THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

EARL L. WARRICK

Transcript of an Interview
Conducted by

James J. Bohning

in

Midland, Michigan

on

16 January 1986

(With Subsequent Corrections and Additions)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Dr. Earl L. Warrick

(Date) February 11, 1991

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

EARL LEATHEN WARRICK

1911	Born in Butler, Pennsylvania on 23 September

Education

Carnegie Institute of Technology

1933	B.S., chemistry
1934	M.S., physical chemistry
1943	Sc.D., physical chemistry

Professional Experience

1933-1934	Teaching Assistant, chemistry, Carnegie Institute of Technology
1934-1935	Teaching Assistant, chemistry, Brown University
1935-1937	Assistant, Mellon Institute
1937-1946	Fellow, organo-silicon chemistry, Mellon Institute
1943-1945	Instructor, mathematics, Carnegie Institute of Technology
1946-1955	Senior Fellow, Mellon Institute
1947-1948	Instructor, chemistry, University of Pittsburgh
1955-1956	Administrative Fellow, Mellon Institute

Dow Corning Corporation

1956-1959	Assistant Director of Research
1959-1962	Manager, Hyper-Pure Silicon Division
1962-1968	General Manager, Electronic Products Division
1968-1972	Manager, New Products Business
1972-1976	Senior Management Consultant

Saginaw Valley State College

1979-1980	Interim Dean, Science, Engineering and Technology
1983-1984	Interim Dean, Science, Engineering and Technology
1984-	Special Assistant to the Vice President for Academic Affairs

Honors

1976	Goodyear Medal, Rubber Division, American Chemical Society

ABSTRACT

Earl L. Warrick begins the interview with a description of his parents and childhood, which involved frequent moves between cities. He remembers a seventh grade teacher who inspired his interest in chemical engineering by having him build a one-tube radio. He tells of his undergraduate years at the Carnegie Institute of Technology, where the chemical engineering department was a bit disappointing. This led him to switch to physical chemistry, in which he received a master's degree. After recounting his year at Brown, Warrick describes his experiences at the Mellon Institute, where he developed a glass coating. He received his Sc.D. for a kinetic study carried out almost exclusively on nights and weekends while he continued work at Mellon. Warrick summarizes his career at Dow Corning, including the development of the "200 fluid," extensive rubber, polymer, and silicone research, his invention of "Silly Putty," and his work with silicon. He mentions the influence of several colleagues, especially McGregor, Collings, Hyde, Bass, and Speier. Warrick concludes by commenting on his position at Saginaw Valley State College, his current writing, and the changes that have occurred in chemistry throughout his career.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Beckman Center, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1987, and has been associated with the development and management of the Center's oral history program since 1985.

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INTERVIEWER: James J. Bohning

LOCATION: Midland, Michigan

DATE: 16 January 1986

BOHNING: Dr. Warrick, I know that you were born on December 23, 1911, in Butler, Pennsylvania. Can you tell me something about your father, Samuel Edward Warrick, and your mother?

WARRICK: My dad was with the Bell Telephone Company all of his life. He had never been to college, but he had attended a small business school in Pittsburgh. My mother was from a farm just outside Pittsburgh. I guess her education probably went through high school but that's about it. They met and married, and he moved around a great deal as a telephone man. I used to have a number (but I'm not sure of it anymore) as to how many different places we lived in about twenty years. We moved an average of once or twice in a two- or three-year period. At one point I recall we moved without unpacking. [laughter]

BOHNING: Do you have any siblings?

WARRICK: No, I'm an only child.

BOHNING: What was it like growing up under these circumstances of moving around a lot?

WARRICK: It was a little bit traumatic, because I never quite became a member of the local group. I'd always come in late. Everybody had known everybody else for a long time and I was the new kid on the block. And I was redheaded and I had a little bit of a short fuse, I guess. So maybe I didn't get along too well either. [laughter]

BOHNING: Were there any particular people that had any influence on you during the time before you went to high school?

WARRICK: Yes, I had a very interesting time in Harrisburg when we finally lived there. I guess I was in Harrisburg from about the fifth grade through the ninth grade. When I reached the seventh grade, my dad bought me a slide rule and a set of drawing instruments. The man who ran our drawing class in the seventh grade was quite interested in a lot of things, and he taught us to use the slide rule. He had us build a one-tube radio with a UV-199 tube. My dad was willing to go along with that idea and he even put an antenna on the roof. We put this thing together and it was operated with batteries. We began to bring in stations from all over. It was amazing. Of course, the air wasn't as cluttered then. We got Chicago and Pittsburgh and WGN Schenectady. Finally, everybody got tired of me being the only one that could listen, so I was allowed to buy two more tubes and put in two stages of audio amplification. We got a big paper cone speaker and now the whole family could listen. During that seventh and eighth grade period, I spent a lot of time with that equipment. We really enjoyed it and it was a lot of fun in the early days.

BOHNING: Do you recall the name of the teacher?

WARRICK: No I don't, unfortunately. He was a very strong influence on my life, though. I can recall having to write an essay on what my plans were for the future. And even at that early stage I wrote on chemical engineering. Had we stayed in Harrisburg, I might have gone to Penn State. However, when we finally moved to Pittsburgh, we decided on Carnegie Tech.

BOHNING: You went to Perry High School in Pittsburgh?

WARRICK: Yes, in Pittsburgh. Well, between Harrisburg and Pittsburgh we went to Williamsport for a short time and then moved back to Pittsburgh. Much of that moving was done based on my mother's health. She was an asthmatic and Dad had to give her shots of adrenaline every so often. It was a pretty rough time. So we moved back to Pittsburgh in 1927, and I went into the eleventh grade in Pittsburgh. And again, I was the new kid on the block and not a member of the group. So, I studied and I guess I did very well because I graduated as valedictorian. That was almost forced on me because there wasn't anything else that I was doing. I also ended up grading papers for the chemistry professor because he found out I liked chemistry and I was good at it.

BOHNING: Was that your first exposure to chemistry?

WARRICK: Well, I'd had a chemistry set when I was growing up in Harrisburg. Although chemistry sets were not really very good training, they did introduce you to chemicals. I never did blow up the house or anything of that sort. [laughter]

BOHNING: What about mathematics and physics?

WARRICK: They went very well. I was always pretty good at math. I took all the math that the school had and I took all the physics they had. When I finally went to Carnegie Tech, I took all the math they had, so I had no problem with math.

BOHNING: Are there any high school teachers that you remember?

WARRICK: Not really. They were all pretty good. I do remember a Latin teacher who taught me most of the English that I really learned, because he was insistent on diagramming sentences which I had not met up with before. That taught me grammar relationships a lot better than almost any other situation. The math and physics teacher was good but I certainly can't remember his name. He was an awfully good teacher.

BOHNING: Was your choice of Carnegie Tech just because it was located near you?

WARRICK: Location, and because of our economic circumstances. We were not poor but we certainly were middle class and I just didn't have the option to go away to a bigger school. Carnegie Tech was a good school, and I did it by commuting and that meant it was a lot less expensive. During the four years, I drove a car and took a friend of mine from the same borough and my Dad to work. We would be there all afternoon because we had long laboratories in the afternoon. Then we'd come through the city and pick my Dad up on the way home. So we ended up having pretty good transportation during the period.

BOHNING: For most of that time that was the Depression.

WARRICK: That's right. It was right in the Depression. It affected us because, although my father wasn't laid off or anything, one of the banks that he was involved in closed. So some financial problems did rear their heads shortly around that time. Although tuitions in those days were ridiculously low. It was something like sixty-five dollars a term, for the whole half year. So it really wasn't a great expenditure.

BOHNING: Were you in the chemistry department or chemical engineering?

WARRICK: I was in chemical engineering. Now, Carnegie Tech at that time did not have what I would say was a good chemical engineering department. I recognized that by the time I graduated. In fact, during the course of my work at Carnegie Tech, I got to know Dr. Jake [John C.] Warner. At that time he was not head of the department but soon did become the head of the department. He knew, as I did, that the chemical engineering department wasn't very good. So he encouraged me to take a master's degree and get some chemistry, and I emphasized physical chemistry from that point on. So I stayed on for an extra year.

Well, I tried to get a job. I graduated third in the whole school, and second in chemical engineering. But out of our class of about seventeen in chemical engineering one person got a job, and that was as a male secretary downtown at some magnificent sum like sixty dollars a month. So there were just no jobs available. A friend and I even looked into the possibility of cleaning up oil for fleets of trucks, because we did know something about what you could do to oil to take out the acids and filter it and so on. We found that one fleet already was doing it and we didn't think there was enough business in that, so we dropped that idea. So I accepted a four-hundred-dollar-a-year fellowship at Carnegie Tech and did my master's degree there for Dr. Warner. I worked with him for a year. That was all in kinetics of reactions in solution.

BOHNING: Let's go back for a moment to the chemical engineering department. What was the department like?

WARRICK: It really didn't have good equipment. They had a little bit of a laboratory with pipe fitting tools. When they talked about chemical engineers as being pipe fitters, well that was about it. We did a few experiments on fluid flow and that sort of thing, but nothing really that taught us anything. The books were about our only source of information. The man who was head of the chem engineering department (I can't think of his name right now) had a lot of consulting work on the oxidation of petroleum products to make other things. He would lecture and talk and get off into discussions about when he consulted here and when he consulted there, but he was not teaching us any chem engineering. His assistant prof in the area was a man who really hadn't had good training either. So, I felt after I'd graduated as a chem engineer that I certainly couldn't accept a job as a chem engineer. I just wasn't trained. After going back and getting a master's degree in chemistry, that was different. I felt that I had a good background because Dr. Warner was good and the chemistry courses were fine. They were excellent and I had no problems.

BOHNING: How many chemistry courses did you take as a chemical engineer?

WARRICK: I took all of them they had. [laughter]

BOHNING: Are there any chemistry faculty that you remember?

WARRICK: Well, yes. There was Dr. Warner and Dr. [Harry] Seltz. I can't think of the organic man's name. He was one, though, that we recognized the fact that his notes were at least ten years old, and he was just going through the notes, really. We recognized that he wasn't as good a teacher as he ought to be. The analytical man was good, but he had a business on the side. I don't know if you've seen the product "Sea Breeze," which is a skin-cleansing lotion. That was his product, and I guess he was making a good amount of money off it. But he did teach us analytical procedures. So I did learn chemistry there.

