CHEMICAL HERITAGE FOUNDATION

LOUIS C. RUBENS

Transcript of an Interview
Conducted by

James J. Bohning

at

Midland, Michigan

on

19 August 1986

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Oral History Program

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(Revised 24 February 1988)

> Louis C. Rubens, interview by James J. Bohning at Midland, Michigan, 19 August 1986 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0048).

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

LOUIS C. RUBENS

1919	Born in Escanaba, Michigan on 9 November

Education

1939	A.A.S., chemistry, Jordan College

Professional Experience

	Dow Chemical Company
1940-1946	Laboratory Technician
1946-1952	Chemist
1952-1962	Group Leader
1962-1967	Associate Scientist
1967-1992	Research Scientist
1992-1996	Research Fellow

Honors

1979	Herbert H. Dow Award
1980	Saginaw Valley Patent Lawyers Award for Lifetime Inventors
1982	12th International Foamed Plastics Award
1982	Dow Inventor of the Year Award
1983	German Fachverband Schaumkunstoffe Medal for Pioneering Research
1994	Outstanding Achievement Award, Society of Plastics Engineers

ABSTRACT

Louis Rubens begins the interview by discussing his family background and early education. Rubens grew up in Escanaba, Michigan, where his parents ran a tourist park. He attended parochial school, and credits his teachers there for sparking his interest in mathematics and chemistry. After high school, Rubens followed in the footsteps of his older brother and attended Jordan College. There, he studied chemistry, and when the school closed in 1939 due to financial difficulties, Rubens received his Associate's degree. Though he tried to transfer to other colleges, he was not successful, and soon decided to take a position at Dow. Rubens rose through the ranks of research, working on the stabilization and impact enhancement of polystyrene, the production of co-polymers, and the development of the composite foam system. Rubens concludes the interview with a discussion of the importance of management support for research, the influence of H.H. Dow's research philosophy, and the future of the foam industry.

INTERVIEWER

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

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INTERVIEWED BY: James J. Bohning

DATE: 19 August 1986

PLACE: Midland, Michigan

BOHNING: You were born on November 9, 1919, but I don't know anything else. Where were you born and can you tell us something about your parents?

RUBENS: I was born in Escanaba, Michigan, the upper peninsula. My dad was a laborer. I think he got about as far as the eighth grade. My mother was also a hard working woman, and I think she got up to the third grade. We were three children in the family: myself, my brother, and a sister. I was the second child. Like so many people at that time, my parents wanted their children to get an education, to go to college. They knew nothing about it themselves. They were honest, hard-working people.

BOHNING: I'm not familiar with that section of the region. Is that copper country there?

RUBENS: No. The copper country is up in the northwestern part. In fact, Escanaba was largely an ore shipping port, primarily for iron ore and it was also a lumbering town. Very pretty town of about fifteen thousand people at the time. My folks did run a tourist park and also a picnic park for the local Chamber of Commerce. I remember that kind of a life pretty well. I worked with commercial fishermen when I was growing up, but also I did a lot of fishing myself, mostly for perch. I cleaned them up and sold them at the tourist park to make pocket money.

BOHNING: Your father's name? Your mother's name?

RUBENS: My father's name was John Joseph Rubens. My mother's name was Irene. Later she said it was really Mary.

BOHNING: Somewhere I recall reading she was responsible for your nickname "Bud". Is that right?

RUBENS: I think it was my brother who came up with it. I am not really conscious of where it really originated, but everyone called me “Bud”—maybe because “Louis” sounded much too formal.

BOHNING: Did you get your early schooling in Escanaba?

RUBENS: Yes. I went to St. Joseph’s, a parochial school. The Sisters of Notre Dame were the teachers.

BOHNING: Was that all the way through high school?

RUBENS: Yes. I still remember my high school chemistry and math teacher, Sister Raina. She was about four foot six, but a very strong disciplinarian, and she was an excellent teacher. I think she was responsible for much of my very early interest in chemistry and math.

BOHNING: You had her for both chemistry and math?

RUBENS: Yes, even physics. The nuns at that time were very dedicated. Between praying and studying and teaching, they had their full allotment and we did very well.

BOHNING: Were there any other teachers that had an influence on you? I want to come back to this, but was there anyone else who had an influence?

RUBENS: No, not really. I never really remembered working very hard at school, except for the things that I was interested in. It turned out to be science.

BOHNING: Did you have much laboratory work in that?

RUBENS: Surprisingly, yes. For a high school of that period, we had a pretty good little lab. We did simple analytical type experiments. Simple quantitative analysis which was taught in high school. It was limited to color and flame types of tests that by today’s standards are pretty unsophisticated.

BOHNING: Had you given any thought to a career at this point?

RUBENS: No, not really, because there were few people then who could give advice about potential careers. The idea was that you tried to get through high school as well as you could, and hopefully you would get a chance to go to college. Once you did that, you weren't really sure, even at that point, where you should go. These were the Depression days. People knew very little about what the opportunities that existed, particularly in a relatively isolated region where most jobs were manual. While in high school—in the summers, Christmas vacation, etc.—I'd work in the saw mill, and I got a pretty good feeling of what it was to load slabs onto gondolas in the winter when the temperature was twenty five below zero. So I decided that, if nothing else, I was going to find a job that was not in the open. But then a rather interesting thing happened.

My brother had gone to a college that was opened at Monalmany, Michigan by a Salvatorian priest who had transformed an old agricultural college into a liberal arts school. My brother went to that school when it opened in 1930, and graduated with a major in English. He and the Dean, Father Bill Murray, had become very good friends. There was also a politician, whose name I forgot, but Bill Murray had known him well and felt that together they could arrange for me to go to Annapolis [United States Naval Academy] or West Point [United States Military Academy] after a year or two at Jordan College. That sounded great to me as a youngster. I hadn't really made by then any career commitments. So I went to Jordan. High schools at that time offered many scholarship opportunities. One that seemed most attractive to me was at the School of Mines, but since I could also have the scholarship to Jordan, and keep open this other opportunity, and under the influence of my older brother and his friend, I decided to go to Jordan. As it turned out, it was good for me. That was in 1937.

When I graduated from high school I started at Jordan. I took the usual courses of a liberal arts school. I ended up taking the chemistry, physics, and math that they had to offer. I can remember the math professor was Rossnay. Probably the guy that had the biggest influence on me was Dr. Flanagan. Flanagan, interestingly enough, was a medical doctor, and he had come to Jordan when the school opened. Doctors were having a tough time, too, with their local practice because of the Depression. So, he got a job on the faculty to teach chemistry and biology. I got to know him quite well and we became good friends. I got a National Youth Administration job. That involved taking care of the laboratory at the school. So I had the opportunity to be in the labs all the time, and Flanagan and I became very good friends. He was the first person I knew who ever had an industrial chemistry experience. He had worked for the Ford Motor Company back in the late 1920s and 1930s. Old Henry Ford became interested in making cars out of plastic materials. They were interested in cellulosics and casein plastics, and Ford had expanded his operations into the upper peninsula. He had logging operations there and he had a plant on our mountain, and a laboratory, where they were trying to make plastics from natural materials that could be used in cars. Dr. Flanagan had worked in that laboratory in the early part of his career for four or five years, so he had a little experience about what it was like in an industrial laboratory. I managed, in the two years that the school remained open, to get all the science and math courses in which I was primarily interested. I seemed to gravitate towards that type of course and they let me take as many as I wanted, so it was a good experience.

Unfortunately, “the best laid plans of mice and men”—the politician didn’t get reelected, so the opportunity to get the appointment at West Point vanished. Also, Jordan College was right out of money after I’d been there a couple of years. I tried to get other schools in the area like Michigan State to accept my transfer, but at that time, unless you could prove that you had the wherewithall to take care of yourself, you would not be accepted. You could get scholarships for tuition fairly easily, but there was a real tough time getting jobs for your room and board and books and other expenses, so they were reticent to accept me. There weren’t as many student advisory services as there are today to help.

So that’s when I decided to look for a chemical company and see if I could use what I had learned up to this point in a laboratory. I had heard about Dow and on 9 April 1940 I hitchhiked down to Midland and applied for a job. They did have a technical employment office—[Steven L.] Starks was head of it, and a fellow by the name of Gross, Bill Gross, was really handling the routine. I went into this office and told him that I was interested in a job in one of their laboratories and I had hitchhiked into town and had very little money in my pocket to survive for very long. They brought Ray [Raymond H.] Boundy and [Raymond F.] Boyer over to interview me. Ray Boundy was assistant director of the physics lab to John Grebe. They interviewed me and then left and made no commitment whatsoever. I was twenty years old at the time and I thought these fellows must not be the ones that make these decisions, so I decided to see if I could find the president of the company. I wandered upstairs (this was over in 47 Building) and there was a receptionist named Kyle at the desk. I asked her, “Where does the president of this company have his office?” I guess I was just brash enough at the time so that when she said “Down that hall.” I was quickly gone. I came down to where Dow had his office. I got into the outer office and I ran into his personal secretary at the time, Ruth Perry. She asked me what I wanted, and I told her, “Well, I’d come for a job, I had had an interview, but I didn’t seem to get much action out of the people I interviewed with.” I added that I felt I could do the company some good! She talked to Willard Dow by the intercom and his reply was to go back to the employment office. When I went back there I was given the application to complete, got a physical examination, and was asked to go to work that afternoon. That was it. it wasn’t much of a story, but that’s the way things happened.

BOHNING: You talked to Dow personally?

RUBENS: No, I talked only to his secretary. I did get to know him during the war, several years after I was working for the company. I ran into him one time when I was going to go back to Escanaba. I was waiting for a ferry boat at Frankfort to go back to Manistique, which was just about sixty miles from Escanaba. Gasoline was short and that was the main motivation for taking a boat. I was at a gas station having a flat tire fixed and I noticed this fellow lounging against the gas station. He had a simple slacks suit, the kind that looked like khaki cloth at that time, and about a two or three day growth of beard. He saw the license on my car and asked if I lived in Midland and if I liked living there. I talked to him for about ten to fifteen minutes before it dawned on me that this was Willard Dow, president of the company. Then, we had a good discussion. I missed my boat, and probably spent about three or four hours talking to him at the gas station. As it turned out, about a year later, he called up the Physics Lab and asked if I would do some projects that he wanted to do. That helped me out quite a lot. So, while I didn't get to see him when I went to his office, I did get to spend a surprising amount of time with him, by accident.

