THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

HERMAN E. SCHROEDER

Transcript of Interviews
Conducted by

Raymond C. Ferguson

in

Greenville, Delaware

on

30 December 1986 and 12 January 1987

Herman E. Schroeder
JH
3/15/96

CKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 24 February 1988)

Herman E. Schroeder, interview by Raymond C. Ferguson at Greenville, Delaware, 30 December 1986 and 12 January 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0063).

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

HERMAN E. SCHROEDER

1915	Born in Brooklyn, New York on 6 July

Education

1936	A.B., chemistry, Harvard University
1937	A.M., chemistry, Harvard University
1939	Ph.D., organic chemistry, Harvard University

Professional Experience

	E. I. du Pont de Nemours & Co., Inc.
	Chemical Department
1938-1945	Research Chemist
	Organic Chemicals Department, Jackson Laboratory
1945-1946	Research Chemist
1946-1948	Research Supervisor
1949-1950	Research Division Head
1951-1956	Assistant Laboratory Director
	Elastomer Chemicals Department
1957-1963	Assistant Director of Research
1963-1965	Director of Research
1965-1980	Director of Research and Development
1980-	President, Schroeder Scientific Services, Inc.

Honors

1936	summa cum laude, Harvard University
1936	Phi Beta Kappa
1979	General Award, International Institute of Synthetic Rubber Producers
1984	Goodyear Medal, American Chemical Society, Rubber Division

ABSTRACT

Herman Schroeder starts the interview by telling of his family background and growing up in Brooklyn. An early learner, Schroeder attended public and private high schools where his scientific interests were first aroused. After an early graduation he entered Harvard, where he initially planned for a career in medicine but soon changed towards chemistry, in part influenced by his tutor, John Edsall. Staying on at Harvard for graduate study in the physical aspects of organic chemistry, Herman Schroeder investigated the rates and mechanism of the closure of large rings. He discusses the choice between industrial and academic careers and the advice of the Harvard faculty. Arriving at the Du Pont Experimental Station in 1938, Schroeder outlines his initial assignments and his important wartime research on tire cord adhesives. Transferring to the Jackson Laboratories, he worked on both dyestuff synthesis and the mechanisms of dyeing synthetic fibers, as well as obtaining experience in production control. Moving to greater research responsibilities, Schroeder played an important role in the development of several polymers, which is described towards the end of the first interview. In a second interview, Ferguson asks Schroeder to comment on some of his Du Pont colleagues and on some of the academic consultants to the company. The interview concludes with some of Schroeder's retirement activities and a full account of the Louisville plant explosion.

INTERVIEWER

Raymond C. Ferguson obtained his degrees in chemistry from Iowa State University (B.S., M.S.) and his Ph.D. from Harvard University. He worked in research divisions of the Organic Chemicals, Elastomer Chemicals, and Central Research Departments of Du Pont, principally in molecular spectroscopy, organic structure analysis, and polymer characterization. Currently he is affiliated with CONDUX, Inc., a consulting association of ex-Du Pont professionals.

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INTERVIEWEE: Herman E. Schroeder

INTERVIEWER: Raymond C. Ferguson

LOCATION: Greenville, Delaware

DATE: 30 December 1986

FERGUSON: Herman, I would like to start with your family background. What was your father's occupation?

SCHROEDER: My father sold real estate and insurance. He started out in a wholesale hardware business that his father had founded and owned. My dad didn't really want to get into that, but I guess his father was too strong a character for him. That can often happen. My father grew up in New York City. His father was born in Germany and came over as a ten year old. He was the last of six brothers, each having brought the other one over in successive steps. It started during the German fuss in the 1840s, when there was a lot of turmoil. The other half of my family was Danish and sort of mingled American.

FERGUSON: This was your mother's side?

SCHROEDER: Yes.

FERGUSON: Did her family come over early too?

SCHROEDER: Yes. Her father came over from Denmark in about 1865. Her mother's family had come over to this country around 1740. It was the immigration of Central European people from the Rhineland and Alsace in France. They had lived in New York all that time. They had a farm in the Bronx and Lord knows who married whom.

FERGUSON: You mentioned one time that you had some problems growing up in Brooklyn with a German name.

SCHROEDER: That was because I was born in 1915, so I was a young kid when the furor of the first World War was around. With a name like Schroeder there was a lot of prejudice, just as there was prejudice in the last war with the Germans, the Japanese, and the Italians. My family was basically a New York City family.

Dad was born in New York and my mother was born in New York.

FERGUSON: Do you have brothers and sisters?

SCHROEDER: I have one sister who is two and one-half years younger than I am.

FERGUSON: What did she go on to do?

SCHROEDER: She went to Radcliffe and didn't like that. Then she want to Earlham College in Indiana. It's a Quaker college and she loved it. She studied the Classics, Greek and all that. Then she got married and had two boys. She married a physical chemist. Terrible fate. [LAUGHTER] Actually, she was [Professor] Herman Mark's secretary for a while.

FERGUSON: Is that right?

SCHROEDER: Yes. I ran into Herman, whom I had known, on a consulting trip. He was going around to the plants in our [Du Pont] departments to talk about polymers. I was detailed to go along with him to be sure that the fellows didn't tell Herman anything they shouldn't because he had a tendency to assimilate everything that was given to him and immediately put it out in a lecture. I was there as a a watchdog and there wasn't any problem. It was fun. As we were sitting on the plane, I was talking to him and I said, "Herman, do you remember my sister? She worked for you and was almost your first secretary." He said, "No. I don't remember that. What was her name?" I said, "Her name is Rose." He said, "Oh, Rosie!" My sister said that Herman used to chase her around the desk. She was twenty-one or twenty-two at the time.

FERGUSON: It sounds like other tales I've heard about Herman. Did he ever introduce you to his girl friend?

SCHROEDER: He may have. I don't know. I've been introduced to a lot of people at parties and so on, but I don't recall.

FERGUSON: Let's go back to elementary school.

SCHROEDER: I started in New York City in a public school and graduated from grade eight in a grammar school when I was twelve years old. I skipped a couple of years. Then my parents put me in a private school. I went to the local high school to see where they wanted to put me, and they decided they wanted to skip me two more years. My father thought that was absolutely ridiculous. So I ended up in a private school called Polytechnic Preparatory Country Day School in the City. It's called Poly Prep. It's a good school.

FERGUSON: Is it located in Brooklyn?

SCHROEDER: Yes. It's not too far from where the Verrazano Bridge goes from Staten Island into Bay Ridge. It's a nice school. There you get a typical old-style, classical, private school education. I studied Latin, German, History, English, Science, and Math. Interestingly, I had no chemistry. They had it but I never took it. But I took all the Math and Physics they could possibly give me, and also Biology. I took four and one-half years of German and of Latin.

FERGUSON: How long were you there?

SCHROEDER: I was there for four and one-half years. I gave back part of the time I had jumped. In fact, I was all ready to go to college, but I was so young that the headmaster said, "Why don't you stay and take what you want."

FERGUSON: How old were you then?

SCHROEDER: I graduated from high school when I was sixteen, but I could have been fifteen. I was just a good learner.

FERGUSON: Did you read a lot?

SCHROEDER: Yes.

FERGUSON: What got you interested in science?

SCHROEDER: There were a couple of things. One was that it just interested me. I loved math. Taking a course in math was like eating candy. It was a real gut course. I also loved physics. I had a family doctor who thought I would be a good physician. He would take me around Brooklyn in his red Cadillac convertible. He would visit his patients and then he would talk to me about them after he got out. He got me interested in that. So I went off to college interested in biochemistry and possibly medicine.

FERGUSON: You said you were in college while still at this prep school?

SCHROEDER: No. I took everything the prep school had to offer.

FERGUSON: How would you describe your parents?

SCHROEDER: Dad was a very gentle, relaxed soul who really encouraged me. He said, "Learn! You can then get to do whatever you wish." Mother was more a pusher. They spoiled me. They were middle income. We were not prosperous, but we weren't in any pain. I was able to get some scholarships. I was in high school from 1928 to 1932. Money disappeared fast then and I needed help to go to Harvard. For a good student, it's relatively easy to get. I also did some tutoring work there.

FERGUSON: Did your parents have any education beyond high school?

SCHROEDER: No. My father started to study law and decided not to go on so he studied some accounting at a commercial school. He wasn't a good student. He was intelligent, but he didn't really like that kind of stuff, nor did my mother. My mother's sisters did, though, and my mother's second older sister, Rose, was a high school teacher. She helped me a lot when I was a youngster, especially with things like math. Basically, if you read a lot, all that you need to get through a primary school fast is to understand the mathematics. She would teach me whatever happened to be the necessary trick for the various mathematical mechanics. That's what led to my skipping, because I could read so much. I could learn the math and I would understand it easily. I went through the necessary rigor of just getting hold of the detail that I needed so that I could jump ahead.

FERGUSON: How did you get to Harvard?

SCHROEDER: My headmaster was an exceptional man named J. D. [Joseph Dana] Allen. He suggested that I go there. I didn't really know much about the fit between various colleges and universities and people and their talents. I knew they were there and I knew which were good. He suggested that I ought to go there, maybe because he had been there, and I was one of his prize students.

