

CHEMICAL HERITAGE FOUNDATION

CHARLES E. REED

Transcript of an Interview
Conducted by

Leonard Fine and George Wise

at

New York City, New York

on

11 July 1986

(With Subsequent Corrections and Additions)

CHARLES REED

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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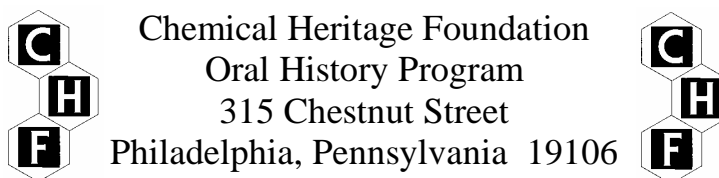
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CHARLES E. REED

1913 Born in Findlay, Ohio on 11 August

Education

1934 B.S., chemistry, Case Institute of Technology
1937 D.Sc., chemical engineering, Massachusetts Institute of Technology

Professional Experience

1937-1942 Assistant Professor of Chemical Engineering, Massachusetts Institute of Technology

General Electric Company

1942-1945 Research Associate
1945-1952 Engineering Manager, Chemical Division
1952-1959 General Manager, Silicone Products Department
1959-1962 General Manager, Silicone & Metal Products Department
1962-1968 Vice-President and General Manager, Chemical and Metals Division
1968-1971 Vice-President and Group Executive, Components and Materials Group
1971-1979 Senior Vice-President, Corporate Technology

Honors

National Academy of Engineering
Fellow, American Institute of Chemical Engineers
1971 Commercial Development Association Award

ABSTRACT

Charles Reed begins the interview with a discussion of his family background and early education. Reed grew up in Findlay, Ohio, and credits his high school teachers with fostering his interest in the sciences. He attended Case School of Applied Science, earning his B.S. in chemistry in 1934. At Case, he was influenced greatly by Professor Carl Prutton, and Reed decided to continue on to graduate school. Reed wanted to pursue both chemistry and chemical engineering, and combined his interests at MIT, where he earned his D.Sc. in chemical engineering in 1937. His thesis there was on colloid chemistry, which led to his later fascination with polymer chemistry. Upon receiving his doctorate, Reed became an assistant professor at MIT. While there, he also began to consult for various companies. In 1942, he accepted a permanent position with General Electric Company, where he spent the rest of his career. His first work involved organosilicon polymers and the scaling up of processes. When G.E. decided to set up a chemical engineering department, Reed was selected as the manager. Through the years, Reed gradually moved up the management ladder, becoming senior vice-president of corporate technology in 1971. During his time at G.E., he was helped to scale up the silicone processes, worked on phenolic laminates, the commercial development of synthetic diamonds, and the development of both polycarbonates and polyphenylene oxide. Reed concludes the interview with his thoughts on the future of G.E. and his experience as a member one of its Sector Boards.

INTERVIEWERS

Leonard Fine is Professor of Chemistry and Director of Undergraduate Studies in Chemistry at Columbia University. His special interests include polymer chemistry and materials science, industrial inorganic and organic chemistry, engineering plastics, problems in solid waste management and the recovery and recycling of post-consumer plastics. Among his recent publications are two practical manuals on principles and practices of infrared spectroscopy and a general chemistry textbook for engineers and scientists. He holds a B.S. in chemistry from Marietta College and a Ph.D. in chemistry from the University of Maryland at College Park.

George Wise is a communications specialist at the General Electric Research and Development Center in Schenectady, New York. He holds a B.S. in engineering physics from Lehigh University, an M.S. in physics from University of Michigan, and a Ph.D. in history from Boston University. He worked briefly as a systems engineer before entering his current career in public relations. He has published a book and several articles about the history of industrial research, invention and science. His current research interest is how people can learn from history.

TABLE OF CONTENTS

1	Family Background and Early Education Growing up in Findlay, Ohio. Influence of high school teachers. Interest in science. Siblings.
2	College Education and Early Career Attending Case School of Applied Science. Interest in both chemistry and chemical engineering. Influence of Professor Carl Prutton. Decision to attend graduate school at MIT. Thesis on colloid chemistry. Teaching position at MIT. Consulting. Growing interest in polymers.
12	General Electric Company Decision to work for G.E. Organosilicon polymers. Scaling up fluid bed process. Working with Eugene Rochow and Abraham Marshall.
24	Chemical Engineering at G.E. Setting up Chemical Engineering department. Designing full-scale silicone plant. Cross-licensing agreements with Dow. Work on phenolic laminates. Moving up in management.
33	G.E. Research Discovery of polycarbonates by Daniel W. Fox. Commercial development of synthetic diamonds. Development of polycarbonates and PPO. Emphasis on Noryl.
55	Concluding Thoughts Recent expansion of G.E. Sector Board.
58	Notes
59	Index

INTERVIEWEE: Charles E. Reed

INTERVIEWERS: Leonard W. Fine and George Wise

LOCATION: Columbia University, New York, NY

DATE: 11 July 1986

FINE: Let's start with your early life and your early career. Where are you from: what was your early life like? Can you say something about your parents and family and perhaps some thoughts on how you were predisposed towards science and technology?

REED: I was born and raised in Findlay, Ohio. We lived within a block of a small liberal arts College—Findlay College. I left Findlay in 1930, after graduating from Findlay High School and went to the Case School of Applied Science, as it was known at that time, in Cleveland. My parents continued to live in Findlay for all their life. My father passed away in 1957, and my mother stayed there until her death last year at ninety-two. I have maintained ties with Findlay. My brother and I have just funded a scholarship in honor of our mother at that little college.

I was interested in science because I was very fortunate to have in high school some very good teachers in mathematics, physics and chemistry and I think that they predisposed me towards engineering. A number of prominent Findlay residents had gone to school at Case because Findlay was the headquarters of what was the Marathon Oil Company. The president of the company at that time, O. D. Donnell had graduated also from the Case School of Applied Science in 1905. So there were good reasons for me to attend that school. I was also fortunate in being able to get a scholarship, a competitive merit scholarship from Case that took me through four years during the Depression so that I never really realized its severity.

FINE: Before we get off the subject, were either of your parents involved in science or industry?

REED: No. On my mother's side, her father was a physician. My father came from a family of farmers in Ohio and then went into banking. There was no science tradition in the family, except for medicine.

FINE: Brothers and sisters?

REED: I have a brother, eight years younger than I, who studied medicine and is Clinical Professor of Pediatric Radiology at Bowling Green State University and head of the Radiological Department at Childrens' Hospital in Detroit. He is a very good radiologist.

FINE: Did you think about a career in medicine at all?

REED: I thought about it because a number of my friends in high school had gone into medicine, but I was more attracted to science and engineering.

FINE: When you were at Case were you more interested in pure science and chemistry rather than engineering and chemical engineering?

REED: Well, Case was in those days oriented considerably more to engineering than it was to science. Again, I was fortunate in having some very good professors in applied chemistry and chemical engineering that just kept me interested in chemical engineering. One, in particular, Dr. Carl F. Prutton, who was later head of the Department of Chemical Engineering at Case and then went into industry, was particularly influential because he was one of the great combination applied chemists/chemical engineers in the country, and in fact, one of the best I have ever known.

FINE: It is a small world. I knew Prutton by his world famous physical chemistry textbook.

REED: Prutton and Maron (1).

FINE: Prutton and Maron. Yes. That's even more interesting, coming from that kind of a background.

REED: Well, [Samuel H.] Maron was a good teacher, but Prutton was in a class by himself in combining chemical engineering/applied chemistry. He was granted dozens of patents for his research.

FINE: Was Prutton the reason that you went to graduate school rather than going out to get a job?

REED: I would say Prutton and the head of the department at that time, Dr. William Veazey, but also other teachers in the Chemistry and Chemical Engineering Department. It was obvious to me that with my interest in chemical engineering I ought to go on to graduate school and then I was fortunate in being able to get what in those days was a very liberal fellowship. The Tau Beta Pi Graduate Fellowship. It paid one thousand dollars a year, which was enough for one to live for a year.

FINE: You didn't actually interview with any company, then.

REED: No. I had made up my mind to go on to graduate school. I had always looked forward to chemical engineering at MIT [Massachusetts Institute of Technology] because I was familiar with their school of Chemical Engineering Practice.

FINE: How did that come about?

REED: I was simply aware that graduate work in chemical engineering at MIT was outstanding. The whole field of unit operations was originated at MIT by W. H. Walker and W. K. Lewis, who wrote the textbook on *Principles of Chemical Engineering* which promulgated the theory of the unit operations as they were identified in the 1920s and the 1930s (2).

FINE: When did you get to MIT? That would have been in 1933?

REED: 1934—The Fall of 1934.

FINE: Right in the heart of the Depression. What was it like being at MIT in those days?

REED: Well, I can't say that there was much shock in going from Case to MIT. I can see much more of a contrast today than at that time. Case had not really yet modernized its curriculum in the 1930s. I certainly think I got a very good education there. Certainly it inspired me to go on and gave me enough of a basis so that I was able to do pretty well at MIT. But when I look back it seems that they could have done better. For instance, they had a general physics course without calculus, if you can imagine that. I had taken calculus of course, at Case, but it was not integrated into the general physics curriculum. Now if you went on to advanced physics, or majored in physics, obviously you had fundamental mathematical physics. But not for the other engineers who were taking physics simply as a requirement.

FINE: Did you have a strong mathematical background from Case?

REED: No, I didn't. I should have had. I suffer from what I'd call a "not really good mathematical background." But I've always been very interested in mathematics and I'm good enough to work on the original edition of this book (3). I've had a lifelong interest in mathematics and have continued my own mathematical education. But when I think of what it could have been compared to what it was, I feel that it could have been a lot better. It has taken me years to understand or feel that I have an understanding of mathematical fundamentals.

FINE: What kinds of collegial relationships developed while you were at MIT? Were you close with your professors?

REED: I was again, in my opinion, very fortunate to go to MIT at a time when they had an absolutely outstanding faculty. In particular, I can recall Warren K. Lewis, who was in a class by himself. A very different man from Prutton, but a great applied chemist, chemical engineer, a teacher and industrial consultant. And [William H.] McAdams, who was really the world authority on heat transfer at that time. [Thomas K.] Sherwood, who became a particularly good friend of mine, a pioneer in the quantitative treatment of absorption and extraction. A rising young star at that time by the name of Ed [Edwin R.] Gilliland, who again was one of the best chemists/chemical engineers that I have ever known and my opinion remains unchanged to this day, although I've met many of them in the meantime. Lewis and Gilliland just simply stood out among all the outstanding people I have known.

FINE: They influenced you?

REED: Yes. I was very fortunate in that. And the curriculum at MIT was very good. For example, there was a course called Economic Balance in the Chemical Industry. Well this was a course that considered plant design from an overall point of view: chemical engineering, and chemistry, and superimposed was the economic balance—how you could optimize plants on any criteria you have selected, including the economic criteria. This course included basic cost accounting principles which have stood me in good stead to this day and were just excellent as I got into the General Electric Company [GE] because a lot of people have never had them. I would say that I was fortunate at MIT in having curricula that were ahead of their time as far as economics and engineering, and in particular the economic considerations of plant design.

FINE: Would you say that perhaps that was unique to MIT?

REED: I really should not say because I was not familiar with the curricula of other schools at that time. The Sloan School didn't even exist then. I mean, at that time MIT had a Department of Economics under [Davis R.] Dewey and that was it. He had integrated chemical engineering with economics in that department and I'm sure they did it in other MIT departments too.

FINE: Did people like McAdams, Gilliland and Lewis end up involved with General Electric because of these early programs?

REED: I'll come to deal with that a little bit later but the answer is, briefly at this point in the interview, that I was able to retain Lewis as a G.E. consultant for a number of years, and Gilliland as well. We will get back to that a little bit later.

FINE: What about your peers, the graduate students at the time?

REED: That again was one of the great things about MIT. The average level of the graduates was outstanding. We were at the beginning of what I call the Golden Age in Chemical Engineering. Chemical Engineering was of vital importance in the development of the oil industry and the petrochemical industry because they depended on large scale unit operations—distillation, heat transfer, absorption, extraction—and the chemical engineers were the experts in plant design and operations. Lewis, as a consultant to Standard Oil of New Jersey, was outstanding. He pioneered the fluid bed catalyst technology for the oil industry. The old Burton process of thermocracking was developed back in the 1920s by Indiana Standard and Universal Oil Products had introduced many improvements. Catalytic cracking was introduced in the 1930s by [Eugene] Houdry. The catalyst was placed in a fixed bed and hydrocarbon vapors were passed through at high temperatures and pressures yielding a much higher content of gasoline hydrocarbons than with thermocracking. Lewis pioneered the use of the fluid bed catalytic process where the catalyst is not fixed but is a boiling mass of catalyst particles with the hydrocarbon gases blown through thus getting a much better mass and heat transfer. That was a thrilling process at the time, and was applied to many other chemical processes. I seem to be getting a little ahead of my story. But I was so fortunate to be exposed to that development because when I eventually went to General Electric it was obvious that that was the way to go about making silicone polymers—the methylchlorosilanes from silicon and methyl chloride.

Another aspect of my education at MIT related to polymers and colloid chemistry. It was necessary after passing my oral exams in 1935, to choose the subject for a thesis. I was as much inclined towards chemistry as I was towards chemical engineering. A new professor came to MIT in the Fall of 1935 who convinced me to do a thesis on colloid chemistry. Lewis had always been interested in colloid chemistry, but was particularly interested in the colloid chemistry of clays because he was a consultant on petroleum production research as well as catalytic research in refining, and clays were used in both cases. So he encouraged me to go on with the thesis in clays, and the subject of my thesis was the effects of particle size on the

thixotropy of bentonite clays. Thixotropy, you may recall from colloid chemistry, is an isothermal, reversible sol/gel transformation. In other words, when a suspension of these clay particles in a test tube is left to settle, in a few seconds or a few minutes it will become a gel and it won't flow out of the tube. The speed of transfer from sol to gel is a function of particle size. The smaller the particles at a given concentration, the more rapid the change. This phenomena is very important in drilling muds which were used, or beginning to be used to control the pressure in drilling oil wells in deeper drilling in the 1930s. That got me interested in colloid chemistry, and it is only one step from there to polymer chemistry.

FINE: Did you practice experimentally or theoretically?