BOHNING: Before we look at your Dow career, I'd like to back up for a moment to Jordan College. I had a question I wanted to ask there. Did they offer a two year diploma?

RUBENS: Yes, I got an Associate degree in Arts and Science from Jordan.

BOHNING: That was part of their regular curriculum?

RUBENS: Well, no. That happened when they closed. They had never given it before, but they gave it because it was going to be the end of their operations.

BOHNING: How many students were there at that point?

RUBENS: Seventy-five. They didn't have the resources to take more. I think there were too many like myself who were getting scholarships while there and leaving to go elsewhere. Yet, it was a very interesting place because the professors were living at close proximity. Some of them were single. I remember MacNamy, the English professor, he was serving as dean, and he was also living in a dormitory, as did Rossnay. Flanagan lived in a house right on the other side of the school. We had very good access to all the people. In fact we had a language professor, Dr. Shenk, a very knowledgeable teacher who had been a General in WWI. Apparently, he had the ends of his toes cut off from both feet when he was in the service, and he wore his shoes always turned up at the end about ninety degrees, and he looked like he was wearing skis. Then we had the one fellow who taught philosophy, I've forgotten his name, who was an ether sniffer. I always wondered where the ether cans in the lab disappeared. What he was doing was to get these ether cans—he didn't drink booze—and sniffed that ether. He always smelled of ether, and so finally we figured out what was going on.

BOHNING: What kind of laboratory experience did you have there?

RUBENS: Well, we had a pretty good laboratory from the days that Jordan was an agricultural school. The laboratories had been built into that building in the basement. For the size of the class and the period, they were really pretty good laboratories. I think that we really had pretty good laboratories compared to what the University of Detroit has today. Of course, we had very poor second-hand equipment that the students have been able to put together and make it work, but as a facility, I'm struck with the fact that it was pretty good.

[END OF TAPE, SIDE 1]

BOHNING: Were there any other students that were science oriented?

RUBENS: Oh, there were some. I can remember Garrity Jordanson Hupie, but I really never built up lasting relationships the time that I was there. I got along with them well, as far as I was concerned, but when the school closed everybody pretty much went his own way. In fact, I don't think I've ever heard from or seen a person from Jordan College since then, except of course, for my brother. He was one of the first graduates and he is still in contact with two or three people from that period.

BOHNING: Your associates degree was in chemical engineering?

RUBENS: No. It was in chemistry. There weren't people knowing chemical engineering at that school. Flanagan knew, pretty much, classically oriented chemistry and a little bit of what he had learned in the industrial laboratories at the time. He knew inorganic chemistry too, so that was pretty much the type of thing he taught. He knew also biology and he did teach a course in comparative anatomy.

BOHNING: Based on his experience with the Ford laboratories and natural polymers, did he give you any exposure to that?

RUBENS: Well, he gave very elementary descriptions of polymers, and perhaps one or two elementary lectures at the latter stages of the chemistry class. You should consider that it was just 1928 and [Hermann] Staudinger was proving the chain nature of polymer molecules. While few people prior to that knew something about the properties of rubber and of natural polymers, I don't think they had much of an understanding about the chemistry that was involved.

BOHNING: The school closed in 1939?

RUBENS: Yes. I finished two full years there.

BOHNING: But it wasn't until April of 1940 that you came down to Midland. What did you do in the interim period?

RUBENS: Well, what I ended up doing was going to work in the saw mill and spending some time trying to get accepted at other schools. It soon became apparent to me that that wasn't going to work.

BOHNING: Let's go back then to Dow. You've described your interview. Did you start the next day then?

RUBENS: That afternoon I went over and got my physical examination and went over to the lab, and the next day I started work.

BOHNING: Many of the people that came in at that time went through that student training program. Did you go through that?

RUBENS: No. I was hired on an hourly rate. Of course, Dow had not been hiring much for several years because of the Depression, and things were then just starting to improve. They had various hourly classifications in the lab—the bottom was lab helper and that's where I started. You started as a lab helper and then you took a series of examinations that were generally prepared by the technical professionals in the lab, and you went through lab assistant and senior lab assistant, laboratory technician and senior laboratory technician. There were altogether about five classifications. I remember starting out, though, for sixty four cents an hour. But I'll tell you something funny. A friend of mine, Leo Marshall, came from the University of California at the same time, and he came with a Master's in physics. Do you know how much his salary worked out to? Eighty cents an hour. Bill didn't stay at Dow. He worked there possibly for a year and left. I never heard from him since, but I remember us comparing notes about what a Master's degree was worth. Sixteen cents an hour, isn't that something? The senior lab technician could get up to maybe ninety cents an hour, i.e., above the entry level for a Master's degree in physics or chemistry at that time.

BOHNING: What was your first assignment?

RUBENS: My first assignment had to do with styrene. Styrene was being produced over at the 323 Building and it was sold to the Bakelite division of Union Carbide. They were the only manufacturers of polystyrene. In fact they were the only customers for styrene that we had at the time. What we used to do was analyze for aldehyde, peroxide, residual sulfur, polymer content, etc. My job was to follow through and make sure that all those analyses were done and to watch the quality control. The Physics Lab was still closely associated with production at that time. The other job that took most of my time dealt with the light and heat stability of Saran—which we called at that time "Ventyloid." Ralph Wiley had been working on Saran. Light and heat stability were already identified as the key problems of that material. Two colleagues tackled that problem. Lorne A. Matheson and Ray Boyer. They had developed techniques to measure the light and heat stability. One of them involved making fifteen percent solutions of vinylidene chloride and an acrylate co-polymer in dioxan and putting them in little vials. It was an Edisonian research program, but people really didn't know how you would stabilize a polymer against light degradation. They knew it was photo-degradation. They had pieces of film that they have exposed to various wavelengths and measured the rate of color development. But they really didn't know how to prevent it.

Ray and Matheson were both physicists—Matheson was from the University of Michigan, and Ray from Case [Western Reserve University]. So they did know something about optics. Ray had built a circuit just using a simple potentiometer and a couple of photocells, with which they measured the light absorption at 3650Å and detected the yellowing of the polymer. I remember this apparatus very well because we used to cast these lacquers onto little glass tabs, and then expose them for different periods in a Fadeometer and then we measured the decrease in transmission at 3650Å as an indication of yellowness. Another technique we used was for measuring heat stability. We used a magnesium plate that was about a foot long and maybe about four or five inches wide with holes drilled in it. The magnesium was suitable because it offered a nice, polished surface. What we did was to cast test samples in these holes. We had sample boxes that held a hundred vials. We prepared the solutions by putting them on a tumbling stirrer and then dumped them all into these little pits which were about half an inch wide and a half inch deep. We let the lacquer dry, and obtained a nice film in there. We subjected this plate to different heat treatments, and from the blanks we could pick up whether some additive was stabilizing the polymer or catalyzing degradation. These were the techniques that we used. It was primarily an Edisonian research program that was initiated by Boyer and Matheson and my role, really, was that of a lab assistant doing it for them. I certainly wasn't finding the solutions, but I was learning what was going on.

Then I worked with Ray trying to identify the types of chromophores that formed when the HCl split off from Saran. I did the work with morpholine and the dehydrochlorination experiments, which provided the data that Boyer used for publishing some of his papers. It was at that time when the development of conjugated unsaturated sequences in dehydrochlorinated polymers was noted by Boyer and he identified compounds like the 2,2-dihydroxy benzophenone as a good light screen for Saran (1). There were quite a number of other compounds with similar properties—eg., salo but it was not as good. I notice the benzophenone derivatives are still used today in marine varnishes. Photodecomposition was a key problem then, as far as Saran was concerned. I think that Ray and Matheson were the pioneers in solving this problem. In any case, I was fortunate because I was working with very intelligent people. Polymer science was in its embryonic stages, but the staff was very competent and multi-disciplinary. Most of them were willing, if a neophyte was curious enough, to take him under their arm and help him. I go up and down the hall of the old physics lab today, and it is as if every one of them was there: I remember what they did, and what their work was. I was then eager to absorb everything, a real sponge. The whole work interested me so much that I learned quite thoroughly what every one of them did.

It was a very well staffed lab at that time. Not only did it have many good professionals, but also many good people if you wanted to start development of a new product. You could count on a good engineering department that was under Porter Hart, who went to Texas early at that time and you can read about him in the book on pioneers (2). [Edwin O.] Barstow and Joe Frank. My goodness, there were so many people that I can remember at the time, up and down the hall. We had people like Larry Amos, who developed mass polymerization. Steve Stowe and Freddy Soderquist, experts on cracking. Bobby [Robert] Dreisbach, was the inventor of the particular styrene process that Dow used. There was Harold Robinson, a very good chemist, who played a major role in the development of the precipitation of magnesium hydrate from sea water. Jack Chamberlain and Paul Shelley, who were doing a lot of work on acidizing wells for the Dowell development. Ray [Otis R.] McIntire, who played a big role in the styrofoam development and also in the very early work on impact polystyrene. Pauly Hansen, who had done much work on the very early stages on using the twin screw extruders for devolatilization, also did a lot of work on the development of copolymerization. After finding out about Alfrey's scheme: he was the guy who figured out how to keep a constant monomer composition so that he made good constant composition of co-polymers, e.g., styrene with maleic anhydride. I remember Dow having the first electron microscope. Bob [Robert D.] Heidenreich, who went to Bell Labs, was the first guy who worked on that.

The Physics Lab was multi-disciplinary—competent, with many inventive people. John Grebe was an inspiring director—there is no doubt about it.

BOHNING: Did you have any interaction with young lab technicians?