FERGUSON: Was admission to Harvard at all complicated in those days? Was it comparable to now?

SCHROEDER: No. This was in 1931 and 1932. In those days you had to take board exams and other exams, but what got you in was money plus the ability to get a certain grade on these board exams; or social status, money, and the ability to get by; or being quite intelligent and doing very well on the exams. It wasn't a great thing. It was a lot more difficult than getting into some other places, but I was admitted to Harvard, Yale, and MIT with no problem.

FERGUSON: You said that you were interested in biology or medicine when you entered? Did that continue?

SCHROEDER: Not for long. I took an elementary course in chemistry my first year. That stirred me up a bit on chemistry, so I decided to really go after biochemistry. I got a 99 grade in [Arthur B.] Lamb's course. The next highest grade was an 88, so they got interested in me. As a result of that, as I went into my second year, they gave me a really fine tutor. He was John T. Edsall, who was over at the Harvard Medical School working with Professor Edwin J. Cohn. Edsall was a marvelous tutor. Having a tutor in those days meant that you just sat and talked with this fellow. He gave you books to read and you would tell him what you thought of what you were learning and what was in the book. The books supplemented what you were picking up in the courses. I've forgotten the names of the books I was devouring on biochemistry and the physical processes of biology. In my second year I took organic chemistry, which I really liked.

FERGUSON: Who taught that?

SCHROEDER: Professor Louie [Louis F.] Fieser. He was good and I found organic very easy. I had Professor [John H.] Van Vleck for physics.

FERGUSON: Did you take a course from Percy Bridgman?

SCHROEDER: Bridgman would come in and lecture to us periodically, but he wasn't the main lecturer. There were a couple of fellows who later won Nobel Prizes. They were damn good and very stimulating. That's what gets to you about a place like Harvard -- the stimulation you get from men like E. Bright Wilson, [George B.] Kistiakowsky, and [Edward M.] Purcell. I developed a stronger chemical interest and I gradually started to lose interest in going to medical school in my sophomore year. In my next year I took physical and analytical chemistry, and some more physics and some more math.

FERGUSON: Who taught those courses?

SCHROEDER: Analytical chemistry was taught by [Gregory P.] Baxter. It was an easy course, but very boring for the likes of me. There was very good math in it. Physical chemistry was taught by Henry E. Bent. He went out to Missouri. He was an awful nice guy, but a terrible lecturer. He used a British text [Findlay] (1), which used the British symbols for thermodynamics, but he lectured in the American ones. He was soporific. The net result of this was that I learned physical chemistry from the texts by Lewis and Randall and Getman and Daniels (2, 3). I just chewed up the books.

FERGUSON: Was Bright Wilson teaching at the time?

SCHROEDER: He was there but I wasn't aware of much teaching. For math during my first year I had a brilliant man named [Harold] Marston Morse, who later went on to the Institute for Advanced Study at Princeton. For my second year I had two fellows, [William C.] Graustein and [William F.] Osgood who were both good teachers. Osgood was brilliant and Graustein was a conservative. In the theory of functions I had a young professor named Lars V. Ahlfors who received an Honorary Degree from Harvard in 1989 for his brilliant career -- and someone else. They were all very nice, fine teachers and very stimulating. During my last year, I took Paul Bartlett's course in physical organic chemistry and George Kistiakowsky's course, Chem 13, advanced physical chemistry. That was a very fascinating course and quite a shock. I remember the first exam very well. I got a 43 on this exam, which turned out to be a good grade on the curve that he was using. But that was an awful shock to a fellow who had been churning out top grades throughout his career. It was very good for me. It gave me a sense of realism about science.

FERGUSON: I'm afraid that Kistiakowsky continued that tradition when I was there, because the comprehensives in physical chemistry were just horrendous. If you got half of the questions right, you were really something.

SCHROEDER: The language gap was on top of these fearsome questions that he would ask you. I had George when he didn't speak English as well as when you had him. As a result, what he meant and what he said weren't exactly the same thing. But, it was still very good.

FERGUSON: How about James Bryant Conant? Was he in chemistry at the time?

SCHROEDER: No. Conant became president of the University in the Spring of my freshman year. He was an influence on the chemistry department, particularly in some of the work that Elmer P. Kohler was doing, because they were good friends. In my senior year, when I took Kohler's advanced organic chemistry, Conant came in a couple of times to talk about selected subjects. Kohler's twist in this course was largely the physical side of organic chemistry, the understanding of what you were doing. It was not as physical as Bartlett's and some of the others, but basically that was the drive. There were certain subjects that Conant would come in and talk about.

FERGUSON: Did you have a senior thesis?

SCHROEDER: They didn't have any senior theses in chemistry. They had a senior thesis in biochemistry, but by my senior year, I had decided that I was going to go into some kind of pure chemistry and not medicine. Kohler gave me the job of checking out the Ph.D research of an earlier student which he thought weak and I confirmed his fears. Then I did a short research project.

FERGUSON: The tutorial system at Harvard was kind of unique at that time and is even now. I'm surprised that you started as a sophomore. Was that usual?

SCHROEDER: Yes. The only thing that was unusual in my experience was that I received more personal attention because of the stellar grades that I got. A more normal behavior for some of my friends who were in history or sociology was to have the tutor talking to a small group of fellows, maybe six or seven. Mine was always one or two. I had some very bright friends in my class who were also concentrating in biochemistry. Bernard Davis is a professor at the Harvard Medical School. John Hickam became a research professor in medical school, first at Duke and then Indiana, where he chaired the department. I had a lot of very bright friends who probably had as much influence on me as many of these professors, because we talked to the tutors and we talked to one another and also we were competitive. This was very good for us because we had different interests.

FERGUSON: Which house did you live in?

SCHROEDER: Leverett House.

FERGUSON: The house system was not terribly new then?

SCHROEDER: It was about four years old. By then the houses were starting to get some of their individualistic character, but they hadn't hardened. Our housemaster's name was Kenneth Ballard Murdock. He was an English professor, American Literature I believe, and a fine man.

FERGUSON: Were you involved in any extracurricular activities?

SCHROEDER: I did a little track in my freshman year, throwing hammer and shotput and was on the freshman rifle team. I started playing chess and was on the team for four years, and became president of the chess club. I didn't do much else. For sports, I used to get a shell and row up and down the river when I could. After the lab work I would play handball or squash. The laboratory work really consumes you if you're really taking all of those chemistry courses. My friends would have time for this, but mostly, I was up in the laboratory. For example, in my third year the nominal lab load was twelve hours. You may be able to grind out books fast and understand them quickly, but the laboratory work has to be done right. Being relatively young, I was a bit ham-handed. I had to learn how to do it.

FERGUSON: Had you had any projects at home while you were growing up?

SCHROEDER: Yes. I had chemistry sets and played with them. I later found that I had learned an amazing amount. Having had no chemistry, I found that when I took elementary chemistry I knew more than I should have because I read so much and because I had some feel for the behavior of substances. That's undoubtedly why I did so well in it. Also I had an extraordinary physics teacher at prep school. His name was Floyd L. Darrow and he really was a good teacher. I'll never forget one gadget he had out in his office. He had a complicated engine out in the anteroom to his office. It was supposed to do certain things. My experience with this gadget was that I looked at it and read what it was supposed to do. I started playing with it and it didn't work. So I looked at it and studied it awhile. Finally, I saw something that was wrong so I took it apart and put it back together again and it did work.

FERGUSON: Was this an internal combustion engine? Something like a Stirling engine?

SCHROEDER: No. I forget just what it was. Let's say it was a mechanical contrivance. Anyway, I got it to work. You could even call it a Goldberg. I got it so it would do what the little plate said it should do and it was working beautifully. Then I went and saw Darrow and went about my business. Later that day, I happened to go back to see him, and there he was putting that damn machine back as it had been before. It was there as a teaching device. He used it to find out which of his kids thought, and which just accepted things.

FERGUSON: How many did?

SCHROEDER: I asked him that in later years, and he said he got maybe five percent, maximum, of the students who took physics who would go in there and see that it didn't work, and try to fix it and did fix it. Most of them would just look at it and give up in disgust. Fiendish. It wasn't a very fancy thing. He was just trying to see if the people that he was teaching were either dead or alive. It didn't take any genius to see that.

FERGUSON: Yes. Like the questions of Kistiakowsky. He'd ask unexpected questions.

SCHROEDER: Yes, utterly disconcerting questions.

FERGUSON: Who in particular influenced you during your undergraduate career?

SCHROEDER: There were a lot of them that really stimulated the thirst for knowledge and the questioning mind.

FERGUSON: Was it just the general atmosphere?

SCHROEDER: Yes, but the people who had a particular affect on me besides my tutor Edsall were Professor Marston Morse whom I took math from. Louie Fieser was a fantastically good teacher in organic chemistry. George Kistiakowsky was also very good, as were Kohler and Paul Bartlett.

FERGUSON: How did you come to stay on at Harvard for graduate school rather than go elsewhere?