BOHNING: You also had an assistantship when you were doing your graduate work.

WARRICK: Yes. As a master's degree student I monitored freshman chemistry labs and recitation sections for that four hundred dollars. That was a good experience. It wasn't anything outstanding and I had no problem with it. I felt I had a good background in chemistry. Dr. Warner was an excellent teacher. He went on to become dean of the whole college and then president of Carnegie Tech.

BOHNING: After you got your master's degree in 1934 you went to Brown.

WARRICK: Well, Dr. Warner convinced me that I ought to go on for a Ph.D., and he had a good idea. I'm sure he made it possible for me to go to Brown. He must have written and talked with Dr. Charles Kraus at Brown. I went there and found that Dr. Kraus had quite a laboratory. He had some fifteen Ph.D. students or postdocs working for him. When I went to Brown, I looked at that area, and I was pretty sure that I was committed to Kraus. But I wished I had the opportunity to work for W. A. [W. Albert] Noyes, Jr., who was also there. He ran a very fine ship. In fact, he gave all of his people special exams before they were admitted to graduate school, one in each of the disciplines. I recognized that he was a very fine teacher and all of his people were doing well. Now, Kraus's people were doing well, too, but Kraus ran a different kind of a ship. If he got you on board, the chances of your getting out in less than four years were very slim. In fact, I knew one chap there who was in his sixth year. He really got his people tied down. [laughter]

I worked for Kraus for one year. He wanted me to raise the frequency and lower the wavelength of the measurement of the dielectric constant. He was doing a lot of dielectric constant measurements of ionizable materials in solution. That was the time that the peanut tube came out, about the size of the end of my thumb. So I did start on that and I got an oscillator to work. I got a harmonic with a local radio station as a frequency measure, but where I ran into trouble was getting a precision condenser, because as you were at longer wavelengths, you had big precise condensers that you could get reproducible settings on. But now the condenser that I used was a long tube with a rod in it and I could move the rod in and out and get changes in frequency. To get a precise condenser of that type would have taken quite a lot of machinery hours and I never really got a thing that worked. Interestingly enough, after I left, the man who took on the project worked on it for another year and never solved it either, and he went on to another project.

About the end of that year, when I was finishing at Brown, I decided I had to get out and get a job. My father had remarried and the woman had a daughter who was about to go to college, so I felt that they probably needed some help. I tried to get a job. I wrote letters from Brown to fifty of the major chemical companies, and never got an answer from any one of them. That was in 1935. On the way home, I took a bus into Philadelphia and visited Atlantic Richfield, which was one of the companies I had written. I managed to get an interview. The personnel man took me into a series of offices and left me sitting on a bench and said he'd be back. Pretty soon people were starting to leave. I looked at my watch. It was five o'clock, and I said, "Where's so-and-so?" and he said, "Oh, he left." [laughter] Basically that was the kind of interview you got in 1935. I just went back to the bus station and got my bus back to Pittsburgh. So there were no real jobs available either. But I did make contact with Mellon Institute and Dr. W. A. Hammer, who was head of personnel at Mellon Institute at the time, and he was interested.

BOHNING: Had you written to Mellon?

WARRICK: No, I hadn't. I hadn't recognized that they would be a potential place, but after I got to know Dr. Hammer and the place, I found out that that was probably a better place to apply than almost any company. At that time they had well over four hundred chemists working there on about seventy-five or eighty fellowships. It was at its peak, more or less, of growth as a research institute. A job did open up on the so-called sulfur fellowship and they wanted a chemical engineer. My training in engineering wasn't all that great, but I tried for it. I even went to the library and looked up some things and even drew up tentative plans for a pilot plant for handling liquid sulfur. And I didn't get the job. A friend of mine did. [laughter] So I thought, "Oh, well."

Later on, a temporary job opened up for two months in the summer and I was glad to take that. It was working for Dr. Rob Roy McGregor and a small glass company, the MacBeth-Evans Glass Company. We melted glass in little platinum dishes in a great big glow bar furnace. This was in the summertime and we were on the third floor. The temperature in that room was 92° to 95° all the time, and here we were running a 3000°F oven and taking out a half dozen of these little dishes. I learned a lot about glass. We were working on opal glass and studying a triaxial composition diagram of where the best opals were.

In the fall of 1936 Corning bought the MacBeth-Evans Glass Company, and we had a new boss. Dr. McGregor went to Corning to see what problems they might want us to work on because we'd been in close contact with the MacBeth-Evans Company. They were interested in opals and that's why we were doing that. But now, with the new arrangement, we didn't know what they would want. He came back with a whole bunch of glass blocks. He said, "These blocks have a coating on the outside," and it looked like it was a plastic coating with some grains of sand. "The masons don't like these because when they put the mortar on, the coating skids right off the block and the masonry doesn't adhere." We tested, and sure enough, you could put water on the coating and it just dropped off. We thought, "Wow, that wouldn't be good."

We didn't know too much about the coating. It developed that the coating had been suggested by Dr. Frank Hyde of Corning because years before that, in the middle of the Depression, when the RCA building went up in New York City, Corning had the job of making giant blocks of glass, about thirty inches by twenty inches and about a foot deep. The window over the main door in the RCA building is a glass mural. If you go inside you can see it. The question was how to put stuff between these blocks, because obviously they need something. Well, Frank had suggested a vinyl acetate, vinyl chloride copolymer with monomer mixed in, and then laid in there as a slab. As the monomer cured, there was no change in volume. The whole thing stayed that way. That window is still there. But that was the coating that he suggested to put on glass block. Well, what he didn't recognize right away was that it doesn't adhere to glass. [laughter]

We didn't know much about adhesion then. We wrote to various paint companies and resins companies and got some ninety-six different materials. We put them on glass and dropped them in water and sure enough, every last one of them floated off. We thought, "Damn, that's awful." So, friends of ours from Union Carbide came down with a gallon of ethyl silicate. We partially hydrolyzed it and found that it was nice and sticky and it really stuck to glass. But if it was hydrolyzed more or less completely and put on glass, it was sticky for a while and then it finally dried up to flakes and just flaked off. So we said, "Well, we'll only take it part way." We only put enough water in to hydrolyze it part way. We painted it on and then painted this vinyl acetate copolymer over the top, and stuck it in the oven and baked it. By golly, it worked! It wouldn't come off in water. It was really tight. Well, we went through an awful lot of testing and I made thousands of these crazy little concrete dumbbells with glass strips stuck in them. We'd even pull them apart to make the measurement. But we proved to ourselves that we could make a coating like that. We finally mixed the partially hydrolyzed ethyl silicate in with the vinyl acetate and patented it (1).

That was the coating that solved the problem. Put it on the block and it would adhere and they could put sand on it, and the mortar would stay on, and everybody was happy. About five years after that, we went through the Port Allegheny plant of Pittsburgh Corning, which was spun off to make these glass blocks, and sure enough they were still using that same coating. However, now instead of putting it on from solvent, they put it on hot and still had the ethyl silicate in it. We got finished with that project and we're sitting around.

BOHNING: What year was that?

WARRICK: That was about the beginning of 1938.

BOHNING: And McGregor first went to Corning in 1936?

WARRICK: The fall of 1936. And we moved into the new Mellon Institute in 1937. We had this great big, new laboratory, and we had a wonderful time.

BOHNING: And Corning continued the fellowship?

WARRICK: Corning continued the fellowship. We got to looking at each other and said, "You know, what if you put something on the ethyl silicate that wouldn't hydrolyze off?" So we went down to the library and looked around. Sure enough, there were a lot of papers there by Frederick Stanley Kipping in England. He had put a lot of things on with the Grignard reaction and they didn't hydrolyze off. Of course, he wasn't interested in that. As he said, he got a lot of uninviting glues. [laughter] He just wasn't interested in those things. So we started to make Grignards very quickly and sure enough, we made things that wouldn't hydrolyze off. Well, that started off a whole series of processes. We worked with regular ether Grignards for a long time, until we found a paper by [K. A.] Andrianov, a Russian, whereby he showed that you didn't need ether if you used ethyl silicate (2). You could run a Grignard in ethyl silicate and it would not only run, but it would react with the ethyl silicate. He claimed that he could only make mono-materials by that method. So we started up making that kind of stuff. We found that we made mono-, di-, triderivatives. We were making ethyl, butyl, amyl, and we said, "Why don't we try to make methyl?" Of course, it was a little difficult to get a hold of methyl, because methyl bromide is a gas and so is methyl chloride. But we borrowed a small bomb from the Gulf people and transferred in some methyl bromide. We took turns shaking this Grignard and the darn thing warmed so we knew it would work. Then we had a big bomb built, about a gallon or two gallons.

[END OF TAPE, SIDE 1]

WARRICK: From that point on we could make Grignards pretty well. We used this improvised bomb, with bomb heaters around it so we could distill things out of it, and then we stirred it from a drill press at the top. My friend John Speier down the street still has the drill press in his basement. [laughter]

BOHNING: Was Corning aware of this work?

WARRICK: Well, we got started on it in January of 1938. Mr. [George] MacBeth, who had been the MacBeth of the MacBeth-Evans Glass Company, was, I think, one of the reasons they bought MacBeth-Evans, because he was a fine financial man. One time he was going back to Corning and McGregor was on the same train. He said, "You know, I think they're doing something the same way up there with Frank Hyde that you're doing. You'd better get in touch with somebody." So lo and behold, we did make arrangements to meet with Frank Hyde in April of 1938, and sure enough we were doing pretty much the same thing. At that point we drew up a long agenda for the two groups and pretty much divided the field, in that he was already working in the aromatic field and we'd been working in the aliphatic field. So we kept on working in the aliphatic field and he kept on working in the aromatic field. This is about the way it continued for quite some time.

We finally got a pilot plant in 1940. By that time we had shown that dimethyl was the thing that we wanted to make. We'd made some dimethyl fluids and had shown their interesting properties. Lots of people wanted to try them. But we could only make a pound at a time, so Corning decided to support a pilot plant for at least one year. This is amazing. For six thousand dollars they got the equipment, and the man, and the supplies, and the overhead for Mellon Institute. They spent fifteen hundred dollars on the equipment, and his salary was fifteen hundred dollars for the year. Supplies were a little, and ten percent of the six thousand was overhead for Mellon Institute. So that's all that went into that first year. It's a little hard to believe anymore, considering today's dollar value. [laughter] We put up a fifteen-gallon kettle that was able to take pressure of about a hundred and fifty pounds, and a small column about ten feet high packed with carbon rings, and receivers and so on. From that point on, we made quite a lot of dimethyl fluid.

