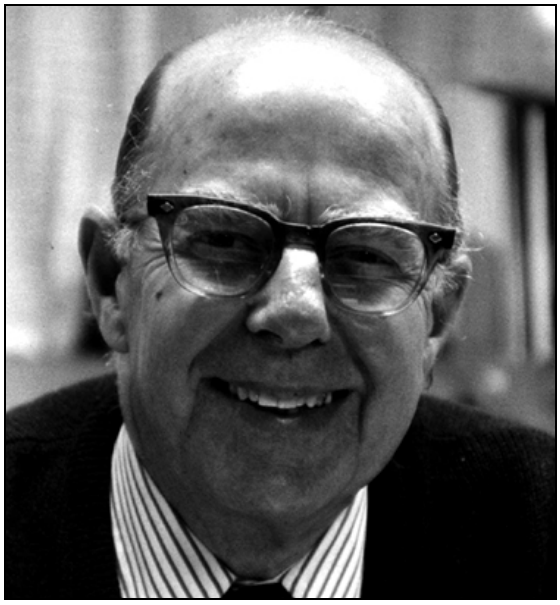

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NICHOLAS W. TSCHOEGL
(Born 1918)

INTERVIEWED BY
SHIRLEY K. COHEN

April – June 2001

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Engineering, chemical engineering

Abstract

An interview in five sessions, April-June 2001, with Nicholas W. Tschoegl, professor of chemical engineering, emeritus, in the Division of Chemistry and Chemical Engineering. Dr. Tschoegl, a native of Moravia, recalls his French/Austrian background, World War II service in Hungarian Army, and postwar control of Hungary by the Communists. Leaves Hungary in October 1948, via Austria, arrving in Australia 1949. BSc from New South Wales University of Technology; PhD in rheology with A. E. Alexander, University of New South Wales, 1958. Discusses his work on dough rheology, Bread Research Institute of Australia.

Work with J. D. Ferry, University of Wisconsin, 1961-1963, on polymers. Stanford Research Institute, 1963-1965. Joins Caltech faculty, 1965, as associate professor of materials science in engineering division. Works on solid propellants for rockets, funded by air force. In 1967, becomes professor of chemical engineering in Division of Chemistry and Chemical Engineering. Discusses polymers and synthetic rubber. Recalls visiting professorships: Delft; Gutenberg

University, Mainz; Imperial College, London; Centre de Recherches sur les Macromolécules, Strasbourg; ETH, Zurich.

Discusses block copolymers and spectral functions; time-dependent properties of polymers; WLF [Williams-Landel-Ferry] equation to block copolymers and other multitransition systems; development of FMT [Fillers-Moonan-Tschoegl] equation. Formation of International Congress and International Committee on Rheology. Recalls Caltech's interaction with Soviet scientists and subsequent estrangement, mid-1980s.

Discusses managing Caltech's Watson lectures; post-retirement visiting professorship at University of Ljubljana, with Igor Emri; Emri's work at Caltech with Wolfgang Knauss; founding of journal *Mechanics of Time-Dependent Materials.* Discusses his two books: *The Phenomenological Theory of Linear Viscoelastic Behavior* and *Fundamentals of Equilibrium and Steady-State Thermodynamics.* Recalls consultancies: with JPL's polymer section, 1960s; Phillips Petroleum, 1968-1983; Firestone Tire & Rubber, 1974-1980; Naval Weapons Center. Comments on colleagues and past Caltech presidents; recalls death of chemical engineer and Caltech vice president William Corcoran. Tschoegl concludes interview by listing his memberships in professional societies and other professional activities.

Administrative information

Access

The interview is unrestricted.

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INTERVIEW WITH NICHOLAS W. TSCHOEGL

BY SHIRLEY K. COHEN

PASADENA, CALIFORNIA

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ORAL HISTORY PROJECT

Interview with Nicholas W. Tschoegl **by Shirley K. Cohen**

Pasadena, California

Session 1	April 25, 2001
Session 2	May 2, 2001
Session 3	May 30, 2001
Session 4	June 8, 2001
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COHEN: You're going to tell us about your childhood. That's a good starting point.

TSCHOEGL: Well, I was born in 1918 in Moravia, which was then a province of the Austro-Hungarian Monarchy. The Monarchy collapsed about two months after I was born; however, my birth had nothing to do with it.

COHEN: Moravia is part of what country today?

TSCHOEGL: Today it is part of the Czech Republic. The Czech Republic has two major constituents, Bohemia and Moravia. The capital of Bohemia, and of the Republic, is Prague—Prag in German, Praha in Czech. Moravia's capital is Brünn in German and Brno in Czech. The Austro-Hungarian Monarchy was a multiethnic, multilingual entity; hence place names usually come in several flavors, so to speak. So that's where I was born.

My maternal family, the Roberts, is of French origin—the name is pronounced *RobAIR* in French and *RAWbairt* in German. My great-great-grandfather, Florent Robert, was born in 1795 in a small town called Izeron in the Dauphinée. Members of my maternal family left France

[*] Throughout, the interview contains corrections and much additional material inserted later by Dr. Tschoegl and will thus not exactly conform to what is on the tapes. —ed.

during the later stages of the French Revolution, because of the terror under Robespierre. Florent was taken to Bavaria as a young boy by two of his adult French cousins. He grew up there, married, and later moved to Moravia, in Austria, where he spent the rest of his life.

We are conscious of our French origin. I sent my two sons to France for a while, to learn to speak the language fluently. We have plenty of French cousins and relatives with whom we keep in touch, even though the French and Austrian branches separated 200 years ago.

Florent Robert was an outstanding individual. When he grew up there, industrialization was taking off in the Austro-Hungarian Monarchy, and he became a leading entrepreneur. He founded several industrial and commercial enterprises. There used to be a Robertgasse—Robert Street—in Vienna, named after a Robert Bank that once stood there. One strong interest of Florent's was the sugar industry. He founded the Robert sugar factory in Gross-Seelowitz—Židlochovice in Czech—where the family grew sugar beets, among other crops. The little township due south of Brünn became the family seat of the Austrian Roberts. Florent Robert was also something of an inventor. He designed an evaporator for thickening and enriching the sugar extract. When I had to pass an exam in machine design at the Technical University in Budapest, the professor asked me to describe the Robert evaporator. I said, "I am delighted that you ask me that, because it was invented by my great-great-grandfather." He replied, "That's why I am asking you." It is ironic that when I later toiled as a laborer in Australia, I had to operate and maintain a Robert evaporator. I had to crawl into it and remove caked-on scales with a hammer and chisel. I'll come to that later.

COHEN: Did the family retain the French language?

TSCHOEGL: It had become Austrian, but everyone spoke fluent French as well as German. My mother spoke perfect, accent-free French, even though she visited France for the first time when she was seventy-two years old.

COHEN: I see. And they spoke Czech also?

TSCHOEGL: Yes, many of them did but not all. My mother was fluent in it. As a child, I also spoke Czech fluently, although with a rather meager vocabulary, because we used it with the servants and in the street.

COHEN: But the language in the house would have been French?

TSCHOEGL: No, usually German. My great-grandfather— Jules Robert, Florent Robert's son— went back to France to study. He got a degree in chemical engineering in Grenoble, not far from Izeron, where his father was born. Back home in Moravia, he introduced the modern process of extracting the sugar from the beet by countercurrent osmosis and not by the terribly wasteful procedure of maceration, in which you simply leach the sugar out from chopped-up beets. The new process was his invention. During the Napoleonic Wars, cane sugar could no longer be imported from overseas, because of the continental blockade by the English. So the beet sugar industry developed in Central Europe, and my great-great-grandfather Florent Robert and my great-grandfather Jules Robert had no small part in that development. Jules never patented his process. He wanted it to be available to everyone. He didn't need the money anyway. The family was rather rich.

COHEN: So Jules was the first scientist in your family.

TSCHOEGL: Jules was the first to get an engineering degree. His father, Florent, was more interested in founding industries and being active in business affairs—very much a leader, and a man of importance in the industrial and commercial progress of the Monarchy. For his merits, Florent was offered a seat in the Austrian *Oberhaus*, the Senate of the Monarchy. But he just didn't care. He just smiled and said he couldn't accept it. He said, even though he spoke fluent German, "Everybody would die laughing if I got up and gave a speech, with my strong French accent." He was a remarkable fellow.

My grandfather, Julius Robert, lived in Gross-Seelowitz all his life. He and my grandmother, Henriette, known as Rika, died from complications in the aftermath of the flu epidemic that ravaged Europe right after World War I. My mother, also named Henriette but called Riki, only had high school education, although she was highly intelligent and by the time she died spoke five languages fluently. She lived in the Robert mansion in Gross-Seelowitz with her parents after the death of my father, Paul Tschögl, who was killed at the very end of World War I at the age of twenty-seven on the Italian front. He was then a lieutenant in the Hussars. I was three months old when he died. This is why I was baptized in Gross-Seelowitz.

COHEN: The family was Catholic?

TSCHOEGL: Yes. I have some Protestant ancestry also on my father's side, but little. So I come from a Catholic family, although I'm not a practicing Catholic.

My mother remarried again soon after my father's death, because—rather stupidly—the family thought that a lady could not exist without a husband at her side. Well, she married a widower with three sons. The marriage was an unhappy one. It didn't mesh at all. I have few memories of my stepfather, except that he never showed any interest in me.

With her second husband, my mother moved to Hamburg, in Germany. I went to a small private elementary school there. This is one of my earliest childhood memories. I was five years old then. After a year in Hamburg, we moved to Berlin. There I was in a public school and I *hated* it. The teachers were uninterested and uninspiring, and it was just altogether rotten. Fortunately I spent only about half a year there. Mother had gotten divorced and we moved back to Moravia.

We went to live in Kanitz—Dolni Kounice, in Czech—a small city about twelve kilometers from Brünn, where my maternal great-great-grandfather Fritz Mossig owned a cotton-printing factory named Franz Balzar, after Fritz Mossig's maternal grandfather. Virtually the whole town—about 3,000 people—lived on that. I was then eight or nine years old. Kanitz is a cherished memory for me. I began to explore the world. I took long walks by myself, all around the fields and meadows, forests and vineyards. In many ways, these few years in Kanitz defined who I became. I was home-schooled for a year and later finished elementary school—one more year—in Brünn. My mother left Kanitz for personal reasons, and I lived in Brünn then for a while with my mother and my two half-brothers.

At the age of eight or nine, I started to develop an interest in electricity. Out of stone building blocks, I built magnificent castles and churches and palaces on a table in the bedroom my brothers and I occupied, and I lit them up with little colored lights that I could fade in and out using a rheostat. I gave veritable *son et lumière* demonstrations to family members and friends, who dutifully admired what I had done. So I got interested in electricity. Not in architecture! [Laughter] I read books on electricity—on batteries, motors, dynamos, transformers, the lot. I spent almost all my pocket money on that stuff and wired it together. This is how my interest in science started, I guess.

I also developed a strong interest in languages. I already spoke German and some Czech, two Indo-European languages. At the age of nine, I started taking lessons from a tutor in Hungarian, a non-Indo-European language. All three were totally different in structure, phonology, and vocabulary. This intrigued me. How is it possible that you could have languages of such different nature and still have them convey essentially the same thing? I still don't know; professional linguists grapple with this problem to this day.

My interest in languages included a strong interest in writing systems and in grammar. These interests continue to this day. I busied myself with Egyptian hieroglyphics, Assyrian and Babylonian cuneiform writing, and, of course, the Chinese and Japanese writing systems apart from the Arabic, Hebrew, Greek, and Cyrillic scripts. I have assembled a respectable collection of grammars of a host of the world's languages. To discover structure in something that had at first appeared formless always had great importance for me—that may be the reason that I later became a researcher.

I also developed an interest in poetry at that time. To this day I like to read poetry even in a language I don't speak well. A few years ago, I translated for myself three poems of Mao Zedong from the Chinese characters; I wanted to ascertain for myself whether Mao was indeed a poet or not. I satisfied myself that he *was* a poet, although a minor one. I find it interesting that this great revolutionary wrote in the classical style.

The Chinese language, though, had no particular interest for me in my teens; that interest came much later. But I had an interest in many other languages. At the age of fourteen, I composed for myself an Old Icelandic grammar. I had gotten hold of a collection of the Eddas, the Old Icelandic sagas. The collection had a dictionary but no grammar, just some grammatical notes. So I put together a quite respectable little hand-written grammar that helped me a lot in trying to understand the Eddas.

I also immersed myself in the *Niebelungenlied*, an epic poem written in *Mittelhochdeutsch,* Middle High German, which I could understand fairly well. Concomitantly, I became interested in opera, particularly Wagner's tetralogy, *Der Ring des Nibelungen*. This was my romantic period. I dreamed of knights in armor, damsels in distress, and heroic jousts at knightly tournaments. Alas, my idyllic romantic life was not to last.

When I was about ten years of age, my grandfather Gustav Tschögl and his brother, Henrik, realized that the heir to the Hungarian family fortune hardly even spoke the language.

They decided that I should come to live in Hungary and learn Hungarian. Another brother, Wilhelm, had been a lawyer in Vienna, and evidently a good one, because he amassed much money and, having died without issue in 1928, left me a good chunk of it. This was a godsend. My father died without leaving much, since he had not yet started to earn. It was thus financially difficult for my mother to care for me properly. She had hardly any money left after her divorce, and she had to care for my two half-brothers as well.

My move from Brünn in Czechoslovakia to Budapest in Hungary was a wrench, no question about it—a terrible wrench! From then on, I no longer lived with my mother and only went back to Brünn during school vacations. I was ten years old and I was uprooted. I went to a different country, where I hardly knew anybody. It was the first of my three emigrations. Two more were to follow in due course, each a little easier.

During the first year in Hungary, I went to a school that was founded by the Anthroposophist wife of a former government minister. Do you know what Anthroposophy is? It is a sister or daughter of Theosophy. You may have heard of Theosophy. It's a sort of pseudoscientific religion, founded by Helena Blavatsky. Anthroposophy was founded by Rudolf Steiner, a German who lived in Switzerland.

My new school occupied spacious grounds that were the property of the school's founder. The pupils and a few of the teachers lived in her former large villa. There was also a separate one-story school building on the same grounds. The teachers were almost all Anthroposophists—many of them Germans, others Hungarians. The school was not large. Its purpose was to teach German to Hungarian children and to teach Hungarian to kids like myself, who spoke German but not Hungarian. At the age of ten, you learn languages quickly. By the end of that year, I was fluent in Hungarian, so that I had German, Czech, and Hungarian—although I was not that good in Czech. Today I speak, read, or understand at least a dozen languages and I lecture in four, but my criterion for whether you truly know a language is whether you can do a crossword puzzle in it. [Laughter] I can do that only in German, Hungarian, and English. Later I became convinced that the real criterion is whether you can write poetry in a given language. I tried my hand at doing that in German, because it came most easily to me. Although some of this poetry was quite good, I realized soon enough that I would never become a poet—and neither did I want to become one.

COHEN: What year are we at?

TSCHOEGL: 1928. The following year, having successfully mastered Hungarian, I was moved to another boarding school, in Gödöllő about twenty-five miles from Budapest. The school was run by Premonstratensian priests. The Premonstratensians are also called Norbertines; they were founded by St. Norbert in the twelfth century in a place called Prémontré in France. *Pré* is "meadow," and *montré* means "shown." This, if you wish, is reminiscent of Brigham Young's "This is the place," in Utah. The Premonstratensians, particularly in Hungary, were devoted to teaching. The ones in Gödöllő came from a part of Czechoslovakia that had been part of Hungary before World War I. They were expelled from Czechoslovakia in an early attempt at ethnic cleansing after the war, because they were Hungarians. They found a new home in Gödöllő, raised money, and erected magnificent buildings for the order, for the boarders, and for the *Gymnasium*—not a place to exercise the body but a high school to exercise the mind.

The boarding school was posh. At the table, we were served by valets with white gloves. They also made our beds and polished our shoes. We were well taught, although in a somewhat old-fashioned way. The instruction was strong in the humanities, not as strong in the sciences. I am rather happy that I enjoyed a good humanistic schooling. Such schooling, I think, would serve every future scientist well, because it is one safeguard against becoming one-sided. Throughout my later life as a scientist, I kept up my interest in the humanities—languages, literature, history, philosophy… I am pleased that I was thus able to straddle C. P. Snow's two cultures.

Well, what did this humanistic schooling consist of? I had history, geography, mathematics, some science classes, and languages. I had eight years of Hungarian, Latin, and German and four years of French. In addition, I chose English and Italian as electives. So I carried four foreign language courses simultaneously. German and Hungarian, of course, were native languages for me. When I wanted my younger son, Christopher, to take Latin in addition to French here at Polytechnic School, they called me in and said that taking two foreign languages was too much for someone his age. I had carried four. [Laughter] On my insistence, they concurred, and Christopher was always happy about it. You improve your English vocabulary if you know Latin, because English contains so many words that are derived—directly or indirectly—from Latin.

Our teachers, all priests, demanded a lot and were rather strict. In fact, the discipline was a little too strict. I hated it, but I realized that I was getting a good education and that I had to knuckle under. Hungarian high school education comprised eight classes. I started in first class, essentially repeating the year that I had spent at the Anthroposophist school learning Hungarian. Right from the beginning, I was tops in my new school. I probably did so well because I was so lonely and homesick that I did nothing but study and read. To compensate for the lost year, I later skipped sixth class. I went from fifth into the second semester of seventh—unusual in Hungary then, not so unusual here. I needed special permission from the Ministry of Education to do that. I sweated up the material of the skipped classes largely on my own, with the help of a young priest who was assigned to me for that purpose. I topped my new class also, right up to graduation. I learned with great ease and rapidity, although I was not conscious of it then. It was just natural for me somehow. Anything that interested me I absorbed readily, and I was greatly helped by an excellent memory.

So I spent seven years instead of the normal eight in high school. Of course, during this time I continued my interests, which at that time were not scientific and centered largely on languages. I learned some Turkish then, and the Arabic and Cyrillic scripts. I acquired the latter when I vacationed at the Adriatic seaside in Yugoslavia, in what is now Croatia. I was standing in front of an announcement in the little harbor there. It was printed in both Croatian and Serbian. These are almost the same languages, but they are written in different scripts. It is as if British and North American English were written with different characters. I could read the Croatian part in the Latin script, and my knowledge of Czech allowed me to comprehend what I was reading. I compared it with the Serbian part, and standing there for a while I learned the Cyrillic letters.

I had another, similar experience in the school where I first learned Hungarian. We had Jewish boys there also, and when we went to physical education, the Jewish boys occupied our classrooms. A rabbi came and taught them. They had Hebrew texts. One day a Jewish boy forgot his Hebrew grammar book on the desk where I normally sat. I opened it and, oh, that was interesting! [Laughter] And I learned the Hebrew characters. As you probably know, you can't really read printed Hebrew unless you already know the language, because the vowels are generally not indicated. Later I did learn to read the characters fairly well, though. A Jewish friend had given me a photocopy of a short story by Isaac Bashevis Singer, who writes in

Yiddish and got the Nobel Prize for literature. Yiddish is essentially a medieval German dialect interspersed with Hebrew and Slavic words. It is written phonetically in the Hebrew characters. I could understand much of it without even having to consult my Yiddish dictionary. This sort of thing is just, you know, entertainment for me.

COHEN: You were in school until what year? Because I know a war is going to start again pretty soon.

TSCHOEGL: Yes. I was in school until 1936. After graduation, I entered military service in a Horse Artillery Unit. Military service was compulsory in Hungary. With a high school education you could become an officer in the reserve. You did one year of training, including six weeks of boot camp, and you ended up as a *cadet aspirant,* a very junior officer but an officer nevertheless. I finished my compulsory military service in 1937, but during the war years I was quite often called up to serve in my unit, and I often spent several months soldiering. I rose to the august rank of ensign. When Hungary entered the war on the side of the Germans, I spent one year in the Ukraine. Later I was promoted to lieutenant second grade. That is how high I rose in the military. [Laughter]

COHEN: And you remained in Hungary?

TSCHOEGL: Yes. I was a Hungarian citizen from birth, because of my father's citizenship. I should say, however, that the Tschögls were not really Magyars; they came from Austria—from what is now called the Burgenland, one of the *Länder*—states—of the Austrian Federation. The Burgenland was part of Hungary until 1918. My ancestors hailed from a little village there, called Siklós in Hungarian. When my Tschögl grandfather and his brother were ennobled by the emperor of Austria—who was simultaneously king of Hungary—they chose the name of that little village as the predicate of nobility. My full name at birth was therefore Siklósi Tschögl Miklós. In Hungarian the predicate of nobility—if you have one—comes first, then the family name, and then the first name. Miklós is the Hungarian form of Nicholas. The accent on vowels marks length in Hungarian, not the stress accent, which falls invariably on the first syllable. I changed the spelling of my name to "Tschoegl" when I emigrated from Hungary and went to

Australia, realizing that there is no "ö" in English. The "oe" spelling is a traditional variant for it in German. The name is pronounced, close enough, as if it were spelled "Churgle" in English.

I visited Siklós—Sigless, in German—before the war, in search of my ancestors. No Tschögls lived then in that village any longer. However, in the neighboring village of Krensdorf there were still eight families of Tschögls, although these families did not consider themselves related to each other; their respective roots went too far back. When I asked the parish priest, "What manner of people are these Tschögls?" he said, "Oh, they are decent folk. When they drink, they're a little loud." [Laughter]

My paternal great-great-grandfather Caspar Tschögl left the Burgenland at about the same time that my maternal great-great-grandfather Florent Robert left France for Bavaria. Hungary at the time was something like a colony for the western part of the Austro-Hungarian Empire. The Magyars were not interested in industry and commerce. They preferred the land. Land was their everything; they lived on it and cultivated it and that was that. Industry and commerce were pursued by Austrians, Bavarians, other Germans, and by Jews, Serbs, and Greeks. These were the folks who developed business and industry in the country.

Caspar Tschögl's parents were peasants. He left the village life to learn the druggist trade in Vienna, in the *Apotheke zum schwarzen Hund,* the "Pharmacy at the Black Dog." A black dog was its shop sign. When I looked for it before the war, the pharmacy still existed there. Many years later, I could not find it. When Caspar had ended his apprenticeship, he emigrated to Pest—Budapest—in Hungary, where in 1803 he started his wholesale firm selling medicinal herbs under his own name. His son, Johann Tschögl, married Emma Hoffmann, daughter of Joseph Hoffmann. The Hoffmanns were of Germanic, probably Bavarian, origin. I have not been able to trace this line back beyond Joseph Hoffmann's parents. Joseph founded the firm of Joseph Hoffmann—Hoffmann József, in Hungarian—in 1804 in Budapest. The firm dealt in the wholesale of colonial goods: coffee, tea, rice, spices, and the like. In the second half of the nineteenth century, my grandparent Gustav and his brother Henrik, Emma Hoffmann's sons, assumed ownership of what became the family firm. My father, Paul, prepared to enter it also. He completed the famed *Handelshochschule*—business academy—in Vienna and then spent time in London and in Hamburg with valued business connections of the family firm. He never entered it, however, because of his early death. I was also expected to go into it. Nobody ever asked me whether I wanted to or not; it was just obvious—preordained, as it were. I never

questioned it myself, either. I entered the family firm in the fall of 1938. Gustav and Henrik had passed away some years earlier, and the firm was run by my uncle, Andris Tasnády-Szüts, the husband of my father's sister. When I joined, he and I were active partners. My great-aunt, Uncle Henrik's widow, was a silent partner. Under the tutelage of my uncle, I spent several weeks or months as required in the various departments of Hoffmann József to learn what was supposed to become my life's occupation. Fate decided otherwise, later. However, my experience in the relatively limited time I spent in the family firm was by no means wasted when I became a scientist. The experience taught me a host of things: attention to detail, following through on anything I had begun, how to deal with employees, the rudiments of business and finance, and many things more from which I benefited in my later life.

