CHEMICAL HERITAGE FOUNDATION

# EUGENE G. ROCHOW

Transcript of an Interview
Conducted by

James J. Bohning

in

Fort Myers, Florida

on

24 January 1995
(With Subsequent Corrections and Additions)

ROCHOW, EUGENE G.

CHEMICAL HERITAGE FOUNDATION
Oral History Program
RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to my participation in a tape-recorded interview conducted by Dr. James J. Bohning on 24 January 1995. I have read the transcript supplied by Chemical Heritage Foundation and returned it with my corrections and emendations.

1. The tapes, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use, and publish the Work in part or in full until my death.

3. The manuscript may be read and the tape(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.

4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

a. ________ No restrictions for access.
**NOTE:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, PA.

b. X My permission required to quote, cite, or reproduce.

c. ________ My permission required for access to the entire document and all tapes.

This constitutes our entire and complete understanding.

(Signature) Eugene G. Rochow
Eugene G. Rochow

(Date) November 4, 1997

Rev. 3/21/97

Upon Eugene G. Rochow's death in 2002, this oral history was designated **Free Access**.

One may view, quote from, cite, or reproduce the oral history with the permission of CHF.

***Please note*****:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation (CHF) Oral History Program to notify CHF of publication and credit CHF using the format below:

> Eugene G. Rochow, interview by James J. Bohning at Fort Myers, Florida, 24 January 1995 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0129).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

**EUGENE G. ROCHOW**

| | |
|---|---|
| 1909 | Born in Newark, New Jersey, on 4 October |

## Education

| | |
|---|---|
| 1931 | B. Chem., Cornell University |
| 1935 | Ph.D., chemistry, Cornell University |

## Professional Experience

| | |
|---|---|
| 1931-1932 | Research Chemist, Halowax Corporation |
| 1932-1935 | Assistant Chemist, Cornell University |
| 1935-1948 | Research Chemist, General Electric Company Research Laboratory |
| | Harvard University |
| 1948-1951 | Associate Professor |
| 1951-1970 | Professor |
| 1970-Present | Professor Emeritus |

## Honors

| | |
|---|---|
| 1948 | Honorary M.A., Harvard University |
| 1949 | Baekeland Medal, American Chemical Society |
| 1951 | Myer Award, American Ceramic Society |
| 1958 | Mattiello Award, Federal Paint & Varnish Society |
| 1962 | Perkin Medal, Society of Chemical Industry |
| 1964 | Honor Scroll, American Institute of Chemists |
| 1965 | Frederick Stanley Kipping Award, American Chemical Society |
| 1966 | Honorary D.Sc., Carolo-Wilhelmina Universität Braunschweig |
| 1968 | Chemical Pioneers Award, American Institute of Chemistry |
| 1970 | Award for Excellence in Teaching, Manufacturing Chemists Association |
| 1971 | Inventor’s Award, General Electric Company |
| 1973 | Norris Award for Teaching of Chemistry, American Chemical Society |
| 1983 | Alfred Stock Medal, German Chemical Society |
| 1992 | Honorary Doctorate of Natural Science, Technische Universität, Dresden |

## ABSTRACT

**Eugene G. Rochow** begins this interview by talking about his early years in New Jersey and his family background in the Brandenburg region of Germany. Rochow's interest in electricity and silicon stems from his first radio set, which he put together using silicon crystals. Sparked by his brother Theodore's interest in chemistry, Rochow joined his brother as a chemistry assistant both in high school and at Cornell University. He worked as both lecture and laboratory assistant to Louis M. Dennis, then chair of Cornell's chemistry department, who referred Rochow to Alfred Stock as a lecture assistant while Stock was guest professor at Cornell. Here Rochow relates some anecdotes about Alfred Stock. Although the Depression caused severe cuts in job opportunities, Rochow found employment with the Hotpoint Company, a General Electric Company subsidiary, where he conducted research on periclase. During this time, Rochow produced ethyl phenyl silicone, which Corning Glass Works had also just produced, for use as an insulator. He then produced methyl silicone. This led to patent and publication difficulties between GE and Corning Glass Works, now the Dow-Corning Corporation. During this time, Rochow discovered how to produce methyl silicone, first using magnesium, then using silicochloroform and copper. Further, he and Charles E. Reed developed a way to manufacture methyl silicone using fluid-bed catalysis. Rochow continued his research on silicone production and zinc promotor development until his transfer to Richmond, Washington, where he conducted research on nuclear fission as a source of domestic energy. When the U.S. Government requested GE to work on nuclear propulsion for naval vessels, Rochow, a Quaker, left to teach chemistry at Harvard University. Rochow closes with comments on how his Perkin Medal award brought him international recognition and expanded professional opportunities.

## INTERVIEWER

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

## TABLE OF CONTENTS

**INTERVIEWEE:** **Eugene G. Rochow**

**INTERVIEWER:** **James J. Bohning**

**LOCATION:** **Fort Myers, Florida**

**DATE:** **24 January 1995**

**BOHNING**: Dr. Rochow, I know you were born in Newark, New Jersey, on October 4, 1909. Could you tell me something about your father and your mother, and your family background?

**ROCHOW**: Yes. True, I was born in Newark, New Jersey, but before I was one year old, the family moved out to rural Maplewood, west of Newark. My father bought a piece of land in the middle of a strawberry farm there. He built a house there because he didn't think the city was a good place to bring up children. I have an older brother, too.

There we were in the middle of the strawberry patch. Why out in Maplewood? Well, there was an inter-urban rapid transit line that ran through Newark out to Springfield, New Jersey, which is famous for its part in the Revolutionary War. That was about two hundred yards from our place. My father could ride the fast streetcar into Newark and work there; so there I was in Maplewood all during my childhood.

**BOHNING**: What did your father do in Newark?

**ROCHOW**: Well, he was what would be called a manufacturing chemist. That is, he began in a very modest way at a leather tannery. He developed ways of making fancy bookbinding leather—how to color it and make it permanent, and so on. He educated himself and then took courses in business management. Eventually, he rose to be president of the company. He was a very hard-working man. He set us, my brother and me, a good example.

I have here something interesting. This book, which is a family history, is bound in leather that my father made. That was a hundred years ago.

**BOHNING**: My goodness, it's beautiful.

**ROCHOW**: Notice the feel and perfect condition. You go in the library and try to find leather-bound books like that.

**BOHNING**: This says, "Berlin, 1861." Is that where he came from, then?

**ROCHOW**: No. He was born in this country, and his father before him. His grandfather came from Lower Saxony—well, Mark Brandenburg, if you know where Brandenburg is, up in northwestern Germany.

As for the family, that's the family name. The family began in Switzerland. It seems that Charlemagne hired some unemployed Swiss knights to go north with him and fight the heathen Wendisch in northern Germany. When they succeeded in subduing that and partly Christianizing the Wendisch, the knights were rewarded by being given big plots of land. Up there, just a little south and east of Brandenburg—between Brandenburg and Berlin—there are towns where the churches were built by the Rochow family, where they have the castles and the family manor houses, and so on. It's a very interesting region to visit. I went there twice, the second time with my family—a kind of family research project. It's amazing to see one's name on a church that was built in the sixteenth century, and to find the history of all the deeds of the family from way back.

I'm rambling too far, but my most famous ancestor of that time, who lived in a little town just south of Brandenburg, was Friedrich Eberhard von Rochow. He was famous because he wrote a book, *Der Kinderfreund*, a primer for schoolchildren (1). The schools at that time were private schools for rich kids. This man, Friedrich Eberhard, had the very farsighted but revolutionary idea that farm children should have a chance at education. He was the first one to institute a public school. It wasn't public in the sense that the government organized it. No, he built a little schoolhouse, which still stands there. He wrote this primer. He hired teachers to teach all the surrounding children for free. This was an astonishing thing at the time. His influence was felt all over northern Germany. It's a very honored name.

This copy, by the way, of the primer, *Der Kinderfreund*, is a reprint of the original. It was reprinted by the DDR, the Soviet government of East Germany. This in itself is surprising, because the churches—and there were other castles around there that bore the family name—were all carefully—what would you call it? Not just de-emphasized, but the Communists went around and even went so far as to break open the family vaults, remove the bodies, and put them out of the churchyard. They didn't want any trace of the nobility or the landowners to remain. After some fifty years of that, they began to think about their roots. They wanted to prove that the communist lands and people weren't so bad after all. That was one of the results, the resurrection of a few historical books.

Sorry I took so long. Go ahead.

**BOHNING**: No, no that's fine. It must have been pleasant in a way, growing up in the country in Maplewood.

**ROCHOW**: Oh, yes. We had real winters there and went sledding on the hill a short distant from our house, and of course ice skating in the winter. We enjoyed the summer as well.

The nearest school was at a region down near Irvington. It was about a mile and a half from where we lived. Of course we walked it, my brother and I, and later on a cousin who came to live near us. It was a long walk for a little fellow, but gee, I never thought anything of it, because everybody did it. In back of the school there was a ravine with a nice brook back there, so there was an informal nature study place. Crawfish grew under the flat rocks, and there were little fish in the brook. I remember getting sent to the principal's office because I forgot all about time and stayed down there, at lunchtime, down at the brook. It never occurred to me to go back to school. Suddenly it dawned on me, "I should be back in class."

Things like that happened. A very nice school, though. However, after grade school we went to what would be called downtown Maplewood now. That was a village, actually. That's where the junior high school was. From there, we went up to South Orange for high school. That's where I graduated from high school.

**BOHNING**: Your older brother was two years older than you, is that correct?

**ROCHOW**: Yes.

**BOHNING**: At some point, I read that you had a small attic laboratory. Was that his doing or yours?

**ROCHOW**: Well, both. The house had an enormous attic in it, and there was an actual stairway going up there. Of course, during bad weather we could play up there in a big space like a barn, and had games. We set up a movie screen—well, there weren't any movies. We had a projection lantern, and could show slides and then project postcards by reflected light, and have shows up there, and charge the neighborhood kids three cents. It was great fun.

When we got older, my brother was interested in chemistry, but I was interested in electricity. We, of course, set up a little laboratory up there. That's where I made my own battery chargers and set up experiments with the first radio sets. You're not old enough to remember those.

**BOHNING**: The old crystal sets?

**ROCHOW**: Yes. There I made my very first crystal set. I badgered my mother for one of the oatmeal boxes—a cardboard cylinder. I wound cotton-covered wire on it, taking taps every ten turns, and then at the end, taps at every turn, for tens and units. I coated it with shellac to hold the wire on. Then, of course, I needed a crystal detector. The antenna was just a long wire strung from the house to a tree way out there, a hundred or two hundred feet. There was a radio station in Newark, WOR—still famous. We could get a good signal out in Maplewood.

Then, I needed a crystal detector. [shows Dr. Bohning a gray metallic-looking substance] I wonder if anyone ever sees this stuff except me. Maybe you know what it is, Dr. Bohning. No? Yet you use pieces of this every day. This is elementary silicon—a semiconductor.

**BOHNING**: Oh, I see. I should have guessed that's what it would be. [laughter]

**ROCHOW**: When it's crystallized, and a single crystal is grown slowly and carefully from the melt, then it's sliced into thin wafers. From that come all the computers and all the devices—every digital clock, every radio, every television set, and devices that control the motor in your car, and so on. All the marvels of the semiconductor age come from silicon.

You could, of course, get a crystal of galena and break it up and use that for a crystal detector for a radio set. Galena is a natural lead sulfide. Such a crystal detector had to be bought from a supply house. That was beyond our reach, but this stuff, crude silicon, was an alloying agent for making ferrosilicon. Every AC motor, every AC generator, every transformer uses ferrosilicon laminations in the magnetic core.

Maybe I'm lecturing too much about this.

**BOHNING**: That's all right.

**ROCHOW**: Why ferrosilicon? Why not a soft iron core? Well, the alternating current, of course, induces a current in the core as well as in the copper windings. What's induced in the core is pure waste. It only overheats the thing. Using <u>thin</u> sheets—laminations—of ferrosilicon with an oxide scale on the lamination keeps the current from going through one layer to the next. The ten percent of silicon in the iron increases the electrical resistance enormously, cutting the losses way down. There wouldn't be any transformers out there without ferrosilicon.

Many companies experimented with ferrosilicon for drainpipes, because it is chemically resistant. In those days, the 1920s, if you could find somebody who had a five-pound lump of ferrosilicon, you could ask them to whack off a chunk, and then you could make yourself a hundred crystal detectors from that chunk. I got a flat sliver of silicon and strapped it down to a little short piece of copper onto a wooden board, and took a phosphor bronze spring—a little piece of so-called brass spring wire, sharpened to a point. I touched the silicon with the sharp point of the spring until I found a spot that worked. The silicon rectified the radio frequency current that came in on the antenna and gave a direct-current counterpart, which of course was modulated and produced sound in a discarded telephone receiver—or if you were really rich, a pair of headphones. There would be music out of the air! No power input locally—all the power you needed came from the broadcast station itself.

We gloried in that crystal set, my brother and I. When we got enough money to buy a pair of headphones, we took them apart and lay there on the bed listening to opera late at night, to music, to news, in the dark. [laughter] My mother would come in and say, "How do you shut this thing off?" "There is no shutting off, mother." That was magic to her. I went on with my interest in radio.

**BOHNING**: How did your brother develop his interest in chemistry?

**ROCHOW**: Well, the question was, what should he do after high school? He'd like to go to college. All right. We were both admitted to Cornell on certification. Perhaps that's an unknown term to you. Some few high schools at that time, if they had proven the abilities of their graduates long enough, could have outstanding students certified by some universities like Cornell. This meant that any student who was high enough in the percentile of the senior class, and recommended by enough faculty people and the principal, didn't have to take the college board exams. He could go right in.

Well, what to study there? My brother Ted was urged by my father to study some chemistry, because there was a possibility of succeeding my father as the working chemist in the plant. So Ted studied chemistry. I was two years behind him. When it came time for me to graduate, I wanted to go to MIT and study electricity and radio stuff; but Ted said, "Look, I'm established up there at Cornell. Why don't you come up and study chemistry? We could have a lot of fun together. Maybe we could buy a little car then and drive back and forth to New Jersey, and be together."

He convinced me to try it. I guess I've been in chemistry ever since, but I've never abandoned electricity. You don't realize it as you sit there, Dr. Bohning, but this room is wired for sound. That adjoining bathroom is wired for sound; there are speakers there, too. The cable input from cable TV is all over this little place in each room, so we can tap off what we want any place. I installed all that, and I still glory in it.