RUBENS: Yes, I did. But one thing about the way we worked was that once you became established you started doing things quite on your own. When the decision was made to adopt the Dow process—the Dreisbach process for making styrene monomer—almost everyone in that lab started to work on some aspect of the styrene process. Larry Amos was working very intensively on monomer production, and had a pilot plant on alkylating benzene at that time. By then Bobby Dreisbach had moved further and was trying to make divinyl benzene and chlorostyrenes and many other styrene derivatives. He'd been successful with styrene, and he thought that if styrene was available at twenty-five, thirty cents a pound, chlorostyrene ought to be available at ten or fifteen because of the low price of chlorine. When the war started and the decision was made for Dow to build the styrene plants around the country, many of our engineers who were in the Physics Lab at that time went off and supervised the building of those plants in Texas, Missouri, and California, etc. Because of that, we lost many of our engineers at that time, and the technical personnel went out and we ran the pilot plants. I remember the feverish activity of that period. I ran a pilot plant for Amos, from four to twelve, and I worked for Boyer from eight to four. We were just working sixteen hours a day, week in and week out.

We learned how to operate those pilot plants, and how to scale up; scale up work was a little different then it is today. People were often asked them to scale up from the test tube to the full plant without intermediate steps.

BOHNING: How long of a period did this sixteen hour a day last?

RUBENS: I would say for about two years. But then I had gotten quite interested in styrene polymerization myself, mainly because of my very early work on the light and heat stability of Saran. Under Boyer I was also looking how to stabilize polystyrene. I was doing a lot of sample polymerization with many other added materials, so I had learned a fair amount of polymerization of styrene in small samples and one of the problems that I looked at was related to the distillation of styrene. Sylvia Stoesser and Bob Dreisbach working in the early stages of that styrene development had run into polymer formation during the distillation and they identified some chemicals which could be used as inhibitors. When I came on the scene, sulfur was the inhibitor that was used in the distillation towers for styrene and tertiary butyl catechol was being used as a storage inhibitor. That was Sylvia Stoesser's work. There was not too much quantitative data, though, on how polymerization inhibitors performed, so I had started to work on that and I had developed a method to obtain such data. This is described in the old Styrene Monograph (3). The technique is described in E.N. Luce's chapter, which shows the dilatometer. Luce was head of the analytical lab at that time. I made a device that I could focus on the capillary attached to the reservoir in which the monomer was thermostatted. Then I had to be able to record a drop in the meniscus in this capillary as polymerization proceeded, so I used an old mondrome that was nothing more than a six-inch drome that rotated at variable speeds. By putting graph paper on it and using the follower system where the light beam would pass through the capillary it would focus on the turning drome. You can see just how primitive some of these things were. All I had was this capillary but the device made a nice plot on the chart paper so you could get the height of the meniscus.

Well, that instrument led me later on to a lot of work on polymerization kinetics. This apparatus was only good for the first five or ten percent of polymerization, because the system then started to harden up and the technique was no longer useful. It was good for observing the initial phases and for studying inhibitors, as described in this chapter that Ray and I co-authored (4). We have sections on catalysis, co-polymerization, casting bubble-free polymers, and various things like that as well as the inhibitor part. There were other studies done concurrently. Also, it wasn't an accurate test. It sufficed for the general behavior at that time.

We also used another technique in our early work: the modified Ostwald viscosimeter. Here the interpretation [of polymerization] was less accurate than the shrinkage as you might expect because you were dealing with molecular weight (as well as concentration) as it influenced the viscosity. All we did was seal up these Ostwald viscosimeters with the monomer and different amounts of inhibitor and then thermostat and measure the rate of flow after different times of reaction. That's how this type of curve was measured. [Rubens points to graph in note (4).] This is some of my work that was done back at that time. In the course of this work, I did find the nitrophenolic inhibitors for styrene polymerization (5). That is one of the stories which maybe I shouldn't tell. Because I wasn't really looking for the inhibitors, it was simply the fact that in the course of doing some of that Edisonian work and looking for styrene light stabilizers, I worked with some nitrophenols and even with a small quantity I couldn't polymerize my samples. Well, by that time I had learned enough about inhibitors.

This seemed to be far more effective at higher concentrations than the inhibitors we were using. We had never been able to distill that diethyl benzene. I used to get a kick out of Bob Dreisbach because he would take the diethyl benzenes and ran them through his cracker and ended up with some of the uncracked diethyl, but also with some ethylvinyl, as well as the divinyl. When he tried to separate the mixture using a hundred plate pod [Podbelniak still] all of a sudden he would get popcorn polymer formation which busted those pods because the popcorn always expands when it polymerizes. So, at any rate, the first patent we got was on the nitrophenols. You can see the difference here. This was one from my work dealing with the 2-nitro-4,6 dichloride and you notice that its induction period is still going along nicely at an exponential scale at the highest concentrations. That's not true when you work with TBC [tertiary butylcatechol] and sulfur which are almost an order of magnitude less effective. I remember when I wrote the report Bob happened to be at the reporting session and he got all excited. He went out to his distillation set up and he put the nitrophenolic in there and he didn't have any problem with polymerization and breaking his pod.

[END OF TAPE, SIDE 2]

RUBENS: Old Bob, he rushed over to the patent department, and asked Avery Sterns to write the patent immediately. Avery asked him what went on and Bob said, "Well, using nitrophenolic inhibitors to distil divinylbenzene," so Avery said, "Well, I'd be glad to, Bob, but it doesn't make much sense as Dow already owns a patent on that." Bob said, "Somebody stole my idea." It was so funny. I had written a chapter also with Larry Amos (6), but this is work that I had done on the divinylbenzene myself.

I was trying to make low cost dielectrics, as I mentioned before, and trying to copolymerize divinylbenzene. There was a military need for it because we didn't have the polyethylene in this country. I remember Ray Boyer working on a method of making low loss dielectric cables for the coaxials and, of course, with polystyrene, brittle as it is, it was virtually impossible. He had a revolving orifice and he'd take a strand of the hot polymer and wrap it around and he was coating it with polyester butylene as a lubricant, and then they were wrapping a second layer around and putting a sheath over it. It gave a surprisingly flexible coaxial. Ray may have talked about that. There was a need for end connectors so that you could solder without them melting down like polystyrene. Because we had the divinylbenzene available and as I had been making some copolymers with styrene, I thought that might be the thing to do. Other companies were already casting methyl methacrylate—Du Pont was in the business, but I thought that if we made those castings, we might have a product to sell. Although we could only anticipate a small market, this was a viable proposition for a time when Dow probably had thirty-five to forty million dollar in sales of polystyrene.

I became interested in cross-linking because I was making these cast samples and measuring their physical properties. Here was the type of thing we used to do. We'd take the triangular coordinates of composition on a graph paper and set up the experiments by picking the areas that we were most interested in. We had good quality divinylbenzene, particularly the meta-divinyl for making these co-polymers. Boyer had contacts with all the electronic companies at the time, and knew of their interest for plastics with good dielectric properties. Ed [Edward B.] MacMillan, who worked at Dow for four or five years, left Dow and went to MIT and worked with [Arthur R.] von Hippel. You know von Hippel was the man who did so much work on studying the dielectric properties of materials. It turned out that von Hippel was always interested in many of these polymers we made and he tested them. We were making co-polymers with one, six, and ten percent divinylbenzene, and we had really excellent electrical properties. In the late thirties, [Gaetano F.] D'Alelio had characterized a product made from a mixture of diethyl, ethylvinyl, and divinylbenzenes obtained from cracking diethylbenzenes and then adding a bunch of inhibitors, and heating it up to get enough polymer to precipitate out of methanol. He recovered that product and called it a soluble, fusible, cross-linking agent for polystyrenes (7). General Electric licensed the patent and made one copolymer with it that had a cross-link density equivalent to one percent divinylbenzene. Its electrical properties, according to measurements that von Hippel made, were poor but, because of the war, the electronic companies were getting cost plus ten percent for their products. So, General Electric chose to sell their cast polymer at fifteen dollars a pound for their Textalite 1421, and our marketing people decided we could get five dollars a pound for our product. Every electronic drawing of equipment at that time would come out with a specified polymer: 'General Electric Textalite 1421' and in small print underneath it, 'Alternatively Dow 270025'. This was because General Electric had a strong lobby in Washington with the military and Dow didn't. Nevertheless we made the polymer through the war and sold it and I guess it performed its job pretty well.

At the same time then, I became quite interested in a polymer developed by W. O. Baker at Bell Labs, and produced by Rohm and Haas—Paraplex X100. This was an attempt, as I understand it, by Rohm and Haas to make a polyester synthetic rubber. They were following the techniques that had been pioneered by [Wallace] Carothers, and worked on by Baker, and they produced a high molecular weight crosslinkable elastomer. Their contention was that there was no residual unsaturation, but it turned out that they were adding about one mole percent of maleic anhydride which isomerized under the high vacuum conditions to high molecular weight, in the same way that Carothers used to try to make superpolyesters. I started working with that and I was not aware of [Carleton] Ellis' work using unsaturated polyesters at that time, which had appeared in the late thirties (8). What I wanted to do was to improve the toughness of polystyrene. I got hold of some of this rubber, dissolved it in styrene, and polymerized it at the level of maybe forty percent polyester in styrene. It was a very tough material. Fact is, I think that Bill [J.W.] Everson in his chapter has some of the pictures from the samples that I made during that time (9). There was a lot of need for potting electronics at that time in military equipment, to keep things from vibrating loose. At one time, they used tung oil for this type of thing, and again, when WWII started, tung oil was simply not available and so we worked very hard to develop systems that could be used for potting electronic parts in place of Paraplex X100 that was the material previously used for this purpose.

There's a very interesting story about this book (3). The question is about whether it should ever have been written. If you notice, it's written by practically all Dow authors and everybody was concerned that we were going to give the ship away. But I think in the final analysis, it helped to establish Dow as a leader in the polystyrene business. Anyway, it was a tough material, but also a cross-linked elastomer. Every time I'd grind it up and try to mold this stuff, we would get good impact properties but not thermoplasticity. I think that this material had a big effect on the persistence to find a way to make a rubber work. Some of the early ideas of grafting polymers were being talked about at that time, when I finally identified the fact that this type of polymer had about 1% of fumaric unsaturation, which was responsible for the properties it showed when cured. Then we also tried to use this type of polymer in making body armor in WWII. There was a project going on over in the Cellulose Laboratory using fiberglass cloth and ethylcellulose films, laminated and melted together in a press, and it was a pretty good type of material. I was trying to make new body armor in a different way by taking this unsaturated polyester from Rohm and Haas and diluting it about fifty percent with styrene, adding peroxide and making a laminate in that way, what is called a low pressure laminate. In fact, we used to make foot-square panels, take them out by the old Dow property in the back of the plant security, get the Thompson sub-machine gun, and work them over with the 45 sub-machine gun. We had pretty good results. Just about the end of the war some of that was put into use in aircraft. It was too heavy for troops to wear for ground service, but it was the start of the modern body armor type.