SCHROEDER: When I got to my senior year I decided that I really wanted to go ahead and get a doctorate in the realm of the physical side of organic chemistry. I liked Kohler, so I tried to persuade him to let me study with him. He said that I really ought to go somewhere else. I just worked on him and he said, "Okay." I had him in a difficult position, too, because by the time I had graduated from Harvard I had taken all the chemistry courses they had and I had taken my comprehensive exams for a master's degree as a senior. All I had to do to get a master's degree was sit and suck my thumb. I was ready to do research.

FERGUSON: So he gave you a master's degree?

SCHROEDER: I had to have a year's residency to get the master's degree.

FERGUSON: What about the thesis?

SCHROEDER: There was no thesis. I had completed an adequate research project as a senior.

FERGUSON: Just course work?

SCHROEDER: The course work was done. I had to pass their comprehensive exams. I was very practical and realized that if I stayed another year, I would have forgotten a lot of the course work I had taken, since I would just be doing research. So I asked them if they would please let me take those exams as a senior because the courses were all fresh in my mind, and they did.

FERGUSON: Tell me about Kohler.

SCHROEDER: Kohler was a Pennsylvanian Dutchman. He went to Muhlenberg College and studied with Ira Remsen.

[END OF TAPE, SIDE 1]

SCHROEDER: Kohler was a little fellow, maybe 5'4" or 5'5" tall, with enormous ears and a very clear mind. He was a hell of a good teacher, very well organized. After he got his degree with Remsen, he taught at Bryn Mawr College in Philadelphia. While he was there he started to put out some very fine papers, continuing the work he had done with Remsen. He was more or less a founder of the science of physical organic chemistry. He had taken organic chemistry to the standpoint of understanding what's going on, rather than just knocking out molecules. He had a profound influence on a few students at Harvard when he came there, namely Elmer Bolton, James Conant, and Roger Adams. He poured out some magnificent students like Frank Whitmore, Lee Irvin Smith, Frank Westheimer, Gilman and many others.

FERGUSON: Who was the first one?

SCHROEDER: Frank Whitmore, I think. He went to Penn State. Kohler was a very interesting man to listen to in a lecture. He came in with no notes and delivered lectures which were remarkably lucid and clear. He would take you through the development of a particular area in chemistry, first in a semi-historical way, showing you the trend of thought that led somebody to their conclusions. He would convince you that something was right and then he would go ahead and show what was wrong in this particular line of logic -- why the chemistry was wrong and what was right and how you go about showing it was really right. At the end of a year of that you had a pretty good feel for the weaknesses that are inherent in the study of a science, and also the strengths. He showed how to tear something apart.

He didn't use a text. He insisted that if he became a slave to a text, his course would become stereotyped and not keep moving forward. When I pressed him for a couple of books to read, he suggested I work on Walter Hückel's Theoretische Grundlagen der Organischen Chemie which is Theoretical Foundations of Organic Chemistry (4). It was a marvelous exposition of the science of understanding organic chemistry from the standpoint of how atoms work and why things happen. It was in hellishly difficult German, even for a fellow like me who could read German easily.

FERGUSON: Did you speak German at home?

SCHROEDER: No. I spoke no German at home but I took four and one-half years of it in small classes at Poly Prep from a fellow named Hermann August Buschek. I liked it and found it easy, just as I found Latin easy. When I got to Harvard I took a couple of graduate courses which were basically literature courses. I read German literature and then wrote essays on it in German. That was good training.

FERGUSON: Did you find a big difference in scientific German?

SCHROEDER: No. Scientific German is like cream of wheat compared to the other stuff. It's very easy. The bulk of the words are common across science and the context that stuck everything together was easy for me. I could read scientific German just as easily as scientific English. It was the science that was difficult, not the German.

FERGUSON: Did you start reading the current literature as an undergraduate?

SCHROEDER: Yes. Two courses made me do this. One was Kohler's. Kohler put me to work reading it. Harvard had a very interesting teaching method. They called it the reading period. Between Christmas vacation and the examination time you were off. You got assignments of things to read which were supposed to supplement the course work. Because I had a tutor, I had an unusually fine list to work on in the science area. Then Kohler exposed me to polymer science, which was interesting back in the fall of 1935. He started me earlier than the Christmas vacation and said, "Why don't you read Wallace Carothers' papers?" I chewed through Carothers' papers. Then he gave me some other things which were related to what was going on in polymerization, especially the physical side of polymerization which was very primitive back then. Paul Bartlett made us read much current literature and comment critically. We were suppose to find flaws in very convincing articles by famous scientists which Paul had selected because of their errors in reasoning.

FERGUSON: Did this lead you back to [Hermann] Staudinger or some of the other earlier theoretical papers?

SCHROEDER: The Carothers work didn't lead me to Staudinger but Paul Bartlett's course in theoretical organic chemistry did. Paul was just then learning how to teach that course. He had only freshly come from Minnesota where he had been teaching. He was one of Conant's students but he went to Minnesota and came back. He was just trying to formulate this course in theoretical organic chemistry. He put me into a bit of Staudinger's work. Thus I had a little feel for polymer science even though working with Kohler, I didn't pursue it in graduate school.

FERGUSON: What did you work on in graduate school?

SCHROEDER: I worked with Kohler. We started out going after an optically active allenic structures with the aim of basically studying the rates of racemization under certain conditions, to get a feel for the processes. It was a molecule that was built out of four big groups on allene [$H_2C=C=CH_2$]. The chemistry really didn't work, maybe because I was inept or too heavy handed. I think the ideas were sound.

So after about nine months of that we moved over into another field that was a subject of curiosity for Kohler. They used to call him the King because he was their top organic chemistry professor. Kohler was interested in the big ring compounds that Karl Ziegler had made. Ziegler had observed, in a rather beautiful series of papers on the closure of large rings from bifunctional materials (5), that there was an interesting dip in the yield curve which then went back up. At six and five [ring atoms], you get 98% yield. At nine, ten, eleven, twelve, thirteen, you get practically none. Then you come out of that trough up to 60 or 70% yield at around 16, 17, and so on up, with a very interesting alternating effect.

Kohler was curious about that affect and asked me if I would like to take it on. He wanted to get a feel for whether there was an inherent property of these rings, other than the steric hindrance and the probability of trying to get the ends together in cyclization that was reflected in their chemistry. He wanted me to study certain rates of reaction of these things. We tried to make the thirteen-member ring, which was difficult the way he started me out. The procedure he suggested didn't really work, so I went back and duplicated Ziegler's work and made 13, 14, 15. Then I started to study the reaction rates of ring enlargements with diazomethane. This is a reaction of the carbonyl group.

To get there you started out by ring closure of a dinitrile with a very nifty catalyst, which is usually the lithium or sodium amide of methyl or ethyl aniline or of another secondary amine. That got me into some very interesting chemistry, because the methods that Ziegler used for making those alkali metal amides were well hidden in patents.

FERGUSON: Was this the Karl Ziegler of coordination chemistry?

SCHROEDER: Yes. It's the same Karl Ziegler. He got me interested in organometallic compounds. You close the dinitrile to an alpha-cyano cyclic ketimine. If you take the ketimine and stew it up with dilute sulfuric acid, you hydrolyze it to a beta-ketocarboxylic acid which decarboxylates and gives a cyclic ketone.

FERGUSON: Were you studying their reactivities?

SCHROEDER: Yes. Incidentally, starting to look at these and playing around with the Stuart-Fisher-Hirschfelder models, I stumbled on to some of the concepts that are inherent in conformational analysis. I could have been famous if I had pursued it. From a look at the molecules with Stuart models you could see that the natural conformation the various rings wished to assume was very interesting. In some of them, the carbonyl was buried inside the ring and in others, the carbonyl was wanting to stick out, more or less as it was in cyclopentanone or cyclohexanone. On the basis of this, before I had done any rate work, I suggested that when we got to studying the reaction rate of cyclic 15 that it was going to be a lot more reactive than we were guessing. It was going to be basically like C_5 or C_6, with an appropriate correction for the dilution. Kohler said, "No," and that he would stake his reputation that that was wrong. When I ran the measurements and I was right on the button, I went in to tell him about it and he laughed and said, "I'm not going to give you my reputation because you didn't give me all of the facts. You didn't tell me all of your thoughts." Anyway, we analyzed the curve and got a feeling for the reaction rates of these things and the distributions as you went up and down this curve. Kohler said that I had done enough and ought to get out of there.

FERGUSON: What was your thesis title?

SCHROEDER: The Reaction of Large Ring Ketones with Diazomethane.

FERGUSON: Did you ever publish this in the external literature?

SCHROEDER: No. I didn't publish this outside for a complex set of reasons. Kohler became ill in February of my second year in graduate school. They thought he had pneumonia and he didn't get well. Then they took him to the hospital. It turned out that he had a malignant brain tumor and he died in May. It was about March when he told me that I was through and should collect my thoughts and start writing a thesis. In all the confusion of that, it wasn't possible to get things done with Kohler. I lost him as an advisor. At Kohler's suggestion and with the concurrence of the Department of Chemistry and of the Du Pont Company, with whom I had decided to work, it was agreed that I would start work for Du Pont in June or July and send in the thesis and take the exams later. So we did that and I got my degree the following January.

