

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

RAYMOND F. BOYER

Transcript of an Interview
Conducted by

James J. Bohning

at

Michigan Molecular Institute
Midland, Michigan

on

14 January and 19 August 1986

With Subsequent Corrections and Additions

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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James J. Bohning

on 14 January and 19 August 1986

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RAYMOND F. BOYER

1910 Born in Denver, Colorado on 6 February

Education

Case Institute of Technology
1933 B.S., astronomy
1935 M.S., physics

Professional Experience

The Dow Chemical Company
Physics Laboratory (Physical Research Laboratory)
1935-1945 Physicist
1945 Group Leader
1945-1948 Assistant Director
1948-1952 Director
1949-1952 Secretary, Executive Research Committee*
1952-1969 Director of Plastics Research
1969-1972 Assistant Director, U.S. Area Research and
Development (Polymer Science)
1972-1975 Research Fellow

1975- Research Professor and Affiliate Scientist,
Michigan Molecular Institute

Honors

1955 Honorary D.Sc., Case Institute of Technology
1968 Gold Medal, Society of Plastics Engineers
1970 Borden Award in Organic Coatings and Plastics
Chemistry, American Chemical Society
1972 Swinburne Gold Medal, Plastics Institute of Great
Britain
1978 Member, National Academy of Engineering
1983 Best Papers Award, Midland Section, Sigma Xi
1991 Election to Plastics Hall of Fame, June 20, 1991,
sponsored by the Society of the Plastics
Industry, Washington, D.C.

* This group, consisting of Dr. William R. Veazey, chairman; Dr. Edgar C. Britton, vice chair; and R. F. Boyer, secretary; was responsible for Dow's R&D operations for a three-year period following the death of Willard H. Dow, president, Dow Chemical, in a plane crash in March 1949. This committee ceased to operate when Ray A. Boundy was appointed as the full-time research director for Dow.

ABSTRACT

Raymond Boyer begins the interview with a brief description of his family, childhood, and school days in Ohio, touching on his early interest in electricity. He then tells of his undergraduate and graduate years at Case Institute of Technology, focusing on the influence of the faculty there. In discussing his career at The Dow Chemical Company, Boyer provides accounts of discoveries and innovations, especially involving polystyrene; several leading figures there, including Willard and H. H. Dow; and major organizational changes that occurred during his career. Concluding with a summary of his most recent research at the Michigan Molecular Institute, Boyer includes an interesting anecdote involving a Canadian chemist with the same name.

INTERVIEWER

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

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INTERVIEWEE: Raymond F. Boyer
INTERVIEWER: James J. Bohning
LOCATION: Michigan Molecular Institute, Midland, Michigan
DATE: 14 January 1986

BOHNING: Dr. Boyer, you were born on February 6, 1910, in Denver, Colorado. Can you tell me something about your parents?

BOYER: Well, my father grew up on a farm in Mount Hope, Ohio. He wandered around a lot, finally ending up in Denver, Colorado, where he met my mother. She was born in Sioux City, Iowa and had gone to Denver as the metropolis of that area. So that's where they met. Several years after I was born, my father decided to move back to Ohio because his mother had died and his father was getting on in years and was all alone. They came back and first went to the village of Beach City, Ohio. They were there for several years, and then moved about twenty miles away to Millersburg, Ohio. That was the county seat of Holmes County, population around 2000 to 2500. There's where I had my schooling from grades one through nine.

BOHNING: Were there any particular teachers in Millersburg at that time that influenced you?

BOYER: I'd say the teachers by and large were excellent. They tended to be. Perhaps the eighth grade teacher left the most lasting impression, partly because she was a tyrant, but she was extremely good. She was Mrs. Carrie Marvin. My mother bought a set of books--The Book of Knowledge--which I read incessantly (1). They had a major influence on my career.

BOHNING: Did you go to high school in Millersburg?

BOYER: Only junior high, the ninth grade. My father was an entrepreneur. I think that is the best description for him. In Millersburg he operated a dry cleaning and laundry establishment, and for many years it was the only one in town. Then, competition moved in with some newer equipment. He decided that Millersburg did not need two such places and so he simply got out. For several years, he leased land for oil, using the old witch hazel stick to prospect for oil.

BOHNING: Was that in the Millersburg area?

BOYER: Yes. Well, it was all over Ohio. Ohio was a hot state then for oil potential and so that's what he did. That didn't last very long, and then he took a temporary position in Cleveland, Ohio, operating a confectionery stand with popcorn, peanuts, salted nuts, and so on. He didn't like being away from home, living in Millersburg, and working in Cleveland, so he decided to move to Canton, Ohio, which then had a population of about a quarter million. Again, he had a position then at a park concession, with popcorn, peanuts, and soft drinks. From there, he went into business for himself in a new market which was opening up in Canton. I worked after school in that market of his for about three years--after school, Saturdays, nights, and so on. That led to a tug of war between him and my mother. She wanted me to go to college. He wanted me to stay and inherit his business, so to speak. Well, she won. But I went to high school in Canton, Ohio. It had one senior high school for a town of a quarter million. It had about four thousand students. That was grades ten through twelve. Because I did very well in the first year, I got put in a class for outstanding students with special teachers--the best.

BOHNING: What influenced you to move into physics?

BOYER: Since I was about ten years old I had an interest in electricity. I always had a shop in my house. It was mostly full of junk, but important to me. I think, as far as I know, this was probably the Thomas Edison influence in Ohio. Also, there was a magazine by Hugo Gernsback called The Electrical Experimenter (2). Those were the influences that were probably critical to my career from about ten on. For some reason I never went into radio. I had friends who did. But no, it was Thomas Edison, that's all I can imagine. Now, I don't know if you're interested in the choice of Case?

BOHNING: Yes, absolutely. But before we get to Case, were there any teachers in your high school in Canton that you remember, in terms of the chemistry or the physics or the math that you may have taken, that influenced you?

BOYER: Yes. There was a Miss Heinreichs in chemistry. There was a Miss Lutz in Latin, and a Mr. Metzger in physics. There was a superb math teacher whose name I can't remember, but he was teaching advanced math then.

BOHNING: Did you have any leanings towards physics over chemistry?

BOYER: No. I'll get to that. I missed being valedictorian of the class by a bad grade in physics. [laughter] I just goofed on one exam. But anyway, I think the choice of Case is rather crucial because Case led to Dow. A high school friend of mine and I had looked around for colleges, and we traveled. I wanted electrical engineering--no question about it--and he wanted mechanical. So we made the rounds from Carnegie Tech to Ohio State to Purdue, Notre Dame, University of Chicago, and we couldn't make a decision. About a month before college was to start, in the fall of 1929, we were visiting at a friend's house, and the mother said, "Why don't you go to Case? There's a famous man there who works in musical sound and has written a book on the flute (3). His name is Dr. Miller." Well, we grabbed it; without ever having seen the campus which was only sixty miles away, we enrolled.

BOHNING: So you visited all the other schools! This Dr. Miller, was he in physics?

BOYER: He was Dayton C. Miller, who was in acoustics. The musical sound was a hobby. He was doing ether drift experiments, and was a wonderful lecturer. I was still committed to electrical engineering when I went to Case and I indicated that choice on my enrollment form. At a fraternity rushing situation, I met a man who was working in radio propagation at the Bureau of Standards. When I told him my interests, he said, "You don't want to be an electrical engineer, you want to be a physicist." And that was it.

BOHNING: Were there any specific faculty at Case that influenced you?

BOYER: Well, yes. Because in a placement exam I was in the top ten, it turned out that we got some good teachers that way. One of them was the professor of astronomy. His name was Jason J. Nassau. He taught freshman math. I think he was the first one to recognize any potential in me, and he remained a lifelong friend, advisor and mentor.

BOHNING: What about physics?

BOYER: My father died at the end of the freshman year, during exam week. Sometime after that, I had a chance encounter with Professor Nassau and told him that I didn't think I could go back to school because of a lack of funds. This was in the spring of 1930 and the Depression was on. He told me that there was an opening at the school observatory for a student custodian and he thought he could get that for me. Well, he did. So, I moved into that observatory. I had a shared free room, a kitchen with

cooking privileges, bath, and so on. That really was what allowed me to go to school. My roommate and I swept and scrubbed floors and cleaned toilets as the exchange for staying there.

I enrolled then in physics at the start of the sophomore year and stayed with it throughout. Nassau was a proselytizer and he persuaded me to do a thesis in astronomy. I had been familiar with all the equipment. I had assisted at public nights and I knew how to run the telescope. I knew the stars, and the whole business just by living there. He was certainly the dominant figure. He was nominally a member of the physics department. But the rest of the staff there, well, they weren't very good. They weren't outstanding. Miller was approaching retirement. The editor of the Handbook of Chemistry and Physics(4), [Charles D.] Hodgman, was on the staff. He let the students teach themselves. We'd get assigned a chapter in a book on optics and we gave the lectures. He sat back and did nothing. There were six of us enrolled in that physics course.

[Christian] Nusbaum was in x-ray crystallography. He was quite good, but he never amounted to anything in the big scene. There was a man named Clarence William Wallace who was simply drawing his pay. He taught, but indifferently. There was [John G.] Albright who was a good teacher, but his chief claim to fame was that he discovered that spider webs made very good cross hairs in microscopes. He had a business to supply spider webs. There were some good people. Of course, you took courses all over the campus in different disciplines. The math was generally very good. So there were a few good teachers around, enough that I had a decent education. Later, during my master's degree work, I had a course in physical metallurgy with A. A. Bates, who achieved national fame. [Robert S.] Shankland, a physicist, was very good but was away working with [Arthur H.] Compton on a Ph.D. degree while I was there.

BOHNING: How many chemistry courses did you take?

BOYER: Three: general; analytical laboratory; and physical chemistry.

BOHNING: Do you recall who you had?

BOYER: I don't remember analytical. General chemistry was taught by William Reed Veazey. Have you ever come across him?

BOHNING: Yes. I'm sure we're going to mention his name some more before we're finished, because he was already, at the time you were there, working at Dow in the summers and consulting for Dow.

BOYER: Yes, and then later he moved there permanently. Veazey was on Dow's Board of Directors and director of research for Dow starting prior to the death of Willard Dow in 1949. I worked for him at Dow later; I was an assistant to him for three years. The physical chemistry class was taught by Norbert Lange. He was the co-editor of the Handbook of Chemistry and Physics (4). I think that's right, Hodgman and Lange. He was excellent as a teacher.

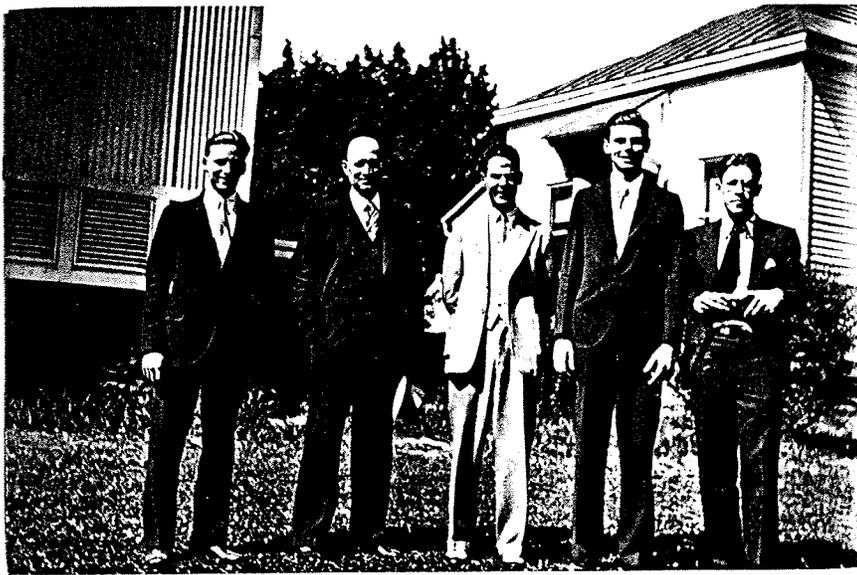
BOHNING: You took general chemistry and physical chemistry. No organic?

BOYER: No organic. I never had a course in organic chemistry. Somewhere along the line there was a lab in analytical chemistry, and that was the extent of it.