At one point Corning asked if we would make some phenyl ethyl, which is what Frank Hyde had been making for his resin work. We bought some additional equipment from the Rubber Reserve as they began to phase out of their work. It was a big hundred-gallon kettle that didn't have a jacket on it. We just reacted the things in it. We did try it. I can remember helping Ed Mease, who was our chem engineer pilot plant man. We made the original Grignard in the kettle and then pumped it up into another fifty-five-gallon drum above the kettle. However, the Grignard we made was an etherless Grignard of phenyl Grignard in xylene which is not a normal Grignard, but it will run. It was a Dow Chemical patent that we were following. When it got up there, however, it began to cool. The darn stuff thickened and we couldn't drip it back into the kettle. So here we were, on Christmas Eve, and Ed Mease and I were ladling this damn stuff out in the open air and just dripping it out into the kettle. We finally did get it all in because the kettle had silicon tet in it now and we were trying to put phenol Grignard on it.

Well, it soon developed that Mellon Institute was not the place to run moderate-scale reactions. We got away with our first work because it was done without using chlorides. But when we started to use chlorides, we had to filter out the salt. The filter press leaked and in order to stop the choking from the HCl that was around, we put out great big beakers full of ammonia. Well, of course that only accentuated it. Now we had ammonium chloride in here! [laughter] So it was a mess. We decided that Mellon Institute was not the place to conduct this experiment.

Frank Hyde had suggested to Corning that they should contact Dow Chemical, because Dow had the engineering know-how to make this thing go. This was about the summer of 1942 when contact was made with Dow Chemical, and they began to work more or less right away on it. Dr. E. [Edgar] C. Britton was the man who was put in charge to begin with. He wrote to us and asked for the directions for our etherless Grignard. He did some work on that although he did most of the work on Frank's regular Grignards which were phenyl and ethyl, to put them on silicon to make a phenyl, ethyl material which later was resinified. This was the resin which was ultimately to go on to motors for the Navy.

All behind the scenes, things had been going on in Corning. Corning didn't know what to do with this kind of field, because it was organic and they had been in the inorganic field all their lives. At one point Frank Hyde had worked for the so-called fiberglass division of Corning, before Owens-Corning Fiberglas was formed. A man by the name of [Les] Morrow was head of that, and he invited General Electric to come down and see what Frank had done. Frank didn't know any differently, so he told them exactly what he'd done. I think Morrow and maybe some of the others at Corning had the idea that GE, being interested in resins, might say, "Well, why don't we get together and work on this." No. They just took the information back home and started immediately to work on it. They put a man by the name of [Eugene] Rochow on it, and of course we got into interference later.

But the effort with Corning and Dow was a joint effort and it did go forward. Behind the scenes Corning then had formed Owens-Corning and the man who headed up Owens-Corning was a man by the name of [Harold] Boeschenstein. He tried to interest the Navy in electric motor insulation. The real problem with the glass fibers and glass tapes that they made was that they needed a resin to impregnate this glass. All organic resins up to that point were not as heat resistant as the glass itself. So they said they wanted something that would go into this and Frank's resin was that thing. However, Boeschenstein tried to get the Navy interested in it. He talked to Admiral, then Commander, [Hyman G.] Rickover. Rickover was interested when he saw Frank's tape. As a result of his interest, the formation of Dow Corning went ahead. They held a meeting in Detroit, where three people from Corning and three people from Dow went down to talk about the possibility of forming a joint venture. They came away from that saying, "Yes." Merely a handshake. "Yes, we're going to go ahead."

I think Boeschenstein was hopeful that somehow the fiberglass company could be involved in it, but the fiberglass company was only involved during the first year or so as a sales agent for the resin, because they wanted to sell tape. But, from that point on Dow Corning was formed and took over sales of its own product.

At one point Owens-Corning invited Commander Rickover to come to Midland to see the making of this resin. The plane was grounded in Detroit, so we sent a car down for him. By this time a little bit of Dow Corning had been worked on so we had a sales manager and a few other people. They had arranged a luncheon out here at the country club and when they pulled up in front of the club and were going to get out for lunch, Rickover said he wasn't going to get out for lunch. He wanted to see the resins. So they had to take him back to the plant. [laughter] And they had to arrange for the country club to fix up sandwiches that they could take down to the plant and eat while he saw everything. He just wasn't about to get out and have any fancy lunch. He wanted to get down to business. So on the way back to Detroit, he asked them what they needed to make this resin. And they said magnesium and a number of other things. So he gave them the order for twenty-five motors, and grease and paint for the motors. That was the priority they needed to build a plant, because in those days during the war (this was the summer of

1942) we needed priorities to get anything. In fact, we couldn't get new things. There was a used property list that the Navy had, and we bought a lot of stuff off of that list for our plant. If you happen to go by Dow Corning, the water tower there was forty-two years old when we bought it in 1942. It was old at the time it was put up and painted, and it's been used ever since. There were many things like that. We couldn't get a brand new boiler. Later on some of that changed. But nonetheless, it was a little bit of trouble to get started in those days when priorities were not obtainable and things were a little scarce.

The company was formally formed in February of 1943. Formal papers were signed and things went ahead. At that point they had already drawn up plans for the plant on the site where it is now and they started to build. Original authorization was for something like seven hundred and forty thousand. Before they got finished with it, it had to be doubled and then it had to go up to two million one because prices were going up, and things were inflating even then. Meanwhile Willard Dow got into the picture during the formation of the company and had appointed W. [William] R. Collings to take over the whole project.

Bill Collings had headed up the cellulose products group in Dow. That group had its own sales, its own technical development and research, just every element. It had an engineering group of its own. It was like a small company within a company. About seventy-five people were involved at that time. They had been making ethyl cellulose and they were on their way to making methocel. Actually, they'd been doing things like making records and looking at the possibility of making dictation belts. That's when belts were still available and useful.

Collings then stepped right in and even though Britton continued to work on the so-called Hyde resin, Collings and his people started to work on our so-called etherless Grignard process for making dimethyl. They jumped right into using a six-hundred-gallon kettle, where we'd been working with fifteen. This is an interesting story. We had changed the people that were involved with us on the pilot plant, and the new man was John Goodwin. He came up to show them how to run it. Here was this great big six-hundred-gallon kettle and he knew how difficult it was to start these things. They didn't start easily. We always had to put warm water in the kettle and heat up the outside just to try and gently get it started. Well, he gave instructions to run warm water through the kettle. Of course, they had operators to run it around-the-clock. So he gave instructions because they hadn't started it when he left around nine o'clock at night. He said, "Don't change it. Just keep that warm water on it and keep stirring." And I guess he may have added some more methyl bromide. They used that as the charging agent whereas methyl chloride finally was the thing that we were using. But he came in the next morning at six and found that the operator had cold water running in the kettle. He said, "Oh, what's happened?" "Well," the operator said, "Dr. Collings came through here and said to put cold water in. He was afraid that it was getting too high a pressure." Well, there may have been a little pressure from the methyl bromide, of course, but not much. John said, "But I told you to keep warm water on it." "Yeah," he said, "But, Sonny, you know what Dr. Collings does. If he told me to move that wall six feet, I'd move that wall six feet." [laughter] So that was the kind of experience he had with Dr. Collings early on.

Bill Collings is a very personable leader. He did everything himself, so to speak. There are many, many stories about him. In fact, the company just published a booklet about him (3). It contains stories that were submitted by various people who knew him that showed various parts of his character. But he got things done, and he was the right kind of person to run Dow Corning at a time when it was getting under way because it needed somebody to force it through and get it going. He was vice president and general manager. The president of Dow Corning was Dr. E. [Eugene] C. Sullivan, who was director of research and a vice president at Corning Glass. We knew Dr. Sullivan. He was a fine gentleman, very definitely a gentleman of the old school. He was very formal and very dignified. He never told jokes or anything of that sort. Everything was very serious with him. He was the president for a number of years and Bill Collings was the vice president.

BOHNING: Sullivan was at Corning when Hyde had started his work. You and McGregor were doing your work at Mellon. Now, in 1943, let's say, as the company was getting under way, were you still doing work at Mellon?

WARRICK: Yes. We continued to work at Mellon and Hyde continued to work at Corning. In December of 1942, we held our first research conference in Midland when people from Corning and people from Mellon came up. From that point on, we held research conferences every three or four months, something like that. It was not on a real regular basis, but every so often. This was good, because it brought the groups together, and they had a chance to exchange information. At the first one that we held, they tried to record our proceedings on these ethyl cellulose records. They played them back for us and most of us didn't recognize our voices. Somehow your voice records at a lower level than you recognize as you speak. On Dr. McGregor's there was a funny tick, tick, tick, tick in the background and we couldn't understand what it was. Then all of a sudden we recognized it. He was emphasizing his points on the blackboard with the chalk! [laughter] There was a very nice ticking sound in the background.

BOHNING: So you were coming to Midland then on a regular basis?

WARRICK: Yes, on a fairly regular basis. We were doing this by train in those days. We'd catch a train out of Pittsburgh and get to Detroit by morning. We'd get off and have breakfast, and then get on a two-car train or maybe three-car at the most, and sometimes only one, up to Midland. It was a slow trip, really, because it stopped everywhere. But we finally made it. And going out was the same way. I can remember the first time we left, we picked up the train down behind the courthouse here, and got on board the last car. The heat in that car was a wood stove that sat in the corner. [laughter]

BOHNING: At the time you were doing all of this, you managed to get an Sc.D. at the same time. Can we talk about that, because you actually did a separate thesis on another kinetic study.

WARRICK: Yes. It was another kinetic study. One thing about the Mellon Institute, they allowed you to take outside courses. I took a few courses at Pitt. But finally, again, I was talking with Dr. Warner and he said, "Why don't you come over here and do your work and take a thesis here?" I finally had to get some special arrangements so they'd allow me to do that. Normally they didn't do that. But they finally did accept me as a graduate student on a part-time basis.