FERGUSON: You were married to Betty at about this time?

SCHROEDER: I got married in June of 1938. That was basically the signal that we could get married.

FERGUSON: Did you meet her at Radcliffe?

SCHROEDER: She was in the same class. We met in our sophomore year, on a blind date through friends. We had no course together until our senior year, when we both took an English course in American Literature, which was interesting. She took it at Radcliffe and I took it at Harvard.

FERGUSON: Where was she from?

SCHROEDER: She was from New England, born in New Hampshire, lived in Maine for awhile and reared in Newton, Massachusetts. Her father was a Harvard graduate and her mother was a Radcliffe graduate. They were from old New England families, Barnes and Loring.

FERGUSON: Had you ever considered an academic life?

SCHROEDER: Yes. I was loose about it at first. Then I really got more interested in the kind of life that I could lead in industry, because it was mainly a question of liking to accomplish things. That was possibly prompted by a lot of the stuff that I had read about the accomplishments of great scientists. Kohler offered me a job as instructor at Harvard. Then he got me a job at Illinois. Harvard also offered me a very interesting position in the science side at the Medical School. They said they had funds to undertake a scientific study of the birth control problem. They had plenty of money, and they wanted me to go over there. I could do that as a Fellow and pick up a medical degree. It was very tempting and almost got me there.

FERGUSON: You could have been another Carl Djerassi.

SCHROEDER: I don't know what I could have been. It was very tempting. The only reason that I didn't do it was that they had no facilities for doing the kind of work that needed to be done. I knew a little too much about the state of biochemistry. I realized that, at that time, what they wanted to do and the way I would go about it, I would have to spend an awful long time learning the necessary biochemistry, biology, and physiology to get into the right areas. It was a long, long run and I wasn't really that interested with fighting through to get the facilities. The motivation was lacking although the intellectual stimulation was great.

FERGUSON: Was this offer through John Edsall?

SCHROEDER: I don't know where it came from.

FERGUSON: I recall Edsall as being concerned with population control. Is that the same John Edsall?

SCHROEDER: Yes, but I didn't know that then. I knew his work on the chemical processes in physiological behavior and the chemistry of blood. It was physical chemistry with an organic switch, working mostly with Cohn. I don't know where he went after that in his work.

FERGUSON: There is a very distinguished John Edsall at the Harvard Medical School and also in public policy roles.

SCHROEDER: John Tileston Edsall, that's him, my tutor. I never thought of that connection. There was another young fellow I knew at the Medical School -- Alwin M. Pappenheimer, Jr. But I never thought of Edsall possibly being behind that offer.

Basically, I was more interested in chemistry. Kohler suggested that if I really didn't want to go into an academic career, then Du Pont would be a good place to work.

FERGUSON: That's interesting because Harvey Hoehn told me one time that the Harvard faculty had a very low opinion of Du Pont. They had some grudges against Du Pont going back to firings in the early 1930s. Was that true? Did you hear anything about that?

SCHROEDER: I didn't see any prejudice against Du Pont from either Kohler, Fieser, or Bartlett. As far as I know there was quite a close relationship between Jimmy Conant, Roger Adams, and Elmer Bolton. They had all been students together at Harvard. I understand that it was moot back in the 1913-1915 period which one would go into academic work and which one would go into industry. There have been many apocryphal stories told about that. I didn't catch anything of prejudice. The case where there might have been a little bit of reserve or unfriendliness was on George Kistiakowsky's part. He had an experience which led him to develop a low opinion of some of the people down there.

FERGUSON: Well, he was at the Experimental Station for a short time in the mid-1930s.

SCHROEDER: It was before that, because by then he was at Harvard. It might have been the early 1930s. They all had great respect for Carothers. After all, Carothers had gone from Harvard at Conant's urging, besides Du Pont's pulling.

FERGUSON: When I was at Harvard, the faculty all seemed to be very oriented towards preparing people for academic careers.

SCHROEDER: I'm sure of that.

FERGUSON: It certainly was not a training school for industrial jobs.

SCHROEDER: But they were closer in sympathy with an outfit like Du Pont, partly because of all that fundamental work that Carothers had been doing. I don't remember any hostility, quite the contrary. Some of their best graduate students they cheerfully sent down there like Jud [A. Judson] Wells, Elmore Martin, and Frank [William Franklin] Gresham. I got a *summa* so I wasn't the junk part of what was going out there, in any case. Yet, I remember years later, we had a young man that we were going to hire at our laboratory, who was a Westheimer student. This kid was convinced that he should come and work for us. He was very pleased [with the offer] but Frank Westheimer told him not to do it.

FERGUSON: Professor Bright Wilson was that way with some of his students. Norbert Muller interviewed at Jackson Lab. He was one of my classmates, a crackerjack guy who apparently had had his fill of academic life. He told me that Bright had become decidedly cool when Norbert was out looking at industrial jobs. After he decided to go back to Purdue, Bright was very happy about it and wrote him a nice letter.

SCHROEDER: Of course, the complexion of opportunity for academics changed greatly between the 1930s and the 1950s, with all that government money pouring in. The complexion of industrial work also changed.

FERGUSON: In all fairness, Wilson really pushed me to go to Du Pont. He thought it was an impressive place and the best of my opportunities.

SCHROEDER: It depends on the person.

FERGUSON: He had his own view on what a student should do.

SCHROEDER: It could well be that both Wilson with Norbert and Frank Westheimer with his student, were really looking at the characteristics of the man and the place that he would do best from a personal opportunity standpoint.

FERGUSON: How did you fit in at the Chemical Department at Du Pont?

SCHROEDER: All right. The Chemical Department was good. I found that the work they were doing varied from mundane to excellent, as you might expect. The emphasis was nowhere near so much on fundamentals as I had been led to believe by the Carothers work. In fact, if you read a little bit about the story of Carothers in the book by [David] Hounshell and [John K.] Smith (6), that's coming out of the Hagley Museum and Library, you get a feel for this. I found that it was a hot bed of Edisonian research. Theoretical understanding of what you were doing wasn't exactly encouraged. They wanted you to try things and make inventions without inhibitions.

FERGUSON: Wasn't this a change in philosophy from [Charles M.] Stine's view to Bolton's?

SCHROEDER: A bit. Bolton was very pragmatic, and he wanted results. He was a hell of a good man, but he was very well focused.

[END OF TAPE, SIDE 2]

FERGUSON: Let's go back to the Experimental Station.

SCHROEDER: I want to be fair to them, because you have to realize that when I hit the Chemical Department at the Experimental Station in early July of 1938, they were in the full throes of the nylon development. Furthermore, they had opened a veritable lode of potential in the polymer field. They were perfectly well aware of it. It's like walking into virgin territory. No matter where you looked there was gold. Their motivations were pretty much, "Let's go out and dig like mad and see what we can find. Let's make lots of new polymers. We know what we're doing." They did know what they were doing, certainly in the field of condensation polymers. They were making some fair headway with some of the vinyl sorts of polymers, although they didn't have the same understanding of the processes involved as they had of the condensation polymers. Thus, there was a reason for doing all this digging, for being so Edisonian.

They put me to work on two jobs developing new intermediates for condensation polymers. One was air oxidation of fatty acids under pressure to see if I could make cheaply some dibasic acids like adipic acid. That turned out to be fun because it got me into transition metal effects on free radical behavior. I had a nice time making some rather interesting molecules that surprised me very much, because I didn't quite understand some of the rearrangements that were possible with these unsaturated peroxides which were formed.

The other job turned out to be a very straightforward physical organic job, as I approached it. ICI, with whom we had a research agreement, had claimed that if you chlorinated an aliphatic hydrocarbon using a particular wavelength of ultraviolet light, the chlorination would be at the ends. Therefore, it should be very easy to make nice, clean bifunctional materials from very cheap source materials by this route. That didn't make much sense to a fellow who had a fair training in physical organic chemistry. I took a model molecule, straight chain octadecane, which I synthesized by a decarboxylation route. I got the proper UV light set up, got the right wavelength and chlorinated. Then I analyzed the constituents which were converted to mono- and di-basic acids through appropriate chemistry. I got an absolutely perfect

Gaussian distribution -- chlorine distributed all over the chain -- absolutely perfect. You couldn't ask for a nicer proof of the randomness of chlorination in a hydrocarbon. It was marvelous. I presented that to the Steering Committee and that was a very unpopular result. They didn't like it.

FERGUSON: This was the Chemical Department Steering Committee?

SCHROEDER: Yes.

FERGUSON: What was wrong with the ICI work?

SCHROEDER: The ICI work was absolutely wrong, poor chemistry. It was very sad, because when I wrote the report on this, the ICI people immediately went about confirming what I had done and they had to write a retraction. The man who had done the work said, "Well, what this fellow says is correct. But if you do it with our particular kind of Scottish shale wax, then I'm right."

FERGUSON: Did the messenger of bad news suffer?

SCHROEDER: A little bit. It was funny. I was brought into the front office after presenting this and told it wasn't good form to destroy an idea like that.

FERGUSON: What should you do?

SCHROEDER: I don't know.