**BOHNING**: Did you have any chemistry classes in high school, before you went to Cornell?

**ROCHOW**: Oh, yes indeed. We had a very good physics teacher in the Columbia High School in South Orange, and a capable chemistry teacher. Both were very bright and engaging teachers.

My brother and I got along so well in chemistry that we were designated to set up the reagents for the laboratory sessions in high school, and later at Cornell, too. First Ted, and then

I, got that job as an undergraduate dissolving the potassium hydroxide, diluting the acids from the carboys, and refilling all the bottles in the enormous freshman laboratory. We'd load the stuff in the basement prep room onto an elaborate kind of tea cart, take it up in the elevator, and go all the way around the laboratory, filling the bottles. If the right stuff was not out on the right shelf at the right time, we caught hell. [laughter] We earned enough money that way to see us through our undergraduate years.

**BOHNING**: You had mentioned two teachers. I'm assuming these were your chemistry and physics teachers. Was Olin D. Parsons the physics teacher?

**ROCHOW**: Yes, he was the physics teacher. Now, who was the chemistry teacher? You have so much information there.

**BOHNING**: Frederick J. Crehan?

**ROCHOW**: He was the math teacher.

**BOHNING**: Oh, all right.

**ROCHOW**: He taught me to respect math, even to like it—and almost to love it, because he showed us so many things that could be done with math as a tool and as a language, if you just have the patience to learn about it. Here is my yearbook from Columbia High School in science. Here is Mary Allen. Parsons taught the physics courses. Mary Allen was the chemistry teacher, and she was a very pleasant, easygoing person. We often turned to Parsons for enlightenment on the applications of mathematics to science.

**BOHNING**: You graduated from Columbia High School, then, in 1927?

**ROCHOW**: Yes, I graduated in 1927, my brother in 1925, yes. I went straight to Cornell.

**BOHNING**: How did you finance your way? You said you did lab preps. Did your father help in financing?

**ROCHOW**: My father did, yes. My father contributed a certain amount. He said, "That's all I can do. From there on, you'll have to work part time." Rather than take a job in a restaurant or cafeteria or what not, we tried to get jobs in the chemistry department and got various odd jobs. Setting out the reagents was one of them. When I got to my senior year, I got a fellowship there. Well, that went on into my graduate years.

The Heckscher Research Foundation established some extra fellowships for promising students, especially graduate students at Cornell. They gave a stipend equal to what the teaching assistants got. It wasn't just for free, though. You had to work on a research project and turn out a publishable piece at the end (2). This was entirely in addition to working on the thesis project and turning in a thesis (3). In effect, it was a post-doctoral fellowship that was pre-doctoral. It ran concurrently with the doctoral work. That's why it took me four years to go through graduate school at Cornell.

**BOHNING**: Before we look at your graduate work, I'd like to come back to the undergraduate work. Who were some of the professors there? What was the department like when you got to Cornell in 1927?

**ROCHOW**: It was a very diverse and interesting department. You see, Cornell was founded by a businessman who made his money in telegraphy, installing the transatlantic telegraph cable. He said, and so it stands on the seal of Cornell University, "I would found an institution where any person may obtain instruction in any study." Now, that's a very big order. He wanted to found a place where all sorts of subjects were taught. Of course, this was founded in his hometown of Ithaca, New York, way up on the bluff overlooking Cayuga Lake. It's a beautiful location there—cold as anything in the winter, but beautiful. [laughter] Ezra X. Cornell.

It's very important, the wording that's on the seal, because he said, "Where any person may obtain instruction." Now, at that time, there was no such thing as a coeducational college. There were some colleges for women founded further east; but Ezra Cornell and his first president at Cornell, Andrew Dickson White, had it very firmly in their minds that women would be admitted as soon as they could arrange separate living quarters for them. They built Sage College for women. They admitted the first women, and it's been coeducational ever since.

[END OF TAPE, SIDE 1]

**ROCHOW**: There you had asked about people. I said, "It's diverse." Consider Wilder D. Bancroft of physical chemistry, Harvard educated and very much Harvard in his manners and customs, who carried that green bookbag of Harvard Yard with him all his life there. He built a house near by, and was a familiar figure trudging up the hill to Baker Lab carrying his bookbag.

In chemical microscopy—you don't hear anything about that now, but Cornell was an actual seat of learning in chemical microscopy. A Frenchman by the name of Emile Monan

Chamot started the course. Well, we have to go back to Louis Monroe Dennis, because Dennis was the first and only head of the department in Baker Lab, right up to the time he retired. Dennis was the dynamo who established and enlarged and ran the department of chemistry at Cornell from the early days, right until the time I left there. He was trained in inorganic chemistry in Europe and got his doctorate degree in Germany. He believed thoroughly in Karlbaum and Schering reagents, and in German glassware. What was that called?

**BOHNING**: Jena glass.

**ROCHOW**: Jena glass, yes. Of course, it was very fragile, broke easily, and was subject to thermal shock—but it did not put glassware constituents in the preparations you had inside the flask or beaker. That was what convinced Dennis that we should use it all the time. He went to Germany every summer, and bought the supply of reagents and the glassware for the laboratory, and brought it back. The organic chemists hated that glassware, because if you had something set up and put a burner under it, crack, it would go! That was before Pyrex. [laughter]

Dennis pointed out very definitely that Pyrex put boron, silicon, and some alkali reagents in anything you boiled in it. In fact, he had a neat lecture demonstration of this. He took a piece of Pyrex and pounded it to a powder, ground it up in a mortar. He then put the powder in distilled water, overnight or until the next class. He then filtered off the water, evaporated it on a watch glass, and there was a white residue. The proof was there.

There was Jacob Papish in spectroscopy, and such things as gas and fuel analysis. Of course, there was industrial chemistry, with "Dusty" Rhodes in charge. They had a very diverse department. The key people were diverse in their training and personalities and background, but they got along together. Why did they get along so well together? Dennis was an absolute commander and autocrat at the top. His word was law in everything. He handed out all the money from endowments for research and equipment. He designed the building—the George Fisher Baker laboratory. He went out and got the money to build it. He established the Baker Nonresident Lectureships, leading to the famous Baker series of books, right on through Pauling's *Nature of the Chemical Bonds* (4).

Because he was an autocrat, Dennis had the whole thing under his thumb. He ran it just so, the way he was sure it should be run. You can imagine a lot of resentment boiled up under the surface, so that when Dennis retired, oh, they wanted to switch everything around and get rid of all the customs that he had set up—the chemistry museum for example, and all that.

Yes, Cornell had a colorful history, but I won't go any further into that. It was Dennis who noticed me. Why me? Well, I had to apply for a job there. Oh, of course, since Dennis was in charge of everything, we went to him for the job of preparing and setting out the reagents to begin with. He would go through the laboratory and inspect our work with an eagle eye. He also taught a remarkable course in inorganic chemistry; it would be called advanced inorganic chemistry now. He gave a set of demonstration lectures in it. There came a time when he needed a lecture assistant to show the slides, set up the experiments, wash up afterwards, and so on. I applied for it and he chose me. That's how I got in with Dennis.

For his research interest, Dennis turned to fourth-group chemistry and all the rare elements: gallium, germanium, indium, and so on. He bored in where no one else saw any reason to do research there. At that particular time, when I was a graduate student, Dennis was particularly gung ho for germanium. Very few people knew anything about it, but he found a particular source of germanium, which he called germanite. It was a complex sulfide of copper, silver, germanium, and zinc—a mixed sulfide.

He would set his assistants busy extracting the germanium from this complicated mineral. That was quite a task. They would start with a hundred pounds of germanite, powder it, and then cook it up in nitric acid to separate the various metals—a sort of glorified quantitative analysis. Finally, the last step was to precipitate germanium disulfide from a neutral solution. I can picture it right now very clearly—a white sulfide. How many white sulfides do you know? Actually, there's only one other one in the whole periodic table. That's zinc sulfide, used for years as a pigment in paints.

After the lecture assistantship, I was put onto preparing new compounds of germanium. The organometallic compounds struck my fancy then. I thought, "That's a very interesting area." Gee, think of making volatile, distillable, very interesting compounds of these elements that we consider ordinarily just metals—or metalloids, if you will. These rare elements exist, of course, in geochemistry and mineralogy; but the idea of organic derivatives, that seemed fascinating to me.

The first preparation I did for Dennis resulted in a publication: making indium trimethyl (5). I didn't know indium from a hole in the ground when I went to Cornell. It's a very interesting element with an interesting history, which I won't go into here. Through it I learned about organogermanium compounds and, by relation, organosilicon compounds.

Then came an event that was to make a decisive difference in my life and in all the subsequent history of silicon and organosilicon compounds. I told you Dennis had established the George Fisher Baker Nonresident Lectureships. He brought in a visitor for a semester, or preferably a whole year, who stayed there and gave lectures and participated in the department, and guided graduate students if he wanted to. At the end of his term, he had to write a book. Well, Dennis invited Alfred Stock that time, from Karlsruhe in Baden in southwest Germany, to come give lectures. Alfred Stock was a very hardheaded, stubborn, determined German. He didn't know any English. Dennis had said, "That's all right. Come lecture in German. It will be good for the students to listen to it and to learn, and so on." Of course, his audience dwindled down because of the language barrier.

Stock had some great ideas. He had developed vacuum systems for preparing, purifying, isolating, identifying and studying compounds that were sensitive to air and moisture. He used this to make boron hydrides. The whole sequence of boron hydride chemistry begins with Alfred Stock. Remarkable techniques he developed, working with tiny, tiny amounts in the form of their vapors and completely enclosed in a glass vacuum chain. He devised and made, by his own glass blowing, mercury valves. You could transfer something from flask A to flask B but never expose it to stopcock grease, or air or moisture.

In his earlier work, he used vast amounts of mercury. The result was that he got mercury poisoning. The first devices he used were old-fashioned eudiometers, the long graduated tubes like a burette closed at one end. He would stuff this down in a great pool of mercury. Picture a soapstone sink, maybe two-and-a-half feet wide and a foot-and-a-half deep, and of course, containing mercury at least a foot deep. He would plunge his forearms in there up to the elbows and introduce the gas into the eudiometer, and so on. Mercury was absorbed through the skin, and as a result, he got mercury poisoning. That's a horrible malady to have for most of your life, because it sensitizes the poor victim. He can never again come near mercury. He just has to avoid it at all costs.

Stock devised methods for determining the amount of mercury in bread and in milk, especially in beer. Everything that came into his household had to be tested for mercury. He himself avoided anything that might have mercury in it, like a felt hat, because mercuric chloride is used in felting.

Stock came to Cornell. All right. The first thing he wanted was to get everything done the way it was done in Germany. I don't know how many tales about Stock you've come across. I have just two or three. He was put up at the new student union at Cornell, Willard Straight Hall. He was in a tiny room up on the third floor. He was disturbed because the students working at the desk in the lobby didn't understand German. He had to get somebody to come and interpret, tell them what to do. Okay, came the first Sunday morning he was there, he marched down to the lobby and demanded his mail. They tried to explain to him, "They don't deliver any mail on Sundays." Well, in Germany, he got mail on Sunday. He <u>wanted</u> <u>his</u> <u>mail</u> <u>delivered</u>, because it's important, from Germany.

A very resourceful Cornell Hotel School student was working at the lobby. He took the mail that came in the next Saturday, divided it in half, and gave Stock half of it on Saturday and half on Sunday. Stock was perfectly satisfied with that. [laughter]

Well, when Stock went downtown with Dennis to start a bank account, he stormed into the bank and went past all the tellers' windows up to the manager at his desk in the back, and said, "Wer spricht Deutsch hier?" Of course, they didn't know what he was talking about. So he went from one teller to another, "Wer spricht Deutsch?" Nobody, of course, spoke it. Dennis watched all this and then he said, "You won't find anybody who speaks German." Then Stock tried his French, a very Germanized French. They didn't know what he was talking about. Americans are so language deprived that they just didn't know what was going on.

So it was that Stock gave his lectures in German and wrote his book in German. Later, it was translated (6).

Stock needed a lecture assistant. Dennis said, "Well, Rochow was my lecture assistant. You can have him." I suddenly became Stock's lecture assistant. [laughter] It was a very interesting job, because he had all of these slides—of course in German words, but the writing on them was clear enough.

For his lecture experiments, Stock had shipped over or brought with him a Zeiss epidiascope. I imagine you've never seen one—a device that used a powerful carbon arc as a light source and reflected light from books or whatever you had down inside. Mostly, Stock was an expert glassblower and made tiny glass vessels for experiments he wanted to show. These fitted inside the black-curtained enclosure of the epidiascope and put an enlarged picture on a huge screen. He could show anything to anybody in the large lecture room this way. It was a great idea. The optics, I thought, were marvelous—front surface mirrors and lenses this big. [shows six-inch lenses]

I learned to use the epidiascope. Cornell actually bought one, but nobody else would use it. Only two were sold in this country, one at Cornell and one at Harvard. When I got to Harvard many, many years later, that epidiascope was still in the basement. [laughter] Arthur B. Lamb, who was longtime editor of the *JACS*, had bought it. He was the only one who would use it. Of course, when he became dean of the arts faculty, it went in the basement. For all I know, maybe it's still there.

I'm getting far afield. What do you want to know?

**BOHNING**: No, I was glad to hear the Stock stories. I did want to talk a little more about Cornell. One thing that I don't have is a list of your papers. I have a list of the books you published (7), but we never did receive a copy of a list of all your publications. Do you happen to have an extra set?

**ROCHOW**: I have a list.

**BOHNING**: Oh, you have a bound volume. [laughter] All right.

**ROCHOW**: Maybe I never prepared a list. It's always changing, anyway. I have only one bound volume as far as it goes. By the way, here's a picture of Dennis at the time he retired.

**BOHNING**: I'm just going to make a note of this, but I'll have to get a bibliography. Here's one from the *Journal of Chemical Education* (8).

