to do you in."

One of the interesting questions, which also sort of started during the period when I was involved in the Society leadership, was where we should meet. Originally, most of the meetings were either on the East coast or in the midwest. Some of these cities, their names shall not be mentioned, are pretty dismal. It was no fun to be there for a whole week. There wasn't much to do and if there was something to do in the evenings, it was depressing.

I suggested, "Why don't we meet in some interesting places like Hawaii," [laughter] "Florida, Puerto Rico, or Las Vegas." Hah! "My company would not support anybody's travel to these places." Well, let me just tell you, time has passed and our best attended meetings are in Hawaii, California, Las Vegas, and Florida, while our worst attended meetings are in the midwest. I can't speak for New York, but the last meeting there was not that well attended. We shall have a meeting in Paris.

BOHNING: Mmm.

TOBIAS: A joint meeting with the International Society of Electrochemistry.

What should the Society do in the future? I think more of the same good stuff. It should retain its vigor and its flexibility and should accommodate new technologies and new knowledge. It should continue to encourage young people to fill its ranks. It's not important what fifty and sixty year old members do; it's important what thirty year old and twenty-five year old members do. As long as the Society remains an attractive forum for advances in science and technology, it will be very successful.

I believe that the future of electrochemistry is very bright indeed. After all, electrical phenomena involving chemical changes are fundamental to our existence. Not only do we

produce some essential materials by electrolysis, but the storage and conversion of energy occurs via electrochemical means. I don't see on the horizon any other concept that promises to replace it. I think there are exciting developments in the solid state end and exciting developments that tie the wet ends and solid state electrochemistry together. I should mention that polymeric materials can be doped to become good electrical conductors, or you can make out of them a transistor. You can graft transistors onto a polymer and I suspect that eventually we will see entire microcircuits with chips made on a polymer molecule.

I think electric phenomena and electrochemical phenomena will remain critical and will contribute important items to human welfare and civilization in the coming decades and maybe hundred years. Who would dare to predict anything beyond that? [laughter] I will only predict for fifty years. I don't know what's going to happen after that. Well, you have listened to a lot of material now. I could augment some remarks or cover some things that you think I didn't talk enough about?

BOHNING: I think we've covered the Society aspects pretty carefully. I don't think there's anything in there that we would need to go back and add to at this point. One of the things I did want to ask you about was the book series that you began editing with Paul Delahay.

TOBIAS: Yes.

BOHNING: That was not through the Society, was it?

TOBIAS: I forget whether it was or not. I think we tried to do it through the Society but eventually we ended up doing it outside with the same publisher. It was Paul Delahay who approached me and then eventually became impatient because others wouldn't donate their own time. He in fact ditched electrochemistry completely. Then I ended up with Heinz Gerischer, a very distinguished German scientist I regard as the best electrochemist until his death just this last Fall. Gerischer and I eventually continued with a different publisher—VCH publishers. Originally, we were encouraged to publish this series by Carl Wagner, and in fact he wrote an introductory statement for the first volume. I think we maintained a reasonable standard. We did not try to provide complete coverage but tried to select authors very carefully for various areas.

BOHNING: That first volume was 1961.

TOBIAS: Yes.

BOHNING: It was titled *Advances in Electrochemistry and Electrochemical Engineering* (12), but the term electrochemical engineering was not a common term at that time, was it?

TOBIAS: Well, not really, although it is a logical thing. It's the chemical engineering, not the electrochemical processes. I may have pushed the name. I can't swear to that. I think the article that Carl Wagner wrote concerned what electrochemical engineering was. I may have given him that question or he may have suggested it. I can't tell you.