Then I became interested in the laminates. By that time I was aware of Ralph Ellis' work in making unsaturated polyesters back in the late thirties. We continued our work and it seemed to me a good opportunity for Dow because of the rapid polymerization. The fact that there was sisal, glass, and other reinforced materials in the market suggested that it could be used for the manufacture of large plastic articles. In fact I was in Chicago in 1943 and I remember seeing some of the things that were being made and it looked like a good opportunity for a new market. But Dow had a rather interesting philosophy at that time which prevailed for many, many years, and that is, if we're making the basic component, as in this case, we should not get into the business of our customers. I recall companies like Reichold, Rohm and Haas, and Corning were making unsaturated polymers. But Dow did not want to compete there, so they backed off. They reasoned that synthesizing the unsaturated polyester was a cooking process that almost anybody can do if they had the proper kettle, and we were going to sell them the styrene. When the war ended, the question came up: "Well, what are we going to do now?" This was a very interesting time throughout this period. We were doing almost everything, whether it was precipitation of magnesium hydrate from sea water, or styrene or these co-polymers to meet defense needs.

Now, all of a sudden, the military didn't need us anymore, so there was much soul-searching in the laboratory as to what to do next. John Grebe went off to the Bikini atoll to watch the testing of the nuclear bomb and the atomic bomb. People throughout that laboratory were very uncertain. I remember that while a lot of people were griping about the way things were going, Ray Boyer had a real vision of the future of polymers. In 1947 Willard Dow came over and while many people were griping, Ray had prepared his flop charts and had made many projections of where the plastics industry [might be] and how Dow might participate in it. He must have impressed Willard pretty strongly because the next week the announcement came that Ray would be the new Assistant Director for the lab, and that a year later he was going to take it over. By that time, John Grebe had become enamored with nuclear energy and went to Oak Ridge for a year or so. It was then that Boyer took over. Bill MacIntire played a very crucial role in the work on impact polystyrene. Interestingly enough, its properties were pretty good when you made the polymer fresh and molded it, but they generally tended to deteriorate over a period of time. Larry Amos and Bill MacMillan realized that the rubber underwent some oxidative degradation and they started putting quinol in it, which helped tremendously.

There was the question of the manufacturing process. Dow had made a polymer called Styroloy during the war. It was a toughened polystyrene. It was more of a core-shell emulsion type polymerization loaded with stabilizer, but it never really made it anywhere. Only in the latter part of the war, 1945, did they start to get to a point where they thought it might add up to something. Larry had a stirred polymerizer that he'd built to take care of the heat problem of the old Dow can process. The eighty-pound tin cans used to make polystyrene led to a product highly heterogeneous in molecular weight. The can was held in a water bath at 80°C but once the reaction thickened up and started to auto-accelerate, it wasn't unusual to get 260-270°C. The cans were then opened, the polystyrene milled and dried in tumble blenders, finally running through rubber compounding rolls to try to homogenize it. Larry recognized those problems and tried to develop a bulk polymerization system where he could indeed control the polymerization exotherm. Having an agitator in it was important to maintain contact of the polymerizing styrene with the heat exchangers as the reaction progressed. When it came to putting rubber in it, I was getting insoluble polymer after polymerization, so I was getting enough co-polymerization. I wasn't doing it with agitation. I could still compression mold and get the impact properties, but as Ray Boyer said to Amos, "You'll never be able to run that polymer because it's cross-linked. So your agitator won't work." Well, they ran it anyhow and indeed it worked. But no one knew anything about phase separation or phase inversion until some years later when Gunther Molau was studying the emulsion systems. As I remember it, the real reason was that some of the polystyrene that they were making had what they called "fish eyes" in the molding, i.e., accumulations of polymer that didn't flow and showed up as little depressions and irregularities. He felt that if he could polymerize it in a stirred reactor he wasn't going to have those fish eyes. Q475 came out as an experimental impact polymer, about 1948 when we were selling some of it.

An interesting point about industrial chemistry. It wasn't too many years ago, maybe five or six years ago, at a meeting here in Midland we were talking about how to capitalize on fabrication—plastic fabrication—that we were studying in Dow labs. I had lunch with one of the consultants who told me that his most exciting time in a lab was when he discovered how to improve the impact properties of polystyrene by rubber modification. When I asked him where he was working, he said, "In Monsanto, about 1954." He was not aware that Dow was actually selling GRS modified polystyrene as an impact polymer, maybe six years before that. But I don't blame him, because much of that stuff is never made public, and it's just the fact that success has got many fathers and failure is an orphan. The point is that you have to go back to the written word to get a sound basis for such facts.

BOHNING: I have a couple questions in that war period. You had mentioned that you went to an SPE [Society of Plastics Engineers] meeting in 1943. Did you go to many meetings and do a lot of reading?

RUBENS: I was always a voracious reader. I think that was very important for me, as was the fact that I was never one to depend much on talking to a one person. I liked to talk to my colleagues and get their ideas and hear what it's all about, but usually what I did was go to our excellent library in Dow. I would take four or five books that treated the subject in question and after reading them draw my own conclusions on what the state of the knowledge was. As I said earlier, we had a wonderful group of people who were always willing to help and tell you what they knew on a subject. My own preference, however, was to thoroughly review the literature. If I were to single out one instance which benefited me more than any other at Dow it was about 1948 when Ray asked me to write this chapter on the polymerization of styrene (4). It forced me to go back and examine literature in depth. This was the first experience that I had in writing. He said, "You go ahead and write it. I will be the editor with Ray Boundy. I think it will be good for you to write." So I wrote it and I think it was the best experience I could have had at that stage of my career. Of course, the other chapter was on the polymerization of divinylbenzene, where I described my work and reviewed the literature (6).

[END OF TAPE, SIDE 3]

BOHNING: I wanted to ask you some more about this book. Where did the idea originate?

RUBENS: Well, it was going to be written in memory of Willard Dow. Ray Boyer and Ray Boundy were the key people who thought that this would be a good thing to do.

BOHNING: Was company approval needed along the way?

RUBENS: Yes. Boundy was Director of Corporate Research and he had the power. There were people who criticized it. Boyer was at that time serving on the research triumvirate, and he had a lot of influence. People listened to Ray. I think of all the people that I have ever met in industrial science, Ray was the greatest champion in basic research. He really believed that studying and understanding fundamental science was the way a company could grow very effectively.

BOHNING: Of course, Herbert H. Dow also established the company on the same principles.

RUBENS: Well, I never knew old H.H. because he died in 1932. But yes, I've read his biographies and he would have been very much along those lines. In fact, I think that it was Boyer's reading of H.H.'s research philosophy that had influenced him. Certainly Boyer's beliefs influenced me tremendously in the way I approach problems throughout my entire research career.

BOHNING: Before we started taping, you were looking at the section of Ray Boyer's transcript on impact polystyrene. Could we go back and talk about that a little bit?

RUBENS: Polystyrene grades were extremely brittle at that time. We were looking, first of all, at all sorts of low molecular weight plasticizers. Materials like polynuclear aromatics and polyalkylbenzenes soften polystyrene, but they never enhanced its toughness. They simply dropped the glass transition. The only lead that we had was that the old Ostromislensky patent where he had added rubber into styrene and claimed to have got a tough, dry material. The next thing we had was a Paraplex X100 casting system where we had found that, indeed, a polyester type of additive copolymerized with styrene and gave a very tough casting. But we realized that we had always assumed a soluble gel. We didn't know how it originated, but we kept looking for something that would give us good impact properties and moldability at the same time instead of such a cross-linked network. Do we had looked at Bobby Dreisbach's rubber, that was methyl isopentyl ketone and butadiene rubber, we looked at all sorts of natural rubber or deproteinized rubber and everything else that was available. We had looked at some of the early butadiene styrene rubbers, we looked at straight polybutadiene that was made in bulk. All of these things, when put into styrene, would swell, but none of them dissolved. An indication that we might be able to make something that was moldable was the addition of dodecyl mercaptan into the formulation, as was done in the case of butadiene-styrene synthetic rubber (GRS) for reducing its molecular weight.

The GRS really came from Germany. The Germans tried, in the thirties, to teach companies in this country to make styrene butadiene rubber. But the British and the Dutch had the monopoly on natural rubber, so every time some company seemed to be interested in making synthetic rubber they'd just drop the price of natural rubber like a yoyo and then, when the interest subsided, they'd bring it back up again. By looking at the wild fluctuations in price of natural rubber from the twenties to 1940, you realize just how well those people managed it. OPEC [Organization on Petroleum Exporting Countries] could take good lessons from them.

But the point was that when the decision was finally made to go and make GRS, the available emulsion formulations gave polymers with such a high molecular weight that the rubber was practically intractable and couldn't be broken by rubber mills like the natural rubber. So the first polymers that I remember were made by adding a lot of tall oil and other additives to get enough plasticity and be able to handle it. The tires that were made would run about two to three thousand miles and then disintegrate. But in 1937, Frank Mayo came with the chain stopper idea (10). It took a couple of years after they were manufacturing GRS before they started adding dodecyl mercapten and getting completely soluble GRS. That really wasn't an assurance that we were going to be able to make anything that was much better, but we felt that we'd make something much more homogeneous. Some of that early polybutadiene rubber that we put into styrene led to an heterogenous mixture. If we were lucky to get the right piece out of the polymer, it felt tough. You could hit it and the impact was obviously improved, but we didn't know of any way to make it consistently. It just seemed like a good idea.