REED: This was an experimental work. I developed a method of fractionating the bentonite clay according to the various particle sizes. It was done in what was called the super centrifuge in those days. If you can imagine a cylinder, a long narrow cylinder which is rapidly rotating along the vertical axis. The suspension of clay particles was heated at the bottom and flowed up as the centrifuge rotated. The coarser particles stayed at the bottom. The finer the particles were, the greater the distance they traveled up the rotating bowl before they were thrown out on the wall. That was a way of separating the fine from the coarse particles and actually I had to develop equations for determining the particle size distribution of the clay and was able by a series of fractionations to collect sufficient quantities of the various particle sizes in order to carry out the tests on the sol/gel transformations. We published a couple of papers on that (4). It was a very interesting study. It took me about a year to do the experimental work and I finished writing the thesis that year and received a degree in 1937 from MIT. At the end of 1937, I was offered an instructorship. But as I say, it was only a step from the colloid chemistry of clays to polymers. Clay is a polymer the way we look at it today—an inorganic three dimensional polymer. Bentonite is a particularly interesting one. I haven't studied the picture of bentonite research in recent years, but my guess is it hasn't changed a great deal. It is an interesting material with a structure made of a number of sheets on top of each other which allow water and ions to flow in between these sheets and attach themselves to the aluminosilicate structure and swell it to enormous dimensions.

FINE: Were you more interested in the practical aspects of your research versus the theoretical?

REED: This is again a very interesting question. I was more interested in studying this particular subject systematically. Some experiments required me to pour a solution onto a glass plate and let it dry in order to be able to cut off strips from that plate, measure and weigh them. That was the way I determined the weight of particles separated by centrifugation. I had a liner in the centrifuge bowl that I could pull out and unroll the material on a flat surface. Here beginning at the bottom was a layer of particles that were heavy, and at the top they were light, and after letting them dry I could peel them off and weigh them. I didn't think much then that those films of bentonite clay might find some practical applications. But Professor Hauser, who

was my supervisor in this research, was very practically minded and he saw immediately that these films might have practical value, particularly in insulation. He brought them to the attention of Dr. [Willis R.] Whitney who was still active in the lab and to [Abraham L.] Marshall over in Schenectady and were instrumental in inspiring Marshall to set up a program on bentonite as an electrical insulating material. I was not involved in that at all. Jim Morrison was put in charge of the program and they even had a name for the material: Alsifilm.

FINE: You were not involved in this at all?

REED: I was not involved at all.

FINE: Did you know what was going on?

REED: I knew there was a general interest over there but at that time, I was involved in teaching chemical engineering, applied chemistry and applied mathematics. So I didn't pay too much attention to it. I was, you might say, naive and knew nothing at all about the way graduate students today look for opportunities, start venture capital companies. With hindsight though, I am very fortunate that I did not fall in love with Alsifilm as I am sure I would have if I knew what I know today and was encountering a new film material like that. I would have delved into it and thought perhaps that I could make my first million dollars. But, really, to sum up the whole thing, it never amounted to anything. It was exciting at the time for many people and they did a lot of research on it but it never amounted to something. We got a few patents based on that work of mine when I was peeling those strips off of the centrifuge liner (5). If I had been very interested in it, I never would have gone with the General Electric Company. I would have gone with somebody else and it would have been a dead end. When I say a dead end, I mean that particular material. I don't know where I would have been, but I probably wouldn't have been as happy as I was at G.E.

[END OF TAPE, SIDE 1]

FINE: Did you get to know any of the G.E. people at this point?

REED: I knew no G.E. people at all. I remember Hauser coming into the office one day and he had a small automobile generator motor. He put that down on the table and he said, "Boy, look at that. We made that out of Alsifilm over at G.E.'s laboratory." I looked at it. I wasn't too impressed because I had other things on my mind, too.

FINE: So you went straight into teaching again without any thought or any interviews for going into industry.

REED: At that time, in 1937, I did have a number of interviews but I was offered an assistant professorship in the department and that convinced me to stay on in teaching for a while.

FINE: There obviously could have been a lot more money in industry so there must have been other reasons.

REED: Well, I wasn't thinking, again, too much about the money in those days because what I had was quite sufficient. Financially I never had any problems in college. When I was at Case I had a scholarship and I was an officer of my fraternity which helped to pay for my board and room which in those days ran fifteen to eighteen dollars a month. To get through Case I think it cost my parents twenty-eight hundred dollars. The rest was scholarship money and what I was able to earn. At MIT it was all fellowship money until I started my first job there at twelve hundred dollars a year. In those days I just wasn't strapped for money at all because I was so interested in what I was doing.

FINE: Was there much of a chemistry department connection with applied chemistry and chemical engineering?

REED: I took my minor in chemistry at MIT and, again, there were very good people and I had a number of excellent courses in physical chemistry, thermodynamics, and organic chemistry so that my chemical education was improved dramatically. But I've got to give Case credit, for making me interested to go to MIT. I've always enjoyed the combination of chemistry and chemical engineering and the minor taken in chemistry was very important for me.

FINE: Were you interested in things like [Herbert] Freundlich's book on colloid chemistry (6)? That was the great theoretical work of the day.

REED: Well, I really wasn't except as a cursory reading in colloids. I would say [Herman] Staudinger was very interesting to G.E.

FINE: I never read Staudinger.

REED: Hauser had met Staudinger.

FINE: Which Hauser is this?

REED: This is Ernst Hauser. He was a rubber chemist who was brought over from Austria to this country by Lewis in the Fall of 1935. He came over to strengthen the applied colloid chemistry in that department. Lewis felt that more should be going on in colloid chemistry and Hauser had a very good name in rubber.

FINE: These things—jumping ahead—all have interesting connections into the future: High impact polystyrene, particle size, colloidal dispersions, PPO, things like that.

REED: Right.

FINE: Your thesis was published. You became an assistant professor. Were you thinking of becoming a tenured, full professor?

REED: At that point I simply didn't have any long-range plans. I was interested in what I was doing. I was very much aware of opportunities in the petroleum industry because I had a summer job working for Standard Oil of California as a chemical engineer in 1935. In 1936, I was doing my thesis and in 1937 I was an assistant instructor in an MIT Chemical Engineering Practice school station at the Everett plant of the Monsanto Chemical Company. It was devoted to heavy chemical manufacture—sulfuric acid, alum, some heavy commodity dye chemicals and gave me a lot of experience in chemical manufacturing. Then Sherwood talked me into becoming more and more interested in the mathematical aspects of chemical engineering. He realized that there was no systematic presentation of the mathematics underlying unit operations. He felt that there were many good mathematics books, but they were all written for mathematicians and had no interest in presenting the applications of mathematics for the chemical engineer. He had the vision that there should be a textbook on how to apply mathematics to chemical engineering problems. That included, of course, calculus and how to set up elementary differential equations as contrasted to solving them, because the problem most people have is that they don't know how to set them up. Once you get the correct equation set up or have the right model, you can get all kinds of help from a mathematician, but the mathematician is usually of no help in setting up the equation because he is not familiar with the physical problems involved. Obviously you can educate them, but if you are a chemical engineer you have an advantage if you learn how to set up your own equations. Of course, we didn't anticipate at that time the computer. What we did know was that you could set up many equations which you couldn't solve analytically. We had to solve them by numerical methods of approximation and there again we devoted considerable space in the book to explaining how to solve these more complex equations numerically.

FINE: Did you get involved at all with the MIT differential analyzer?

REED: No. That was segregated in the Department of Electrical Engineering. No chemical engineers were involved in that at all. That included Lewis, Gilliland and all the rest of them. So I spent the summer of 1938 in writing this book with Sherwood (3). We finished it up in the fall of 1938 and it was published at the beginning of 1939.

FINE: That's fast.

REED: It is fast because we didn't have to do a lot of literature review work. What Sherwood had, and what he taught me, was a lot of experience in setting up these equations in various phases of chemical engineering. It's been an amazing book. I still get a check twice a year for this book. We had it revised by [Harold S.] Mickley in 1957 (7). That was almost twenty years after it was published. Mickley, deservedly, gets the main share of the royalties. Sherwood got royalties until he died in 1975. We tried to get somebody else to revise it once more, but we haven't been able to find someone. It's been very well sold. It still sells. So write a book on mathematics. I never thought that after almost fifty years, it would sell one or two hundred copies a year. It's more of a phenomenon than anything else. And even today it is still possible for somebody to get something out of it on how to set up the equation. But obviously it's got nothing on the use of computers, and if it were to be revised, a big section ought to be added on the use of computers.

FINE: Was this book the basis for an MIT course?

REED: Oh yes. I taught a course, a graduate course, in applied mathematics and chemical engineering with that as the text.

FINE: What else did you teach?

REED: I taught Economic Balance in the Chemical Industry, Applied Mathematics in Chemical Engineering, Distillation and Adsorption, and Applied Materials Chemistry. Again, I was very fortunate because as we all know, you don't really know a subject until you have taught it. Applied Materials chemistry was a pioneering course again that Lewis had developed. It covered all materials—the industrial chemistry materials. Included all of the amorphous materials, structured rubber, cellulose polymers, glass, clay, et cetera. It adopted an integrated approach, which was unique and important. It covered the whole area of materials and wove it

together with some fundamental structure concepts. Teaching this course was very good for me in building a basis for my education on materials.

FINE: That's really very interesting—a confluence of ideas here. Herman Mark came to the United States, I think, about 1938. [The] Svedberg had demonstrated that these polymers were macromolecules and not agglomerations of fine particles at the time and you were working in the field and about to go on to G.E.

REED: And at the same time, [Wallace Hume] Carothers was doing his work. I think Carothers was the most influential in getting acceptance for the concept of macromolecules as against the theory of colloidal particles. The idea that bifunctional and three dimensional trifunctional molecules interact, and that the length of a chain can be controlled by putting in a monofunctional molecule was very simple and anybody could comprehend.

FINE: Were you familiar with this picture at the time—from 1939 to 1942?

REED: I became familiar with it as I taught these courses on amorphous materials around 1939. It was just beginning to be realized in phenolic plastics, polystyrene—which was one of the early petrochemical polymers—and cellulose.

FINE: Did you do any industrial consulting while you were on the faculty?

REED: Yes. I did some industrial consulting for Dewey and Almy, who were very interested in bentonites for a number of their products. For a company that was known as West Virginia Chemicals on refrigerants, working up a handbook of refrigerant properties for them because I was, again, very much interested in thermodynamics and taught courses in graduate thermodynamics. And on general polymer chemistry for Standard Oil of Indiana.

FINE: Were you directing graduate student research at the time?

REED: I had never been very good at research at MIT because I was more interested in expanding my knowledge in these different fields than in starting a graduate research program. Of course, it is publish or perish at MIT. Before I ever got started on that, the war began and I became interested in polymers at MIT. I really hadn't made up my mind which industrial sector to choose. In 1939 I had summer work at Mallinckrodt [Incorporated] out in St. Louis. Here again, it was chemical engineering unit operational work where I had to develop a process for making sodium sulphite. They were doing it by a batch process and I developed a continuous

process for them. Then in the summer of 1940, I worked at Dow Chemical Company in Midland and really got into the field of polymers.

I had become more and more interested in polymers as time went on and taught extensive courses in polymers along with my regular work at MIT. I taught a special course on polymers to the Monsanto people at their plant in Springfield, Massachusetts. At Dow in 1940, I was involved in a survey of the whole field of polymers. I was invited to go back to Dow again in the summer of 1941 and that's when I was first introduced to organosilicon chemistry because it was known at that time that the organosilicon monomers were going to be very important, as monomers for polymerization. People were just starting to get into the field.

When I was at Dow I hadn't realized that they were working with Corning [Incorporated] to get into the field of organosilicon chemistry and I proposed to them that summer to let me try a project of making organosilicon monomers from methane and silicon tetrachloride at very high temperatures. So I spent that summer developing the apparatus and experimenting on the synthesis of methylchlorosilane by reacting methane and silicon tetrachloride at high temperature. The results were negative. As I said, I didn't realize at that point that they had an agreement with Corning on silicones. I went back to MIT in the fall of 1941 and the war was getting hotter and hotter. In the winter of 1942 I was interviewed by [Chauncey G.] Suits in connection with G.E.'s interests. I was aware that G.E. was interested in organosilicon chemistry because [Eugene G.] Rochow had made a couple of radio talks, sponsored by the American Chemical Society, on his work on organosilicon monomers and methyl silicon polymers. So I knew, at that time, that they were working on it. I didn't know that they were working on it when I was at Dow the summer before. Suits was a consultant to the radiation laboratory at MIT and he was looking for somebody to hire. I should say that the first person I met with at G.E. was Abe Marshall. I was very much interested in distillation for a number of years and I was also interested in mathematics. I developed, with Gilliland, a mathematical model of distillation columns (8) and I gave a paper on that at Case in Cleveland in the fall of 1941. Marshall attended that particular meeting. I don't know whether he attended out of general interest or whether he wanted to see me, but anyway at that meeting he got hold of me and said, "I'd like to have you come over and see G.E. as soon as you can after the holidays" It was during the holidays that I had given that paper. So I said, "All right. I'll come over."

We met in Pittsfield and he told me that G.E. was trying to get into chemicals and that as an indication of how serious they were, he wanted to show me the foundations of the phenol plant they were building. And that thrilled me! I felt very much like a petrochemical oriented chemical engineer—in addition to my polymer side! He said, "We're going into the phenol business because we're in phenolic molding compounds and we feel we've got to be in phenol. We developed this process in the Pittsfield laboratories which was a modification of the Dow process which is based on high pressure hydrolysis of chlorobenzene."

G.E. had a very inventive chemist in Pittsfield by the name of [Gaetano F.] D'Alelio. He had more patents in polymers and polymer chemistry, and phenol chemistry than 99 percent of the chemists you could think of. I mean he was terribly productive. I think he wound up with

over two hundred patents, many of which were very valuable. He had the basic patents on sulphonation of polystyrene which turned out to lead to very important ion-exchange materials. G.E. made, I think, a couple of million dollars in royalties on D'Alelio's sulphonated polystyrene packs. So he introduced me to D'Alelio and at that time I didn't realize how much rivalry there was between that lab and the Schenectady lab. They were cooperating as far as I understood, but Marshall wanted me to come to Schenectady. He showed me all this in Pittsfield to make the case of G.E.'s interest in getting into the manufacture of chemical products. I went through the phenolic molding compound plant in Pittsfield.