COHEN: But now you were in the army? How many years? There was a war on?

TSCHOEGL: World War II started in 1939. The Germans wanted Hungary to be on their side, so they tried to satisfy the political expectations of the Hungarians. When the Monarchy was divvied up in 1918, large areas with a purely Hungarian population were made part of the so-called successor states. For instance, a large, contiguous, ethnically Hungarian part of today's Slovakia was given to the Czechoslovak Republic as a breadbasket; nobody asked 700,000 or so ethnic Hungarians whether or not they wished to become part of Czechoslovakia. Some of these ethnically Hungarian areas were returned to Hungary under the two so-called Vienna Awards. The Hungarian Army had to reoccupy these territories. I rode into Czechoslovakia, Romania, and Yugoslavia in successive years under these arrangements with the Germans.

COHEN: These were Hungarian areas that were returned to Hungary?

TSCHOEGL: Yes, temporarily! After the Second World War, everything went back to what it was before. But for a while these areas were Hungarian again.

COHEN: Did the Germans ever come into Hungary?

TSCHOEGL: They did, but later, in March of 1944. Until the Nazis invaded it, Hungary was nominally free but of course had to play along with the Germans. Under Nazi pressure

Hungary's prime minister had declared war on the Allies, including the Soviets. The regent, Hungary's head of state, had reluctantly agreed. Hungary was headed by a regent because after the First World War the Allied powers had forbidden the Hungarians to elect a king, lest they bring the Habsburgs back to the throne; therefore they elected Miklós Horthy as regent. Horthy had been an admiral in the Austro-Hungarian Navy.

The Hungarian military forces consisted basically of three armies. The idea was to keep one army out in Russia for one year and exchange it against another one the next year. The regent, no friend of the Nazis, mistakenly thought that in this way Hungary would have one or perhaps two war-hardened armies available at home when Germany finally collapsed. He hoped that Hungary perhaps could thus retain the gains it had made under the Vienna Awards. He was convinced that Germany would lose the war, and in this he was not mistaken. He failed, however, in his objective to keep the ethnically Hungarian territories. When Nazi Germany's collapse seemed imminent and the Soviet armies were advancing to occupy Hungary, the regent sent emissaries to Moscow to negotiate a separate peace. Earlier attempts to do this with the British had failed. I know about all of this from a cousin of mine, a general staff officer in Hungarian counterintelligence, who headed the small delegation that clandestinely tried to negotiate with the British. When the Germans got wind of the regent's overtures to the Soviets, they engineered a takeover by the Hungarian Nazi Party, the so-called Arrow Cross Party—the arrow cross imitated the swastika. These people were such a hopeless bunch of no-brainers that not even the Nazis had previously wanted to have anything to do with them. Eventually, though, they felt they had to, because there was no one else in Hungary that they could get to work with them. They arrested Horthy and spirited him to Mauthausen concentration camp, were he stayed in a special section for non-Jewish political prisoners until his release at the end of the war. He died in 1957 in exile in Portugal. Nobody here knows much about these things, apart from a handful of historians.

During 1942-1943, I spent a year in the Ukraine. To begin with, I served as first officer in command of my unit's gun position. Later I became aide-de-camp to the commander of the Fourth Corps of the Second Hungarian Army, a two-star general. In the Hungarian forces, as in the forces of the old Monarchy, when you had attained that or a higher rank you were addressed as "Excellency." As an aide-de-camp, I was essentially a glorified flunky, but General Heszlényi was an outstanding person to serve and I learned a lot from him. We often spent many hours in

his car riding around, visiting all the units that served under him, and during these hours we conversed about anything that came up, military or whatever. Again, my time in the Ukraine was not wasted.

Let me record a couple of amusing incidents. At one time, my Excellency had to have his stomach ulcers attended to and spent a week in a German military hospital in Kiev. During that time I had the corps commander's limousine—flags in front and back—all for myself. I roamed through Kiev sightseeing, and when I passed the HQ of a German unit, the watch sprang to arms and saluted. [Laughter]

Another memorable field experience: The Hungarian Army had been pulled back after the German debacle at Stalingrad. For a while, we were staying in a little Ukrainian city to gather the troops and reorganize. And of course there were Germans there as well. Military etiquette demanded that the Germans invite us. My guy had one more star than the highest-ranking German officer there. And, you know, in the military, if you have one more star on your uniform, that means not only that you rank higher but also that you are smarter, wittier, and even more handsome than the guy who ranks below you. My Excellency and I went to have lunch with these Germans, a dozen or so of them. We took place at a long table. I sat next to my lieutenant field marshal. His opposite number, a German lieutenant general, faced him with his aide-de-camp next to him. The atmosphere was somewhat strained. These Germans had never met a high-ranking Hungarian general before and didn't quite know how to handle the situation. And you know, Germans can be pretty stiff—particularly if they are Prussians, and there were some Prussians among them.

Well, we sat down and the two generals started to talk with each other. My Excellency spoke perfect, fluent, Austrian German, having gotten his spurs still under the old Monarchy. It soon turned out that the German general was not really a German at all: He was an Austrian in German uniform. Furthermore, he had gone to the same military school as my Excellency had. So instead of using the formal *sie,* the third-person plural, they at once switched to the informal *du*, the second person singular, the use of which was *de rigueur* among Austro-Hungarian officers that had attended the same school. These forms of address have no counterpart in English, where one always uses just *you*. The whole atmosphere changed at once. I was politely asked a few questions myself and of course answered in the same flawless Austrian German.

Even the Prussians in the party relaxed. Now we were all the same crowd! The cultural significance of this was not lost on me.

COHEN: It doesn't sound like you suffered too much during this war period.

TSCHOEGL: Oh, no! I just related some humorous incidents, which I prefer to the serious ones. I guess I subconsciously keep those memories pushed back. My real war experience is another matter altogether. We are assuredly not talking fun here! I was in three major battles; I suffered hunger, thirst, and sleep deprivation. My orderly was badly wounded in an air raid on our gun position, of which I was then in command. The bullets narrowly missed me. Once, when we halted after a long ride, I fell asleep at night totally exhausted in pitch darkness just in a field, without ascertaining where I was. I woke up in the morning lying on parched grass next to the bloated corpses of five enemy Tatars. Experiencing so many dead and wounded was all too often nauseating. I faced mortal danger more than once.

A totally different incident sticks in my memory, because it was so unusually frightening. I was already aide-de-camp and was dispatched to the very front line to transmit a sealed order that had to be delivered in person. It was again a totally dark night, without even a single star showing. I passed one unit still knowing where I was, but soon after that I completely lost my orientation. I knew that the enemy line was *very* close. I didn't know where to turn. Surely I didn't want to stumble in among the enemy. Carefully, very carefully I tried to proceed first in one direction and then another, listening tensely whether I would hear Hungarian or Russian words. Suddenly: Hungarian! I soon found my destination and nonchalantly delivered my sealed orders. Even today, though, I shiver when I recall that feeling of total disorientation in pitch darkness, facing possible capture, death, or at best a POW camp in Siberia. I had never before been in a similar situation of total abandonment.

At one point during the summer, I contracted something like an amebic infection—it was never actually diagnosed; that was impossible out in the front line. I was very, very sick for several weeks. No one in my unit ever became sick of the same unidentified disease. There were no medications of any sort. Antibiotics had not yet even been invented; there wasn't even a field medical orderly to look after me. I deliberately consumed nothing except some rather poor mess coffee for over two weeks. After several days of misery, where I was just lying half dead

on a cot amid constant shooting and shelling, my battery commander somehow managed to get hold of a dilapidated jalopy and had me transported from the front line to a field hospital. The journey took several days. The hospital had no medical supplies, either, but it had a doctor. My medication consisted of milk chocolate and red wine, but at least I had *some* solid food and could dispense with the lousy coffee. Eventually I regained enough of my strength to rejoin my unit. I was young, my constitution was good, and I had the unwavering, caring attention of my loyal orderly—not the one who got wounded in the air raid on our gun position a couple of months earlier—throughout this ordeal. I rewarded him handsomely after my unit returned home in the course of the exchange of the armies I mentioned before.

There were, of course, hundreds of other incidents and experiences, but let me get to the grand finale.

At the end of the war, I was shot one morning during the siege of Budapest by a Soviet advance scout when I was reconnoitering. This was in Budapest, only about two miles from the house in which I lived. The shot from his automatic broke my left upper arm; the impact threw me on my back and wrenched my right arm out of its socket. I was lying there helplessly waving my legs around like a beetle turned on its back. My assailant kept firing away. I never even saw his face; he stood behind a break in a wall. I saved my life kicking like crazy until I made it around a corner, somehow got up and ran into the basement of an apartment house. The women there took as much care of me as they could. It was precious little, but a tourniquet was applied to my left upper arm to stop me from bleeding to death.

One could not go outside the house until darkness fell, because of Soviet air activity. Finally I was carried on a stretcher into a field hospital of sorts, where my left arm was bandaged and my right arm was wrenched back where it belonged by two medical students. I was then moved into a school basement, which I shared with some two dozen or more sick and wounded. There I lay for two weeks on a straw sack with hardly any food and only melted snow to drink. I could not wash myself and I contracted lice to keep me company. Eventually the Soviets "liberated" us. A young artillery officer—from Leningrad, as it turned out—came down into our hellhole and asked if anyone spoke Russian. I volunteered. My Russian was not that great, but we managed nicely. He was intensely interested in the capitalist world and asked a number of questions that I tried to answer as well as I could. From then on, he came to see me three days running and every time brought me a bowl of thick nourishing soup that he spoonfed me, since I

could not use my arms. The last day he said that he would not come back, and he wished me luck. I don't even know his name. Go figure! People are not enemies. Governments are enemies.

I was not yet married at the time, but I had already met my future wife, Sophia [Sophia M. Glazmak], a short time before. She had come to Hungary as a Polish refugee and was working in a hospital as an assistant physician while trying to complete her medical degree. After some fourteen days of utter misery, I managed to send word to her in the hospital where she worked, by one of the women of the wounded. I knew that she spoke Russian. Two days after I had sent the message, Sophia and her Polish girlfriend, who also worked in the same hospital and also spoke Russian, arrived at my "bedside" wearing Red Cross armbands. They were accompanied by a Soviet medical officer of the rank of captain, whom they had volunteered for good measure. They brought my winter coat with them, from my house, to wear over my uniform. Then the three of them chatting merrily in Russian, with me in tow as a "victim of fascism" in case anybody asked, footed through snow and slush to my house three miles away.

I shared ownership of a house in Buda with a great-aunt of mine; the house had several apartments. My apartment was on the top floor. It had been hit by three Soviet artillery shells and was uninhabitable. Sophia guided me into my great-aunt's apartment on the first floor, removed my lice-infested tunic, and went outside to melt snow for ablutions. I think I needed that badly; I must have stunk from a mile away. While she was outside, two Soviet soldiers sauntered in and informed me that I was a Nazi and I was to come outside with them to be shot. I received this information with equanimity; I was past caring. Nevertheless, I argued with them, saying I was a Hungarian officer, that they could see that from the tunic of my uniform lying on the floor next to me. I said I was not going to move. If they wanted to shoot me, they would have to do it right there. Evidently they were tidy people, because they tried to convince me that it would best be done outside.

To this day I do not understand why during this altercation the idea to call Sophia did not even occur to me at all. Fortunately, she returned just when we had run out of arguments and counterarguments in matters of my future. She immediately assessed the situation and addressed the two Soviets in Russian: "*Tovarishchi* [comrades], can I help you?" An amiable discussion followed, during which Sophia told them that I was indeed Hungarian, not German, and that I had done a great deal for Polish refugees. The spokesman of the two, quite politely, said that

that was OK then; if she didn't want them to do it, they wouldn't shoot me. With that, they left. This is how Sophia came to save my life that time. But it was touch and go. I tell this with some wry humor, but you can see that the war was not all that humorous for me.

However, let me go back to my wartime experiences. At Christmastime I went on furlough for two weeks. The time spent in train travel, about ten days each way, did not count as part of the furlough. I first spent a few days in Brünn with my mother and then went on to Budapest. The day after my arrival in Brünn, while Mother and I had breakfast together, the bell rang. Two guys in civilian clothes entered. After an exchange of only a few words, they led my mother away. My insistent entreaties were simply brushed off. I felt completely at a loss and didn't know what to do. No one else I frantically turned to knew how to help me, either. Sometime late in the afternoon Mother returned. She had been taken to a Gestapo HQ. Why? A few days earlier she had—unsuccessfully—tried to intercede in favor of a half-Jewish friend who was to be deported. Mother was released after receiving a stern warning. She was told that she got off lightly only because both my half-brothers had died "in the service of the Fuehrer." That taught me something about Nazi Germany, of which we were hardly aware out at the front.

After a rather pleasant stay in Budapest, recharging my batteries in the arms of my lovely girlfriend, I boarded the train back to the front, together with another officer of my unit who had been on furlough at the same time. Before we reached our destination, our train was halted as the evening fell, and everybody was ordered out. Bombs were falling and there was chaos. Eventually we found out that the front had just collapsed farther ahead at Stalingrad. The German and Hungarian troops of that front section were retreating in disarray. For several days, we spent most of our time watching the road, on which we expected to spot our own retreating unit—or what was left of it. Eventually, miraculously, our unit arrived intact, with all our field cannons, ammunitions, and horses. Among the crew was my orderly, with all my personal belongings. The Hungarian general who was engaged in reestablishing order among the Hungarian troops in the area included a special commendation of my unit in his Order of the Day. Next morning we started on a weary, arduous trek on foot over endless fields covered knee-deep in snow. Riding was impossible. The temperature was never above - 40° F. Eventually we reached Ovruch in the Ukraine, where the remains of the Fourth Army Corps were reassembled and made war-ready again. It was after we reached Ovruch that I became aide-de-camp of the commander of the Fourth Corps.

From this experience, I learned something about the human spirit—its ability to cope with hardship and doggedly survive tribulations. Rarely was any one of us ready to abandon hope. I remember one incident, though. The officers of the unit had to take turns and fall back from time to time. We had to let the unit pass in front of us and make sure that no one had dropped out left or right or had fallen behind. At the end of the long weary line, there were usually some stragglers that had to be herded back into the fold. Not keeping up meant certain death.

Well, one day when it was my turn, one of our men, a corporal, had simply given up. He was sitting in the snow some distance behind the last man. I trudged up to him and urged him to pull himself together. I reminded him that he was going to die of cold if he stayed behind and ordered him to hurry to rejoin the unit. He just shook his head and said he couldn't do it, that he just didn't care. I sharply ordered him again to get up and get going. He didn't answer. What was I to do? I had never been in such a quandary before. To take out my pistol and threaten to shoot a man who was ready to die anyway? I realized that I simply had to shock him. I sharply hit him twice with my riding crop. Wearily, he rose and began to make his way back. I stayed close behind him until he joined his comrades. We spent the rest of that night in a small village hastily quartered among Ukrainian peasants. Next morning, rested and refreshed, the man reported to me. Saluting smartly, he thanked me sincerely for having saved his life. I patted him on the back and said I was glad he was OK.

Believe me, the war made a profound and lasting impression on me. I wouldn't be the same person that I am today had I not had that experience. I learned what it means to survive. I learned to persevere. I learned to duck under and endure hardship. These invaluable lessons served me well in later life. I also became conscious of the fact that large numbers of people could be directed by one central will—for good or evil. All large-scale organizations in the world originally arose from the need to organize masses of people into fighting units. That is reflected today in the organization not only of our military but also in that of our firefighters and our police and shows up even in much of the organization of our large corporations.

Because of my command of German, I sometimes acted as liaison officer with the staff of a neighboring German division. I saw how the German forces were organized and how they operated. The German war machine was formidable. It was an organization of tremendous efficiency. The German General Staff was the equal to, or even surpassed, practically every

other in meticulous planning. However—and here is the rub—they were not as good as the Allies, including the Soviets especially, in readjusting and improvising when something went wrong—as it always necessarily did, usually not too long after the execution of even the most meticulously planned action began. [Tape recorder turned off]

NICHOLAS W. TSCHOEGL

SESSION 2

May 2, 2001

Begin Tape 2, Side 1

COHEN: Last time we got almost to the end of the war. We were at 1943 already. And if you could just tell us a little about how things changed when the Germans actually took over in Hungary.

TSCHOEGL: When I came back from the Ukraine in the spring of 1943 and resumed civilian life, things were more or less normal back home. Hungary was basically an agricultural country. We had plenty to eat until almost the very end and at home hardly even noticed that there was a war going on. Life was relatively easy on the surface; underneath it was full of anxiety, but we tried not to make too much of that. Sticking our heads into the sand, I guess. I need to say that most people in Hungary only gradually learned what the Nazis were perpetrating elsewhere. There was much sympathy for the Germans, because of the Vienna Awards. After the Nazis finally invaded the country in March of 1944, they introduced all those evil measures that had become so well known later. Concentration camps were established; Jews, Gypsies, and other so-called undesirables were rounded up and confined in them.

The German occupation didn't last long, because by April of 1945 the Soviet Army had overrun all of Hungary. They started in the east and gradually moved west until the whole country fell under their domination. The Hungarian Armed Forces, generally well led but ill equipped, retreated with the exhausted and often demoralized Germans.

Under Soviet tutelage, the Communists began to assume power in Hungary. In early 1948, an election was scheduled. We nominally still had five political parties, and there was an attempt to make it appear that the upcoming election was a democratic process in a democratic country—which of course it wasn't. The Hungarian Communists had already—aided and abetted by the occupying Soviet forces—secured the Ministry of the Interior, which in Hungary traditionally controlled the police. Let me give you a couple of examples of the tactics the Communists employed to consolidate their power. In one of the apartments of my house there lived two elderly retired teachers. As the election was coming up, these two women were

arrested on the trumped-up charge that they were prostitutes. They were released without any explanation or apology shortly after the election. They had been arrested simply to prevent them from voting, because the Communists expected that they would not vote for them.

I had to vote, too, of course. In the voting hall, the representatives of the five parties were sitting behind a table with the Communist representative in the middle. In front of the table they had placed the ballot box. When I entered, the Communist representative gave me the ballot paper and said, "Comrade"—that's how he addressed me!—"if you vote Communist just drop your ballot into the box here. If you want to vote for any of the other parties, you may go into one of the voting booths to do it." He had the register of voters lying in front of him on the table. I had a wife, a child, and a mother, and I knew what had happened to these elderly teachers in my house. I took the ballot paper and dropped it in the box. That was the only time in my life that I voted Communist! [Laughter] And in October of that same year I left the country.

In the meantime, though, other things happened. The Communists staged several of those Stalinist show trials that they had put on in Moscow. They ferreted out people in the various government agencies who they knew were not for them and whom they therefore wanted to eliminate. There was such a fellow in the Ministry of Agriculture whom they had already arrested and against whom they now wanted evidence of criminal acts. They looked through the records of his department and tried to follow up on whatever they thought looked promising. They found that my firm had sold to the Ministry of Agriculture a large batch of a certain animal vitamin D, used as supplement in pig feed. We had been extra careful about that. Since we had acquired it before the war, we had it analyzed chemically to make sure that it was still up to scratch. We badly needed money just to keep going and pay our loyal employees, who depended on us. We no longer were granted any import licenses. I think I had said earlier that imports were the main activity of our firm. We offered the ministry a rather good price for the vitamin and they bought it gladly. There was a need for it; the pig industry, run down during the war, had to be revived again. The Communist investigators thought, Maybe we can use this!

So, the oh-so-democratic Communists, armed with the entry they had found in the books of the Ministry of Agriculture, embarked on a fishing expedition. They arrested my uncle. I was with him in the office when that happened. One early morning, two men in civilian clothes walked into our office, briefly identified themselves as police agents, and said, "We want you to

come with us; we need you as a witness in a car accident." My uncle replied, "What car accident? I have not witnessed any car accident." They replied, "Yes, you did. Just come with us." Well, what could he do? We were all still law-abiding citizens. They left with him.

Several hours later they came back and now asked *me* to follow them. One of our lawyers, a sharp-witted young man, had dropped into the office, and we were talking about what had just happened with my uncle. When the two thugs now left with me in tow, he followed us to see just where I would be taken. That was the headquarters of the economic-affairs arm of the AVO, the so-called secret police. This dreaded police, firmly under Communist control, was secret only in name, of course. The English name is a misnomer anyway, arising from an incorrect translation of the German term *geheim*—as in "Gestapo," which is an acronym for *GEheime STAatsPOlizei*, which actually means "confidential"—not secret—"state police."

I was led into a courtyard and ushered into a room that opened from it. Several people were sitting there on benches, all facing the wall. I recoiled, thinking that I had been led into the wrong room. However, I was ordered, "Go in there, sit down, and face the wall." So I did. Not long after, they led me upstairs. I saw my uncle on the way up. His tie and shoelaces had been removed, but he did not look abused. I tried to talk to him, but I was whisked away. They started to interrogate me: How did we sell this animal vitamin? How did we know that ministry official? And so forth. I had never even personally met that fellow against whom they wanted evidence of wrongdoing. He was an acquaintance of a physician whose son was a friend of my cousin Andy, the son of my uncle and partner. The physician played a quite innocent role in bringing the deal about. It finally dawned on me that they wanted to find evidence that we had bribed that official. So I told them, "Please come to my office and search our books. You'll see that this was a perfectly legitimate business deal. I don't even know Mr. Such-and-Such. We had nothing to do with him personally." Somewhat to my surprise, they agreed, and I went home. Sophia and I spent a sleepless night, full of anxiety about what was going to happen next day.

COHEN: Did they let your uncle go, too?

TSCHOEGL: Not then. He was released the next day, when all three of us were finally let go. My cousin Andy was working in the cashier's office of our firm at the time. He had an apartment in

the same house in which I did. He was arrested during the night at two o'clock. Sophia and I found out about that only in the morning, because our concierge came up and told us. My cousin didn't fare quite so well when they interrogated him. He was a young man, just out of his teens, good-looking and of a cheerful, pleasant disposition. They found his engagement calendar in his pocket. It had entries such as dinner with Countess Such-and-Such and tea at Baroness Such-and-Such—all aristocratic friends of his, surely no Communists. He was also a bit careless and somewhat snippy in his answers. He had no experience of how to behave in the hands of thugs. They hit him hard in the face, twice, so that he fell from his chair. I remember the name of the thug who hit him as Elephantidi. Despite his name, he was small of stature and clearly enjoyed hitting defenseless taller victims. They found nothing else against my cousin, except that he seemed to be a playboy and evidently not good Communist material. He lives in Argentina today and is happily married. I encountered Elephantidi also during my interrogation, when they subjected me to the good-cop-bad-cop routine. Fortunately I escaped injury at his hands, because I had sized him up correctly just by looking into his eyes, and I behaved with due circumspection.