I will mention here, since you bring in other angles, that I was also active, and in the 1970s I became quite active, in the International Society of Electrochemistry, which has a seat in Europe. It was started by scientists in Belgium, in France, in Britain, and somebody in Switzerland in the late 1940, early 1950 period. It doesn't have a solid-state component, only electrochemistry. I started to go to their meetings in the 1960s and eventually was nominated for the vice-presidency and for president. I was president for two years in 1977-1978. It had a fairly large American membership. The whole membership is only about 700, because it grew out of a private club, the International Committee of Electrochemical Thermodynamics and Kinetics. That was the original name. The name actually is in French but I don't want to put that down.

[END OF TAPE, SIDE 8]

BOHNING: Is there or has there been much of an international flavor to the Electrochemical Society?

TOBIAS: It has a significant international membership, especially Japanese. I wouldn't be surprised if at least 10 percent of the membership were to be foreign. Our Society puts out a really fine journal with page number well in excess of two thousand for a membership fee of ninety dollars, and it's a bargain if you consider what you pay for books nowadays. In addition to the Journal, members have the privilege of attending meetings at a very reasonable cost, so the meetings have always been attended by foreign people. I was actually really surprised when I looked at what a huge Japanese membership we have. It's remarkable.

BOHNING: That was not the case when you joined back in 1948.

TOBIAS: I can't tell you what the foreign membership was because I was totally unaware of that. We started to put out a printed membership list much later than that. I imagine there

was a foreign membership; in fact, there was a foreign membership right away. I think Fritz Haber was a member, and maybe Walther Nernst was a member also. I think that recently foreign membership has grown significantly, and that is primarily due to the solid state side of the Society, not so much the wet side.

BOHNING: Okay.

TOBIAS: I would be surprised if it were otherwise.

BOHNING: I wanted to ask you about the development of electrochemical engineering. I've been hinting at the fact that you coined the term and developed the first course in electrochemical engineering. Is that correct?

TOBIAS: The first course called by that name. I can't vouch for it because I didn't go through the course offerings as far back as the 1920s of MIT and Wisconsin, which both had electrochemical people on their faculty. There was Burgess at Wisconsin and Thompson at MIT. There was a division of applied electrochemistry at MIT, but I don't know what they called their courses. Certainly, as evidenced by the writings I have seen, it didn't satisfy my criterion for what engineering is about, which after all is a quantitative science that deals with realization, scaling-up and optimization of processes and devices. Mathematics is a key tool in this, and descriptive angles take a minor role, sometimes I feel even too little a role. It's an extreme on the other side of the issue, so to speak. I mean, you get on the horse and fall off the other side. By the time this whole field developed validity and recognition, there were a lot of other people besides myself. I don't even claim any leadership. I mentioned Norbert Ibl, and John Newman. They contributed massively and became very well known. Other names that come to mind include Richard Alkire of the University of Illinois, Huk-Yuk Chek of Columbia, and Douglas Bennion of UCLA, and later Brigham Young. I shall omit mention of a few Japanese and European academics who made significant contributions to the engineering science aspects of electrochemistry.

In America I have to claim that all those who became important contributors were my students, or their students, but that is almost by default because they couldn't be somebody else's. [laughter] We were an engineering department born in the womb of a chemistry department, moreover an exquisite first-rate chemistry department, perhaps the best in the world in physical chemistry. We had a tremendous advantage right away; because of the special relationship to chemistry we were perhaps more receptive to dealing with areas besides the traditional areas of American chemical engineering, namely the petroleum and chemical processing industries. That is how this development was possible here, plus the fact that of course there was personal interest in it by Latimer, although of course this was

only the very beginning. He died in 1956, much to my sorrow, but the initial encouragement he gave me was very important.

BOHNING: How long did it take industry to sit up and take notice of what you were doing?