I was thinking about this opportunity after I got back from this interview, and then Suits got hold of me again and made me an offer to come to G.E. He thrilled me by telling me how much progress they were making on the chemistry of the crude monomers including the organosilicon monomers and how strongly they needed to develop that process. In the late winter of 1942, I finally decided to accept the offer of G.E. because I more and more felt that I had to get into something more related to war work and Suits had stressed the potential of the organosilicon polymers.

FINE: This was considered war work because of the insulation?

REED: Yes. The high temperature insulation for electrical equipment on bombers. That was the main thing. The time I took the job was a very exciting period in the chemistry of organosilicon polymers. They hadn't discovered silicone rubber yet—that discovery was made about a month after I joined G.E.

FINE: Let me back up a little bit here. Carothers, of course, had discovered polyisoprene rubber back in the 1930s. Was there considerable interest and recognition on the part of the government that these silicones offered great potential?

REED: I was not aware of that at all at MIT. The G.E. people were the first ones that told me that. However, I was aware that polymers were moving pretty fast. Some of the MIT people were consulting for Standard of New Jersey and they were involved in the development of butyl rubber which was turning into a very exciting project in 1939.

FINE: That was polyisobutylene?

REED: Polyisobutylene, yes.

FINE: It seems to me you could have gotten involved in the more conventional synthetic rubber business at one of the big synthetic rubber programs.

REED: There was the big U.S. synthetic rubber program but I never gave it a thought. It was really just starting in 1941. I remember talking with Pete Low at Dow when I was there. We had lots of work going on emulsion polymerization in 1941 and someone had said, "You know, they don't want to get into the synthetic rubber business. They are just doing this because the government was insisting on it and there are a few specialty rubbers coming along. We don't really want to get into anything that's displacing natural rubber." Of course, events changed pretty rapidly after Pearl Harbor and we saw that we might be cut off from South Asian rubber. We put Bradley Dewey in charge of the rubber program who brought many MIT people with him because he was a chemical engineering MIT graduate. I had already accepted the G.E. offer and started working at the end of March 1942, and left for Schenectady in May of 1942.

FINE: Had you had any kind of tenured period?

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REED: There was a five year rule at that time and I may have been up for it but I wasn't bucking for it at all. That didn't come into my decision at all. There were several things that made me make this decision. One, I felt that I ought to get into something that was directly related to the war effort. Two was the very exciting combination of requirements for the job—chemical engineering, plus applied chemistry, plus polymer chemistry, plus new polymer chemistry plus organosilicon chemistry. Organosilicon polymers were new and were particularly exciting and it was that combination that drew me to G.E. In retrospect it was such a fortunate combination because at that time they had to develop a full scale process for the reaction of methylchloride and silicon. I was very well equipped as a chemical engineer to work on this problem.

FINE: Did you know Carothers at all? Did you have anything to do with the people at [E. I.] Du Pont [de Nemours & Co., Inc.]?

REED: I was interviewed by Du Pont for a job on a couple of occasions and I knew quite a number of people who went to work for Du Pont—classmates of mine. One of my classmates wound up as an officer in Du Pont. He was helped by the fact that his mother was a Du Pont. He was a very good man himself. He ran their Neoprene operations and all their rubber operations for years.

FINE: Who was that?

REED: Charles Harrington, an MIT alumnus. His father was a Du Pont executive. His mother was the daughter of one of Lamont Du Pont's brothers and sisters. Another fellow who was a faculty member at the time was Robert Hershey. He wound up running all of Du Pont's polymer operations for years and was a vice president of Du Pont. A classmate of mine going clear back to high school, by the name of Norman Copeland, who took his graduate work in MIT in Mechanical Engineering wound up as chief engineer at Du Pont. He passed away a couple of years ago.

FINE: Wasn't he a CEO at one time?

REED: No. He was an executive committee member at Du Pont, but not a CEO. Another classmate of mine by the name of Fred [Frederick A.] Holloway wound up as the chief technical officer for Exxon. Another one, who was a couple of years before me and was my boss at the MIT Chemical Engineering Practice School in 1936, wound up being president of Standard of Indiana.

FINE: It seemed like MIT had the reputation of being the pipeline to Du Pont.

REED: They did. I was the first fellow that ever went to General Electric. You know, at that time they would say, "What's a chemical engineer doing at General Electric?" That's why I was so fortunate, to be the first one there—there was no competition.

FINE: People still don't understand what chemists have to do with General Electric.

REED: I hope they do more and more because you know it's such a big polymer operation today. You talk to the average person and they wonder what chemists are doing at General Electric.

Another one classmate of mine, Jerry McAfee, wound up being the CEO of Gulf Oil. He retired about the time they were taken over by Standard of California. Nearly all of my classmates in that class wound up with very good, very responsible positions in the chemical industry. Chemical engineers were flying very high as the petrochemical industry developed.

FINE: When were you interviewed with Du Pont? Did you meet Paul Flory?

REED: No. I was interviewed at their Belle, West Virginia plant which dealt with polymer production. That was the site where they were planning to do the big production of methylmethacrylate—called Potolite in those days. That was the first name for the acrylate polymers. I think that is a defunct name now, but Potolite was what they called it then. And I was interviewed also by Standard of New Jersey, and several others. But I wasn't ready to leave until the G.E. job came along.

FINE: So your job at G.E. from the first was 100 percent to get this direct process?

REED: I don't think there would be any place in the world that would manage these things the way G.E. did in those days. I mean Marshall in the Chemical Section, or whatever you called it in those days. It was a group of chemists, under Marshall, who had this process under development. Rochow had done the original chemistry where he passed methyl chloride through a fixed bed of silicon copper alloys and came out with methylchlorosilanes. It was very interesting. A fellow from Brooklyn Poly [Polytechnic University] has just come out with a text that reviews the history of the polymer industry up to 1960.

FINE: [Herbert] Morawetz?

REED: Yes. It just came out (9). I bought that book because I'm interested in keeping up with developments in the polymer industry. There is practically nothing after 1960 in it. He has one paragraph devoted to Rochow's work. I hadn't realized it but the first use of copper silicon alloy was back in 1895. He had unearthed something that far back where they passed chloroform over silicon alloy and HCl and they got some kind of reaction that did not amount to anything then. But the reaction of chloride compounds with the copper silicon alloy was in the literature.

Rochow finally got an alloy that turned out to work and as with so many inventions, there was some luck involved in it. But he did this the way a chemist would normally do it—by packing copper silicon alloy or a mixture of copper and silicon into a tube and having a heating element around it, vaporizing methyl chloride, passing it through the bed and condensing the product. Then he had to analyze that and he analyzed it by fractional distillations, and separated out all of the various monomers. Using these monomers, Rochow did his original work on the methyl silicone polymers. Well, this is a highly exothermic reaction and if the rate of the reaction is too fast, you get a lot of caking in the tube and the tube is blocked and you can't get any more gas through it. The problem of scaling up this from a small tube maybe an inch in diameter, to something where you could produce pounds per day, rather than grams per day, would be a four hundred and fifty-fold increase in scale from where Rochow started out.

It was obvious to me that the thing to do was to try to develop a fluid bed catalyst process because that was the answer for getting better heat transfer and better temperature control. So my first work was along two lines. First, to develop a better and more effective process for the reaction itself that could be scaled up i.e., to develop a different kind of reactor, and, second, scaling up the distillation columns. Well, before you could scale up the distillation, it was obvious for a chemical engineer—perhaps not to the chemists who weren't thinking that way—that you had to have vapor-liquid equilibrium data on the various methyl chlorosilanes. Again, one of the first things I did was to set up a program to develop the necessary vapor liquid equilibrium data on the different chlorosilanes. I had a young man who was graduating in 1942 in Course 10. He was an outstanding student of mine by the name of Jerome T. Cole and I talked him into coming to Schenectady in the G.E. Research laboratory which I thought was quite a coup at the time because, as you said, no chemical engineer had ever gone to G.E. But he knew me and I was able to get him over there and his first job was to develop experimental data on vapor liquid equilibrium. When we got that, we were able to do a rational design of a distillation column—batch distillation column for separating the methylchlorosilanes—and we were able to place an order with the Foster Wheeler people for a six inch diameter packed column which was up from maybe one inch that they were using in the laboratory to separate these compounds from the very small scale reactors.

FINE: What kind of packed column?

REED: This was called a Stedman column and Stedman packing had been developed by Foster Wheeler for use in the petroleum industry. It was really the first structured packing material that enabled them to develop large scale packed columns of very high efficiency with what we call the height of a theoretical plate of maybe a few inches, rather than a few feet which was common with packing before that. So we were able to order a six-inch Stedman that put us into separating pounds per day instead of grams per day.

FINE: When you say these things were obvious to you, was it because the petroleum industry was doing a lot of this?

REED: Yes. I was trained as a chemical engineer and I saw similar processes in the petroleum industry.

FINE: It's the cross-over. It was obvious to you but it wasn't obvious to the G.E. people who didn't have the experience.

REED: They didn't have experience. They had never heard of fluid bed reactors and the chemists knew very little about distillation on an industrial scale. It was like taking candy from a baby.

FINE: It was a classic example. They never had a chemical engineer?

REED: They never had a chemical engineer. I say that I was really fortunate in having this combination of training and no competition. So I came to the attention of the right people in management. They were impressed.

FINE: As far as polymers, was their sophistication similar to yours?

REED: Oh, they were terribly sophisticated in polymers. They had excellent people, and I remember after being there for a month, Maynard Agens invented a silicone rubber. They had also a few people in the lab who were very skilled in compounding. Again, you may turn the instrument off, but they called it "shit-mixing" and the G.E. research laboratories were very skilled in mixing anything under the sun in rubber. They had just been working on polyvinylchloride. They did excellent work on polyvinylchloride and, of course, they had been studying it as insulation for years. They had been studying rubber insulation for years so they were very well equipped to take a new rubber, and put it through its paces as far as compounding was concerned. They tried putting everything into it and were able to get many good patents in the very early stages. A fellow by the name of Gil Wright was one of the world's best compounders. He knew all these obscure kinds of things that you could mix in.

FINE: Was that all going on in Pittsfield?

REED: No. This was all in Schenectady. There was some rivalry developing, not in the Pittsfield Plastics Department, but in the Transformer Department which had a very good laboratory and which also had many people who wanted to see the company move forward in chemistry and chemical manufacturing because they were ambitious chemists. We had a very good director there by the name of Guilford under whom PCBs [polychlorinated biphenyls] were developed for transformers. That was, of course, a revolutionary development in its day and all the work on the polychlorinated diphenyls was done in the Pittsfield laboratory. They were oriented towards the developments of new materials for use in electrical apparatus. So they had started a little program of their own on transferring the lab process to a larger scale, and they even built a small pilot plant. But they never tried to apply the fluid bed catalysis process. The interesting thing is that I built a fluid bed unit that, on a small scale, could turn out a couple of drums of this material a day. That could produce several hundred pounds of the material in a short period of time. That greatly impressed the G.E. management.

The fluid bed process is a very difficult process to scale up. Eventually it worked beautifully in larger scale but on an intermediate scale you have all kinds of practical problems on plugging up pipes and feathering out particles from gas suspensions. It took us eighteen years before we got up to a large enough scale that we could operate a fluid bed process in production. So we had to use for production the very early pilot plants which were simply scaled up versions of fixed bed reactors. We had a laboratory in the Schenectady works, again, that was making insulating materials because this was a major concern of G.E. chemistry. G.E. had gotten into alkyd resin chemistry very early in the game because alkyds were not only of interest as the basis in certain insulating materials, but turned out to be also the basis for coating materials. G.E. developed back in the late 1920s and 1930s, in the research laboratory the early alkyd resins which they then used to make insulating varnishes. From insulating varnishes they went into coating materials and a business developed in the 1930s making alkyd resins for the coating industry. They were skilled in making varnishes and wire enamels and out of the G.E. research laboratory came the Formvar enamels. This was polyvinylformal placed in solution to make an enamel that turned out to be, in its day, the world's best coating for wire because it was a very tough insulating coating that was good at higher temperatures.

The Schenectady lab also did the pioneering work on plasticizing polyvinylchloride for flexible insulation. Evidently, this group, the insulating materials group, was very interested in seeing the company advance into chemical manufacturing. This is the second group. The first group I talked about was transformers. They aspired to get into chemical manufacturing because of their experience with chlorinated biphenyls. The second group that I am talking about came up through the insulation side. They were not oriented towards any one particular type of electrical apparatus, but more toward rotating apparatus rather than transformers. They were responsible for mica insulation which was of great importance in turbine generators and large motors. G.E. had integrated very early into making its own insulation for turbine generators and for large motors because it turned out that the quality of the insulation was critical in the scaling up of rotating electrical equipment. They were responsible for making insulating varnishes from mica, and coated cloth which was used as insulation in many motors and in cables. They had a big cable manufacturing plant in Schenectady. So this was a group that was oriented towards insulating varnishes. They knew about alkyd varnishes and they knew about alkyd coating. They were physically located near the research laboratory that was trying to develop silicones. So the lab tried very hard to take the responsibility of scaling up these processes from the laboratory to commercial scale. When they succeeded in taking on the responsibility, they developed a reactor called a stirred bed reactor where a large cylindrical vessel is equipped with a wormed screw rotating inside it that keeps the powder turning over and gives better heat transfer and temperature control than a fixed bed reactor.

FINE: This was a batch process?

REED: This was a batch process.

FINE: Like a blender?

REED: Yes. Like a blender, but on a vertical scale. A vertical pipe with a rotating screw inside of it. Cooling material flowed around this pipe on the outside and a condenser was on top of it and a boiler for the methyl chloride. This was the reactor which we had to use in the first plant because the scale of operation was such that we simply could not use a fluid bed reactor at that point in time.

FINE: Did you recognize this or did you fight for the fluid reactor in the first place?

REED: We recognized it. It was simply too small a scale for fluid bed at that time. I was in charge of the plant design on the first plant. We had to go with the stirred bed reactors. But we knew that we would come to the point when demand for silicones would be large enough to call for a bigger plant where we could put a real fluid bed. If we had built a fluid bed reactor, only one and a half to two feet in diameter, it would have produced vastly more chlorosilanes than we could sell.

FINE: Oh, yes. The market wasn't big enough then. You could have developed it.

REED: We could have developed it immediately if the market had been there. But what do you do with all the chlorosilanes that were coming out.

FINE: So here again this was an opportunity for using your economics background.

REED: Yes. That's right. We just couldn't do it then.