FERGUSON: I can remember some people who did bear bad news in later years.

SCHROEDER: I came out of this one all right. By then, I got involved in a lot of reactions involving bifunctional materials and their use in synthesis for dyes and things like that. I also got involved in one piece of polymer work. I was put on a job of trying to improve the fastness of dyes on fabrics and studied the reactions of polyfunctional materials with the dyes as a way of fixing them to the fabrics, possibly making polymers that would stay there.

FERGUSON: These were sites in the polymer chain?

SCHROEDER: Yes, amide on wool, silk or nylon or hydroxyl on cellulose. Basically, I would take things like tris-methoxymethylmelamine, bis-methoxymethylurea, and polyisocyanates, take a dyed fabric and treat it with the polyfunctional reactants to see if its fastness was improved. It was, but unfortunately, it changed the shades too much and the fabrics were embrittled!.

FERGUSON: I believe you had a patent on that (7)

SCHROEDER: I believe so.

Then with the outbreak of the War, I got dumped right into an aspect of the polymer field to find an adhesive for bonding nylon tire cord to rubber. This was very important because the B-29, which was under development, couldn't fly unless they could stick the nylon to rubber. The plane was so big and so heavy that to make tires strong enough out of cotton or rayon would make them too heavy, too multi-plied, and too thick. They would build up too much heat when they landed. So, for physical reasons of strength and weight, the tire cord had to be made out of nylon, the only fiber strong enough, but nylon didn't stick to rubber.

FERGUSON: Was this a monofilament nylon or a yarn?

SCHROEDER: It was a monofilament nylon woven into a complex twisted 2 or 3-ply yarn. They started with monofilament, and then these were twisted and made into a multifilament yarn, so many plies, so many twists, and that sort of thing. The physical structure of the fiber has a great effect on its properties. That's quite a science. We found a damn good adhesive. In fact, it's still being used today for almost all of the tires around the world. This was after much beating around. I had a lot of fun with some mechanistic work, finding out what stuck things to what. Basically, the issue was to find an adhesive. We found the butadiene-vinyl pyridine polymer, mixed with resorcinol formaldehyde, which really wasn't a great discovery. It wasn't anything fancy. Resorcinol/formaldehyde was the constituent that as a resin mixed with rubber latex had proved best for sticking cotton and rayon to tires (8). It was just a question of finding a polymer that would have the proper adhesion to nylon and still be compatible with the rubber.

For largely theoretical reasons, since I worked on problems that way, I came up with the idea of vinyl pyridine. I'm not sure the theoretical reasons were right, but the adhesion was marvelous. You know how such things are.

FERGUSON: How did you test the adhesive?

SCHROEDER: We got some square woven nylon fabric made out of the tire cord and dipped the fabric in the adhesive that we developed. We made a sandwich with unvulcanized rubber in a press, then measured strip adhesion in a Scott, Instron, or similar tester. When we finally got a hold of something that looked pretty good, we went to more sophisticated tests. The test that we ultimately used was an H-shaped article, nylon as the cross bar, which was pulled apart. You have two strips of rubber, with cotton backing to give them strength, connected crosswise to a tire cord fabric immersed in the adhesive. Then you pull the two rubber strips apart to pull the tire cord out. You would measure the strength of the bond by seeing how much it takes to pull the cord out of the H, and you also see whether the rubber sticks to the fiber.

FERGUSON: The cord was transverse?

SCHROEDER: Yes. The cord was the crossbar in the "H". That's the standard test. There were lots of other tests. That was very successful.

FERGUSON: This was related particularly to nylon development.

SCHROEDER: Yes. But really it was necessary for the war effort, Nylon tires were essential for the B-29. Before our adhesive they got 1 or 2 landings per tire. After our first test they exceeded 100.

FERGUSON: Were you involved in the World War II Rubber Project?

SCHROEDER: Yes, in a different way. I had a separate project which was related to chemicals for synthetic rubber. This was done by Du Pont, independent of the Rubber Project, but related to it. We were looking for new curing agents, new accelerators that would work with SBR and Neoprene, and in particular, for peptizers.

The real problem was that as SBR was made back then, the molecular weight control wasn't too good. It was too high in molecular weight and gel content and had to be softened to a more usable or workable consistency. Peptizers which had been used for natural rubber, the aromatic mercaptans, really weren't good enough. Later on ones were found in that very field that were excellent in synthetic rubber. So I did a lot of work on chemicals for synthetic rubber and discovered several (9). Also there was a fair amount of work done by Du Pont on the Neoprene side of the synthetic rubber project.

FERGUSON: By the time you came, had Neoprene work gone over to Jackson Lab?

SCHROEDER: Yes, pretty much so. We were still making new copolymers, more or less in cooperation with Jackson Lab. We were looking for new polymers that would be better than polychloroprene itself. We also looked at some butadiene copolymers and related things that might give better synthetic rubbers, particularly with better freeze resistance. Rubber was hot on their mind. Most of the polychloroprene work was going on across the river. The intercourse between the Central Research, as it became, and the Organic Chemicals Department was excellent. The man running the polymer work at Jackson Laboratory was Howard Starkweather, Sr. He had come from Central Research and was a good scientist. So the interchange was excellent.

FERGUSON: I had heard that Walter Carpenter was very concerned about getting involved in the Rubber Project. He didn't really want Du Pont to get involved in it. Was that true?

SCHROEDER: Probably. It was pretty much a Du Pont credo not to get too deeply involved with government projects, mainly generated by the desire to keep the technology to ourselves. The basic philosophy was that if the government has a job that needs to be done, let's not ask them for money, let's do it. So we did it. We just poured our own money into it. That way you weren't subject to somebody telling you that you had to give the technology you developed to Goodyear or somebody else.

FERGUSON: Did the Manhattan Project have any impact on you as far as your work there was concerned?

SCHROEDER: No. I knew something was going on, but I didn't have the foggiest idea of what it was. The only hot project that I stumbled into was one on mustard gas antagonists, many of which turned out to be sulfur compounds which were curatives or accelerators. I stumbled into some of those, and they just swore me to secrecy as I was playing with them.

FERGUSON: Who were your supervisors in the Chemical Department?

SCHROEDER: The first group leader was Bill [Wilber A.] Lazier. The second one was Dick [Richard S.] Schreiber. The third one was Charlie [Charles J.] Mighton. Lazier was basically a high pressure catalyst man -- ammonia synthesis, hydrogenation, things like that. He had done a fair amount of work in support of the nylon project on catalysts for hydrogenation of adiponitrile to hexamethylenediamine. Lazier was a Homer Adkins student.

Schreiber was a Roger Adams student and a wild man. He was a nice guy and had lots of ideas, but not much judgment. He was kind of fun to work with. Mighton was a Chicago product, a Canadian and a bit unscrupulous. He was overdriven by ambition. Then I had Dave McQueen at the last and he was by far the best.

FERGUSON: Did they continue on with Du Pont?

SCHROEDER: Lazier left and joined the Southern Research Institute as Director. Then he worked for Pfizer. Schreiber was hired away and became Research Director for Upjohn. Charlie Mighton did continue with Du Pont. Central Chemical dealt him away to Orchem [Organic Chemicals] and they dealt him away somewhere else. He ended up in the Biochemicals Department at I & B [Industrial and Biochemicals Department]. Of course, McQueen became director of Central Research.

FERGUSON: I know a Harold Mighton.

SCHROEDER: That's his brother. Harold's a nice fellow. Charlie was not very popular. He was a bit grabby of his men's idea.

FERGUSON: Was Arthur Tanberg the laboratory director when you were there? Did you have problems with him?

SCHROEDER: Not at all, really. Tanberg was okay, but he couldn't get out of his formal clothes. He was an earnest hard-working man, dedicated to Du Pont's welfare and his research organization. He was a seething mass of prejudices -- anti-Semitic, anti-Italian, anti-this, etc. Of course, that was common elsewhere in the company. At the time Du Pont was a WASPy organization, particularly in the chemical department.

FERGUSON: Was he a good scientist?

SCHROEDER: I don't think so. But he liked things done right. He was very good that way. He had very high standards and he was very proud. He did his best to make the Chemical Department the best. "That's good. That's great!" I think he was probably a good administrator, and he was a marvelous judge of people. He did almost all of the hiring for all the Central Chemical Department. He did it conscientiously and very well and assembled a superb staff. He hired very good people. He was a reserved character of a paternal type, not the kind of man you could dislike.

FERGUSON: You mentioned earlier about going before a Steering Committee. Did you have to report to the Steering Committee directly often?

SCHROEDER: About once a year.

FERGUSON: Was everything done in monthly reports?

SCHROEDER: Yes, there was an awful lot of writing. We had bimonthly summaries, very formal, and we had to write progress reports every three months on every project. From 1939 on, I was a whirling dervish because I had three projects. That meant an awful lot of writing. It helps make your writing better, but it raises hell with your ability to do work.

FERGUSON: Is that why you didn't publish in the external literature?

SCHROEDER: Not really. I did publish a little but not much. I wrote a paper on some heterocyclic chemistry that I did with George Rigby (10). We weren't really encouraged to publish much.