I first learned the terms "polymer" and "polymerization" at the observatory. A Case graduate, Frank Herzog, lived in Cleveland and worked in Akron (Goodrich). He was an amateur photographer and used the fully equipped observatory darkroom mostly on weekends. On one occasion, he brought a 2-3 liter bottle of vinyl acetate to this darkroom and left it there. It had an Eastman Kodak label. This liquid gradually thickened. I knew because I cleaned the darkroom. My curiosity made me tip the bottle and learn that this liquid gradually became thicker. Then I questioned Frank about my observations. The bottle originally contained a monomer, vinyl acetate, which he was testing in photographic chemistry. At room temperature it gradually polymerized and thickened the monomer. I can still "see" in "my mind's eye" that large bottle of vinyl acetate. The exact meaning of this finally became clear to me after several years at Dow Chemical. Herzog later was inventor (or co-inventor) of the tubeless tire.

BOHNING: You said you did a thesis. That was for the bachelor's degree in astronomy. Then you continued on for an M.S. degree?

BOYER: Well, when I tried to get a job in the spring of 1933, I went around to the industrial centers, and they wanted to know what my thesis was. When I said astronomy, that was the end. They made it very clear that they couldn't possibly want an astronomer. [laughter] I didn't really see how I could possibly go back, but then some teaching assistantships opened up in the fall of 1933. I think it was Nassau who got me one of them. Now I was in the physics department at Case, and I was a laboratory instructor. This was a two-year arrangement, fifty dollars a month, part of which went home to support my mother. That got to be pretty exciting. But I decided that I wasn't going to do any more astronomy even though Nassau wanted me to. So I signed up with Nusbaum, and the master's thesis was on the x-ray



crystallography of steel at high temperature in the region of the so-called alpha transition.

BOHNING: Was that done in conjunction with any of the steel companies in Cleveland?

BOYER: No. I don't know whether Nusbaum consulted with any of them or not. It's a good possibility, but I wasn't aware of that. And the joke of it was, we had a persistent problem in getting the photographs and Nusbaum was of no help. On the day I graduated, I realized what the problem was and I solved it. It was so damned simple that it was ironic and I started turning out decent x-ray spectrographs. The red hot iron specimen darkened the photographic film! That made the thesis.

BOHNING: What happened to your friend who went with you to Case, and were there any other students with you at the time that you recall?

BOYER: Well, my friend went through Case and had an indifferent four years in mechanical engineering, got an indifferent job somewhere in Cleveland and did nothing with his career. We roomed together freshman year and lunched together sophomore year on sandwiches I made at the observatory.

BOHNING: Were there any other student colleagues?

BOYER: The original five of us in the physics department were a rather remarkable group for the most part. There was Briggs Hazelhurst Napier--he was a descendant of the two men who invented logarithms, [John] Napier and [Henry] Briggs, and was quite a brain. He never made anything out of his life, did some teaching, and had a mental breakdown--nothing. There was a man named Robert A. Herrington, who ended up at Goodrich. He was very brilliant. He never did anything with his career at Goodrich. He was useful. For instance, he had memorized a four-place logarithm table. [laughter] But that was the kind of person he was. These guys were so sharp that they used to make me feel like an ignoramus. Another man was named Irving Prettyman. He ended up at Firestone, and he retired from there. I'd say he didn't distinguish himself at all. He just did good work on polymer and rubber physics.

Our original group of five was joined by a sixth student for the senior year--a William Nels Simons from New Orleans. We commonly designated him as the one who speaks Southern drawl with a Harvard accent (or perhaps vice versa). He came to class impeccably dressed--full business suit, clean shirt each day, and tie. I, for example, looked like a tramp. [see following page]

I last saw Bill on graduation day in 1933 and have neither heard from or about him since. It's almost as if a year in a good engineering college were a pre-requisite for inheriting a fortune.

BOHNING: Well, I guess that brings us to your finishing up at Case. We had mentioned Professor Veazey earlier. I understand that he was quite instrumental in getting people to Dow if he saw a student who was good. Did he influence your decision to go to Dow?

BOYER: Not at all.

BOHNING: Did you have other choices?

BOYER: I don't know. What happened was that the professor of astronomy, Nassau, went to Midland in the spring of 1935 to give a lecture on astronomy before the Midland Case Club. And, as you know, there were plenty of Case men there. He got the grand tour of Dow. Of course, he knew no chemistry. But he came back to Cleveland and one thing he really remembered is that they were using electrical currents of ten thousand amperes to generate chlorine. He saw these big cables and all the electrical machinery and everything else.

[END OF TAPE, SIDE 1]

BOYER: Dr. Nassau came back to Cleveland and just said, "Ray, you have got to work for Dow." [laughter] So I never did any job hunting after my M.S. degree.

BOHNING: I see. You had never talked to Professor Veazey about Dow?

BOYER: I didn't even know the scope of Dow. It meant nothing to me, except what Nassau told me.

BOHNING: Did you have anything in mind, as you were finishing up your master's degree, about what you thought you were going to do?

BOYER: Oh, I don't think I had faced that decision yet. And of course, the job market wasn't all that promising in the spring of 1935. I was aware of General Electric's Lighting Lab, Union

Carbide's Lab, both in Cleveland, and the "big three" tire companies. This came along really kind of early. As I recall, it could have been in March or April. It turned out that the head of the Physical Research Laboratory at Dow, John J. Grebe, was going to get an honorary doctorate degree from Case at the graduation ceremonies in May, 1935. Nassau arranged that I would meet him. He hired me on the spot, on the strength of Nassau's recommendation, because we scarcely had time to meet. It was just too busy a period. So I showed up at Dow and went to work on 5 July 1935.

BOHNING: And you started in the student training course?

BOYER: I went through the student training course.

BOHNING: Could you tell me something about your experiences in that course? As I understand it there was almost a full year in different sections of the company.

BOYER: Forty-nine weeks. Seven sessions of seven weeks each. We went around to the different shop departments of Dow-- mechanical, machine shop, the boiler shop, the lead shop, electrical shop, instrumentation. They even had us work in one of the warehouses where pipe fittings and stuff were handed out. I had one session of seven weeks in the Physical Research Laboratory. Of course, I was fascinated by all of this, because these were new techniques. We went out in the field with the field machinists and repair men and things like that. We got to see a lot of the plant. I knew the plant inside and out. I also spent a few weeks in the X-ray and Spectroscopy Lab under Don [J. Donald] Hanawalt, a physicist who became nationally famous.

BOHNING: Did you have a choice when you finished that student training program, or were you assigned?

BOYER: I went there with the understanding that I was going to be working for Grebe. I couldn't know at the time, but Grebe was in disfavor, for he had not followed proper proceedings in hiring me. He wasn't the personnel director who hired people, and so Grebe was in double or triple jeopardy. All of a sudden, I was told that I was going to work over in the X-ray Crystallography Department. Well, I didn't like that, and I finally went to the personnel director and said, "I am not very happy about that assignment. I came here with the understanding I would be working in the Physical Research Laboratory under Grebe." Well, okay, that was arranged. I was ideal for this X-ray Department because Dr. Hanawalt was a physicist who was in charge of the department. He was an x-ray crystallographer, and made quite a name for himself eventually with the techniques that applied

particularly to the magnesium development. By going into the Physics Laboratory, that's where I got exposed to polymers. Of course, everything followed from that.

BOHNING: You had an early paper on acid solvents for oil wells (5). Did you start your work in the PRL right on polymers?

BOYER: No. Dowell was a big thing. It had been developed in the Physics Laboratory by John Grebe, and they were giving the technical support for Dowell. The first summer I spent with a physicist who was applying theories to the flow of oil in underground porous rock to a central gathering place. It was the same as the theory of heat flow and diffusion problems and things like that. So he had me calculating Bessel functions, because they were used in interpreting oil flow. That was one of the most boring summers of my life. Then, one of the people in the laboratory, Mr. [Leonard C. "Jack"] Chamberlain, had a strong interest in the experimental aspects of the acidizing of underground rock, particularly limestone. Somehow he and I got together and did that work. That was the Chamberlain-Boyer publication, acidizing of oil wells. That's how that came about.

But the man who had put me on the Bessel functions became interested in polymers. He was a physicist. There were problems with light and heat stability of both polystyrene and Saran. His name was Dr. Lorne Matheson, a Canadian who got an undergraduate degree in Canada and a Ph.D. at the University of Michigan. He was a genius.

He had the idea that all we had to do was take known stabilizers that were used for rubber, food stuffs, preservatives generally, anti-oxidants, and add them to polystyrene, and we'd solve the weathering problem which polystyrene had. He further had the concept that if we put ultraviolet screening agents into this polymer and into Saran that that would protect these polymers from UV. We went to the drugstore to buy suntan ointments, but none of those things worked. So we had to branch out on our own.

The Organic Laboratory under Edgar C. Britton sent samples of every chemical it synthesized over to the Biochemical Laboratory. That Biochemical Laboratory, under D. D. Irish, was also located in the Physics Lab. All these chemicals came over from the Britton lab to the Biochem Lab and they were stored upstairs on the second floor of the Physical Research Laboratory. Well, that became our hunting ground. There were hundreds and hundreds of chemicals, many of them absolutely new, not even available from Eastman Kodak or anything like that. We began trying those out and we began striking pay dirt. There's where I learned organic chemistry. We were screening hundreds of organic compounds and I just somehow absorbed it, because I was interested in structure-performance relationships. I learned also which chemical groups were chromophores because of the

ultraviolet screening. So, that was my course in organic chemistry. [laughter]

Then it turned out that Britton was convinced that any compound that his group synthesized in the Organic Laboratory should, if it turned out to be useful and patentable, have either his name or one of his men's name on the patent. He began raising hell that we were stealing his compounds there, behind his back, without his permission, using them, and applying for patents on them. He tried to block the leakage. Well, we compromised and some of his people's names got on our patents. And all they had done was synthesize the compound, with no conception that it might be useful for anything.

BOHNING: Did Grebe and Britton get along pretty well? Did they have a cooperative relationship?

BOYER: Philosophically, they were north pole and south pole. Britton was the cut and dried, methodical, organic chemist, and damned good. Grebe was the creative guy who sparked and didn't want his name on patents. Britton ended up with three hundred and fifty patents and Grebe ended up with fifty patents. But, Grebe made for Dow a hundred times the profit over the years that Britton did. So yes, they were competitors.

BOHNING: I want to come back to that competition later on. There are some other aspects of that when we get into the styrene era. In 1936 there was an accident in the carbon disulfide plant.

BOYER: I don't know how you know about it!

BOHNING: There is a brief mention of it in the history of the Dow research pioneers (6). Dow was involved in making carbon disulfide at that time? It was 1936, I believe.

BOYER: There were two processes for carbon disulfide at Dow. One was the use of steel pots that were fired underneath by gas and oil. They might have a capacity of about one hundred gallons. Into the top of those pots, containing molten sulfur, workmen would dump charcoal and out would come carbon disulfide. They had a building that must have been a hundred feet long with maybe thirty of those pots in it. The stink was unbelievable. There were blue flames shooting out from leaks in those pots. At one point we had to go through that building when I was still in the training course. I always considered that an example of what hell would be like. The smell of hot, burning sulfur, and the flames, and the heat.

Dow had a newer process which used a big steel vessel electrically heated through three electrodes which were three phase, huge carbon rods. The charcoal went into that big vessel and being electrically conducting, all that got hot. Sulfur was run into that from one of these pots again, vaporizing the sulfur. Now this furnace was very inefficient electrically because of eddy currents in the steel shell of the big container. I've forgotten the power factor, maybe two-thirds, something like that. So the Physics Lab had a project of trying to improve the electrical efficiency of that carbon disulfide furnace. My boss Matheson had the idea that if we put stainless steel plates instead of steel around each of the three electrodes, it would improve the electrical efficiency. And that was done. But he still had the problem from the Physics Lab. There was Dr. Lorne Matheson, who was a physicist from Canada. It was in the student training course when I was in the Physics Lab, Matheson was working on making electrical measurements on that furnace to check out power factors, efficiencies, and so on. While we were in the building, the pot that vaporized the sulfur sprung a leak and put sulfur vapor out into the whole volume of that building and it caught fire and exploded. Well, I got burned and another fellow, a co-worker, also got burned. One of these chance-in-a-million things, because it wasn't our normal place of work and we weren't there very much. But it happened. Well, I got treated in the Dow first aid. It turned out the burns were not serious. But it did happen. I still have the scar from the burn.

BOHNING: How much longer did the older process continue?

BOYER: Oh, I don't remember. Eventually (because they were labor intensive) those pots were phased out and then eventually along the line, Dow got out of the carbon disulfide business. I never had any more to do with it after that summer of 1936.