**ROCHOW**: A list of publications, a bibliography—I don't have any, I believe. Here's a list of books.

**BOHNING**: That I have.

**ROCHOW**: The list of patents I can give you (9).

**BOHNING**: Oh, that would be interesting, also.

**ROCHOW**: Publications, I don't know. There are one hundred and sixty or one hundred and seventy publications. The books you have, you say. Suffice it to say for the present, this begins with a couple of papers I published as an undergraduate at Cornell. This impressed some Harvard people to no end, you know, as the subject of my research work was on fluorine—especially the oxides of fluorine. I looked for oxyacids of fluorine, and I found indications of hyperfluorous acid. Nobody would believe them, but they're thoroughly established now. Here's "The Preparation of Fluorine by Electrolysis" (10). That was published in 1931—early 1931—before I graduated. "The Oxyacids of Fluorine" (11)—let's see, the date on this is 1932, based on my work as a senior. There's "Oxyacids of Fluorine. II," which came the following year (12). Here's "Fluorine Preparation Cell," and so on.

**BOHNING**: You had a number of papers as an undergraduate, then.

**ROCHOW**: Yes. See, I regard this as a finished list because I published a paper this last year and continue to put them out. I can give you the beginning and the end, but in between, a computer could run this off for you if you wanted to look these up in *Chemical Abstracts*.

**BOHNING**: Yes, we can put more of a list together. I'll do that.

**ROCHOW**: I have a lot of loose reprints, too, besides this bound volume, which only goes up to the time I retired from Harvard, and beyond that, loose things all over the place. Well, you're interested in the patents.

**BOHNING**: Yes, I am, if you have a list. We'll touch on that when we discuss your time at General Electric.

[END OF TAPE, SIDE 2]

**ROCHOW**: Well, it's not complete. You know, there are some thirty-eight U.S. patents, in addition to a great host of foreign patents, of course. As far as that list goes, I can take it across the hall and get it duplicated if you want.

**BOHNING**: All right, that would be fine. [short break] Midway through your undergraduate career at Cornell, the Depression came in 1929.

**ROCHOW**: Oh, the Depression years, of course.

**BOHNING**: I wondered what effect that had on you, halfway through your undergraduate career.

**ROCHOW**: It influenced my life indelibly right up to this day. Nobody who has not lived through the Depression knows really what it was like, and can't even imagine. Secondly, anyone who has been through the Depression bears its indelible stamp. My father had some stocks, utilities and conservative stocks—lost everything in the crash. To this day I don't own any stocks. I have absolutely no faith at all in stocks—bonds, maybe, if they're backed by the U.S. government, but they succeed in making those worth less and less every year. [laughter] Well, of course, a very stable currency, but few people give thought to that. When the government not only devalued it and devalued it, but then debased the currency here so that the dollar is worth absolutely nothing but the piece of paper it's printed on, I thought, "This is a horrible event." I couldn't understand why no one else was disturbed by it, even. Yet look at the consequences: here's the U.S. federal deficit in 1950. Add another piece of paper up to 1990.

**BOHNING**: My goodness.

**ROCHOW**: Now add another piece of paper. Now add another piece of paper, and you're way up there. You see, it's becoming well-nigh vertical. This sort of thing disturbs me greatly. It doesn't worry a single other person here in Myerlee Manor. This is financed by borrowing money. No one here and no one in the Democratic Party thinks we have to change anything—we'll just keep on borrowing, that's all. You just do that. Now, of course, the interest amounts to, what, a quarter of the federal budget? That is not an entitlement. This will go up and up. Finally, they will stop lending us money. The whole thing has got to crash someday.

Yes, the Depression made an indelible impression. Sure, I've been very conservative, in my own affairs and everything else.

**BOHNING**: Your brother had graduated, essentially, at the time the Depression started. He was two years ahead of you. He would have graduated in 1929.

**ROCHOW**: He graduated in 1929. Sure enough, he went to work in, not my father's plant, but a chemical company in Newark that made dyes and leather finishes and so on. He worked there for a year, and then he said, "I'm not ready for this." The pay was poor and the hours were tremendous. He decided to go back to graduate school. He was only one year ahead of me in graduate school. Then he left without completing his thesis—a terrible thing to do. In the end, he got his doctorate after I did.

**BOHNING**: Where did he spend his career after Cornell?

**ROCHOW**: American Cyanamid Company in Stamford, Connecticut.

**BOHNING**: Oh yes, yes.

**ROCHOW**: They brought him in as a microscopist, because at that time they made—and they probably still make—ore separation chemicals, flotation chemicals. He would take powdered pieces of rock and identify the ores—the profitable ores—in it. He would determine, by optical means, the percentage and so on. Then he would study the wetting of these by various chemicals, and so on. Well, he went on to become an expert in microscopy. He got his doctorate degree with Clyde Mason, who got his under Chamot at Cornell.

Well, he went on to extend microscopy to identification of non-mineral things—and principally, polymers and plastics. It was amazing what he could do with optical characteristics—not only identify the composition of a plastic sample that you gave him, but tell you who made it and approximately when. Other people say, "Well, what can you measure in a polymer?" You measure the index of refraction, and that's all. Well, Ted had proved that there are seven reproducible, identifying properties of plastics of any polymer.

When he retired from American Cyanamid, he thought, "Boy, it would be nice to be a professor too." He got a job at North Carolina State University in Raleigh. He's still there. When he gave a course, he found he had to educate biology students on the optics of the microscope first and then on how to use it, because they didn't know how to make the most of the high-priced, high-powered microscopes that they had. Well, from there he went to metallurgy and all of that.

Out of those courses of instruction at North Carolina State, he wrote a book (13). Like his thesis, it remained unfinished, year to year. [laughter] He's different. I'm stubborn and insist on finishing what I started; but his wife Betty, who is Helen's cousin and childhood playmate, said, "Every morning he goes to that big desk to continue writing the book. What does he do? He goes back to the beginning of chapter one. He begins to rewrite chapter one instead of going on to chapters six, seven and eight." [laughter] He had Betty type each version, and this went on for twenty or thirty versions. Finally she said, "No more of this. I just won't type until it's finished." Well, I had put the fire under Ted to publish his thesis. He came to me and said, "Will you help me to finish this book and get it out of here?" I'd already published a couple of books. I knew the mechanisms and timetables and so on, so I helped him do it.

Now he has revised it. I refused to go along and revise the whole thing, and write the index and proofread it all again. I have my own interests. He didn't like that one bit. I got him to prevail on a young colleague there in Raleigh. Ted taught him not only microscopy, but also how to teach microscopy, so together, they worked out a second edition (14). That was a long time a-borning too, but it came out just two months ago.

Even the publisher didn't like the fact that it didn't have my name on it. The editor-in-chief said, "This is unheard of, to publish a second edition without your name on it as well as your brother's." Well, then it turned out that she put my name on the first proof anyway. [laughter] I wrote her a letter, "This is none of my work. It's all done by my brother and by Paul Tucker. I don't deserve any credit for it. If you put my name on the printed volume, I shall sue you for unauthorized use of my name for profit." Boy, that did it. [laughter] I had to force Ted to accept full credit for his hard work.

I never put together a proper list of publications and patents, because the sequence was never finished. Maybe someday.

**BOHNING**: I have some indications that you worked for an entity called the Halowax Corporation. Was that while you were still doing graduate work at Cornell, or was that between your undergraduate and graduate degrees?

**ROCHOW**: That was while I doing graduate work at Cornell. That was a summer job, Depression years. The Depression continued on and on. In order to get some money, of course, I was lecture assistant and so on. I had that just for the academic years. During the summer I got a job, first with the DuPont branch in Newark, New Jersey. They made pigments—zinc sulfide and lithophone. I learned all about making pigments—all I could learn there. Then, because I had experience in fluorine chemistry, I got a job with Halowax Corporation. They made chlorinated naphthalenes as dielectrics for capacitors. There was no such thing as an electrolytic capacitor then. Capacitors were made from paper and foil. They were wound up, impregnated, dried out—a lengthy procedure. They were impregnated with something—there's no point in just putting in paraffin, when you can put in something with a dielectric constant two or three times as high. The halogenateds were used for that.

What about Halowax? Well, that was absorbed by Union Carbide. They stopped making Halowax because they saw some of the dangers of halogenated aromatic compounds, especially the halogenated, chlorinated, polynuclear aromatic compounds. The fellows who worked with me in the lab the whole year, exposed to Halowax, developed tremendous boils—hickeys, they called them—all over their necks and their faces, wherever they were exposed to the vapor. Well they, the Halowax people, wanted me to work there after I got my degree. They didn't like it one bit when I didn't go there. I didn't like the effects of Halowax. I thought, "This is a bad area to be in."

The chlorinated biphenyls that Dow Chemical Company made were just as bad—causing trouble with skin and bad if taken internally, even back then and sure enough, later on. Look at the trouble with the PCBs—chlorinated biphenyls in transformers. It was an awful business. I think the whole chlorinated hydrocarbon business, including Freons, is doomed, but there's still money in it. Did you notice yesterday—no, you wouldn't, you weren't here. At the airport here, a shipment came in. It was seized by federal authorities because it turned out to be a shipment of a hundred or so tons of Freon-12 made abroad—see, contraband coming in.

All right. I was looking for a job after graduate school.

**BOHNING**: Had you thought about a teaching career as opposed to an industrial career, or was it because of the Depression that you'd wanted to get a job?

**ROCHOW**: That is to laugh, because I had been lab assistant and lecture assistant at Cornell. At least I had that title on my record. While I was working for Dennis there as a graduate student, the man who worked in the next laboratory bay there had a degree, a Ph.D., from Harvard. He was on a postdoctoral fellowship. He was separating, or trying to separate, the isotopes of boron by vacuum system distillation of boron esters or boron alkyls—a project of Dennis'. Dennis thought, "If you distill enough, you should be able to separate these things." It went on for four years. Well, that fellow had a wife and a child. He was looking for an academic job for four years. He had, as credentials, a degree from Harvard and a post-doctoral fellowship at Cornell. He got a job selling Easy washing machines on straight commission.

During my last year there at Harvard, nobody came to interview the graduate students—none at all. There was one letter offering a teaching position. That was for teaching high school chemistry at a city in Pennsylvania, for a salary of eight hundred dollars a year. That was it—no other teaching jobs. When you speak about the Depression, these things made a searing impression on my mind. Everybody who left there at that time—so many people wanted to stay in graduate school. There was competition for the graduate fellowships. The pay for those had reduced and reduced, and gotten down to six hundred dollars a year for that. Fellows had to pay for their board and room and clothes and transportation out of this. Many of them were just living on boiled potatoes and rooming together in an apartment, five or six of them. Yes, those were bad years.

How in the world did I ever get a job at General Electric? You're going to ask that. Makes me wonder to this day. It's just that a man named Winton Irving Patnode, who was graduated from Cornell some years back in the 1920s, got a job at the GE research lab in Schenectady. Well, it wasn't the fluorine chemistry part. The Hotpoint Company, a GE subsidiary in Chicago, I guess it was, made electric ranges. It was a most astonishing use of an inorganic substance, fused magnesium oxide, as insulation.

Well, picture the Calrod heating unit for electric ranges. You have a metal sheath about so big around. Inside of it, more or less concentrically, there is a spiral of hichrome wire—the heating element. The sheath is grounded, so the housewife won't get a shock. The helix runs at two hundred twenty volts. Now take that up to red heat—let's say eight hundred, nine hundred degrees C. Here we have a space of one millimeter or less between the helix and the sheath, with a potential difference of two hundred twenty volts.

At nine hundred degrees, tell me what you would use for an insulator. You ask most people about this and they say, "Well, I'd use quartz." Or can you use corundum—aluminum oxide. In the first place, if you know how these things are made, by swaging—you know the machine shop procedure, swaging—you have a couple of steel dies, wack wack wack as they go round. It squeezes down the metallic sheath, compressing what's inside. You do this with quartz, and it punctures right through the metal and comes out. You do it with corundum, it's so hard it won't break up.

The answer is periclase—crystalline magnesium oxide—that has the sodium chloride structure and cubic cleavage. It will break up. If you put it in as a granular stuff, it will cleave and compact and squeeze out all the air, and get down to something like flour in there, and conduct heat like mad. Yet the electrical insulation is way up there. Now, name me one other substance that has a very high heat conductivity and <u>extremely</u> low electrical conductivity. You'll be stumped there, too. It's periclase or nothing.

The Hotpoint Company was cut off from the foreign supply of magnesite to make fused magnesium oxide, to get their periclase. The fusing is done in an arc furnace. They had to use a domestic source, because there's a fellow named Adolf Hitler who wanted all European resources controlled by Germany and all satellite countries that way. He didn't want to export any magnesite. Furthermore, the European magnesite was variable in composition. Hotpoint Company said, "We need a clever inorganic chemist who can analyze these samples of magnesite as they come in and can tell us which ones make the best periclase—fused crystalline magnesium oxide."

Well, the head of the chemistry division of the GE research lab in Schenectady was a physical chemist who didn't know beans about inorganic chemistry. He didn't know where to turn. The Hotpoint Company said, "We'll support a research project if Louis Navias—who was a mineralogist, and head of the ceramics division, and a microscopist—will oversee this. We'll provide enough money, at least, so that he can have an assistant and a livable salary—namely, three hundred dollars a month for the summer. He should get a student who will come there for the summer." Abraham Lincoln Marshall, head of the chemical division, didn't know anyone; William D. Coolidge, the director of the lab, didn't know anyone.

They turned to Patnode, and he said, "Oh, I know just the one at Cornell. He's finishing up, and he could do this thing for you." Patnode wrote to me. I thought, "Well, it's an interesting thing, but I don't know beans about periclase. Operating an arc furnace? I never did that." Besides, I had a fellowship with Professor Alfred Stock in Germany. This was the *Austauschdienst*, an exchange fellowship.

Say, am I taking too long on this?

**BOHNING**: No. I'm just checking the tape, that's all.

**ROCHOW**: Well, the *Austauschdienst* worked this way: a few American recent graduates, American young men, went to Germany, a certain number—four or five each year. Germany sent a corresponding number of German students over here to selected universities. This was the exchange fellowship, which had gone on for some years. Dennis said, "You should apply for that. You don't have a job. It would be good experience for you, working a year or two in Germany." I applied, since I had been Stock's assistant and had drawn the diagrams for that book of Stock's in the Baker series (6).

I wrote to Professor Stock. He said, "Well, I don't have anything. There's nothing possible here." He put me in touch with Professor Goldschmidt, a geochemist who was in Berlin. I applied, and boy oh boy, I was awarded this exchange fellowship. I thought, "Wonderful. I can be in Germany for a year."