Why were we released next day? We found out a few days later, quite unexpectedly. The official in the Ministry of Agriculture against whom they had wanted evidence had died under torture. Thus the evidence had become unimportant. A former classmate of my cousin's was the assistant to the doctor who was supposed to supervise the torture medically. The doctor had evidently misjudged the condition of the victim's heart and the fellow died. His assistant told my cousin, and so we knew why we were all let go.

Not long after that, though, we three, plus the physician who had been instrumental in arranging the fatal business deal, obtained summonses to appear as witnesses in one of those infamous political show trials. Our physician friend was due to appear several weeks before us. He was arrested by the judge in the courtroom, as an enemy of the people. His license was revoked, and he was sentenced to six years of hard labor. No explanation, no justification. There was no evidence of any wrongdoing whatsoever against him. Simply being declared an enemy of the people was justification enough. He was let go after six months, though, but for two years he didn't get his physician's license back and shoveled coal—literally! We knew what was going to happen to us next. So we decided that we had to leave the country before our trial date came up.

Before I get into this, however, let me relate how I had met my wife, Sophia. I mentioned her earlier, when I told the story of how she saved my life. The Soviet troops had slowly started to move into Hungary but were as yet far from Budapest, when I had a conference in my office with one of our firm's tax advisors. When we had finished our business, he said, "Well, I've got to go over to Buda"—our firm was in Pest—"my wife's in the hospital there. She just had a baby." He told me about her. He had served some time ago in a Hungarian Army unit that was then stationed in Poland. There he had met his wife, a Russian concrete engineer. She had come to Poland under the Soviets, and when the latter had been forced back by the Germans, she saw her chance and didn't go back to the Soviet Union.

Well, the Russians were coming, and so I was interested in meeting this Russian woman. Although I had spent a year in the Ukraine—which was part of the Soviet Union then—I had never met an educated Russian woman before. I still had the use of a small company car, the last of our fleet of vehicles, all the others of which had been requisitioned by the army. I said, "I'll be glad to drive you over; I live quite close to that hospital." When we got there, we went to see his wife. Sophia was in the room with her and was conversing with this Russian woman. Introductions were made. This is how I met my wife.

I had been in there for hardly a couple of minutes when a doctor came in to examine the young mother, and so we had to leave the room. Sophia and I were walking up and down in the corridor waiting to be readmitted, and that was *it* for me. [Laughter] I had read about that—what the French call a *coup de foudre*—a lightning strike. Of course, I had had all sorts of affairs with women—I always loved women—but with the exception of one hopeless and unrequited involvement with a happily married aunt when I was fifteen to seventeen years of age, I had never been seriously in love before. The encounter with Sophia changed everything. She had evidently also become interested in me almost immediately. [Laughter] Our interest in each other has lasted for over fifty-five years now.

I asked a friend who had already met Sophia—I don't remember how I found out about that; love has its ways—to host a dinner, so that I could meet her again. This was arranged and we were seated next to each other, naturally. A day or two later, I invited her and her Polish friend to my penthouse apartment for a dinner. We began seeing each other a good deal after that. We married in Budapest in April of 1946, and our first son, Adrian, was born in 1947. My younger son, Christopher, was born in Sydney in 1954.

I owe it to Sophia that I became a scientist. Before we married, Sophia lived in a room in the hospital where she worked. One Sunday we were lying on her bed, resting after lunch, and wondering what would become of me under Communist rule. Sophia said, "Look, I have lived in Poland under the German occupation for two years, and I have lived under the Soviet occupation there also for two years. I can tell you this: The only way in which you can survive under the Soviets without prostituting yourself too much is either as an artist or as a scientist." I thought, Well, I love art, I love music, but I have no ability in these areas. Science—well, that's a different thing. Let me consider this." So, with her insistent urging, I enrolled at the Müegyetem, the Technical University of Budapest. This was a tough school. You had to have *very* good high school credentials to even be considered for admission. Well, I had those. You also had to have somebody of importance to support your application. Sophia happened to know the son of the rector of the Technical University, an economist of wide repute. We visited Professor Heller. He already knew about us from his son, and he supported my application forthwith.

There was also a *numerus clausus*: Only ninety people would be admitted to freshman chemistry. At the end of the first year, thirty students were weeded out with the aid of what we called guillotine exams, because only sixty people could be accommodated in the sophomore chemistry labs. Eventually, only twenty-four of my classmates graduated after the senior year. I selected chemistry because it was considered to be the toughest of the options, and I thought I had a greater change of survival if I had something that is valuable. I was rather naïve about that because politically it didn't count. You could have had a Nobel Prize. If the Communists judged you to be an "enemy of the people"—read "an enemy of the party"—you were done for anyway.

So I enrolled in chemistry and finished three years of the four years required for the diploma. In the last year, I got a C in physical chemistry. In Hungary, A was the best, D was failure. This C was the only C I ever had in my life, so I decided to become a physical chemist. [Laughter] You know, as Yogi Berra said, "When you come to a fork in the road, take it." [Laughter] And I developed the attitude that whenever I came to a fork in the road, I selected the more difficult path—and that always paid off.

COHEN: How many years were you at the university in Budapest?

TSCHOEGL: Three years. I had to leave the country before I could get the diploma, which would have been the equivalent of the BSc here.

When we had to flee Hungary, we managed to get a passport for Sophia and Adrian.

COHEN: How old was your little boy then?

TSCHOEGL: He was eight months old. Together, the three of us would never have gotten a passport. We bribed a young officer in the police to get it for Sophia and Adrian. He delivered it personally on a Sunday morning, clad in a splendid white dress uniform. He was evidently proud to be a police officer and had not yet cottoned on to the realities of his job. He handed us the passport; we handed him 100 forints, and that was that.

Sophia and my little son departed in the morning for Italy, and my uncle and I departed in the afternoon for Austria. We had left for good. Sophia had friends in Intra, on Lago Maggiore. During the war, the Italians, as allies of the Germans, also had had some forces on the Russian front. There was an Italian HQ in Lemberg—now Lviv, in the Ukraine—where Sophia was living then. Her brother Edward, a mechanical engineer, had studied in Italy and spoke fluent Italian. He served as interpreter for the Italian Transport Command. Two of the Italian officers there had helped smuggle Sophia, her brother, and her Polish girlfriend into Hungary. They had put Edward into an Italian private's uniform; Sophia and her friend were disguised as Italian Red Cross nurses. These times were just absolutely wild. Smuggling people out of and into various countries had become something of a cottage industry.

My cousin, Andy, had already been smuggled out earlier. He was young and had no family of his own. He joined his mother, my father's sister, who was then already in Austria, in the French Occupation Zone. She served as an interpreter for the French authorities, since she spoke fluent French as well as German, aside from Hungarian.

His escape route required only a moderate amount of money, but we judged it to be too risky to take for us. When my uncle and I set out to find another route, we had four offers within a short time. Just by my nose, so to speak, I opted for a guy who made a good impression on me. He looked trustworthy. I quizzed him. He had been a smuggler practically all his life. Before the Second World War, he had smuggled coffee and tea, and even just onions and other vegetables, into Austria as a part-time occupation. When the Nazis came to power in Germany,

he smuggled Jews into Hungary, where they were then relatively safe until the Nazis finally occupied the country. You had to be careful with this. You could easily fall into the hands of an *agent provocateur*, and if you did, that was just too bad. This guy evidently knew his stuff inside out. [Laughter]

COHEN: So you had a professional.

TSCHOEGL: Yes, absolutely a professional. We went with him. The fare was 4,000 forints each, which was quite a lot of money then.

It was just like in the movies. We took the train from Budapest to the Austrian border. The eastern part of Austria was occupied by the Soviets, as was all of Hungary by then. Our firm was the sole distributor for a sugar factory on the Hungarian side of the border. It was Austrian-owned and we were good friends of the owner. We had armed ourselves with letters to show that we went down there on business. If you got off the train at a station near the border and looked around uncertainly, you were apt to get nabbed right away by the AVO agents, who were everywhere. Our friend had been advised of our coming and was waiting for us at the station. He knew what we were up to, although we had not mentioned anything on the phone. You never knew who was listening in. That night we had a very nice champagne dinner at his house.

A taxi, part of the deal, picked us up next afternoon. We rode down to Lake Fertö—*Neusiedlersee* in German. The border goes right through that lake. It's a large lake, but it is quite shallow; it's only about two to three feet deep on the average, and it's covered with reeds over much of its expanse. We stopped on the road that led down to the lake as evening was falling. We waited for a while, and then we heard something like a birdcall. The taxi driver said, "This was the 'all clear' from our watcher. The AVO patrol has just passed along the road we must cross." There was an inn down at the lakeshore, rather well hidden from the road. It normally served food and drinks during harvest time. We had dinner there. It, too, was included in the fare. Our guide—I called him Rudi *bácsi,* Uncle Rudi, according to Hungarian custom—said that we had to wait until the moon went down around ten o'clock. When the time was right, he quickly shouldered our luggage. We each had just a small overnight bag. Rudi had told us

that we should not take with us any money or valuables, because if they caught us with any, the punishment would be even more severe than it might otherwise be.

There were six of us in the escape party: Rudi and a young helper of his, us, and two young men, politically engaged Social Democrats. At that time, the Communist Party had started to crack down on the Social Democrats and they were trying to inactivate the two guys. They had to flee.

We learned from them that Rudi had charged them 1,000 forints—not 4,000—because they couldn't afford the higher amount. I thought to myself that I was correct in my judgment—Rudi was a decent guy. He clearly risked his life, but he charged only as much as his customers could afford. For him, more was involved than just money.… Several months later, we learned through the refugee grapevine in Vienna that Rudi, with uncommon lack of caution, had tried to smuggle out an officer of the AVO who had fallen out with his unsavory crowd. Unfortunately, the AVO had watched that guy closely, and that led them to Rudi. He was beaten to death. This is what life was like then.

Back to the escape party. The six of us stepped into a small boat, and Rudi and his helper started to pole. The lake is too shallow to row and there is a danger of getting entangled in messy weeds if you do. We had a bottle of slivovitz—plum brandy—that we had been given for the road. My uncle and I started sipping it and pretty soon fell asleep. I woke up sometime during the night. Our guides had stripped to the waist, the sweat was pouring off their backs; they were poling like hell. At two o'clock we stopped in a little reed island. Rudi whispered, "We're already in Austria but we're still close to the border. The guards also have boats. They, too, can only pole; they can't use a motor so they're not very fast. However, they have machine guns mounted on the boats and they are quite ready to use them, Austrian territory or not. This part of Austria is Soviet-occupied, anyway. So please do not speak loudly. You may smoke but you must cup your hands over the lighted ends of your cigarettes." We had a little rest; we didn't need it, but Rudi and his helper surely did.

Soon we continued, and at four o'clock in the morning we landed in Rust, on *terra firma* in Austria. For whatever reasons, the church bells were ringing as if welcoming us. I'll never forget this moment in all my life. It was the 10th of October, 1948.

Rudi announced that breakfast and the bus fare to Vienna was included in the fare. Tea, salami, and ham and eggs were waiting for us on the kitchen table in a villager's house. We had little appetite; we were emotionally drained.

There was still some significant danger. At irregular intervals the Russians checked the buses, and we had no identification papers whatsoever. As luck would have it, the bus ride took us right through to Vienna. The city was then divided into five occupation zones: the British, American, French, and Soviet zones, and in addition the International Zone, the Inner City, which changed hands every month by rote. The British Zone was iffy, because you were never quite sure with the British. It was a standing joke among émigrés that the British couldn't distinguish between Slovenia and Slovakia. The French were OK, but a little difficult for other reasons. The grapevine had it that if you didn't speak fluent French you were considered to be a lesser human being. Although I did speak French well, I opted for the Americans, who were judged to be the best and the least complicated.

As soon as we arrived in Vienna, I went to a very dear Austrian friend and mentor of mine who lived in the International Zone. He had been advised already of my coming and welcomed me warmly. I called Sophia's friends in Italy and found that she and my little boy had made it safely to Intra. I heaved a great sigh of relief. They could have been turned back at the Hungarian border for whatever reason. If that had happened, I would have had to be smuggled back into Hungary. So my little family was safe, but we were far from each other.

Before I could go into the American Zone, I had to see the Austrian official who dealt with asylum seekers. I've told you that I speak perfect Austrian German. The official got up from behind his desk, shook my hand, and said, "Willkommen in Österreich!" He gave me the ration tickets and everything else I needed. Then he said, "But I'm terribly sorry. I'm truly sorry, but I have to fine you." I got worried; I saw myself in the slammer already—"Well, what's the fine?" He said, "Ten schillings." Ten schillings was a purely nominal fine. I gladly paid up and was now legally in Austria.

Then I went to see the American Zone authorities. The office was manned by another Austrian. He recorded my story and then he asked me, "What are your plans? How are you going to go from here into any of the western zones?" Vienna, with its five zones, was completely surrounded by the Soviet Zone. I said, "I don't know yet. But many people have done it, so I'll find out how to do it, too." He said, "If you need help with this, come back to me

and I will see what I can do for you." Talking to other Hungarian émigrés, I learned that his was known to be a safe route. I went back, paid this fellow twenty dollars, gave him the passport photo he had asked for—I had had it made quickly—and next day obtained an Austrian ID card with my photograph under an assumed name. He said, "This document is perfectly legal, except that there's absolutely no trace of it in the Ministry of Internal Affairs." Naturally the American authorities were well aware of this and condoned it, because they considered it in their interest that the refugees should get out of Vienna.

I could not stay with my friend in the International Zone. I would have had to move out of his place every two months out of five, when the International Zone was either under Soviet or under International Control. So I moved into the bed-and-breakfast that a widowed Austrian aunt of mine ran in the American Zone. Sophia was able to come to Austria from Italy with her current Hungarian passport sometime later. We discussed what to do. We would have liked most to go to the United States. An old family friend, who knew the American consul in Vienna well, got me an interview with him. The friend was a Prince Hohenlohe, and that he was a prince worked in my favor. Nobody would assume that a prince would be a Communist or that he would have a Communist as a protégé. You never knew whether somebody had, for whatever reason, denounced you as either a Communist or a former Nazi. Once you got into the records of one of the occupying authorities as a bad risk, your chances for immigration vanished.

I discussed my situation with the consul. I spoke tolerable English—not as I speak it today, but fairly well. He said, "You see, you're still technically an enemy alien." Hungary had declared war on the United States and no peace accord had as yet been signed. "We could put you on the quota for immigration to the United States," he continued, "but you would have to spend a *minimum* of two years on the waiting list." I didn't want to—I couldn't—wait. And he said, "But if you did select this route, how would you travel?" I said, "Well, I have a Hungarian passport," and I handed him my pre-Communist passport. It had expired, of course, and I had had it extended in Vienna in a coffeehouse. [Laughter] He smiled and asked, "How much did it cost you?" I replied, "Thirty dollars." He said, "Well, they didn't rip you off; that's the going rate." I asked him, "Would you accept it?" He said, "Yes, of course. We know that it has been changed, but we would accept it because we know that it is genuine despite its fake extension." Anyhow, the USA was out.

After Sophia had spent a couple of weeks or so with me in Vienna and in Salzburg, she returned to Italy. She had moved Adrian from Intra to Bolzano, because her friends in Intra were not well off and she did not want to burden them. In Bolzano she had other Italian friends, also hailing back to her time in Lemberg, and when she came to Vienna she had left Adrian in their care. I had to engineer it somehow that we would be reunited.

Vienna was then a hothouse of émigré activities. For another thirty dollars, I had an Italian visa put into my extended Hungarian passport. The visa was genuine, but I wouldn't have gotten it without a fixer. You know, these are things that I wouldn't do today, but then I had to. It was a question of survival. I went down to Italy to Sophia and Adrian and to meet her friends. Italy, however, was hopeless for us as a refuge. It was not kind to émigrés. We decided to move my little family up to Austria, to Seefeld in the Tyrol. Tyrol was reasonably safe; it was in the French Zone.

We spent a year in Austria. Sophia and Adrian had to remain in the Tyrol, out of harm's way. I visited them as often as I could, but I had to go back to Vienna to try and find a permanent country of refuge for us, and this had to be done in Vienna. I had started the process of getting Austrian citizenship. My mother was Austrian by birth. I would very likely have succeeded, but Austria after the war was a difficult country in which to live. With the help of our worldwide network of friends and relatives, I accumulated visas to Argentina, Brazil, Chile, Cuba, Peru, and Venezuela. However, I did not consider South America safe. We wanted an Anglo-Saxon country. The USA was not available. Canada was then not open, for whatever reason. Australia was. One problem with it was that if you were admitted, you were obliged to work for two years wherever the government assigned you. We decided to emigrate to Australia as still our best choice.

I brought Sophia and Adrian from Seefeld to stay with friends in Grundlsee near Alt-Aussee in the American Zone, and we went to see the Australian consul in Salzburg. We chatted amicably, and I felt confident that we would be accepted. A few days later, however, the Consulate informed me by letter that my application for immigration to Australia with my wife and son was rejected. No explanation why. Oh, boy! When we had gone to the Consulate, I had run into an old acquaintance from Budapest who acted as an interpreter there. I went back to see him and said, "Look, this is what happened to me. Have I been denounced?" He went to look at the file, came back laughing, and said, "You had not been denounced. The consul wrote on your

file, 'His application was rejected, despite the very favorable impression that he and his family made, because he is not the laboring type.' That's all." My friend was a Count Majláth and not the laboring type, either. We had a good laugh over this.

He also told me that Australia accepted only people for manual labor at that time. Armed with that knowledge, I went back to see the consul and said that I didn't care—if I had to go as a laborer, I would go as a laborer. I was really quite desperate. Also, I trusted myself that I would be able to improve my lot even before my obligatory two years of indentured labor was only half over. Well, the consul evidently liked me. We were accepted and eventually made it to Australia.

My mother had arrived in Vienna in the meantime—that's another story.

COHEN: Where was she coming from?

TSCHOEGL: When the war ended, Mother had been ethnically cleansed in Brünn, now Brno.

COHEN: What does that mean?

TSCHOEGL: Brünn became, for a while, part of the German Reich. The Germans occupied what is now the Czech Republic and called it the *Protektorat Böhmen und Mähren*—the Protectorate of Bohemia and Moravia. After the end of the war, the Czechs, who had taken part neither in the war nor in any resistance worth speaking of, suddenly became very nationalistic and expelled all German speakers. There were two kinds of German speakers in what had formerly been part of the Austro-Hungarian Monarchy. In Bohemia, there were the so-called *Sudetendeutsche*, who looked towards Germany. In Moravia, my family and others like us were essentially Austrians and gravitated towards Vienna. The German-speaking minority in Czechoslovakia was quite large and, as far as I can remember, lived peacefully together with the Czechs until the Hitler days.

My mother had never engaged in any anti-Czech or pro-German activities. Nevertheless, she was simply taken from her home one morning and was marched on foot with many others in a column to Vienna, with nothing except what she could carry on her person. She had to leave everything else behind. That was ethnic cleansing, although it wasn't called that at the time.

[Laughter] I must say, though, that the nationalistic excesses committed then have been recognized and condemned by the post-Communist Czech authorities.

When she arrived in Vienna, my mother was safe but destitute. She was staying with one of our numerous relatives. After a while we managed to bring her to Budapest. This was engineered by Sophia, whose proficiency in Russian again proved eminently useful. We had found a black-market operator who plied his trade back and forth between Budapest and Vienna. He first got Sophia to meet Mother in Vienna and a few days later took both back into Hungary. They traveled on a flatbed truck and crossed the border at dead of night. There were as yet neither Austrian nor Hungarian border guards there. The truck was stopped at the border by a Soviet military border guard, a *barishnya* [buxom woman] who wanted to see Sophia's and Mother's nonexistent *dokumenty*. Our black-market operator had secured for himself some Russian safe-conduct from his black-market contacts in the Russian occupation forces, but Sophia and Mother had none. Sophia quite simply bamboozled the *barishnya* in rapid Russian until she gave up in total confusion and waved them on. Without Sophia's knowledge of Russian, the *barishnya* would have had them unloaded, and no end of trouble would have ensued. It must also have helped that she surely realized that Sophia and Mother on the otherwise empty flatbed constituted no danger to the glorious Soviet forces. To give you a feel for what crossing this border or that in Europe at the time was like, imagine, if you will, that there would be borders in this country between all the states, and that, for example, you needed a passport to travel from California to Nevada or had to be smuggled across the state line. In the European Union, all this nonsense has now vanished, and it is there much as it is here in the United States.

Once she was in Budapest, I had no difficulty in getting Mother a permanent residence permit, since she had held Hungarian citizenship by dint of her marriage to my father. Thus, Mother was living in Budapest when we decided to emigrate to Australia. I spoke to her on the phone from Vienna. You could telephone, but you could not travel from one country to the other. I feared that we might never see each other again. We rather sorrowfully said our goodbyes, and two days later Mother arrived in Vienna.

She had sold much of her jewelry to get some money together. With it she procured the services of a guide, who took her to the [Austrian] border. All this she accomplished in a hurry. The border was already fortified with a barbed wire fence. The guide knew a place where there

was a hole in the fence. At that point he left her, saying only, "Austria is on the other side." Mother started to crawl through the fence. A little bit farther down, evidently, somebody else tried to get through at the same time and had set off flares that were attached to the fence at various points. She heard dogs barking and AVO men shouting. She continued scrambling across as fast as she could. And then she was in Austria. She got up and started walking. Soon she met a railway man in uniform, swinging a lantern. He said, "Did you just come over from the other side?" "Yes, just now." "What are you going to do now?" She said, "I have to get to Vienna, but I have no money. I really don't know how to do it." The guy gave her ten schillings for the bus fare. They had never met before. That shows how you can always find people who are decent and will help you in your need, especially in the face of a common hated and despised enemy.

Incidentally, I have a piece of that barbed wire. I got it, nicely housed in a box that formerly contained a pearl necklace, as a gift from a friend who was present at the ceremony when this hated sign of occupation finally came down after the fall of the Berlin wall.

When Mother arrived in Vienna, I went back to my contact and for another twenty dollars he gave me yet another legal but fake Austrian ID for her. My own ID had served me well. Rather stupidly, in hindsight, I crossed the so-called Enns Line eleven times, the last time to get my mother across. The Enns River marked the border between the Soviet and the American zones of occupation.

COHEN: And yet with all this, you never had a money problem? You were able to get money?