BOHNING: You wrote a paper with [Robert D.] Heidenreich on an automatic heat distortion recorder for plastics that later became an ASTM method (7).

BOYER: Not Heidenreich but [George] Hierholzer. It had better be Hierholzer. Is it?

BOHNING: Yes. I made a mistake in looking at my notes.

BOYER: I know. Hierholzer. Heidenreich ran an electron microscope. Hierholzer worked for me as an instrument technician. He built that instrument.

BOHNING: What was the rationale behind that? What was the need for doing that?

BOYER: Well, heat distortion tests were one of the common characterization tools for thermoplastic polymers--where did they soften? There was an ASTM test, and it consisted of a strip of the polymer (it was mostly polystyrene at that time in the Physics Lab) resting between two supports, with a weight in the middle, and a micrometer to read the displacement of the top of the weight--the deformation. This was tedious, hand stuff that you assigned to technicians. And maybe they were paying attention and maybe they weren't. Now, one of Grebe's big contributions to Dow Chemical was automation. He, with [Ray H.] Boundy and [Robert W.] Cermak, devised the first automatic controllers that were ever used at Dow for process control. One of his strong philosophies was to automate. That was a characteristic theme in the Physics Lab. So, I thought, "Why not automate the heat distortion recorder?" All it took was an electrical sensing device. I've forgotten now, I couldn't tell you the exact principle, but that's what we did. We published it, and it was eventually adopted by ASTM because it was more accurate, foolproof and automatic.

BOHNING: Well, let me go to a different paper. Maybe I'll be correct with Heidenreich this time. [laughter] This was a technique for direct observation of the polychlorostyrene single molecules (8). This was the first time that this was done?

BOYER: Yes. To my knowledge, that was a first. Now things like tobacco mosaic virus had been seen before in the electron microscope, but they were multi-million molecular weight. And they were crystalline, of course. The concept of this was my own, because Heidenreich simply operated the electron microscope. He was exceedingly good at it, so good that he eventually got stolen away from Dow by Bell Labs. I had learned, by that time, enough theory about polymer solutions to realize that if you got down sufficiently dilute, you ought to have individual polymer molecules. I also knew that if one added a non-solvent to a solution of polystyrene, you'd precipitate the polymer out and you could collect it. So the key idea then was to get down to a dilution which would have individual polymer chains floating around in the system and then to add a non-solvent to try and capture them as individual molecules. We went to chlorostyrene to get more electron density to make them more visible in the microscope. So that was the basic idea. Well, that thing has been copied all over the world and Boyer is almost forgotten. Techniques, shadowing in particular, got refined tremendously. What's your background, by the way?

BOHNING: Physical chemistry.

BOYER: From where?

BOHNING: I got my degree at Northeastern in Boston.

BOYER: My son got a Ph.D. there in atomic physics, much, much later. Well, probably not that much later.

BOHNING: Going back to the early 1940s now, you and [Otis Ray] McIntire did some work in modifying polystyrene with natural rubber. Would you say something about that work?

BOYER: Yes. When radar got developed in England, electrical cables were needed which had low electrical loss and could be used to transmit the high-frequency signals through cable from the antenna dish to receivers and amplifiers. Natural rubber seemed to meet the electrical needs and had the flexibility. But these cables were frequently used in very warm places and were under flexure frequently, going around corners, and so on. The central cable that carried the signal would tend to displace toward the copper-braided outer shell which was an electrical shield and a return conductor. The idea started early on to modify rubber with polystyrene which was an even better electrical material to have because of low dielectric loss. This thing started, as I recall, in England. Dow was approached to help out in this venture, because we were then "Mister Polystyrene" already in the United States. That was the start of that venture.

[END OF TAPE, SIDE 2]

BOYER: Well, it happened that rubber and polystyrene are incompatible and you need other agents to induce compatibility since two-phase systems can be bad mechanically. The real payoff came with the invention by ICI scientists of polyethylene, because it was so good.

Being crystalline, it had a melting point above 100°C, so there is no problem with heat. It was electrically perfect, and that was it. That's what put polyethylene from ICI on its feet originally. You see, there were two consequences. Two people in the Britton lab discovered what we now call sequential polymerization of two monomers--styrene and butadiene. Either one first, followed by the other one. (That's now big, hot polymer chemistry--interpenetrating polymer networks.) They had a composition back then that had excellent electrical properties, low temperature flexibility, and was almost good enough on heat distortion because of the polystyrene that was in there. And it was pure polystyrene at a molecular level, intimately dispersed.

I worked on that product trying to peddle it to the military during the war.

BOHNING: I wanted to ask you about that. Was that Styraloy 22?

BOYER: Yes. That's it.

BOHNING: So you were taken out of the Physical Research Lab for a while?

BOYER: Yes. Half-time during the war, except when I traveled, and then it was more than half-time. But I had a half-day in the Physics Lab and a half-day in [William C.] Goggin's group.

BOHNING: Did you do much traveling?

BOYER: Oh, yes. I was always going to Washington, New York City, MIT in Cambridge, to see Professor [Arthur R.] von Hippel, the one dielectrics expert on the then secret radar project, wherever the commercial activity was on trying to make these cables. Somewhere, with Dr. Matheson, I had gotten exposure to dielectric testing and that became a responsibility of the Physical Research Laboratory. That was in the late 1930s. It was one of the centers for dielectric knowledge, and that was a proper polymer physics thing, of course. So I got drafted into that. But Styraloy got killed overnight by polyethylene.

BOHNING: What time would that have been? When the war was over?

BOYER: The mid-1940s, somewhere in there. But out of that work with rubber and polystyrene, Grebe brought back from Washington the first soluble styrene-butadiene rubber, and he gave it to me to copolymerize with styrene in little glass vials. And out of that came remarkably tough, high-impact polystyrene. Then McIntire took over on that, and out of that came the [J. Lawrence] Amos, McIntire, and [John L.] McCurdy patent on impact polystyrene (9).

BOHNING: You did the original co-polymerization.

BOYER: The very first one, which I did at Grebe's request; it was not patentable.

BOHNING: I want to move on to your early research administration, starting in 1948, when you followed Grebe as director of the PRL. You were assistant director for three years starting in 1945. How did this appointment come about?

BOYER: Well, as usual, there's a story. [laughter] I think you've got a good nose for that. By 1945 the Physics Lab, under Grebe, had three directors. Grebe himself, who couldn't care less about administration, Lorne Matheson, the physicist, who was a low-key, easy-going guy, and this man L. C. Chamberlain, of the oil well work. They eventually added a fourth man named [Lewis R.] Drake who was an organic chemist. Grebe--physics. Matheson--physics. Chamberlain--physical chemistry. They finally decided that they really needed an organic chemist. So here was this four-man team running the Physics Lab. As always happens, the place became a shambles, with different people going to whomever would give them the answer they wanted. The morale was very bad. It was so bad that it finally was brought to the attention of the president, Willard Dow. He actually came out there in the early summer of 1945, and interviewed every technical man in the Physical Research Laboratory. He interviewed them in the absence of supervision.

I don't know, and never will know, why I got chosen. I can only speculate. First of all, I had no gripes. I just told him, "Well, there are problems out here, but I'm getting along quite well. I'm very interested in what I'm doing." And every time he'd try to pry into a personnel problem I'd tell him about some exciting work I was doing. Then he asked me what I thought the real problem was with the Physics Laboratory. I said, "Well I think it's caused by the fact that you and John Grebe do not get along and Grebe thinks that you don't like him." Well, this shocked him. It was true. But, the next thing I know Grebe told me that Willard Dow wanted me to take over the Physics Lab. I said, "John, I don't want that job. It's not what I want to do. I'm enjoying my research." Well, he began putting the heat on. My duty and all that sort of thing. I talked to Willard Dow, and I talked to Veazey, and others. I finally decided, yes, this is my duty, and I took the job of assistant director.

Now Grebe failed to tell Matheson and Chamberlain and Drake about this change in status. There was one very traumatic session in Grebe's office with all of these other people. Grebe was trying to smooth everything over and not ruffle any feathers. Things were going on as usual. I was just added to the team. I finally said, "John," (Grebe that is), "this is not what Willard Dow wanted done. He told me that I was to be running this place under your overall supervision." Well, John got up and dashed out of the meeting. Matheson wasn't going to take my word for it. He went out (a mile across the Dow plant) and talked to Willard Dow, and Willard Dow confirmed it. Chamberlain got out. He didn't raise any questions. He went to work then with Ray Boundy, who was the first co-director of the laboratory, but currently the head of the new Plastics Department. That kept

Chamberlain occupied for the rest of his career at Dow. But anyway, we got that thing straightened out. Grebe wasn't interested in any administration of the Physics Lab. In fact, he was getting interested in atomic energy then. He took a year off in Washington, and spent time in Oak Ridge, and he was at the explosion of the first atomic bomb. So, by 1948, just by default, I was in charge. It's that crazy. [laughter]

BOHNING: Did you do any laboratory work in that 1945-48 period?

BOYER: Yes, I still kept my hand in that.

BOHNING: What about after 1948?

BOYER: I had a lot of contact with Willard Dow, because he was running research in those days. Not only was he president, but he was the nominal head of research. He had Veazey around somewhere by then too as an advisor. But any key decisions went through Willard Dow. So I had quite a bit of contact with him. In the spring of 1949, Willard Dow was killed in a plane crash. The board immediately put Veazey in charge of research and made Mr. [Leland I.] Doan the president. I don't know who proposed it, but there was to be a triumvirate consisting of Veazey, Britton on the organic side, and Boyer on the physics, physical, polymer side. That was called the Executive Research Committee.

BOHNING: Perhaps this might be a time to talk about Willard Dow a little bit since you had a chance to interact with him on a number of occasions. Are there any impressions that you have about how he operated as the nominal research director of the company?

BOYER: He clearly didn't have time to get into many details. He operated in terms of people. If he had faith in the individuals, that was good enough for him. He was certainly always available. His door was always open. There was never any serious problem getting an appointment with him. I liked him as an individual. I thought he had a rather pleasant personality, a nice sense of humor, and he was certainly sharp. He was quite intelligent. Obviously he had a terrible time following in his father's footsteps. I don't think that he tried to be his father. He was his own self. That's what pops into mind right now.

I still occasionally think of examples of his sense of humor. He'd get you in the office, or maybe a group in the office, and he'd ask this chemical conundrum. If benzene is called benzol, and toluene is called toluol, what's arsene called? [laughter] And I remember, then he'd tell me the names of Dow people who weren't amused by that story, or maybe didn't

even get it. He had these little things like that. I think that was one of his ways of kind of judging people, and it made me like him.

You asked about the human side of Willard Dow. There was some social gathering out at the country club one time. He was the after-dinner speaker, and he told a story about the lady who had a favorite pet dog which gave birth to three pups. She named them Teeny, Weeny, and Rachmaninoff. Well Teeny was the teeniest and Weeny was the weeniest and Rachmaninoff was the pianist. [laughter] It was something for the president of a company, to unbend that way and get a crowd with him. That story was a tremendous success.

BOHNING: Did he have the loyalty of his employees through that kind of attitude?

BOYER: Oh, I think so, yes. But there was this conflict with Grebe.

BOHNING: Did that ever resolve itself?

BOYER: No. Well, you see, Grebe was hired by H. H. Dow. H. H. Dow set Grebe up in a laboratory, and he was H. H. Dow's man. When H. H. Dow passed away here's Grebe working with a less superior, less creative, and less inventive man. I don't think that Grebe ever personally made the transference to Willard Dow, and he let it show. I think it was that simple. Willard Dow had the job of writing an article about salt. This was an incident I remember quite vividly. It was to be a semi-technical article--Dow's activity in the field of salt and the importance of salt to Dow Chemical. So he wrote an article and submitted the draft of it to many others, but certainly to John Grebe. John Grebe proceeded to tear that draft apart. Well, enough said. It seemed to me that I saw the draft from Willard Dow. I'm quite sure that he had asked me to look at it. And it was an acceptable draft for its purpose. I'm sure he didn't write it himself. He had people who wrote it--that's almost a forgone conclusion. Have you met Ned Brandt yet?

BOHNING: Tomorrow.

BOYER: Well, the Ned Brandts of that day, they're PR people, but smart. Most of them were chemists of some kind or another. But Grebe proceeded to find one fault after another with it. Grebe told me so. Now I think that Grebe realized that he paid for this. I think the fact that Mr. [Stephen L.] Starks, the personnel director, didn't want me working for Grebe already in 1935 was a clue.