As soon as this was done, solutions of the soluble Buna S rubber were prepared with up to twenty percent in the styrene. However, it was very difficult to get a homogeneous solution which was pourable at room temperature with more than ten to twelve percent of the rubber. For this reason work was limited to concentrations of ten percent rubber in styrene. We used benzoyl peroxide as an initiator and one and a quarter inch diameter one foot long sealed bombs as reactors. Polymerization was maintained for eight days at 60°C, two days at 80°C, and two days at 120°C. After this, samples were allowed to cool slowly before being turned over to Ray McIntire for molding and physical testing of the polymer. From there on, Ray McIntire and Leo Kin continued the work. Ray Boyer had built a heat distortion apparatus at the time which was nothing more than a little device that would take one-eighth inch thick samples and load them on the center. We would then heat it at constant temperature rate until there were five mils deflection and that was the point we took as the heat distortion temperature. We varied our soluble rubber in increments up to ten percent, and used constant peroxide. We measured the flat Izod impact strength, which isn't a very good impact measurement, but that's what we had at the time. Even at one percent, there was quite an improvement. Five percent rubber was pretty good, and ten percent was even better, but we noticed that at five percent, our heat distortion values were still all staying up. Our tensile strengths were still fairly constant, but at ten percent, we started to get the drop in heat distortion. That really was what we were shooting for. Also, we used to look at the dielectric constant and the loss factor of these materials because the electrical properties were a key interest.

After this initial piece of work, I don't think I touched anything on impact enhancement of polystyrene until about 1949. That's when I did work on trying to establish that these were branch structures. There was no understanding of the morphology of the rubber modified polymer until, at least in our company, Gunther Molau did electron microscopy. He looked at the osmium-stained cross-sections and recognized that it was indeed a continuum of polystyrene with included rubber particles (11). It also had inclusions of polystyrene in that matrix. But when he found that out, the first realization came that our competitors were using the Amos patent to make their products (12). See, that's ten years after the patent was issued. I dare say, if Dow had known that ahead of time, it would have gone to these companies, and they would have been able to make an equitable settlement. As it is, you can not go after ten years and say, "Well, you must owe me about fifty to seventy-five million dollars in back royalties," and have any chance of getting them. I felt that discovery really was a true discovery—a true patent. There wasn't any precedent to tell you that if you polymerized rubber in styrene by an agitated system you were going to get a phase inversion and that it was going to have these properties.

Fact of the matter is, there was no phase inversion, but there were impact properties. If you look at the microscopic structure of this material, you'll find is that it's almost like a three dimensional fishnet where the rubber phase is a continuum and the polystyrene in between. It was a discontinuous phase. It was not exactly the same thing that we think of today for the morphology of ABS [acrylonitrile/butadiene/styrene] or of an impact polystyrene.

BOHNING: How rapidly did you move through those five ranks that you had indicated earlier?

RUBENS: Well, there was officially a minimum timetable that you had to follow, but I think that I managed to convince them that I should have all the tests in about four and a half years, or something like that. So I went right through that. Willard Dow had some experiments that he wanted to work on. He had called our lab management in 1945 or 1946 and said to shift me over to a salaried position.

I went through project leader, group leader, lab group leader, and then I had the opportunity, around the mid-1950s, to become a division leader of what was then the polymer division. The lab had about one hundred and twenty to one hundred and thirty people at that point and had an engineering division, an inorganic chemistry division, a polymer division, and a foams division. But about that time, Boundy announced the dual ranks system, the opportunity to be a professional scientist or to go into management. I turned down the management opportunity, and chose the other way. Most people said I was crazy with my educational level. They thought that the professional scientist career would be reserved for Ph.D.s. As it turned out, this was not the case because in 1957 I had the Associate Scientist rank and in 1967 I had the Research Scientist rank. I don't know whether I became a development engineer or something like that within twenty-four hours. But that was the way it went.

BOHNING: When you made the choice to stay in research versus management, what was the general feeling about the other people who had to make that choice?

RUBENS: You know, there weren't very many who did at that point. I remember reading Ray Boundy's summary on what was expected of the people who followed the professional ranks—what their responsibilities would be and so forth—but there were not too many that were consciously making that choice at the time. There were folks like Wiley and Turner Alfrey who never went through the Associate Scientist rank. A number of other people did, and it turned out that very few people who became Associate Scientists reached eventually the rank of Research Scientist. I dare say that there probably wasn't one in ten of those who went the professional route that became even Associate Scientists. They were moved out.

BOHNING: That's interesting. Is there still such an arrangement today?

RUBENS: Yes. Boundy's perspective was that when that type of classification was fully developed in the mature company, there would probably be about a hundred Associate Scientists and around twenty-five Research Scientists. And this is the situation today, more or less. I felt it was a good way to go for me. I really couldn't conceive of any other way, I guess, because the things that turned me on the most happened in the laboratory, and I could see management as the less attractive area. So I guess I did the right thing.

It was around 1954 or 1955 that I had the opportunity to move up in research management, but from the time I made the decision till I was promoted over into that category it took another year or so.

BOHNING: Let me back up for a moment. Something you did mention earlier and I didn't follow up on. You said that Willard Dow was responsible for moving you to a salaried position because he had certain things he wanted you to do. What did he ask you to do?

RUBENS: Well, this is kind of an off-beat story, but it goes back to some work that was being done by one of the fellows in the physics lab in the forties. He became interested in chemotherapy for cancer. There were some ideas on the oxidation reduction systems—that you could look at, for example, the ascorbic and dehydroascorbic acid systems; the inositol and rhodozonic acid (triquinoline) systems of that kind. I had been working on compounds of that type because of my interest in free-radical inhibitors and in antioxidants. So it turned out that I had got together with this fellow. He was getting mice from Bar Harbor, and I was synthesizing compounds. I had oxidized TBC to the corresponding quinone and so forth. He'd take these compounds and make ointments out of them and apply them topically to the tumors on these mice. Bar Harbor was producing strains of mice that you could predict almost to the day in their lifetime when they were going to develop a tumor. Researchers around the country were trying to find chemical answers to how you could treat cancer.

Mice really were the animals of choice. Jim Sheridan had done much work on trying such compounds and was getting statistically significant results. At that time also there was a medical doctor who had been a professor at the University of Michigan and who had come up with some similar types of compounds that he was giving to people experimentally. It was very primitive work on chemotherapy. This fellow's name was Bill Cook. He had come under very severe criticism from the American Medical Association (AMA) for the type of work that he did. He was on his own, having left the University of Michigan, came up here, and somehow he got acquainted with Willard Dow. The question of him being able to continue some work and to have somebody he might interact with at Dow came up and Willard Dow, knowing about Sheridan's work, got him involved. Sheridan mentioned that I was making these compounds for him, so Willard, who had met me, said, "Well, let's get him to work, too."

Well, that became very controversial because the Food and Drug Administration was very much opposed to Cook continuing to do that kind of thing. Generally, he was working with terminally-ill cancer patients. I suppose in many respects it would have been fine if Cook was supported by the AMA. This went on for a couple of years and we made compounds which were tested in this way. I tried to get Dow to build a cancer research laboratory in Midland in order to be able to do screening work, and to pursue this type of chemistry. I don't know whether Cook was afraid that the compounds that he was promoting would be shown not to work, but I synthesized many of those compounds for him at the time. Finally, there was a federal trial against Cook which ended as a hung jury, and Cook left the country. It was at about the same time Willard was killed. The rest of the Dow management didn't want to get involved in it and I was told that I should terminate my work in that area.

BOHNING: How long were you involved in that?

RUBENS: Oh, from about 1945 to 1949.

[END OF TAPE, SIDE 4]

RUBENS: It was an interesting thing. Just about six or seven years ago I ran into some articles in some Russian journal that had been translated into English. Some of the compounds I was making then were things that the Russians were working on, and they were claiming that some were broad spectrum antiviral agents and so forth. Now, whether they really are or not, God only knows. It was too late for me to get back to it. So that's the way it went. After Willard was gone, there was no support toward this kind of work.

BOHNING: Were there any other Dow employees working on that?

RUBENS: Well, Jim Sheridan, who left the company and was off on his own, and God knows what he did after that. But he was an interesting guy. The fact is, he took his law degree while he was working for Dow and became a lawyer.

BOHNING: Did you get much contact from other professionals as a result of your chapter on polymerization of styrene, outside of Dow?

RUBENS: Oh, yes. I had a lot of contacts over the years with professors as a consequence of that—like Bob Simha, who used to tell me that in the courses that he was teaching the chapter on polymerization of styrene was required reading.

BOHNING: The styrene divinylbenzene copolymers—some of those ended up used as ion exchange resins?

RUBENS: At the time, Bill [William C.] Bauman occupied the lab with Harold Robinson, and Bill became interested in the business of softening water. What was available at that time, I think, was called green sand. That was the packing in the very early water-softening industry. Bill got the idea that if he could attach the functional group to an insoluble polymer this would allow him to use these polymers as ion exchange resins and to regenerate them with appropriate salt solutions, such as sodium chloride. I was making these copolymers in rod form at the time. We had not done any bead polymerization then. Bill got hold of some of the styrene divinylbenzene polymers, ground them and sieved them, and then did a sulfonation. He tested them for their ion exchange capacity and they looked pretty good to him.

In fact Dow started a major program on making them. Bead polymerization, of course, was already known at that time, and the logical way seemed to be to make the copolymers by suspension polymerization. They had proven successfully that the ground up cast rod would work. But, as it turned out later, our friend D'Alelio had upstaged him and patented the use of the sulfonated styrene divinylbenzene copolymer for ion exchange. Dow licensed it and there really was no great difficulty in making it. D'Alelio was a shrewd guy. He had a marvelous ability to read what people were describing as problems in the specifications in their patents. Then he'd figure what he could use that for. I'm sure that he must have written an awful lot of paper patents but he had tremendous intuitive capabilities to figure what they'd be good for. Anyway, Bill did that work, and it culminated in a major success for Dow over the years. I don't know if it was Nalco or if it was one of the other companies that was involved in selling the ion exchange systems for water softening that Dow made a deal with in the initial phase. Now they're making these modern dispersed particles with a system that Ed Timm has developed to get rid of the normal particle size heterogeneity in the regular suspension polymerization. Ed came along with a method of using a baseplate in the bottom of a tank and pumping the styrene plus divinylbenzene monomer containing appropriate initiator through the small holes in the plate and then sonically exciting it. The droplets that come off are absolutely uniform, and there's enough Methocel colloid stabilizer in the aqueous system to keep them uniform during the entire polymerization.