FINE: What was the problem in building a modified, small fluidized bed reactor.

REED: The problems were just practical: to keep it running when output had to be small because the amount of chlorosilanes that we could sell was just a few hundred thousand pounds a year at that time. Unless we were using millions of pounds a year, the fluid bed process as we understood it at that time, could not be used. It was impractical on that small scale due to a whole series of problems involving builders, small pipes blocked by gas solid suspensions, et cetera. It is only when the reactor diameter is one and a half to two feet, in this particular reaction, the fluid bed process can be used. Now distillation was quite a different proposition.

We understood distillation so thoroughly that we put some distillation columns in the first silicone plant in Waterford, New York in 1947 that are still being used today. Now, we have of course, many additional distillation columns that have been brought in since. But this indicates that we understood which materials to use for construction.

FINE: So it took five years from the time you arrived until the Waterford plant.

REED: Yes. Five years.

FINE: You transferred the company operations from the research laboratory with that particular group to the full plant?

REED: Again, it was recognized that the best way to transfer technology is to transfer people and that's why I went to the Insulating Materials Department because they were given the responsibility for designing the first plant.

FINE: How much credit do you really give Marshall?

REED: I give Marshall tremendous credit because he sponsored the whole project and kept with it. Marshall was a radical thinker. He would try anything. He wasn't hide-bound at all. He was really in a class by himself as far as chemists trying to do new things.

FINE: Was he a chemist?

REED: Oh, yes. Marshall was a chemist from Princeton.

FINE: Was he easy to work with?

REED: It depends on who you talk to. I always got a great kick out of working with him. He was not what you call an organization man. He was getting down directly to the business of the bench. Marshall would think nothing of coming in at 8:00 at night and assuming command of whatever experiment was going on. Or trying something new.

FINE: To get a little bit more about Marshall—some of the people who went on in G.E. polymers said that although at the early times he pushed people into the business, later on he wasn't very helpful for the business.

REED: I think that was right because Marshall got off on a lot of other things. I felt that he wasn't particularly helpful in supporting silicones. When I was on the other side of the fence, running the Silicones Department, he had a lot of other irons in the fire. For example, boron chemistry was very exciting at that time. They were into new chemistry on air conditioning. Their job was to develop new things for General Electric overall and silicones was just one more thing that they had thought of and that they felt should go on and always spent more money on its own, did more R & D on its own.

[END OF TAPE, SIDE 3]

FINE: Let me go back to this discussion of Marshall and Gene Rochow.

REED: Rochow, again, had his own laboratory under Marshall and when Rochow got this result—the fairly good yields in the bench process, Marshall put more effort onto it and had a couple of people to try to scale it up. Rochow's first reactor, I think, was three-quarters of an inch in diameter, or something like that, and Marshall started in 1940 to try to build larger diameter units—fixed bed units. He realized that he had to have more material in order to develop sufficient quantities of varnish-distilled polymers were used as varnishes at that point to coat cloth or glasscloth—and test them as insulating materials in a small machine. Marshall realized that he had to have more material so he did his best at that time by trying to scale up the fixed bed reactor. When I arrived in 1942, they had built a small stock which they were very careful to distribute in small quantities to people who would use it sparingly, maybe a few gallons of the different materials that they had very laboriously prepared in small fixed bed reactors and small diameter distillation columns.

Rochow, in the meantime, had gone on to trying to develop the phenylchlorosilanes using chlorobenzene. He finally hit upon silver-silicon as a catalyst. It's a much, much more difficult process compared to the methylchlorosilanes. I don't think they had ever gotten it up to the fluid bed stage. But I really should not say, because I haven't followed it in the last five or six years.

FINE: Did they get on well together?

REED: Rochow and Marshall? I don't know Rochow's opinion on it, but I think he did well with Marshall. He was a very independently minded fellow and so was Marshall.

FINE: That's why I asked the question.

REED: Certainly I don't think it interfered with Rochow's creativity because Rochow, if he didn't like something he could turn right around and go down to see [William J.] Coolidge. That's the way the laboratory worked in those days. Rochow felt a greater responsibility I think to [Louis] Navias than he did to Marshall because I think Navias brought him in to work on inorganic chemistry.

FINE: That's right.

REED: And he always felt a certain responsibility to Navias and Marshall didn't have Navias involved in the silicone work at all. You'll never find an organization the way that one was, anyplace. But if you want to look at Marshall's overall record, and since you have asked me, my opinion is that Marshall had an outstanding overall record as an industrial chemical research director because he should be given a lot of credit for Formvar enamel, silicones, permafills (although G.E. never really exploited those and later one of the G.E. chemists did, Bob Freepol of the Lockite Corporation), diaryls, polyxylenes. Not very much on polycarbonates, although he did provide a lab. There are very few directors who have a record like that and it can't be attributed to broad strategic planning of some kind. It was just because he had some good people there and he was always, as I said, in favor of trying anything. He had an unusual type of mind.

FINE: Who was your boss?

REED: Marshall was my boss when I first came. Then when I moved to Insulating Materials, my boss was Ed Pining, the general manager of that department. A major change took place in 1945 when General Electric set up an organizational structure that made it clear that it was going to support chemical manufacturing on a more serious basis. They had a study made by Dr. [Zay] Jeffries who came out of the Lamp Division in Cleveland.

FINE: Jeffries was also from Case.

REED: Yes. Jeffries was also from Case. He was a 1914 graduate and taught at Case for a while. He was a metallurgist and had done some solid work for the Aluminum Company of America, as he did all his life. He worked primarily for the Aluminum Company and also for the Lamp Division as a consultant. He had many interests and one of his big achievements was

in bringing the tungsten carbide manufacturing process from Germany to this country and setting up the Corraloy Company as a subsidiary of General Electric. Later on, he was taken to court because of the anti-trust laws which were much more stringent than they are today. Jeffries was commissioned by Charlie Wilson, who was then President of G.E., to make a general study of General Electric's potential in going into chemicals. This had been much talked about in the Research Laboratory, in the Insulated Materials Department, in the Transformer Department in Pittsfield and the Lamp Division, which was in a way also in chemistry, because it had its own plant for refining tungsten ore to sodium tungstate in Cleveland. That was another part of G.E. that had always impressed me and it was in the back of my mind when I went there, because many faculty members at Case were consulting with the G.E. Lamp Division. As an undergraduate we took a number of plant trips around the country one summer and we went through the G.E. Lamp Division in Cleveland and they had a brand new plant for converting tungsten ore into tungsten that was terribly impressive to everyone. A brand, sparkling new plant.

So Jeffries was commissioned to make this study and he suggested setting up a Chemical Department that would have as its basis the Plastics Department of General Electric and the Insulating Materials Department i.e., to combine those two. They were not departments, they were really divisions I guess at the time, or operations. Plastics was in Pittsfield and was involved in custom molding and in making phenolic compounds and had grown up as a result of General Electric's early years in making phenolic molding compounds. This was another side of the G.E. business that I haven't touched upon until now. General Electric had developed their own molding compounds and made them in a plant at the Pittsfield Transformer works. They had five plants scattered in the East. They were not only molding Bakelite compounds as they were called at that time. G.E. had its own trade name for its phenolformaldehyde resins: Textolite molding compounds. They molded other plastics at that time as well, cellulose acetate primarily. Then they got into polystyrene which came into the picture in the 1930s. Those were really the only significant molding compounds of that time. The Plastics Division that I had come to in my first visit was going into the manufacture of phenol because they had this phenolic molding compound plant. That phenol plant was just being completed in 1945 while this study was in progress, so Jeffries suggested combining them. He became vice president of what was called the Chemical Department. They didn't bring metallurgy in at that point, and Jeffries continued his role as Chairman of the Board of the Carboloid Company, a subsidiary to General Electric. Later, in 1951, when Carboloid folded, they named it Chemical Metallurgical Division.

In 1946, the department headquarters were set up in Pittsfield. The department was looking for growth opportunities and they found one in the big growth possibilities of silicones. Silicones was still under the aegis of the Insulating Materials Department. Jeffries saw that if they were going to succeed in silicones they would have to have a new technology sponsorship in the Chemical Department itself, so he set up a Chemical Engineering Department and made me the general manager in 1946. My first job was to continue designing the first full scale silicone plant, but the business responsibility for that development stayed with the Insulating Materials Department. The Chemical Engineering Division was charged not only with getting our silicone plant on stream, but also with developing new polymers which were being

investigated by the laboratory of the Plastics Department in Pittsfield under this fellow D' Alelio. D' Alelio had gotten upset with General Electric for not moving fast enough in chemistry. He left, I guess, in 1944. He went with the Prophylactic Brush Company in North Adams, Massachusetts that was trying to get into chemistry at that time. He continued many very innovative projects in chemistry that were beyond the capabilities of that company which was simply very small. Later on D' Alelio became a professor of chemistry at Notre Dame. He was in a class by himself as a polymer chemist.

FINE: Something you just brought up—the need for a lot of capital to introduce new plastics, which made G.E. a good candidate for such activities compared to small companies.

REED: There's been all this talk about G.E. making excellent chemical innovations which they would then turn them over to somebody else who would exploit them and profit from them. G.E. felt that they had made a great innovation, Formvar wire enamel, but Monsanto and others made a lot of money on that because they produced the polymers. And in polyvinylchloride it was the same thing. It was a very successful insulation in its day. G.E. had done a lot of work on it and again, [BF] Goodrich [Company] became the main developer. With that experience G.E. was poised to exploit the next innovation internally, while there were all sorts of arguments within the company as to whether this was a wise thing to do or not. The apparatus people at G.E. were dead set against it because their customers were the big chemical companies and some of the chemical companies brought all kinds of pressure to bear on G.E. not to go into the chemical business. Union Carbide was one of the worst. They threatened to take their business away from G.E. if they integrated into chemicals. But opinion, fortunately, prevailed that G.E. should grow by exploiting its inventions from within and silicones were such an unusual opportunity that they became the first vehicle of that policy.

FINE: Their one big champion, was it Jeffries?

REED: Jeffries was a big champion. Of course the research laboratory was a big champion as well. And the Insulating Materials people were champions but ambivalent in a way because it fell on their shoulders to take the responsibility and spend the money, they had to be pushed some. Ralph Cordiner was a good proponent of going into the chemical field.

FINE: You were in charge of the Chemical Engineering Department? What were your biggest challenges?

REED: My main job as general manager of this new division, which was called The Division of The Chemical Engineering Department, was getting the silicone plant built and also for some of the work that was going on in Pittsfield.

FINE: The Chemicals Department in 1946, dealt with silicones, and this new venture into phenol manufacture.

REED: That's where we were in 1946.

FINE: They went into phenol to support their molding compounds operations, to provide them with raw materials.

REED: Right. This decision was made to get into phenol was taken back in 1938 or 1939.

FINE: Developmental work went on?

REED: The plant was started in 1942. Again, there was strong disagreement within the company as to whether they should do it or not. The Insulating Materials Department was very much against it and it turns out they were right. It was one of the most abortive ventures that the company ever got into. When you think today of trying to get into phenol you should think of a very large plant. The first nameplate design built by the Lummus Company, to G.E.'s specifications, was seven and a half million pounds a year. At that time U.S. phenol production was maybe one hundred to one hundred and fifty million pounds a year, so that was a larger proportion of the market than it would be today. But it was still a small pie even when it was doubled to fifteen million during the course of early design and went on stream in a nameplate capacity of fifteen million pounds. It wasn't however the economics of the project that killed it, but the fact that the process just would never work on a larger scale. We could not overcome some of the corrosion problems in the reactor itself. To make a long story short, it never really operated and was finally shut down in 1949. It made, a few million pounds of phenol but it could not be kept running for long because of the reactor problems.

FINE: Of course by 1949—

REED: In 1948, Hercules [Incorporated] started doing work on the cumene process. I went down, at a very early stage, and went through the Hercules cumene process and it was quite clear that that was what was really the process for the future. It took a great many years before the first big cumene plant was built and a few more years before Dow shut down all their high pressure chlorobenzene phenol operations and Monsanto and Reichhold, their sulfonation phenol process.

FINE: Anyway, we have a Chemicals Department, silicones, and you are building a plant. It almost seems like a meteoric rise. You are out of school four years and you are the general manager of this new Engineering Department. Did you have a sense of your presence in the company at that time?

REED: No, I really didn't.

FINE: You still considered yourself a technical person and an engineer?

REED: I still considered myself a technical person, although I was very much aware of the economics of plant design. I remember during the course of that plant design they started off at one capacity and I remember one weekend making a rough calculation on the overall plant and I said, "It will never go unless we double it." So we doubled the size of the plant on that weekend.

FINE: That turned out to be the right size?

REED: Yes. If we had not done that we would have been in big trouble because some of the operations in the plant are scale sensitive. Distillation, for example. A plant that is going to have a capacity of five million pounds a year, and distillation columns that balance the five million pounds a year, it can be doubled by spending only 40 percent more. It goes upwards by the square root, roughly. It takes a lot of money for such an expansion, but if you have any confidence at all in what is going to happen to the business, then you gamble on a larger plant because you then take advantage of the economies of scale. Some of the processes in the early plant were scale sensitive and others were not. But the total infrastructure was very sensitive to scale—the water supply, waste disposal, inventory package and so on and particularly the distillation.

FINE: Did you have a hard time when you made these decisions or were you left more or less on your own?

REED: No. It was more or less on my own. I was working with the Insulating Materials people, and with Jeffries.

FINE: Rochow was gone at this point?

REED: Rochow left in 1946, I think. Rochow never had any relation at all with engineering.

FINE: Because it didn't interest him?

REED: He was in another field. I think Rochow started in doing some work on boron chemistry at that time. Boron just took over for a while, in the lab. They said, "Maybe we can do the same thing with boron that we did with silicon." I could understand that feeling on the part of chemists. They felt that they had skimmed off the cream of the silicones by that time. From a chemical point of view, the cream was skimmed. It was coming so fast from 1939 to 1942 or 1943 and from that time on it was a sort of a development job and the chemical thrill was over.

FINE: You got the plant going, I guess, about 1949 or 1950.

REED: The plant got going in 1948.

FINE: How was the business doing at that time?