BOHNING: When you became director in 1948, what was Grebe's position with the company?

BOYER: Boundy let him start up a new laboratory. He built it from scratch. It was called the Nuclear and Basic Research Laboratory. Grebe was in charge of it, and a man named Alden Hanson was the assistant director. Alden Hanson was a very creative researcher. He'd also married the daughter of Mr. [Earl W.] Bennett, which didn't hurt his career any. But, he was brighter than hell on his own. They had this huge, new building where Grebe's office was, and that's where he stayed until he retired. While it was called Nuclear Research and Development Laboratory, it had a lot of polymer stuff in it and general chemistry and so on. Who knows why Grebe gave up the Physics Lab and turned it over to me. It could have been a very generous gesture on his part. It could have been his being fed up with the place. I don't know. He had a genuine interest in nuclear atomic energy, that's for sure. Anyway, those are the facts.

BOHNING: You've been described as having a liberal attitude as a laboratory research director, primarily when you helped produce a continuous process for manufacturing superior impact polystyrene during the post-war retrenchment. How would you characterize your managerial style at the time you were directing PRL?

BOYER: I didn't know that I had this moniker of "liberal."

BOHNING: Apparently it at least fit in that one instance.

BOYER: I think that I really got that from Grebe. Grebe was an unusual research director. He was loaded with ideas. He could keep a thousand men or more going. He could get any number of people going, because his mind just turned out ideas like crazy. But, he also listened to others and he challenged others to come up with an idea that was better than his. He just used to say, "Well, either you top me or you do what I say." But he would generally never insist unless he got absolutely convinced. I suspect he got this from H. H. Dow, but I don't know. But I certainly had that attitude, and liberal is a word for it. I discovered early on, after I became director, that if one of the troops came to me with an idea and I didn't think much of it, and I fought that idea, that guy would dig in his heels and do it--undercover or however. Whereas if I said, "Well, why don't you try it?", that's the end of that idea because the guy gets thinking about it and, well.... So that was an on-the-job learning situation.

By and large, we had a bunch of pretty competent people in that lab. It came out at the trial on impact polystyrene that I had told McIntire I thought going ahead with that experiment, which later led to rubber-modified polystyrene and the famous Amos patent, was a dangerous experiment because it had the potential of blowing up on them. The next thing I knew, they had done the experiment and it worked. That was what I thought had happened. But, of course, it came out at the trial that I had actually jolted them to the point where they took a lot of safety precautions. These precautions did prove valuable because the runaway polymerization that I had predicted did start, but they were able to control it.

There is another example of one of the things I know I did. McCurdy came to me one time with an idea for a continuous process for making styrofoam. Well, until then it was a very inefficient batch process. I thought what he was talking about made sense, and he wanted to know, "Where do we go from here?"

[END OF TAPE, SIDE 3]

BOYER: In many laboratories the boss would want to carry that ball. It takes endless time going through all the machinery. McCurdy was quite surprised. I said, "Look, you're capable of handling all this paperwork and getting the approvals that are needed, from management and so on. It's in your hands." Well, he went ahead and did it. He did it better than I could have done it. I didn't like details like that. I plain didn't care for them. Well, if this is liberal management, then so be it. [laughter]

An even more outré example is as follows. I had an early interest in a concept of creativity advanced by the French theoretician, Henri Poincaré, as follows: The subconscious mind, working all the time with personal knowledge stored in one's brain, comes up with new combinations of fact at random. Suitable ones surface in the conscious mind which has the ability to recognize a logical new concept. Voila! A creative idea is born.

During my period of leadership in PRL I obtained from the Midland Police Department a confiscated slot machine. Instead of the usual apples, pears and peaches, I wrote on the first wheel the names of monomers; on the second wheel the same or different group of monomers which could be used as comonomers; the third wheel contained additives such as plasticizers, fillers, colors, stabilizers, etc.

The game was to spin the wheels and note if any interesting, random, three-component composition seemed novel and exciting.

Well, I demonstrated my slot machine before a PRL group of chemists, chemical engineers and a few physicists. Their acceptance of my illustration of the creative mind concept was greeted with mild politeness and soon forgotten. I may have insulted their respective intelligences. Turner Alfrey was to advocate this same creative process in the late 1950s and received ovations.

BOHNING: Maybe we should talk a little bit about that Executive Research Committee, which I think existed from 1949 to 1952. How do you think that committee functioned?

BOYER: Like any triumvirate. There was a pecking order which I discovered early on. Veazey was number one, Britton was number two, and I was at the bottom of the totem pole. If I had a management type idea which I thought we ought to consider, and went directly to Veazey with it, he would ask Britton about it, and Britton would veto it. Well, all right, that happened a couple of times and I got smart. So I'd go to Britton with it. Well, he'd say either yes or no. But if it was yes, he'd take it to Veazey and the thing flew.

About that time Dow had started the use of industrial psychologists. They actually hired teams of industrial psychologists as consultants. These people wouldn't take employment. They wanted the independence of being consultants. I was so damned frustrated with that Executive Research Committee that I spent a fair amount of time talking to these people and I think they saved my sanity. It was a situation that was designed to drive a younger person off his rocker. That's the simple fact. It was very ineffective and management finally caught up with it, and just abolished it.

BOHNING: Did you meet on a regular basis or was it just individual meetings?

BOYER: For half a day I'd come from the Physics Lab to Veazey's suite of offices, where I had a desk. Britton would do the same. We'd be available and then we'd meet as occasion required it. We'd pass judgment on patent applications from the Patent Department. We'd meet with patent attorneys. God, that was one of the dreariest things of that whole three-year period, because our committee was asked to make judgments which should be made on the basis of facts. There was a lot of wheel spinning. Veazey was not a manager in the sense of cracking down where necessary.

BOHNING: Before we move to your later administrative experiences, perhaps we could go back and look a little more at some of the research that you did. Since you left Dow in 1975 you've published a large number of papers. While you did publish

during your Dow years, it was probably at a lower frequency. Could you describe your feelings about the contrast in dissemination of information in an industrial setting versus academic work?

BOYER: In 1952, Ben Branch, who was then head of the Plastics Department, asked me to join his staff as the director of plastics research and development. This was a very painful decision because it meant cutting all ties with the Physics Lab where I could do research and did do research and kept different things moving as long as I was over there. But again, I was told it was my duty. So I went to work for Branch in a purely administrative job of following all the R&D activities in the polymer field at Dow. It was a challenging, exciting job, and I was learning all the time. I once told Branch I thought I really ought to have a research man working for me. He said, "No way," and that was it. But, I used to go to research conferences, like the Gordon Research Conference on polymers, ACS meetings, and American Physical Society meetings. Of course, I got into all that new stuff about Ziegler-Natta chemistry, because Dow eventually licensed those processes. I got to meet [Giulo] Natta and [Karl] Ziegler and others, so that it was not a stagnant atmosphere. It was just that I couldn't do much publishing. But a Gordon Conference around 1960 got me going on a field of interest that was so challenging that I later wrote a long paper on that topic. All my writing had to be done in my spare time--at home, weekends, vacations, whatever. Three and a half years later out comes this hundred page paper which I think I could say opened up a new field in polymer science. That was done while I was still at Dow. This is publication number 51 on my list (10) and was to determine my career at MMI [Michigan Molecular Institute].

BOHNING: Which area was that?

BOYER: Well, it was the dependence of transition temperatures on chemical structure in polymers. It was a landmark paper. It got cited by Current Contents as a "Citation Classic," one of the most cited papers in its field (11). It was translated into Chinese, and it was imitated.

BOHNING: Is this where your work with [Robert] Simha began?

BOYER: Simha's work goes back quite a bit earlier than that. He was a Dow consultant. I hired him for help on practical problems that had a basis in fundamentals that I wanted to be aware of. But returning to that particular paper, if you publish one paper that's a hundred pages long and pretty significant, it counts as one paper. If an organic chemist makes fifty compounds that are homologues of something, and they are each a page or two pages

long, that's fifty papers. (I just made this up by the way. I hadn't thought of it that way before.) But those were lean years. Practically everything that I've done at MMI was an outgrowth of a basic idea in that paper. I just defined a new field that nobody else wanted to listen to. In fact, they have spent years telling me I was crazy. I guess they're finally changing their tune a bit. I had an open field to run it. The experts either said it was wrong (this included [Paul] Flory), or wouldn't touch it. So, if you look at the publications here, there has been some variety to the topics, but the majority of them stemmed out of the 1963 paper. It had one key idea in it: the existence of a transition above T_g which I called a liquid₁ to liquid₂ transition.

BOHNING: You were still at Dow another ten years after you published that paper. Did you follow along those concepts?

BOYER: Yes, I really did, but in a low key. I published a couple of papers in 1966 at an ACS meeting. I published a paper in 1970 that was a follow-up and there could have been one or two little ones in between (12). But when I came out here and had a free hand, that's where I decided to concentrate.

BOHNING: We have been talking about your move as director of plastic research. We should go back and look at a few of your other research areas. I have two that I'm interested in. One takes us all the way back to 1946. You had a paper with [Robert S.] Spencer and [Ralph M.] Wiley on a density gradient tube which also became an ASTM method (13). What was the impetus for that particular work?

BOYER: Well, the Physics Lab generally was the seat of excellence for both polystyrene and Saran resins. The commercial Saran co-polymers were predominantly vinylidene chloride, with a minor proportion up to fifteen percent or so of vinyl chloride. Those two monomers didn't go together very well. The net result was that one got a drift in co-polymer composition throughout the reaction. The co-polymers species that were made had a range in densities. A colleague of mine was interested in that problem and he was lamenting one day that there was no convenient way of easily measuring such things as a range in density of particulate polymers. His name, R. C. Reinhardt, is named in patents. Vinylidene chloride always came out of solution because of the crystallinities of its own monomer. The stuff came out as dry powder, and the polymer made late in the reaction, having a different composition, would come out as discrete particles. They weren't blended with the initial stuff. Just by grinding up the product, one had these particles with a distribution of density.

I used to read Nature a lot. I read it religiously because it was a source of lots of new ideas. And as this paper would say, I'm sure (I haven't read it in years), I noticed a paper by [K.] Linderstrom-Lang from one of the Scandinavian countries, on a density gradient tube used in biological experiments (14). I immediately realized that this would be an answer to the problem that my cohort was talking about. We set up a density gradient tube, and sure enough, it worked. Those pictures in the publication (13) illustrated how well it worked. We could demonstrate differences between co-polymers that were made to have a narrow distribution of composition and those with a wide distribution. It was a simple thing to set up. It was a simple experiment and it gave results. I think it wouldn't have amounted to much if polyethylene hadn't come along. There were all the different densities of polyethylene and stuff like that. I think that's really what put it on the map as an ASTM method. It's just that simple. It was going to another discipline, totally different and picking up a technique.

BOHNING: I also wanted to ask you about styrene. You've written extensively about styrene polymers in the ACS monograph, the Encyclopedia of Polymer Science and Technology, and Ray Seymour's History of Polymer Science (15). Dow was the first company to produce styrene commercially in the 1930s. What was the status of the project when you arrived at Dow?

BOYER: Styrene monomer was already being made on a pilot plant scale and done by the Physical Research Laboratory. As I related in that Seymour article, the impetus was encouragement from both Du Pont and the Bakelite Incorporation. They wanted a source of styrene monomer. Dow went ahead and (I think that was 1937 or 1938) put up this half-million pound a year commercial production plant. I was certainly around when that was built. Then Bakelite was acquired by Carbide which had its own monomer process. Du Pont did a market survey that said that styrene would never get anywhere. All of a sudden our market dropped to nothing. I found, in some of those historical searches, an order from Du Pont for five thousand pounds of monomers. This was in the late 1930s. They were deadly serious about it in research and got clobbered by the market research people. That's the status of it. I think the facts in that Seymour story are certainly the way I remember them.

BOHNING: As a physicist, how were you first involved with styrene?

BOYER: Through the light and heat stability of polystyrene, started by my immediate supervisor, Dr. Matheson. Polystyrene exposed to sunlight would develop color and haze. I know we set up a haze meter that was developed at the Bureau of Standards and some very primitive photometer that would measure the

transmission of polystyrene after exposure to sunlight at three different wave lengths. I think that was it--red, green, and blue. Of course the blue was getting absorbed most rapidly by the yellow color which developed. Those were the simple tools of interest to a physicist.