[END OF TAPE, SIDE 3]

**ROCHOW**: It seemed so promising that on the strength of it, I got married. I'd been going with a young woman from Cornell, who actually had been Dennis's private secretary. She had typed my thesis at home. See how these things get started? I went to her and said, "Look here. We can go to Germany and live there for a year. It'll be a great experience. Then we'll come back and find something. Surely the Depression will be over by then." [laughter]

I had my passport. Then just five weeks before I was to sail, I got a telegram saying, "The *Austauschdienst* program has collapsed. Hitler refuses to let any young man, especially one with a technical training, leave Germany under any conditions. Therefore, they can't send anybody over. We can't send any Americans there."

There I was. I had a passport, I had a steamship ticket, and I had no fellowship. Well, I said to the young lady, who unfortunately has died long since, "See here, I've saved up a sum of money"—that amounted to two hundred and eighty dollars, or something—"it'll be enough to get us across to Germany. Maybe I can borrow enough to get us through for at least a tour of Europe and come back. We can travel third class." That's not tourist. That's almost steerage on some of the lines.

Well, the young lady was Priscilla George Ferguson. She contributed some money of her own. We were married in Sage Chapel at Cornell, and the two of us went to Europe and stayed there for a month and a half. We toured all over Germany, France, a piece of Italy, and into England to visit Nevil Vincent Sidgwick and others at Oxford, whom I knew, and came back—all on three hundred dollars. Now, when you speak of the Depression and, well, speak of inflation and so on, just take a look. People complain about the increase in postal rates, but my goodness, look at the minimum wage. Jim Lingane calculated it out from his four years of work right out of graduate school, and it came to 34 cents an hour. Now, the minimum wage is $3.50 or $3.85.

**BOHNING**: It might even be higher.

**ROCHOW**: Well, they want to make it higher. Maybe it <u>is</u> higher—four dollars, or four-fifty-something. Anyway, it's more than a hundred times what it was then. People said, "Inflation? We've got that under control." Malarkey.

Well, all right. I got the job at GE, thanks to Patnode. I went there.

**BOHNING**: How did Patnode know about you?

**ROCHOW**: Well, he was at Cornell. He got his degree with Louis Monroe Dennis, just as I did.

**BOHNING**: All right. It was all part of the Dennis fraternity, as it were.

**ROCHOW**: Yes. Yes, that's it. As a matter of fact, it was academic—father and grandfather, and so on.

**BOHNING**: You had already told Patnode you were going to go to Germany. Did you come back and say to him, "I'm not going. Do you still have something?"

**ROCHOW**: Five days before I was to sail, I immediately rushed to Patnode and said, "This has happened. Is the Hotpoint job still open?" "Well, yes," he said, "we haven't found anybody else. I think you could do it." Wham, I got the job there. Yes. It was not three thousand dollars. It was twenty-seven hundred dollars—two hundred and seventy dollars a month.

I moved to Schenectady, and learned about the GE research lab and the arc furnace and fusing magnesium oxide. It's a great thing to run an arc furnace, even an experimental one. Navias had set up one in the laboratory, because he wanted to make fused beryllium oxide and fused aluminum, and so on, for his insulation studies.

Well, they had a big steel cylinder, a meter across. You fill this full of calcined magnesite. First, at the bottom, put a short piece of graphite rod between the electrodes. Then fill it all with magnesium oxide, and then turn on the current. It'll burn through the short rod there and start an electric arc. Then keep piling in magnesium and keep it going with, I don't know, maybe sixty KVA. Gee, it gets awful hot in there. Magnesium oxide—periclase—melts at twenty-eight hundred degrees, twenty-eight hundred and five. That's way above the melting point of quartz or of corundum, or anything like that.

It's very interesting. You gradually raise the electrodes, or they gradually burn away. You build up an irregular ingot, building up from a puddle of fused magnesium oxide. Then you shut off the current and let it all cool in there. It'll take maybe two days to cool, but finally you dig it out. You have an irregular ingot, and you whack it with a sledgehammer and break it up. Inside are crystals of magnesium oxide. If it's real pure, you get magnificent crystals so long. [demonstrates]

I said, "Aha. Now I'll settle this thing. What's the electrical conductivity of magnesium oxide at nine hundred or a thousand degrees C?" Nobody knew. All right, I got some crystals of quartz that size. I got some pure magnesium metal and hydrated it in an autoclave with steam converted to the hydroxide. I converted that to oxide, fused the pure oxide, and made crystals of pure magnesium oxide so long. I had crystals of corundum, too. It's in here. "Electrical Conductivity of Quartz Periclase and Corundum at Temperatures up to a Thousand Degrees" (15). Oh, boy. Periclase is the one. There's no doubt about it. There's nothing remotely like it.

We did that as a side issue. It was just for the scientific value. We told the Hotpoint people, "Look, you've got to have the fundamental facts." They said, "We can't use anything but periclase anyway." "Well, I know that." Then he said, "What's the effect of impurities? Can you get pure magnesium oxide?" I said, "I'll find out. I'll add one-half, one-tenth percent, one percent, two percent of silica, of alumina, of calcium oxide—anything you want. We'll have to do this successively. We fuse it, go through the whole rigmarole." They pulverized to the periclase with the impurities in it and made experimental Calrod units, and tested them—electrical conductivity, life, and so on.

It was very illuminating. Well, they thought, "Lime wouldn't be bad at all." Oh, but lime combines with the oxide of the Inconel sheath, and pretty soon the sheath fails. Of course, you can't have any alkalies there because of the conductivity. Alumina was not good, but silica wasn't bad. The interesting thing I found out was that if you had a certain amount of lime—because you're using a natural product, magnesite—you could then add silica to it to counteract the alkalinity of the lime, and so make a perfectly useful product. Oh, the Hotpoint people thought this was great. They said, "Gee, you're coming along so well on this, suppose we continue the fellowship there. You stay with us." Sure, stay with it—would I stay with it? It went on extended for a year. Then it went on from there, and all sorts of things happened.

**BOHNING**: You went there in 1938, then.

**ROCHOW**: In 1935.

**BOHNING**: In 1935, okay. Were you reporting to Patnode during this time, or were you reporting to someone else? Were you dealing with Hotpoint directly?

**ROCHOW**: No, I reported to Louis Navias—the microscopist, the mineralogist, the ceramic chemist. He came there from the U. S. Tariff Commission. They had him analyze ores and things that came in. He was an expert microscopist. He was astonished to find that I knew one end of a microscope from another. I had taken the course—we all had to take the course—in elementary chemical microscopy at Cornell. I knew how to identify isotropic from anisotropic things, and index of refraction and extinction angles, and all of that. Navias was amazed at this. He'd never found anybody in graduate school who could do this. I was kept on as assistant to Navias for quite a while.

You're going to ask how I got into silicones?

**BOHNING**: Well, I know that in 1938 there was a visit to Corning. However, you weren't included.

**ROCHOW**: See, I worked on the sixth floor with Navias. All the chemists—including the hotshot guys in physical chemistry, like Marshall—were on the fifth floor, and we didn't mix much with them. I was busy, had my own work. They had their work in all sorts of polymers.

Well, the chief things were wire enamels. They wanted something that could coat on a piece of wire and keep the wires apart during the winding of motors and transformers, without having to wind cotton fibers on the wire first. They got very far with this. A polybasic acid or a dibasic acid was condensed with a polyhydric alcohol to make a polymer. The trade name was Glyptal. They made wire enamels that really did the job. Gee, it looked like bare copper wire. They could put it all through the winding machine in intricate shapes, and then ram it in the spaces in between laminated core that punched out spaces, and not short circuit the thing. This was amazing.

Well, the organic and physical chemists had their place. I was with Navias. Navias was my mentor, and I was getting along working as his assistant. Another thing came up too, which surprised him and William David Coolidge. That was somebody by the name of Otto Hahn, who had been Baker lecturer at Cornell—very genial, such a nice fellow. He mixed with all the students. Everybody got along with Otto Hahn.

**BOHNING**: Hahn was there during the time you were a student there.

**ROCHOW**: Yes. It came out that Hahn had working with him a young colleague and assistant named Lise Meitner. She had discovered something. They were trying to extend the periodic table. They bombarded heavy metals. They chose uranium, and they tried thorium and all sorts of things—bombarded them with neutrons to make heavy isotopes, which indicated new elements. Hahn had done this with uranium. The reaction product would go through a chemical separation, and they would follow the products by the energy of the rays that come off.

They discovered something very strange. They had to put in a carrier to take along the radioactive substance because it wasn't enough to precipitate by itself, and absorbed it on the carrier, which is chemically similar. They found the product was going along with the bromine fraction. There was no explanation for that. There was no heavy analogue of bromine that would do this. Nevertheless, there it was. It appeared as though the uranium atom split into two unequal halves. Now, Hahn couldn't believe it. Meitner was certain of her results. She did it over and over and over.

Finally, Hahn wrote a paper about it and sent it in as a letter to the editor (16). Then he told me later, he put it in the mailbox and immediately wished he had it back. He said, "I'll be a laughingstock." In that paper, he said he had no explanation for the phenomenon. It appeared as though a bromine-like isotope or element was being formed, and had the radioactive properties of such an isotope. Lise Meitner's nephew, Karl Frisch, said, "Why, the uranium atom must be splitting."

Of course, then it was taken up by others who said, "Why, this should be a tremendous source of energy." You mention energy to the General Electric Company, and you get a reaction. They immediately wanted to know whether this was a possible commercial source of energy to use in their heat engine generating system, the central station. That's their big business, central station equipment—the huge turbines that they made, the huge generators and all the switching equipment and all that. They still do.

They wanted somebody who knew something about fluorine to make some uranium hexafluoride. They wanted a volatile compound of uranium. Well, gee, you know, any inorganic chemist will tell you right away, uranium hexafluoride is it. Sulfur hexafluoride, carbon tetrafluoride, all these things—to a fluorine chemist, these are so clear. "Could you make us some uranium hexafluoride?" Well, I could, but I'd have to start by building a fluorine cell and making the fluorine. "Well, go ahead and do it. Take the time. We'll supply the money."

I built a fluorine cell and got some uranium, and built the vacuum system and made uranium hexafluoride, and turned it over to Kennith Kingsley, head of the physicists' group. I did this, all sealed up in a nice glass ampule, and drew it off with a torch. I said, "Look, these beautiful, very refractive, highly refringent crystals of uranium hexafluoride—there's hexafluoride vapor above it, of course." They took this, they opened that up to the air—spoiled the whole thing. "Well, I'll have to do it all over again," and so on.

I was the fluorine chemist. I was with Navias, and I had lots of inorganic tasks. No, I did not go on that visit to Corning. I didn't know a damn thing about the visit to Corning. But Patnode came back and I was having dinner with him over at his house one night. He said, "Corning has made ethyl phenyl silicone."

He explained about Frederick S. Kipping's work, and so on. The Corning Glass Works vice president, named Dwight Morrow, had gone to General Electric with the story first, and then had invited Marshall and Patnode to go see this. He wanted to sell glass fibers and foresaw a big business there. He wanted it used in electrical insulation and machinery. You can't use it there unless you have some binder or something to fill up the interstices and give dielectric strength. Corning had brought in James Franklin Hyde from Harvard, an organic chemist. He knew about Kipping's work. I think he even had a copy of that famous textbook (17). William H. Perkin and Kipping.

**BOHNING**: Perkin and Kipping, yes.

**ROCHOW**: Kipping and Perkin, Perkin and Kipping. All right, he had made phenyl silicone—the easiest one. Of course, it's resinous; it melts easily. Well, it turned out he put one phenyl group and one ethyl group on silicon to make the dichloride. When you hydrolyze this, you make a siloxane chain with ethyl and phenyl groups. Hyde made his silicone varnish from this. That was what was explained to the GE people on the visit to Schenectady. Marshall and Patnode went to Corning and Hyde showed them his samples—what he'd done and how he made it and so on—and urged GE to take up this work, because Corning wanted to sell the glass fibers and they had other things to do.

**BOHNING**: It's my understanding that Corning initially was hoping for a joint venture with GE. I think this is based on something Earl L. Warrick said in his book (18). However, the stories don't always come together.

**ROCHOW**: Well, as William David Coolidge said to me, "Look, Corning Glass Works is virtually a branch—a department—of General Electric." They'd worked so closely, developing machinery for the automatic manufacturer of lamps. That was GE's business at the beginning, to make the lamps as well as the equipment for generating the power. Lighting was their thing. The GE Lamp Works at Cleveland worked with Corning people all the time. Coolidge encouraged Marshall to engage in some aspect of this. He said, "We get along so well with Corning; we've shared patents, and so on. Let's try to do something."

[END OF TAPE, SIDE 4]

**ROCHOW**: Patnode didn't have time. He was busy on the polyester wire enamel and a number of other projects. Patnode told me about this informally, at dinner out there at his farmhouse one night. I thought, "Well, that's interesting. It involves organosilicon compounds." I'd worked with organogermanium and organosilicon things. Then it struck me: Patnode had once written to Dennis, asking for advice about organic silicon compounds. Oh, he wanted to make a polyester using silicon to combine with glycerine or ethylene glycol to make an ester. He wanted something more thermally stable than the organic derivative. Being an inorganic chemist himself, of course he thought silicon—silicon oxide.

He wrote to Dennis. Dennis showed me the letter. I said, "Well, no matter what he puts on silicon, if it's connected through oxygen to carbon, that's an ester linkage. It will hydrolyze, just the way ethyl silicate does. There's nothing you can do about it. The hydrolysis will be catalyzed by alkalies, or it will be catalyzed at a different rate and different mechanisms with acids." I didn't see any hope with that. Patnode gave it up.

But, "Aha. If you fasten organic groups with a direct carbon-silicon bond, you've got a horse of a different color then." Patnode said, "Well, why don't you take this up? First, we'd like to have some ethyl phenyl silicone. Could you make it for us?" I said, "Well, sure I can make it. Directions must be there in Kipping." I went to Navias. He said, "Oh well, those guys in the organic group are always asking for stuff, but go ahead and make him a batch."

I made it and gave it to Patnode. I thought that was the end of it. Of course, I began to reflect on this. "Look, even if they do get together with Corning Glass Works—you say joint venture. All right, a joint venture—still, we're only making what Corning has made already. They will patent it, and so on. What's in it for General Electric?" There should be some originality, I thought, in this. Patnode didn't agree with me, but Louie Navias said, "Well, if you've got any ideas, write them down in your notebook. Give them to me and I'll witness them."

I thought about it. "Ethyl groups—what happens when you heat ethyl phenyl silicone up to thermal decomposition? Well, it carbonizes. You have carbon-carbon bonds in there. You pyrolize the stuff. You have black carbonaceous residue, which is conducting. How can we avoid that? Well, by avoiding all the carbon-carbon bonds. What, make an organosilicon derivative without any carbon-carbon bonds? Sure, you can do it. Use methyl."