REED: That's a totally different story. From 1946 on, when the Insulating Materials people had been given responsibility for the project, it was realized that some product development work was necessary to build the first plant to a size that would be economically attractive. There is always a chicken and egg proposition when developing a brand new product like silicones. There may be exciting applications, but they may add to just one hundred thousand pounds a year. A lot of excitement and a lot of enthusiasm, but one hundred thousand pounds a year is nothing as far as an optimum size plant is concerned. There must be outlets to support the building of a new plant and the plant is necessary to meet the demand for possible new applications. There is always a gamble on that first plant. If it is too small, it is not going to make a profit. If it is too large, and it takes too long to develop applications to utilize the full capacity, it is going to lose a lot of money. So it is very much a gamble and a lot of judgement involved on how large that first plant should be. Also knowing that process technology will continue to change, it is not advisable to commit oneself at that point. So, again, I considered myself very fortunate to have had that experience on the fundamentals of plant design.

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FINE: Perhaps we ought to go back to silicones and pick up something which interests me: patents. It struck me curiously that you had actually started out doing some things for Dow before G.E. and of course Dow is the principal competitor in silicones. Yet, there seems to have been all kinds of interrelationships with Dow over the years. Could you talk a little bit about patents?

REED: The work I did for Dow, was on the presumption that there might be a reaction between silicon tetrachloride and methane. There never was. Dow, in the meantime (1942) had started to work with Corning. Dow's work was entirely on scaling up the Grignard reaction as a means of increasing the quantities of silicone. They knew nothing at all about G.E.'s direct process until Rochow announced it (10). They also were doing a lot of work on phenyl chlorosilanes. I can't give you the details of all the disputes between [J. Franklin] Hyde and Dr. [Eugene C.] Sullivan, Rochow and the rest of them on the early patent situation. It never came to the courts as I remember, but it was subject to a lot of negotiations. Jeffries was very broad minded on patents. He had a lot of experience and in 1946 said that instead of letting the lawyers profit from all of this, we ought to cross license. So in 1946 Jeffries and Dr. William F. Collings of Dow Chemical who had worked on silicones, a Case graduate and almost a classmate of Jeffries, arranged the cross licensing. We had a complete cross licensing as far as the companies were concerned. This never did assuage the feelings of the scientists and the chemists who were involved, but from a business point of view the whole thing was resolved in 1946.

FINE: Dow was cross licensing everything?

REED: Cross licensing of everything. We had a general exchange with Dow for about three years. Then Jeffries got into trouble on the Carboloid patents. G.E. came into Court under the anti-trust registration, on Carboloid and on some lamp cases, and the G.E. lawyers decided that they could not take a risk on the same type of cross-licensing, which includes a certain amount of know-how. There was a lot of fencing on how much know-how was exchanged between the two companies, but we could not have a general, open-ended cross-licensing as we had originally. We had to terminate that. It was terminated in 1949 and from that time on it was on a case by case basis. There was then no general cross licensing agreement between the two companies.

FINE: Now this cross licensing—that was just an even exchange. Neither company had to pay any more.

REED: At that time it was an even exchange.

FINE: Did G.E. really have anything besides the direct process?

REED: Oh, yes. G.E. had a lot of things. G.E. had the basic patents on rubber, on some methyl silicone oils, and on straight methyl silicone resins. They did not have patents for methyl phenyl resins which turned out to be very important.

FINE: So you would say it probably was pretty even?

REED: Oh, yes. As far as patents were concerned. But the direct process was of overwhelming significance and rubber was terribly important—more important as the years went on. They didn't realize how important the rubber would be in 1946.

FINE: Did Dow manufacturing and engineering process design help you out?

REED: Not a bit. Dow had much experience in exploiting new products. W. F. Collings, who was the chief executive of the Dow Corning Corporation, had been with Dow and was responsible for Dow's ethyl cellulose development which was a classical development of a new polymer product. Going out and working with customers, developing new applications, and so on. Dow had a tremendous amount of experience and we were handicapped by our lack of commercialization experience in comparison to Dow. It is very interesting to note that the relative market positions of the two companies that finally emerged in the early 1950s remained unchanged until today. Dow is around twice the size of G.E.

FINE: And no one else has really gotten into in a significant way?

REED: Of course, we licensed the Europeans and the Japanese, and when those patents ran out, some eventually became important in the U.S.A. Bayer in Germany eventually teamed up with Stauffer in this country. Union Carbide came in entirely on their own, on an entirely different track, in 1950. They developed copolymers of organics and they did a very good job of development from that whole different point of view.

FINE: You credit Dow's lead to their ability to tie into the market rather than anything at the technology or production end. Let me get back to your career. You made a big switch then, after 1952, when you moved into the general manager's role.

REED: At the beginning of 1948, Ralph Cordiner brought a management expert by the name of Harold Smitty. I don't know if you remember him?

FINE: I sure do.

REED: Smitty was an expert on management and in a class by himself, again, as my book goes. He was first brought in to survey general management problems in the company and was then made vice president and general manager in charge of the Chemical and Metallurgical Divisions, succeeding Jeffries who was scheduled to retire in 1949. Smitty was only there for six months but he made a tremendous impact during that period on the Chemical and Metallurgical Department as far as its organization was concerned. He brought me from Waterford. I had originally been in Schenectady but was working in Waterford then as manager in the Chemical Engineering Division. He brought me over to Pittsfield and enlarged my responsibilities to include all of the laboratory development work that had been going on in the Plastics Department at Pittsfield. So I had responsibility for chemical engineering in the new silicone products plant and the development work that was going on in plastics in Pittsfield, and the development work that was going on in the phenol plant at that time. That was a considerable increase in responsibilities.

Then Smitty was moved out of that job to become head of the Air Conditioning Department of General Electric. G.E. was trying to see what they could do to become a factor in the air conditioning business. He was succeeded by Robert L. Gibson, who was quite an important figure in the early history of the Chemical Department in General Electric. I worked under Gibson for three years in Pittsfield as head of the Chemical Engineering Division of the Chemical and Metallurgical Department, with responsibilities for engineering development at Waterford and for engineering development and new product development in Pittsfield. We were trying to see what we can do to find another silicone at that time—to move the company forward in the chemical business.

FINE: Did you have much to do with the other operations in Pittsfield at that time?

REED: Yes. I did. The Plastics Department had a product and process development operation that had developed the phenol process and was working on developing other new polymers. They had delved into emulsion polymerization and were trying to develop molding compounds that would compete with polystyrene. They were also trying to develop some of the patents that D'Alelio had come up with early in the 1940s. Also, right after the war General Electric experienced a tremendous shortage of sheet steel and it was extremely important for them to have sheet steel if they were to continue making refrigerators—to maintain a market position in the refrigerator field. It was decided to use what they called “phenolic laminates” instead of sheet steel for making refrigerator doors.

G.E. was making phenolic laminates for many years, primarily for decorations, what is generally known to the trade as formica. G.E. called it Textolite. A number of layers of paper saturated with phenolic resin was put together and covered by a top sheet printed with a design on paper and overlaid with a transparent sheet of melamine resin to give it abrasion resistance. The whole thing was put in a press and came out as a decorated phenolic. We had been doing that for years. It was decided, in order to keep the Refrigerator Department going, to build additional capacity to make phenolic laminates that could be molded into refrigerator inner doors. A plant was erected in Coshocton, Ohio as a venture of the Chemical and Metallurgical Division. Unfortunately for them, as steel became available in the early 1950s, it was a much better material than phenolic resins because it could be deep drawn and give a much deeper inner door. You just can't draw phenolics very readily because they are three-dimensional polymers. G.E. wasn't into any other polymer as a major structural material at that time at all. So this product was forced out of the refrigerator door business, which accounted for a big percentage of its business. They produced decorating laminates and electrical insulation sheet laminates for many years. I was responsible for the chemical engineering at that plant.

One of the things we did was develop a process for making what we called mica paper. Small mica crystals can expand like popcorn. There is water of hydration in mica and when it is heated up, it expands and if it is dried properly a suspension can be made from which a sheeted paper of mica can be produced and used as an electrical insulating material when impregnated with varnish. That process is still in use today. That is an example of the type of development that we did in the Chemical Engineering Division of the Chemical and Metallurgical Department. And we were responsible for silicone engineering developments and other polymer product development as we hoped to find another silicone but failed to do so, at that point in time.

FINE: You did find polycarbonates.

REED: That was later.

FINE: In the meantime, you actually went into general management.

REED: In the meantime, they decided to set up the Silicone Products Department as a business department and I was appointed general manager and that's when I moved from chemical engineering and product development into general management.

FINE: Did that give you any pause? Your whole career up to then had been as a technical person and you were now moving into management.

REED: I had been exposed to product development all along. They were still important to the development of the silicone business. It really did not give me much pause at the time, although I was certainly a novice in commercial chemical development.

FINE: Did you, in all of this business, ever think of yourself as a marketing person?

REED: I became increasingly aware of the importance of marketing as time went on but I was quite aware of the importance of the development of commercial markets for chemicals for many years. That had started to take place almost as a profession in the 1930s, pioneered by companies like Cyanamid, Du Pont and Union Carbide who had made new polymers and wanted to find uses and markets for them. So the profession of what is called "chemical development" emerged then. The job was, to get a cadre of people who would take these new products to customers and say, "Here, they are. What are your problems? What can you use them for?" And then go back to research and development headquarters and say, "Now here's this opportunity. How can you change our product or adapt our product to the use that this potential customer has told us about?"

FINE: So when G.E. set up a chemical development operation it was following in a mold that had already been set by other chemical companies.

REED: The chemical development operation had been set up by Robert Gibson. That assignment was given to the Chemical Engineering Division originally and then, as I moved from the end of that Chemical Engineering Division, Gibson decided to put even more emphasis on the market development problems which were also the products which were in the development stage. But there was nothing to develop at that time. They were searching for something new when polycarbonates came along in 1953 or 1954. Dan [Daniel W.] Fox discovered them in Schenectady in 1953 and it became apparent then that that they had the potential of a major new product. The chemical development operation then was given the responsibility for developing polycarbonates. This was in 1954 when I had moved to the silicone business.

FINE: You were more or less a watcher in this, rather than a director?

REED: I had nothing to do with the original development work on polycarbonates. It was done by the chemical development operation which was set up by Bob Gibson as a successor to the Chemical Engineering Division which had that responsibility previously, as well as the engineering development responsibilities for a number of these processes.

FINE: Before we leave silicones, I guess it was during your tenure that the silicone business changed from losing money to a profit business. Was it ever in danger of being shut-off because it wasn't making money?

REED: I would say that it was in constant danger of being shut off because it was losing money and it turned around and went into the black in 1957, I think.

FINE: That was much longer than anyone had thought in the first place?

REED: It was much longer, but quite par for the course when you look at the chemical industry in general.

FINE: Do you think if G.E. knew it was going to take that long, they would not have gone into the business?

REED: They might well not have, but they weren't familiar with the broad problems of developing new polymers as for example, Dow Chemical was.

FINE: Did Dow become profitable earlier?

REED: I think they became profitable earlier because they had more volume. Probably three or four years earlier. But they had been through the process of marketing development several times before. They had been through it in ethyl cellulose, in polystyrene, while G.E. had never been through it before the silicones. They had edged into it on a much smaller scale in phenolic molding compounds and alkyds, but only as a sideline to the manufacture for internal use. There has always been the problem of captive use versus sales to third parties, the argument being that we're going to manufacture this internally because it gives us an advantage over the competitors. Now if we could go out and try to sell this thing to our competitors and to the market in general, we will undercut some of our product advantage because our new material gives us an advantage in the final product. On the other hand, the advantage of economics of scale by adding those external sales to internal sales, is extremely important as far as the cost of manufacturing goes. It is always a thread of contention in a large company like General Electric which starts from captive use of a new material to get an advantage over its competition in the final product while the department which manufactures the new material and wants to expand prefers to tackle the demand at large. You've got that constant tension. We had it in silicones in the early days.

FINE: What attitude did Cordiner take towards this in the early days?

REED: He was generally supportive of the silicone business but quite impatient, of course, to see it turn into the black.

FINE: Was there any one turn around thing or was it just the fact that the volume gradually went up?

REED: It was just that the volume and the applications had to expand into the fields in which we were experts. Broadly speaking, we had a great advantage in bifunctional silicones because theoretically the direct process, running at maximum efficiency, produces dimethyldichlorosilanes. Any products that are based on bifunctional linear polymers, are made at an economic advantage when based on the direct process. These turned out to be the cases in rubber and oils. So we were very strong in silicone oils and rubbers and it took us some time to develop applications in the field. We were less strong in resins which are three-dimensional products where the Grignard reaction was quite effective in leading to a variety of monomers for three dimensional polymers.

FINE: That's really an interesting point. When you hear that chemistry is really affecting who does what in which market?

REED: It affects your economics greatly. In more recent years, development work on the direct process has continued and they have made vast advances in the chemistry and in catalysis. The yields of dimethyldichlorosilanes have increased to the point where the capacity they have in Waterford these days for refining the crude mixture of methylchlorosilanes into the purified monomers, is very large. When you have 100 percent yield of dimethyldichlorosilane out of the direct process, a distillation column has infinite capacities because there is not any separation to be effected. And as you approach that point, the capacity of the column in place goes up exponentially. Today, I am sure that the Waterford plant is the most efficient manufacturer in the world of dimethylchlorosilane monomer that goes into silicone rubbers and oils.

FINE: You were saying earlier that Dow Corning had the rights to use the direct process, but they still didn't use it effectively.

REED: They started doing development work on the direct process in 1948, practically the minute they got cross licensed. But they were on the Grignard process for a number of years before they changed over. They changed over in the late 1940s or early 1950s.

FINE: It is hard to imagine a Grignard process being economical outside of a pharmaceutical company.

REED: It certainly is. That's why the direct process was such a great boon to their business.

FINE: It is interesting too, in a modern context, that there would be a cross fertilization. The Japanese have advanced dramatically in their success in competing with us, because they have shared ideas between companies. You would call that collusion in this country and you would probably be in court if you sat down and planned who was going to build a plant for what.

REED: That's right. We had an interesting history in Japan. We did not license the Japanese at the beginning and the Japanese went ahead on their own and developed a lot of good silicone chemistry. Dow Corning had quite a different philosophy with them. They went into a joint venture early in the game in Japan and they went into joint ventures in Europe whereas we licensed companies to use the G.E. technology and these same European companies had to deal separately with Dow Corning to get any needed licenses. As time went on, our European licensees came back to bite us as competitors and Dow Corning, having joint ventures in Europe, did not have that same problem. Finally Dow Corning went ahead and bought out their European partners and I think they have now 100 percent ownership in Europe. In the meantime we formed a joint venture in Japan which has been very, very successful in silicones. But the Japanese did a lot of their own development work. Much of it on the same fundamental patents that G.E. had invented or pioneered but the Japanese went ahead and did all the development necessary to build a plant. As I say, later on in the 1960s, we went into a joint venture with the Japanese and that's what we have today.