TSCHOEGL: I did not have a *serious* money problem. My close friend in Vienna, to whom I referred earlier, was rather well-to-do and lent me money whenever I needed it. I paid it back later. We lived very frugally in every respect, staying with relatives or friends practically everywhere, so we didn't need to spend much money on lodgings, not too much on food, and none on clothing. We did have $2,000, which we had obtained by selling the furniture in our Budapest penthouse just before we left. But we had to smuggle this money out of Hungary. We gave it to somebody who was recommended to us but whom we didn't really know well. He was a Jewish businessman who had gotten out of Hungary in time, acquired a consulship for Monaco, and thus traveled on a Monegasque diplomatic passport. At that time $2,000 was much more

than it is now. Alas, we couldn't get it back from him for a while; apparently he had some money problems of his own. But eventually the $2,000 materialized, and I paid back whatever I owed. And all four of us—Sophia, Adrian, Mother, and I—set off for Australia, embarking in Naples.

COHEN: This was 1949?

TSCHOEGL: This was 1949.

COHEN: Why did you choose Australia?

TSCHOEGL: Well, I didn't want to go to South America, because I did not like the political situation there. Also, I did not speak Spanish, although I could read it fairly well; languages were never a problem for me or for Sophia, either, so that was not the main reason. We seriously considered going to Argentina, where I had another uncle. He had been the last *chargé d'affaires* of the Austro-Hungarian Monarchy in Buenos Aires and had stayed there after the Monarchy collapsed. It was he who had obtained the Argentinean visa for us. Many Hungarians I knew had already gone to Argentina. But I had no diploma. I had no money. And I was afraid of losing face. In Australia nobody knew me, so I couldn't lose face.

Another reason for choosing Australia was that my uncle and former partner, with whom I had escaped from Hungary, was already in Sydney. He was working as a laborer at Taloil Industries, a small factory owned by a Slovak who spoke Hungarian well and had hired a number of Hungarians who, like my uncle, spoke no English. I presented myself to the owner and he forthwith asked for me to be assigned to Taloil Industries as an indentured laborer under the terms of my contract. In this way, I could stay in Sydney instead of being sent to cut sugar cane in Queensland. I spent eight months at Taloil Industries, working as a yard hand first and then as a plant operator. This is when I ran, and also had to clean from the inside, one of the evaporators that Florent Robert, my great-great-grandfather, had invented.

In Sydney I met a classmate from the Technical University in Budapest. His family was Jewish and had been able to wangle exit visas. He had obtained his degree and was working as a chemist at Taubman's Ltd., a paint factory. He said, "I'm pretty sure that I can get you a job there." And that is what happened. With him at my side, I presented myself at Taubman's and

they hired me right away. I then went to the competent authority and told them that I had had three years of chemistry and that Taubman's wanted me. They assigned me to Taubman's without any difficulty. The fact that I spoke English at least well enough counted for a great deal. The official commented on that. This is another example of how useful and important it is to speak languages.

Now I was no longer a laborer but worked as a chemist! There wasn't much chemistry in what I had to do. [Laughter] But it meant I could work during the day and at night I could attend Sydney Technical College to finish my studies. It took me two years of night work to get a diploma, and after that it took me another two years to get a BSc from the newly established New South Wales University of Technology.

COHEN: Were you working in this factory the whole time?

TSCHOEGL: No. While I was working at Taubman's and getting my diploma from the Sydney Technical College, I was introduced to Professor A. E. Alexander of the university. He took me on as an assistant on one of his research projects. After I had obtained my BSc, I was able to work for my PhD degree with him. Alexander was an authority in the field of surface chemistry and a wonderful man to work with. He had been professor of physical chemistry at Cambridge University before he accepted the professorship at the NSW University of Technology. The latter soon became the University of New South Wales, and I obtained my PhD degree from this institution in 1958, at forty years of age.

A little anecdote here: After the conferring of degrees at the University of New South Wales, a few friends gathered at our house to celebrate. I was still in my resplendent black-and-red academic robe, modeled after the Cambridge gown, when to everyone's merriment little Adrian, who had until then only seen colorful ecclesiastical robes, earnestly asked me, "Daddy, are you a *bishop* now?" I lifted him up, kissed him, and said, "No, Ady, I am just a doctor of philosophy!"—again to everyone's merriment.

Of course, I had to earn money all the time that I pursued my studies. I never would have been able to manage otherwise. I had to support my family, including my mother. She looked after Adrian, because Sophia was working also.

Begin Tape 2, Side 2

TSCHOEGL: I think I have mentioned that Sophia had worked as an assistant physician in Budapest while being enrolled at the university to obtain her MD degree. She did complete her studies and had put several of the mandatory internships behind her. She was never able to obtain the degree, however, because we had to leave Hungary in a hurry. Since she then spoke no English, she did not try to complete her degree in Sydney. This would have required repeating almost all of her course work in English. Although Sophia hardly knew more than a dozen words of English, the Pathology Department of the University of Sydney took her on at once as a pathology technician. She was served tea on white linen and with silver cutlery, while I dirtied my hands as a laborer; she then actually earned one pound more per week than I did. [Laughter] Sophia already spoke Polish, Russian, and German, and she had picked up quite a bit of Hungarian. She soon acquired enough English also and rapidly became highly proficient in her job. She worked the whole time that we lived in Australia.

Our life in Australia was not too easy at first, primarily because of the difficulty of finding suitable accommodation. The country accepted something like 120,000 immigrants per year and never considered that these people had to live somewhere. Of course, they were supposed to labor mainly in the cane fields! The rents were pegged during the war, so nobody built, because it wasn't profitable. Later they revised all that, and it became easier to find a place to stay. We had found a very attractive house on the shore of a bay in spectacular Sydney harbor. The owner, Mrs. Frazer, a thoroughly pleasant older lady, lived on the first floor. We rented the upper floor from her. The rent was not really high but still too much for us. Luckily a wealthy young Austrian whose family I knew back there had come to Australia just recently to learn something about wool. His family owned a wool-spinning mill, among other things. He looked us up and was only too happy to room with us at Mrs. Frazer's. He contributed more than half of the rent. Sophia, always a matchmaker, arranged for him—long distance, so to speak—to marry a first cousin of mine, after he returned to Austria.

About a year after we had moved into Mrs. Frazer's house, she needed to sell the house, and so she asked us to try and find another place to rent. With some effort, we did find a small house elsewhere, but the owner demanded 500 pounds key money. Because of the impossible housing situation in Sydney, you generally paid a rather low rent—we were to pay a pound and a half per week, less than a tenth of what we earned—but you had to pay key money to get the key

to the house. This was illegal, and you had to do it furtively. Mrs. Frazer, anxious to sell the house, advanced us the money, which I then paid back in monthly installments. So we moved into 263 Sailor's Bay Road, where we lived until we left Australia.

While I was working for my doctorate, Professor Alexander had found me a job as senior research officer at the Bread Research Institute of Australia. The institute was financially supported primarily by the baking industry of Australia but obtained considerable support also from CSIRO, the Commonwealth Scientific and Industrial Research Organization of the federal government. Under the terms of its agreement with the CSIRO, the Bread Research Institute was obligated to undertake a certain amount of basic research. The institute seconded me to the university—that is, they paid my salary while I was working on my PhD on the surface chemistry of cereal proteins under Alexander's guidance. During my last year with him, I actually did my experimental work at BRI, where I had built an instrument to measure the rheological properties of monolayers of cereal proteins at the air/water or oil/water interface. I obtained my PhD in what was essentially a study in surface rheology [*Physicochemical Studies of Wheat Proteins and Dough,* Sydney, December 1958]. Rheology is the science of the deformation and flow of matter, which was then still pretty much in its infancy. I became greatly interested in this area. Fairly soon, it became my main field of scientific endeavor.

My time at the Bread Institute was a happy one. The director, Eric Bond, an eminently capable man, was well inclined towards me and helped me wherever he could. I learned a lot, and—I think—did good work there. My job at BRI, after I had finished working for my PhD, involved, among other things, some pioneering work in the rheology of wheat flour dough. This work required the use of statistics, and I took a course of it at the university. Getting acquainted with statistics opened a whole new world for me. I think at least the rudiments of statistics, like those of economics, should be taught in high school. You understand the world around you in a more fundamental way if you understand these two so-called dismal sciences. I had earlier also taken a course in microbiology, because I needed it to complete my course requirements. This, too, opened new vistas for me. In microbiology, sterility largely takes the place of high purity in chemistry. Both courses were well taught, and to this day I am glad I took them. I would now also add thermodynamics as a tool to understand the world better. Having written a book on it, I nowadays find myself thinking thermodynamically quite often.

NICHOLAS W. TSCHOEGL

SESSION 3

May 30, 2001

Begin Tape 3, Side 1

COHEN: Now tell us about your adventures in the New World, the time when you came to America.

TSCHOEGL: Well, Australia is as much, if not more, "New World" as are the Americas. I had discussed with Eric Bond, the director of BRI, the possibility of spending a sabbatical year abroad to further my knowledge of rheology. He proved receptive to the idea, and I started working on implementing it. I had just published an article on the theory of linear viscoelastic behavior, the basic theory underlying rheology ["Characterization of Linear Viscoelastic Behavior by Respondance Functions. Stress Circuit Theory." *Kolloid-Zeitschrift* 74:113-123 (1961)]. This article became the seed of my later 770-page treatise on this topic. I sent a reprint of the article to Professor John D. Ferry at the University of Wisconsin, a world leader in the field of the physical chemistry and rheology of synthetic polymers. I asked him if I could get an appointment with him for a year of advanced study. I had been working in protein chemistry, and I thought—quite mistakenly, as it turned out—that if I learned something about synthetic polymers, this would help me in my work on proteins. Proteins, as you may know, are also polymers; however, because of structural differences between proteins and synthetic polymers, the two fields form fundamentally distinct areas of study.

Professor Ferry was then chairman of the Department of Chemistry at the University of Wisconsin in Madison. He replied at once and told me that he liked my article very much ("You said a number of important things for the first time") and offered me a position as a project associate at the University of Wisconsin.

COHEN: You had never been to the United States before?

TSCHOEGL: No. I came to the United States for the first time in 1961. I had read several of Ferry's publications and through them became convinced that he was the man I wanted to work

with to further my knowledge. It turned out to be an excellent choice. He is only six years older than I am, and we hit it off, so to speak, from the beginning. His text, *Viscoelastic Properties of Polymers,* saw three editions and from its inception became the leading publication in the field.

COHEN: Did you enjoy being in the United States?

TSCHOEGL: Both Sophia and I enjoyed it right from the beginning. We spent two years in Madison, Wisconsin. Life there was very, very nice, and I derived immense pleasure and satisfaction from working with John Ferry. He had an encyclopedic knowledge of his field and combined this with a vast range of interest and with a gentle and understanding nature. My working with him developed into a friendship of which I remain extremely proud.

These were fruitful years. I published several papers with Ferry, some together with other people in his group and some as sole author. Originally, I had planned to spend a year in Madison and then return to Australia—actually, doing a trip around the world. Once you leave Australia for the United States, you might as well go around the world. [Laughter] I hadn't been too much into the first year of my work with Ferry when I decided that the USA was the country for me. Sophia wholeheartedly supported this decision.

It was primarily the rhythm of life here, the go-ahead attitude that was rather different from the more leisurely attitude in Australia, the prompt supportive response to anything reasonable that one wanted to do. There was none of that "Hey, go slow, let's see if this really *can* be done." Everybody's attitude seemed to be "OK, let's see how we can *do* this." So I set out to do whatever was needed to enable us to stay in the United States. In those days it was relatively easy for an alien with the right qualifications to obtain permanent residence status. At the time, the United States used nationality quotas, and the Australian and Hungarian quotas were filled, but because I was born in Židlochovice we were admitted under the Czech quota. *Sputnik*—which is pronounced *Spoot-nyik*, not *Sput-nick*, as is common in this country—had launched the USA into orbit. People were wanted everywhere for work on scientific and engineering projects.

The work on which I was engaged with Ferry was funded by the Office of Naval Research. It was going well, and Ferry was happy to have me stay on for another year. He wrote to the ONR saying that he needed me to continue and explained that this required that I

obtain permanent resident status. Under these circumstances, all four of us, Sophia, Adrian, Christopher, and I, obtained it quite promptly.

When I had finished my work with John Ferry, I immediately got several job offers. The most appealing among these were one from Stanford Research Institute and one from the US Department of Agriculture. I had visited the department's laboratory in Berkeley when I was on my way to Madison from Los Angeles, where we had made landfall coming from Sydney. The USDA people knew about my work at the BRI on the rheology of wheat flour dough and would have liked me to continue this work in Berkeley. However, I preferred the offer from Stanford Research Institute. I worked there with Thor Smith, and we became close friends. I spent two years at SRI. The USDA readily granted me a substantial amount of money to work on dough rheology. With Thor Smith and others of our coworkers there, I wrote several papers on this work. It was something rather new, because dough rheology had rarely been treated in a scientific way before and we did it.

COHEN: You were living in Palo Alto?

TSCHOEGL: In Menlo Park, right next to Palo Alto. We first stayed in an apartment not far from SRI and then bought a charming house in a lovely area just as close. We quickly made good friends. In the United States, we felt at home more readily than we had in Australia. We were at home in Australia, too; there was really no problem about that. We were well treated. But we could never escape the feeling that we were considered *different*. It was an aura that always hung around us. There was even a word for this: They officially called us "New Australians." This was well meant but really did not sit all that well with most newcomers—and not with the Old Australians, either. I don't know how the Aborigine Australians felt about it. Here nobody talks about New Americans. [Laughter] Australia was not yet used to highly educated immigrants who had been well-to-do at home and came to Australia because of political persecution. Australians hadn't had the experiences that we had during the war, and we were not on the same wavelength with them, so to speak. We essentially lived in a European ghetto. Some Jewish immigrants had preceded us, because of persecution by the Nazis, but there were not that many of them. The usual European immigrants to Australia had generally been poor and uneducated Bulgarians, Southern Italians, and even similar Anglo-Saxons and Irish, who had

come to seek a better life. The immigrants from the British Isles were not necessarily the cream of the crop. In fact, when I talked to an English friend before we emigrated and told her we were emigrating to Australia, she said, "What? To the rubbish bin of the empire?" [Laughter]

Here are two revealing little anecdotes. I needed a bank loan to finance our trip to the USA. I had no collateral. We knew the secretary of the Bank of New South Wales. This was one of the largest banks in the country, and the secretary was the man who ran it. He readily agreed to guarantee the loan. I saw the manager of the local branch where I had my account. Somewhat perplexed, he inquired how I had gotten the secretary's signature on my papers. I told him that we knew the secretary socially. Incredulously he asked, "You, an immigrant, know the secretary of the Bank of New South Wales *socially*?" Needless to say, I obtained the loan forthwith, with the terms I required. This man was educated, he had a responsible job, and he had not woken up yet to the fact that his country had opened its doors to a new wave of immigrants. Such a gaffe could not have happened in the States. Here all anybody cares about is what manner of person you are, what you can do, and how you can do it. We liked that very much.

Next to us in Sailor's Bay Road in Sydney lived a couple of retired teachers. One evening they invited us to tea. Somehow the conversation turned to the Soviet occupation of Budapest. Being still naïve, we told them about how, during the first fourteen days when they had been given the right of pillage as a reward for taking the city, the Soviet soldiery destroyed, raped, robbed, plundered, and killed at will. The good ladies listened awestruck. "But why didn't you call the police?" asked one. After that, we generally refrained from being too forthcoming about our experiences.

COHEN: Now, had you gone to Middle America somewhere, it may have been more like that here, too.

TSCHOEGL: Possibly. Fortunately, Menlo Park and the surrounding area are quite cosmopolitan. But Sydney was also cosmopolitan. [Laughter] Of course, things have changed very much in Australia since then. We have been back a few times and we're always amazed at the rapidity and the thoroughness of the change.

COHEN: Did you have your mother with you?

TSCHOEGL: No. When we left Australia, we thought we were only going for a year and would then return to Sydney. But then she, too, decided to leave Australia and went to Germany, where she had a good friend from her earlier days in Brünn. She lived in Augsburg until she died peacefully of a heart attack during the night in 1972. We had been in touch, of course. She visited us here; we visited her there.

When we came to the United States, Sophia's visa didn't allow her to work. And neither was it really necessary anymore, because I earned enough for us to live reasonably comfortably. Besides, she had to care for our two boys. Mother had looked after them in Sydney.

Maybe I should say something about my coming to Caltech. Max Williams, a professor of aeronautics, had a great deal of funding from the air force for a rather large research project concerned with the properties of solid rocket fuels. These consist essentially of a matrix of a rubberlike synthetic polymer filled with particles of a powerful oxidant, such as ammonium perchlorate, apart from other minor ingredients that promote burning. The air force had at that time developed a great deal of interest in solid rocket fuels. This, by the way, was another effect produced by *Sputnik*. The alternative to solid rocket fuels is liquid rocket fuels. These are generally more powerful but far more difficult to handle, and they require an entirely different and quite costly engineering approach. Because of his air force project, Max Williams was trying to recruit for his group at Caltech someone with expertise in polymers. He had persuaded the materials science group—it consisted solely of metals people at the time—that Caltech needed a presence and strength in synthetic polymers.

Max came to see me at SRI, where Thor Smith and I had already started some work on solid propellant model systems. He asked whether I would like to come to Caltech. At first, I wasn't—we had settled in nicely in Menlo Park and had bought a lovely home in Felton Gables. And I had never really taught before. But I talked about it with Sophia, and eventually, again on a Sunday resting after lunch, we just mulled over it again and I suddenly said, "Well, let's go to Pasadena." And so we went. However, this was not the snap decision that it might appear to have been. Both Sophia and I had given the idea a great deal of thought and we were both ready for the decision. I was appointed associate professor of materials science in the Division of Engineering and Applied Science in 1965.

COHEN: Ah, so this was the beginning of your teaching career?

TSCHOEGL: Well, yes. I had given a number of talks in Australia, and in Madison and Menlo Park in this country, and thus had gained a good deal of lecturing experience. I really liked lecturing. But I had not taught a course, except one in Australia, if you could call that exercise in futility "teaching a course." Just to earn a little extra money, I tried to teach a class of electroplaters some basic chemistry at Sydney Technical College, a respectable but not a high-grade academic institution. It was a bitter experience! When I lectured to these prospective electroplaters, they would just sit there and stare at me. I did not have the feeling that I was getting through to them at all! No notes were taken, not a hand was ever raised, not a question was ever asked. I tried to do my best, believe me! At the end of the course, I had to give an exam. Everybody failed. [Laughter]

I felt that there had to be something wrong with me as a teacher. I went to see the man who was in charge of the vocational courses and told him, "I was supposed to teach these guys chemistry that would qualify them for their jobs as electroplaters, and they just sat there. They don't know anything. They all failed miserably and so did I." He knew most of these guys from other courses he administered, and he said, "Well, let's see. Let me have a list of these people. Oh, I know this guy. Let's give him a two. We'll give this guy a three." The grades ranged from one to four, one being the best. Thus everybody passed. [Laughter] I asked incredulously, "But how can you do that?" He answered, "Oh, don't worry about it. They need the diploma to get their certificate, but they will work in their fathers' shops and they will learn there whatever they need." So we passed them all. But that's not what teaching is all about. Of course, at Caltech it's different.

Max Williams arranged for me to come to the institute for interviews. I spent a day here seeing numerous people singly and in groups. Evidently I made the right impression, because in due course I got the letter of appointment as associate professor of materials science in the Division of Engineering and Applied Science signed by then Caltech president Lee DuBridge. I still have it, and I am proud of it. We moved to Pasadena in September of 1965.

In June of 1966, Max Williams—with whom I thought I would be collaborating—left, to take up a position of dean of the School of Engineering at the University of Utah. It turned out that he wanted me on board because he needed someone to take over his air force project. Suddenly I was in charge of a large project with a group of people. I went out to the air force's Rocket Propulsion Laboratory at the Edwards Air Force Base to see Frank Kelley, the project

monitor for Max's project. Max had, of course, already prepared him about my assuming directorship of the project at the institute. Frank had agreed to this—sight unseen, as it were. He turned out to be a splendid chap, a very capable individual, and again we hit it off right away. We are still good friends, although we see one another only sporadically nowadays.

I fostered good relations with the people at the Edwards Air Force Base. From time to time, I invited Frank, often with some of his crowd, to come to Caltech and get acquainted with those in my group who worked directly on the project. I also took my people out to the Rocket Propulsion Laboratory. Among other interesting things, we witnessed the trial firing of a tethered rocket, which was truly awe-inspiring.

The work of my group was concerned largely with elucidating the behavior of filled elastomers—rubberlike polymers—serving to model solid propellants. To work on actual propellants would have been prohibitive, because expensive provision would have had to be made to guard against the danger of explosions. And it was, indeed, not necessary. We simply used the same matrix polymers that were used in actual propellants and filled them not with oxidants but with inert salts, such as sodium chloride.

The propellant, called the grain, in a solid-propellant rocket is actually a thick-walled hollow cylinder. When the rocket is fired, it burns from the inside surface of the grain towards the casing. The trajectory of the rocket is determined largely by the design of the grain. If a crack develops in the grain, the flame proceeds into it. This causes unstable burning, which causes the rocket to go awry. To design the grain, its mechanical and ultimate [failure] properties are tested by subjecting a so-called dog-bone specimen of the propellant to a uniaxial stress at ambient atmospheric pressure. However, as the grain burns, a pressure develops in the interior and this affects the mechanical and ultimate properties of the grain. This was of course known to the designers, who therefore designed the grain largely by trial and error. I had Caltech's Central Engineering Services construct a large bomb, complete with pressurizing equipment. Into the bomb we placed a uniaxial testing mechanism. This allowed us to carry out the tests at any desired pressure within the range expected to occur during firing. Without going into the technical details, let me just briefly remark that we found that under firing pressure the propellant indeed behaved quite differently from the way it behaved under atmospheric pressure. Tests made under the latter therefore were essentially useless. Our work showed the way to obtain the information needed for designing better solid-propellant rockets.

The project was not classified, but we had to get air force approval before submitting a paper for publication. I never had any difficulties with this. However, this general topic later became a bone of contention between Caltech and the Department of Defense. Caltech believed—rightly so, as far as I am concerned—that it could not be subject to outside control in its academic dealings. The issue was eventually resolved in the institute's favor. If I remember correctly, this involved not only Caltech but academic institutions all over the country.

NICHOLAS W. TSCHOEGL

SESSION 4

June 8, 2001

Begin Tape 4, Side 1

TSCHOEGL: On July 1, 1967, I was promoted from associate professor of materials science in the Division of Engineering and Applied Science to professor of chemical engineering in the Division of Chemistry and Chemical Engineering. I introduced courses concerned with the mechanical, and generally the engineering, properties of polymers. To round out the course structure, I enlisted the help of Alan Rembaum and Robert Landel of the Jet Propulsion Laboratory for courses on subjects with which I was not sufficiently conversant then. Rembaum taught organic polymer chemistry and Landel taught ultimate properties—fracture mechanics, why and how polymers might fail. Both of them were appointed senior research fellows in the Division of Chemistry and Chemical Engineering while maintaining their appointments in the polymer section of JPL.

COHEN: Did you have students at this time? Graduate students?