In 1936 I started my thesis work at Carnegie Tech under [James] Paul Fugassi. We started out with the idea of doing the kinetic study with a lead pot as our constant temperature bath and a controller for that which I rigged up. I had to lay bricks. We ended up with a brick pedestal about three feet square and about three feet high with this lead pot in the middle surrounded by glass wool insulation. All of this was just to try to make a high-temperature thermostat able to run at 200°C to 400°C. The temperature controller was a galvanometer with a moving light beam. However, this was before the days of all the nice new controllers. I put up one photocell so that as long as it was on that photocell it was telling it to shut off the heat. However, if it overshot, then it was on the side where it was telling it to heat again, so that didn't work. We finally gave up on that as a temperature control. I worked on that for a couple of years. Paul Fugassi was a good glass blower. He insisted on blowing a Toepler pump, which is a pump for moving gases at low pressure. You just raise the mercury level and it pumps the gas out and then you lower it and it sucks the gas in and pumps it out again. Well, we went through a number of runs like that and it wasn't going to work. We just finally gave up on that one. So we went over to another system altogether which was much more convenient. We had a smaller thermostat and a glass system and did work on tertiary-butyl propionate decomposition.

I did this work on Saturdays and nights and Sundays. I got married in the fall of 1940. After that Jean and I would go over on a weekend and take readings on the equipment and make runs. I would call out the numbers and she would write them down. We had some interesting times because we had some very fast runs that only took about a minute and a half, so it would be hard to make the readings. But nonetheless, we did that. Of course, during all this work, Paul Fugassi had lots of ideas. We developed an article on the calculation of equilibria and I had to do a lot of calculating. It was a big, long formula based on infrared frequencies and energies as to which bond was going to break and so on. He managed to find an old brass calculator, and I don't know where he found it. It was a solid brass machine, where you turn the crank. I spent one whole winter turning the crank! [laughter] But we got a paper out of it and published it (4). So, in June of 1943 I got my doctor's degree, but working part-time and at nights and Saturdays.

Later on, John Speier, who followed me at Mellon, and who was part of our group, made arrangements to use what he was doing during the day at Mellon on our work as his thesis at Pitt under Dr. [Bernard F.] Daubert and that was fine. It worked out beautifully, and there were others who came and did that kind of thing too.

BOHNING: I was going to say, that in actuality you did two theses at the same time. I wondered whether the proprietary nature of the work at Mellon might have precluded you from using that.

WARRICK: I guess at that point I had thought of that and I was afraid they might not agree. By the time John Speier joined us, it was in the summer of 1943 and he had spent at least a year with us before he got to the point where he wanted to talk about a thesis. There was no thought that it would be too proprietary. He was doing some new things. I guess the company was only interested in delaying publication long enough to apply for the patents.

[END OF TAPE, SIDE 2]

BOHNING: You made the comment in your Goodyear Medal address (5) that you spent quite a bit of time in the early 1940s learning to polymerize and characterize the results. You said that without the sophisticated techniques that we now have available, the whole study was an exciting adventure. You wondered whether some of that adventure is missing from current research.

WARRICK: Well, I often wonder about that. The young people have all these tools at their command. In fact, John Speier and I reminisce about this. John is a little upset because most of the people who work for him work with 5 cc's. They take it up and get an infrared and an NMR and a few other things and they conclude that they've accomplished what they wanted. We never could do that. We had to make a pound or two. And as John said, when he asked one of his men to make a pound or two, he said, "I've never done that." They can't make a pound or two of anything. We had to make pounds of stuff all the time. We were working with twelve-liter flasks, twenty-two-liter flasks. Fortunately we had Glassco heaters and Variacs and that sort of thing. Without that, we wouldn't have done all we did either. We were forced to make quantities of material and then we had to distill the doggone stuff. We had to learn to distill and it wasn't easy. Operating a still is a little bit of an art and not really a science, so that was a little bit of a problem all the way along.

BOHNING: Could we talk a little bit about some of the people you may have interacted with at this time? Then I wanted to look at some of the results of the work here at Dow Corning. Any comments on Frank Hyde?

WARRICK: Yes, we interacted with Frank Hyde regularly. We would go to Corning for meetings, and he also came up here to Midland for meetings. We had a good chance to interact. Frank was a real chemist's chemist. In other words, he was always involved in his work, no question about it. He still is today, as a matter of fact. He comes up to Midland and talks about things, so he's really still interested in what goes on. I think you might enjoy interviewing him sometime. The only problem is Frank's wife has a very severe case of Alzheimer's. He's got a twenty-four-hour-a-day job, really. We were down to see him last winter for a short time, and she just constantly wants to take walks. My wife walked with her three different times. She'd come back from a walk and sit still for five minutes and then want to go again.

We met a number of people from Corning. The other Corning people were Dr. O. Kenneth Johannson, Dr. William Daudt, and Robert Fleming. Those were three of Frank's group. There was a girl there by the name of Mary Purcell. Fleming, Daudt and Johannson came to Midland later when the Corning group moved to Midland. They moved in the early 1950s. The Mellon group partially moved to Midland. Dr. McGregor came in 1955 and John Speier and I came in 1956. We couldn't convince anybody else on the Pittsburgh fellowship to come to Midland, so we continued the fellowship at Mellon Institute for about another six years, with decreasing numbers in it. For a time there were about six people left in it, and then finally we decided to cut off all that outside work. If they wanted to come to Midland that was fine.

If not, we were not going to continue the work at Mellon Institute. This was in the late 1960s.

BOHNING: Shailer Bass.

WARRICK: Shailer Bass was the director of research under Bill Collings. He was quite a man. He was always interested in new things and he did a lot of research. He ultimately had two men working for him, Dr. Melvin J. Hunter and Dr. Arthur J. Barry. They were heads of two different groups of researchers within his bailiwick. And there was another man, T. A. Kauppi. Kauppi was head of product development under Dr. Collings. He was more or less what we call TS & D today, technical service and development. Shailer was really head of development for a time, before he became vice president and then later president of the company.

Shortly after the war ended, he went to Europe on an invitation from the British government to come over and tell them about silicones because, I guess, through war channels they'd been hearing something about silicones. He went over and talked in a number of places, including the British General Electric Company, which has no relationship to ours. It's GEC over there. They were pretty much the moving force behind European, or English interest at any rate. While he was there he did talk to Dr. Kipping, and Kipping wanted to know if there was anything really new in silicones. Shailer was telling him all about these polymers. He said, "No. Anything new in the chemistry." [laughter] Later on, Shailer was able to take back to him a full set of his papers and that was good, because they'd had a fire in his laboratory at Sussex and he'd lost all of his papers. Shailer had copies made of all of his papers and gave him a volume of his collected papers. He was really quite touched by that.

BOHNING: I understand that Britton did that with the people here. Did you get one of those?

WARRICK: Well, we had a number of those volumes made up. Mel Hunter started this and put the Kipping papers in a set of volumes. From that point on we had some other papers and other volumes. We had a set of about four or five volumes, loosely bound, of papers that Dr. Britton said he regarded pretty much as a bible for this thing. However, really all they told us was the Grignard technique. He was looking for optical activity. He was looking for unsymmetrically substituted silicones and how that optical activity related to carbon. Well, that's a viable field, but there was a lot more to it than that. So these books rattled around the lab for a long time. I went down recently to check in the library and I said, "Do you have those books?" And the librarian said, "What books?" I said, "You don't mean to tell me that you don't have these any more?" They don't have any of them any more. I don't know what they did with them.

BOHNING: That's a shame.

WARRICK: I had a set here for a long time and then I finally said, "Well, why don't I give them back to the library and they can keep them?" They must have gotten tired of keeping them, and then just threw the whole bunch out. There's really no one that remembered that there had even been a set of these.

Mel Hunter started something else after that. He started to collect individual papers that people at Dow Corning published. We had a paper for every person that had published. We made numbers of these collected volumes up, and they were thrown out. Again, I'd kept my own, but I didn't have a volume of the collected papers of everybody else.

Dr. Hunter ultimately became director of research, and Art Barry became his associate director. When I moved to Midland in 1956 my title was assistant director of research, so I was part of the group. I had a small group working for me here. The field we worked in was radiation chemistry. We were doing a lot of work in radiation chemistry. We'd learned the effect of electrons and electron beams. We'd done work in Boston using an accelerator that they had. And we'd done some gamma ray work. Dow had a gamma source at that time, and we did some work over there.

BOHNING: Didn't [John] Grebe move into that area?

WARRICK: Yes, he had done work at Dow in that field. I didn't know Grebe except by reputation. I never actually met the man. But I did get to know a number of people who were in charge of that radiation facility because we did give some things some very high doses. We actually sent things down to reactors at General Dynamics. We put things in a reactor down there to see how they'd behave. All of this started from work we did back at Mellon. We did our first radiation in a cyclotron. The man who ran the cyclotron at the University of Pittsburgh asked if we'd like to put something in the beam, and we said, "Sure." So we put some things in, and lo and behold we found that it really knocked the tar out of them. In some lighter doses we found that it could do some vulcanization, so we figured on the worth of vulcanization and that sort of thing, it could be done. Later on we showed how much radiation it would take, and that phenyl-containing materials were more resistant. When I first came up here in 1956 I had four men working with me, and several of them got interested in this field. We developed the G factors for various bonds and various groupings in silicones and that sort of thing, as to how much radiation it would take to do various things.

BOHNING: Could we go back to the beginning of Dow Corning? You were still at Mellon and there were a number of applications that were coming out. Shailer Bass had used your "200 fluid" to work on the grease.

WARRICK: Yes, to make the so-called DC-4 compound, which was the first product that the company had. It was used in the aircraft that flew from this country to England and to North Africa. Planes couldn't fly over water at high altitudes in those days because they were piston-driven rotary engines. The ignition system would be subject to corona at high altitudes and especially over water, and you'd lose ignition and the engine would stop. They knew that, and they wanted to deliver these things because they were running into trouble with the convoys. The convoys were getting shot down. So, they developed this harness, a series of pipes that led out to each spark plug. Each cylinder had two plugs. I've forgotten the exact number of cylinders, but at any rate, this whole network was a hollow tube with an ignition cable going down the center of it. Then they just filled it with this grease from a grease gun because our stuff didn't subject itself to corona. It also resisted moisture very well and kept moisture out. So they found that they could fly. They could stay up only a few minutes prior to that, and after that they were gone. Now with this stuff, they were up eight hours and it didn't make any difference. So they began to fly them.