FINE: Can we go ahead to 1959 when you took the next step up to become the manager of the Chemical Division? Let's do that and then you can jump back to businesses. Let's try to stay chronological. We should get off of silicones and into polycarbonates. That was your first real involvement with polycarbonates wasn't it, when you moved over to the other—

REED: I went to silicones in 1952 and stayed through 1958. In 1959 Bob Gibson appointed me general manager, of the Metallurgical Products Department which, as I told you, had been folded into the chemical business back in 1951. I left Silicones and was then superceded by this young man who had come with G.E., out of MIT back in 1942, Jerry Cole. He became general manager of silicones in 1959 and I was general manager of the Metallurgical Products Department. At that time they had received the responsibility for developing synthetic diamonds and it fell to my lot to have to do a lot of work in supporting the early commercialization of synthetic diamonds. My experience in developing silicones put me in pretty good stead in supporting that commercial development work that had to be done on

diamonds, which had been, again, developed under Marshall in the G.E. laboratory back in the 1950s. They had started a high pressure research program in 1952 on the assumption that here is a new technology. We are learning how to operate at higher and higher pressures and temperatures, we ought to be able to make some interesting new materials out of it. G.E. was the first to make synthetic diamonds. That was in 1955. Bob Gibson was general manager of the Chemical Department and was continually looking for new things to develop and exploit. He had polycarbonates at that point and he latched onto diamonds and it was very logical to assign this new product to the Carboloid Division.

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REED: Carboloid had responsibility for what was called specialty alloys which were refractory alloys made out of cobalt, tungsten, and so on, that were useful in aircraft engine parts, buckets of gas turbines, et cetera. Carboloid belonged to the metallurgical department of the chemical and metallurgical division and I was the general manager of the Metallurgical Products Department.

FINE: So that took you away from the polymer field?

REED: That took me away from the polymer field, although I always considered that diamonds, broadly speaking, were an inorganic polymer. They had also developed the only other high pressure, high temperature product in the form of boron nitride which was 2-boron nitrides. In one sense high pressure chemistry has been the basis for G.E.'s special materials business. They use that as a rather cryptic name for the diamond business at any plant, because of security problems. But it was probably a good thing just in the same sense that they have taken the word nuclear out of nuclear magnetic resonance.

FINE: I'm sorry about that.

REED: You're sorry for that as a chemist, but if they hadn't done that in my personal opinion we would not have magnetic resonance to sell to hospitals. We wouldn't have it. It would be tied up in law suits in every town of the country.

FINE: I understand.

REED: It was a stroke of genius to take the word nuclear out of that.

FINE: You know, Du Pont's "Better Things for Better Living"—Chemistry has been dropped. Which is very sad.

REED: Chemistry may be in disrepute with the general public vis-a-vis these forty thousand people being evacuated yesterday in Miamisberg [Ohio] after a railroad tank of phosphorous derailed. If there is an accident, it will be a terrible thing for the chemical industry. It will be another Bhopal but no one was killed in this one. I don't think the papers have begun to report on the repercussions of that.

FINE: I think to this day, G.E. is probably interested in finding a phosgene-free synthesis.

REED: We certainly are. We've hoped for that for years. It's been a challenge to get a process for making polycarbonates that do not use phosgene. That's a consummation devoutly to be wished.

FINE: I guess I didn't get the chronology right, but how long were you at the metallurgical end.

REED: I was just there for about ten months. I went in February of 1959 and was appointed vice president and general manager of the Chemical and Metallurgical Division in 1960, succeeding my boss, Bob Gibson.

FINE: So this was the first time you were supervising the polycarbonates.

REED: That's the first time I had any responsibility for polycarbonates.

FINE: What was the state of it by then?

REED: The state of it was that we were just going into the first commercial plant in Mt. Vernon, Indiana. As you know, polycarbonates were invented by Dan Fox. The responsibility for developing them was placed with the chemical development operation that Bob Gibson had set up. Dan was one of the chief scientific people. Neil Christopher, who worked there, was quite a brilliant man on marketing and new product development and did an excellent job in setting up polycarbonates. And Gene Shubert, who was the Manager of the chemical development operations. We might have not mentioned him up to this point, but he worked with me on distillations in the early days of silicones and on the early design of the first silicone

plant. He then remained in the research laboratory and did many different things in the succeeding years until he finally was transferred to the chemical development operation in Pittsfield in 1954 or 1955. Gene Shubert is a key figure in the early development of polycarbonates because he went to Germany and dealt successfully with Bayer. We foresaw a patent dispute coming up with Bayer because they had developed polycarbonates about the same time as G.E. and had a number of basic patents. A litigation would have been inevitable and Shubert did a very fine job in getting a license from the Bayer people on polycarbonates that covered us as we went into the field. That was in 1958. It cleared the way for going ahead with the first commercial plant, even though we had a terrible process at that time. This illustrates another point, that if you are first in the business, and we were the only people developing polycarbonates at that point in time, go ahead and take any process you have in order to get into production and make the product available to customers. Get your market position and in the meantime go ahead with the development process and try to improve the economics of the process that you have. But get your market position first. That's very important. We had the reverse of that situation in silicones. We had the outstanding process first, but no market. We learned and profited from that experience. Polycarbonates benefited vastly from our experience and our learning and know-how in market development.

FINE: Why was G.E. able to cut such a good deal with Bayer?

REED: Shubert just did a good job and Bayer had not. He seized the importance of getting going in the United States. Our license was limited to the United States and we had absolutely no rights to even look at Europe. Bayer was most concerned for Europe and to this day, Bayer retains a dominant position in Germany.

FINE: Not by much.

REED: Not by much. Their position has been eroded over the years by G.E. because they were such a dominant, and almost an arrogant, competitor in their own country. But I think they probably still have the major position in Germany.

FINE: But what about Mobay in this country?

REED: Bayer joined with Monsanto to form Mobay and exploit polycarbonates in this country, a little bit later. But not much later than when we had started.

FINE: Where was the prime technical difference in the patents?

REED: We didn't have any strong patents and I don't think there were any. The details of any chemical importance in the patents slip out of my mind at the moment. We had a totally different process than Bayer. We used pyridine as a chloride acceptor in our early process and I think they were using caustic from the very beginning.

FINE: You really didn't get the patent, as it turns out, at the end.

REED: We would never have been in polycarbonates. Fox's work in seeing what the product might do was totally responsible for G.E. being in that venture. We would never have gone into it if we had simply observed from a distance while Bayer was coming along in Europe. Never. But we had the silicone experience behind us and we had confidence that if we had a new material we would now know how to go about developing it.

FINE: Just as a historical curiosity, what is your version of the Fox discovery?

REED: I never really got into the details of it. Fox was working first in the product development laboratory of the plastics division, as I recall. He then got into the research laboratory for a little while. Bob Gibson and the Pittsfield people were always in close contact with the research laboratory. They were just groping. Gibson, in contrast to many of the earlier people in the insulating materials part of the business, was groping for things to push along because that was the way to make a name in the new division. And he latched right on to it. He got Fox over to Pittsfield and took up the development work in a big way over there. The Schenectady research lab had, I would say, practically nothing to do with the early development of polycarbonates at all. Two years later, in 1956, Al [Allan S.] Hay invented polyphenylether, the oxidative polymerization of 2-6 dimethylphenol, and that was totally a G.E. research lab development and was to Marshall's and Hay's credit. And that was slowed down at the very beginning because they were then so preoccupied with developing polycarbonates. Marshall and Hay had nothing to do with polycarbonates.

FINE: So you came in at this point where you had this new development, and polycarbonates were draining a lot of money.

REED: Yes, polycarbonates were draining a lot of money. But again with my experience in seeing how important it was to get into market development at an early stage, the first thing I did was to push very hard for them to take the polyxylene work from the research lab to development. I said if Dow Chemical had had this material, they would have already spent five million dollars on it, which was big money in those days, not much today. So I was in charge of the chemical development operation as one of the many departments that I was responsible for

as general manager of the Chemical and Metallurgical Division and I insisted that they go ahead. I just went all out in supporting it with the chemical development operation in the Chemical and Metallurgical Division. Many of my colleagues were reluctant, including Fox.

He'll admit it now. I don't blame him. We were not putting as much money into polycarbonates, in the opinion of many of the people working on polycarbonates, as probably we should have. There is always a balance as to how much you can put in. My feeling was that you've got to move over fast, at a very early date, when you have something that was as fundamentally good as I felt polyxylenol was.

FINE: After the silicones were making money at this point, and polycarbonates, did people have a feeling that this was going to be a potentially big market?

REED: Oh, yes.

FINE: People understood that at the outset?

REED: Yes. Well, the people who were closely involved were very optimistic. I remember a little bit before I took over as general manager, they had Arthur D. Little make a survey on polycarbonates in competition with other new polymers at that time. A.D. Little came back with the conclusion, I'll never forget, that if we could get the price of polycarbonates down, nobody would use anything else. Their physical properties were so much better than polystyrene, polyvinyl, and all the other polymers we describe them today as commodity plastics, that they simply do not belong to the same league.

FINE: At this point were people beginning to understand that there was a market—a higher cost market for properties?

REED: That was just beginning to emerge and again, the commercial development work that was in progress learned a lot from what we learned in silicones. How to go out to customers and try to identify opportunities that might be able to accommodate the higher prices for a polymer of better properties. I think you'd find it very interesting to interview Bill Christopher, who is a management consultant now in Stanford. He has his own consulting company and Bill deserves an awful lot of credit for his commercial development work on polycarbonates and Dan Fox would tell you all about that.

FINE: Did G.E. ever consider going into the commodity plastics end or did it always restrict itself to the high value products?

REED: We considered it, but we had so much on our plate with silicones and polycarbonates and diamonds and then later polyxylenols that we didn't do anything about it, fortunately.

FINE: Would you say that polycarbonates really cut the field and introduced the idea of engineering high performance plastics?

REED: I would say that they went along in parallel with a number of others. After all, you had nylon coming along as a molding compound, in those days. We had polyformaldehyde. That was again Du Pont's product. And we had Delrin coming along. So I would say that G.E. sort of did it in parallel, but since it was the only material we had that was new, I think G.E. put more emphasis on it. After all, Du Pont had a lot of other things in their stable—Teflon, sulfonated polyethylene, polymethyl methacrylate.

FINE: This was also at the time when the low pressure process for polyethylene and polypropylene came along.

REED: I would say polyethylene came along in the early 1950s with Phillips Petroleum Company. Polypropylene was about the same time, although it didn't develop into a commercial product until the 1960s.

FINE: So really the whole nature of the plastics industry was seriously changing at this point. We're beginning to see a fragmenting into two major segments—the commodity and high performance plastics.

REED: I would say, very slowly. In those days probably you wouldn't have said that anything was a commodity plastic except perhaps polyethylene. I remember, year after year, seeing articles in the weekly news edition of *Industrial Engineering Chemistry* that polyethylene is going to be the first billion pounds per year plastic. That was 1956 and 1957. The phenolics were a few hundred million pounds at that time so I would say that polycarbonates was sort of a totally separate field, like Teflon, for example. Nylon hadn't yet become a very big molding compound material in those days. But as I look back on it, I don't think the concept of specialty plastics as a big business was in the consciousness of most of the chemical companies in the 1950s. They were in it, you know. It was something new. But I don't think they really realized that twenty-five years later it would be the most important market to be in, from a business point of view, in contrast to the commodities.

FINE: Did you make any changes in the polycarbonate business—in strategy at the time you came in, in 1959 or 1960?

REED: It was on a very good track and the major preoccupation was to get the first plant built. We did that in Mt. Vernon, Indiana and that was done under Shubert, again. Shubert, who reported to me, got that plant built and on stream in 1961 even though, as I said, we had a very poor process involving expensive pyridine which had to be recovered and recycled. But, we were in the market with million pound quantities and that was what was important. We were developing the market.

FINE: And you were losing money on it, too.

REED: Oh yes. We were losing money and polycarbonates were, at one time, in much greater danger of being dropped by the company than silicones had ever been. I remember in 1963, as I recall it now, that, for a while, the president of the company, Fred Bort, had us on a thirty-day basis.

FINE: That was the time that G.E. got into that bad price fixing thing and had to lay out hundreds of millions of dollars so it was a bad, bad time.

REED: From 1960 to 1964, we were engaged in settling the anti-trust suits with the federal government and with the utilities. That's really what propelled me into the general management of the Chemical and Metallurgical Divisions after just nine months in a new job in the Metallurgical Division, because Bob Gibson was moved on to general manager of the Transformer Division in Pittsfield to take over for somebody who was involved in the anti-trust case. It was very interesting. Gibson was, of course, a great proponent of the chemical business which had its headquarters in Pittsfield, where the transformer business was predominant. The chemical business and its people were treated as poor relatives and a drain on the company because the transformer business was very profitable and looked down at this little chemical business on Plastics Avenue which was also irritating many of their good customers. Nobody in his wildest dreams could have conceived that twenty-five years later the poor relations would be the best relatives. The Transformer Division has gone from ten thousand employees down to sixteen hundred. It's been, for somebody with my perspective, an incredible series of events.

FINE: In this 1960-1964 period, you had to defend the polycarbonate business before the Boards.

REED: That's right. Defend the polycarbonate business and continue to put our confidence in the project, which it turned around and made profits in 1964 and 1965. Now again, that's eleven years from the time of the invention.

FINE: G.E. is now able to handle that because of the experience in silicones. They're willing to constantly invest.

REED: We then had the experience with polycarbonates. That gave us the confidence to go into Noryl and gave us the confidence to go into Valox and Oliphants. But in any one of these cases, we were in for several million dollars before we broke into the black.

FINE: To get back to the chronology. First of all, you had the PPO before Noryl.

REED: Well, we recognized that most of our polymers used a common raw material and that was phenol. We were in the phenolics compound business, the polycarbonates were based on phenol, Noryl was also based on phenol which again could also be methylated into dimethylphenol.

FINE: Was that really understood and became a conviction that they would follow that kind of a trend. Everybody essentially uses phenol nowadays. It's ubiquitous in the polymer business, goes back to the raw material.