They have some very special uses, but that's the type of resins that Dow makes today. Bill did so much work on separation of rare earths through these resins, and he worked on an awful lot of ion exchange chemistry. He is probably the best-known ion exchange scientist the company has had. Probably many other people came in later and did a good job with Wheaton and Anderson and so many others.

BOHNING: When did you start work on foams? Did you do any urethane work first?

RUBENS: Yes. The fact of the matter is that after the war, when this group went to Germany and had access to the German manufacturing—[J. M.] De Bell, from De Bell and Richardson; [William C.] Goggin (you've run into Goggin before); and another fellow by the name of [W. E.] Gloor. They went to Germany and studied the German plastics practice, and wrote a book on it which was published, I think, in 1946 (13). I'm not sure, maybe I've even got a copy of it in my library here. I know I have at work. But as I read that book, I looked particularly at the urethane chemistry that had been done at Bayer. I thought that was a tremendous opportunity for Dow. In fact, I've got reports which identified all the compounds that we were making with multiple active hydrogens, e.g. diamines, glycols, polyglycols, etc. I was able to buy toluene diisocyanate from both Monsanto and Du Pont. The Du Pont material was certainly a higher quality product than Monsanto's, but in addition to making the toluene diisocyanate, Du Pont also made the 1,6 hexamethylene diisocyanate. They had the 1,6 hexamethylene diamine from making 66 nylon, so they reacted it with phosgene to prepare the acid chloride and from that made the isocyanate.

I was able to buy those isocyanates and I made a variety of products, including some very primitive foams. I knew about them by reading the German information: Bayer had done a very nice job, I thought. His publications in the late 1930s and early 1940s, when they came to light after the war, were very nice pieces of work. So as it turned out I became very much interested in the opportunities for Dow in this area, and in 1949 Otto Bayer came over, knowing Billy [William] Hale, one of the early directors of the organic lab. He was brought in to direct it in the first place by Herbert Dow, and later married his daughter. Bayer wanted to have a joint venture with Dow on urethanes. I knew Billy Hale quite well because he was interested in these cancer drugs too. I got invited to a cocktail party and had a chance to talk with Bayer. Well, Bayer was pushing very strongly to form a joint venture called DowBay. Our people did an economic study on isocyanates and said there's no way in the world we're going to nitrate and reduce and phosgenate and dehydrochlorinate and go through all those unit processes and finally distill and purify and make anything for less than one dollar a pound. Well, in 1949 actually one dollar a pound meant twenty dollars a pound at today's prices. So the net result was that they turned him down.

I remember something rather amusing about Mr. Bennett, the company treasurer at that time. When Bayer was here we were making a lot of styrene. We were cracking a lot of hydrogen off, so Mr. Bennett, who was very frugal with the buck, was most concerned, not so much about the new isocyanate and urethane chemistry, but what could Dow do with all the hydrogen they were getting from cracking? That was his interest. I suppose that's why things worked out pretty well—he kept close control of the buck. A bird in the hand was worth many more in the bush. I was only twenty-nine years old at the time and I saw many opportunities there, but it looked like it wasn't going to amount to much.

The following summer I had Frank Brower as a summer employee. He was taking his Ph.D. at Michigan at that time. I told him that we were not likely to make much out of urethanes but there was one thing I wanted him to do before we gave it up. Len Moseley and some of the guys over in Britton's lab were making some polyols using base catalyzed reactions with different types of diol and triol initiators. The foams that were being made by Lockheed and others were using primarily hydroxyl terminated polyesters. For his summer project I asked him to get hold of some of those polyols and isocyanates and find out whether he could make some foams. Frank did a very interesting piece of work, although we didn't know much about catalysis. We used sodium peroxide as the catalyst, and Frank wrote the report on it at the end of the summer. I took him over to what was then the organic chemicals group and were presumably looking for opportunities to market something. Frank described his summer work, and they looked at it and said, "That's interesting." So I had him apply for a patent. He said, "Well, we ought to be co-inventors on it." I said, "No, I'd like to have you get your first patent yourself, so you write it." It's probably the first patent on using polyols to make quite simple foams. It really didn't teach the critical things. It only taught that it was possible to make a soft cushion foam from a polyol. We knew nothing about the chemistry of getting cells to open at the right stage by using the right balance of tin and amine catalysts.

At any rate, we gave up at that point on urethanes. That was just about the time that Turner [Alfrey] and Ed Coward did the analysis on the styrofoam process. We had the old tower process, which was invented in 1935 and developed during World War II based on the old Munters patent—the Swedish inventor. It was largely developed at Dow by Ray McIntire and Larry Amos. They made the old process that fired out the logs of styrofoam from the old modified aniline reactors that had been salvaged during the war. Initially, that foam found some uses on the troop ships in life rafts and various things of that kind. But at the end of the war the whole thing went to pot. There was no civilian use for that kind of product. People started to search for uses and found that thermal insulation should be a potential use. That's when Olin Matthieson and some other companies came in and identified the cell size of the foam as being much too large for efficient insulation. Much work went on at that point, but again the people doing process economics and doing marketing studies said, "There's no use for this."

Grebe was the product champion in that case. It was because of John that that new plant was built in the first place. It was still a tower process, but it did make some of the foam that was cut up and handled much like the old logs were in the century before, and used for constructing beer cooler rooms and that sort of thing. The man I remember most was Lloyd McCurdy, who came along and started to develop a continuous process for feeding polymer, blowing agent, nucleating agents, to an extruder and developing the appropriate equipment so that the mixing of the blowing agent and the nucleating agents were facilitated when the polymer was hot enough for it to have low viscosity, and then being able to cool the gel to exactly the right temperature. You needed to know when it was going to emerge from the die to make a stable foam and that's where Turner Alfrey came in. He and Ed Coward, who was an engineer, got together and worked on it. It was mostly Turner's ideas. Ed had provided me with some of the engineering information. That's where I found it to be particularly interesting because, when working with Boyer and working with styrene-divinylbenzene copolymers, I had done quite a lot of work for him because of his interest in solvents for polystyrene. We used to use the old equilibrium swelling of gels to get to what was called the Flory-Huggins value, a measurement of how good a solvent was for the polymer.

This was the thing that intrigued me because we started talking about the role of the blowing agent in making foams. I already knew that permeability was a function of the product of the diffusion and the solubility coefficients. It seemed to me that the behavior of a blowing agent in a polymer would depend on its solubility coefficient. Measuring solubility by techniques we had at that time weren't that good, because the blowing took place at 25-50° below zero, so I devised a technique using sealed systems and quantitatively measuring the swelling of styrene-divinylbenzene copolymers. In that way, I was able to characterize virtually all of the volatile compounds that might be considered as blowing agents for polystyrene foams. Out of that work came a fairly coherent theory on blowing agent selection and what parameters you had to look at. I also got involved in looking at the effects of polymer structure during foaming. I had these cross-linked particles that I was swelling, nothing more than a quarter inch in diameter. I put quarter-inch pieces of cast polymer in a special piece of equipment where the resin imbibed solvent until it reached equilibrium and I could see from the height in the capillary how much solvent had been imbibed. I noticed that, after I had opened the vessel up when it had come to equilibrium, I could take the particle out and I would be able to weigh it but some of the samples would, as they approached room temperature, foam up into expanded replicas of the piece I started with. Well, that was for some of the better solvents like butadiene and CO_2 and Freon 11. I realized that other samples that had not taken up quite so much solvent and their glass transitions were higher than room temperature. So all I had to do was to warm them up and they ought to foam, and indeed they did.

1951 was really my start of work on this, shortly after Turner started to try to make expandable particulates of polystyrene. At that time I was completely unaware that the Badische was doing anything alone those lines at all. BASF [Badische Anilin und Soda Fabrik] didn't have a continuous extrusion process, and they were looking for an entry into expandable polystyrene. I had these particles and was foaming them up, and they formed expanded replicas of the original size. Very nice. They'd duplicate a piece and as expanded foam. In fact, I've published stuff on isotropic expansion of polymers, too (14), and have done quite a lot of work on that. I tried to convince people, "Look here, you've got this particle of polystyrene. Heat it to 105°, 110°C and it puffs up thirty or forty times the initial volume." Very nice cell structure. We had to figure some way of being able to heat that stuff up and mold and fuse it into coherent structures. At that time, I still believed that what you did was talk to engineers and convince them that that would be a good project. I might as well talk to the moon because no one took it up. This was a big fiasco for Dow as far as I'm concerned because we were right there at the right time and did not put anybody to work then on that phase of the problem. Fritz Stastney in Germany went to work on trying to mold it and discovered the steam molding process (15). It turned out to be extremely valuable to BASF.

We have an attorney who represents us on patent matters in Europe; I met him about 10 years ago. We were talking about when he became an attorney, and who he worked for, and it turned out that it was right at the time that Stastney did his work on steam molding. He said that when he went to work for BASF he told them he'd never written a patent case in his life. So they dragged out Stastney's disclosure and said to him, "We don't consider this to be a very important case but why don't you practice on it?" Thus he wrote Stastney's case, and of course it was issued as a U.S. as well as a European patent. BASF made more money, according to Michael Hahn, on that one patent than they made on all their other patents combined. It just shows you how really difficult it is to look at something in the early stages and accurately assess where it's going.

[END OF TAPE, SIDE 5]

RUBENS: We ultimately did learn how to mold it and we have a number of patents in that area. But the very fact that we had a continuous board extrusion plant did more to suppress our involvement in that type of work than anything else. About 1955 Koppers took a license from BASF and they started manufacturing expandable beads here. We had the whole thing on the shelf ready to go, but we were not manufacturing it. Once Koppers did so, then there was a management decision to make expandable polystyrene beads here. It was very important to Koppers and my counterpart there, Alvin Ingram. Some of his colleagues told me the story later—to get into foam via the bead route because they wanted to compete. They did not have an extrusion process as Dow had tied that up pretty well with patents. So they made it go (16); they put plenty of people in marketing and they even set up a molding demonstration in a major hotel in New York and invited a thousand people to come and see it. They were able to dominate the market and they won the game hands down as far as I'm concerned. We manufactured expandable beads for thirteen or fourteen years and it was never really the success it could have been for Dow because it was actually given to the wrong people. They gave it to the regular polystyrene granule salesmen to sell. It required more technical service—it was more of a specialty product. So Dow never really made out well on it.