TSCHOEGL: Oh, yes, I did. I had postdocs, graduate students, one or two lab assistants, and a full-time secretary. The general subject of our work was mechanical properties of polymeric materials.

Perhaps I should say here something about polymers, because this topic will come up here again and again. Polymers are long-chain molecules. Typically, a polymer is composed of monomers that are strung together into a long chain. If more than one monomer forms the chain, the sequence of the monomeric units composing it is generally random. Such a polymer is referred to as a random copolymer. Although there are also newer polymers with different chain structures, homopolymers and random copolymers form the largest group of industrially important long-chain molecules.

The chain is normally very long indeed. If you scale up its diameter-to-length ratio, you might compare it to—let's say—a twenty-yard-long clothesline, though that would still be only a small polymer. Thus polymers are really very long and therefore they are also called

macromolecules, "macro" really meaning "long," not "large," in Greek. They can take up any number of configurations—they can fold up, they can coil, or they can crystallize.

Every polymer has an overarching property known as the glass-transition temperature. This differentiates between the two main categories of polymers in about the same way as gender differentiates between boys and girls. [Laughter] That's very important. Those whose glass-transition temperature is high—say, above 100º centigrade—would generally show glasslike behavior at room temperature. The random thermal motion of the chain segments is frozen in, as it were. Those with a low glass-transition temperature—say, below - 40º centigrade—generally exhibit elastomeric (rubberlike) behavior at room temperature. The segmental motions are then more or less free.

In a rubber, the elastomeric chains are interconnected by chemical crosslinks at certain points to form a three-dimensional network. This network is a crucial structural feature of a rubber. In a typical rubber, the crosslinks are permanent. Once you disrupt them, the three-dimensional network is destroyed. An ordinary rubber band illustrates this. You can only extend it as far as it is designed to yield. When you exceed that point, it breaks. However, when you let go before it breaks, it snaps back into its original shape. Were it not crosslinked, it would just keep extending indefinitely, getting thinner and thinner until it separated.

The term "rubber" should be reserved for a network of chains interconnected by crosslinks; however, the terms "rubber" and "elastomer" are often used interchangeably and often without distinguishing between crosslinked and uncrosslinked materials with a low glass-transition temperature. To add to the confusion, the term "elastomer," applied in a general sense, comprises natural rubber, which is a plant product, and the many synthetic elastomers. The latter resulted from the drive to replace the former after the outbreak of World War II, when the United States, Britain, and their allies were denied access to Southeast Asia's natural rubber sources. Natural rubber is a form of polyisoprene containing other constituents in addition. Industry has learned to produce synthetically a whole slew of "rubbers"—even synthetic isoprene—or synthetic natural rubber, a contradiction in terms. For various technical reasons, synthetic polyisoprene is, however, inferior as a tire material, one of the main applications of elastomers.

A rather new—at that time—form of elastomer is the elastomeric tri-block copolymer. In block copolymers, you have one kind of polymer forming part of the chain, and then you add another kind of polymer to it, and then possibly another one again. Block copolymers therefore

show more than one transition. The blocks have generally completely different temperature-dependent as well as time-dependent properties. In an elastomeric tri-block copolymer, two relatively short end blocks have high glass-transition temperatures and behave like a hard glass at room temperature; the much longer middle block has a low glass-transition temperature and is flexible like a rubber molecule at room temperature. The high and low glass-transition blocks cannot mix with each other, even though they are parts of the same molecule; thermodynamics forbids it. The glassy chains form tiny blobs that hold the others—the rubbery ones—together in a three-dimensional network. These physical crosslinks act similarly to chemical crosslinks.

Elastomeric block copolymers thus show many of the features of a typical elastomer. There is a crucial difference, however. When you bring an elastomeric block copolymer to a temperature above the glass-transition temperature of the glassy blocks, the three-dimensional network structure falls apart. You can then injection mold or dissolve the block copolymer. However, the network reforms when you cool the melt down or you precipitate the dissolved polymer from solution.

Synthetic polymers, first created about 100 years ago, are a triumph of man's inventive genius. Today polymers are ubiquitous. Just look around you. Virtually everything you see is a polymer or contains one in its makeup. Without rubbers, for example, you wouldn't have bicycle or car tires, and planes could not take off and land.

Well, so much here about polymers in general. With respect to the academic activities of my group, I practiced some of what I had learned from John Ferry at Wisconsin: We had weekly research meetings in which either somebody from the group spoke—so that the others would know what was going on in the group—or we had an invited speaker, a visiting fireman or a colleague from Caltech or from JPL. Their talks were intended to widen the exposure of the group to the wild and woolly world of polymers.

Everybody was required to a write a monthly report, half a page to a page. Some could do this easily; for others it was a problem—above all, for some of the Asians who didn't write English very well. I painstakingly went through any drafts that needed it with the respective authors. In this way I taught them not only some English but also the art of concisely and logically structuring their reports. The writing of these monthly reports was not popular even with several of the students who could write them well. Some considered them as superfluous extra chores. I made it clear, however, that later on they would *always* have to write reports,

wherever they were. Either they would have to apply for funding and write proposals and reports in an academic environment or they would just as surely have to do the same if they were finding themselves in an industrial setting.

We did many things as a group also on the social level. I tried to be true to the Caltech lore that we treat undergraduates as graduate students, graduate students as postdocs, and postdocs as colleagues are treated elsewhere. The whole group ate together in the Athenaeum once a week. From time to time, with Sophia's enthusiastic participation, we arranged ethnic dinners at our house. Those who came from foreign countries were able to show off their cuisine. That was a very popular group activity. When our schedules permitted, we went on outings—to Santa Barbara, to the Mojave Desert, and other places. Once we went to the desert to see the wildflowers, and Nobel laureate Rudi Mössbauer came with us. Sophia and I had befriended Rudi, who at that time often came to Caltech. The group was very impressed to spend the day in the company of a Nobel Prize winner.

COHEN: How big was the group?

TSCHOEGL: At its largest, it comprised ten or so individuals, apart from myself.

COHEN: So it was a good-sized group.

TSCHOEGL: Most of the time. My younger son, Christopher, also ate regularly with us in the Athenaeum. He held a job as photographer at Caltech while he attended Art Center College of Design here in Pasadena, learning to become a professional photographer. He joined the group as an unofficial member and in fact formed several long-lasting friendships with some of its official members. After he graduated from the art center, he still returned often to have lunch with us.

At American universities the social life of the professors revolves quite a lot around the university. Social activities involve colleagues and, of course, students. In Europe there is nothing quite comparable to this. It seems to be customary in the English-speaking world above all others. In Canada and Australia, it is more or less as it is here. My first academic mentor, Professor Alexander, an Englishman from Cambridge, often invited his students and postdocs to dinner at his home. But there is even more of this academic socializing going on in the USA.

Sophia and I always had a nonacademic circle of friends, but our life also revolved a good deal around Caltech. Many faculty members became friends, and we regularly exchanged visits with many of them. Sophia became an active member of Caltech's Women's Club. She led the group referred to as the Gallery Goers to art galleries, art exhibits, and private collections. She found that the Caltech name opened many doors for her. She worked tirelessly at her job, often helped by Colene Brown, the wife of Caltech's then president, Harold Brown. These activities gave her the opportunity to introduce many younger faculty wives to her special interest, contemporary art.

Another organization where she used her social skills to advantage was the Caltech Associates, a group of wealthy patrons that support Caltech through their membership dues and in many other ways. The membership dues represent unrestricted funds, of which the institute never has enough. It is easier to obtain $1,000,000 for a building that is named after the donor than it is to obtain $100,000 of unrestricted funds. To become a member, a prospective Associate has to be "induced" by another member. Sophia had persuaded Henry Lee, a wealthy industrialist of our acquaintance, to join the group. He was so pleased with this connection that he made both Sophia and me lifelong members out of gratitude. He later served a term as president of the Associates. Sophia then set out to induce a large number of our friends, and of their friends, and of the friends of their friends, to become Associates. Bill Corcoran, then head of institute relations, acknowledged her outstanding efforts in a formal letter of recognition to her.

We enjoyed our social connection with Caltech very much. It gave us a feeling of belonging, of being part of an institution. I consider this to be rather important. Traditionally, you were part of your tribe, clan, ethnic group, or whatever else that you did not join of your own free will. Today this is overridden by your being part of an organization you chose yourself to belong to, be it your church, synagogue, mosque, social club, or—most important for most of us—the institution where you work. If it's too large, that's too bad. Caltech is ideal from this point of view.

I had other activities also, of course. I consulted quite a bit. And I traveled widely, in the United States, Japan, China, Australia, the Soviet Union, and most countries in Europe. I regard that as one of the extra bonuses that research and professorship at Caltech entail. I liked to travel and so did Sophia, who generally accompanied me on our travels outside the USA. I attended

meetings and visited various academic and industrial establishments. Either my grants and contracts or the institute defrayed the costs of these trips—except, of course, when they were part of my consulting activities. I attended virtually all the meetings of the Society of Rheology and all the international congresses on rheology. I also attended many of the IUPAC [International Union of Pure and Applied Chemistry] Macromolecular Symposia.

In 1974 I was invited by the Brazilian Academy of Sciences to chair a session at the Macromolecular Symposium held in Rio de Janeiro. The academy defrayed my costs in Brazil but did not come up with travel money. I asked my travel agent what I was facing. He consulted his computer and said, "Well, if you fly directly from Los Angeles to Rio and back, it will cost you X dollars, but if you travel around the continent of South America it will cost you $10 *less*." The NSF [National Science Foundation] was then supporting one of my projects about which I was to talk in Rio. I asked if they would allow me the money, and if it was OK if I took the trip around the continent. They readily agreed. Those were the days! I organized a passel of colleagues and their wives, including John and Barbara Ferry, who were all going to the IUPAC meeting, and we set out together from LAX [Los Angeles International Airport]. We flew first to Panama City and then to Bogotá, from there to Manaus on the Rio Negro, a tributary of the mighty Amazon, and then on to Rio. After the conference, Sophia and I spent a couple of days in São Paulo to visit with an aunt of mine. We rejoined our travel group in Buenos Aires. After a few days there, we went on to Bolivia, Peru—Lima and Cuzco—then to Mexico City and home. It was a most delightful trip in good company.

Unfortunately things have changed. The airlines no longer offer such cost-effective travel, and the funding agencies would no longer allow them either, because of the supposition that the prospective grantees might elect to go to a meeting only—or primarily—to enjoy a junket.

Among scientific conferences, the outstanding ones for me were the Gordon Research Conferences; they were named after the Rhode Island University professor who invented them, and each was usually devoted to a particular subject. I do not know whether the format is still the same, but in my time attendance was limited to about 100 participants invited at the discretion of the chairman, who arranged the program of the conference. You could, of course, signal your interest in any conference and the chairman then invited you if he thought that you could contribute meaningfully to the subject. Usually the vice chairman became next year's

chairman, and a new vice chairman was elected at the business meeting at the end of the conference. The conferences were held during the summer in New Hampshire. The participants lived in the dormitories of one of the many private schools, such as Andover, Exeter, and others, which were empty during the summer vacation. There were normally two one-hour lectures in the morning and another one in the evening. The afternoons were free for various activities—hiking, swimming, playing tennis, gathering wildflowers, or just lazing. All meals were taken together. The aim was that the participants should socialize, exchange ideas and information, and establish contacts. It is quite possibly the best format for a scientific conference that anyone had ever devised. I attended one or the other of the conferences in the East virtually every year for many years.

At some later date, the Winter Gordon Research Conferences were established in the West. They were usually held in Santa Barbara, initially in the Miramar Hotel. The format was the same as that of the eastern conferences. In 1976 I was the chairman of the Winter Gordon Conference on polymers; I had been vice chairman the year before. I took great pleasure in arranging it, and I am proud to say that it was a success. It was customary to thank the chairman at the end of the conference, but I obtained a standing ovation. One of the board members of the conferences had attended it. He came up to me at the end and said that it was a "humdinger of a conference." These conferences were another of the highlights of life at Caltech.

I also had several stints as visiting professor. In 1971, I spent the months of June to August in Delft in the Netherlands as a visiting professor at the Technische Hogeschool, where Hermann Janeschitz-Kriegl had earlier become a close friend. I held a joint appointment in Delft with Fritz Schwarzl, another close friend, at the TNO, short for "Toegepast Natuurwetenschaplijk Onderzoek"—Applied Scientific Research, the research arm of the Dutch government, well known worldwide for its significant scientific efforts. In 1977 I spent three months at the ETH [Eidgenössische Technische Hochschule], the Swiss Federal Technical University, in Zürich, and in 1986-1987 another six months at the University of Ljubljana, in Slovenia.

A sabbatical year that I took in 1976 was a major break in my and Sophia's Caltech life. One of the academic accrediting committees that from time to time visited the Division of Chemistry and Chemical Engineering had found that the members of the faculty didn't go on sabbatical enough.

COHEN: Well, it's not an automatic program at Caltech.

TSCHOEGL: Right, you apply for it—and it will almost always be granted. Many professors didn't scramble to do it, because it caused interruption in their work. In 1976 I thought it would be good for me to go on sabbatical and get a break.

Having just received the Senior US Scientist Award of the Alexander von Humboldt Foundation, an arm of the German federal government, I spent my sabbatical primarily in Germany. The award is also known as the Humboldt Prize. It was set up as a prize because this way it did not become taxable income either in Germany or in the USA. It provides a full professor's salary at a German university or other German scientific institution where you wanted to work. I carried an identification card issued by the federal government that exhorted all government agencies to accord the bearer every conceivable assistance. I had a telephone installed in the apartment we rented within a week. This apparently took several months for an ordinary citizen. One of the highlights of my stay in Germany was a meeting in Bonn, then the capital of the German Federal Republic. There were lectures by a number of notables, sumptuous dinners, a cruise along the Rhine, and a garden party at the residence of the president of the republic. I not only shook his hand but even had a lengthy conversation with him. And now I cannot even recall his name.

During the course of my sabbatical, I spent six months in Mainz at the Gutenberg University with Erhard Fischer, then three months in London at Imperial College with Anthony Pearson, and then two months in Strasbourg at the Centre de Recherches sur les Macromolécules. Finally, I spent two weeks each in Czechoslovakia and in Hungary. I also spent a week in Israel at the invitation of the Technion in Haifa. I once lectured there to a full house on Sunday, which, of course, is just an ordinary weekday in Israel. I finally ended my sabbatical with three days of sightseeing in Istanbul.

The sabbatical was a useful break. It refreshed my attitude and I resumed work with renewed vigor.

At Caltech, my research work at that time—apart from work on the behavior of filled elastomers, which I have mentioned before—was concerned largely with block copolymers. These had new and exciting properties. Several people had been working on the synthesis of

block copolymers, but their physical properties needed to be explored and described. We did quite a bit of work on block copolymers.

Another aspect of my work involved approximation methods for the determination of spectral functions. These functions contain the information on the time-dependent properties of polymers. Any mechanical property of a polymer—such as, for instance, its shear relaxation modulus—is always a function of time. At very short times, a polymer typically behaves like a glass. It is then said to be in its glassy region. At long times, it flows if it is not crosslinked, or it is flexible like a rubber when it is. It is then said to be in its flow region or its rubbery region, as the case may be. At intermediate times—that is, in the transition region—its properties are intermediate between these extremes. The properties in the transition region show the time dependence most prominently…. But I don't know whether I should go into all this here in detail.

COHEN: No, I don't think we need too much detail, just the general idea.

TSCHOEGL: Fine. Since the mechanical—and electrical and optical and so forth—properties are time-dependent, they are concomitantly also frequency-dependent. This time or, equivalently, frequency dependence is analogous to temperature dependence; either dependence is considerably more marked in polymers than it is in most others materials. People who design with metals don't really have to worry too much about it and neither do people working with glasses, including stones and ceramics.

COHEN: So who is concerned with this? What would be the interest in this?

TSCHOEGL: Anybody who has anything to do with the mechanical properties of polymers needs to know how the polymer behaves at any time after it has been deformed, or at any frequency to which it might be subjected, just as he or she needs to know how it behaves at any given temperature. There are hundreds and hundreds of varieties of polymers, and they all behave in a somewhat different way, but all have time-dependent properties. When you put deformational energy into any material, part of that energy is stored elastically, but part of the energy is invariably dissipated by viscous forces. Of course *every* material has time-dependent properties.

The time dependence, the unavoidable dissipation of deformational energy, arises by dint of the second law of thermodynamics, according to which you always lose. [Laughter]

Time dependence can be a bother, but it can also be a tremendous advantage. The frequency dependence of polymers allows you, for instance, to construct dampers and shock absorbers. You may be surprised to learn that today they even put huge bridges on rubber blocks for their damping effects. Thus anybody who has anything to do with polymers is interested in their time-dependent properties, in how to characterize them, and how to efficiently store—and when needed, retrieve—this information.

We did a good deal of work on the spectral functions that contain the information on the time-dependent properties of a polymer. I developed several of the approximation methods for determining them. These became known as the Tschoegl approximations. And by now—again, largely because of my own work in collaboration with my friend Igor Emri, of the University of Ljubljana—they're all more or less obsolete. [Laughter] We developed an algorithm for determining the respondance time distributions of materials with time-dependent properties. Respondance times are called relaxation times when the excitation that brings them into play is a strain. If it is a stress, they are called retardation times. Once you have serviceable respondance time distributions, the spectral functions are superfluous.

Now, whenever material properties are time-dependent, they will necessarily also depend not only on temperature but on pressure as well. Just as there is a glass-transition *temperature*, there is also a glass-transition *pressure*. Above a certain pressure, polymers become glasslike, because the molecules no longer can move around more-or-less freely. These pressure effects are just as pronounced in polymers as the effects of time or frequency. We did pioneering work especially on the effect of pressure, and this work has now become classic, as it were.

COHEN: Who was funding your work at this time?

TSCHOEGL: The air force, the NSF, the ONR, and other agencies.

COHEN: So you had no problem getting money for your research?

TSCHOEGL: No, not really. Our work was considered important, and different aspects of it were funded by different agencies at one time or another. I did not have funding problems until

money suddenly became tight as a side effect of the unpopular Vietnam War. There was much public clamor at that time that universities should not do work for the military. The funding agencies of the air force, the army, and the navy supported a huge amount of research, much of it of a basic nature, not exclusively or not even predominantly of military use. From one day to the other, my large air force grant was severely cut. This seriously disrupted my work. I soon recovered, however; I got funding from other sources.

COHEN: What year was this? Do you remember?

TSCHOEGL: Not exactly—the 1970s, I suppose. Barring the military agencies from funding basic research was, of course, a very unhealthy situation for science and engineering in general, not only for the military. After a while, the agencies resumed funding for the latter.

As I mentioned, the mechanical properties of polymers depend not only on time and temperature but also on pressure. The information on the effect of temperature and of pressure must be available, just as that on the effect of time. The former two effects, however, cannot be described either by spectral functions or by distributions of respondance times.

My old mentor John Ferry and two of his collaborators, then at the University of Wisconsin, had developed an equation—the celebrated WLF [Williams-Landel-Ferry] equation—to describe the effect of temperature. This had semi-empirical backing and it worked rather well. We did some work with it also, particularly on trying to apply it to block copolymers and other multitransition systems such as polymer blends, another industrially important form of polymers. The WLF equation is not applicable to these, because each block has its own transition and therefore its own temperature dependence. It took us some time to realize what could and could not be done about this. I published a number of papers on it. The Chinese sage Confucius said, "Shall I teach you what knowledge is? When you know a thing, to recognize that you know it, and when you do not know a thing, to recognize that you do not know it."

We were trying to find a way to describe the pressure dependence of the mechanical properties of polymeric materials by an equation similar to the WLF equation. Two of my students, Bob Fillers and Bill Moonan, were working with me on this problem. We found that the pressure dependence is a little more complicated than the temperature dependence. Our work

led to the development of the so-called FMT equation—the Fillers-Moonan-Tschoegl equation. This handles both pressure and temperature dependence and contains the WLF equation as a special case. Furthermore, we showed that by obtaining the required information on both temperature *and* pressure dependence, a bothersome ambiguity in the original WLF equation could be removed and its capabilities could thus be improved.

All right, I think I have said enough about our research work.

COHEN: You were on the International Committee on Rheology?

TSCHOEGL: Yes. There are societies of rheology in most countries that have a polymer industry or in some way are seriously involved with polymers. Professor Eugene Bingham of the University of Lehigh founded the first Society of Rheology, here in the United States, I believe in 1926. Later, there sprang up others—the British Society of Rheology, the Groupe Français de Rhéologie, the Deutsche Rheologische Gesellschaft, the Japan Society of Rheology, and others. The American society, however, is still named *the* Society of Rheology, not the *US* Society of Rheology.

At the first International Congress on Rheology, it was decided to hold another one every fourth year from then on.

COHEN: Where was the first one?

TSCHOEGL: It was in Holland, and the second one was in England. After a while, it was decided to form an International Committee on Rheology, to which all the national societies would belong. Originally, the job of the International Committee was primarily to decide on the venues of the congresses. To this was then added the task of accrediting the national societies. One of the requirements for admission of a newly formed society was that its rules should be compatible with those of any of the societies that were already members of the International Committee.

I had been the US delegate to ICR from 1974 to 1976. At the International Congress that was held in Göteborg, Sweden, I was asked to become secretary of ICR, and I held that post for twelve years, from 1976 to 1988. After I retired from active duty at the institute, I no longer felt that I could do the job properly, because I was not traveling as much anymore and couldn't keep in touch with things sufficiently. But it was a very interesting post. One of my jobs was to

encourage the formation of societies of rheology in other countries. The number of societies grew apace, because more and more countries rapidly became sophisticated in the use of polymers. When you are, you naturally wish to form a society to exchange information, arrange meetings, and so on. I helped form a number of these societies, counseling them on how to do it and vetting their by-laws. I liked that job very much; it was an interesting job—and of course I was helped in this by speaking or reading many of the languages that would come into consideration here.

COHEN: So you did travel a great deal in those days. But you still maintained all your work here, at home? Your research here continued, and you were teaching also?

TSCHOEGL: Oh, yes. I've always managed to keep in touch. Except for my sabbatical year, I was never away from the institute for any length of time when I traveled.

Traveling was a great experience. Sophia and I spent two weeks in the Soviet Union upon invitation of the Academy of Sciences of the USSR. That was a very good deal for seeing the Soviet Union. A scientist at one of the institutes of the academy was assigned to be your host and accompany you everywhere. The guest's way was smoothed in every respect. Lectures were organized, travel was arranged—we visited Moscow, Leningrad, and Kiev—and tickets were available to the Bolshoi in Moscow, the Kirov in Leningrad, and other cultural institutions of which the Soviets were, I think, justifiably proud.

COHEN: The aristocracy of Communism.

TSCHOEGL: That's right, although the *real* aristocracy of Communism was, of course, the Communist Party. Scientists were a sort of lesser aristocracy. Scientific exchanges between the United States and the Soviet Union had become quite frequent. People would fly from the USA to Moscow and elsewhere in the Soviet Union, and Soviet scientists would come here. I speak some Russian, and Sophia, who was invited with me, speaks it fluently, as I have said earlier. Pretty soon it became known among Soviet scientists that they were well taken care of here in Pasadena, so many of them came and visited. We established friendly relations with a number of scientists over there. All this was very nice while it lasted.