REED: That's exactly right. It's in many resins.

FINE: One of the interesting sidelights is that G.E. got back into the phenol business, this was in 1978 or 1979.

REED: That was something I followed very closely over the years because of the original experience I had with phenols. Our business grew at such a rate that it was obvious that we could support a world class phenol plant. At that time, a four hundred million-pound plant was considered a world class phenol plant. So I pushed very hard to get into the phenol business and Welch, who was running the division at that time, was very conservative because he was the one who had to take direct responsibilities for it. But he saw that it was going to be very good business for us to make our own phenol because we would have control over the purity of the phenol and also ensure the supply of the raw material.

FINE: Why had Jack [John F.] Welch been conservative on that issue?

REED: Well, because there were a lot of phenol suppliers.

FINE: But the phenol was also for internal consumption, was it not?

REED: To start, yes, but if you have a four hundred million pound plant and all of a sudden your internal market is reduced to three hundred million pounds because of the business cycle, you have a millstone around your neck. So you've got to have confidence that you can ride through a business recession without too much trouble. If you're buying your phenol and a recession comes along, the onus is on your suppliers. That's always a decision you have to make.

FINE: G.E. ended up pushing Monsanto out of the phenol business because they were their principal supplier.

REED: When G.E. threw that much phenol capacity on the market and there was unfortunately a recession in 1981—just what Welch had always been afraid of—it had a traumatic effect for a while on the market. But it's turned out that in the long term it was a marvelous decision for G.E.

FINE: The PPO opportunity came along. Why did you decide to go ahead and produce that in the early sixties and when it was clear that the material was still in the early stages of development.

REED: Again, it seemed to me that we had something that was totally different from any product on the market and it was based on phenols so we were going to have a good economic position. We had a process position that was unique. Nobody else in the world had anything like it. We had the experience with silicones and polycarbonates of developing and marketing new products under our belt, and it just seemed as though we had a number of unfair advantages in going ahead and developing this new material.

FINE: This was oxidative coupling?

REED: This is oxidative coupling that was developed by Al Hay. And then we had the decision to push it through market development and the second big breakthrough was the

discovery of its miscibility with polystyrene so you really had an alloy there. PPO itself turned out to be very disappointing as a polymer. It had too many weaknesses.

FINE: Were you aware of these weaknesses?

REED: Oh, yes. Unfortunately, we became more aware as time went on. I won't say we were aware from day one, but we had hopes.

FINE: You didn't become aware of this until after the product picture.

REED: You have a whole profile of properties and if it wasn't glaringly weak, it had enough unusual properties to make you hope that new uses could be developed. It was highly water resistant, for example. But it was weak from the solvent resistance standpoint. And it had the high temperature properties. It was very difficult to mold and work, but we hoped we could find a niche, and we did find a few niches. We did have a few niches where PPO was unique. Its profile of properties was encouraging, but the minute we made the polystyrene connection, the picture brightened enormously. We were never on the point of abandoning PPO, but the potential size of the business just moved up, let's say an order of magnitude after we were able to mix it with polystyrene. The cost of the molding compound went down and many applications became possible and economical.

[END OF TAPE, SIDE 6]

FINE: So you were still on a pilot schedule?

REED: We would have never gone ahead with the Selkirk plant without having found the polystyrene/PPO potential. I mean we were going to go ahead with a larger scale development plant, but we would never have started at a new location without the polystyrene/PPO alloy under our belt.

FINE: But at least there were plans afoot to build a large—

REED: Oh, yes.

FINE: Before polystyrenes?

REED: Oh yes, but nothing like a new location. We might have built a larger pilot plant in Pittsfield.

FINE: Because PPO is a very brittle material and not very moldable.

REED: It has all these weaknesses.

FINE: The marketing people, what did they bring you back?

REED: The marketing people were brought in at a very early development stage and were uncovering certain small scale applications where the profile of properties was adequate. For instance, certain types of surgical instrument. It was a very good material for this purpose because it could be sterilized.

FINE: Hydrolytic stability?

REED: Hydrolytic stability, a plastic you could sterilize. That's an example of a niche where it was fitting in and was identified. I would say, dozens of products—each one exciting in itself could be made, but as far as potential volume was concerned, it was relatively small.

FINE: It's like Ultem in a sense that you can find little markets.

REED: Exactly like Ultem. That's a good parallel.

FINE: And the discovery of adding polystyrene. I've heard a number of versions of that one. Would you care to add yours?

REED: My recollection of that is that Bob [Robert W.] Finholt developed that and he was with the Erie laboratory of the General Electric Company at that time. He discovered it and then they moved him in 1963 and put him in charge of development of PPO. He was a very good, very enthusiastic development man, but a less than adequate manager. He was with us only two or three years. Welch had come on as a developmental chemical engineer in the chemical development operation in 1960. Welch was really outstanding and I spotted Welch right away.

FINE: Outstanding as a manager?

REED: And as an engineer. But really taking hold of managerial work at a very young age. We owe a great deal to Finholt, including this polystyrene development. He has since deceased. But he just wasn't good as a manager.

FINE: Was Welch involved in the polystyrene development at all?

REED: No. He was involved in the development of the molding compounds of polystyrene but not in the chemical discoveries. That was done by Finholt.

FINE: Finholt was the first to add HIPS [High impact polystyrene] to PPO.

REED: I can't give you the laboratory details of whether he got in the lab and did it or whether he had an assistant do it, but from my point of view, Finholt was the key individual and he was so key and so enthusiastic that Ruben Guttoff, who was reporting to me as general manager of the Chemical Development Operation, brought him over as manager of the PPO Development Operation. We had followed our previous experience and set it up within the Chemical Development Operations as a separate section and put Finholt in charge of it. He first came to Bridgeport where we had the headquarters of Chemical Development at that time.

FINE: Was that done in Bridgeport?

REED: The laboratory work was done in Erie and then transferred to Pittsfield. But Finholt came on as manager of the operation in Bridgeport, reporting to Ruben Guttoff. We then moved Finholt up to Pittsfield, where he acted as general manager of the PPO Section of the Chemical Development Operation until early in 1965 when he was moved into general strategic planning in the company in New York, and Welch took over as general manager of the PPO.

FINE: Welch was essentially with G.E. about five years at that point and he was the G.M.

REED: Four years.

FINE: Who was the one who actually said, "Let's have this product Noryl which we will put the emphasis on?" Who made that decision?

REED: That wasn't made by any one person. As that came along, we simply gave a big kick to the overall marketing development operations and we uncovered more and more possibilities for using Noryl. It became obvious that it was a good gamble to put up the first plant.

FINE: So there was no meeting where somebody sat down and said, "Look, take this thing called Noryl and make it." There was no one day.

REED: No. No one day where that happened. You're constantly having reviews, budget reviews and product reviews. The data comes in. The applications start accumulating. You're constantly assessing what you would do in terms of capacity and location on a new plant. In the meantime you have some pilot plant work going on on the process itself and on the molding compounds. This was all going on up in Pittsfield and we came to the point where we said, "We know enough now and we can take a gamble in the first plant." But it wasn't the first time that we did it. We did it on Lexan, we did it on the silicones. So we had confidence. I had confidence at that point in time, to go out and sell this new plant in a new location so that we could have enough material to get a strong market position.

FINE: Was that your responsibility?

REED: Yes.

FINE: So this thing funneled up from Finholt to you and then it was your responsibility.

REED: My responsibility to decide on the new plant. So on to the G.E. Management and Board of Directors.

FINE: How much was understood about the chemistry, the physics, the rheology, et cetera? The reason I ask this is because just about this time Flory's book, which provided the mathematical basis for understanding rubber elasticity, cold flow behavior which would effect moldability et cetera, was published. [Maurice L.] Huggins at [Eastman] Kodak [Company] and [Frank R.] Mayo at Stanford Research Institute were all providing theoretical treatments. Presumably everybody who was working there was reading the literature and there were people like yourself in the trenches who must have understood the mathematical implications of these

things. How did it come together? My guess is that Finholt threw the stuff in there—strictly Edisonian.

REED: Yes, Finholt threw the stuff in there and it was totally experimental at that point. You would get into compounding with polystyrene and with all these other polymers to make molding compounds, lubricants or what-not. And I would say, it was almost totally empirical in those days and the thing that would give you confidence was simply the practical results that would come in from customer applications.

FINE: So you'd take something—an application—and send it out and see how it works.

REED: You would go out and work with customers on applications. For example, Finholt, I'll never forget it. He went to one of the big medical drug distributors who had a catalogue three or four times this thick with all kinds of surgical instruments, hospital trays and buckets and so on. He went through that catalogue with this customer and said, "Now let's take this product on this page. Could this be made out of Noryl?" "It certainly could and you would have a great advantage for this reason." Next page. "What about these products? Could they be made out of Noryl?" He just went through the whole catalogue with several hundred products in it that may have been out of metal and enamels, and he said, "Here's a plastic, you may use at a much lower cost. You can sterilize it and it will do everything you want it to do and it will save you money." That's the way you do chemical development operations. In total ignorance, I'm sorry to say, of the fundamentals, or the brilliant work some people like Flory were doing. That's the way it was at that time.

FINE: That's what one would expect. But now in retrospect, court cases have been won by G.E. against the Japanese, for example, because people understood the role of the particle size in the dispersion of the rubber particles.

REED: Now you're getting into a later period.

FINE: That's right.

REED: This was a new development where you can introduce immiscible polymers but bind them in with dispersing agents or coupling agents. This is a tremendously important development which came in the 1970s, not in the 1960s. Maybe some of the laboratory work went back as far as the 1960s.

FINE: One of the important people in all of this I thought might have been Arthur Beuche. He was certainly a professional—

REED: Arthur Beuche gave great support to the Al Hay work and to subsequent work on the process. The role of the laboratory was primarily in developing the Hay oxidative polymerization process rather than the compounding, and they have continued to this day to develop the oxidative coupling process which is complicated chemistry, and continues to improve over the years.

FINE: But if you had to name one, I don't know if you care to do this, who was foremost product champion in all? You would say Finholt was the best?

REED: I wouldn't say he was a great product champion, but he was responsible for improving the product in a way that vastly changed its outlook.

FINE: He also did marketing and took it on the road.

REED: He was very good in marketing, also.

FINE: What was Reuben Gutoff's role in all of this?

REED: Gutoff supported it strongly, but he was trying to get into a lot of other things at the time too. This was just one thing that he had. He was quite involved in evaluating the Finholt work and had supported Finholt.

FINE: What was Welch's role?

REED: Welch's role was in the trenches at this time, working on the process.

FINE: He wasn't a marketing man?

REED: He was involved in developing the plants.

FINE: In other words, there wasn't a mix of chemists and engineers in the laboratory doing the development work. Welch was really involved, in a stage beyond that.

REED: Welch was involved in the laboratory but it was process development work primarily and some product development work. But we had a cadre of people who were working on making the molding compounds. Welch's job was to make the PPO.

FINE: Welch and people like Bob Shenian and people like that.

REED: That's right. The big job was to develop a full scale process for making the 2,6-dimethylphenol. I insisted that we devote efforts to making our own 2,6-dimethylphenol. At the beginning, we were dependent upon buying that product from U.S. Steel and from coal tar refiners. And I said, "No way. Knowing what is important about the purity of the monomers going into polymers, we have got to get control of this 2,6-dimethylphenol." We put that up to the lab in Schenectady and they developed the original process of methylating phenol and Welch took on the job then of building a pilot plant on that.

FINE: Was that Hamilton's?

REED: Hamilton. Now, I don't recall that name.

FINE: That was the lab that was built around 1960 or so.

REED: That was done in the early 1960s. I don't remember the name of the chemist who actually did the work. But then Welch built the first pilot plant in Mt. Vernon. He did a great job on that. But that was absolutely key to our going ahead with this business—getting that monomer under control. And again, it was because of our previous experience. I just knew that we had to do it. So I gave it great support.

FINE: How did it become apparent that he was also the guy to hand this thing over to?

REED: Welch just turned out to be a pretty good manager and a lot of people in Pittsfield who were involved in Lexan came into contact with Welch and felt that he was a coming young man. I was in fairly close contact with him in business reviews. Guttoff felt that he was very good. So we decided to take a risk on him when he was twenty-six or twenty-seven.

FINE: It does seem parallel to your own career—within three or four or five years, you were moving rapidly along a management-directed ladder. Ph.D., Chem. Engineer. Who is this fellow out of Illinois—Ph.D., Chem. Engineer?

REED: It was similar but he did it several years earlier than me.

FINE: If he was at the age when you took over, he would be the president of G.E. by now. But you spent five or six years at MIT playing professor for a while. We could probably stop here, but there is one thing. It seems like from silicones to PPO, you had something coming along every five years. There was quite a long gap.

REED: We got Lexan into the black, as I said, around 1965. We knew that it was going to take a few years to get Noryl into the black. We called it Noreel at that point. We had a joint venture going in Europe and sold some licenses in Japan.

FINE: Was this the AKU [Algemene Kunstzijde Unie] business?

REED: This was the AKU venture. The AKU people had done some work on polyxylenol and they felt that it might have a future as a fiber. We were very excited over that because new types of fibers were very much in vogue in the 1960s. Du Pont was coming along with Qiana and the bloom was not off the rose on nylon yet at that point. Polypropylene was a fiber and we just thought, "Maybe we can get into the fibers with PPO." Then we had what we call P3O, which, instead of two methyl groups on the benzene we had two phenyl groups. We had 2,6-diphenylphenol and that was a marvelous polymer. We thought, at one point, that we had something that was going to be a poor man's Mylar with much improved properties. We devoted a lot of effort to trying to make a film out of the P3O but unfortunately the profile is so complex on a film like that, the required profile, that that didn't amount to anything. The fiber also didn't lead to anything. It's major weaknesses was staining. The profile of properties that you had to have on a new fiber, is just terribly complex and you could do a lot of things with this fiber, but it had the weakness that many things would stain it and you couldn't wash out the stains.

FINE: When you were selling that PPO was it on the basis of a combination of products—P3O and fiber together.

REED: P3O was strictly developmental then and in the back of our minds it was a real plus that here was something else that might come along based on the same chemistry and engineering.

FINE: But that was not the case.

REED: But that was not to be. The successes of PPO were that polystyrene was miscible with the PPO and the development of the monomer process.