TOBIAS: They noticed me, if you judge by the consulting contacts I had throughout my life. I always had two or three companies I would consult for, but my involvement in consulting activities was not intense. I don't believe that a university professor should have the time to spend as much as one day a week on consulting. Today that is regarded as the norm, and I think that's too much, although I did learn a great deal from talking to industrial people and I've seen a large variety of industries. Whatever influence I may have had on them is questionable. I think the influence you can have is much slower than people would expect because changing something in industry is not easy. You don't just redesign a cell where you have many millions of dollars worth of capital invested producing goods at a reasonable cost. You are not going to just demolish this and put in a brilliant new conception. Therefore the influence is slow, but it's evident because the students they hire will be much better educated in these areas dealing with electrochemical processes. I consider raising the level of knowledge and the capability of students to deal with these problems involving electric fields in addition to chemical changes to be a very important influence. I think that it's a change that has occurred quite massively.

BOHNING: I know you consulted with Dow for a long time, and you mentioned the problem of secrecy.

TOBIAS: Yes.

BOHNING: Were other companies that way too? How do you deal with this in a consulting relationship?

TOBIAS: Of course a consultant cannot break down secrecy. I always took the position of, "They tell me whatever they want to tell me," and I never pushed for more, because if I asked a question and the answer was evasive, then I said, "That's not a topic we shall discuss."

At Dow Chemical I had some very funny experiences. My good friend Dick Bechtold, with whom I had many technical discussions which interested him and vice versa, was at the Pittsburgh plant. I visited every once in a while, and one day he had to leave me in an office. He told me he would return in twenty minutes. In between, one of the younger engineers took me through the cell room where they were assembling the Dow chlorine cell,

and I was not supposed to see that. [laughter] Dick was quite upset that this mistake was made. I said, "Dick, I didn't look, and even if I looked I wouldn't tell." [laughter]

Actually, I consulted for Dow Midland only. I was there once, and once I was in Freeport. Then I consulted for the research lab here in Walnut Creek for several years on their sodium sulfur battery development, which was quite an ingenious idea. They did very respectable development on it but unfortunately it fizzled at the end because of enormous material problems.

BOHNING: You mentioned yesterday that you had also been in Europe. What were you doing over there?

TOBIAS: Dick Bechtold was at that time assigned to Europe, and he arranged for me to be there and do something useful for Dow, for about six to eight weeks. My assignment was to travel around Europe to the major sources of manpower for Dow and prepare reports on special problems, how to interface with the schools, and what type of staffing problems they might encounter. Don't forget, the American chemical engineer is a different quantity than a German, vastly different. German industry doesn't operate like we do. In the laboratory the chemist is the boss, and the engineer assigned to him is not what you might call a chemical engineer but a process engineer, and he is subjugated to the chemist's leadership. This is how I.G. Farben always operated. They provided me with a rented car and my wife and I just went around and visited twelve, maybe fourteen universities in Germany, Holland, and Belgium. We didn't go to France. I prepared several reports on each of these places and on the personalities with whom I established contacts. I figured out who would be suitable for them to get in contact with if they had personnel problems or wanted something done in the University laboratories. It was nice. We put our children into a kindergarten in southern Switzerland and they were there during the time we travelled around. Of course I visited a great deal of the universities there and became acquainted with their work very well.

BOHNING: You had mentioned Hirschkind earlier, and I'm wondering whether you want to talk a little more about him.

TOBIAS: Well, I didn't know him very well. It was not very much before his death but he visited me and was very friendly. Actually, he asked me to do him the favor of checking out from the library some old German journals which printed Fritz Haber's speeches at the end of the war, already 1920, 1921 and 1922 (13). I read them and found, amazingly, that he was as I had said, a superpatriot, and he had a major role in Germany's gas warfare. Anyway, he told me just a few historic notes about his work on thermodynamics. He was an interesting man and must have had a very interesting life. It's too bad that I really only got acquainted with him well after I came out here, maybe as much as ten years after.

BOHNING: What other companies did you consult with?

TOBIAS: I have to refer you to my summary. It has it in it, it's a long list. It's not here but you have my summary (14).

BOHNING: Yes.

TOBIAS: There is a big list there, and it is grouped according to whether it was a single occasion, one or two occasions, or a longer relationship. It is a long list.