At one point, however, these visits suddenly stopped. And thereby hangs a tale. Paul Flory, the 1974 Nobel Prize winner in polymers; Herman Mark, the father of the polymer industry in this country; Walter Stockmayer of Dartmouth, and I were invited to an International Macromolecular Symposium in Tashkent, Uzbekistan. At the time, some Jewish scientists wanted to emigrate to Israel, and the Soviets didn't want to let them go. They did get out after a while. One of them was Natan Sharansky, who later became a government minister in Israel. You may know the story. Paul Flory decided that we had to do something about this affair. He felt strongly about it; it was important to him. He proposed that we should not go to Tashkent. He said, "We have to write a letter to the head of the USSR Academy of Sciences that we are opposed to the treatment of these scientists and that we cannot go to this meeting." I didn't think that that would accomplish anything much. It was well known that you could not pressure the Soviets in matters like these. For them, politics was paramount. You could be the world's greatest scientist, but if you were in political limbo, as a number of great ones were—Pyotr Kapitsa, for instance—there wasn't anything anybody could do to help you. I said as much to Paul Flory, but he was adamant that we had to do something. So I said, "All right, if you insist. But let me first talk to the other two guys"—Herman Mark and Walter Stockmayer. And neither of them was really happy about that either. Herman Mark felt that we should not get into politics. Walter Stockmayer felt the same. Walter had never been engaged in any politics, and neither had I. So we were not too happy. [Tape ends]

Begin Tape 4, Side 2

COHEN: So what finally happened?

TSCHOEGL: Well, we all respected Paul Flory because of his great merits. And we were all, of course, in sympathy with the Jewish would-be emigrants. So we decided to go ahead. Paul Flory said he was going to draft a letter and send it to me first and then to the others after I had given him my views. He wrote an uncompromising, completely undiplomatic letter. When I got his draft, I called him and said, "Paul, we cannot *possibly* sign this letter. This is impossible! You are insulting our prospective academy hosts. Virtually all of them are friends of ours. We can't embarrass them by telling them that we don't like the Communist Party and its policies." He said, "Well, what do you suggest?" I replied, "Let me try to change your letter." I sent him

the amended version, and he agreed that we should publish the letter in this form. Herman Mark and Walter Stockmayer also accepted my version. [Laughter] I'm sorry but I may not have kept copies of the correspondence. Much of our deliberations were, of course, by phone.

So we finally sent the letter. Obviously, we didn't go to Tashkent, that beautiful metropolis of 2.3 million inhabitants in the middle of Central Asia that sports a completely modern Soviet-style subway. I've never been to Tashkent and now, alas, will never be able to explore its 2,000-year history. At any rate, that was the end of it. I never got to visit Uzbekistan. It is of course an independent country now

What happened next is that the stream of Soviet visitors that used to come to Pasadena suddenly stopped. We were no longer on the list of places that they were allowed to visit. Several of them were good friends. They would come to other meetings in the USA and would call me on the phone and say something like "We are here, but we can't come to Pasadena, we feel sure you understand." [Laughter] Those were peculiar times, indeed.

All right. I think I have already talked earlier about the Watson Lecture Committee.

COHEN: Well, we may not have it. Tell us about it again.

TSCHOEGL: As you know, the Earnest C. Watson Lectures, now presented in the Beckman Auditorium, expose the educated public to the work of the Caltech faculty. The public attends the lectures free of charge. They are named after the professor who had started it all in 1922 with an immensely popular demonstration lecture on liquid air.

When I came to Caltech, the lectures were arranged by the Watson Lecture Subcommittee of the Institute Committee on Programs, chaired by Harold Wayland. Harold and I were then both members of the Watson Lecture Subcommittee. He asked me one day if I would take over management of the lectures. He had had enough of it, and he was also close to retirement. I really didn't want to do it, because I had a lot of other things to worry about. I also didn't feel that I was qualified. I had never done anything like that before. But Harold was quite insistent. He said, "You would be the right person at the institute to do it." OK, fine, so I gave in. I tried always, when presented with a challenge, to accept it, following Yogi Berra's advice: "When you come to a fork in the road, take it." [Laughter]

It turned out to be a very gratifying experience. I really liked it. I learned not only about what goes on at Caltech in research but I also learned many things about our professors. A lot of them were excellent lecturers. Others had fantastic work going but were just not very inspiring speakers. Sometimes I had to do some detective work and find out "Would this guy bore people, even though he has an interesting subject?" I would call the prospective speaker's division chairman and discuss it with him. The chairman would usually know whether someone was a good speaker or not. In some cases I would hear "Well, you'd better stay away from him; he's excellent at research but not that great a lecturer."

In principle, Caltech professors—and some people from JPL—would give the talks. Occasionally if a visiting professor had something to say that was really interesting and also topical, we would put him on. The lecture didn't have to be on the speaker's research topic; it could be on something quite different. Most professors talked about their research.

Prospective speakers were asked to submit abstracts for publication. I would send all the abstracts to the members of the subcommittee, and we would then vet them and arrange the program for the next season. The abstracts were sometimes a bit of a problem. Many prospective speakers wrote good abstracts. They knew that they were going to present a lecture to the educated but nonexpert general public. A few, however, sent in abstracts written as if they were going to deliver a talk at a meeting of colleagues working in the same field. This, of course, wouldn't do. I had to work with them on the phone and via campus mail to get their abstracts into suitable form. This could be a challenge, and I really liked to do it. I had to be diplomatic. I couldn't offend a colleague by saying, "Listen, you don't know how to write a proper abstract." [Laughter] Sometimes I edited an abstract and sent it back for approval. Most prospective speakers were willing to work with me.

COHEN: Most people agreed to give one of these lectures?

TSCHOEGL: Yes, most did, because they enjoyed doing it and because there's also a certain amount of kudos in it. Sometimes one would say, "No, I cannot do it now. I'm too pressed right now. Maybe I'll do it next year." Or some other excuse. If they were interesting candidates, I would make myself a note and later went back to them and twisted their arms. Opening Beckman Auditorium for a Watson lecture costs the institute a good deal of money. They are

free to the public, and so there is no financial return. My concern, therefore, was to fill the house with these lectures, to get maximum benefits by getting maximum exposure of the Caltech faculty. Going through the attendance numbers during the years before I started putting on the lectures, I learned what subjects interested the public. In all modesty, I'm proud to say that I was probably the best manager that the lectures had ever had up to then.

COHEN: How long did you do it?

TSCHOEGL: I did it for several years—I don't remember exactly. I started doing it fairly soon after I came to Caltech; I stopped doing it around the time I retired [1985].

COHEN: What did you find the public liked the most? What was most popular?

TSCHOEGL: That is rather interesting. There is a core group of about 300 or 350 people who would attend *any* lecture. But Beckman Auditorium, when full, holds around 1,200 people. What lectures would fill it to capacity? People were then greatly interested in outer space—that is, astronomy, astrophysics, planetary missions, the moon, other planets and their moons, anything dealing with our planetary system. Nowadays this is no longer all that interesting; the results that are now coming in are perhaps less spectacular and more like reports on research. There are exceptions. The Mars lander, for instance, was a sensation—the Mars projects always will be of interest. After all, Mars is Earth's closest relative, so to speak. Today I had some work to do in the photo lab, and there I saw a huge color photo showing a Martian landscape—with the lander right in the middle! It had been obtained by JPL's planetary probes. I looked at it and my heart almost burst. We still don't know enough, but it's unbelievable that we now know so much more about Mars than we did in the days, not all that long ago, when I was in my teens! No more of Lowell's Mars canals, and no more Edgar Rice Burroughs, although he reads rather well. Thus, Mars always drew.

Apart from outer space, anything that had something to do with the human body was always very well received. For a while, Irv Bengelsdorf, the science editor of the *LA Times*, gave a course in scientific writing in chemical engineering. I often talked to him. He characterized the other prime topic as "inner space."

When the topic was mathematics, the attendance was generally poor. Mathematics, and to a lesser degree chemistry, were not favorite topics. Physics—all right, if you could make it palatable. Incidentally, I cannot understand why people dislike mathematics so much. We have some professors who are excellent mathematicians but are also excellent lecturers and who could present a mathematical topic beautifully. And yet not many people would show up. It seems that people are afraid of mathematics.

COHEN: Well, I think that's our society. We are not fond of mathematics.

TSCHOEGL: Possibly, because mathematics may generally not be taught well in high school. For me, fortunately, mathematics was one of the easiest subjects in high school. It was so logical; I never had any problem with it. In fact, although I've published two books whose subjects are essentially mathematical and are treated in terms of advanced mathematics, I actually never had any formal education in mathematics beyond the high school level. You might say I was self-taught.

COHEN: Not everybody is like that.

TSCHOEGL: And that's unfortunate. It's a beautiful subject.

COHEN: Right. So when did you yourself get involved, with your Atlantis talk? How did that happen?

TSCHOEGL: Ah, yes. As I said, your Watson lecture did not have to be on your research subject. A good example was a lecture by Max Delbrück. Max was an exceptional character. He was a physicist by training; he became interested in biology and is famous for having been one of the first to introduce the rigorous methods of physics into biology. I asked him and he agreed to present a Watson lecture. OK, fine. He sent me his abstract. It was well written and it was clear that he was talking about a historical personage and that this person had something to do with science. But the abstract didn't say who he was or what Max wanted to say about his subject. I was a little apprehensive that the public might be equally puzzled and the attendance might suffer. I called him up and said, "Max, I have your abstract, but who are you going to talk

about?" He said, "Well, come to the lecture and you will find out." And, you know, with someone else I would probably have insisted, or at least tried, to revise the abstract. But I knew Max quite well. We had had sometimes quite long conversations, and we occasionally exchanged books. I knew that he would do an excellent job, no matter who he was going to talk about. I had heard him talk before. He was not a magnetic speaker but always presented his topic in an unusual and interesting way. So I accepted his abstract, but it bothered me that I couldn't figure out who he was talking about.

One night several weeks later—the abstracts always had to be ready well in advance—I woke up at four o'clock in the morning. The moon was shining into my bedroom, and I was suddenly wide awake with the absolutely clear knowledge of who Max's mystery subject was. I forthwith went to my Encyclopedia Britannica and looked it up. Bingo! Not having recognized his subject must have worked in my subconscious all the time. Our brain works in wonderful ways!

I called Max next day and said, "Max, what are you going to give me if I don't divulge your secret? I do know who you're going to talk about." He said, "Well, well, come to the lecture." He didn't say yes or no [laughter] but he was amused. He gave the lecture and it was a success.

COHEN: Who did Max talk about—it's not a secret anymore.

TSCHOEGL: Of course not. He talked about Copernicus, who revolutionized scientific thought by announcing that the Earth revolved around the sun and not the other way around.

Max Delbrück was not the only one who talked out of school, as it were. Before my time on the Watson Lecture subcommittee, Richard Feynman talked about the Mayan calendar. That also was a very good lecture.

I thought that the time had come for me to show what I had learned from arranging the Watson lectures. Let *me* give one, too. I chose Atlantis as the topic. In 1977 I had been in Greece. I had visited Crete among other fascinating sites and had gotten much interested in the Minoan culture. Several people believe that the Minoan world forms the background of the Atlantis myth. When I came home, I assembled quite a library on Bronze Age archeology. For a while, it became one of my side hobbies.

The sole source of everything we know about Atlantis are two of Plato's dialogues, the *Critias* and the *Timaeus.* Everything else is derived from those two dialogues, and you could fill a whole room with publications—books and papers and whatever—on Atlantis. A lot of it is absolute rubbish, such as the readings of the so-called psychic Edgar Cayce. I only learned about these after I had given the lecture—thank God!—and thus did not refer to them.

I put my abstract together *à la* Max Delbrück. It said that we now know what Atlantis was, where it was, and how it was destroyed, but I did not mention the Minoan connection with one word. Of course, the subcommittee accepted my abstract, no problem there! [Laughter]

The night of the lecture arrived. And would you believe it?—I had not expected it—a *huge* crowd showed up. The crowd was so large that the police had to come out and direct the multitude. At one point, the ushers tried to bar access to Bob Christy, who was then acting president of the institute. They had not recognized him, and there wasn't even any standing room left. Sophia was at the entrance to direct our friends and special invitees to the seats reserved for the guests of the speaker. She literally had to pull Christy in. [Laughter] The overflow was directed into the hastily opened Ramo Auditorium—another 500 people above the 1,200 that filled Beckman. And they still had to turn hundreds away. Some stood or sat outside the auditorium and listened to the lecture being broadcast over loudspeakers. Talk about maximum attendance!

The people in Ramo could not see my visuals; they could only hear my voice. That was unfortunate, because I like to have something meaningful on the screen always. People are used to watching television. If they have nothing in front of them, their attention tends to wander. So does mine! My slides were an integral part of the lecture. I used parallel projection—I projected two slides side by side, using two projectors simultaneously. The Beckman people had fashioned a device for me that would allow me to actuate both projectors at the same time. In this way I was able to present more information than I put into words. For example, when I showed a picture of Plato, I gave his dates and other information on him on the parallel slide. I think I was the first to use this technique in the Beckman Auditorium.

The great hullabaloo happened not because of my fame as a lecturer. I had never given a Watson lecture previously. Nobody had heard me speak before; nobody knew what a *marvelous* speaker I am. [Laughter] What interested the audience was the topic—Atlantis. I was talking

about the only theory that reasonably seems to fit the myth as it has come down to us through the ages.

Plato wrote about what he had learned from his great-uncle Solon. Solon, whose name even today is synonymous with "lawgiver," was an *archon* in Athens and had given the city a new constitution. Having done this, he thought it wise to disappear for a while and went to Egypt. At that time, Greeks would travel to Egypt as Americans will travel to Europe—to an older culture worth learning from. And Solon did learn something in the Temple of Isis about Greek history. As Plato reports it, the priests of Isis had told him, "You Greeks are like children; you don't know your own history." Solon then did learn something about this history, and it came down to Plato.

Well, according to the current interpretation of Plato's dialogues, he was talking about what we now call the Minoan culture. Its center would have been on a vanished island, the remains of which are now known as Thera, which is also called Santorini. The island is a caldera—Spanish for "cauldron"—a large bowl-shaped depression with sharp walls which is the remnant of an ancient volcano. In about 1450 BC, a tremendous eruption blew away the center of the island. This is well known to vulcanologists. About 100 BC or so, the center started to build up again slowly. The newly created islands in the middle of the caldera are as yet quite small. In due course, the caldera will fill up again. And the vulcanologists say that there will be another explosion, but we don't have to worry about it too much; we'll be safe for at least 25,000 years. [Laughter]

What blew away in the volcanic eruption might well have been the capital of the Minoan civilization. We will never know what really was in Plato's mind, but I supported the Minoan theory with a good amount of circumstantial evidence. An abbreviated version of the lecture was published in Caltech's *Engineering and Science* [vol. 35:7, pp. 17-21, 1972].

The talk was a huge success. The topic brought the crowd in, but I must have done a good job, too, because at the end I got a standing ovation. Since so many people had to be turned away, I repeated the lecture, and it filled the house again. It was the most popular lecture in Beckman ever—it beat even Feynman's talk on the Mayan calendar. As I have said, it was not because of me. But the word got around, and when I gave my next Watson lecture, which was on Chinese writing, many people already knew me. Arnold Beckman [chairman emeritus, Caltech board of trustees] attended both lectures and very kindly told me that he had enjoyed

them very much. Christy's wife, Juliana Sackmann, came to me afterwards and said that she would come to *whatever* lecture I would give.

My lecture on Chinese writing was right on time. Our relations with China had just opened up. I wanted to tell people that Chinese writing is not just a jumble of strokes, that there is rhyme and reason in it. The Chinese language has an enormous number of homonyms, words —or syllables more correctly; Chinese is a syllabic language—that sound exactly alike but differ vastly in meaning. The characters, on the other hand, are unambiguous. Let me give you an example in English. It also has many homonyms, although not nearly as many as Chinese has. The words *write, right, rite,* and *wright,* as in playwright or cartwright, for example, are all pronounced identically but they are spelled differently and thus clearly express their particular meaning in writing. The majority of Chinese characters are compounds consisting of a phonetic element, which renders the pronunciation, and a classifier, which points to the general area to which the character refers. When you come across a character that you may not have seen before, the phonetic element tells you how it is pronounced and the classifier removes the ambiguity presented by the homonymity. It's rather like when you meet a word of Greco-Latin origin that you have not met before—if you know enough Greek and Latin, you can figure out what it means. If you master something like 1,200 or so phonetic elements and about 200 classifiers, you can read Chinese quite well, provided that you *speak* the language. Both the phonetic elements and the classifiers were originally pictograms or ideograms—that is, they represented an object or an idea. Egyptian and cuneiform writing developed quite independently on rather similar lines.

My talk on Chinese writing was very popular also. I had to repeat it, too. The first time it filled Beckman; the second time, only about 750 or so people showed up. Not full capacity but more than what most lectures on mathematics or chemistry would bring in. [Laughter]

I gave the Atlantis lecture about seventeen times, all over the world. The Chinese lecture I gave five or six times. I never talked about my research work in public, because I found that much more difficult to do than talking about one of my hobby subjects. [Laughter] [Tape ends]

NICHOLAS W. TSCHOEGL

SESSION 5

June 18, 2001

Begin Tape 5, Side 1

TSCHOEGL: OK, so now I'm retired. [Laughter] In 1985, when I decided to retire, I was sixty-eight years old—it's quite astonishing to think of it now, when I'm eighty-three. I had *always* intended to retire at age sixty-eight. This was the mandatory retirement age for a faculty member when I came to Caltech. At the time I retired, I actually would have had to retire at age seventy. For a few years that was the rule, and then it was changed again, so that a professor did not have to retire at all. For me the question was, Should I retire at age sixty-eight or at age seventy? What weighed heavily into my decision was the fact that at age sixty-eight Caltech stopped putting money into my retirement fund. At the same time, it had become increasingly difficult, tedious, and aggravating to raise money to keep my research group going. Also, a graduate student will be here generally for five years, so I would not be able to keep a student all the way through, or I would have to stay on in one capacity or another. I decided that since it was a matter of two more years only, why should I bust myself to raise money? Thus, I decided to retire at age sixty-eight. The powers that be gave me every help. I received half of my salary for another three years. They evidently were happy to get rid of me. [Laughter]

Retirement was not easy for me. I was used to having a group of students and research fellows. Interaction with young people, one of the things I liked so much at Caltech, was important to me. For about two years—I always take about two years to adjust to *any* change in my life—I was a little at sea, so to speak. I felt that I had lost direction, that I had lost my momentum. It was not for too long, though.

I spent six months in 1986-1987 at the University of Ljubljana in Slovenia. Igor Emri, a professor of mechanical engineering, had offered me a visiting professorship there. Igor had been here at Caltech, working with Wolfgang Knauss. I got to know him at that time, when he needed one of my instruments that was not available in Knauss's laboratory. He came over to my lab and we chatted. I told him that I had been in Ljubljana; he found that I was one of the few Americans who knew what and where Slovenia is, and what on earth Ljubljana might be. [Laughter] I even knew something about the Slovene language, which has many affinities to

Czech. So we hit it off right away, and sometime after he had returned home, he invited me to present a course on viscoelasticity at the University of Ljubljana. I agreed readily. The course was attended by some professors and people from industry as well, and it was well received. I also used the time to finish some of the chapters of my treatise on viscoelasticity.

My stay in Ljubljana started my cooperation with Igor. It has lasted until today—and I hope that it will last for some time to come. We have published numerous papers together and even have embarked to cooperate on a book. Igor had come back to Caltech for six months as visiting professor of aeronautics in 1985 and returned again for three months in 2001. To me, Igor Emri was a godsend, because this connection gave me again the opportunity to interact with a group.

COHEN: What language were you speaking when you were in Ljubljana?

TSCHOEGL: English, of course—the international language of science. Slovene is spoken only by about 3 million people worldwide—2 million in Slovenia, about 700,000 or so in the adjacent Austrian province of Carinthia. Igor insists on the use of English. All of his students have to know English. They have to be able to present periodically accounts in English of what they're doing. He speaks fluent English himself and writes it well.

He has *one* difficulty with English, though, as all Slavs have. The Slavic languages, with the exception of Bulgarian, dispense with articles—the grammatical kind. This poses tremendous problems for a Slavic speaker wanting to speak English, or any non-Slavic western language. If you speak a language that doesn't use articles, you can't figure out where and how to put an article in your sentence. I was trying to chase down for Igor some rules about the use of the definite and indefinite articles in English. Rather surprisingly, I could find nothing in print for several years. Eventually I found a single page of instructions somewhere and sent a copy to Igor. He forthwith distributed it among the members of his group, who all do battle with the accursed articles.

Igor operates his own Center for Experimental Mechanics in Ljubljana under the aegis of the university. CEM is largely independent of the university, which does not support it financially. Igor has substantial industrial support in Slovenia as well as abroad. He also has a

consulting agreement in Germany with BASF—Badische Anilin- & Soda-Fabrik, certainly one of the largest polymer manufacturers in the world. It's actually larger than DuPont.

Igor had been a member of the American Society of Experimental Mechanics for some time and had always deplored the society's lack of involvement with polymers. So he set out to form a new division of the society which would be devoted to the mechanics of materials whose properties significantly depend on time. Polymers are the materials *par excellence* that qualify for this distinction. He felt that he needed the collaboration of somebody well known in this country, and Wolfgang Knauss, his erstwhile mentor at Caltech, joined him in his endeavor. Shortly, the Society of Experimental Mechanics acquired a division of time-dependent materials.

He and Wolfgang then founded the journal *Mechanics of Time-Dependent Materials*, and became its editors-in-chief. I am on the editorial advisory board of the journal. I think the title is incomplete, because *all* materials are *always* time-dependent—if nothing else, they will just go to pot in due course of time. [Laughter] The title should have been something like *Mechanics of Materials with Time-Dependent Properties*, but a certain member of the editorial advisory board had not been asked for advice in time. Too bad!

The after-effect of all of this was that Igor became the first non-American president of the American Society for Experimental Mechanics. He became president for two years and now is past president for life.

COHEN: So he's something of an honorary American?

TSCHOEGL: [Laughter] Almost, that's right. He's very much at home here.

COHEN: He has students in Slovenia?

TSCHOEGL: Oh, yes. They are students at the University of Ljubljana, but they do their work at CEM with him. The center also accommodates a number of people from industry, who are seconded to CEM by their companies. Igor is very good at getting support. He is helped in this, because there are for some reason relatively few people in continental Europe that have his expertise in the area of the mechanical properties of polymers.

COHEN: So you really continued doing what you were doing before you retired, only you were doing it now with him and his group.