Then there was a second point about getting the planes over there. On the ground those engines absorbed material in the oil in the crank case and at high altitudes that stuff foamed out and just dropped out of the engine. Now you lost engines from lack of lubrication. Gulf Oil very early found that a small amount of our "200 fluid" in a petroleum product defoamed it. So from that point on, all aircraft and tanks and everything else that had this foaming problem used oil that was defoamed with our "200 fluid." The "200 fluid" was also used in instruments for damping the needle. That was one of the uses during the war. So there were only about three uses during the war, and that was about all.

In the meantime we were working on other things. We were working on laminating resins, work which we had started at Mellon on what we called mono-tri resins. These were resins with only one substitution, and resins with three. A paper by Paul Flory (6) indicated that you could carry a so-called mono-tri resin further towards gelation without gelling it than you could with any other system. So we tried this and we found that we could make some good laminating resins this way. And those resins later became some Dow Corning products. As you make a motor with glass cloth winding and all that, you still need laminates, so-called slot sticks and various other things. Even control panels had to be made out of glass cloth with laminating resins. So one of the resins that was needed was not only this impregnating resin but also a laminating resin. Frank Hyde's resin was used for a long time in the very early electric motors. Westinghouse made most of the motors in the early days, because they worked with us at Mellon. They had a fellowship at Mellon. They made the motors and we tested them up here. We had a testing laboratory up in Midland where full-sized motors were run continuously at various temperatures. We would run them for life tests at 200°C, 225°C, and 250°C and measured the so-called life test of these resins. A chap by the name of George Grant did this for Dow Corning.

BOHNING: Did you do any applications work yourself? I had heard that you used silicon caulking in your house in Pittsburgh.

WARRICK: Yes, I did. I built a house outside Pittsburgh in North Hills in 1948. We put aluminum windows in with the idea that that would cut down maintenance and all the rest of it. About a year afterwards, I began to find that the putty was falling out. I said to the chap that put them in, "What's the matter? This is falling out." He said, "Well, that needed painting." I said, "You mean to tell me I was going to have to paint that doggone stuff?" He said, "Oh, yes." I said, "Well, I thought with aluminum windows you could get a caulking material that I wouldn't have to paint." He said, "Oh, no." I thought, "Well, for heaven's sake, we need a caulk now." So we did a lot of work in the lab, mixing up various things. We did make some caulk, and I put caulk on my windows. Then I made a big four-foot, double-paned window, like thermopanes, and along the front edge of that window I put a bead of this new caulk that I'd made. It had a curing mechanism by painting an amine in solution on the outside. So the skin cured but the inside didn't. For all I know it's still there. This was more or less the beginnings of interest in caulking compound. We actually never did any work at Mellon on the sealants in the caulking compounds, although that was done in Frank Hyde's lab up here.

BOHNING: But you did the original work.

WARRICK: We suggested the idea that caulks were needed and we did make some of these compounds. The one that I put on the window was a chlorine containing one. When you treated it with a diamine, it would react and form a cross-link. It would vulcanize with the addition of a diamine.

Well, my only application work was finding out very early on that the "200 fluid" had this so-called flat slope. Gulf reported back to us that it had what they called a very good viscosity index. So then I got into the business of measuring viscosities over a range of temperatures. Sure enough, the darn thing was very, very flat. We began to exploit that and point out in our literature that that was one of the big benefits of this product. The company published a book about "200 fluid" very early on in which we listed a lot of properties of the material. But truly, most of the applications came from people on the outside saying, "I have this problem." And we would suggest, "Well, why don't you try this." Most of the time, those things worked. But they were not really things that we worked on initially.

BOHNING: Maybe this is the time to move on to the rubber aspect. You said you knew early on that high molecular weights would be the key. How did you know that?

WARRICK: We more or less knew from reading about rubber that high molecular weights were used, and we knew what raw rubber looked like. Every now and again, we'd have a batch of gelled DC-4 compound or "200 fluid" and it was quite rubbery. But nothing was ever done about it until the fall of 1943. Bell Labs published a paper on their Paracon rubber (7). They didn't say anything in that about what the vulcanizing agent was, but we knew it was a rubber that didn't have any unsaturation in it. A friend of ours from across the hall at Mellon said, "Oh, the vulcanizing agent is benzoyl peroxide." I said, "If that works on a saturated rubber for Bell Labs, it ought to work for us." I tried it, and sure enough it gelled our "200 fluid."

So, right away, we took some ordinary "200 fluid," of about a thousand centistoke, and added a bit of benzoyl peroxide, gelled it up to the point where it was now a rubber-like gel, milled it with some more filler, and a little bit more peroxide and put it in a mold and sure enough, we had a rubber. It wasn't very strong. I can remember bringing the first samples up to Andy Kauppi. He crushed it up in his fingers and it was nothing but a powder. I said, "Andy!" [laughter] "You didn't need to do that to tell me that it was weak. I knew it was weak."

We went on from there trying to improve on it. A lot of people got involved in improving the rubber. O. K. Johannson, for instance, was one of them. He developed an alkali process for polymerizing it, and during the course of that polymerization, if they ran it a little too hot, it would gel. A number of the gels, called K-gels, were pretty high-strength materials because they were pretty high molecular weight before they gelled. They became good raw materials for rubber.

In 1950 I had a chap by the name of Silas Braley working for me. One of the fellowships at Mellon Institute had asked us to prepare a thin sheet of rubber, because they had the idea that they could separate oxygen and nitrogen because of the difference in speed of diffusion through that film. An ordinary sheet of unfilled rubber wasn't strong enough. So I said to him, "Take some of that high polymer that we've got out there and add some of that Lindy-A silica that we have out there." We hadn't done anything with that. And he did, milled it in, and molded a real thin sheet. He came back real excited because it was strong. Sure enough, we measured it and it was about four or five times stronger than anything we'd seen up to that point.

We went on from there to show that it needed two things--high molecular weight polymers and a low particle size silica. We looked at silicas. We looked at everything else that was fine. We looked at copper phthalocyanine. We even looked at hemoglobin from blood, because it was a small particle material. Everything that was small did work to some extent, but it turned out that silicas were ideally suited for this. When we showed that high-strength rubber in Midland, they got excited right away. That was the quickest transfer from lab to product of any product that they had ever had. I'd say within two months they had a product out. Of course, because we milled things and molded them right away, we didn't know that if you milled one of these rubbers and left it to stand around before you molded it, it would set up in the can. It did what we called crepe hardening. Pretty soon, even though they started selling, more product was coming back than was going out. The company was really upset. A lot of work went into showing what was needed to get around that. We did work on treating the filler itself to make it less reactive because what was happening was that the filler was reacting with the polymer. Finally Keith Polmanteer up here came upon what we called PA fluid, which was a hydroxy-ended silicone. That worked beautifully. They added that as sort of a plasticizer. From that point on there was no problem with the crepe hardening.

BOHNING: I should ask you about Silly Putty. You did some early polymerization with boric oxide. Was there any reason why you selected boric oxide?

WARRICK: Yes, because we thought, maybe a little mistakenly, that one of the reasons we weren't able to go to high molecular weights was that they were hydroxy-ended. We, of course, read [Wallace] Carothers's work on ester changes, so that we knew what we had to do to make them go high. We thought that since boric oxide is a good dehydrating agent it might pull water out. We put it in, and lo and behold, it went into the chain. It became -Si-O-B-O-Si-. It behaved like a rubber. It bounced and you could mold it like a putty. We didn't know what to do with it, really. We made a lot of it and just kept it on the shelf.

[END OF TAPE, SIDE 3]

WARRICK: I can remember there was a visitor in Mac's office one day. I can't think of his name now, but he was from Sperry Gyroscope. Mac was showing him this and he bounced it on the floor. He said, "Ah, what the hell! What's it good for?" And Mac said, "Well, I'll tell you what it's good for. It's good to draw it out like this, roll it out, bounce it on the floor. It's good to make people say, 'What the hell is it good for?'" [laughter] So that was it. That was about all we did with it.

BOHNING: You did have a patent on it though (8).

WARRICK: Yes, we did have a patent on it. It's long since expired but we applied for a patent on it. The company apparently gave out samples to various salesmen. One salesman managed to sell it to this chap with an enterprising idea of putting it inside an egg and selling it. He must be a millionaire well over.

BOHNING: Do you know where the name Silly Putty originated?

WARRICK: I think he did it. We called it bouncing putty, but he called it Silly Putty.

BOHNING: I also came across the term rheopexy. Is that a standard rubber term?

WARRICK: That's a rubber term for things that behave like that, that behave like solids on sharp blows, but like fluids on long-term pressure.

BOHNING: All during this period, were you aware of what GE was doing?

WARRICK: To some extent. Near the end of the war, we were thrown into interference with General Electric Company. We had to give testimony at our local lawyer's, a deposition in front of a GE lawyer, on various things. Frank Hyde did the same thing, and I'm sure that also occurred at General Electric. The upshot of the whole thing was that the two companies decided that we had so many patents that were overlapping, or that were in fields of each other's, that neither one would be able to operate without license from the other. So there was a general agreement made that we would license them and they would license us over the first four- or five-year period, over which those patents up to that point would be licensed to each company. And from that point on, we would go on our own ways. So we did know what they did.

I went to the Gibson Island conference in the summer of 1945. Rochow was there, and he gave a talk. He actually demonstrated passing methyl chloride over heated silica and getting silicone chlorides out the other end. I gave a talk about cyclics and linears in the dimethyl system. Dr. Bass gave a talk on the general field of silicones, and Dr. Hyde gave a talk. So there were three of us there from Dow Corning. That was the first we had known of Rochow's work on the so-called direct process. It was shortly thereafter that we were thrown into interference. We were fortunately able to license that work and go back and forth. Otherwise we might have had trouble.

BOHNING: In 1948 Hyde developed a controlled alkaline polymerization and you worked on the acid type.

WARRICK: Right. We worked on sulfuric acid type polymerizations. Dr. McGregor did some of the early work, and then I did some subsequent work. I worked with fuming acids where we could take cyclics. We were intrigued with the idea that we had seen these other demonstrations of very rapid polymerizations, that you could stir it once and pick it up. Well, it's possible to do that with a silicone, but it takes fuming sulfuric acid.