Kipping never made methyl silicone. Nobody else had made it at that time, either. I said, in my stubborn way, "I'm going to make some methyl silicone." Well, you look in the textbooks and you find you can't make methyl magnesium chloride readily; methyl chloride doesn't react much with the magnesium. All right, you can make it, but in low concentration. It has a low solubility. Of course, I labored through the whole thing. I told Navias, "I'd like to work on this thing, but it will take time, and I don't have any support from the organic people or anyone else." We had to fill out time sheets—work orders, they were called. If there was no support for this, you couldn't work on it. "Well, he said, "slip it in with the Hotpoint work, and I don't think anyone will know the difference. I'll see to it that it gets by in the front office." [laughter]

I did that. Of course, I made methyl silicone eventually by the classical method. Hey, it turned out to be marvelous stuff as an insulator—marvelous dielectric stuff. I remember one piece I kept in my desk for years and years—a cast disk of methyl silicone. I had put it in an air oven at two hundred degrees, left it there for a year. At the end of that time it was not decomposed—not one bit. It had just turned more brittle.

I made methyl silicone in solution. I applied this to glass fiber on copper wire, and wound the wire into coils. I demonstrated it to the engineers, first to the physicists in the GE research laboratory, then at the weekly colloquium—there's a gathering of three hundred people in the auditorium. I wound two identical coils—copper wire, "served," as they called it—with glass fiber. They were big heavy coils! One of them I impregnated with the prevailing best phenolic varnish, which was used at that time for insulation. The other coil I impregnated with a solution of methyl silicone. I evaporated the solvent and baked the coils in an oven at appropriate temperature. I connected the two coils in series to a big Variac and increased the current gradually. I had a big ammeter in series.

**BOHNING**: Was this like a rheostat?

**ROCHOW**: Well, it doesn't use resistance. A Variac is an autotransformer. I increased the current. The meter went up and up and up. I kept on increasing the current. It caused losses in the copper, and the coils got hotter and hotter. By and by, the one with the phenolic varnish began to smoke. The one with silicone did not. I kept on heating, and the phenolic resin smoked more and more. The silicone did not. Same current, same time—everything the same. Finally, the one with phenolic varnish burst into flame—black soot coming out. [laughter] The silicone one just stood there; nothing happened.

I said, "Now, we'll just increase it to failure—to see what happens." Well, eventually the wire got red hot, and the silicone oxidized. What's left between the windings? White silica, nonconducting. I thought this absolutely would convince everybody. Of course, the engineers only said, “Great. What does it cost? How much can we get for a test?” [laughter]

It was a long struggle, but I knew that methyl silicone had a future. Maybe I was the only one in the whole company who knew it and believed in it, and made sure it had a future. They raised all these objections, including the people in the chemical division downstairs. They said, “Well, is it any better than what Corning has?” “Sure, it’s better. Look, I’ll show you.” I was winding coils with ethyl phenyl silicone and with methyl and showing that it was better. “Besides, it’s original. Nobody’s made it before. We can get a composition of matter patent on it” (19). We did, by golly. They said, “How very interesting.” [laughter] It got nowhere.

**BOHNING**: This was the first thing you did that was mostly on your own, rather than under direction of somebody at GE.

**ROCHOW**: Completely on my own.

**BOHNING**: All right. That’s an interesting point. It seems that the Corning people, from what they’ve written, may have thought that when GE came back from that visit, they saw a good thing and decided to get in it on their own, right away. But really, you’re saying that Patnode told <u>you</u> this story at dinner.

**ROCHOW**: Informally.

**BOHNING**: When was it that you made the methyl silicone, then? Would that have been around 1938?

**ROCHOW**: In 1938.

**BOHNING**: All right. Just after that visit, then, or shortly after that?

**ROCHOW**: Well, once I had thought it out, I made methyl silicone. Yes, it dates from 1938. Of course, when Navias heard about this and saw the samples that I had produced and heard what had happened, that I’d demonstrated in the auditorium, he said, “We’ll have to file a patent application right away. Is your notebook in order?” “Well, yes, it’s all in order there, witnessed and everything.” I filed a patent application (19).

According to the custom at GE, once a patent application is filed, you may then write up a paper for publication. Of course, being publication-minded from my undergraduate days, I wrote up a paper (20). I thought this was the natural and proper thing to do. Well, not so. You see, an entirely different attitude existed at Corning Glass Works. In the first place, glass compositions are not patented. There's a matter of shop secrets. I don't even know if they can be patented anyway, because you put together well-known raw materials according to a well-known procedure, and you make glass, which has been made for thousands of years. There's nothing patentable there, so everything was done by secret in the glass industry.

We have to go back to Dwight Morrow, the vice president at Corning Glass Works in charge of glass fibers. He's the one who went to Schenectady and showed Marshall and Patnode what Hyde had made. Yes, I've no doubt he was all for it—not a joint venture as I understood it, as explained to me by Patnode. They wanted to turn over the whole thing of insulating varnishes to General Electric and Westinghouse and Allis-Chalmers—anybody who made big electrical machinery—because he said, "Corning Glass Works is not in the organic business and not interested primarily in insulation, but in selling glass fibers."

The quickest way to get Fiberglas on the market is to give the information on what Hyde had done to GE, Westinghouse, Allis-Chalmers. Get them interested in going after it. Let them develop the binders and the varnishes, because they had known about GE's work on wire enamel and varnish there, which has taken over the whole industry and has gotten rid of all the cotton and rags and pitch that Edison used. [laughter] Modern insulation, phenolic varnish—these are great developments.

All right, at GE the custom is to apply for a patent. Back up this, get the application in, and get the first claims accepted. Then you may write a paper. In fact, you're encouraged to write a paper and explain this. I wrote a paper. I gave it to Navias and he said, "Well, that's fine. Get it approved and send it in."

Well, it had to go to Marshall, the head of the organic people. He got it and he took it to Coolidge and said, "See here. The Corning people have not published. I don't even know whether they have applied for patents. Is it proper for us to do this, considering our long-time good relations with Corning Glass Works?" Coolidge said, "No, that wouldn't be right." He said, "I'll call up Eugene Sullivan at Corning, a good friend of mine. We've always had good relations. I'll tell him what Rochow has done. He has written a paper, and now applied for a patent on a new composition of matter never made by Kipping or Hyde." He called up Sullivan.

In the meantime, there had been a change of heart at Corning—a big change of heart. They said, "What? You're giving away our secrets?" Secrets are the whole heart and soul of Corning Glass Works. They fired Dwight Morrow for doing this. They told him, "You should never have gone to GE. You should have never given up any information from Corning. It was a horrible thing to do." They fired him.

Well, what to do? He'd already been there and already given the information. You can't pull it all back in. Coolidge talked on the telephone to Sullivan and said, "Here's the situation.

Rochow wants to send this paper to the *JACS*, but we don't want to undercut or shortchange Hyde. Has he sent in any publications concerning his work?" "No," said Sullivan. "He hasn't sent in anything." Well, in effect, "That is not our custom to do this." "Well," said Coolidge, "then we'll withhold Rochow's paper until Hyde can write up a paper of his own (21). They will be submitted simultaneously, so there'll be no question of priority."

I was called down to the front office and told this. Not asked, told. "You must hold up your paper until Hyde writes this." Then Sullivan came back and said, "Well, Rochow should have some kind of acknowledgement to the effect that Patnode and Marshall had gone to visit Hyde. There's some sort of derivation in there." Coolidge said, "Well, we'll write up some preface or something to the article, in order to acknowledge that you were in the field before us." He called me down the office again, and handed me a piece of paper and said, "Put this acknowledgement in the beginning of your paper." I hadn't been to Corning. I didn't know Hyde from a hole in the ground. I'd never met him. I didn't see why that should be put in my paper, which I regarded as a highly personal thing. I was telling about a discovery I had made, and I didn't like it one bit.

Now, you ask, "What effect does the Depression have on people?" I had a family. I had built a house, had a mortgage at that time. I had to do what the director of the laboratory told me to do, or else resign. I put it to you, would you have resigned in the 1930s under those conditions? I talked to Patnode about it. He said, "Oh, don't bother about it. GE's relations with Corning are real good." I said, "It shares the priority in the case, even a derivation." "Well," he said, "that's a matter for the patent department to worry about."

The upshot of it was that I sent in a paper. Hyde had hastily written a paper and sent it in on the same date. My paper went through the referees more rapidly than his did. His was held up. "Well," I said, "I did what I promised to do. I'm not going to bend any further back. I'm not going to arbitrarily hold up my paper after it's approved. No, go ahead and print it." That's why it appeared in the *JACS* before Hyde's paper did. The Corning Glass Works has hit me over the head with that statement ever since. They have spoiled my whole research career with that damn sentence. It plagues me right down to the present.

This little book I wrote on silicon and silicones (22).

**BOHNING**: Oh, yes, yes.

**ROCHOW**: Gee, I had always had good relations with Hyde and with Hunter and the other fellows who were in the chemical fraternity together. At the ACS meetings, we would get together and talk. We'd have meals together, and so on. I thought we had a good understanding with each other.

I sent Hyde and Warrick and Arthur Barry copies of this book—a professional courtesy. Wow, they came back like a ton of bricks. "You should never have written this without letting us know. It doesn't contain the history of Dow-Corning at all, and it should." I had actually written to the Dow-Corning people while I was writing this, told them what I was writing, and asked them. "I would need the properties of Pyrex and a little bit of history." I said, "Would you submit something to go into the historical part of it?" I never got an answer from them. They just ignored me. Yet, when it came out, they clobbered me. "You should never have done this—a terrible, terrible thing. There's not a single Dow-Corning person mentioned in it." That's not true—not true at all. I'd put in pictures of Kipping and of Hyde. That whole Hyde history, as far as I'm concerned, is in there. You can't always trust the industrial people.

Wonderful machines they've developed at Heidelberg. I don't know whether you've seen them, these automatic publishing machines. You feed in the manuscript at one end, and it not only sets up the type and prints it, but cuts the pages, binds the book and packages them at the end. My older son lives in Fort Lauderdale. He was interested in this book; he's an artist, and he got a job—it's hard for an artist to get an job—in industrial art, designing letterheads and doing the layouts and so on. He'd heard about the operation at Heidelberg and the company, so I took him there. He was astonished! He spent a whole day watching the operation.

Well, look, I've never written any book that hasn't been asked for. This book was asked for. The very first one I wrote on silicones, *An Introduction to the Chemistry of the Silicones*, was asked for (23).

**BOHNING**: Oh, yes. It was published in 1946.

**ROCHOW**: Thus it has gone on. Springer-Verlag I knew about from Dennis and Stock. Their editor in physical science, when he was in this country, came to see me and said, "See here, we have an interesting little book on polymers, but it's all about organic polymers. Couldn't you write something, a little book for the general reader, telling about silicones (22)?" "Well sure," I said, "but we'll need some background—something about silicone itself." "Well, go ahead and write it." I wrote it. I sent it over there and said, "Look, it's too much of me. There's nobody else. You should have a junior author or somebody to collaborate." "No," he said, "it's perfect the way it is. We'd like to publish it right away." They went ahead and did so. I sent the copies to the Dow-Corning people, and wow, they were so mad about it.

Well, what to do about it then? Dr. Reiner Stumpe of Springer-Verlag in Heidelberg was the one who came to see me. He asked for the book. Okay, I wrote it. There it is. Unfortunately, they omitted the photographs entirely in the first edition. Reiner Stumpe said, "It doesn't matter. We'll put them in the next edition." All that artwork for nothing, left there. Well, this is the German edition (24). The portrait photographs, they're not in here either. I thought they would put them in. Anyhow, they said, "There's no photographs in there. Hey, we don't even have a photograph of you in there." They asked for a picture. I sent them this one, which was taken in the last days of Chemistry One. That's reproduced in the German and also in the Japanese (25). The Russian doesn't have any photographs or diagrams in it (26). They said, "They don't have the money to do that."

[END OF TAPE, SIDE 5]

**ROCHOW**: Dow-Corning—I don't know what to do about that. However, let's go on to the main thing. I had methyl silicone. Okay, what good is it? If you make it from magnesium, who makes the magnesium? This was a Dow Chemical Company monopoly, magnesium. Kaiser made magnesium on the West Coast during the war, but gave it up afterward. You know about this. I guess you've probably read these things.

**BOHNING**: I've looked through it, yes.

**ROCHOW**: I had a new substance, methyl silicone. I had a composition of matter patent on it. I also had a copolymer—methyl phenyl silicone—that I thought was pretty good stuff. I had a string of patent applications, five in all (27). Patnode collaborated on one with me (28).

Corning Glass didn't like this at all. They filed interference suits on the five original patent applications that GE put in. I had to drop everything in the laboratory, go to Boston, and work with the old Boston firm of Fish, Richardson, and Heave on the interferences. Then, of course, there were endless depositions. I had to be there to travel with the lawyers when they went for their interviews—their depositions. I had to go over all the transcripts of the testimony and proofread, and so on.

We ended up with a stack of books that high—the proceedings of all the interference procedures they went through. We sent it into the patent office, and Dow-Corning contested every one of these. They lost every one of those interferences. They didn't have a case. However, they did clobber me forever after with that acknowledgement that Coolidge wrote and insisted I put in my first paper, but there was no conflict of priority there at all. There still isn't to this day.[1]

I had methyl silicone. What good is it unless you can make it cheaply? Marshall said, "Well, you've got methyl silicone—so what? You use magnesium. They can always undercut us, because they can charge us more for the magnesium than they charge themselves." Very logical, yes. It was up to me to find an alternative method for making it. That occupied me for two years or more. I tried everything I could think of—dozens, nay, hundreds of flasks and bottles of the results of all the experiments. I could not make it by any organic method or combination of methods—nothing. I won't go into all the things that I did. It's all in my notebooks, anyway.

**BOHNING**: You were being supported in doing this by Navias, however.

---

[1]From Eugene G. Rochow, *Silicon and Silicones* (Heidelberg: Springer-Verlag, 1987): Footnote 10, p. 70: "The decisions of the U.S. Court of Patent Appeals were published in *U.S. Patents Quarterly*, the first in Vol. 73, p. 534 (1947). The lengthy printed testimony in the cases gives a detailed and interesting, but seldom read, history of the early research on silicones in the USA."