BOHNING: Okay.

TOBIAS: But it was never a very intense relationship. I never did any work in the University for them. I would never do that, and I never did a once a week thing, et cetera. No no, that's too much. I would say not even twice a month. I doubt that I spent 26 days on consulting to industry in any given year. I doubt that I did that. I believe that people who do a significant amount of consulting are engaging in dereliction of duty, because I know darn well that it takes all the time you can muster to be a decent teacher, prepare for your classes, guide your graduate students, and write proposals. You have to chase money more and more today. It is a very corruptive influence on young people and it spoils life very much in universities.

BOHNING: You were there when the chemical engineering department became its own entity and split from the chemistry department.

TOBIAS: Yes.

TOBIAS: That was in 1953, I believe. I can't be absolutely sure.

BOHNING: What were the circumstances surrounding that?

TOBIAS: Well, we first were not even a division, and then around 1950 we became a division. There was a Department of Chemistry and a Division of Chemical Engineering. By this time our leaders, including Ted Vermeulen and Charles Wilke, were agitating quite seriously to have two departments in the college—Chemistry as one and Chemical Engineering as the other. I think it had a political significance because we needed accreditation by the engineering departments, just like the science departments need to be accredited. As long as we were not even a department this thing didn't work. At Berkeley there was a very special situation because the College of Engineering started a competitive program.

BOHNING: Really?

TOBIAS: They called it process engineering. There were quite a few tense years during which we were in danger of going under, but fortunately our dean was much more powerful, and Chemistry had a much better credit line than the College of Engineering. They also made the mistake of hiring some not quite first-rate people, and so they were the ones who went under. But I have to give credit to my colleagues Charles Wilke and Theodore [Vermeulen] for fighting the battle.

BOHNING: How big a group was it when you joined in 1947?

BOHNING: When I joined there were only two people there, Charles Wilke and Leroy Bromley. I joined along with Ted Vermeulen and Don Hanson. The two who were there came the year before, and so there were five of us. That's when the program could really get started and we grew slowly. We added two, three or maybe four people in the next ten years. Eventually we ended up with 22. Because of the recent severe financial pressures in the state university system, we have lost some. I believe currently we are back at 18 full-time academic positions.

I am sure that it takes quite a lot of ingenuity to organize this into a sequence, because it goes in and out of topics, returns to it somewhere else, and it's not necessarily true that they are mentioned again.

BOHNING: Yes. It's meant to be a conversation with all of the things that go on in a conversation. We have followed a chronological sequence for the most part and that's usually the best way to approach it, but there are times when other things are prompted later on.

TOBIAS: One thing that I would like to come back to as sort of a concluding statement is the following. If I tried to pinpoint what I consider the most major contribution I made to the development of this field, it is that I was successful in influencing a fairly large number of talented young people working in the field, and I passed on some enthusiasm toward it. There I give myself credit; I was a good salesman. I got some people really interested by firing them up, by giving them colorful, maybe sometimes even exaggerated, accounts as to what there is "in them thar hills." The gold that there is. I think it's one of the roles of a teacher, to inspire other people to do good work. I think in that category I would not rank myself very low. Nineteen of my graduate student collaborators made teaching chemical engineering their career choice. Fourteen of them are still so engaged.

In areas of research there were some things that I did with my students which I regard well. In preparatory electrochemistry, new process development, we did some lucky things. Among other things we were sort of the discoverers of the most important solvent used in making lithium batteries: propylene carbonate, ethylene carbonate and gamma butanol electrodes, et cetera. Oh, we did that work in 1962. My sin is that we didn't publish it; it only came out as a report.

[END OF TAPE, SIDE 9]

TOBIAS: Harris did this very good work with me. When he wrote his thesis I told him, "Now let's write a paper. Give me the rough draft, and I shall work on it, and then you go and present it at such and such a meeting of the Society." He just said, "No, I don't want to do that." I said, "Now why don't you just want to try?" He became adamant and even shed some tears in my office. I don't remember any of my students ever crying in my office, including the females. He did.