I had learned a fair amount of stuff on polymer structure, viscoelastic behavior and foam agent behavior. However, no one was successfully producing a polyethylene foam, so I became very much interested in that. There the key problem was that we did not have equipment that would really control the temperature of the gel melt at the die, and with polyethylene it's a much more critical factor than it is with polystyrene. Although I knew what should be done to polyethylene to make stable foams from it because of the work with polystyrene, I didn't know of any way to do it at the time. I happened to be in California at an ACS [American Chemical Society] meeting; from there, I went up to our Pittsburgh plant, where Herman Mark was giving a lecture. He started talking about Art [Arthur] Charlesby's work at Harwell on radiation cross-linking with polyethylene. I said, "That must be the way to do it, to modify viscoelastic properties. Lengthen out the disentanglement plateau, so that there is less resistance to foaming, but on the other hand the viscosity is high and there is no shear thinning of the melt." Bill [William H.] Beamer, who was working at Dow at the time, had a two-million-volt electron accelerator and I went over and devised a piece of equipment with an orifice which allowed the beam to pass through a thin titanium window and irradiate the polymer just before it emerged from the orifice. It was a dramatic experiment. You could be making an absolutely lousy foam and turn that beam on and get a nice rectangular cross section with uniform cell size. It worked very nicely. With that piece of equipment, I was also able to evaluate diverse blowing agents. That was something I did not have before. It led me to select a blowing agent, in this case, the 1,2 dichlorotetrafluorethane. I soon found out the permeability of low solubility blowing agents through molten polyethylene is very high compared to that of polystyrene. Even as you start to cool, you blow the foam up and the fluorocarbon was leaving the foam faster than air could come in. So you were continuously subjecting a relatively soft foam to a positive compression. What would then happen was that the foam would just shrink so fast that a cross-section initially twelve inches wide and two inches thick a half hour later it might be nine inches wide and one inch thick. I applied the Hildebrand solubility parameter theory and selected blowing agents which had a reasonable probability of having a permeability more in tune with that of air. It worked, and the consequence was that I patented the only successful volatile organic blowing agent for polyethylene foam extrusion (17). It became the foundation of our foam business for many years.

We had a lot of headaches with it for a while—selecting feed stock, identifying critical feed stock parameters, etc. It was quite a job. I did that work in the early part of 1956 and again, Boyer was a very strong supporter. I often mention that as far as getting a new product off the ground, you absolutely have got to have product champions. As researchers we provide options for innovation, but if we don't have the champion to back it and to see it through until it can finally be commercialized successfully, we get nowhere. The assistant lab director at the time was Bill [William R.] Nummy and Boyer was director of polymer research for the company. Those two people supported it very strongly. But it took us some four or five years from the initial work until we were successfully selling some products, and over that period, I would say that nine out of ten people who were in management positions wanted to kill it, arguing that we will never make any profit out of it. But the persistence of Boyer and Nummy in supporting it ultimately was the reason it finally became as a successful product. You can't do it alone, that's all there is to it. If you don't have the support of management, the champions who strengthen you, you're whistling in the dark no matter how good the work you do.

BOHNING: Ray talks about what's happened at Dow recently in terms of having fifty cash registers instead of one, as he puts it.

RUBENS: The abominable no-man, as he saw it. No, I'll tell you what's happened, and I probably shouldn't but I can say it. In a rapidly growing company, and I don't think it matters whether it's Dow or any company, there is such a need to expand management globally that many of the managers who are hired are mediocre and are confined to the medium levels. Unfortunately, these levels are given the power to control research project budgets. Most of these people are not visionaries. They're sitting there with some god-like powers as far as controlling your budget is concerned. If your project comes to them and they're making project decisions which are not predicated on sound business, or sound scientific, judgment, the net result is that all they've got to do to squash a project is withhold the money. A well-known researcher probably can go beyond those people, but that's not the case for ninety-five percent of your younger scientists. But it isn't only Dow; I think it's the entire United States. I think that's why we have not innovated as a country as effectively as we should in the last twenty years.

BOHNING: The alternative seems to be that you innovate by acquiring instead of developing.

RUBENS: Well, have you seen the other pasture greener than yours?

The foam business has been an excellent business for Dow. I'm not necessarily enamored of how well it's been handled as a business, because I think we've abdicated many major opportunities that could have been converted into very successful businesses. But that's my judgment as against other people's. I still see it, as you probably know by reading that publication (18), as a very good growth industry, and I think it'll only increase in importance in the long run as the need for good thermal insulating materials and acoustic insulating products continues to develop. I would think that if you got the proper management support for young scientists that there's tremendous opportunity in that area. As I mentioned to you, in the case of Bayer and urethanes, we turned them down. In 1954 they went to Monsanto and formed Mobay. Monsanto ultimately had the misfortune to have produced TDI [toluene diisocyanate] back in that period between 1945 and 1950. So they were stuck in the antitrust legislation and had to divest themselves.

But isn't it funny that today, Dow is second in the world in urethane chemicals in the world, behind Bayer? It took us a long time to reach this position. Plus, I think, one person—Jim Linhouts, who was a research director at Dow back in the early 1950s when Dow started producing the Voranols—claimed that the reports we had written on opportunities in urethane chemistry in the late 1940s did have an influence on Dow's decision to go in and make the Voranols in Texas. So our effort wasn't all wasted. The interactions are rather complex in a company as big as Dow and you should just keep doing your best and hoping that every so often the things you do will have a chance to catch and lead to something. When we came into the period between 1955 and 1960, Dow and their American companies realized that they had really well-developed processes—for making products that could be marketed worldwide. It looked like a good opportunity to expand geographically the manufacture of these products wherever there was an affluent population. This was much less risky than to develop some of the new ideas that had never been proven commercially.

There are Dow historical data on how long its taken to develop the average product. Jake Eichhorn did an excellent job of looking at the Dow crown jewels. He looked at the gestation period from concept to successful commercialization for products that made profits of one million dollars before taxes. He found that for the crown jewels at that time there was, on average, a gestation period of about ten and a half years. In fact, it ranged all the way from as little as four to as many as thirty-five with magnesium. The very short gestation periods were for products based on acquired technology—like the ICI technology on high pressure polyethylene. But for in-house generated innovations, he found that the period from ten to twenty years was a good average. I would have put it longer than that because I know that his initial dates are in reality late by three or four years. It's a tough thing to handle if you're managing and optimizing money, and there is much less risk in building a plant on known technology for proven market of an affluent population.

I think people like old Herbert Dow believed the risk was worth taking. They believed that you prepared yourself for the next business upturn by making new products, you were going to capitalize on the opportunity. But I think it took a different breed of manager. I always think back at the time when I first came—I think that we were selling thirty-five million dollars worth of chemicals and I think Du Pont was some twenty-five, thirty times bigger than we were. Their profitability on sales was two billion dollars and everything else was great. You look at about 1974 and Dow at last caught and possibly passed them on that year. That was the result of tremendous management. But it seems that one part of the formula that may have been neglected was the innovation part of it. Now, they have reemphasized the fact that we are going to make specialties and we're going to innovate. It's an interesting thing to see what's going to happen because it takes a special kind of person and management to innovate. It takes a confidence and a championing of your research people and your other people to be able to see it through, and I'm not sure yet that U.S. industry as managed today is geared to do that.

BOHNING: How many scientists today are in management ranks?

RUBENS: Not many any more. They've really thinned out. It used to be that many of the people were like, for example, Ben [C. Benson] Branch, who had come through with the whole cellulose lab and had moved up through management, and had ultimately become Chairman of the Board. There were many people who had come up through research and ultimately were in major positions in the company. Like Goggin, who started in the old physics lab and worked his way through to become president of Dow Corning. There were a lot more than there are today. Many of the people today, I think, come because of their abilities to manage business and to manage finances. The people who have emerged at the top of the company: I'm sure they do a good job as far as what they've got to manage. I'm not so sure that the answer to innovating in the U.S. is going to be found in this type of management.

BOHNING: I gave Ray this quote and might as well give it to you, as well. I had read somewhere, I guess about half a year ago, that in the past a lot of basic research was done and then, once something had been found, there was a need to market it. Now the trend is the reverse—you have a need, and then you go out and find something to fill the need.

RUBENS: Well, that's an interesting point. I think the arguments of whether discovery- or need-oriented research as the way to go will always have proponents both ways. Forrest Stark, who was director of research at Dow Corning, came and talked to our scientists. We have a scientists' organization, in both central research and also in the Midland Astel groups. We have speakers coming in about once a month. I asked him specifically, "Well, how much of your work with Dow Corning do you think is based on outside or on inside stimulus?" He said, "Very little. We attempt to be the best there is in silicone chemistry and to have a very thorough basic understanding of what we can do with it. Then we look at where the market opportunities are for what we know we can do." So it seems to me that they lead from the idea of product strength. That is the thing that I felt we were doing many years ago in Dow. Their profits on operations still are around twelve percent by applying that philosophy. With a lot of our commodity business, at least, and the way we've operated, Dow profit on operations a couple of years back was running around two and seven-tenths percent or three percent, like a lot of the rest of the industry.

I guess I feel more comfortable having an expertise in a certain area, and if someone tells me or identifies an opportunity in the foam area, for example, I can tell them right off what we can do from the standpoint of science and technology—what kind of economics are involved, whether we have a particular capability of responding to that opportunity or not. I don't have to go and learn about foam. That's the same thing needed in all the various areas where business opportunities may emerge. You need that kind of technical capability in your organization right at the time because if you stop to think of the gestation period for successful innovation, if you have to come back and resolve the problems before you can do anything, rather than to have some capability of putting something in a potential customer's hand today. I just don't think you can do it. I think people are kidding themselves if they do. Because in any organization that's going to perform effectively you need that kind of basic knowledge, basic expertise. I would agree wholeheartedly with Ray that that was the way to do it.

BOHNING: I think it was H. H. Dow who actually had different groups within the company competing against each other.