**ROCHOW**: Yes, Louis Navias kept me going.

**BOHNING**: He was still supporting you through all of this, and giving you the time to work on this?

**ROCHOW**: Well, until the patent application went in (29) and the first paper was submitted (30). Then Navias said, "Well, see here. You want to go on with this, don't you?" "Yes." "Well."

He went to Marshall and said, "Look, this is not ceramics. Why is it in the ceramic department? You give Rochow a shop order to go on with this." Marshall said, "Well, we didn't develop this." [laughter] "Not Invented Here." He said, "I don't think there is any future there, because you can't compete with Dow"—then, by that time, Dow-Corning. He said, "Look, they took their process to DuPont in order to ship the chemical part out of Corning Glass Works." DuPont considered it and turned it down, you know. They said, "We have projects of our own. We don't want to take up a new thing like this. We don't know the future. There's no market for it." Hey, there's never a market for a new thing anywhere. They turned it down, and other people they went to turned it down. Marshall said, "Look, you can't compete with DuPont on a chemical project."

Do you believe this? An idea arises in one person's mind, in one brain. There's no such thing as a joint invention. You can have contributors; maybe the patent examiner will allow it. Anyway, I had an idea, and it was proven. I was stubborn enough not to give it up.

I went back and consulted the publications of Alfred Stock. He had made the hydrides. I had done the diagrams for his book, *Hydrides of Boron and Silicon* (6). How did he make the hydrides of silicon? Well, he went through a laborious procedure with magnesium silicide and acid, then collected the gases evolved. He also tried something else. He ran hydrogen chloride over ferrosilicon; got nothing but silicon tetrachloride. He tried it over an alloy of copper and silicon, and he got a useful but minor proportion of $SiHCl_3$—silicochloroform, it was called then.

"Fine," I said to myself, "I'll set up to make silicochloroform. I'll put a little methyl chloride in with the hydrogen chloride and see whether I can sneak in some methyl groups while all this is going on." I knew nothing about the mechanism. "It's all mysterious, but let's try it, okay?" I went down to the GE so-called chemical stockroom, which is just a junkyard of everything anybody had ever ordered and discarded. It had all kinds of bottles and boxes on the shelves, everything—going right back to bottles of stuff made by Charles P. Steinmetz, and labeled in his handwriting. [laughter]

**BOHNING**: Goodness.

**ROCHOW**: I had one of those bottles myself that I kept for many years in my office. Well, I found on one shelf, along with other alloys under the general heading alloys, a canvas bag. It was labeled copper-silicon alloy—fifty percent copper-silicon alloy. I opened it up and inside was a solid—but gee, it broke apart in my fingers, actually. It was very friable. It said, “Fifty percent copper.” Okay. I took it out and put some in a glass tube and passed hydrogen chloride over it, and yes, I got silicochloroform. I put in a minor amount—say, about ten percent by gas volume—of methyl chloride with the hydrogen chloride. I let the vapors that came out of the tube pass through a cold trap at minus-eighty degrees. After a run of several hours, I hydrolyzed the condensation in a mixture of ice and ether.

Well, the silicon tetrachloride and the silicochloroform hydrolyzed, of course, to give white solids, siloxanes—inorganic stuff. The ether layer I separated and dried and then evaporated. It left a sticky residue. “There must be methyl groups on there.” I heated the sticky material overnight to two hundred degrees, and in the morning I found a disc of clear resinous solid. Behold, methyl silicone that has never seen magnesium!

Well, I developed a method of analysis to establish the exact composition of the resin. I put the sample in a platinum boat in a silica glass tube—of fused silica—and subjected it to combustion analysis. Maybe it’s all in the book there.

There is an easier qualitative test you can conduct on any substance you suspect of being a silicone. You put a sliver of it in a Bunsen flame or even touch it with a match. If it’s a silicone, it will burn with a very low blue flame, and it will give a white smoke. Any organic polymer will give you a black smoke. If you want to be permanent about it, put a watch glass above the smoke. If the deposit is black, it’s carbonaceous. It’s organic. If the deposit is white, it’s methyl silicone, which has no carbon-carbon bonds in it. It burns with a very tiny blue flame, because there’s enough carbon and hydrogen there to burn, but it burns to a white smoke.

I showed this to Chemistry One students again and again. I showed this to a talk given at Edison College over here, just three weeks ago. Shall I do it for you? Do you see the point?

**BOHNING**: No, that’s all right. If you have the time, though, I would like to see it.

**ROCHOW**: I’ve got a sample of silicone rubber here. I’ve got a scissors here. I’ll take a sliver of silicone rubber and touch a match to it. [It burns as described here, giving a white smoke.]

Okay, going back to my experiment with silicon and methyl chloride, I knew the product was an organosilicon compound. It could only be a methyl silicone compound. It must be methyl silicone. Fine. I cut down the amount of HCl and raised the proportion of methyl chloride, and went through the whole procedure again and again. Finally, I got up nerve enough to put in methyl chloride alone. I hydrolyzed the product and got a sticky syrup after evaporating in the ether layer. I put that in an oven and it hardened.

I went racing down to the fifth floor to Patnode. He had just come back from a trip. I said, "Look. Methyl silicone that has never seen magnesium! What do you think of that?" He said, "How in the world did you make that?" I told him. He said, "Why, that's the old intermetallic couple. They make zinc diethyl that way"—from a zinc-copper alloy, is it? "Anyway, yes, it's an application of electrochemistry to organic synthesis." Well, I never thought of it that way. I just followed what Stock said, and then I worked at it till I got this.

"Well, that's great," he said. "Can you do it again?" "Well sure, I'll do it again." Next day I went back to him and I said, "Look, I made this after talking to you yesterday, the same thing. It's methyl silicone, all right." "Hey," he said, "is it all in your notebook?" "Yes." "Is it witnessed?" "Yes." "Can you do it ten times in a row?" I went back to the lab and I did it ten times in a row, witnessed by my lab partner—another Cornell inorganic chemist named William F. Gilliam, brought in to assist with the magnesium oxide work. Yes, it was all properly witnessed in the notebook and everything. "Yes, I did it ten times in a row." "Aha," said Patnode, "you've got something there. I want to talk to Marshall about it. I'd like to set up a little pilot plant and make enough of it so we can put it in electric machinery—some motors, some transformers especially—just to see what it'll do."

Okay, he set up to make methyl silicone this way. I guess he had argued it out with Marshall. Marshall had to agree. It bypassed the whole magnesium argument. He saw no reason why Patnode shouldn't do it, grudgingly. "Go ahead and finish your pilot plant." Patnode did. Gee, it all developed from there.

We soon used up that five-pound sample of copper-silicon alloy found in the old chemical stockroom. Patnode said, "No problem," or the equivalent in those days. "No problem. I know the fellows over in the foundry. I'll get them to make up a hundred pounds of copper-silicon alloy." They made up the copper-silicon alloy, the first twenty-five pounds of it. Sent it over—a massive metallic-looking alloy, of course.

It was hard to break up. We smashed it up and packed lumps of it in a glass tube. I ran methyl chloride over it, and nothing happened—nothing, absolutely nothing. Patnode had set up his pilot plant, using a two-inch copper pipe, six feet long. It was wound with hichrome to heat it. It had automatic controls for the temperature. It had a temperature recorder on there and all the fancy things. He put some of the new alloy in his copper pipe and ran methyl chloride in for a couple of days. No methyl chlorosilanes!

He told the patent people, "Look, hold up the patent application on this. It doesn't work with our new copper-silicon alloy. "Oh," they said, "this happens quite often. It's called losing the art." [laughter] Boy, we lost the art. Said Patnode, "What can we do?" "Well, the other stuff was aged for I don't know how long—years, at least twenty years. We may have to age this stuff for twenty years." "Well, let's accelerate the aging." We put some in a tube, heated it, and ran air over it. Then we ran oxygen over it. Sure enough, it began to disintegrate by intragranular oxidation. We put some of the disintegrated stuff in the copper reactor. Yes, it worked!

“Well,” said Patnode, “we can improve on that. We ought to be able to make silicon-copper alloy out of a mixture of copper and silicon powders. They would be in close contact, almost as good as the alloy where the intermetallic compounds crystallize out.” Navias watched this and he said, “There’s still a better method: the way we make ceramic samples. You mix the powders, and I’ll put them in a pill machine under high pressure. That’ll squeeze them together. Maybe some of the copper will wipe off on the silicon grains, too.” He did that. “Yes, that’s very much better.” We pressed pills—pellets—by the hundreds, by the thousands. We loaded them in Patnode’s reactor and sure enough, they made methyl silicone. I had a patent application on that with Patnode.

Well, what’s the next step? I wrote a paper about it and sent it in. Now, how did Corning get their hands on that? Oh, through what I would call the rank stupidity of the management level in big corporations. They learned about it in some other way.

**BOHNING**: Was it at the Gibson Island meeting?

**ROCHOW**: Say, that may be it. I went around giving talks, and I told about this method at a Gibson Island conference. See, you remember it much better than I do.

**BOHNING**: I only remember it because Warrick mentions it in his book (18). That’s the only reason. [laughter]

**ROCHOW**: I actually took along a little glass tube and lecture bottle of methyl chloride, and I set it up, and I ran that in front of them. You hydrolyze what comes out, or better still, take a piece of filter paper and hold it at the exit where the gases come out. You show that the paper becomes water repellent.

Patnode had discovered this water repellency thing. He pressed that to completion too, with patents on it (31). Yes, they learned about it at Gibson Island. They went back and oh, there was uproar in the Dow-Corning Corporation.

**BOHNING**: I think that was Art Barry who picked it up at that meeting.

**ROCHOW**: Yes. They took it up. Well, they wanted immediately to get patent rights on this thing, too—what was our patent situation, and so on. Meanwhile, Patnode’s pilot plant had come along anyway. It all developed until Marshall said, “There ought to be a chemical division of the GE Company now, and I should be the vice president to head it.” Well, the directors finally decided, after a long discussion of the engineers and the dozens of vice presidents, that they should have a chemical division. Who should head it? Hey, they didn’t chose Marshall. They said, "We have a good man down in Cleveland, at the GE Lamp Works in Cleveland, a metallurgist by the name of Zay Jeffries. Look, this involves an alloy, doesn't it?" "Yes. It involves a metal. Yes." "Then Jeffries is the man."

They brought him in and gave him this file, and installed him as the head of the new chemical division. It had no plant, no laboratory, no personnel. The first thing he did was ask, "Well, what's the Dow-Corning situation on this?" "Well," I said, "they'd like to get their hands on it, if just to provide methyl silicon trichloride, methyl trichlorosilane. They'd be very delighted with it, because they can then add the phenyl groups by Grignard's reaction. It saves them the first half, the much more troublesome step of getting the methyl groups on there. If they could get that, they'd go on from there."

Zay Jeffries came up with a brilliant idea. He said, "We don't know what they have up their sleeves. They know what we have. They keep their stuff secret. We don't know. I'll draw up a patent exchange agreement with them, whereby they can have a royalty-free license to every one of our patent applications in the area of organosilicon compounds, in exchange for every application of theirs that comes up."

He did that. Marshall agreed to it. It still raises my hackles that a man could go and sell his hard-working chemist down the river by offering to give up, free, the results of years of work—and for no reason; that is, no good sound reason. I said, "I don't care what they have coming along. This is something that we have—not only coming along, but proven. A proven concept, and we should give it away?" I got nowhere with Marshall. Navias said, "Not in my department; I'll wash my hands of it. Go see Coolidge." Well, by that time, it was C. Guy Suits.

I went to see Dr. Suits and he said, "Look, you talk to Zay Jeffries about it. That's his department now. That's his brainchild." I got an appointment at last, went to see the great man. I told him my tale of woe. In effect, he said, "There, there, little boy. We know best what's good for the General Electric Company. You have no experience in management or business." In other words, "You don't know what you're talking about. We'll take care of all the ramifications of this thing." He just dismissed me out of hand.

Who's the largest manufacturer of silicones today? Dow-Corning, worldwide. What's the product that out-measures everything else, by more than ninety-nine to one? It's methyl silicone. How do they make their methyl silicone? Every little snitch of it is made by the Rochow direct synthesis. How did they get it? They got it for nothing, because of the deal that Zay Jeffries made. That's the truth.

[END OF TAPE, SIDE 6]

**ROCHOW**: There's a GE joint venture with Toshiba in Japan. They make silicones now, with great success over there. They celebrated their twentieth anniversary and invited me over, along with Charles E. Reed, who is the MIT assistant professor GE brought in. He's a chemical engineer. "Here's a laboratory process Rochow has invented. Can you make an industrial process out of it? Can you devise the equipment?" "Yes." He gathered together a group of chemical engineers, and they actually designed a plant for this. Charlie Reed brought with him an important MIT development—the fluid-bed catalysts fluidized for petroleum refining.

Reed said, "Gee, the fluid bed is made to order for this. If we could run heated methyl chloride vapor over fluidized copper and silicon powder—mixed powders—we could make methyl chlorosilanes by the ton. We could make them by the thousands of tons." It's the way it's made today.

Yes, Toshiba invited Charlie Reed and me over there. If you go over now to their plant in Ohta, outside Kyoto, you'll find they have planted two trees, one in honor of me and one in honor of Charlie Reed. They have our names on them, standing there in the Japanese manner.

A wonderful race, the Japanese, the way they do things. Out came all of the laboratory personnel and most of the plant personnel, all dressed up. They arranged a number of benches with an awning—a marquee—overhead, and there were two holes in the ground to plant the trees. The president of the company delivered a little speech.

Then he had his assistant bring him a shovel—not an ordinary shovel, mind you, a brand new spade. They called it a shovel. A brand new spade, shiny, with a white ribbon attached to it. The assistant handed it to the president, who took up a little spoonful of dirt from the pile provided for him and put it in the hole with the Rochow tree in there. Then he took another spade, another ribbon-tied one, took a little dirt, put it in the one for Charlie Reed. A great deal of bowing, congratulations with the handshakes. Then they called upon me. I must go up, make my bows, take the beribboned spade—a new one, of course, not one that has touched dirt before—and help plant the tree. [laughter] Well, I actually put my foot on the spade, brought up a shovelful of dirt and put it in, and they all applauded wildly. Then Charlie Reed did the same for his. Then there were more speeches and some music.

The trees are there. Everything that we did was recorded on videotape. They sent me a copy of the tape, which I could play for you or loan to you if you want it.