FERGUSON: I was going to ask that, because I think there have been a lot of cycles in Central Research. When I got in, it was "publish or perish."

SCHROEDER: The first cycle, with Carothers, was to publish and they did publish. Then, when they ran into this veritable gold mine in the polymer field, they weren't too anxious to spill stuff out. I don't blame them. They had an Assistant Research Director, Ernest Benger, who realized some of the dangers of publishing things before you really knew what you had. So he dampened some of the early fervor for publishing. When I got there in 1938, things were published only when it was absolutely certain that it was harmless to publish them. The real trouble there is that you're doing a lot of other work -- especially when the war hit. By the time that you know that something's publishable, you're hotly immersed in something else. You haven't got time. Then with the war, we went from working five days a week to six days a week and longer hours each day. There was a long period there when we just didn't publish.

FERGUSON: But you did get several patents out during this period.

SCHROEDER: Yes, on some dye fixatives, rubber chemicals and adhesives (7-9).

There were two accomplishments that interested me more than the things that we're talking about. One showed up in a patent. I was working in a laboratory with George Rigby. He was trying to get alcohols and amines to add to tetrafluoroethylene (TFE) but couldn't get any reaction. He asked me why he couldn't get the materials to react. It was very simple, if you understand some physical organic chemistry. I suggested that TFE has fluorine atoms strongly attracting the electrons from the double bond so that catalyst like a base or a strong acid was needed, such as H_2SO_4 or a strong base. As a result, we found out how to do it. George made very many adducts, and the first of which is in a patent that I have for adding ammonia and amines to TFE (11), and that got him started.

The other was a piece of "bootleggy" work on the first rubbery fluoropolymers. I made a copolymer of ethylene and trifluoromethylethylene (3,3,3 trifluoropropene-1) (12), but this copolymer didn't turn out at all the way I expected. I was interested in that monomer because of its functional resemblance to acrylonitrile in pulling the electrons away from the double bond. I wondered what you could do by adding things to it and polymerizing it. I was very surprised to find the extraordinary affect of a single CF_3 group on a long ethylene chain in pushing the chain apart. That had later consequences in synthetic rubbers, e.g. Viton.

FERGUSON: Have we summarized the Chemical Department experience?

SCHROEDER: It was fun. It was a nice place to work. It had good spirit and lots of investigative fervor. At the time I left it, which was my own election, the morale was very low because of the pressures of the war, plus too many good people and too poor a selection of supervisors, plus some poor selection of projects. So many chemists left them. They wanted to get out either into other Du Pont Departments or elsewhere.

FERGUSON: Had the wartime demand damped down new research on synthetic polymers?

SCHROEDER: A lot of things were damped. Some of the programs were allowed to continue, naturally, because they were compatible with the wartime effort, but perforce work had to be better focused. It was a difficult period. There are many reasons why morale can go bad. You have to consider the times we were in and all the stresses that were on the management and the staff.

FERGUSON: Did you pick Jackson Lab?

SCHROEDER: Yes. I had a very interesting experience with Du Pont here on which I was really very well treated. I told Dr. Tanberg that I wanted to get out and work in another department. I said, "I don't know these well enough so I'd like to see a couple of them." He wouldn't do it. He said, "You're a good chemist. What could be a better place to be than here? You have a good future here." I said, "I'd like to do something else." I couldn't persuade him, so I got a job outside with a pharmaceutical company. I walked in and quit. He said, "Oh, you meant it." I said, "Sure I meant it. Why didn't you listen?" He said, "Well, you're a good chemist. I can get you anywhere you want in Du Pont. Where do you want to interview?" I said, "Oh, you tell me." So I interviewed at a bunch of the departments and on the basis of what I saw I decided to work at Orchem. I had an interesting interview experience going around these departments.

FERGUSON: These were all the various operating departments?

SCHROEDER: Yes. The only other one that really tempted me was the Ammonia Department with Roger Williams and Alfred T. Larsen. That was very tempting. That's where my friend Gresham was. But I had been out of the kind of chemistry that I liked to play with for a long time. I thought I had a better chance of getting in some chemistry of the sort that I might like at Orchem. That's basically how I made the decision. It was less ambition-driven than research-oriented. The ambition drive was probably better answered in the Ammonia Department or Fibers. I had more desire to have a crack at some chemistry after this wartime excursion in the Chemical Department.

[END OF TAPE, SIDE 3]

SCHROEDER: Jackson Laboratory had good laboratory directors. They had a fine attitude toward the work. They told me, after I had had the experience that I had had in Central Research, they thought it would be a good idea if they just dumped me into some practical problem in a dye field not of my choice. I didn't object. I ended up with Mel [Melvin A.] Perkins, a fine man in the anthraquinone dye field, on a war-related problem dealing with behavior of vat dye dispersions, before they were padded onto fabrics. Out of this, I had to learn quite a bit about colloid chemistry, which was damn good for me.

I found that the attitude toward research at Jackson Lab was freer than it had been in Central Research. I was less directed. Nobody cared what I did as long as I did a reasonable amount of work on the project they assigned and came up with some kind of an answer, which I did. I found that it was an easy project. I was spending more of my time just synthesizing molecules and having fun, than I was working on the project. The first thing I knew, I made a rather interesting discovery of a new synthesis of polynuclear compounds. It was a nifty synthesis. I was able to put together in one fell swoop, a molecule with five rings from a benzene derivative and naphthalene. The synthesis was generally applicable with naphthalene derivatives and anthracene etc. The net result of this was that I opened a new field and was promoted to run a group on exploratory vat dye chemistry.

FERGUSON: You were listed as a research chemist for only a year there.

SCHROEDER: Yes.

FERGUSON: Could you continue synthesis after you became a supervisor?

SCHROEDER: Yes. I was in a laboratory and I had a group of about six people. I was encouraged to do work to the extent that I wanted to. This was great because I had chemists doing work, some of which we had to do, and some of which we cooked up. I had a chance to cook up ideas and get people interested. Also, I could do work myself on something that was compatible. I had a lot of fun in this period. This is when I taught myself microchemistry to make things move faster. You can get the answer on a small scale and decide whether it is worth pursuing.

FERGUSON: Was the general strategy empirical synthesis?

SCHROEDER: Not at all. The general strategy was on a higher level than that. Color research, particularly of vat dyes, was a highly elaborated science which had been systematically built up by exploratory work largely done by the Germans. Some programs were just a rational extension of what was good. There was a knowledge of the basic principles that governed color and fastness in colored molecules, which had been fairly well elucidated empirically and supported theoretically. We were doing work in areas which we knew had a chance of being fairly fruitful. We knew pretty much what the color was that we were going to get. Out of this, I did get some good patents (13, 14).

One approach involved straight empirical work, where we replaced benzene rings in certain selected vat dyes structures with thiophene rings. We made the surprising discovery that many molecules had better fastness to light when constructed partly out of thiophene. We knew about what the color will be because we understood the amount of the absorption shift that comes from plugging in the thiophene ring (13).

The other was a synthesis, for the first time, of a series of light-fast yellow vat dyestuffs of good properties, which turned out to be very profitable. This again was not fancy chemistry. I knew exactly what I was doing. The Germans had made some colors that had rather surprisingly good light fastness, but relatively low tinctorial strength and not too good wash fastness. They were using a molecule with an azo group, two biphenyl groups on either end of it, and, in the 4,4' position, a carboxylic acid which was converted into an amide with anthraquinone. I looked at that and said, "That's kind of interesting but they ought to make that a completely conjugated system. Let's insert some oxazoles and thiazoles and make a great big, long structure," We hit pay dirt. The tinctorial strength went up by almost ten-fold and the wash fastness was improved. We had a profitable goal in sight! So we had a very rapid force-fed development of these colors of which we put out three or four. The most fun I had on that problem was finding a good synthesis for the azobiphenyldicarboxylic acid, which I did myself while the other chemists were making the colors. That was cooperative fun for all.

Just as that was about ready to go out to the plant, I got thrown into running another division, Miscellaneous Colors. These were phthalocyanine (CPC) pigments plus a bunch of old fields, miscellaneous dyes, that didn't need much research such as basic colors, sulfur colors, thioindigo, indigo. They just needed somebody to keep an eye on them. So we were playing mostly with the CPC Monastral pigments. That was very good for me, because we had just built a new plant to manufacture copper phthalocyanine and the various blue and green chlorinated phthalocyanines. The process put into the plant was based on phthalic anhydride and urea and was not the same one we had been using in our semiworks which was based on phthalonitrile. It wasn't working and the stuff had really hit the fan. The lovely old gentle German Division Head, Otto Stallmann, who was a good man and had been running this division, really wasn't up to the hurly-burly of dealing with a management that was furious because the plant didn't work -- bad pigment was accumulating and customers were screaming. He wasn't responsible for that. It was the engineers who had designed this cheaper new process. They put me in there because they thought I was a little cantankerous and could maybe fight it all out.

FERGUSON: Who was this that you replaced?

SCHROEDER: Otto Stallmann. He was a marvelous man and a good friend. He was very helpful to me as I went into this job. Initially Otto could supply the knowledge that was needed in this field, and I could supply the fight.