Dielectric properties were another thing. That was at one point believed to be the big market for polystyrene radio sockets and the electronic industry generally. We were making dielectric measurements in the lab there. These were good legitimate physics problems. Later on, it turned out that the styrene monomer columns would plug up periodically with cross-linked gels. By that time I was aware of the Flory-Reiner theory of gelation. We had experiments going, and I published a paper with Spencer (16). But I was in the middle then of trying to solve what caused the gelation in the styrene distillation columns. Now that was an internal report that never got published. But it occupied me for a couple of years. It made a fascinating problem.

BOHNING: I think I read that when those columns plugged, they literally had to be shut down and the contents chopped out.

BOYER: That's for sure. I wrote an internal report for Dow on styrene column plugging. Another thing that we discovered was that the discoloration of polystyrene in sunlight was directly proportional to sulfur content in the polystyrene and that sulfur was following through from the distillation because it was the first very effective inhibitor to be used in stills. As a result of that, we got styrene production personnel to switch over to other non-sulfur inhibitors. That was a case of going from physical measurements of discoloration, going back through infrared and elemental analysis, tracing the sources of the discoloration, and then back to the stills and practices of distillation.

BOHNING: Where did you stand in what you termed the ethyl cellulose-Saran-polystyrene controversy?

BOYER: Well, I was totally on the outside but partial to the Saran-polystyrene advocates.

BOHNING: Was this before you were involved as director of the laboratory?

BOYER: Yes, before and after. I remember we'd have contact with the chemists in the cellulose group so that on a personal level, they were always lording it over us poor polystyrene guys and the Saran guys. I think I said somewhere that we used equipment that

they had in the Cellulose Lab, extrusion equipment and stuff like that, which we could not afford. The cellulose project was a pet of Willard Dow. It was a kind of a friendly rivalry.

BOHNING: We had talked earlier about some rivalry between the PRL and the ORL. At one point, PRL was following steam cracking of ethylbenzene, while ORL was looking at side-chain chlorination.

BOYER: That's right.

BOHNING: Was this typical, to have this kind of competition?

BOYER: Yes. I'm told it was encouraged by H. H. Dow. He felt that it's one thing to develop something inside of Dow, but Dow is not the world. Once you get into the outside world with a new process, then you've got to be sure of competition. So he actually encouraged this thing. That was one of the things which I encouraged as director of plastics research, competing processes for making latex.

For instance, one was run by research and the other was run by production. Production people solved production problems and the research people came along with new twists. Once the money people took over at Dow, that was one of the first things they killed--that duplication of effort. I guess that's the reason I mentioned it. When Dow Chemical Company broke up into these businesses, and you had a business manager who was responsible for a narrow line of products--boy, he used to sob to me about, "Why this duplication?" Those were expensive pilot plants. Half a million dollars a year. As a profit-minded manager one could understand it. But, Dow killed the goose that laid the golden egg. I think there's no question about that.

BOHNING: Some of your written comments about middle and upper management are not always the most favorable. What changes do you think would make that relationship between management and R&D more productive?

BOYER: I don't know. The fellows tell me now it's been getting worse. It's worse than it's ever been. At one point, management said, "Let's set aside a block of money for new venture stuff. That money doesn't get judged by these business-minded people with current products." Well, the first thing you knew, they were after management, saying, "Look, if you've got that money, why don't you give it to us to spend?" That's what happened. Now, I think it's a universal problem.

My understanding of management's side, circa 1968-1970, is cited. Attorney James G. Williams, a member of Dow's Board of Directors and also manager of the Plastics Department after Goggin, would reply, when I was ranting at him about management's neglect of research, "Ray, please keep in mind that the profitability on large volume products is much lower today because of competition (circa 1968) than it was when profits from caustic soda supported styrene and polystyrene until styrene itself became profitable" (not exact quote). I had to agree.

Of course, I personally recalled that in my early days at Dow there existed in isolated spots, mostly production "laboratories," activity by individuals which resulted in patents, new products, better processes. It was not necessarily recognized as research until the bean counting began.

Moreover, even large chemical companies cannot escape the views of prominent financial companies who can and do recommend the sale or purchase of each company's stock, common or otherwise. An esteemed financial analyst recently (Spring 1991) commended Dow Chemical for its partnership with Merrill Drug (item in Midland Daily News). Whether for that reason or not, the price of Dow common promptly went up with the market.

Financial analysts tend to be critical of the unpredictable cyclic nature of chemical and plastics products. They prefer the relatively dependable and expanding sale of drugs, consumer products and such. No current management group can ignore this factor which already existed in the earlier years. For example, President Willard Dow was told by Wall Street, circa 1948, that Dow Chemical was spending too much on research. One of my first unpleasant duties as director of the Physical Research Lab was to discharge summarily, with notice, ten percent of my staff. At that time the most vulnerable group was the recently employed hourly workers who were governed by the union priority system. Dow did discharge some salaried college graduates who were not performing to required standards.

A second such incident occurred, circa 1958, when Dow management, with an eye on profits, ordered a ten percent across the board budget reduction in all departments, including research.

On that occasion Turner Alfrey, as manager of the Polymer Research Laboratory, offered to resign for two reasons:

- A. He could readily find employment elsewhere as a professor or researcher,
- B. His salary as director more than covered a ten percent budget cut.

I spent an anxious three hours on Saturday of Memorial Day weekend trying to persuade Turner that such an action would hurt his staff and that the ten percent reduction could be readily

achieved in other specified ways. Alfrey reluctantly agreed--"a man persuaded against his will," resulting in such subsequent cooperative projects for Dow as multi-layer film, rotational injection molding, Zetabon and many others, plus numerous publications, many of them joint with Turner.

Incidentally, C. B. Branch, as manager of the Plastics Department, approved (as my superior) of my action on this group which reported directly to me.

[END OF TAPE, SIDE 4]

BOYER: I stew about that problem. I tell myself, after all I'm a stockholder, that I ought to go to management and just tell them what, in my opinion, is going on. I made such a pitch to middle management in 1963. That's a story that's never been published. That's a dilly. I spelled all this out. In the beginning there was one cash register at Dow. That was up in the main office and it was run by H. H. Dow and E. W. Bennett. Caustic supported styrene and polystyrene, and Saran and Ethocel, no question about it. As long as the total profits were good it was fine. But when they broke up into ten to twenty cash registers and each one had to make a profit, that was the end. I don't think you can cure modern management of that habit. Dow is coming up with new products all the time in the research lab. But when they take it to management they do a market survey right away. There's no market for that. It's not Dow, it's Du Pont, it's ICI, it's everybody. So, I think it's hopeless. Please note that I retired from Dow in 1975 and I do not know subsequent events. But I still hear gripes.

BOHNING: I read somewhere recently that the old Dow style was to create a new molecule and then to go out into the marketplace to find a potential use, but that the new Dow does not have the development of products take place in a vacuum, but listens to what the customers want and then comes back to research. Does that make any sense?

BOYER: In principle yes, with many examples, more recently less so. Most anything that's said by many members of Dow middle management for publication, may not reflect events at the research level. Now I've seen it happen. Research people don't and shouldn't get to talk to journalists. And the guys in top management are isolated from middle management about the facts of life. I feel pretty strongly about it, and the record shows that no new products are coming out. It's much easier to buy a successful business and add it as an increment to your operation. You don't make many mistakes that way. I was responsible for Dow losing, I'd say, a million dollars on radiation chemistry as the way of the future. I was a hundred percent wrong. But in those days, Dow permitted people to make mistakes, and people did make

mistakes. I wasn't the only one who lost such money. That's the spirit that's gone.

Well, I've told the styrene monomer story in that Seymour article, but look at something tremendously successful at Dow like latex for paints and paper coatings, rug backing and all. Dow really pioneered that. The research people came up with this sequential polymerization of styrene and butadiene. It looked like a good cable insulator. Dow got money to build a plant during the wartime. By the time the plant was built and running, polyethylene came along. That plant was idle, and some chemists said, "Let's make some latex products in it. Not sequential polymerization, but co-polymerization with styrene and butadiene." And they did that. Someone discovered these copolymers in the composition range (not the styrene-butadiene rubber range) up around forty percent styrene or more, were film forming and gave tough, strong films. And that was the styrene-butadiene latex paint. It started right here in Midland as a non-planned sequence. No customer told anybody what to do there. But once the chemists, the creative chemists, realized that here was an interesting phenomenon, then Dow went to Glidden and the two of them together put latex paint on the market. Any management guy who tells me that that was doing what the customer wanted--well, that gets you small things. Recall the styrene monomer story: it was started in the Physics Lab by one man, [Robert R.] Dreisbach, and backed by Grebe, even later by President Willard Dow who published his views in the styrene story.

I know damn well that GE came to us at one point when their Noryl was new, and they wanted a special impact polystyrene to toughen Noryl. Dow management said to them, "We can't be bothered. That's small potatoes," and turned them down. Another company in New England, Foster Grant, accepted the challenge and it turned out to be a hundred million pound a year business. So I lived through these things. I think maybe 3M operates a little bit more along that style because they're tailoring products to specific needs, but that's not what made Dow big.

BOHNING: But if you tailor products to what the customer wants, you're really making more of a modification of something that exists rather than moving into something that's totally new.

BOYER: Well, sure. In this case of GE, few could have anticipated that Noryl was to be an engineering polymer of preference, that and polycarbonate. So, there went a hundred million dollars of something we could do as well as, or maybe better than, someone else. A middle management person says, "We're going after the big customers. We don't want any of this kid's stuff." Of course, they antagonized General Electric. Well, it's a sad story. As I said in my anecdotal history, styrene wasn't planned, it happened--then management let it happen, until synthetic SBR elastomer put it on top.

BOHNING: You made the comment, I think in the Seymour article, that there were numerous irreversible changes at Dow between 1935 and 1963. Do you care to comment on that, in keeping with what we've just been talking about?

BOYER: Well, I think the two changes that had the biggest effect are the single cash register up until about 1965, and finding that internal competition was too expensive. Going from a single cash register to multiple cash registers or so-called profit centers, that was a serious blow to research. And then there was the lack of planned internal competition, sponsored internal competition, again on the grounds of economy, the profit centers and so on. I guess I'm starting to heat up here a bit. I'd better watch myself. [laughter]

BOHNING: Around 1969 there was another reorganization, and you became director of U.S. Area Research and Development in Polymer Science.

BOYER: That was window dressing. I was being kicked upstairs. I'll tell anybody who listens. It's a high-sounding title that is totally meaningless. The young turks were taking over and they didn't like my style of management of research. And I didn't care to adopt the new style. The new style was knowing every fact there is to know about any given project, having it on instant tap so you can tell your manager about it. So, this is management. You know everything. And management says, "Gee this guy is good. He's right on top of everything." That, I'm afraid in my experience, is not where success lies. But there was this large group of young fellows who could and did do that. That's not my style. You know, Oscar Wilde once defined a critic as a guy who knows the price of everything and the value of nothing. [laughter] I don't know if you've ever heard that one.

BOHNING: No, but it's very appropriate.

BOYER: I hadn't thought of that particular analogy before, but it's true. And the fellow who appointed me to this high-sounding position was Julius Johnson. I don't know if you've run into that name. He succeeded Boundy as vice president for R&D. He was a guy that didn't know what research was all about. But he had conducted the market development on Tordon, and other chemicals like that that got used in Vietnam, and Dow was minting money from them like crazy.

BOHNING: What was his background?

BOYER: He was a biochemist. He studied under [William C.] Rose at Illinois and was a damned competent man, but he had never been on the firing line in research. Shortly after he got me out there to work with him and gave me this high-sounding title, he got booted out as director of research, when the Texans took over. They realized he didn't know A from whatever about actual research, and he was through. He got put on the shelf, too.

BOHNING: In 1972, you became the first Dow Research Fellow. What did that entail?

BOYER: Well, it was a nice honor without obligations. And judging from people who have subsequently been appointed to the same thing, it was really a select group at Dow, and I was certainly honored by it. It was a newly created way of doing something for outstanding people whose career had been in research. I'm pleased about it.

BOHNING: Did that give you more opportunity to continue some research work?

BOYER: It didn't mean anything. It was just a title.

BOHNING: You came to MMI in 1975. Judging from your list of publications, that's where a lot of your research interests started to bear fruit.