At one of these early research conferences, McGregor was demonstrating sulfuric acid polymerizations, and he added the acid and stirred and stirred and stirred, and it just never got any thicker. What had happened was that Dr. Bass had added a little tri-substituted material so that it end-blocked it and it wouldn't go any higher. That was the end of it. So, he said, "No more. If we ever go to demonstrate anything, we're going to bring all our own stuff." When I went to demonstrate this high-speed acid polymerization, I brought up little vials of fuming sulfuric acid and a vial of water and everything else. We went out on the driveway outside and mixed the tube and broke it, so that we could come in and make the test afterwards. We demonstrated that we could make a five-second polymerization of "200 fluid."

The acid polymerization has been used, because it turns out to be desirable in certain polymers, especially in those that are useful in food defoaming. Antifoam A that we sell is for organic defoaming. It was originally designed for removal of a monomer from a polymer, because Du Pont found that it worked. We thought they didn't know what they were doing, so we sent a whole slew of things, plus a little bit of the same old sample, under a different number. The only one that worked was the old stuff. So we finally figured out they knew what they were talking about, whereas we didn't really know what it was about that polymer. But later it developed that it was traces of the acid left in it that made the thing work. We eventually used acid polymerization as well as alkaline polymerization. Alkali polymerization is used in the plant largely for rubber now. But there are some cases where you do use acid to polymerize things.

BOHNING: From 1950 on, it was primarily a study of the basic properties of polymers.

WARRICK: Yes. I did an awful lot of work on the fundamentals of rubber. We managed to get a machine which was like a small Instron, before the Instrons were really developed. We measured stress under various conditions. I started to make measurements of crystallinity in silicone rubber, because I was fascinated by one thing on this machine. I cooled a sample down after it'd been stretched. With ordinary rubber, when you cool a sample down, and you start to crystallize, the stress drops off. In our rubber, when you came down and it started to crystallize, the stress went back up. Boy, that had me confused for quite a long time. But it turned out to be true. Then we went downstairs to the people at Mellon in the x-ray department and had them measure some x-ray measurements of crystallinity. We did some dilatometric measurements of density as well. All of this was done with the idea that we'd try to check these various measurements against the same point. It surely worked out that way, and as a result we have a couple of papers in the Journal of Polymer Science on crystallinity in our rubber (9). No one has ever challenged the fact that it rises because I think we demonstrated it quite effectively.

BOHNING: You've made the statement that the technology in a new field almost always precedes the science, not as we're accustomed to think of it as research and development.

WARRICK: That's right. And that startled me, but you know, even Goodyear knew how to vulcanize his rubber before he knew what he was doing. We knew a lot about rubber before we developed the science of the rubber, the crystallinity, and all the rest of it. A lot was done and known before you got down to the fine point. I think I said something else about the fact that the time interval between the technology and the science gets shorter. The time from Goodyear until the science of rubber was sixty years or so. In our case it was about five or six years at the most. So I think things are moving faster. It may be that science and technology are moving together more closely today.

BOHNING: I also wanted to ask you to comment on McGregor. You worked very closely with him for a long period of time.

WARRICK: He was a fine boss. He was an ideal boss in the sense that he encouraged you to do everything you could. He didn't criticize and he didn't hold you back from doing anything. In fact, John Speier and I talk about it. He's been our favorite boss. There's no question about that, because he did keep an open mind. Whatever you felt you had to do, you had to do. I can only recall one time when he was a little upset with me. It was June 22, 1944. A group from Corning and a group from Midland were coming to Mellon to talk about rubber. We all knew this. We were all scheduled for that morning. And so everybody assembled, and they were all there except me. And I walked in at ten o'clock. Boy, I could just see Mac's hair rising. Here I walk in at ten o'clock on a meeting that started at eight. He wanted to know where I was. I said, "I'm sorry, but I had to attend--my wife gave birth to our first child." "Oh." Well, all was forgiven. [laughter]

BOHNING: After you started the silicone work, did McGregor do anything else, or were all his efforts directed toward silicones?

WARRICK: We were all working on silicones. Mac did an awful lot of contact work outside the lab. He knew the fellow at Gulf who worked on the defoaming. He knew the people at Westinghouse. There was a lot of outside contact work that he did. He did some traveling, too, to various places to talk about silicones and show samples and get cooperative work.

One of the very early things was that Corning put in the pilot plant basically so we could enter into work with people. Mine Safety Appliance in Pittsburgh wanted a lubricant for an oxygen compressor. We knew that that was a pretty dangerous operation. We took some of our fluid and put it on a piece of glass cloth. We put it in a big bomb and stood back and locked it up, put a gauge on it, and measured the pressure. We took that bomb up to 200°C with pure oxygen in the bomb, and the pressure didn't change. It just went up and stayed there. We drained the gas off through a beaker of barium hydroxide so that if any oxidation had occurred, we'd measure the CO_2. There wasn't any. We took it out and looked at it and sure enough, the fluid was still there intact. No problem. So we assumed it was all right. For a time we did sell some gallons of fluid to Mine Safety and they used it in the pump. It worked all right. The only reason they dropped the whole project was that our dimethyl fluid is not a good lubricant and it eroded the pumps. The pumps wouldn't work well after a while. If anything, our "200 fluid" is an antilubricant. It's a material that wears and you have to have additives to keep it from wearing, or you have to build in different compositions. That was one of the very earliest applications we worked on, high-pressure oxygen pumps.

BOHNING: You came to Dow Corning in 1956 and McGregor came the year before.

WARRICK: Yes, he came the year before and he more or less started to answer inquiries that had been coming in from doctors and surgeons all over the world about what you could do with the silicones. One of the first applications that was successful was the so-called hydrocephalus valve, a Holter valve. That is a small piece of silicone rubber tubing with a stainless steel valve and it was worked on by a man by the name of [John] Holter, whose daughter had hydrocephalus. The surgeons install it in the back of the neck. The one tube goes up to the brain and the other tube goes into the abdominal cavity and the fluid is drained off. It saves the life of the child and it's left in. It's just left in for the life of the child. I don't know how many have been installed. The last I heard was well over six hundred thousand.

BOHNING: My goodness.

WARRICK: McGregor started answering this correspondence, and by 1959 they formed the so-called Center for the Aid to Medical Research, which had a formal publication and worked with doctors and surgeons all over the world. Mac did an awful lot of work in that field until he himself got Parkinson's disease. He died in 1965. In the last few years he was in pretty bad shape.

BOHNING: Was he responsible for your coming out to Midland or was it the company?

WARRICK: I think we talked about coming out with Dr. Bass in 1955. We decided to do it in phases and I stayed on at Mellon to run the fellowship for a time and then after I came out, I was the liaison back to the fellowship at Mellon. I came out here as assistant director of research and had a small group of four men working on polymer and mostly radiation chemistry at that time.

BOHNING: Were you still doing lab work yourself during that period?

WARRICK: No. By that time I had been getting to the point where I was doing mostly library work and directing the work of the others, writing reports and that sort of thing. It had been the same at Mellon, too. I had a number of people working with me and they did the work. We had a number of people come from here. That was interesting too. Young people who wanted to finish up even a bachelor's degree, came down to work with us for a few years at Mellon and finished up their degree at the University of Pittsburgh. Shailer Bass even sent two of his sons, Dolph Bass and Harlan Bass. They were with us for a year and a half or so and then came back. Another one, Forrest Stark, came down from here and worked with us for a time. He then went to Penn State, because at that time we had worked with Leo Sommer at Penn State and had fellowships with him. We sent people to Penn State to get their Ph.D. I would say four that I know of got their Ph.D. at Penn State under Leo and our sponsorship and then came back to Dow Corning.

BOHNING: Another of your coworkers was John Goodwin.

WARRICK: John Goodwin was the man who ran our pilot plant in Mellon and then he came up here fairly early on. He did some work on organic alkyd resins. It was a time at which these alkyd resins, using aromatic dibasic acids, were pretty popular, and he was able to show that they were pretty good. He tried to interest the company in working in and selling alkyd resins, but the company decided they were only going to stick to those things that contain silicone. I think that since his things didn't contain silicone, he didn't follow through on them. He did some work on that and continued to work here.

BOHNING: In the period from 1956 to 1959 you were primarily involved with the radiation work.

WARRICK: Right.

BOHNING: Then in 1959 you moved into something quite different.

WARRICK: Yes, I did. I moved into silicon. I went to the West Coast because Corning suggested it. Corning had been making the optics for the Falcon missile. The optics that Corning made were pure quartz, but the Hughes Aircraft Company wanted something that gave better transmission than quartz. They knew one thing that did that, and that's silicon itself. So I went out to talk to Hughes about this, and they said, "Yes, they would like to do that." They knew of a man who was making the raw silicon for these optics. The only thing was that he was such a small operator that they didn't trust him. If we would go up and see what he was doing and maybe take some of those ideas back and develop it, they would be interested.

I was only there for one day and we went right up to San Francisco and I met Dean Knapic in Palo Alto. Now Dean Knapic was sort of a throwback. One of the men from Bell Labs who invented the transistor, [William] Shockley, went to the west coast in the late 1940s, and started up a laboratory for the development of the transistors and silicon devices. Knapic was included in that group, but there were others. From that group various spin-offs came about, including Fairchild Semiconductor. A few years ago, the chart of the hierarchy of this was a really complex thing, because somebody would leave somebody and start another company. It was just wild. People were making money hand over fist, just leaving companies and starting other companies.

Dean Knapic had been in this Shockley laboratory and had learned about silicon crystal growing. So he started to make a crystal grower of his own. It was designed to grow big crystals, although he never actually grew a big single crystal. Most of his were polycrystalline material. When I went to see him, he showed me his crystal grower and he told me all about silicon. I spent about four hours with him altogether and he so filled me up with silicon technology that on the way back on the airplane from California, I wrote continuously for five hours in order to have a report to give Dr. Collings when I got back. I gave the report when I got back in his office. I couldn't believe that I'd learned all that much. It sort of just poured out! Well, the upshot of it was we tried to get in the business of making these crystals. We made a deal with Knapic to build us a crystal grower and we sent a man out there to work with him and get a crystal. By the time we got crystals made and had Perkin Elmer grind out some optics and took them back to Hughes, Hughes had already developed another method. It was a heat forming method, a casting method. So we never went ahead with that process. In fact, we got the crystal grower here and grew a couple of crystals and pretty much junked the operation. We decided against that.