FINE: Of course, this was the first product in which the polymer was not the plastic as with silicones and with polycarbonates. Now you have the first of these garbage can polymers where you throw everything in but the kitchen sink. That presumably gave people time to think. I guess what I'm driving at is Valox came along and was a similar—

REED: Yes. We then realized that we had to have a polymer that was solvent resistant. Noryl is not solvent resistant. That's not in its profile. That would be one of its weaker points. I felt that we had to have in our stable a polymer that was solvent resistant. We did work on polyesters in order to get that. And again with four new materials under our belt, silicones, synthetic diamonds, Noryl and polycarbonates, I had the confidence that we would be successful once more. I've always said that we had to have some "fair or unfair advantage" and the only unfair advantage we had in Valox was our previous bank of experience in developing the new polymers commercially. I felt that we had such an outstanding market development operation under Welch that we could take on Valox even though we had no advantage in monomers or in process chemistry. But what we had was a powerful marketing organization that would take this material out and exploit it.

FINE: So you have reached the point where you had built these tremendous contacts in market-driven technology—that side of science and technology—so you could courageously go out and pick up a me-too product—

REED: We said, "Now we have this fine sales organization and contacts with customers and from an incremental marketing cost point of view, it's very favorable to add one more product with a profile of properties that would cover our major weakness," which was lack of a product with solvent resistance.

FINE: And that was successful as compared with your own developments?

REED: From a commercial point of view, it's been a pretty successful product.

FINE: It seems that a constant theme running through this is market considerations. The more you learn about business, the more important marketing is.

REED: And the more and more of an asset that becomes in getting into something new.

FINE: One of the things that I thought to open this discussion with, and maybe suitable to close with, is that the growth of the entire field of engineered plastics had materialized during your career.

REED: Well, I was also involved in the authorization of the first Ultem plant. We had gone through an expensive pilot plant. You are probably more current on how Ultem is doing these days than I am. I am going to be up in Pittsfield talking to them this next weekend. I heard its coming along pretty well.

FINE: Yes, I think it is. They've expanded the plant in Mt. Vernon dramatically.

REED: That's by far the most complex chemical operation we've ever taken on.

FINE: And of course that should not surprise you that they would venture into something as complicated as that.

REED: I was really still involved in authorizing that plant because General Electric—just at the time I retired back in 1978—had undergone a reorganization in which they set up several so-called sectors in the company and each one, at that time had a Sector Board, like an internal Board of Directors. I was fortunate to be asked to stay on as a member of that Sector Board for several years after I retired. Then there was another reorganization that phased all those Boards out and they even phased out the sectors, which I think is a good thing because I've always thought, tutored by Harold Smitty, that you need a minimum number of managerial layers in a company. Welch feels strongly on that same thing. So I was still on this Board, which authorized the Ultem plant, which I think was a seventy million dollar plant.

FINE: As I look back on it, almost from the opening remarks you made, you've been predisposed to marketing and markets by your course in economic engineering at MIT.

REED: But coupled with some very strong advantages in engineering and chemistry. It's the combination that gives you the best opportunity of all. If you can build strongly on any one of

them you minimize the risk in going ahead with a new venture, but also the experience brings home to you the importance of timing, getting in at the right time. Now we are late comer in Valox, but not too late. I mean—the polyester molding compounds still hadn't made much of an impact. Our fellows in the field who were out in contact with customers, could see that there was a tremendous opportunity for that type of product. That's why we had the confidence, even without the chemical advantage in that case. We again saw the importance of making our own monomers in that case. And we've spent a lot of money over the years in trying to make 1,4-butanediol and I think to no advantage.

FINE: They always claim that they scare the suppliers into lowering their prices.

REED: I'm not clear on it. I don't believe that they've got a process—they may have one on the fire but I don't know.

FINE: There is one that they've done work on but I think they're just about to start up.

REED: And then the suppliers make a concession.

FINE: That's right. I'm not sure what happened on this last round.

REED: I'm not either but I'll find that out.

FINE: That's like going into the phenol business. You integrate backwards—move into 1,4-butanediol. Do you see General Electric moving into something like polypropylene?

REED: No, I don't. Not unless there would be some very unusual process or advantage coming along that would be comparable, let's say, to what Union Carbide did to revolutionize polyethylene manufacturing with their gas phase process. It's not in the Welch philosophy at all to get into the commodity plastics.

FINE: This may get cut off by the tape, but I'm sure that you were asked this a lot when you were vice president of technology. Why was it that G.E. could succeed so well in invading the plastics business, which is not a typical product of the G.E. main line, whereas they failed in areas like semi-conductors, computers, electronics. What was the difference there?

REED: The difference. I kept trying to figure it out for years. I think it goes way back to the original G.E./RCA combination. When it was split up, and this was back in the 1920s, G.E. took a back seat in electronics. And they did not really get back into electronics at all until the war when they had a big business of making radio transmitters in the Schenectady plant. Then they had W.R.T. Baker, whose initials were on the television station, who was an outstanding electronics man and they had big plans for electronics in Syracuse, where they were combining the consumer electronics business and the military business. Baker died in 1957. He had started them in the computer business and sold Cordiner on going into computers. He said, "Now get into it. We've got to get some big contracts to get this started." They got a big contract from the Bank of America. Baker died about that time and they just did not put the amount of money it required in the case of computers. Cordiner, in his later years, actually admitted that he didn't support it with enough money.

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

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INDEX

A

Agens, Maynard, 18
Algemene Kunstzijde Unie (AKU), 53
Alkyd resins, 19
Alsifilm, 7
Aluminum Company of America, 23
American Chemical Society (ACS), 12
Arthur D. Little Consulting Company, 41

B

Bakelite, 24
Baker, W.R.T., 57
Bank of America, 57
Bayer, A.G., 30, 39-40
Belle, West Virginia, 16
Bentonite, 6-7
Beuche, Arthur, 51
BFGoodrich Company, 25
Borozon, 37
Bort, Fred, 43
Bowling Green State University, 2
Bridgeport, Connecticut, 48
Brooklyn Polytechnic University, 16
Burton process, 5
1,4-butanediol, 56
Butyl rubber, 13

C

Carothers, Wallace Hume, 11, 13-14
Case School of Applied Science, 1-4, 8, 12, 23-24, 29
 Department of Chemical Engineering, 2
Cellulose, 11, 30
Cellulose acetate, 24
Childrens' Hospital, Detroit, 2
Chlorobenzene, 12, 22, 26
Chlorosilanes, 17, 20
Christopher, Bill, 41
Christopher, Neil, 38
Cleveland, Ohio, 1, 12, 23-24
Cole, Jerome T., 17, 36
Collings, William F., 29-30
Colloid chemistry, 5-6, 8-9, 11
 Thixotropy, 6

Coolidge, William J., 23
Copeland, Norman, 15
Cordiner, Ralph, 25, 31, 35, 57
Corning Incorporated, 12
Coshocton, Ohio, 32
Cumene process, 26
Cyanamid, 33

D

D'Alelio, Gaetano F., 13, 25, 31
Delrin, 42
Dewey and Almy, 11
Dewey, Davis R., 5
Dewey, Bradley, 14
Dimethyldichlorosilane, 35
2,6-dimethylphenol, 40, 52
2,6-diphenylphenol, 53
Donnell, O. D., 1
Dow Chemical Company, 12, 14, 26, 29-30, 34, 40
Dow Corning Corporation, 30, 35-36
Du Pont, E.I. de Nemours & Co., Inc., 14-16, 33, 38, 42, 53
Du Pont, Lamot, 15

E

Eastman Kodak Company, 49
Erie, Pennsylvania, 47-48
Ethyl cellulose, 34
Exxon Corporation, 15

F

Findlay College, 1
Findlay High School, 1
Findlay, Ohio, 1
Finholt, Robert W., 47-51
Flory, Paul, 16, 49-50
Fluid bed technology, 5, 17-20
Formica, 32
Formvar enamels, 19, 23, 25
Foster Wheeler Corporation, 17
Fox, Daniel W., 33, 38, 40-41
Freepol, Bob, 23
Freundlich, Herbert, 8

G

General Electric Company, 4-5, 7-8, 11-19, 22-26, 29-34, 36-45, 47-50, 53, 55-57
 Air Conditioning Department, 31
 Carboloid Company, 24, 29, 37
 Chemical and Metallurgical Division, 24, 31-32, 38, 40-41, 43
 Chemical Department, 24, 26-27, 31, 36-37
 Chemical Engineering Department, 24-25, 27, 31-33
 Corraloy Company, 24
 Erie laboratory, 47
 Hamilton laboratory, 52
 Insulating Materials Department, 21, 23-28
 Lamp Division, 23-24
 Metallurgical Products Department, 36-37
 Pittsfield laboratory, 12-13, 18, 24-25, 31, 39-40, 43, 47-49, 52, 55
 Plastics Department, 18, 24-25, 31
 Refrigerator Department, 32
 Schenectady laboratory, 7, 13-14, 17-19, 31, 33, 40, 52, 57
 Sector Board, 55
 Selkirk plant, 46
 Silicone Products Department, 22, 32
 Transformer Department, 18, 24, 43
Gibson, Robert L., 31, 33, 36-38, 40, 43
Gilliland, Edwin R., 4-5, 10, 12
Great Depression, The, 1, 3
Grignard reaction, 29, 35-36
Guilford, --, 18
Gulf Oil Company, 15
Gutoff, Ruben, 48, 51-52

H

Harrington, Charles, 15
Hauser, Ernst, 7, 9
Hay, Allan S., 40, 45, 51
Hercules Incorporated, 26
Hershey, Robert, 15
Holloway, Frederick A., 15
Houdry, Eugene, 5
Huggins, Maurice L., 49
Hyde, J. Franklin, 29

I

Illinois, University of, 53
Industrial Engineering Chemistry, 42

J

Jeffries, Zay, 23-25, 27, 29, 31

L

Lewis, Warren K., 3-5, 9-10

Lexan, 49, 52-53

Lockite Corporation, 23

Low, Pete, 14

Lummus Company, 26

M

Mallinckrodt Incorporated, 11

Marathon Oil Company, 1

Mark, Herman, 11

Maron, Samuel H., 2

Marshall, Abraham L., 7, 12-13, 16, 21-23, 37, 40

Massachusetts Institute of Technology (MIT), 3-6, 8, 10-15, 36, 53, 55

 Chemical Engineering Practice School, 3, 9, 15

 Sloan School, 5

Mayo, Frank R., 49

McAdams, William H., 4-5

McAfee, Jerry, 15

Melamine resin, 32

Methane, 12, 29

Methyl chloride, 5, 14, 16, 20

Methylchlorosilanes, 5, 12, 16-17, 22, 35

Methylmethacrylate, 16

Miamisberg, Ohio, 38

Mickley, Harold S., 10

Midland, Michigan, 12

Mobay, 39

Monsanto Chemical Company, 9, 12, 25-26, 39, 45

 Everett plant, 9

Morawetz, Herbert, 16

Morrison, Jim, 7

Mt. Vernon, Indiana, 38, 43, 52, 55

N

Navias, Louis, 23

New York City, New York, 48

North Adams, Massachusetts, 25

Noryl, 44, 49-50, 53-54

Notre Dame, University of, 25

Nylon, 42, 53

O

Oliphants, 44
Organosilicon chemistry, 12-14

P

P3O, 53
Phenol, 11-13, 24, 26, 31-32, 34, 44-45, 52, 56
Phenolformaldehyde resins, 24
Phenolic resin, 32
Phenylchlorosilanes, 22, 29
Phillips Petroleum Company, 42
Phosgene, 38
Pining, Ed, 23
Pittsfield, Massachusetts, 12-13, 18, 24-25, 31, 39, 43, 47-49, 52, 55
Polycarbonates, 23, 32-33, 36-45, 54
Polychlorinated biphenyl (PCB), 18
Polyethylene, 42, 56
Polyformaldehyde, 42
Polyisobutylene, 13
Polyisoprene rubber, 13
Polymers, 5-6, 11-16, 18, 22, 24-25, 30-35, 37, 41, 44, 46, 50, 52-54
Polymethyl methacrylate, 42
Polyphenylene oxide (PPO), 9, 44-48, 52-54
Polyphenylether, 40
Polypropylene, 42, 53, 56
Polystyrene, 9, 11, 13, 24, 31, 34, 41, 46-48, 50, 54
 High impact (HIPS), 9, 48
Polyvinylchloride (PVC), 18-19, 25
Polyvinylformal, 19
Polyxylene, 40
Polyxylenol, 41-42, 53
Potolite, 16
Princeton University, 21
Principles of Chemical Engineering, 3
Prophylactic Brush Company, 25
Prutton, Carl F., 2-4
Pyridine, 40, 43

Q

Qiana, 53

R

RCA, 57

Reed, Charles

 brother, 1-2

 father, 1

 grandfather, 1

 mother, 1

Reichhold, 26

Rochow, Eugene G., 12, 16, 22-23, 27-29

S

Shenian, Bob, 52

Sherwood, Thomas K., 4, 9-10

Shubert, Gene, 38-39, 43

Silicon tetrachloride, 12, 29

Silicone rubber, 13, 18

Silicones, 5, 12-13, 16, 18-36, 38-45, 49, 53-54

Smitty, Harold, 31, 55

Sodium sulphite, 12

Springfield, Massachusetts, 12

St. Louis, Missouri, 11

Standard Oil of California, 9, 15

Standard Oil of Indiana, 5, 11, 15

Standard Oil of New Jersey, 5, 13, 16

Stanford Research Institute, 49

Stanford, California, 41

Staudinger, Herman, 8-9

Stauffer Chemical Company, 30

Stedman column, 17

Stedman packing, 17

Suits, Chauncey G., 12-13

Sullivan, Eugene C., 29

Svedberg, The, 11

Synthetic rubber, 14

Syracuse, New York, 57

T

Tau Beta PI Graduate Fellowship, 3

Teflon, 42

Textolite, 24, 32

U

U.S. Steel, 52

Ultem, 47, 55

Union Carbide Corporation (UCAR), 25, 30, 33, 56
 Bhopal, 38

Universal Oil Products (UOP), 5

V

Valox, 44, 54, 56

Veazey, William, 3

W

Walker, W. H., 3

Waterford, New York, 21, 31, 35

Welch, John F., 44-45, 47-48, 51-52, 54-56

West Virginia Chemicals, 11

Whitney, Willis R., 7

Wilson, Charlie, 24

World War II, 11-14, 31, 57

 Pearl Harbor, 14

 Synthetic Rubber Research Program, 14

Wright, Gil, 18