FERGUSON: Was this copper phthalocyanine process running at the Chambers Works?

SCHROEDER: Yes. They had been making the dye by the process of baking phthalonitrile with copper chloride. It was a very easy process. It works beautifully and gives a very pure pigment. Basically, it's derived from R. P. Linstead's and ICI's first work (15). To make it cheaper, our process people switched to the process where you cook up copper salt, urea, and phthalic anhydride in a solvent like dichloro- or trichlorobenzene. There are many by-products in this process. You get a pigment that's not as pure, and it's more difficult to purify it to obtain the desired bright shades. The customers were screaming for the beautiful stuff that we had been making by the bake process from phthalonitrile. Thus we had a plant that was pouring out this not-so-good pigment at a great rate. It was a huge plant. We had to find out what was wrong, straighten that process out, develop a better finishing method, and handle the general management who were worried to see so much money pouring out the front door.

I learned a lot about politics, dealing with the plant and over-anxious, almost hysterical, people who want to do things when they don't know anything. I had two years of marvelous experience in what you might call intertribal warfare. Also I got to run a great big semiworks that made copper phthalocyanine and the green chlorinated copper phthalocyanine while the other one was in trouble. We also made a bunch of rubber chemicals and miscellaneous antioxidants and all kinds of stuff. I was responsible for this on the side. All this was good for me, because it gave me a feel for the pressures of a practical world. Incidentally, there is something else that I ought to mention, going back to Central Research. One of the great joys of working on that adhesive problem for the nylon tire cords for bomber tires was the fact that I had complete responsibility for it. When I found something, since it was such a hot project, I didn't pass the development on to somebody else. I just took the development. They gave me a plant. The first thing I made was a rubber derivative. They sent me up to Fairfield, Connecticut, confronted me with a Banbury mixer and told me go ahead, make it. I had made it in a little Banbury mixer that held about two hundred grams and there I had this monster that held a thousand pounds.

I had a bunch of experiences like this. I made this new latex on a laboratory scale. Then I had a great big autoclave and they said, "That's yours! Go ahead and do it." Out of this you learn what it is that works in the laboratory and about which you really don't know enough to do it on a large scale. You have to get data on the physical side of it; rate data, heat data, consistent results, etc. It was marvelously educational. Having had that experience, then running the semiworks was very educational because I was learning what you needed to know to make things work.

FERGUSON: You were involved in some later papers on dyeing rates on Orlon and Dacron (16-18).

SCHROEDER: At the tail end of my two years running the Miscellaneous Dyes Division, at the end of 1950, I was given responsibility for running the project on the dyeing of the synthetic fibers which were coming out -- Orlon polyacrylonitrile and then Dacron polyethylene terephthalate, in addition to nylon. I had an extraordinary opportunity since these fibers were difficult to dye and I had a chance to go about a project the way I like to go about it. That is, find out what's going on before you dive in and synthesize the molecules.

We undertook studies on the mechanisms of dyeing nylon, Orlon and Dacron by various methods to find out, in the physical sense, what really was happening. What's going on as the dye goes into the fiber? What molecular features in the fiber were important, what molecular features in the dye were important, what is the physical process and what are the equilibrium and kinetic processes? When we unsnarled that and knew what kind of dyes we needed to have to get into the fiber then it was a rather easy job to use the best of our knowledge of dyes to synthesize some very fine dyes. It was good fun. I really enjoyed that project because it put together physical chemistry, synthetic chemistry, and polymer chemistry.

FERGUSON: You had Matt [Mathias J.] Schuler. I think a picture of him was shown in the Chemical Engineering News article that you wrote (17).

SCHROEDER: That job also involved Bill [William R.] Remington. That was a classic job they did. The nylon work was by E. K. Gladding and Remington.

FERGUSON: Remington was the physical chemist on this?

SCHROEDER: Bill Remington was an excellent physical-organic chemist. We had him as a group leader working with it. Bill was a Chicago man. He worked with one of the very physical organic men out in Chicago. [George W.] Wheland. Bill was a marvelous chemist. He was a very prickly, difficult person but one of the best chemists I saw at Du Pont. He was just marvelous for going into something and figuring out what was going on. He had just the proper attitude of skepticism and doubt. Bill and Matt Schuler did that job on polyester which was a classic. They showed that the dye dissolved in the amorphous regions of the polyester.

FERGUSON: At the time I came, Sam [Samuel S.] Lord was working on the tristimulus color system. Did that come out of your work in any sense?

SCHROEDER: It came from outside as well as inside with Fred C. Chromey. If you knew what was going on in the general science of color then, it was just a natural thing to go after that. It was latent in the world outside of us and ready to be worked on. We did some fine mechanistic work on the fibers. That includes the work that Remington and [Edward K.] Gladding did on nylon, the work on the various mechanisms of dyeing Orlon that [Robert H.] Blaker and I and others worked on, and the work that Schuler and Remington did on the solution dyeing of polyester fibers.

FERGUSON: What was the state of the dyeing theory at the time that you got into the field? Was it on the physical side?

SCHROEDER: It was in a state of confusion. The understanding was there, but, as in many things which haven't been brought to a conclusion, there were many, many conflicting theories. The only one that was really understood in principle and practical certainty was the fact in the case of wool and silk, you were dyeing with acid or basic colors on the appropriate groups in the wool or silk.

In the case of nylon, there was much confusion. Nylon was dyeable with all kinds of colors largely due to the fact that, besides its chemical end group effects, nylon is one very fine solvent. It has all those amide groups, and it has an extraordinary ability to dissolve things in its amorphous areas. The real mechanism of dyeing a fiber by an approach which involved dissolving in the fiber was very poorly understood. The work done on cellulose acetate was pretty good, but a little sloppy. The corresponding work on nylon was poor and work on Dacron was nonexistent. They were all kinds of theories floating around to the effect that the dyes going into cellulose acetate or nylon or the like, were going on to specific sites, even if they seemed to be dissolving.

Remington's work showed beyond any doubt that basically, what you had was a separatory funnel behavior. You had a partition coefficient. The dyes would go in with accordance to their solubility in each medium. Remington's work just completely destroyed all the fog that floated around this field. To illustrate how thick that fog was, I can take you to about 1955, well after we had done this work. I was invited to a conference in Washington on the connection between molecular structure and biological specificity. It was run by the biological scientists. It included all of these hot rocks, many later Nobel Prize winners, people like Linus Pauling and Lederberg, who were concerned with enzyme interactions, proteins, etc. The reason I was invited was that we had done some relatively definitive work on the relationship of a polymer and another smaller molecule. They wanted me to talk about that.

I got down there and delivered this paper which was on all of this mechanistic stuff pulled together (19). When I got to the part on the distribution coefficient -- partition coefficient between a dye and polyester -- Linus Pauling got up and wanted to start an argument. He said it was preposterous, and there was obviously a site in the polyester. He really gave a ten-minute lecture showing what an ass I was. Fortunately, I had a whole bunch of Remington's slides with me. I proceeded then to leave my talk and I destroyed Pauling's hypothesis. After I got through delivering this rebuttal to Pauling, people got up and cheered. They really liked to see him put in his place. You know what Pauling is like. The only reason I'm focusing on this incident is that it shows you the miasma that surrounded this field. There were too many mechanisms and no clarity.

FERGUSON: Of course Pauling's field was more crystallography, and that sort of thing.

SCHROEDER: But he had gotten into this. Of course, Linus was into everything. I like Linus and admire him greatly. There wasn't anything personal in this, and he knew it.

FERGUSON: Did he hold it against you?

SCHROEDER: No. As a matter of fact, he liked it. After it was all over and we were having a drink later on, he said, "That was good for me." [LAUGHTER]

FERGUSON: Did you ever get into any arguments with Paul Flory?

SCHROEDER: No, none. Paul was at the Chemical Department at Du Pont when I joined there. He left after I had been there for about two months. I barely got to know him. He was right down the hall from me. I got to know him later when he was consulting with Textile Fibers and Central Research Departments. We used to use him once in awhile. He was always very positive, but I'm so ignorant and poor in the areas that Paul is so good, that there wasn't any basis for a conflict. I used Paul pretty much as an encyclopedia, for his magnificent knowledge. He had a very incisive reaction to things.

FERGUSON: When he made up his mind on something he was almost unchangeable.

SCHROEDER: I suppose he may have been wrong but he was unchangeable. I was never one to take him on.

FERGUSON: Let's finish off on dye chemistry. I pictured Orchem as being the hot bed of dye chemistry, maybe one of the best places in the world during that period.

SCHROEDER: It was good, but it wasn't the best. The Germans were probably the best. In the mechanism stuff, we were the best. There was no question about that. I've had people who worked in England, like [Raymond H.] Peters, who wrote a lot of papers in this confused area of dyeing mechanisms for acetate and nylon (20). He came up to me afterwards and said, "You fellows really clarified those mechanisms. Those were classic papers." On that, we were undoubtedly the best, and I ascribe most of that credit to Remington.

FERGUSON: What about the marketing and business strategy of the dye works and Orchem?