BOYER: Yes. I think it's sort of like striking a mother lode in a gold mine. (This refers to T₁₁.) Here was this simple, basic idea that the experts said was wrong (Flory in particular, but he had lots of company). When you have a whole field to yourself anymore, it's a little unusual. I still have my fingers crossed how right I was about it and so on, but I'm gaining more confidence as time goes on. Of course, I will want out once the "real" polymer experts take over. I don't say that in any derogatory fashion. I don't know if you've ever heard the anecdote about a college professor who was looking for post-docs or even graduate students. One of his colleagues at another school said, "I recommend so-and-so because, when he says something about a project, that's the last word on the project." And the professor answered back, "I'm looking for somebody who can say the first word." [laughter] I'm sure you've heard this concept. I think I would have to say that my whole career has been largely in saying the first word on something. Then the experts take over, and that's fine. That's the way it should be. They say the last word. I think this is an apparent success here at MMI, and I hope it's real with all these publications and so on. I've just had a field day being able to say the first word.

I made it easy.

BOHNING: On several occasions, you were invited to lecture at the Soviet and Polish Academies of Sciences. How did these invitations originate?

BOYER: Well, my wife and I would go to the IUPAC meetings for the polymer group and we got to meet the international community quite well. We very, very gradually struck up a feeling of mutual trust with some of the leading Soviet scientists in the polymer field. The initiative for visits came from them. I went to an IUPAC meeting in Moscow in 1960, but this later phase started certainly while I was still at Dow and could afford to go to all these meetings in the late 1960s and early 1970s.

BOHNING: Is there any one person that was instrumental in getting you these invitations?

BOYER: Well, I think it was Nicolai Platé at Moscow University. He was one of the several heirs to [V.] Kargin in the Soviet Union, but he was perhaps more personable and outgoing and gregarious than some of the others. He had the language ability, too. I think that one has to recognize that those people were over here looking for invitations, and I'm sure that I was a conduit to a lot of that. I never lost sight of that because the CIA was after me after any trip abroad. I'd have a succession of visitors and I'd get pumped. So one had to assume it was going on on both sides. I think my wife and I got accepted by them. She's a very personable young lady and friendly and talkative and so on. I would say that she did the work. They didn't want me, they wanted to get Peggy over. But they have to take me along with her. (Well, you know that's a joke.) I once sent a Dow plane to a Gordon Conference in New Hampshire to pick up Platé and bring him to Midland, and the Dow pilots flew him over Niagara Falls. Hell's bells! The Russians couldn't do enough for us after that. We got trips to Tbilisi, Samarkand, Leningrad and all over for free.

BOHNING: You were also in Poland once.

BOYER: Yes. Well, the man there, [Marion] Kryzjewski, had been coming to the States for a long time. He had done a postdoc at Brooklyn Poly and that's where I first got to know him. He invited me through the Polish Academy of Science.

BOHNING: I have a number of names I'd like to mention and see if you'd care to comment. We've already talked about a few of them. But, there are two names that aren't on my list that you did

mention earlier. You said you'd met both Ziegler and Natta. What were the circumstances?

BOYER: Well, in each case, Dow was considering patent licensing, and did get a Ziegler license directly from Professor Ziegler since he handled all of his own business arrangements. So, a Dow group of us went to Mülheim and met Ziegler and his whole staff. In Natta's case, one had to deal with Montecatini, but if you went there to Milan, you were invited to Natta's laboratory. So those were the circumstances. Natta, of course, came to this country and lectured at Brooklyn Poly. I heard him there, but had no further personal contact.

BOHNING: The other names that I have are those that you were associated with at Dow. Let me just mention some of them. Maybe you'll have others to add. We talked for the most part, about J. J. Grebe and William Veazey, unless you have anything else you wanted to add about either of them. What about Ray Boundy?

BOYER: Well, of course, I had a long association with Boundy, because he was assistant director of the Physics Laboratory when I joined it. Then he was put in this styrene program, toward the end of World War II, and that took him out of the Physics Lab. From there, he went to being director of research in 1952. For any of my research activities, I had to somehow work with him. That was a long and very pleasant association. Of course, we were co-editors of the Styrene monograph (15a). He supplied policy decisions and I did the technical side.

BOHNING: William Goggin?

BOYER: I think he joined the Physical Research Laboratory around 1937 with an M.S. in electrical engineering from the University of Michigan. He went off into management in the war period to technical service in the business side of the plastics effort. He later then became head of Plastics Technical Service and still later (1964) he succeeded Branch as head of the Plastics Department. I worked for Goggin for I've forgotten how many years, until he went over to be president of Dow Corning. I had close to a fifty-year association with him. He's on our board of directors here at MMI so I still see him. (He died of cancer about 1988.)

BOHNING: Sylvia Stoesser. She was the first woman in Dow research. I noticed you acknowledged her in that article for Seymour's book too.

BOYER: Yes. Her name is on the Styrene monograph because she helped out a lot on that. She's a very competent chemist. She's still alive. I saw her about a month ago.

BOHNING: That's something else I need to ask you about and that is people that you think we might talk to. We can do that a little later, but you might keep that in the back of your mind. Bobby Dreisbach. I guess a lot has been said about him.

BOYER: A lot has been said and it's all true! [laughter] I inherited him when I took over the Physics Lab. He was a real nut. But he introduced styrene monomer and the steam cracking method of producing it.

[END OF TAPE, SIDE 5]

BOYER: Who's next on your list?

BOHNING: Ben Branch.

BOYER: He was a man of outstanding ability who, in my opinion, certainly deserved to go right up. He was executive manager of Dow Chemical, executive general manager and he was president of Dow International and then he was president of Dow, chairman of the board and so on. He had one of these remarkable memories that remembered everything you ever told him. Well, nearly everything, if it was important. He had pretty much instant recall and tremendous association of different things, people, events and so on, which made him quite an effective manager. I have nothing but the highest regard for him. I worked for him for seven or more years. It was a wonderful association. He was extremely competent. He had consideration for his people. He is now retired for some years (1981). He maintained homes in Midland; Houston, Texas; and Marbella, southern Spain for many years.

BOHNING: We've talked about E. C. Britton already. I don't know if there's anything you wanted to add.

BOYER: No, I don't feel qualified too much to judge him because of my lack of background in organic chemistry.

BOHNING: Larry Amos.

BOYER: Amos was a very good chemical engineer. He was a person who knew how to do things. He could solve the maze problems at Dow and did solve the maze problems, including getting along with top management and getting his ideas into commercial development. So, he was very capable. I think he wanted to be director of the Physical Research Laboratory, and I think he felt very frustrated when upstarts like Boyer came along and took over. I don't consider myself a manager, but I'd say that he was even less so, except when it came to picking people at the bench to do jobs and get jobs done. Things really happened with him.

BOHNING: Robert Spencer.

BOYER: Spencer was my full-time technical assistant. We certainly published enough together. He always carried his share of the load on that, and then branched off on his own and turned out some very nice work at Dow. Somewhere along the line he was done with research. I never knew why, but he got off then into computers and into management jobs, and I'd say ended up far below his research capabilities and perhaps even his intellectual capabilities. He died about two months ago. He was only in his sixties. (This was late 1986.)

BOHNING: The last person I have on my list is Turner Alfrey.

BOYER: Well, have you seen the green book (17)?

BOHNING: Yes.

BOYER: I couldn't top that. [laughter] I wrote a lot of that, and believe it all. I think he's the only person in research that I really miss, in the sense of going back and saying, "Gee, I wish Turner was around. I'd take this problem to him." He could help anybody and everybody who ever came to him with a problem. I once hit him with a problem during lunch hour with others. He thought about it and soon gave me the answer.

Here is a more extreme example: I wanted to construct for lecture purposes at MMI a polystyrene tuning fork for demonstrating one fundamental difference between polystyrene and steel. I recalled that the key factor was modulus/density which is about the same for each material. But I could not recall the exact equation. I bothered Turner, who didn't remember, but he eventually derived the exact equation from principles of mechanics. I then recognized his solution as the one I once knew. The demonstrated difference presented to audiences was simple. A steel tuning fork (say middle C) would vibrate for a minute or more. A polystyrene tuning fork designed for middle C vibrated for a few seconds because of internal energy absorption.

I also had tuning forks from other polymers and materials such as polycarbonate, glass, etc. Each had a characteristic pattern. I demonstrated that a polystyrene fork at dry ice temperature vibrated several times longer. I hinted (with tongue in cheek) that PS tuning forks might be sold to Eskimos. Then I introduced the hard science of dynamic mechanical analysis. The tuning forks were long remembered.

BOHNING: My last page of notes says "summing up." Do you have any general comments about what you have seen happen in your career, changes you've seen, what the future might hold for anyone starting out today?

BOYER: Not right off hand. I would have needed lots of warning on a thing like that. I'm really so deep in research now that I think it. I wake up with it in the morning. I'm in the midst of all these problems, and I don't read anymore, I don't ponder things that would be responsive to your question except in a kind of a fleeting sense.

There's no question that I joined Dow at a kind of golden age in Dow where I could develop according to my own personality and drives and interests, with a minimum of interference. I shudder to think what would happen to me now if I were joining Dow as Ray Boyer at the grassroots level. It's not idle speculation, because I get to meet some of these younger people and I don't think I ever was or am now or ever would be the kind of person who's in demand with modern management. I'm sure that Dow will survive and get along and that people will have successful careers, but they're not going to have science careers unless they get out of Dow and other large companies because modern management doesn't want that. They can only see the dollar sign. Research people from Dow don't come out to symposia at MMI unless they can see a dollar sign on the subject matter. I have been told that. A non-Dow research manager has commented recently and privately to a Dow research manager about the low visibility of Dow researchers at key meetings (Gordon Conferences, ACS, IUPAC, etc.) as well as the poor showing in publications (added in 1991). Gone are the days, I am told by many outsiders, of the Turner Alfreys, etc., etc.

BOHNING: Is there anything else that we haven't touched on that you'd like to comment about? If not, I'd like to thank you very much for an extremely fascinating morning and for sharing your insights and your observations and your experiences.

[END OF TAPE, SIDE 6]

INTERVIEWEE: Raymond F. Boyer
INTERVIEWER: James J. Bohning
LOCATION: Michigan Molecular Institute, Midland, Michigan
DATE: 19 August 1986

BOHNING: Dr. Boyer, you have mentioned that there was another Ray Boyer for which there's a very fascinating story. Can you tell me about that?

BOYER: I first heard about a Canadian chemist by the name of Raymond Boyer when there was a newspaper article published with photograph saying that he had been giving Canadian wartime secrets to the Soviets [see following page]. Some of the details are given in that particular newspaper account, which appeared in 1946. Even my aunt in L.A. saw the picture in the L.A. press and was convinced that "my Raymond" was in deep trouble. I never saw any more about it and had pretty much forgotten the entire incident until I went to Europe in the spring of 1952. It was my first trip abroad and I sailed on the Dutch ship, the New Amsterdam. We had a wonderful time and I thoroughly enjoyed all the people I ran into. I then had to leave the boat at Southampton, while the boat went on to Holland. We transferred to a tender after going through customs on the New Amsterdam. Immigration seemed just a formality because people were going through the line as fast as they could show their passports and get them stamped. Of course, this was British Immigration Service.

But when I came up, I could see that there was some delay, some hesitation about me, even some consultation among several of them, and I was asked to step out of line. I was thoroughly puzzled by this at the time. After we got on the tender, I was asked to sit up in the very front deck seats (they were benches, as a matter of fact) with a man who said he was from Scotland Yard. Without explaining why, he began interrogating me about my background. The main thrust of it seemed to be connected with my record as far as going into Canada was concerned. Of course, I had been to Canada many times because The Dow Chemical Company had a production plant in Sarnia and offices in Toronto and I knew a number of the Dow Canada people and some Dow Canada scientists. I had even gone to Canada one time to give a lecture on glass transitions in copolymers. So, I faithfully related these different experiences without hesitation and at first without any clue of where this was all leading.

But then, sometime toward the end of the interview, and apparently from things that were said, or questions asked, I began to get the connection with the other Raymond Boyer. It was a little bit upsetting. In fact, quite a bit upsetting, to have

Wanda Wood is 'Stage' from 22:
and 'Secret' from 22:
**Dr. Boyer Admits
Betraying Secret**

MONTREAL, March 26.—(UP)
—Dr. Raymond Boyer, assistant professor of chemistry at McGill University, acknowledged in court today that he had given one of Canada's top war secrets to Fred Rose, Communist member of Parliament, for transmission to Soviet Russia.