BOHNING: Hmm.

TOBIAS: And he never did that. He was a fantastically talented preparative chemist. He moved in the laboratory with great skill, speed and intuition.

Well, we did some good preparatory electrochemistry in addition to our major engagement in investigations on scale dependent processes: mass transport and charge transport and current distribution. We had introduced the use of computer computations; this was done really early in the fifties, thereby making mass balances, and current distributions in complex geometries tractable.

BOHNING: You started doing that in the mid 1950s.

TOBIAS: We had access to computer machinery through the Lawrence Laboratory. Some of my students became experts. I had an undergraduate student who was a computer operator in Livermore, and they had the world's largest computer facility by a long stretch. He would use it at off times, at two o'clock in the morning, to run these programs. Those were the times when you had to be with the computer when it was running. We did, by today's standards, a very simple problem, but at that time you couldn't have done it analytically, and there was no way to do it numerically other than by a digital computer.

BOHNING: You've also described, in that article, that in the early days you had two parallel paths of research because you were in a chemistry department.

TOBIAS: Well, I wouldn't say that I consciously or shrewdly did so, but it undoubtedly influenced my position as to what I should do. Also, don't forget that I was not in the position right away to formulate good quantitative questions in what you might call the engineering area, because I didn't know enough about what the questions were—the overall questions—how big, how wide, how fast, et cetera. You don't start out with this. You need more closely defined problems, but on the other hand, in the preparative electrochemical area, I could see some very interesting questions emerging where with some engineering knowledge, we could move. I think it worked out to a certain degree. We spent quite a bit of time in the 1950s and 1960s working with ionizing organic solvents especially. We had some interesting results. We also worked in ammonia as an electrolyte, but the major thing remained what you might solve in engineering studies involving transport phenomena. My work on the physics of electrolytic gas evolution, and the introduction of micromosaic electrode surfaces will prove to have been among my most productive investigations.

BOHNING: In your Acheson medal address (15) you made the following comment, "The role of electrochemical engineers is to bridge the gap between scientific discoveries and economic reality."

TOBIAS: Yes. The main issue is that a chemist who achieves something in the laboratory is not really the one to evaluate: what will this cost? Even if he is aware of what the raw materials cost, to realize this chemical transformation on a large scale is a bigger, complex issue. An engineer is also a scientist, but his concerns are economic constraints. Putting together the process involves a good many steps which are not electrochemical. It is vastly different to do something in a little beaker and to do it in a cell that is as big as this house. In fact, the issue arises of how big should a cell be. That's where we suddenly found, at least in the early days, a total vacuum; there were no answers to such questions. The question wasn't even asked in the literature. As I said, when you go to the classroom and teach a subject and you don't even have elementary answers to the questions you raise, you start to work on

them. Then after you find that they're not in the literature, you ask, "Now what am I going to do?" Of course you're not going to do all of it, but you are able to pass on and inspire other people. This, to a significant degree, did happen.

BOHNING: Was it true that in the early days, if you had one cell producing x amount and you wanted two x , you just put in two cells?

TOBIAS: Yes, exactly. Now that's pretty primitive, wouldn't you say? Electrochemical processes had large footprints. It was more like an agriculture operation. You talked in terms of the acres of cells. Well, there should be a better way to do it, but it's not easy to tell how it should be done differently. It's not trivial, because it had been decades or a hundred years since those cells started operation. They were developed very skillfully, very cleverly, and it's not simply, "Move over and I'll tell you how it should be." No, no, not at all.

BOHNING: One last aspect, and this comes back to your Acheson medal address and has shown up in a lot of people with whom I've talked. You said, "All along in the past, imagination, intuition, judgment and invention are made the essential starting sparks on which progress will be based (15)." What I wanted to focus on is the intuition aspect. I've had people say that one of the things that takes part in their making a decision is their gut feeling. I'm wondering if you could comment on that.