**BOHNING**: I'd like to borrow it, if I could. I'll return it to you.

**ROCHOW**: Sure. I believe we were up in Maine at the time. They sent a limousine for me; they carted me to Boston Airport, carried in my bags. We stayed overnight in the Boston Airport, the driver with us to come take our bags in the morning and take them to the check-in counter. They put us in the first-class lounge, a first-class plane all the way, the very best hotel in Tokyo—oh, the very best of everything. This went on for an entire week and two weekends, in a very big way.

Now, as part of that they wanted Charlie Reed and me to hold what they called a symposium. They would sit down, and they wanted to ask questions, and we would give them our replies. There was a blackboard and everything. They put microphones on us. They recorded all the sound. They did videotapes of us, too. I told them the whole story of how the methyl silicone that they were making came about, starting with the Grignard preparations and going on through the discovery of the direct synthesis and the trials of going through that, and how Dow-Corning got it free, for nothing. I didn't hold back on any of that. They sat there bug-eyed. They got it all down.

Charlie Reed then, instead of scolding me mildly for bringing out all this history of General Electric Company, backed me up in it. He said, "Look, I would never have become vice president of General Electric Company if it hadn't been for the direct synthesis. The present vice president who's in there would never be in either, because I brought him through as assistant and all the way up through the ranks. I put him there. He owes a debt to Rochow, too." It went on and on. That's all in there in the proceedings. I don't know whether you could get a copy of that. Anyway, I've told you the whole story.

What else do you want to ask me?

**BOHNING**: Well, I appreciate your giving me all this information. It's very, very helpful. This was in 1940? I'm trying to get the dates straight.

**ROCHOW**: The first experiment was on May 10, 1940. The patent application was submitted later.

**BOHNING**: The patents were issued in 1941, weren't they? The first five are listed as being patented on October 7, 1941 (27).

**ROCHOW**: We applied for them in 1941, but the patents could not be issued. They were put under secrecy orders.

**BOHNING**: That's right. This would have been during the war.

**ROCHOW**: During the war.

**BOHNING**: Oh, all right.

**ROCHOW**: It could not come out until 1945. Then, of course, the cat was out of the bag. Yes. The metallurgical company in Niagara Falls, Union Carbide, wanted to get in on the act then. They were given a license with a royalty attached. Going back to 1946, here's the mechanism of methyl chloride and silicon copper, the Rochow method. Here. "Direct synthesis of organosilicon compounds. Contribution from research labs, General Electric Company" (32).

**BOHNING**: Where was that published?

**ROCHOW**: *JACS.*

**BOHNING**: *JACS*, all right. What year was this? I just want to make a note of that.

**ROCHOW**: Volume 67, issue of June 1945, page 963. That's the original article. I was looking for the date it was submitted.

**BOHNING**: It's usually right under the title or under the abstract, or above the title. Or is it at the end? In those early ones, I don't remember now.

**ROCHOW**: "Received January 29, 1945." Well, that one was after the patent application. This is the article. What I want is the patent. I did a lot of experiments in between May 10, 1940 and the time patented, October 7, 1941; yet the paper didn't appear until 1945.

**BOHNING**: When was the patent submitted?

**ROCHOW**: The application was April 27, 1940.

**BOHNING**: In 1940. All right.

**ROCHOW**: Wait a minute. That's methyl aryl silicones (33). That's not the one I want. I want the direct synthesis. "Composition of Matter," here we are. Patent No. 2,380,995, "Preparation of Organosilicon Halides" (34). This relates to the preparation of organosilicon halides, and more particularly to the product of hydrocarbon production—hydrocarbon-substituted silicon halides. That's the basic patent on the direct synthesis. The application was September 26, 1941.

From May of 1940 to September 26, 1941, I was engaged in trying all sorts of experiments. I wanted to know how extensive this general method was. Was it a general method we applied to methyl silicon bromines, and silicon iodides, and the ethyl compounds, to propanol to phenyl—to aromatic in general? There was experiment after experiment after experiment, and lots of them. Here are thirteen examples in the patent and all the claims.

Hey, that was a great day when Walter Rule came over the lab and said, "We got the final allowance on your application for the direct synthesis. Guess what claim we got as the number one claim, the most basic claim of all?" Well, I didn't know. "The claim that they allowed is the broadest possible thing you can imagine, 'The method of preparing of organosilicon halides which comprises effecting reaction between silicon and a hydrocarbon halide.'" Nothing about how or conditions, or anything. No limitation on the hydrocarbon halide or which halogen—anything. Just "effecting reaction."

Well, great. This is what the whole industry is based on. We're now into the millions and millions, the dozens of millions of tons of methyl silicone made each year worldwide, and made by this method.

**BOHNING**: When did the patent litigation situation develop? Was it in 1945 or 1946, somewhere in there? Or was it earlier?

**ROCHOW**: It was earlier, because it involves the preparation of methyl silicone by the Grignard method.

**BOHNING**: All right.

**ROCHOW**: It began with the results of the 1938 preparation. Once I had methyl silicone and showed that it was good insulating material, then of course, the patent application went in on this. Yes. "Resinous Materials and Insulated Conductors and Other Products Utilizing the Same" (35).

**BOHNING**: You had mentioned in your book that the publication of the patent proceedings of the litigation is probably one of the best description of early silicone chemistry there is (36). It's not something everybody reads, but it was pretty well spelled out there.

**ROCHOW**: Yes. Regarding what you were told by Earl Warrick and others, I had copies of the proceedings and the briefs submitted—everything concerned with those interferences. By the way, on methyl silicones and related products, that was application August 1, 1929 (19). See, that was well before the direct synthesis. Yes. What was I going to say? It was very important, and I lost it. Don't ever get to be eighty-five years old. [laughter] On the other hand, it's more fun than the alternative.

**BOHNING**: That's true.

**ROCHOW**: I get a lot fun out of life and keep active in lots of things.

Now, there was something about this. Oh, the International Organosilicon Symposia are held once every three years. See, there was one at Madison, organized by my former student, Robert West. Now, there was some occasion—some celebration—at Dow-Corning, and I was invited to participate. Helen will remember; I'll have to go back to her. Anyway, I got there and I found that they had assigned a young man from the laboratory to help me—that is, to accompany me wherever I went. He was to make sure I didn't go to any of the wrong places or see any of the wrong people. He stuck with me everywhere we went. He reminded me of the first time we went into East Germany, when they assigned someone to follow us in the street everywhere. Anyway, he was a very nice young man and we got along very well.

Well, he was full of this derivative business. I asked him, had he read the proceedings or even just the briefs of the patent interferences? No, he didn't know there was such a thing. "Well, don't you have a copy of these in your library?" Well, he'd never heard of such a thing and didn't think there was a copy there.

People write histories of the silicone industry. There are all sorts of histories, and they never refer to the basic primary source. You have to look at the testimony of Eugene Sullivan and James Franklin Hyde and Arthur Barry and so on, all in those patent interference cases. This is sworn testimony, and you'll <u>find</u> there what's what. There is no derivation—no derivation at all. The statement that I was required to put in my first paper was purely a matter of courtesy between the GE vice president or director of the GE research laboratory in Schenectady, and the director of the Corning research laboratory. It was a personal communication between them. That's the verifiable truth.

You get hold of the interferences—well, the interesting thing was that this young man in Midland had never heard of these. He didn't believe—he just truly didn't believe—that they existed. He said when he got there, he was told that silicones were a Dow-Corning invention. All the developments came from Dow-Corning. Anything that General Electric did was based on, "Well, GE stole from Corning." That was the word he used, "What GE stole from Corning and has used ever since." "Now," he said, "every new chemist who is brought in is told that and indoctrinated with that." That's not only a fallacy, it's just simply an untruth, what they tell those young people. I would say if you're writing any kind of history, by all means go to the sources.

Look, I had to leave the General Electric Company. I can tell you why and how, if you want to know.

**BOHNING**: That was what I was going to ask. You had a three-year period between this point and when you left in 1948, I believe. Is that correct, that you left in 1948?

**ROCHOW**: Yes.

**BOHNING**: I was curious as to what you did in those three years, and then why you left.

**ROCHOW**: Well, in the first place, I helped to bring in and train a dozen young chemists to get into this silicone project, do lab work—development work, mostly—on it, but to do exploratory research on it—all different things. They were, to a man, organic chemists. They didn't know anything at all about organometallic chemistry—no idea what was going on. The first thing each one wanted to do, individually, was to try organosilicon compounds in every named organic reaction that's listed in the organic textbook.

Well, I had written monthly reports. That was required. I had copies of all these monthly reports. I wrote summaries of the monthly reports, so I was asked to put together a kind of silicone bible (23): "How did this all arise in the GE organization? What's been done so far? Is there an index of every research project that's been carried out so far?" It was up to me to provide this. I had to write. People said, "Don't you find writing hard?" Well, all chemists hate to take time away from the lab bench to do this, but I had to do it.

I wrote the summary, and I traveled the length and breadth of the country, giving ACS lectures on silicones. Then I was asked to write the little sixty-page book—which got me in hot water too, because Marshall wouldn't approve it. It was requested by John Wiley & Sons in New York. Marshall sat on it. I asked about it. Week after week went by; a month went by. Finally, I cornered him in an office and said, "What about my little book, which was requested by Wiley & Sons? They do all the GE electrical engineering textbook things for the incoming students. They're pressing me for it. Where is it?" He opened the bottom desk drawer and said, "I have it here." "Well, they've put the approval on it. It has to go in for publication." "No, it isn't going in," he said. "Why?" "I won't send it in until everybody else in the whole silicone project has a chance to write a book, too."

[END OF TAPE, SIDE 7]

**ROCHOW**: What can you do? I went down to see Dr. Coolidge again. I said, "The policy in the company, as I understand it, is to publish." "Yes," he said. "The John Wiley & Sons editor came to see me. He said he had your approval, and the approval of the vice president in charge of engineering, to come and see me and talk to me about this new development called silicones, and also to give me permission to ask you to write a book about it." "Yes, write the book." "So I wrote the book. Therefore, isn't it time that it was turned over to Wiley? By what authority is Marshall holding it up?" Coolidge, who would never offend anybody, said, "I'll talk to Abe about this." The next day, Marshall's secretary called and said, "The book is ready here." [laughter] I sent it in.

See, there are all sorts of aspects that are personal jealousies and what not. How did this come up and why?

**BOHNING**: Well, we were talking about the three years between 1945 and 1948.

**ROCHOW**: Oh, yes. All right.

**BOHNING**: I'm just checking here the book you're talking about.

**ROCHOW**: *An Introduction to the Chemistry of the Silicones*. This is the first edition. There was a second edition, too (37).

Well first, during the war years, there were very few people who could talk about what they had done. I had published about methyl silicone and the water repellent business and all that, and Patnode had. The ACS people in headquarters asked me would I be willing to go around to local sections. They would make up a tour for me, take care of everything—all expenses, and so on. [laughter] I said to Dr. Coolidge again, "Eventually, we're going to need markets and customers for this. Is it all right to talk about it at ACS local section meetings?" "By all means," he said, "if you're willing to take the time and endure the overnights in the Pullman cars and so on, go to it." It was the whole series—a long series of talks all around the country. First the East, and then the South and the Midwest, then to the Pacific coast and the Northwest, and all over.

When it came time to write the book, of course it was easy to just consider the experience of those lectures and write it. I was busy first informing and educating all the neophytes who came into the laboratory to work on the project. Second, I was busy collecting all the monthly reports, organizing them, writing abstracts, indexing them, handing out copies to all the new people who came in. Third, of course, I had the lectures; fourth, I had my own research—my own investigations. I had long strings of projects I wanted to carry out, but I had no retinue; there was no string of assistants.

I did have Gilliam, who switched over from the magnesium oxide work to work on silicones. William Farr Gilliam, known as Gillam—that's the way he pronounced it. He was a nice Southern gentleman. He investigated under my encouragement and direction. "Hey, is copper unique in this? Are there other metals that would catalyze this reaction? Are there combinations of metals? If so, what are the metals? What are the yields? How much should we be patenting?"  Everything.  The whole development of zinc promoter came out of that work.  I was busy with that for awhile.

Meantime, people at GE—Kingsley and others—had worked on the possibility of energy from nuclear fission.  At the end of the war, DuPont, who had built and supervised and run the big operation up there in Richland, Washington, had answered the question, "How do you operate the chain reaction to get the maximum yields of U-235 and later, plutonium?  How do you separate these?"  The week after the final peace treaty was signed, they wanted out.  They had already had the label of "merchants of death" smeared on them from the First World War, when they had first made explosives and other things.  They wanted out right away.

The federal authorities approached GE and said, "See here, DuPont wants out of this.  It's important, surely, to General Electric, as a way of generating domestic electric power.  We want you to take it over."  The GE people agreed to take it over.

"All right," Coolidge said, "the first thing we should do is establish a branch of the famous GE research laboratory out there in Richland.  Whom do we have to head such a thing?"  Winton Irving Patnode—they chose him, sent him out there.  They gave him an office and a secretary and said, "Now bring in all the people you need.  This is going to be a branch of the GE laboratory.  Our aim is to produce domestic electric power so cheaply that they won't even bother to meter it.  It'll be like water in some communities.  GE, of course, will operate the piles.  Well, they will sell central station generating equipment and sell every electrical device imaginable, all based on nuclear power.  Go to it."

Patnode needed some help and asked Marshall.  Marshall said, "Well, you need some inorganic chemists."  Coolidge said, "Yes, they need all the inorganic chemists we have out there."  All the inorganic chemists they had—I was the only one.  They rooted me out and sent me there.  They pulled me out of the silicone project and sent me out there.  If you're a good company man, you go where they send you.  You do what they tell you.

Well, it was new and exciting, but I hated to leave.  I'd built the house near Schenectady, had a family, and so on.  I went out to Richland, Washington.  Patnode was already established there.  We studied together the separation process they used.  Then Pat said, "Well here, you get to work on this.  There surely must be different and better ways to do this."  Yes, he had the Edison belief.  "There must be a better way; go find it."  I worked out there, went through the separation process in the plant.  Moved into one of their barracks places and after awhile, my wife came out and joined me.  Drove across the country with Evelyn Bassage Patnode.  It was a great day when they arrived.  We could then move into family quarters, Priscilla George Rochow and I.

Well, I followed the process.  I stayed up all night and went through thirty hours straight following the procedures, and the testing and the sample, and operating the counters to get the quantitative information and what to do with it.  "When do they add the carrier, precipitation on the carrier, separation from the carrier, solvent extraction?"  And so on.