On the basis of this and a mass of other evidence taken during a four-day preliminary hearing, Judge Rene Theberge ordered Rose to appear on March 28 for "a voluntary statement," a technical procedure.

Boyer, who also is charged with violation of the Official Secrets Act, was ordered to appear for his "voluntary statement" on April 2 after he had waived an individual preliminary hearing. Both men are free on bail.

a thing like this happen on my very first trip to Europe. I proceeded to London by train and registered at the Dorchester Hotel. I was so upset that as soon as I could get established in the hotel room I went down the street and wanted to walk across the street to visit the Dow Corning offices, which were just twenty or thirty feet away from the hotel, on a side street. I knew some of the English representatives of Dow Corning, and I knew they had an office right there. I needed a shoulder to lean on, because I was absolutely upset. I did talk to one of the people there who had visited Midland a number of times. Well, he tended to make rather light of the thing and to assure me that nothing was really going to happen to me, and that I should stop worrying.

When I went back to the hotel, there was a very suspicious-looking man there, who seemed to be taking quite an interest in me. That further caused me some discomfort. But almost overnight I pretty much forgot it, and really stopped worrying about it. Later on, on that same trip, I went on to France to attend a scientific meeting and give a lecture there, and eventually stayed in a hotel in Paris. There I was interviewed by some official who began asking me questions about my background. It was rather clear that it had to be connected with the Canadian Raymond Boyer incident. But, nothing really happened and I had an enjoyable first visit.

I then went back to England by boat-train. In the first-class compartment, seated opposite me, were two American ladies. I got into a conversation with them. In fact, I knew the husband of one of the ladies. As it turned out, I knew his name through science and we had a very enjoyable conversation all the way to Calais. Then we were on the boat for the crossing. When we got back on the train at Dover, I thought I had the same seat again, but the ladies were missing. I never saw them again, but sitting opposite me was a rather distinguished-looking gentleman who seemed to take more than a usual interest in me. We never exchanged any words all the way to London and I never saw him again. But I think that there was no question there was some kind of surveillance. I always imagined that they would hope that I would try to get away, or that I would do something to throw them off the trail. Of course I didn't, and I went about my normal business. I had all sorts of contacts there and the whole thing then blew away.

A few years later, I was in Zürich and I was visited one day in my hotel room by a man who was some kind of official of the police force. He too began questioning me. Of course, these were all well-mannered sessions with no attempted intimidation or anything like that. But this man had a trick that I think was a dead giveaway. We'd be talking along in English, and he would speak quite understandable English. Then, all of a sudden, he'd drop some French phrase into the conversation. Of course, the Canadian Raymond Boyer from Montreal was presumably quite capable of speaking French. So, I realized that this was an attempt to

trick me. Since I never spoke French in my life I couldn't fall for the thing, and eventually the whole episode passed.

I went to Russia in 1960 for the IUPAC meeting there, the first one in the Soviet Union. To the best of my knowledge, there were no such interviews or anything like that in the Soviet Union. In the Soviet Union, I naturally supposed after all, they knew where he was, and they knew that I was not the other Raymond Boyer. The whole thing seemed to die out until we went to the IUPAC meeting in Budapest in 1969. My wife and I were sitting in a restaurant one night. We were in a booth with two other strangers across from us, young ladies as I recall. Sometime during the meal, a well-dressed man came and sat down at our booth, forcing himself in on the two ladies across from us, and making things rather crowded. His excuse was that they were about through eating. The ladies did leave and he stayed on behind and talked. While we conversed he spoke excellent English, but he was Hungarian. It all seemed so natural that I didn't think anything of it at the time. But one night when I got back to the hotel there was some kind of reception for the newly arrived guests and here was this man. I think his name was Herr Schneider. He hadn't said anything about being any part of the meeting, but it just seemed that he was somehow keeping track of me. I ran into him several times during the meeting that week. He kept popping up at places where we were, but then the whole thing ended like all the others had.

In 1972, I received the Swinburne Award from the Plastics Institute of Great Britain and went over there to receive the award and present the lecture. Incidentally, the lecture was held in the famous room at the Royal Society where the Friday lectures had been given. It was a social occasion as much as anything because a lot of wives were there. I had anticipated this and had actually prepared a rather semi-scientific lecture. After the lecture, there was a banquet. It turned out that at the conclusion of the banquet I was expected to make some kind of general remarks. So on the spur of the moment, without too much preparation, I naturally thanked the committee for my award, and my wonderful accommodations in London at the Brown Hotel and the big suite we had, and everything else. Then I said that my reception this time was certainly far different than it was on my first trip to England. I recounted this experience with Scotland Yard. Well, Cecil Bawn was sitting at the head table and as I was telling this story, he was nodding his head. It turned out that he knew the Canadian Raymond Boyer, because Cecil was in explosives work during the war. In that connection he got to meet this professor from Montreal who was also in explosives. He said something to me across the table while I was still on my feet, and I said, "Well, Cecil has just confirmed my story about the existence of the other Raymond Boyer." [laughter] Cecil believed that this man had in fact betrayed these secrets as the newspaper had stated. So that was the end of the story.

BOHNING: Do you know if this other Raymond Boyer stayed in Canada?

BOYER: Yes. A visiting professor, Dr. P. L. Kumler, SUNY, Fredonia, New York campus found on the current journal shelves at MMI a copy of a Journal of Chemical Education review article dealing with wartime research on RDX, an explosive (18). [See the following page which shows a composite from the framed arrangement which now resides in Boyer's office.] In it were an artist's sketch of Raymond Boyer and a brief biography. He did go "to prison for some years." "He now lives in retirement."

This makes it clear that the Raymond Boyer of this interview was needlessly harassed in several countries except the Soviet Union.

BOHNING: Is there anything else that we should talk about? I had indicated that I wanted to talk to you about Paul Flory and you've got most of that documented in your other files. Would you recount for me again the story of the European meeting when you were giving the paper and he was chairing the session?

BOYER: Yes. I had been invited to the University of Manchester for a several-day symposium in the spring of 1970, as an invited speaker expected to give a one-hour lecture on a subject of my choice. The subject matter that I talked about that day was an outgrowth of my review article on multiple transitions in polymers (10). That had made quite an impact in England. On this particular morning that I spoke there were two other speakers. The first was Professor [G.] Rehage from Clausthal in West Germany, and speaker number two was Dr. John Hoffman, who was then at the National Bureau of Standards in Washington. I was the final speaker of the morning, and of course I was then at Dow Chemical Company. Rehage gave a lecture on the thermodynamics of the glass transition, a topic which he had published on before and talked about before. It was quite an elegant lecture. It was the sort of thing that Flory liked to do and the kind of thing that Flory himself would tend to do when tackling such a problem with rigorous thermodynamic considerations. Hoffman talked about some of his dielectric studies, particularly the alpha transition, which is a pre-melting kind of transition for which he and [J. I.] Lauritzen were working out the theoretical concepts. The main part of my lecture was concerned with the multiple transition behavior of atactic polystyrene, which I had been studying in great detail for about five years. That pretty well took up most of my allotted time.

Then I said I wanted to discuss a subject very briefly, one whose implications I didn't fully understand, but it concerned a rule of thumb for which I had made a certain reputation, namely to the effect that the glass transition temperature for many polymers seemed to occur at about two-thirds of the melting temperature, when both temperatures were expressed in Kelvin. I

WHICH RAYMOND BOYER DO YOU KNOW?

The Political Aftermath

A Soviet technical mission to the United States in August 1943 made enquiries about RDX, and a year later (8 August 1944) two Soviet experts (B. Formin and P. Solodon) visited the Shawinigan plant in Canada. It is not clear how much they learned on this occasion.

In September 1945 Igor Gouzenko, a cipher clerk in the Soviet embassy in Ottawa, defected with a mass of documents which indicated the existence of a Soviet spy network in Canada. A number of Canadians and Britons were arrested, and several were brought to trial on the charge of releasing secret information. One of these was Raymond Boyer, professor at McGill University (see Fig. 3), who along with J. H. Ross had supervised several graduate students (including the author of this article, Ph.D. 1942) in research on RDX. Boyer was identified by Gouzenko as "The Professor," a code name for one of the agents in the network organized by Col. Zabotin of the Soviet embassy.

At Boyer's trial it came out that the Soviet technical mission of August 1944 had contacted Fred Rose, a Member of Parliament belonging to the Labour-Progressive Party (the successor to the Communist Party of Canada, which was banned early in the war). Rose in turn had contacted Boyer in an attempt to learn details of the Shawinigan RDX process. Boyer gave Rose some details, the most important being that acetic anhydride was used. Boyer did not deny these allegations but claimed that the technical details were trivial or common knowledge.

The first trial ended in a split jury. In the second trial George Wright testified on behalf of Boyer. However, the latter was found guilty and went to prison for some years. After coming out he became a criminologist, worked as a Research Associate in the Institute of Forensic Psychiatry of McGill University, and wrote one book about his prison experiences and another on crime and punishment in Canada under the French regime, before the British conquest. He now lives in retirement.

We may note that after May 1945 the Russians probably knew everything of practical importance about German processes for making RDX through their occupation of eastern Germany, and hence any information transmitted to them in August 1944 had only fleeting importance.



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Text and drawing courtesy of the Journal of Chemical Education



Figure 3. Raymond Boyer.

pointed out that after I had published several papers documenting this finding, I learned that Charles Bunn, who was then in the research laboratory at ICI, had predicted such a relationship some years earlier, in fact around 1952 (19).

[END OF TAPE, SIDE 1]

BOYER: I had never been aware of his book since it had to deal with textile fibers, which was then of course a subject of great interest to ICI. I then proceeded to present the evidence which I thought supported this concept of Bunn. What Bunn said in 1952 was that there is long-range order in the crystalline state and that the melting phenomenon signifies the breakup of that long-range order. But there is short-range order at the glass transition and that short-range order breaks up in the amorphous state, but since the order in the glass is of much shorter range than in the crystal, the glass temperature should be less than the melting point. So he had simply predicted an experimental fact which was discovered independently by myself and also by [Ralph G.] Beaman at Du Pont. I presented evidence for various polystyrene derivatives which seemed to support the Bunn hypothesis. This whole thing was a minor part of my lecture and lasted at most ten minutes. It was designed to do that.

Flory was in the chair, although he had nothing to do with the selection of the speakers. After my lecture it was the chairman's duty to make some comments on the lecture and try to get a general discussion started. There had been discussion after each of the three talks. He had very kind words to say about the brilliant lecture given by Rehage. He was somewhat ambivalent about Hoffman's lecture but it was pretty clear to me that Hoffman was not getting the royal treatment that Rehage had received. Of course, I had long been aware of the feuding between [Leo] Mandelkern and John Hoffman from the period when Mandelkern worked at the Bureau of Standards. So I just ascribed these comments to Flory trying to protect one of his own graduate students. Then Flory began discussing my lecture. I can't remember a single word he said but it was quite obvious that it was a pretty scathing rebuke for something or everything that I had said. I was thrown into a status of semi-shock by this unexpected outburst. I remember Professor [Leslie] Treloar coming down front afterwards and saying to me that Flory didn't understand what I was talking about, otherwise he wouldn't have made such comments. Well, I was pretty miserable about the whole thing because I thought I had organized a very good lecture and it had this rather unhappy ending. I still remember I sat with Treloar for lunch choking down my food because I certainly had no appetite for eating.

After I got back to Midland I began trying to sort out why this attack was made on me and it became pretty clear that I had violated one of the commandments in Flory's "Principles of Polymer Chemistry"--that amorphous polymers are free of any

order. So then I was curious about the evidence behind Flory's conclusions and I did a lot of reading of the literature. I discovered that there was a rather widespread prevalence of feeling among many polymer scientists, without regard to Flory's book (20), that certain phenomena, particularly in connection with rubber elasticity, could best be explained by the assumption of some order taking place in the elastomer. Geoffrey Gee, who was then head of the chemistry department at Manchester, was one of them. He was certainly a respected scientist, a fellow of the Royal Society, and an authority on rubber elasticity. But he was just one of many people.

So I began doing a lot of reading on rubber elasticity and things like the Mooney-Rivlin constants. I came to the conclusion that it didn't seem right to simply dismiss the subject as Flory had done, based on really a theoretical concept. I talked to various people. I remember talking to a Goodyear chemist who was here in Midland for a symposium. He had published something that seemed to be indicative of order in amorphous polymers and I questioned him about it. He said that he never started a problem without assuming that he was going to run into some order in the polymer. For him to explain his experimental results, he had to assume and postulate the existence of order. This whole thing then began building up in my mind that maybe Flory was not dealing with the facts of life. He had a theory but hadn't really tried to prove it yet.