RUBENS: I think to a certain extent that's right. But I guess I would rather have people reinforcing and cooperating with one another than competing. I know synergism isn't a good word but on the other hand, I think if you can get people to interact properly and have the team goals, you can make it work. Yes, I know this type of controversy theory but personally I don't subscribe to it.

BOHNING: One of the things that we haven't talked about yet amongst products you've developed is the composite foam system.

RUBENS: Oh, yes. Well, if you look at the open cell flexible urethane cushion systems, there's a certain limit you can go at a particular foam density with the so-called load factors. The stress/strain curve and compression is really nothing more than the flexing of the elastomer struts. Within limitations, maybe a factor of two, you can vary the stiffness of that elastomer but what you're really looking for is a way of improving the shape of the stress/strain curve so that you don't bottom out when you sit down. For example, the load deflection curve of, let's say, a typical 2 lb/cubic foot urethane cushion foam, shows that the stress is about 0.5 psi for fifty percent compression. Also, the curve is very quickly increasing and so by the time you come to rest there may be very little cushioning reaction. You're quite uncomfortable. So the idea here was to try to make a pneumatic particle because if you simply measure the stress/strain properties and compression of, let's say, a closed foam which behaves in a pneumatic manner, it's going to be 15 psi at fifty percent compression, just from P_1V_1 equals P_2V_2. That's thirty times the load bearing that the open cell foam has, so let's go ahead and figure a way to distribute these pneumatic particles in the open cell foam and then we ought to develop composite properties where we can just tailor our load. Because, with closed cell foam particles, dispersed in the open cell foam, you don't influence the initial softness very much, but you can still deform the softer phase. But as you start to compact it down, more and more of the load is being accepted by the closed cell foam, so this is where you ultimately come to a point where you can tailor the shape of the compressive stress strain curve to get just about what you want by controlling the volume fraction of closed cell foam.

[END OF TAPE, SIDE 6]

RUBENS: What you have to be able to do is take a non-foamed particle and disperse it in a reactive fluid precursor system. If you try to put a closed cell foam into the fluid precursor of an open cell foam, the phase ratios are way out of balance as you might expect. There's no way of mixing it up and putting it into a mold cavity and curing it up to what you want. So the composite system that we worked with was one where we designed special expandable beads. We can disperse those particles into the flexible foam precursor only putting in about five percent, so you hardly influence the viscosity of that system at all. You pour it into the mold cavity and as these particles are about the same density as the fluids, they rise up and stay dispersed in the foam matrix. Then, as the urethane starts to cure, the exotherm is enough to soften the particles and causes an expansion, so you have these particles dispersed throughout the open cell urethane (19). It's a system that's been reasonably good. It hasn't really been marketed and developed as well as it should be. I think one of the reasons for it is that people ultimately producing polyurethane foam don't particularly want to build something that's going to last a long time. Manufacturers hope that people will dispose of furniture in two or three years so that they can get another market for the foam. But I don't know whether you will ever see these type of systems developed. They offer a lot of improvements as far as material performance, but I'm not sure that they're ruling the market, that they'll ever take major advantage of it.

Of course ethofoam, as you know, has become a premier packaging for electronic equipment and that's where its major markets have developed. There's a lot of opportunities to improve virtually every foam that we've got and I'm curious myself whether Dow will really do the required basic work or whether any other firm will. One thing that strikes me—I've gone to Gordon Conferences on foam ever since they've started back in 1954, and I've always been surprised at the relatively few people attend fundamental conferences on foam science. They seem to be pretty much the same ones all the time, and relatively few young people who could do the basic research that could enhance the use of products of that type in the way it should.

BOHNING: Just as an aside, I live in Wilkes-Barre, Pennsylvania, just south of Hazleton, and there is a General Foam Corporation in Hazleton. Are you familiar with it? We've had some of our chemistry majors employed there from time to time. The only person I know there is Herman Stone.

RUBENS: Yes. Well, there are a lot of foam producers. There are relatively few people who are really doing much in the fundamental problems involved with foams. It's strange because probably ten percent of all polymers are being sold in the foam form. You gain a lot in modulus—your stiffness is enhanced so much for nothing more than a little know-how. But in a company like Dow, we do not do very well on processes that require significant mechanical fabrication. Some of the older ones that we've had, like for example making Saran film, making extruded styrofoam, lots of foam materials of that kind, and maybe even today making zip-lock bags. Overall we have not taken advantage of a lot of the fabrication technology that this company developed.

BOHNING: That's interesting.

RUBENS: I don't think they've had too many of those. They've always had good processes for the basic products. You could pretty much depend on that year after year as long as you stayed competitive and those products could be sold and that you were going to make a good return. Any time you'd come to making specialties you were always more vulnerable to somebody meeting that particular need with a lower cost or a better performance. I still think that's the sort of thing that people are going to run into. I look at the chemical industry today and I'm hearing the same thing from all companies—that they're going to concentrate on specialties. At the same time, I'm looking at companies like General Electric who cut their personnel by about one hundred thousand people in the last four years. Du Pont has cut twenty-five thousand out of one hundred and forty thousand. Dow is down from sixty-four thousand to fifty-two thousand. My perspective on all of those things is that if you're truly going to make specialties you have to have more customer service and you don't do it with less people, you do it with more. The same goes for Union Carbide, or Olin, or any of those companies.

My son is general manager of the Ramset division of Olin—it was Olin, and now it's Illinois Tool. Interestingly enough, they were doing, it seemed to me, a pretty good business, and the fact is that when he and I talked about it, both their sales and profitability were good. But Olin wanted things consistent with their long-term profit goals. Well, from what their overall profit was on sales, everything looked to me like they were doing a hell of a lot better with that division, but it seemed to me the decision was made to sell it while it's making a good profit. It was going to help their cash flow. I think profitable divisions are being sold because management at that particular time thinks it's not consistent with long-term goals. When I listen to something like that I always think they're short on money and they're looking to raise a few bucks. Particularly if it's been a winner! In research, you never get the clear view of what management is thinking, what their reasons are for a lot of their actions. They must know a lot more about why they make certain moves than I do. But it doesn't mean that you can't think about it.

But I'll tell you one thing about Dow. In no time since I moved down there did they ever reduce their research commitment. If you go back and look at all their annual reports and look at what their support for research is, it's a curve that just moves upwards. The question of whether the research results have been used wisely may be debated. But I don't think that the higher management in Dow has ever failed to honor their commitment to research and development. And we've gone through a lot of managements over the years.

BOHNING: Well, that's fascinating, and I appreciate those views. I have a list of names, most of them people we've mentioned in one form or another. I guess one person I did want to ask you about is Sylvia Stoesser. She was already in the physics department?

RUBENS: Sylvia was, to the best of my knowledge, the first woman chemist hired by Dow. I don't even think they had female johns when she was hired. In the very early days she worked on styrene, and to the best of my knowledge worked on inhibitors and the monomer purifications. I don't know how long she stayed on that, she was gone from the lab when I came.

We had the project on the styrene book and even though it was written almost entirely by Dow authors, there seemed to be a big problem in coordinating everyone—they seemed to be coming in at the same time with their final draft. This was a time also when polymer science was moving along very rapidly. I think before we produced the final publication I went back to the literature at least twice to fix things. But we couldn't get at the final publication and Boundy and Boyer brought in Sylvia. They had a woman librarian earlier but somehow she didn't bring it all together. But Sylvia, when she got on the scene, maybe because of her technical experience and the knowledge of the individuals involved, brought it to fruition and I think she played a very key role in getting that publication finished.

BOHNING: Did you have much interaction with Turner Alfrey?

RUBENS: Oh, yes. Turner and I had a lot of interaction, not only at the plant but also outside. We played golf together and played poker together. In the back of the green book I wrote a little bit about his golf and his card-playing. I had a great deal of respect for Turner. I considered him the best teacher of polymer science or any science that I'd ever encountered, and I had a great deal of faith in his capabilities. Many times I went over and discussed an idea and read the write-up with him. I used to get a big kick out of him because usually about two weeks later I'd either get one of Turner's interoffice memos, with just a note on it: "That's right." Turner was like that. If he didn't agree, it would read: "Come on over, we'll talk about it," or something like that. But we interacted that way over the years, and I found him to be, I guess like everybody else, a very fascinating, very competent scientist, with certainly a tremendous sense of humor, and a guy that was fun to be around.

BOHNING: I have a quote that's attributed to you (20), and I thought this might be a good place to close, if you care to comment on this. "Polymer science was in its infancy (going back to the early days at Dow). There was a wide-open opportunity, but our sophistication level was low. We had neither knowledge nor experience; we were starting from scratch."

RUBENS: Well, I felt that way, and industrially we were unsophisticated. Certainly there was experience out there, and there were some very good scientists who had made contributions, but as far as Dow was concerned, there was very, very little. There had been a commitment by Dow to go in the direction of natural polymers. That's why we had this cellulose lab. That's why we had so many plantations with popcorn and softwoods and everything around here to supply cellulose. Herbert Dow had believed that that was the direction that we were going in the manufacture of polymers. We had virtually no one in Dow at the time when I started that knew anything in depth about polymers. Wiley was already working on Saran and so forth, but there really wasn't very much knowledge about how to process these polymers. People knew very, very little, even about injection molding. I think a lot of the work on rheology had just started to develop. You go back and look at people like [R. S.] Spencer and [R. E.] Dillon, who had done such interesting work on the rheology of polymers (21). It's still kind of classic stuff today. So there was a tremendous opportunity there. If a person really wanted to work, he could stand on the shoulders of our predecessors and hopefully get some better perspectives of where we ought to be going, but a lot of it was pretty much uncharted.

BOHNING: You had commented to me earlier about Herman Mark that he always gave more than he took. I understand from others, that when he visited a company he was really lecturing and educating people.

RUBENS: Precisely. You know, I had such respect for his competence and the kind of an individual he was. I think people were actually anxious to talk to him about their specific developments and about their achievements. Generally, it was not too secret. In all the time that he came to Dow and talked with us, I've never known him to violate a confidence ever. The things that he was free to talk about, he talked about. But he could talk about them in such an interesting way that it sort of inspired you and made you enthusiastic to think about doing polymer science.

BOHNING: I certainly appreciate your time and sharing all of this with me. It’s really been quite fascinating. Thank you.

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

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