At that point, Dr. Collings put me in charge of whatever we were going to do in silicon. Shailer had been in contact with Westinghouse and they had wanted us to take a so-called Siemens-Westinghouse license. The upshot of that was that we did take a Siemens-Westinghouse license. In May of 1959, four of us went to Germany to see this and they gave us a very good story. We went to Nuremberg and the little town of Pretzfeld near there. We went to West Berlin and saw operations there. By the time we came back we had a pretty good idea of what they did and how they did it. So we finally did sign a Siemens license and went on from there.

We started operations. Dr. Collings thought we ought to have a clean area, and we felt that was important. They bought about six hundred acres down south of us at a place called Hemlock, fifteen miles south of Midland. It was in the woods. We wanted to be far enough away from road traffic, because we had tried to grow some crystals inside the plant down here and the traffic on the road would cause the vibration of the melt and it wouldn't be good for the crystal growth. In the spring of 1960 we put up a pilot plant down there, a small concrete building. As the equipment from Germany started arriving, we put it in there and started operating out of there for a year while they were finishing up the main plant. By May of 1961 we finally moved into the full plant and began operations. That was not a straightforward thing but it grew rapidly. It was always a terrific challenge. I can remember developing business plans and taking them in, and two or three months later I'd have to go in to the Dow Corning management and say, "You know the price of silicon now is below what we forecast for the end of the year. What can we do?" So we had to revise our business plan time and time again because of the rapid growth or drop in silicon prices.

We did do some very interesting work down there and developed some unique ways of selling silicon. Silicon was sold before in just chunks. We finally sold it in crucible size charges that would fit directly in a crucible and could be used to grow a specific size crystal. In those days, people were talking about crucible charges that were a couple of inches in diameter. But the last crucible charges that I saw we were selling were six and a half inches or so. Finally they got so big that the pressure on the crucible at that point was too much and it would break the crucible. We finally had to break it up into chunks. Now they don't do it that way at all. They sell the whole rod. They just take it right out of the reactor and package it up in plastic and send it right off to the company. The grower then breaks it up himself. We were very much concerned about impurities. We ran all over the plant trying to find gremlins at times.

[END OF TAPE, SIDE 4]

WARRICK: That was an exciting time, and we got into the business of making a single crystal by zone refining. We bought these zone refiners from Germany, and had to teach young farm boys to run them. But you know, some of them were really fine operators because farm boys understand more about machinery than the average person. We had great success with that.

Then we had to learn to polish the darn crystals and cut them. It got to be quite a mess. About 1968 I was asked to run the New Projects Business and they replaced me with Bill [William E.] Lowery, who had made the body for the modified Mustangs at Ionia, Michigan. He was an expert in plant operations. One of his first moves was to stop all crystal growing and get us out of the single crystal business, out of the slice business, and concentrate solely on the polycrystal business. That was probably a good move, but we had to scrap a lot of equipment and sell a lot of equipment for junk. I can remember one of the stories that went around. We were selling these zone refiners that cost us $50,000 a piece or something like that. Some woman bought one for $25 for her husband. We asked her what he was going to do with it. She said she didn't know, but he liked equipment. [laughter] It was sort of ridiculous. The plant has moved ahead down there now and has gone into wholly new ways of making silicon. They've speeded up the process and improved the process.

We used hydrogen, of course, as the carrier and we used it out of cylinders for a time. We could see that wasn't going to work. We bought a Lurgi cell from Germany. That was a great big electrolytic cell that gave us hydrogen and oxygen. For a time that was okay. We'd charge that into a field of tanks at the back, but then that was going to be too little. We could see that we were growing faster than that needed. About this time the aerospace project came up with liquid hydrogen. We could buy liquid hydrogen. From that point on we installed a cryostat ball down at Hemlock, and we've been using liquid hydrogen ever since. However, even after that, we went on to the process of cleaning up the hydrogen and recycling it so we wouldn't have to use all new material, because that was pretty expensive. We had an engineering firm design the condensers and all the pressure equipment to make it possible to condense everything out of that stream and keep the hydrogen clean and feed it back to the process. This was quite a step, because purity is so important. If you recycle something and you've got an impurity in it, you're in real trouble. Now they do recycle and they do much better, and they've got higher yields and faster turnaround.

We were the largest user of electricity in about a four-county area down there. They had to install extra lines and bring in more power packs because we were using current like it was going out of style. Now they do the same thing with less current because it's more efficient. The unit that is making silicon for the company now has been set aside as a wholly-owned subsidiary. However, it was wholly-owned only for a short time. Two Japanese firms bought partial ownership. So we have two Japanese partners, and they buy from us. This is one of the outstanding efforts in making silicon in the world. It is really a big operation. They turn it out by the ton.

BOHNING: You mentioned that you went to this New Products Business in 1968.

WARRICK: What they wanted to do was formalize, under a business structure, turning ideas that had been floating around the company for a long time into new business efforts. One of our first efforts along that line was to take what had been worked on but had not been really developed--the formation of a so-called foam-filled tire. We developed a process to fill a tire with foam and then drive on it and not have any air in the tire at all. We had cars going back and forth to Detroit regularly that had no air in them, just foam. Originally it was a silicone foam, and then we learned that we could make the right combination of other polymers and get the same hysteresis curve, and therefore the lack of build-up of heat.

We developed processes and got patents on it. We started to license that process and we licensed it to Goodyear and to a man down in North Carolina who was in the business of making recaps. He could see this, and it was a good move, as an off-the-road tire for farm equipment and big equipment. In fact, he made a couple of tires for one of the machines at Dow that worked in their so-called junkyard, where they take equipment that they're junking and break it up. The vehicles there were always getting punctures from sharp pieces of metal or bolts or whatever. They were out of service all the time. So we got them a set of tires that had foam in them, and they didn't have any trouble. Well, I really don't know how well this has gone, but I think for off-the-road vehicles it worked out pretty well. But it never has developed for passenger cars, although in this New Projects Business we went abroad and tried to sell the idea to a couple of foreign companies to do work in Japan and Australia. But nobody picked it, although it may have been since I left.

Later on we developed a process for coating materials like paper or cloth with an anti-microbial material that would be firmly fixed to the paper or to the cloth. Maybe you've seen Bioguard socks. They're advertised by Interwoven, I believe. The sock has an anti-microbial on it and it tends to kill bacteria. That keeps down athlete's foot and it also keeps down foot odor. That was one of the applications. They've put that same material on rugs, carpeting, draperies for the operating rooms, paper drapes, paper things. That was one of the projects that was coming out of one of the labs. It really wasn't going, so we decided to invest some money in getting it going. Those are the kinds of things that we were doing.

Basically I was only in that for four years. Maybe the things that we had success in were useful, but I'm proud of one other thing. We developed what we called tombstones. We took a whole group of about twenty projects that had been floating around the company, and we examined them, one by one by one. We said, "This can't work unless something else happens. We're going to bury it. We're going to put a tombstone on it. If you want to resurrect it then you've got to do this." People were a little astonished, I guess, because they didn't think you ever buried an idea, but we figured it was time to bury some of them.

They retained the New Projects Business for a time, but I think now they've done away with it and may have put it back in each of the businesses so that they have their own new projects effort. Dow went through the same phases too. They had a separate group for new ventures development, and then later on spun it back into each of the businesses separately. I guess that's appropriate action. At one point, they even thought that we might do some outside licensing of ideas, and so I did a little traveling. At one point General Electric had an idea that they didn't use and they were willing to license. We kept good track of that. But, none of those things ever seemed quite to fit with what we were doing, so we went on to develop only the things that were internal.

BOHNING: And then it was in 1972...

WARRICK: ...that I semi-retired. Yes. The company developed a policy which I guess they took from Dow, that anybody in a major operation will relinquish their title and their operation at age sixty. Well, in 1972, I was sixty-one, but it was the first time that they had developed that. So, Dr. Hunter and I were the first two people to do this, to take what they call early retirement. Really it was sort of a semi-retirement. It wasn't retirement yet. And they still do that. A lot of people go into that stage. At sixty they relinquish their title, and then someone else can fill the job and they are on a consulting basis.

During that period from 1972 to 1976, I did get involved in a patent suit that we had that had been pending for sixteen years. We finally spent a month in court, between two courtrooms in Bay City and in Flint. We finally won, but the testimony was a stack of stuff about so high, because we had to educate the judge and bring him up-to-date on what we were doing. It was back in this early stage of treating fillers which we did after we found the high-strength rubber, and it was based on that. This fellow had written a paper patent. Basically, he figured that fillers were going to be important, and therefore he would treat them all, and that would be important. He actually didn't do any work but he just wrote the paper patent like so many people have done. We were able to show that he didn't understand the process, or what was going on, and that he just claimed to understand it. So we managed to get out from under that one.

BOHNING: Since you retired you've been doing some work with Saginaw Valley State College.

WARRICK: Yes. I retired in 1976. Well, let's go back one notch. Before I retired our younger daughter decided to go to Africa to teach. She graduated from Albion College and went to France first to learn French. She had had four years of German, so she knew German well. But of course, she elected to go to a country where French is the language. So she went to France for one winter in a little town outside of Lyon. At this location they had a school that spoke only French, and they brought in potential missionaries from all over the world to learn French. She did learn French very well, but she had to spend nine months at it and go into the classroom in that local high school and teach mathematics, because that's what she was going to teach down in Zaire. She finally went to Zaire in 1973. By Christmas of 1974 we decided to go over and see her. We flew over and spent time with her and then took an animal tour afterwards and came home. She came home in 1976 and I retired in 1976. But that trip to Zaire was interesting.

After retirement we traveled some and we spent some winters in Florida and one in Arizona. We didn't spend the whole winter, mostly a month. We never spent a lot of time away. In the fall of 1979, Dr. [Robert] Yien of Saginaw Valley called me and wanted to know if I could have breakfast with him the next morning. I said, "Well, sure." I went out to have breakfast with him, and it developed that they wanted me to take the interim job as dean of the School of Science and Engineering. They had had a college of engineering, but arts and sciences had been together. They wanted to separate science and put it with engineering and form a science and engineering school. That had not happened before and they did not have a dean. They had a man who had headed engineering, but he didn't have a Ph.D., and they wanted somebody with that degree. So, they asked me would I do it, and I said, "Yes. Sure." I stepped in there full-time, not knowing exactly what I was getting into. It was pretty hectic, because we were also looking for a new man at the same time. By May or June of 1980 we had found the man and hired him. So I stepped out and the fellow took over. I thought, "That's fine."