I came across the proceedings of the Welch Conference on Polymers, which was held in Houston, Texas in 1966, and published in 1967 (21). [P.] Corradini, who was Dr. Natta's x-ray specialist, gave a lecture there on x-ray crystallography. He also included some studies he had made on amorphous poly(alpha)olefins, which had been prepared in Natta's laboratory. In this series was amorphous polyethylene, polypropylene, polybutene, and I think polypentene. He showed that except for polyethylene, there were two amorphous scattering halos on all of the x-ray patterns. If one interpreted those angles in terms of Bragg's Law, one of the distances was constant and small whereas the other distance increased with increasing size or length of the sidegroup. That's indicating an intermolecular spacing, which depended on structure, so that that spacing was bigger for the butene polymer than it was for polypropylene.

Well, after Corradini's talk, Flory got up and disagreed rather violently with Corradini about his interpretation of the x-ray data, claiming that there was no such thing as structure or order in amorphous polymers. This discussion is all a matter of record, published in the proceedings of the Welch Foundation for that particular symposium, including discussion remarks (21). The discussion was long and lively, with the people like Bruno Zimm, [Maurice L.] Huggins, and others getting into it and trying to somehow rationalize these conflicting opinions between Flory and Corradini, suggesting the possibility of some intermediate situation--perhaps less than the perfect order in the crystal,

more than the total lack of order predicted by Flory. But Flory would have none of that. He said there are two extremes--there's the crystalline state and the amorphous state. There the matter seemed to rest. But here again was an opinion by Corradini who was considered an x-ray expert in the field of crystallinity (a co-worker of Natta) talking about order in amorphous polymers.

I became increasingly convinced that Flory was not stating the real-life situation. He was giving a hypothetical argument. At the IUPAC meeting in Helsinki in 1973, Flory gave a plenary lecture and his topic had to do with the question of order in amorphous polymers. He summarized several lines of evidence having to do mostly with behavior of elastomers--polyisobutylene, rubber, and polydimethylsiloxane, and these were thermodynamic lines of experimentation. Then he brought up the subject of neutron scattering, which was then very new. It had to do with a series of experiments performed in Germany on blends of deuterated polystyrene and regular polystyrene as a function of molecular weight. The neutron scattering data showed very clearly that the radius of gyration obtained from the neutron scattering data was proportional to the square root of the molecular weight, which was expected of the random coil model and was what Flory had predicted for unperturbed dimensions of an atactic polymer. That seemed to settle the matter in the minds of very many people.

Professor Gregory Yeh from the University of Michigan had evidence from electron microscopy for what he considered structured regions in atactic polystyrene. Somewhere about this time, Professor [V. A.] Kabanov, from Moscow University, was in Midland and I questioned him at great length about the position of the Soviet school because they had taught for a long, long time that there was indeed order in amorphous polymers. They showed all sorts of experimental data, including photomicrographs indicating ordered structures that had been found in polymer solutions. Kabanov was a protege of [Valentine A.] Kargin, who was the leader of the Soviet polymer school. He had lived with Kargin's thinking and experimental work and concepts from the beginning and had in fact just published a lengthy review article outlining the current views of the Soviet school on this question of order (22).

Yeh, who had just come back from a trip to Europe, had reason to doubt the findings of the neutron scattering results and had particularly run into the feeling in Germany, and even in France, that neutron scattering could not tell anything about order smaller than about 25 angstroms. Since the objects which Yeh had seen in the electron microscope were about this size or smaller, he was convinced that the neutron scattering couldn't and didn't disprove Yeh's findings. To me this whole thing added up to a pretty convincing, but not really thoroughly proved thing yet, that there was some local order in atactic amorphous polymers.

Then in 1975, there was a symposium sponsored by the ACS in Atlantic City on the general topic of structure in amorphous polymers. It had been organized by Geoff Allen from England and Elaine Petrie from Eastman Kodak. The speakers are a matter of record. Of course, Flory was the opening speaker and some of [Erhard W.] Fischer's colleagues from Mainz, Germany, were speakers and various Americans--Dick Stein, and others. In general they seemed to be agreeing with Flory. Professor Yeh was back in Germany and could not attend the meeting and the only one in favor of any order or structure was Professor [Phillip H.] Geil, who was then at Case. He didn't hesitate at all. He had been Yeh's major professor for his Ph.D. thesis, and they shared rather common views about structure in amorphous polymers. So here was Geil all alone, defending the existence of structure--and he was literally alone. He didn't hesitate to argue with Flory. At one point Flory cited a statistical calculation that indicated 5×10^{21} configurations possible in a given amount of polymer. Geil's reply was, "I'm not going to count them." [laughter] But it was a bit acrimonious. It was a one-sided story.

I happen to know, by having read the program, that not on this program, but lecturing in a nearby auditorium, was Don Patterson from McGill University. He had been for some years collecting evidence for structure in normal alkanes, based on heat of mixing. He had really, I thought, persuasive evidence, and since normal alkanes are prototypes of polyethylene, it seemed hard for me to avoid the conclusion that there had to be some order in the amorphous regions of polyethylene. After the all-day symposium was over I was walking on the boardwalk with Petrie and Geoff Allen, and I said that I was really disappointed in the one-sidedness of that symposium. They apologized and said they had tried to do something about it but couldn't get people like Yeh, and apparently others, and that they too were not happy with the biased outcome. So I said I would be willing to write a rebuttal to the symposium, and they accepted that. I spent that summer up at our cottage working hectically on preparing the manuscript. I should say writing a manuscript because all my homework had been done. I had been collecting reprints ever since 1970. I had a whole file drawer full of them.

[END OF TAPE, SIDE 2]

BOYER: Charles Bunn's paper was simply the first in a long sequence. So I put this story together and it eventually got published (23). Petrie and Allen stated in their general remarks that they didn't agree with everything I had said but simply agreed with the principle of having a dissenting opinion. So I had of course thereby cut myself from any possible reconciliation with the Flory school and had undoubtedly damaged myself professionally. There was plenty of evidence that Flory does not brook disagreement. It's shown by the chain-folding battle and other events.

Flory is one of four Nobel Laureates in the polymer field; Staudinger, Ziegler, Natta and then Flory in 1974.

Experts in the several respective fields to be mentioned openly questioned his opinions on:

- A. Chain Folding: He was proven wrong by scientists like Andrew Keller in England, John D. Hoffman in the U.S.A. and others.
- B. Rubber Elasticity: Experts tell me that he savaged a pioneering paper in this field by Guth and James. Later, he was third in line with a theoretical paper to justify the existence of the Mooney-Rivlin Constants. A theoretician in this area told me that Flory's estimated value for one of the constants--the critical one, C_2 , was low by a factor of 3-5. Flory seemed at first to resent these Mooney-Rivlin empirical constants because C_2 was not predicted by his theory of rubber elasticity.

In brief, Flory was a mortal and fallible, deserved Nobel Award notwithstanding.

This has been a kind of a major area of interest with me--that of whether or not there is order in amorphous polymers. My studies on the liquid-liquid transition, which was first announced in 1963, only began in intensity, in depth, after I joined MMI in 1975. The T_{11} studies eventually seemed to be indicative of order in amorphous polymers. Two of the chief lines of evidence for this were the theoretical arguments of [S. Ya.] Frenkel in the Soviet Union and of some rather surprising and unexpected work being done by Eric Baer at Case. Baer and his students found that if you take ordinary, low molecular weight polystyrene below the entanglement molecular weight and dissolve it in any of a variety of solvents and cool them down, at some point the solutions will gel. They will undergo thermally reversible gelation. In other words, any given atactic solution will gel on being cooled and will ungel at the identical temperature on being preheated. This can be recycled back and forth as often as one chooses. The question is, "What is the gelation mechanism?" You have an uncrosslinked, atactic polymer below the entanglement molecular weight.

Flory came by Case, I'm told (I wasn't there), and was shown this work and he said, "Well, it's nothing but a manifestation of the glass temperature." In other words, he dismissed it as evidence, unless they were resolving the glass temperature, of which he approved. I eventually joined forces with Eric Baer and [A.] Hiltner and we published a paper (24) summarizing the evidence that this gelation phenomenon was a result of structure which formed in these solutions and that it was occurring well above the glass temperature. We could put together evidence from the literature showing that it was a separate phenomenon from

classification and occurred well above the glass temperature and in fact it occurred over the entire composition range back to pure polystyrene.

The T_{11} story is a kind of end of the line on the disagreement with the Flory school. It's one end of the line. In the meanwhile, it has become increasingly clear that neutron scattering does not mean and cannot mean what Flory believed it meant when he lectured in Helsinki. Not only Flory, but the people who did the work and a number of other scientists. So the neutron scattering was out. Then I came across, in my studies on T_{11} , a 1943 paper by [Kurt] Ueberreiter in Germany (25), in which he had predicted there would be structure in the amorphous, liquid state of atactic polymers (that early), and he gave a mechanism for it. That doesn't necessarily prove anything but his mechanism was almost identical to the one Frenkel gave in 1968.

Parenthetically, we do not presume, even privately, an ability to judge Flory's conclusions about these several cited matters. It is a simple fact that [T. G.] Fox and Flory reported an event in the liquid state above T_g , both tabular and graphical, by thermal expansion (26). This was one of our earliest citations in the 1963 paper which first suggests a $T > T_g$ phenomenon in amorphous polymers; page 1339 ff, including Figure 18 on page 1341 and later data copied from data of others, i.e., Figures 23-24, 26. One should note our cautious approach to a liquid₁ - liquid₂ transition above T_g tentatively labeled T_{11} , i.e., Table IX, page 1339 and related discussion. That section clearly reads:

"C. One glass transition, T_g , or several glass transitions?"

Only during the 1980s at MMI did we locate in the world literature revealing repeated evidence for events above T_g , not only T_{11} but others. These are summarized in a recent encyclopedia review (27).

This might be a good place to end the story. The battle is far from over. Some French scientist at Strasbourg has just published a paper saying that the Boyer-Baer-Hiltner explanation of gelation of atactic polymer solutions is all wrong, but this is par for the course. We have to simply await developments.

BOHNING: I did want to ask you to comment about one thing. You told me earlier that you had, even in view of this, at a later date, suggested Flory as the head of MMI.

BOYER: It was after the Manchester meeting. We had a list of what we considered to be some of the top polymer scientists in the United States and even abroad, who had the scientific ability and personality, reputation, and presumably some management ability to take on a job like this. So when Mr. [Ted]

Doan, representing the moneyed interests, asked me my opinion, I simply said, "Flory is by and far the top polymer scientist in the United States," and he said, "Well, let's go after him." Since I knew him and since I even had his telephone number, they asked me to call him, which I did. Flory went to Ann Arbor to see family before and after visiting Midland. He spent the whole day here, and gave a lecture. I was a key person in getting that advertised around Midland--posters in all the Dow research labs and so on. We had an auditorium, the biggest one then in Midland, packed to the eaves with over 350 people who came to hear Flory's lecture. I should say that this was soon after Manchester and that I hadn't sorted out the reasons for Flory's attack on me. I certainly am not one to sit around and brood. I never have been and am not now. I think that Manchester was out of my mind or at a very low level.

BOHNING: What year was that, that you were looking for a director here?

BOYER: It started about 1970. We went through a list of people. Flory was the first we talked to. It's all a matter of record here. There were people like [Walter] Stockmayer, [Arthur] Tobolsky, Roger Porter. It was pretty predictable. I suppose that I ought to really verify these dates.

I see the gist of what you're getting at--what happened when? What was the real sequence? It's entirely possible that Flory was here pre-Manchester, the more I think about it. By 1970 I had a talk in Toronto with Eric Baer. We'd gone through a list of candidates and had gotten turned down. Then Eric Baer proposed to us that his macromolecular department at Case take on this operation. I remember we discussed that at the Toronto meeting in 1970.

BOHNING: That's why I was curious about that sequence.

BOYER: Well, you're absolutely right.

BOHNING: The Tashkent affair is pretty well documented. We can talk about that if you feel you have anything else to add.

BOYER: No.

BOHNING: You've got a lot documented on that already.

BOYER: Thank you.

BOHNING: Thank you very much, Dr. Boyer.

BOYER: Ray, please.

BOHNING: That was for the tape.

BOYER: [laughter]

[END OF TAPE, SIDE 3]

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