TOBIAS: Engineering in itself is a scientific endeavor where you use the maximum degree of available quantitative methods. But that's not enough, because the conception, the idea, the essential route to what you want accomplished, you can do on the back of an envelope. That is what I point to, the intuitive approach. I am not looking down on these so-called empiricists. They are wonderful; they have done great things. They are by no means inferior to the brilliant engineers. The problem is, how do you pass on ingenuity? You don't; there is no way. What it amounts to is that you need both. It's interesting that this caught your eye.

BOHNING: It's come up in other discussions.

TOBIAS: Oh really.

BOHNING: As you said, it's not something you can discuss in the classroom setting or train people to do.

TOBIAS: Although we tried to in our departments. I mentioned to you that I developed a course together with my colleagues Scott Lynn and Judd King that emphasized judgmental aspects. We sort of invented things or were put in a position to have to invent something. The very best students who got A pluses in mathematics courses suddenly felt very uneasy because the methodology, the answers, the rote answers, were not there. They had to invent something and use their imagination, and not everybody has imagination. It's rare that you have all these elements come together in a single person. There are some great scientists who had it, but in most cases, one or the other dominates. It's an over simplification, but you rarely have a great experimentalist and great theoretician in one person. Very rare. It has happened; Fermi is said to have been one of these rare individuals.

Well, I greatly enjoyed meeting you.

BOHNING: I appreciate your time.

TOBIAS: I found some of the questions you raised interesting, and of course I learned from them. I must say that I didn't have answers to some of the questions or I wish that I had given you a different answer, but by the time I would push out a reasoned answer, it would lose some spontaneity, and maybe some validity also. [laughter]

BOHNING: In The Electrochemical Society case we have this agenda, but generally I don't have a fixed set of questions that I send somebody in advance (16).

TOBIAS: Well, some of these questions might be said to overlap in a significant way, but I think it's not a bad idea to give people a conception of what they should be thinking about. I was sorry that I didn't have this, but you say that it was all right how we spent the first days.

BOHNING: It was fine, because that's exactly what I wanted to go through, to cover that territory.

TOBIAS: Now, may I ask you what will be the continuation of your endeavor for the Electrochemical Society?

BOHNING: Well, that's sort of directed by them.

TOBIAS: I doubt that you will interview every president; it's a huge number.

BOHNING: That's correct. I've already talked to [N.] Bruce Hannay, but that was in the context of the Perkin Medal (17). I said to the Electrochemical Society I will be talking to him, and some of the ECS activities came up, but they did not want him on this agenda at this point. The executive director and the Society are the ones who are calling the shots in terms of whom we'll talk to. They knew that I was going to be speaking to you anyway so that's why I put in this agenda here. As to Harold J. Read, I made a special trip to Florida to talk to him (18).

TOBIAS: Do they have to pay?

BOHNING: Yes. They make a contribution to the [Chemical Heritage] Foundation.

TOBIAS: Depending on each case? I would like to tell you that there are these Bell people whom I mentioned to you; they did fantastic service for the Society. One of them is Paul Milner.

BOHNING: I know Paul.

TOBIAS: Oh, you do. The other one is Turner.

BOHNING: Okay.

TOBIAS: They both have been secretaries of the Society for many years, and they are very familiar with the historic aspects and personalities, who did what and when, and all that. I just want to mention to you that they are a great source of background.

BOHNING: All of this material will be used by them in some fashion as they work towards their centennial in 2002. They're already setting up for that, seven years in the future.

TOBIAS: Well, it has to be. Time moves all too fast, you know.

BOHNING: Well, I'd like to thank you again for spending these two afternoons with me.

TOBIAS: Well, it was a pleasure and I hope it will be of some use.

BOHNING: Oh it will be, definitely.

[END OF TAPE, SIDE 10]

[END OF INTERVIEW]

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