TSCHOEGL: That is correct. With the consent of the institute, I shipped Igor two of the apparatus that I and members of my group had designed and constructed here to help us in our work of determining the mechanical properties of polymers. They were unique. Igor is a mechanical engineer by training and knows quite a bit about such apparatus. He also has a good understanding of electronics. He and his people immediately set out to update both apparatus. By the time I shipped them, several years after my retirement, they had become partially obsolete, particularly the data acquisition systems. They are now in perfect shape at CEM. You wouldn't even recognize them today. My original conceptions are there, and the material core is there, but the equipment has been completely revamped.

Igor and two of his people have just published a paper, which they kindly dedicated to me, on experimental results obtained with my former and now state-of-the-art apparatus for the simultaneous determination of the effect of temperature *and* pressure on the mechanical properties of polymers. Igor and I have just been drafting two more papers that are concerned with it. The other apparatus is also up to speed and has already been used in work for BASF. Publishable scientific experimental data will be coming along soon. Wolfgang Knauss is now sending one of *his* machines to Ljubljana, so—adding all these to his other equipment—Igor will really be set up beautifully at his CEM.

COHEN: Why is Caltech doing this? What is Caltech's interest in doing this?

TSCHOEGL: Well, these are unique pieces of equipment that would not have been of use to other people here at the institute. Caltech would just have had to scrap them. Igor did want them, and he can use them very well. So why not? After all, what we are interested in is to see science advance, no matter where or who is doing it. Besides, all three of us are good friends and Wolfgang and I are interested in the work at CEM.

For me, the connection with Igor—that I could work with him and his students—revitalized my energies. It is quite important for me that work I had initiated here but hadn't quite finished by the time I retired has now already come to fruition in Ljubljana. I have been going there regularly once a year, and once a year at least Igor comes here.

COHEN: So that's strictly his project in Slovenia. Caltech really has nothing to do with that?

TSCHOEGL: Not directly but indirectly, of course, through my involvement with much of the work that goes on at CEM and the papers I coauthor under the byline of the Division of Chemistry and Chemical Engineering.

Just before we started working together, Igor had a brilliant idea, which we then exploited and further developed together. This was an idea for a mathematical algorithm designed to determine the relaxation and retardation time distributions of polymeric materials. These are obtained in the form of line spectra and are recovered from any mechanical response determined experimentally. I have referred to the algorithm before. Before we came up with it, the approximation methods I also referred to earlier had to be used. However, these methods are awkward, somewhat difficult to use, and not completely satisfactory. The algorithm we developed with Igor is an enormous advance and has had quite a bit of success. We wrote several papers on it, use it ourselves in our work, and are now preparing a monograph on it in book form.

One of the two instruments I sent to Ljubljana and that was rebuilt there is concerned with the determination of the effect of pressure on the mechanical properties of polymers. Until my group and I started to expend efforts on this area, relatively little work had been done on it. Because of certain advances in polymer processing, it has now become quite important. Igor says that whenever he goes to an international meeting—I don't go myself much anymore—and pressure is mentioned, everybody talks about the pioneering work we did on this aspect at Caltech.

The other of the two instruments is concerned with the determination of the so-called viscoelastic Poisson's ratio, a measure of the bulk behavior of polymeric materials—that is, with the description of the properties that polymers have under an isotropic pressure. Those two efforts are now moving forward in Ljubljana, much to my delight.

There is also considerable lateral fallout from all our work. This has also resulted in a number of papers. All this is still continuing. Igor, Wolfgang, and I just published two rather extensive review papers ["The Effect of Temperature and Pressure on the Mechanical Properties of Thermo- and/or Piezorheologically Simple Polymeric Materials in Thermodynamic Equilibrium. A Critical Review." *Mech. Time Dep. Mater.*, 6:3-51 (2001); "Poisson's Ratio in

Linear Viscoelasticity. A Critical Review." Ibid., 53-99], which we dedicated to my old mentor, Professor Emeritus John D. Ferry at the University of Wisconsin, whom I regard as the father of polymer rheology. These papers required quite a bit of work. Both contain material that had not been published before. I wrote them, as lead author. Igor had contributions to make and Wolfgang had several valuable suggestions.

COHEN: And I see they're each at least a centimeter thick—I have to describe them. The papers are a centimeter thick.

TSCHOEGL: [Laughter] They are larger than the usual crop.

COHEN: You are dedicating them to Professor John D. Ferry. Very nice. We spoke of him earlier at length. He was your inspiration.

TSCHOEGL: He was more than that. His support enabled me to stay in this country and make a life here for myself and my family.

COHEN: So you continue with this work even now. Do you go to Caltech? Do you go to your office?

TSCHOEGL: I do. However, now not any longer to work there. I have a first-rate computer installation at home. The two papers I just mentioned are the latest I wrote at home. I go to the institute to pick up and deal with my mail, and occasionally to interact with my colleagues. Every Thursday I join them for lunch in the Athenaeum, if and when I can. Right now I can't, because Sophia's illness keeps me at home. She broke her femur in a fall and is suffering from chronic postoperative neuralgia.

COHEN: So you still have much work to do. That's good!

TSCHOEGL: Have I mentioned my books? I have published two books—so far. One is a treatise on linear viscoelastic theory. That is really my main work. I had an early start on it in 1961, when I published the article on linear viscoelastic behavior that got me the appointment with

John Ferry. I lectured on the same topic in 1967 at a NATO Advanced Summer Institute that Max Williams had arranged in Stresa, Italy. I later presented the material as part of my courses. The book finally appeared in 1989. I first had a tentative agreement with Academic Press to publish it. When I thought it was ready—except for two chapters that I was still working on—I sent the draft to them. Academic Press wrote back and said that they thought that in its contemplated form it was going to be too long for them. They suggested that I reduce it to such-and-such a length. I practically despaired. First of all, I thought it should not be reduced; secondly, I didn't want all the extra work—it's a terrible job to do that, and I did not have a word processor at the time. I resigned myself to not having it published. Strangely, the thought of finding another publisher never even crossed my mind. In my subconscious, I must have thought that any other publisher would present me with the same demands.

Professor Joachim Meissner at the ETH in Zurich came to my rescue—or the book's, really. I had met Joachim first at BASF. We became good friends after we met again at the NATO meeting in Stresa. He later told me that he had understood viscoelasticity for the first time when he had heard my lectures. From the BASF, he went to Zurich. He's a first-rate experimentalist and constructed there an extraordinary piece of equipment for the determination of the mechanical properties of polymers in large biaxial deformations; no one had really tackled this successfully before him. In 1977 he invited me to the ETH as a visiting professor. There, I also presented a course on viscoelasticity and in fact wrote several of the later chapters of my treatise. Thus Joachim was quite familiar with its contents.

When he heard that I had sort of given up on seeing the book in print, he called Springer-Verlag—he served on an advisory board for them—and strongly suggested that they publish it. An editor of theirs came out to Pasadena to see me, looked the draft chapters over, and declared that they would publish the book no matter how long it was. Thus Springer-Verlag offered me a contract, and I sat down to put the last two chapters in final form. Igor, who refers to the book as "the Bible," prepared the drawings for these chapters and carefully checked many of the problems and their solutions, which accompany each chapter. It finally appeared in print [Nicholas W. Tschoegl, *The Phenomenological Theory of Linear Viscoelastic Behavior* (Heidelberg, New York: Springer-Verlag, 1989)] and is now supposed to be something of a classic. Ferry, who is *the* authority on the subject, perused it and wrote to me saying that "It fills the gap between Love's *Elasticity* and Lamb's *Hydrodynamics*, and is equal to them both." That

made me very happy. It was the job of my academic lifetime, really. I worked on it off and on for twenty years.

About my second book: Towards the end of my active years at Caltech, I was asked whether I would present one term of thermodynamics as part of the undergraduate courses in chemical engineering. I had taught the thermodynamics of polymeric materials but not thermodynamics in general. I thought, Well, that's an interesting challenge. I had, much earlier in my life, decided that whenever I faced a challenge I would rise to it. So I taught thermodynamics. I had to sweat, but I did it. Then when I retired, I thought, Well, now I have time to write it all up. I did, and enjoyed the work tremendously. It's thermodynamics unlike any other thermodynamics that I know of. Not the content, of course. I did not invent any *new* thermodynamics. Its novelty lies in the way it presents the subject.

COHEN: What is the name of this book? [Tschoegl directs Cohen to book on shelf.] OK. *Fundamentals of Equilibrium and Steady-State Thermodynamics*, by Nicholas W. Tschoegl [(Amsterdam, New York: Elsevier, 2000)].

TSCHOEGL: It presents equilibrium and steady-state thermodynamics in a unified postulatory way.

COHEN: It's just been published.

TSCHOEGL: Yes, quite recently. The other one was published four years after I retired.

COHEN: So when you say that you weren't doing anything for two years after you retired, that's hardly true. You were working on these books.

TSCHOEGL: Yes, among other things. I always had and still have a number of papers in the works. And of course there is the cooperation with Igor Emri.

About my other activities: When I came to Caltech, I was offered a consulting agreement with the polymer section at JPL and held it for several years. I had to relinquish it eventually, because it had become just too much work for me. I had become quite busy.

COHEN: But you did do it for a while?

TSCHOEGL: Yes, I did, from 1965 to 1969.

COHEN: How did that work? Did you go there one day a week?

TSCHOEGL: At first weekly, and later whenever it was required, because I consulted elsewhere also. I cultivated close relations with the polymer section of JPL. We would discuss whatever problems they had—and when needed, I would try to figure things out for them. Often I invited JPL people to talk to my group. Several of them sat in on my polymer courses. I would go up to the lab—as we called it—often accompanied by one or more of the members of my group, to attend lectures that someone presented there. The head of JPL's polymer section at that time was Bob Landel, whom I mentioned earlier. Bob had obtained his PhD with Ferry in Wisconsin, so we were spiritual relatives, as it were. My consulting agreement with JPL came in handy financially. I had taken some reduction in salary when I left SRI to come to Caltech.

COHEN: And why should you not be paid? Did you do any other consulting—for anyone else?

TSCHOEGL: Oh, yes, quite a lot. At Stanford Research Institute and at the Naval Weapons Center, for instance, but mostly in industry. For sixteen years, from 1968 to 1983, I regularly consulted with the Phillips Petroleum Company. I went down to Bartlesville, Oklahoma, to the company's headquarters and the seat of their basic research activities, every four months for three days. I established friendly relations with Gerry Krauss, who directed rubber research at Phillips, and with Phil Arnold, then vice president for research. The company established a rather well-endowed graduate fellowship in chemical engineering at the institute. This lapsed after the retirement of Gerry Krauss, when Phillips abandoned rubber research in a major restructuring of their efforts, and my consulting agreement was canceled also.

I also consulted at the Firestone Tire and Rubber Company from 1974 to 1980. Glen Alliger, vice president of research, became a good friend. Firestone was later acquired by the Bridgestone Corporation, a Japanese firm. Michio Okuyama, who is now head of research at Bridgestone, spent three years with me here at Caltech. At SRI, I guided the project on dough rheology that I had initiated when I worked there, 1963-1965. The Naval Weapons Center work

became the reason that I acquired American citizenship, on November 21, 1969; for my consulting work there I needed a security clearance, and that made it useful that I become a citizen. And I am still acting as an unpaid but eager consultant at Igor Emri's Center for Experimental Mechanics in Ljubljana.

I also visited the Goodyear Corporation many times but did not have a formal consulting agreement with them. Goodyear was a member of Caltech's Industrial Associates, and so I visited it, as I did many others of Caltech's Industrial Associates, on the institute's behalf, to present lectures and to discuss their research with them. This consulting activity was a service to the institute's Industrial Associates. The institute paid some or all of my expenses on these occasions, but I received no fee. I benefited, though, from these visits by learning a good deal about what went on in the polymer industry.

Among other fruitful contacts, those with Kazuhiko Ninomiya stand out. Kaz, another spiritual relative, became a close friend. He worked for several years with J. D. Ferry in Madison, where we first met. Kaz directed rubber research at Nihon Gosei Gomu, the Japanese Synthetic Rubber Company, and later headed all of research there. I visited the company in Japan several times and kept in contact with Kaz over the years. He also visited me at Caltech on occasion, and he sent me one of his junior coworkers, Keikichi Yagii, who obtained his PhD with me.

Naturally I also had numerous good and productive contacts with academic and industrial departments virtually everywhere in the industrialized world where polymer research was carried out—notably in England, France, Germany, Italy, Belgium, the Netherlands, Sweden, and Switzerland, but also in Czechoslovakia, Hungary, and the Soviet Union. I visited there, and invited people from the respective organizations to come to Caltech. There was a lively coming and going.

I was in demand as a consultant with the rubber firms I mentioned because my research was much concerned with the behavior of rubbers. I have talked about rubbers before. They have no counterpart in the nonpolymer world. I was very much interested in rubbers and in elastomers because of their unique mechanical properties. Rubbers are so unique because they are capable of large reversible deformations. No other material has this capability. They have become absolutely essential for our life today. You can't have cars without rubber tires. You can't have airplanes without rubber tires. When the United States was suddenly cut off from the

supply of natural rubber during World War II, it suddenly was deeply in trouble. People had already started, a little earlier, to try and find a synthetic substitute for natural rubber. That led to the development of… [Tschoegl searches for word]

COHEN: Neoprene?

TSCHOEGL: Neoprene is a synthetic rubber, yes; however, it's not used for tires. The "prene" in neoprene comes from isoprene, the constituent monomer of natural rubber. The word I was looking for is "guayule." The effort to find a substitute for natural rubber first focused attention on this shrub, one of about 3,000 plants that produce isoprene as part of its life activity. Natural rubber is produced by several varieties of *Hevea,* most usefully by *Hevea brasiliensis.* It is possible to use guayule rubber as an admixture with natural rubber, thus extending the supply of the latter. Firestone had bought some 10,000 acres in the Southwest to grow guayule. Pretty soon, however, the GR-S [Government Rubber-Styrene] program was launched as part of the American Synthetic Rubber Research Program during World War II. This eventually culminated in the production of styrene butadiene rubber, SBR for short. It became the No. 1 rubber for most car tires. Because it generates more heat during flexing than natural rubber, SBR is less satisfactory than the latter in heavy-duty truck and airplane tires.

Well, let me now just say something about my one and only attempt to leave Caltech. Only once was I tempted to move away. How did this come about? I came to Caltech relatively late in life. I had never taught before and had never before been in an academic environment, except when I worked for my PhD with A. E. Alexander at the University of New South Wales. Right from the beginning, I liked Caltech immensely. What appealed to me most was—as anybody at the institute knows—its combination of excellence with a small size. I'm convinced that this is an unbeatable combination. My involvement with the Watson lectures gave me a wider view of what goes on at Caltech. That made my stay here even more interesting. In addition, the campus is lovely, and Pasadena is a great little city. With its various cultural institutions, it also combines excellence with smallness. So I have always loved staying in Pasadena at Caltech. Leaving it would not come easily.

I was induced to defect to Switzerland by Joachim Meissner, whom I have mentioned before. Let me digress here a little. Switzerland is a confederation of substantially self-

governing cantons—states. There are cantonal and federal universities. The top federal university is the ETH, the Eidgenössische Technische Hochschule. Hochschule literally translates into "high school" in English, but it is the German word for an institution of higher learning. The ETH is commonly referred to by its German initials, because its full name is quite a mouthful, particularly for non-German speakers. The etymology of the typically Swiss word "eidgenössisch" is rooted in the Swiss constitution, one of the oldest democracies in the world. The official German name for Switzerland's confederation is "Eidgenossenschaft." A *Genossenschaft* is a community of like-minded people, and *Eid* is the German word for "oath." The confederation was founded in 1291 when deputies of the three *Urkantone*—the original cantons, Uri, Schwyz, and Unterwalden—met on a field on the Rütli Mountain and swore an oath to establish an "everlasting league." The *Schwur am Rütli*, the Oath on the Rütli, is the foundation of the Swiss confederacy.

The ETH is a German-language federal institution. It is in many ways similar to Caltech—it's completely international. When I spent three months there as visiting professor in 1977, the Chemistry Department, for instance, had two professors that were Italians, two were English, one was Czech, three were Americans, and two were Swiss. [Laughter] You know, Switzerland is a small country, and if you want to be excellent you can't rely on home-grown talent alone. The cantonal universities are generally quite good, but they are not nearly as great as the ETH. This great institution is, I would say, Europe's equivalent to Caltech. It's also relatively small, although larger than Caltech—more like MIT in this respect, and more diversified, too—but it is a first-rate technical school on a par with Caltech in excellence, both in teaching and research.

According to the Swiss constitution, if the country were to establish another federal school, it would have to be French-speaking, and the third would have to be Italian-speaking. They did establish the second one—the École Polytechnique Fédérale de Lausanne, abbreviated to EPFL, and they were looking for professors. Joachim Meissner, as a professor at the ETH in Zurich, counseled them on academic affairs. On his suggestion, they invited me, and I spent a day in Lausanne.

COHEN: What year was this, now?

TSCHOEGL: 1977, the same year I was visiting professor in Zurich. In Lausanne I met the president and various members of the faculty in the morning. I also gave a lecture in French on polymers, keeping the subject on a level for professional but nonpolymer people. The president afterwards said to me that he had understood for the first time what polymers were all about. I was then ushered into a large room, where I faced quite a crowd of people. Introductions were made. The president of the EPFL was a French-speaking Swiss who understood but did not really speak German. He addressed me in French, saying, "We want to ask questions of you either in German or in French. But we would like you to answer in German, because one of our professors here, who is on the selection committee, does not speak French." That was Joachim Meissner. And so I was asked in French and answered in German. [Laughter]

After lunch, we resumed deliberations, and they offered me the job on the spot. OK. Excellent conditions. The salary was very good. A laboratory technician and a secretary went with the appointment. I was also offered a grant of a million Swiss francs to set up my laboratories. Among the group of people who interviewed me were several from Swiss industries—Ciba-Geigy and others—and they all promised their technical and financial support for starting research projects. I was really quite interested. I would have had to teach in French, a nice new challenge.

I came back, and Sophia at first was quite excited about it. But then she talked to various friends of hers, and they said, "Well, look, you know, the United States and Switzerland, well, you know…." And our two sons were not too happy about their parents moving to Switzerland. In addition, French was not one of Sophia's five languages. Thus, eventually I declined.

COHEN: You were tempted?

TSCHOEGL: Sorely. I told you that what I like about Caltech is the combination of smallness of size with excellence. The ETH has that, and the EPFL was supposed to become like it also. It's perhaps not *quite* there today yet, but it is good.

After I declined it, one of my former associates, Henning Kausch, assumed the job that had been offered to me. Henning has just retired. He was extremely happy at the EPFL, did very good work there, and became very well known worldwide. His field is fracture mechanics.

He and his wife, Karin, built a lovely home with a marvelous view. Switzerland is a glorious country, as all the world knows. So that's what I missed. I never again looked for another job.

Perhaps I should now say something about materials science. I came to Caltech as associate professor of materials science. Max Williams had set out to promote materials science here at Caltech. Materials science, as an independent scientific or engineering discipline, originally did not have academic support. It was largely the creation of the government funding agencies. These agencies had, rightly, recognized that many important research projects had come to require close cooperation between various different disciplines. However, cooperation between disciplines is only considered acceptable academically when it is completely voluntary. As a professor, you are supposed to be the leader in your field, at least in your institution; you don't usually interact with any other professor there. It is, of course, perfectly legitimate for you to interact with a colleague if both of you want to do so. This would get quite complicated, however, when more than two professors would be expected in interdisciplinary work under the terms of a government grant or contract. In government laboratories and in industry, there usually is a 600-pound gorilla who tells the others what to do. The idea of working under the direction of such a gorilla—even a less-than-200-pound one—would generally be abhorrent to professors. The academic culture shapes the attitude of professors to be negative to the idea of engaging in some joint effort under the direction of a paramount leader. Working as a team requires direction, however. In the academic world, therefore, materials science did not really take off readily, despite the fact that ARPA [Advanced Research Projects Agency]—later DARPA [Defense Advanced Research Projects Agency]—a government funding agency, had set a good deal of money aside for research in materials science.

Max Williams and some other people at Caltech were interested in tapping this source of funding and wanted to show that Caltech could do materials science. We had some very good metallurgists at Caltech in the Division of Engineering and Applied Science. Thus the materials science option had been created, but it consisted of metallurgists only; the other ingredients for materials science were missing. So they looked for a polymer person. Max Williams had one. That was Paul Blatz, whom he had appointed senior research fellow in aeronautics in 1959. Blatz became associate professor of materials science in 1964, and I came to Caltech also as associate professor of materials science in 1965. Equipment was moved over from aeronautics into the basement of the chemical engineering building [the Eudora Hull Spalding Laboratory of

Engineering], where both Blatz and I had our offices. Thus the institute now had polymer presence in materials science. But Paul Blatz was a difficult character.

Bill Corcoran was then becoming the executive officer for chemical engineering. One day, after I had been in materials science for a little over a year, he came down to my office and asked, "Would you consider coming over into chemical engineering? We would like to have you. However, how closely are you tied in with Paul Blatz, because we don't want *him*." [Laughter] I said, "I am tied in with Paul only in the sense that he works on the air force research program of which I am the director. I inherited him with the program from Max Williams." Bill was pleased and said, "OK, then, will you come?" [Laughter] So I did.

My metallurgist colleagues in materials science were all great people, but no one had showed much interest in me. We hardly ever had anything to do with each other professionally. We almost never had meetings, we never ate in the Athenaeum together, and we had nothing in common except that we were members of the same group. As soon as I joined chemical engineering, all that changed. I was suddenly a member of a proactive—and interactive—group. We ate together once a week. We had faculty meetings. We socialized with each other. I felt much more at home in chemical engineering than in materials science. I like to interact with people, and I had to feel that I *belonged* somewhere. I also realized that chemical engineering would be a better source of graduate students for me, and I had, indeed, good students right from the beginning. Fred [Frederick C.] Lindvall was then the head of the Division of Engineering and Applied Science, of which materials science was a part. He had evidently already discussed my case with Jack [John D.] Roberts, who then headed the Division of Chemistry and Chemical Engineering. Lindvall supported my move from materials science to chemical engineering. I was to become full professor of chemical engineering on July 1, 1967. That clinched it.

Paul Blatz left soon after that. He had understood that he would not get tenure. He had an excellent mind, but he had personality problems. He was a spoiled character and just couldn't get along well with people. [Tape ends]

Begin Tape 5, Side 2

TSCHOEGL: Let me tell you some of my *scientific* reasons for moving to our Division of Chemistry and Chemical Engineering. A materials science department is supposed to encompass people in all the main areas of materials: the traditional ones of wood, metals, and ceramics,

including things like stones and bricks; and the new synthetic materials—polymers. Wood is still one of the most important engineering materials; however, relatively few academic departments anywhere in the world have people working on wood. There are some excellent government laboratories that do. One, which I had visited, is a division of CSIRO in Melbourne, Australia. The USDA does first-rate research on wood in this country. However, this is generally not an exciting area of research for academia. Also there is not that much cooperation between wood science and the other three. With those three it is different. You now have metal-reinforced polymers, you have metals coated with polymers, you can make metal-ceramic composites, and there are other new materials that combine at least two of the three non-wood groups. Cooperation between the various groups is definitely needed. But for the reasons I have mentioned before, there are difficulties in this respect in academe. The needed cooperation cannot come about by fiat.