Okay, but then came the final blow. The General Electric Company was then further requested by the federal government to drop all the domestic power project and work on nuclear propulsion for naval vessels. Impossible—as a Quaker and a longtime pacifist. Well, I'd been lucky because I was too young for the First World War and just slightly too old for the second, although I was registered then. I thought we were through with all that, and here they wanted me to go into classified work and devote my time to the design and construction of destructive weapons of war. It had only one purpose, and that was to hurt people. I said, "I won't do it." Well, if you're a good company man, you do as they tell you.

I resolved then, I needed more control over my life. I didn't want to be moved out there in the first place. I certainly wasn't going to work on that project, so I wrote some letters around to university people to see what might be available. To cut all that short, I had three offers. One was from the University of Southern California, where I was told they have the very best football team in the country and wonderful sunshine, and so on. One was from the University of Indiana in Bloomington, which is famous for its humanities, and especially music and history, and so on. "What am I going to do, move out to the Midwest?"

I also had an offer from Harvard, so I referred back to Cornell. Dennis of course had died, but his successor in the inorganic area was Albert Washington Laubengayer. I wrote to Lauby, and he said, "Well, there's no question about it. Even if they don't ever give you tenure at Harvard, it's a good place to have come from." [laughter] I accepted that. I was brought in there in order to take over Chemistry One from Arthur B. Lamb, who was due to retire in one more semester. That's how I went there.

That covers all those three years, the events that happened, and why I went there and when. I still had to uproot the family, take them to Cambridge. The interesting upshot of all this, which you should get in your history, was that after I got established at Harvard, well, I was given tenure. I had been through a lot there. Dreadful days—still can't go through that. My wife died of cancer. My older boy was in Children's Hospital with polio. I went through living hell there, but there's where I put down my roots.

All right, I'll get around to what I meant to tell you. Years later, Guy Suits, by then vice president of GE and director of the laboratory at Schenectady, came to see me. He said, "I realize now it was a dreadful mistake to take you off the silicone project. Everyone I meet in the country—all the officials, directors of research—tell me that. I see it thoroughly now. Won't you please come back under your own terms, any way you want to do it." I said, "Well, I put down my roots here. I like the academic life. I'm glad I'm back to it." "Well," he said, "we'll make an arrangement with Union College, so that you are a professor there and also a research associate at the GE laboratory. You can do things the way you want there." I turned it down. I was glad I stayed at Harvard.

Well, I'm sorry about the outburst.

**BOHNING**: No, I understand.

**ROCHOW**: Some things affect me deeply, and that's one of them. I never learned to be callous about it. I should have. Well, so what's next?

**BOHNING**: Well, I think we've covered everything very well. There's certainly much more of your career we could talk about, but I've already taken four hours of your time.

**ROCHOW**: Well, get it all down in one trip. I believe in finishing things up.

**BOHNING**: I think what I would like to do is to conclude by asking you the last question. That is—especially in view of what you've been telling me—what did it mean to win the Perkin Medal?

**ROCHOW**: What did it mean? It gave me one tremendous lift at a time when I needed a lift. In 1962, you see, I had just left GE and was at Harvard. The New Jersey section of ACS had previously awarded me the Baekeland Medal. I should show you the Baekeland Medal. A disk of gold that big and a quarter-inch thick, given to someone every three years—someone who had discovered a new polymer, a new kind of polymer. That gave me a lift then—but say, after the dark, dark years, the Perkin Medal, given to someone who starts a new chemical industry.

Yes, I had two small children when my wife died. We fussed around with housekeepers, and so on. After I met Helen, all that changed. We got married. That was just before the Perkin Medal dinner. Yes, she went with me to New York. We stayed at the Waldorf Astoria, the legendary hotel. Well, it was just a revelation to both of us, of course, who had lived through Depression years. We had never done things like that.

It was a wonderful occasion. Of course, it gave me increased standing, but that wasn't important. It meant nothing, and I mean nothing, to the Harvard administration. They kept themselves separate from industry, even my colleagues in the department there. "That's all right," I thought, "it brought recognition." I had developed a new kind of polymer, and I started a new industry. This was way before anything of the sort was given to any Dow-Corning man. I felt good about the whole thing there, yes. Well, the graduate students at Harvard learned about it, of course. The word got around; incoming graduate students knew about this. It helped in my little squad of graduate students—research assistants and Chemistry One helpers, and so on. The most important thing was the tremendous lift it gave me—international recognition of the work. International recognition has meant a lot to me. Well, you look at these diplomas and awards.

**BOHNING**: Yes, I was looking at these before.

**ROCHOW**: Here is my honorary degree from Braunschweig. They have a laboratory named after me there. That was in 1966. This one is from Dresden, and that's 1992. See, it's still going on, all these things that have happened. I go to all the international meetings—not only the Organosilicon Symposia, but the organometallic conferences, the inorganic things. Hey, last summer we even had a conference in Norway on Nordfjord on the West Coast, about the industrial uses of elementary silicon. They invited me over there—very interesting, yes. More new uses—chemical uses—they're looking for.

The international recognition has meant a lot to me. Gee, I've traveled a lot. Helen has enjoyed this. She's my linguist. You ask her questions in Russian, in German, in French, in Spanish—she'll tell you. I had to learn to read German and French in graduate school, but I didn't have any idea of how the language sounded.

On my first sabbatical leave, we went to Austria, because it was centrally located. I was offered a guest professorship at Innsbruck. I looked at the map, and Innsbruck was about equidistant from Rome, Berlin, and Paris. You could get anywhere overnight from Innsbruck. I went there and moved the whole family over. Helen not only raised those two children I had, but had a son of her own. He was two years old when we went abroad. He still remembers a lot of that. Well, all three kids got a lot out of that year of living in Tyrol, near Innsbruck. We go back whenever we can.

Of course—I guess this derives also from the Perkin Medal—I was known over there. I was invited to give talks, which I did first in English, and then in German. I had to find shelter and food and transportation for a family of five. Going into a foreign country the first time—without knowing how to speak the language—I <u>had</u> to learn, so I got along in my bumbling way.

All right, I don't know whether this provides a sufficient example, but why was I invited by Springer-Verlag in Heidelberg to write a book (22)?

[END OF TAPE, SIDE 8]

**ROCHOW**: All that came out of the international recognition, beginning with the Perkin Medal. Does that answer that question sufficiently?

**BOHNING**: Yes. How do you spell the name of that fellowship that you received?

**ROCHOW**: The Heckscher research grant of Cornell?

**BOHNING**: Yes. It went way back when you were at Cornell. I am not sure of the spelling.

**ROCHOW**: Heckscher. There was a Heckscher Chemical Company. They provided money for this one fellowship, supervised by Dennis, of course. He got the whole department to agree that this would be the equivalent of an assistantship, what was called at Harvard a teaching fellowship. It would carry the same stipend but require laboratory work on a project other than a thesis. That's where the fluorine work came from.

**BOHNING**: All right. Well, I think on that note, I'm going to thank you very much for spending the afternoon with me.

**ROCHOW**: Well, I thank you for being so patient while I spun the whole yarn.

**BOHNING**: Well, it's a fascinating story.

**ROCHOW**: I'm sure I bored the pants off you.

**BOHNING**: No, no. Not at all.

[END OF TAPE, SIDE 9]

[END OF INTERVIEW]

## NOTES


1. F. E. von Rochow, *Der Kinderfreund* (1776; reprint, Berlin-DDR: Der Kinderbuchverlag, 1985).

2. L. M. Dennis, R. W. Work, and E. G. Rochow, “Indium Trimethyl,” *Journal of the American Chemical Society*, 56 (1934): 1047.

   E. G. Rochow and L. M. Dennis, “The Properties of Thallium Triethyl,” *Journal of the American Chemical Society*, 57 (1935): 486.

3. E. G. Rochow, “Contributions to the Chemistry of Fluorine” (Ph.D. thesis, Cornell University, 1935).

4. L. C. Pauling, *The Nature of the Chemical Bond and the Structure of Molecules and Crystals; An Introduction to Modern Structural Chemistry by Linus Pauling*. George Fisher Baker Non-Resident Lectureship in Chemistry at Cornell University, Vol. 18 (Ithaca: Cornell University Press, 1939).

5. L. M. Dennis, R. W. Work, and E. G. Rochow, “Indium Trimethyl,” *Journal of the American Chemical Society*, 56 (1934): 1047.

6. A. Stock, *Hydrides of Boron and Silicon*, English ed. George Fisher Baker Non-Resident Lectureship in Chemistry at Cornell University, Vol. 12. (Ithaca: Cornell University Press, 1933).

7. E. G. Rochow, *Publication List—Books.* See Chemical Heritage Foundation Oral History Research File #0129.

8. E. G. Rochow, “Choices,” *Journal of Chemical Education,* 63 (1986): 400.

9. E. G. Rochow, *List of U.S. Patents*. See Chemical Heritage Foundation Oral History Research File #0129.

10. L. M. Dennis, J. M. Veeder, and E. G. Rochow, “The Preparation of Fluorine by Electrolysis,” *Journal of the American Chemical Society,* 53 (1931): 3263.

11. E. G. Rochow, “Oxyacids of Fluorine,” *Journal of the American Chemical Society*, 54 (1932): 832.

12. E. G. Rochow, “Oxyacids of Fluorine. II,” *Journal of the American Chemical Society*, 55 (1933): 2431.

13. T. G. Rochow and E. G. Rochow, *An Introduction to Microscopy* (New York: Plenum Press, 1978).

14. T. G. Rochow and P. A. Tucker, *An Introduction to Microscopy*, 2nd ed. (New York: Plenum Press, 1994).

15. E. G. Rochow, "Electrical Conductivity of Quartz, Periclase, and Corundum at Low Field Strength," *Journal of Applied Physics*, 9 (1938): 664.

16. O. Hahn and L. Meitner, "The Trans-Uraniums, Radioactive Transformation Products Obtained Artificially from Uranium," *Scientia*, 63 (1938): 12.

17. W. H. Perkin and F. S. Kipping, *Organic Chemistry* (London, 1894. 2nd ed., 1929).

18. E. L. Warrick, *Forty Years of Firsts: The Recollections of a Dow Corning Pioneer*, ed. Jeannine Marschner (New York: McGraw-Hill, 1990).

19. E. G. Rochow, "Methyl Silicones and Related Products," U. S. Patent 2,258,218, issued 7 October, 1941 (application filed 1 August, 1929).

20. E. G. Rochow and W. F. Gilliam, "Polymeric Methyl Silicon Oxides," *Journal of the American Chemical Society*, 63 (1941): 798.

21. J. F. Hyde and R. C. DeLong, "Condensation Products of the Organosilanediols," *Journal of the American Chemical Society*, 63 (1941): 1194.

22. E. G. Rochow, *Silicon and Silicones* (Heidelberg: Springer-Verlag, 1987).

23. E. G. Rochow, *An Introduction to the Chemistry of the Silicones* (New York: John Wiley & Sons, 1946; London: Chapman & Hall, 1946).

24. E. G. Rochow, *Silicon and Silicones*. German ed. (Heidelberg: Springer-Verlag, 1991).

25. E. G. Rochow, *Silicon and Silicones*. Japanese ed. (Heidelberg: Springer-Verlag, 1992).

26. E. G. Rochow, *Silicon and Silicones*. Russian ed. (Heidelberg: Springer-Verlag, 1989).

27. E. G. Rochow, "Methyl Silicones and Related Products," U. S. Patent 2,258,218, issued 7 October, 1941.

    E. G. Rochow, "Halogenated Aryl Silicones," U. S. Patent 2,258,219, issued 7 October, 1941.

    E. G. Rochow, "Resinous Materials & Insulated Conductors & Other Products Utilizing the Same," U. S. Patent 2,258,220, issued 7 October, 1941.

E. G. Rochow, "Aroxy Silicones & Insulated Conductors & Other Products Utilizing the Same," U. S. Patent 2,258,221, issued 7 October, 1941.

E. G. Rochow, "Methyl Aryl Silicones & Insulated Conductors & Other Products Utilizing the Same," U. S. Patent 2,258,222, issued 7 October, 1941.

28. E. G. Rochow and Winton I. Patnode, "Organosilicon Compound," U. S. Patent 2,352,974, issued 4 July, 1944.

29. E. G. Rochow, "Methyl Silicon Halides," U. S. Patent 2,286,763, issued 16 June, 1942.

30. E. G. Rochow and W. F. Gilliam, "Polymeric Methyl Silicon Oxides," *Journal of the American Chemical Society*, 63 (1941): 798.

31. W. I. Patnode and R. W. Schiessler, "Cyclic Organosilicon Compounds," U.S. Patent 2,381,002, issued 7 August, 1945.

W. I. Patnode and R. W. Schiessler, "Method of Preparing Tetrachloroethylene and Chlorosilanes," U.S. Patent 2,381,001, issued 7 August, 1945.

W. I. Patnode and E. G. Rochow, "Organosilicon Halides," U.S. Patent 2,380,996, issued 7 August, 1945.

W. I. Patnode and R. W. Schiessler, "Halogenated Organosilicon Compounds," U.S. Patent 2,381,000, issued 7 August, 1945.

W. I. Patnode, "Organic Dihalosiloxanes," U.S. Patent 2,381,366, issued 7 August, 1945.

W. I. Patnode and R. O. Sauer, "2-Haloethyoxysilicon Compounds," U.S. Patent 2,381,138, issued 7 August, 1945.

32. E. G. Rochow, "Direct Synthesis of Organosilicon Compounds," *Journal of the American Chemical Society*, 67 (1945): 963.

33. E. G. Rochow, "Methyl Aryl Silicones & Insulated Conductors & Other Products Utilizing the Same," U. S. Patent 2,258,222, issued 7 October, 1941.

34. E. G. Rochow, "Preparation of Organosilicon Halides," U. S. Patent No. 2,380,995, issued 7 August, 1945 (application filed 26 September, 1941).

35. E. G. Rochow, "Resinous Materials & Insulated Conductors & Other Products Utilizing the Same," U. S. Patent 2,258,220, issued 7 October, 1941.

36. E. G. Rochow, *Silicon and Silicones* (Heidelberg: Springer-Verlag, 1987): Footnote 10, p.

70.

37. E. G. Rochow, *An Introduction to the Chemistry of the Silicones*. 2nd ed. (London: Chapman & Hall, 1951).

INDEX

**C**

**I**



**J**

**S**