I was governor of Rotary the year of 1980-81, so I was just as glad I didn't have a job at Saginaw at the same time. But then this chap that we had hired didn't work out. He got interested in venture capital things and didn't devote enough time to the job of dean, so they fired him. Just up and fired him one day. About April of 1983 I was at a so-called Torch Club meeting. Torch Club is an organization of professional people from Saginaw, and I was a member. Dr. Yien said, "You know, we had to fire this chap." I said, "Oh, no. You know I'm not really excited about stepping back in, but if you really need someone, why I'd be glad to do it." In about a week they called me and said, "Well, would you come out?" So I stepped in again.

I had to bring order out of chaos, because he had left things hanging fire. In fact, there were piles of paper all over the office. As I said to Dr. Yien, "My first job is to find out what's in these piles, because I don't know, there might be a time bomb in there." I did find two. One was that we had committed ourselves to something, and I just had to say, "There is no way we can do that." We just had to get out of it. Another one was that we had committed ourselves to pay some money, and we had to pay it. We finally got things straightened around and we set out on a new hiring scheme and finally did hire a new man. In the fall of 1984 he stepped in and took over.

But Dr. Yien didn't want to let me go so he kept me on for a couple of days a week for a while, and then one day a week, and I'm still on one day a week, supposedly. This past week I was out there two days, so it's a variable kind of thing. But I'm doing things that they need to do because they're in a big building program now. They're building a classroom building, a library and a science building. All at one time, and it's running around $18 to $20 million. They're going to hopefully move in in April or May, and I was helping the faculty pick furniture for their offices here this last week. That is what I was doing there. But I think that once they move in, I may be able to get away from that. But I've enjoyed it. It's a good school, but I think they need help.

One of the things I did that last year, the 1983-84 year, was work on getting an agreement from fifteen other schools. It wasn't exactly a unanimous vote, but nonetheless we did get an agreement that we could put in an engineering program. We had a so-called engineering technology program up to that point. Now we have an electrical engineering degree and a mechanical engineering degree, and the work I did in 1983-84 enabled us to do that. We installed a degree last year in 1984 and I think we did graduate our first person in 1985. Even though we didn't have the degree for more than a year, people had enough credits in other courses that they could graduate.

BOHNING: I also understand that you have quite an expertise in mineralogy. How did that develop?

WARRICK: That's sort of strange. Back in the late 1960s we took our younger daughter out to California for a trip. We went to San Francisco and stayed there for a while and toured around San Francisco, then rented a car and drove down the hundred-mile road to Los Angeles. (It's more than a hundred miles, but there's a one-hundred-mile stretch in there where you can't get off the road.) We stopped overnight at Hearst Castle and right there was a little jade shop. I went in there and bought a small piece of jade, although I didn't have any equipment to work it at the time. So I got interested in jade, one thing led to another, and I finally bought equipment and more jade. It just kept growing. I've made my wife a number of pieces of jewelry and I made myself some tie tacks and cuff links. I've made gifts for some people. I like to work with jade. It's a very nice, easy material to work with. It's what I call a forgiving material. You don't have to worry about making mistakes, because you can correct them. But if you work with something soft like opal, you've got to be very, very careful.

I've enjoyed it and I've spent time with it. I've been giving talks on it. In fact I gave one just last week. I must have given that talk a hundred times to various organizations. We visited a number of museums. There's a good one in Chicago called the Lazadra Museum. There's a private collector in Toledo, [J. J.] Schedel, who has a terrific collection. He's even published a book on it (10). Even the Flint Institute of Art and the Detroit Institute of Art have some jade, although they are not really extensive collections. I haven't done anything in that field for a good many months now, because I've been busy with other things, especially now since I started the book (11).

BOHNING: Yes, that's right. You are writing. Do you have a target date on the book?

WARRICK: No. I've got the first chapter finished and I'm almost finished with the second chapter, and there are only ten chapters total. The way our contract reads with Dow Corning, once the first chapter is fully accepted, then I have a year and a half to complete it. They said they would not hold me to that, but that would be a desirable goal. I don't see too much trouble. I'm still getting a lot of input. I just got about twenty pages of handwritten material from a fellow I didn't really expect to get something from, but he finally came through. We just sent another letter to another man in England and asked him for some help. I've written well over 120 people to get help, and I've got quite a bit. In fact, I've got to make a decision here soon about what to do about the files. I've got a two-drawer file that's just jammed full of stuff that I've collected. I've even had to work a so-called spread sheet to tell me what's in this set of files. I know when I come up to a certain subject I can look back and say, "Oh, yes. This is the one I have to go to." It is getting to be a little bit unwieldy, because when you get all that much input, putting it together is quite a job, although you don't have all the tasks of researching everything. But I'm still doing research down at the library at Dow Corning. It's a big job trying to get it all down.

BOHNING: Has the company been supportive?

WARRICK: Yes, very supportive. They've loaned me this computer and printer, and they've supplied paper. I just stopped off yesterday and picked up another set of disks. I've already used twenty disks. You have two disks for correspondence, a disk for notes, and this and this, to say nothing of the program disks.

[END OF TAPE, SIDE 5]

BOHNING: I wanted to ask you if you had any comments about the changes you've seen during your career.

WARRICK: Well, I've seen a terrific development in the technology of how you do chemistry. It's gone from simple test tubes to very exotic things. It's really fantastic. One young man that we had working for us in Pittsburgh was Paul Lauterbur. I don't know if you've heard about him, but he's at SUNY, State University of New York, Stony Brook. He was interested in nuclear magnetic resonance. He came back from the army, having been at Fort Bragg, and had used a nuclear magnetic resonance tool there that they had never used. He just got it out of mothballs and used it. He came back to us and said he'd like to have a tool like that because he thought it could be useful to us. We finally got permission to buy it. It was about $80,000 and had a big magnet and a lot of electronic gear. He got it running. Not only did he do hydrogen, which was the going thing then, but he finally did silicon-29 and carbon-13. He did a lot of innovative things.

He left us eventually to go back into academia, but he's done some fine work in the field. The machine that you and I can go over to the hospital and get a nuclear magnetic resonance scan on is his idea. If it weren't for him, that machine still wouldn't be developed. It is quite a machine, because it doesn't subject you to very much radiation, and it also makes it possible to even make a little bit of an estimate of what chemistry is going on at a given site if you have some abnormality. I've been real pleased that that kind of thing did happen. I guess I'm a little sympathetic to John Speier when he says, "These fellows that work with 5 cc's really don't know how to do chemistry." In a sense they don't, because they don't know how to make a pound or two of something. And a pound or two is just the beginning for us most of the time.

BOHNING: There's a new laboratory manual in organic chemistry that is at the micro level in order to cut down on waste and vapors in the environment. They are now having students work at 1 cc levels.

WARRICK: [laughter] I can remember one of the projects that we had in organic was to prepare stearic acid. We had to go out and buy suet. We had to break up the ester and get stearic acid out of it. Then we had to recrystallize it in alcohol. I was heating a great big dish about that big one day when the alcohol caught fire and burned the back of my hands. Making a few hundred grams of stearic was a typical assignment in those days. So you did it on a large scale.

BOHNING: I guess my last question then is what you might see in the future in terms of the industry.

WARRICK: I really think it's got unlimited possibility. We just haven't scratched the surface of silicon chemistry really. We've been dealing with polymers and I would say, but I'm not sure, that eighty percent or more of Dow Corning's business is in the same few polymers that we started out with. Dimethyl, phenyl-methyl, and that's it. But there are so many other possibilities. John Speier himself started some work with Eli Lilly when we were still at Mellon. They had some very interesting drug-related possibilities. Dow Corning did get into a drug-related field at one point with a material that tended to atrophy the prostate gland. In other words, there was some possibility that it might be used in prostate cancer. And a Swedish firm, A. B. Kabi, Stockholm, worked on this for some time. Finally, I think we gave it up because it was going to be so hard for us to prepare the product. It was a product that had both levo- and dextro-rotary products, and only one of them was active. Separating the two was going to make it a real problem. But it did do things. It did work, and there was no reason why it should. It was an unsaturated polymer, and just because of its size and shape, it had these effects.

John Speier has made many products that have reactive groups on them. The question is, "How would they behave biologically?" We haven't really touched that field. One of the things that was of some concern to us, because we'd been making tons and tons and tons of dimethyl fluid over the years, was, "Where is that going?" You know, "What happens to it?" We finally were able to show that if you spilled "200 fluid" on the ground, pretty soon there's nothing left but sand. Bacteria in the soil will destroy the methyl groups. So it's not a contaminant, really, in the accepted sense of the word.

But that doesn't mean there aren't things that can happen and so really we have not explored the shady region between organic chemistry and organo-silicon chemistry. That region is still unexplored and lots of things can happen. New things are developing constantly. We had a little bit of work during that period that we were studying radiation, whereby we were able to show that we could cure silicon rubber with ultraviolet light. We made some printing plates that you could use on printing presses. Now they've got a product that they make the encapsulant for the glass fiber optics. The encapsulant for that is a silicon rubber that's cured with ultraviolet light. It's brand new and they're making miles and miles of it. So, there are new things all the time.

I'm optimistic. I think Dow Corning and people in that field have a long way to go. Lots of things can happen. At times I think that John Speier is discouraged in that he can't sell them on certain ideas. But he does sell them on some ideas and they are put into practice. One of his ideas was put into practice in the main plant down at Carrollton, which is the world's largest plant for the production of silicon materials. It's producing better results because of his ideas. So I think that the technology is growing. There is no question of that.

And who takes the role of putting some of this new technology to work in new fields? I think that's a challenge but I think companies will do it.

BOHNING: I'd like to thank you very much, Dr. Warrick, for a very fascinating account of your career and your ideas.

WARRICK: Oh, you're welcome indeed.

[END OF TAPE, SIDE 6]

NOTES

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