Metals, ceramics, and polymers meet at the very basic level of atoms and molecules, and then goodbye; further on, they've not got much in common, unless you combine them—and that is a new concept. Therefore, only at a rudimentary level can you cover common ground for anyone who wants to embrace the field of materials science. Of course, you can teach students in materials science the basics of the science all three, or four, groups. But after that, the future materials scientist has to specialize in metallurgy, ceramics, or polymers. That doesn't mean you couldn't or wouldn't cooperate, but it means that the cooperation has to be a fairly unique one. If, for example, a polymer person and a metallurgist wanted to work together, they would have to understand each other's discipline at least sufficiently well to be able to cooperate successfully, and that would mean more than just atoms and molecules. So there are some cogent reasons why materials science as a discipline has not taken off very well in academe.

It is, however, well supported by the funding agencies. Money is available. Most universities now include a materials science department. In the meantime, the area has blossomed out. New fields have been added. There is the new area of carbon fibers—carbon fibers are made from polymers—and of carbon-fiber reinforced polymers. Stealth airplanes are now made from them because they are virtually invisible on radar screens. Materials science has since widened the field to include medical polymers and polymer composites and polymeric materials used in electronics, such as encapsulating materials and the many materials used in electronic components such as hybrid circuits. We have some pretty good people working on

those here at Caltech, and there are thriving materials science departments elsewhere at other universities.

I went into materials science in 1965, right at the beginning of the budding excitement about it, and I wasn't sure what was to come of it. So when Bill Corcoran asked me if I would join chemical engineering, I accepted, and I never regretted the move. I might add that the inclusion of polymer science and engineering in the chemical engineering department is perfectly legitimate. Chemical engineering today comprises anything that requires or can do with its unique approach.

COHEN: Would you like to say anything about any of your students?

TSCHOEGL: Over my twenty years as a professor, I was fortunate to have had a group of excellent associates of all kinds: five postdoctoral fellows, one visiting associate, ten PhD students, and seven MS students. I will mention only a few by name. Some I have indeed already mentioned. Most of my graduate students did very well in later life. Some went into academia. Bob [Robert E.] Cohen and Vic [Victor W.] Chang, who obtained their PhDs with me, became professors at MIT and USC, respectively. And there is Henning Kausch at the EPFL and Daniel Froehlich at the Centre de Recherches sur les Macromolécules in Strasbourg, who were both postdoctoral fellows in my group. It was a happy business at Caltech to work with this international crowd of bright young people. I had only one, an MS student whose name I haven't mentioned, whom I had to let go. He was completely undisciplined and didn't take his work seriously.

I tended to treat my young coworkers largely as if they were my sons. Let me therefore digress here and say something about my two sons that I had with my wife Sophia. They turned out to be greatly different characters. Adrian studied political science at UCLA and upon graduation in 1969 served for three years in the military. In so doing, he was primarily discharging his duties to the ROTC [Reserve Officers Training Corps], which had partially supported him in his studies. He spent the middle one of these three years in Vietnam as advisor to a Vietnamese army unit, having volunteered for this assignment. Despite Sophia's and my considerable uneasiness about his decision at the time, I could not very well object to it, since I had myself volunteered for my year in the Ukraine. Fortunately he came back from Vietnam

safe and sound, with a wealth of experience but also with some disillusionment of the military as a career for him.

After his return, he obtained a master's degree in public administration at UCLA and then went on to obtain a PhD in business administration from the Sloan School of Management at MIT. Following seven years as assistant professor of international business at the University of Michigan, he temporarily abandoned an academic career by spending six years in Tokyo working as macroeconomist for the Swiss Bank Corporation. He earned a typical expatriate salary, with all sorts of perks, and came back with a great deal of money in the bank. He and his wife, Naomi, who is English by birth, handle money prudently. Eventually he resumed academic life as a professor at the Wharton School of the University of Pennsylvania for several years. The money he made in Japan now enables him to lead a leisurely life of teaching, studying, and writing. Naomi is associate director of student services at Wharton. They have a daughter, Elizabeth, who is currently working at the Dana-Farber Cancer Institute in Boston. Their son is a freshman at Columbia University in New York.

Christopher, six years younger than Adrian, graduated from UC Irvine in 1976 with a BA in psychology. Although quite bright, he was not given to academic pursuits and preferred to work in more practical fields. During his last year at UCI, he decided to become a professional photographer. Photography had been his hobby since his high school years. He enrolled at Art Center College of Design, in Pasadena, from which he graduated in 1979 with a BFA degree. The curriculum at the art center was a tough one, involving little book learning but extensive and imaginative hands-on learning, not only in the photo lab but also outside in the real world. While studying at the art center, Christopher worked here at the institute part-time for three years as a photographer. I had nothing to do with this appointment! We are not talking nepotism here. He had met the institute's photographer professionally, and he recommended him to the powers that be at the institute.

Both my sons had Caltech connections. While at UCLA, Adrian worked some summers in the geology library, helping with shelving books and other such tasks. And yes, I did have something to do with *his* appointment.

When Christopher graduated from the art center, Caltech offered him the job of institute photographer as a permanent position. However, Christopher was determined to strike out on his own. He set up his own studio, where he worked primarily in architectural photography for large

firms, like Morgan Stanley or Goldman Sachs. His work soon became very sought after, and by the age of forty he had become quite wealthy in his own right.

To the consternation of all of us, Chris was diagnosed with cancer of the stomach in December of 1993. He was operated on at once. A second operation followed in December of 1994. Both operations were accompanied by aggressive chemotherapy, from which he suffered greatly. It was all in vain. He died on December 10, 1995.

He bore his cross with incredible forbearance and immense fortitude. He never railed against his fate. From the beginning—I had already retired by then—I devoted myself totally to the task of aiding him, alleviating his pain, and making him accept the inevitable. For the better part of his last year, I moved into his house. His death affected me enormously. I am not even now, many years later, really over it, and do not wish to dwell on it here further.

I never tried to influence either of my sons in their choice of profession. Having learned that my preordained career in business did not and never would have made me happy, I concentrated my efforts at furthering their chosen career paths by providing both of them with any assistance they needed—and that I was able to afford. I enjoyed with both a close and warm relationship. Neither ever gave me or Sophia any trouble. I never needed to use any disciplinary measures. Very rarely were there even harsh words. We discussed calmly any of the few problems that might have come up occasionally. This was possible because both were eminently reasonable, even though they were so different in many other respects. They grew up in a loving and caring environment, and Sophia and I are proud that we were able to provide it.

I decided to include this digression in my recollections because I am convinced that my personal profile would be incomplete had I not said anything about my role as a father. However, let me now resume my tale about my role as a teacher and as a researcher.

Before I came to Caltech, no one had taught polymer engineering courses at the institute. In chemistry, Jack Roberts lectured on polymer organic chemistry as part of his courses. [Robert H.] Grubbs was and is concerned with polymers but not with their mechanical properties. Wolfgang Knauss in aeronautics, a student of Max Williams, is primarily concerned with the fracture mechanics and the ultimate properties of polymers. We have often collaborated.

In chemical engineering, there is now John Brady, who is working on the rheological properties of dispersions and other solutions largely involving polymers. Julia Kornfield does excellent work on mechanical properties, using optical methods. Zhen-Gang Wang, an

outstanding polymer physicist and statistical mechanician, works with both of them. So there is now a good, viable group at Caltech in chemical engineering that does polymer work. Some also have links—Julia Kornfield, for instance— to the chemistry side of the division. All this is good. But I feel proud of the fact that I was the first one.

Another field concerned with polymers came into prominence in chemical engineering. This is bioengineering. The division now has an executive officer for chemistry, another one for chemical engineering, and more recently added one for biochemistry and molecular biophysics. The bio fields are concerned with biomolecules—predominantly with proteins and DNA. Those are polymers, but they are polymers of a rather different kind. What I said earlier about metals and polymers is also true of synthetic polymers and biopolymers: Their disciplines meet at a rather basic level and then they go their separate ways.

In chemical engineering, we had some very good people doing work in bioengineering. Jay [James E.] Bailey, for instance, who then left [1993] and went to ETH. When Jay was here, we often talked about bio things; I was interested in bioengineering. One area where bioengineering and synthetic polymers meet is the area of the application of synthetic polymers to biological problems—for instance, to implants and prostheses.

COHEN: And they're really very important.

TSCHOEGL: They're important, indeed. We have much work going on in chemical engineering in the bioengineering fields today, partly as joined efforts with the Division of Biology. However, although I was interested, I never went into it actively, because I retired.

Now, you had asked me about Caltech presidents. [Laughter] It wouldn't have occurred to me that I should talk about them.

COHEN: Well, why not?

TSCHOEGL: Indeed. Let me reminisce, then, about those I knew but also about other colleagues active in the institute's administrative work, such as some vice presidents and executive officers, as well as some of their wives.

I was hired by Lee DuBridge. Robert Bacher was provost then. I had a great admiration for both. DuBridge had a matchless way of addressing a group. What he said was always terse,

cogent, and to the point. I did not know either of them intimately, because I was just a lowly associate professor when I came, you know—but I did like them personally and I liked the way they ran the institute. It is, of course, well known that Lee DuBridge as president and Bob Bacher as provost worked very well together and that they were effective—naturally, they had hired *me*! [Laughter]

So when their era passed, the next one who came on board was Harold Brown [1969-1977]. He had been secretary of the air force for four years before coming to Caltech. Before that, he was director of defense research and engineering in the Department of Defense. I was impressed by the fact that Harold Brown went around to meet every faculty member on campus to acquaint himself with his or her work. He listened to me attentively when I told him what I was doing. He asked one or two questions, from which I easily became convinced that he followed my exposition with interest and understanding. I knew his background, of course. I knew that he was a sort of *Wunderkind* and had started his career early in life.

At that time, I was consulting at the Naval Weapons Center out in China Lake with a person whom I don't want to mention. If anybody really wants to research it, they can find out his name. [Laughter] He said to me, "My God, I hear that Caltech has hired Harold Brown to be president. My God! You really are in hot water there." Incredulously, I asked, "Why?" These guys at the Naval Weapons Center and everybody else who was working in the weapons establishment had to interact with Brown from time to time when Brown was director of defense research and engineering. This fellow whom I won't mention had to give a talk to Harold Brown about what he was doing. He told me, "Brown was downright obnoxious." He had hardly let him talk and criticized whatever he said. So this fellow had formed a very low opinion of Brown—not of his intelligence or dedication or knowledge or whatever; that was beyond question all right. But he didn't like the way that Brown treated the people who worked under him.

Thus I was very much surprised when I got to know Harold personally. His behavior was completely different from what I had been led to believe. He was always polite and never flaunted his intellectual superiority. In fact, he seemed almost deferential to Caltech professors in personal contact and interacted well with all of us, as far as I know. I had several occasions to talk to him on official business in my capacity of chairman of this or that institute committee, and I must say that I liked him and admired him.

What is the role of a president? Partly, it is to provide overall guidance, but not in the academic area, because that's the job of the provost. The provost and the president have to work closely together, but the president represents the institution to the outside. He is its figurehead, if you will, but not a mere figurehead by any means. Harold Brown successfully modernized and streamlined the institute's administration. In a self-supporting institution like Caltech, the president's major role, however, is the raising of support, the raising of money. Harold Brown was not really comfortable in this role. If Brown had one problem, it was that he apparently was not completely at ease in dealing with people who he felt were not on his level intellectually. He did not look down upon them, but he seemed to be unsure of himself in their presence. At least that's how I understand it.

Fortunately, there was Bill Corcoran. When Harold became president, he made Bill Corcoran vice president for institute relations. Some people grumbled, "What do we need another vice president for?" But I think Bill Corcoran was a good vice president for institute relations. He had an excellent way of dealing with potential donors and was very good at bringing money into Caltech. He and Harold Brown worked well with one another, much like Lee DuBridge and Bob Bacher did. Harold and Bill complemented each other. Harold gave Bill support, and Bill gave Harold support.

Harold Brown's wife, Colene, really went out of her way to be a good president's wife. Sophia liked Colene and they worked well together as, for example, in the Gallery Goers, as I have mentioned.

COHEN: Colene had Washington training. [Laughter]

TSCHOEGL: Yes, she did, just like her husband. I once asked Harold, "How come you remembered my first name the second time you met me?" He told me, "I learned to do that in Washington."

Harold quit precipitously in 1977 to take the job of secretary of defense in the Carter administration. But he came back often to visit Caltech. I liked and admired him, and I think he was most certainly not a bad president.

Bill Corcoran died, totally unexpectedly [August 21, 1982]. He was a very sportive guy, you know. Although he traveled widely in this country on business, he had never been outside

of the United States. The rest of the world had no interest for him. He thought traveling there totally unnecessary and a waste of time. I have this from him personally. The only time he would leave the continental United States was to go to Hawaii, because Martha, his wife, was born in Hawaii. One day, when they were in Hawaii, he apparently suffered an attack of some sort. He woke next morning and turning to Martha, he announced, "I think I'm dying." A few minutes later he was dead. They were in a remote part of Hawaii—in Maui, I believe—and there was no way to call for medical help quickly enough. He was so athletic, and suddenly he was dead. It was terrible. His sudden death affected me greatly. It was he who had brought me into chemical engineering.

I learned another lesson from his death. He was a big figure in chemical engineering nationwide—in the American Institute of Chemical Engineers and in other professional organizations. He was proactive, forthright, and decisive, and he was vice president of institute relations—a big guy on campus at Caltech. And a few months after he died, no one ever mentioned his name again. *Sic transit gloria mundi,* you know? This is how transitory worldly glory is. I don't expect that there will be tears and the gnashing of teeth when I go [laughter], but it is depressing nevertheless to contemplate how soon the world will forget you. So that was Bill Corcoran, who had been executive officer for chemical engineering before he became vice president for institute relations.

It was Bruce Sage who turned chemical engineering at Caltech into what it has become. He took over from [William N.] Lacey, who was the first chemical engineer at Caltech. Bruce is now dead also. I wish you could have interviewed both Sage and Corcoran. Bruce was a very energetic person and apparently not so easy to work with, but he was a terrific organizer who kept a bevy of people busy. When I came here, Bruce was the senior member in chemical engineering, and it was he who welcomed me officially. He invited me for cocktails and lunch in his apartment, together with Bill Corcoran. This was very different from my debut in materials science. Pretty soon after that, however, he retired and went to Arizona, where he owned a huge cattle ranch.

After Bill became vice president for institute relations, Neil [Cornelius J.] Pings became executive officer for chemical engineering [1969-1973]. Neil Pings did fine work on the structure of liquids, using X-ray analysis. He was a very ambitious person. Later, Pings became

vice provost [1971-1981]. But he had still greater ambitions, and when he realized that he would not become president of Caltech, he left.

COHEN: He went to USC.

TSCHOEGL: That's right. And he became provost but didn't become president there, either.

COHEN: Then he went to Washington.

TSCHOEGL: Then he went to Washington. If I'm not mistaken, he became president of the Association of American Universities. I never had any problems with him while he was executive officer for chemical engineering or vice provost of the institute. What I liked about him was that he was both a good researcher and a good administrator. So that was Neil Pings.

Harold Brown's precipitate departure required that the board of trustees appoint an interim acting president. This was Robert F. Christy. While he was evidently comfortable in his job as provost, he literally hated the job of acting president. The former was primarily an academic position, the latter was not. Fortunately, Christy did not have to suffer long.

Our next president was [Marvin L. (Murph)] Goldberger [1978-1987]. Goldberger was an individual totally different from Harold Brown. He came from Princeton, where he had been head of the Physics Department. I feel that Murph never truly realized what the job of a Caltech president entailed. He was a head of department at Princeton, but I think the job of president at Caltech was not quite the right thing for him. He stayed essentially a department head. One thing I have to say about Murph—he had wonderful presence. He had this leonine mane—he looked very much like a president. And he was very civil and nice. Whenever I had anything to do with him, he stood up to greet me, and he opened the door for me when I left his office. A lowly professor notices things like that. Several good things happened on his watch, but as far as I know he had very little to do with either. The institute got $70 million from the Keck Foundation for the Keck Observatory in Hawaii. And then this woman, whom nobody here ever even knew anything about, left $20 million to Caltech.

I had told Murph that I had lived in Hungary. At least three times, at a barbecue or some other social function at the institute, he told me that his parents were poor Hungarian Jews. He mentioned to me the name of the Budapest ghetto where they came from. You know, I found

that peculiar. What the hell do I care whether he's Jewish? I couldn't care less. And to rub it in! Not that I really minded it, but I found it slightly ridiculous. He never forgot to bring it up. He did it at least three times. He was—rightly—proud of the career he had made, but I think he carried a chip on his shoulder. Once, Sophia overheard his tale and asked me, baffled, "What's the matter with him?" [Laughter]

Murph had another habit I found strange. Whenever he met anyone, he told them right away that he was a Democrat. He told everybody that, including our rich Republican donors. I heard from someone in the know that one donor in particular was upset about it. He said, "I know that he's a Democrat, but why does he rub it under my nose, knowing that I am a Republican, when he wishes me to contribute money to Caltech?" This illustrates why I said that Murph never really made the transition—at least, as I see it—from department head to president.

Murph's main problem, however, was his wife. I say this quite consciously. Mildred Goldberger had a technical background—I don't remember what—and she was very proud of that. She did not respect anybody who was not on what she thought was her intellectual level, and she could be downright abrasive about it. She alienated practically everyone on campus. Even though I had hardly any direct contact with her, it bothered me greatly, and I know that it bothered most of my colleagues, too.

There was a wealthy elderly widow, Bridget Emerson, who lived in a two-bedroom suite in the Athenaeum. Her husband had been a professor of geology at the institute, I believe.

COHEN: Her husband was not a Caltech professor. I think he was a Caltech graduate, but he was not a Caltech professor.

TSCHOEGL: OK. Anyway, Bridget liked to stay in the Athenaeum.

COHEN: And she did for many years.

TSCHOEGL: Indeed. Sophia and Bridget became friends. I think Bridget felt lonely, and Sophia has a complex about lame ducks. We invited her to our house, and she invited us to the Athenaeum, where she used to arrange great little dinner parties; these were always very nice. But she was not a highly intelligent person. She was a persistent traveler. She'd been everywhere, including Tibet and I don't know where else, but she never could talk about it. She

could tell you she had been there, but she couldn't be drawn out about it. But at least she enjoyed her life. She joined the Caltech Associates and even became a member of the President's Circle. She generously supported Caltech, not only through her membership in the Associates but, I believe, also through donations or in other ways. She truly loved Caltech.

At some function in the Athenaeum one day, Bridget was standing not far from Mildred, and Mildred turned to somebody else and loudly—*too* loudly—voiced a derogatory remark about Bridget's intelligence. Now, you don't do that if you're the wife of the president! Sophia was standing nearby also, and she related this incident to me.

When the search for the next president was on, each division was represented on the search committee by one of its members. The representative from the Division of Chemistry and Chemical Engineering had lunch one day in the Athenaeum with many of us to tell us about how things were going. He evidently couldn't mention names, but he could inform us what the committee was concerned with and how they were going about it. Somebody asked, "Are we looking also at the prospective president's wife?" The representative said, "You bet we are!" Amused titter all around. That shows you that Mildred, with her egregious lack of tact, was not popular at all.

Murph wanted to stay for another term in office, but the board had already been informed that this would not sit well with the faculty. The faculty did not dislike Murph. As I perceived it, they didn't think that he was a truly effective president, but they did not dislike him. But almost everybody disliked Mildred. So Murph was told that he'd better look for something else. He left here and almost immediately was offered the directorship of the Institute for Advanced Study at Princeton.

COHEN: So then we go on to [Thomas E.] Everhart [1987-1997].

TSCHOEGL: Tom Everhart I knew little. Sophia knew Doris [Mrs. Everhart] better, because Sophia participated in a reading circle at the institute that Doris Everhart led. I never had anything much to do with Tom. Shook hands, nothing else. I had already retired when he came aboard. I don't think that he was an outstanding president. He was personable, he was liked, but he does not seem to have shown new paths for Caltech to embark on. Nothing much of note that I know of happened on his watch.

David Baltimore [1997 - present] became our next president. I shook hands with him, too, at some reception after he took up his new position. I also met his wife [Alice Huang]. I addressed her with the pitifully few words of Chinese I know, and she said, "I am sorry, I don't speak Chinese." [Laughter] That's all I know about the Baltimores. However, everybody with whom I spoke about him appears to harbor a good opinion of him, and so we are looking forward to several fruitful years here at Caltech under new guidance.

In conclusion, let me mention some activities to which I may not have referred before. I chaired, arranged, lectured at, and otherwise participated in scientific meetings and conferences here and abroad—too many to list here. I was and still am a member of several professional societies in this country and abroad. I am an honorary member of the International Committee on Rheology, the Deutsche Rheologische Gesellschaft, and the Sociedad Mexicana de Reología. I served or am serving on the editorial advisory board of professional journals, and I took part in several government advisory activities. For five years, from 1973 to 1978, I served on a blue-ribbon panel of the National Academy of Sciences which wrote ten volumes on the fire safety aspects of polymeric materials. I was editor of the first volume [*Fire Safety Aspects of Polymeric Materials*: volume 1, *Materials: State of the Art. Report of the Committee on Fire Safety Aspects of Polymeric Materials*, National Materials Advisory Board, Publication NMAB 318-1, National Academy of Sciences, Washington, D. C., 1977].

Finally, let me mention that my eightieth birthday was celebrated in 1998 at a symposium and dinner in my honor at the fifth International Conference of the European Society of Rheology held in Portorož, Slovenia, and organized by Igor Emri. For the same reason, John Brady, then executive officer for chemical engineering, arranged a most pleasant intimate dinner in the Athenaeum for the chemical engineering faculty and their spouses. That, again, gave me a good, warm feeling of being appreciated and of belonging, even after I had retired from active duties at the institute. I have always held the chemical engineering faculty in high esteem and have always valued them as friends, not merely colleagues.

COHEN: And your work goes on?

TSCHOEGL: Well, I still publish, and I publish under the byline of the Division of Chemistry and Chemical Engineering. I feel that I'm still a contributing member of our great institution.

COHEN: You're still an asset.

TSCHOEGL: [Laughter] If you like to call it that. But otherwise my work at Caltech is essentially finished. I have contributed—perhaps not as much as I could have and possibly not as much as I should have. The main reason for that is that I came into science at the advanced age of forty. Most everybody does their best work in science at a much earlier age. When you get into science at my age…

I was forty-seven when I came to Caltech. At that age, you are no longer trying to be a firebrand and go into totally unknown, uncharted areas. I had not learned—I learned it too late—that at institutions of higher learning you can do it at any age, and that applies to Caltech in particular. As I didn't realize that, I never even tried to do it. I think I did reputable work, some pioneering work, but no firebrand stuff. So I never got the Nobel Prize, as you know.

COHEN: Well, you're not alone in that. [Laughter]

TSCHOEGL: That's right. [Laughter] [Tape ends]