SCHROEDER: Abysmal! I loved the marketing people. They were very good to me. They took me out in the field. They treated me in a way that the elastomers people never did. They took me out to the customers when I was a chemist, when I was a group leader, when I was a division head and when I was assistant director of the lab. I got to go to our sales meetings in the different branch offices. From this I really developed a feel for the salesmen, the customers, and their problems. I got enough knowledge so that I could separate the real problems from the hypothetical ones. They helped me greatly in this. These were people like Broze Chandler, Boony Newman, and Gordon Markle. These were all college trained people, but not scientists. They were marvelous people.

The abysmal I would apply to many business practices. There was a feeling that you had to stock all of the dyes whether profitable or not. You had to be able to satisfy any request at any time. They had no concept of inventories and the expense of keeping all of those things. They saddled us with a need to make many colors, so many of which were unprofitable. They'd want a dye just because some small customer in the southeast corner of Pawtucket, Rhode Island needed some to dye a spool of silk yarn. They drove us nuts. This drove the plant crazy, too.

The plant had enough problems of its own. The Chambers Works was a very busy place in those days. It made "zillions" of things. They had some very good people like Harvey Stryker, "Doc" Humphrey, Elton Coles, Paul Wingate and Frank Knowles and also some very poor people, the residue of years of attrition. It was a process whereby the best of people got promoted to other departments and higher jobs. A lot of those who were not the best were left at the Chambers Works. Being left didn't foster their individuality or talent. These poor people had no incentive and they weren't the best anyway. So the performance you got out of many was at best mediocre. They required very tight supervision. Some of the practices this led to were very poor.

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FERGUSON: Would you like to say some things about Louis Fieser?

SCHROEDER: Yes. Louie was a marvelous teacher and good organic chemist. In fact he engendered my own interest in organic chemistry at Harvard. Back in about early 1946, we hired him as a consultant at Jackson Lab. For some of the work that I was doing on vat dyes, which was right up his alley, he was a marvelous consultant. But for many of the people around the lab who were doing things that were really of less interest to Louie, it was difficult for him to relate to them. As a consequence, he wasn't as popular with them as he was with me and with some of the people in the management. Louie was a good friend and to me, remarkably useful.

He got into a little bit of unpleasantness with our Assistant Research Director, Dr. Herbert Lubs, because Herb was then the editor of a book on the chemistry of synthetic dyes and pigments, to which various people in Du Pont were contributing (21). Herb was very proud of this endeavor, which was badly needed. He found out too late that Louie Fieser was also working with a chemist named Venkataraman on a book on the chemistry of dyes (22). Herb always deeply resented the fact that Venkataraman and Louie beat him to the gun. He thought Louie took advantage of him. This was just normal. Louie had been working on the job and, as in these days, competition is the name of the game. He who gets in the first shot is the lucky boy. Anyway, that smoothed over and we had a nice time with Louie.

FERGUSON: Let's get into your polymer career, because we haven't touched on that yet.

SCHROEDER: For a while I had practically nothing to do with polymers after I left the work on rubber chemistry and the rubber adhesive in 1945. In 1950, I got back into polymers indirectly when I was given responsibility for dyeing the new synthetic fibers. That got me again interested in polymer behavior and the physical chemistry of polymers. Having had experience during the wartime when I was working with the tire cord adhesives and isocyanates, I was then given responsibility at Jackson Laboratory for our work on isocyanate chemistry which we had just reopened. It included both the development of a process for difunctional isocyanates, which turned out concentrating on toluene diisocyanate for economic reasons, and for work on the products and uses based on isocyanates. This was about the end of 1950.

I got back into polymers here with a vengeance because one of my chemists, Fred [Frederick B.] Hill, had been given the responsibility for synthesizing some polymers of modest molecular weight as textile adjuvants, things for treating the surfaces of textiles. Hill inadvertently did something that we had been told not to do, and that was work on synthetic rubbers. He didn't really work on them. He just fell into a synthetic rubber. He condensed toluene diisocyanate with polyethylene glycol and made what was the first rubbery polyether-urethane, for which he later got the famous Hill patent (23). We dropped everything else in isocyanate uses and went to work on these polyether-urethanes and developed the product which Du Pont named Adiprene. Our management hoped it would be saleable in tires.

As I mentioned in the article that I wrote for the Goodyear Medal reception (24), it came apart because the product was thermoplastic when subjected to very high rates of abrasion or high heat. We solved that problem when Dexter Pattison developed a sulfur-curable product which really was quite adequate for the intended use as a tire tread, but it was really too late and the product proved too expensive. We abandoned it and were left with the business of polyether-urethanes as liquids for casting resins. This chemistry also led us into the promotion of polyether-urethane foams, in which field we enjoyed a lovely business just selling isocyanates. We never chose to go into the manufacture of the polyalkylene glycols except for polytetramethylene ether glycol, the basis of Adiprene, Lycra Spandex fiber, and the urethane used in Corfam synthetic leather.

FERGUSON: The competition got into polyester-urethane foams?

SCHROEDER: They got into the work that had been done by Adolph Bayer in Germany. It was really the pioneering work on polyester urethanes as linear polymers. They developed a product called Vulcolan and out of that were developed a bunch of polyester urethane foams which have marvelous physical properties but a fatal deficiency, a sensitivity to hydrolysis. So in moist atmospheres or in water, they disappear. We quickly realized we didn't have this particular deficiency and promoted the polyether-urethanes on that basis. That particular property, plus the economy of synthesis of polypropylene ether glycol as a primary raw material for the ether backbone, is why the polyether-urethanes quickly superseded the polyesters in foam uses and why they're so popular in so many casting applications where water hydrolysis is a problem. This work was also stimulated by Charles Price's independent discovery of polypropylene ether-based liquid urethanes and foams (25).

FERGUSON: You got into the thermoplastic segmented polyurethanes later, during your tenure as Research Director?

SCHROEDER: Yes. Our first interest in the polyurethanes was as an Adiprene general purpose gum rubber of rather exceptional characteristics. I think the tires were the result of the lust on the part of our management for a large market opportunity. We would have done much better to pursue the general purpose mechanical goods market for which the products were really useful. In the course of this work, we also started to make segmented polyurethanes which were useful as thermoplastic elastomers. When we alerted our Textile Fibers Department to the properties of the material, they quickly used this chemistry to achieve an objective that they had been trying to get at with segmented polyesters, basically derivatives of polyethylene terephthalate with flexible segments. They made the elastic fiber Lycra, which is a polytetramethylene ether glycol urethane based on diphenylmethylene diisocyanate, the chain extended with something which has been variable over time, amines, hydrazines, etc. I don't know what it is now.

We sought to make a good thermoplastic elastomer at that time and really missed because our products were too thermally unstable under molding conditions. [Charles S.] Schollenberger at Goodrich made the first practical thermoplastic segmented polyurethane, which was announced at about the same time that we announced our Lycra, which is a related thermoplastic fiber. We didn't think that products were good enough so we kept working over the course of time.

The first development we had that was adequate technically was a polyether urethane based on a di-secondary amine, piperazine. The reason for the secondary amine was to eliminate the hydrogen in the polyurethane structure so that the reverse reaction of polyurethane to amine and isocyanate would no longer be possible. This new product was extraordinarily good. It had excellent physical properties and processed well plus hydrolytic and thermal stability, but was too expensive. That product was developed by Bill Witsiepe.

Since that turned out to be too expensive, we moved back in the direction of other segmented polymers that were made by straight condensation polymerization. At first we tried polyamides, and when that failed, we used polyesters. W. K. Witsiepe found the product now marketed as Hytrel, which is a random segmented co-polyester based on polytetramethylene ether glycol and polybutylene terephthalate. The significant discovery that Witsiepe made was that polybutylene terephthalate as a "hard segment" was particularly appropriate because it had a very clean melting point and was able to crystallize very rapidly from the melt, far faster than the polyethylene terephthalate crystalline segment. This really is the practical basis for the success of Hytrel. There was nothing else we did in the way of segmented polymer structures of that sort that was really successful.

FERGUSON: Overall, have the polyurethane-based polymers been a great success?

SCHROEDER: Yes, though Du Pont abandoned the field in the 1980s except for Lycra fiber. Du Pont was really a major factor in getting the polyurethanes launched. The original patent was the [William E.] Hanford and [Donald F.] Holmes discovery made in the late thirties (26). It antedated Bayer's work.

FERGUSON: Hanford and Holmes was a Du Pont patent?

SCHROEDER: Yes. Butch Hanford was a supervisor at the Experimental Station. Don Holmes was the son of Fletcher B. Holmes who was Director of Jackson Laboratory back in the 1920s. Adolph Bayer had made a polymeric urethane by condensing hexamethylene diisocyanate with tetramethylene glycol, as an attempt to duplicate the properties of 6,6-nylon. That proved to be too expensive. Hanford and Holmes made the first segmented polyurethanes by condensation reactions involving difunctional isocyanates and difunctional polymers. Nothing practical came out of that early work except the basic patent.

The man who really discovered the segmented thermoplastic polymers, was Mark D. Snyder in the Textile Fibers Department. He carried out an extraordinarily interesting experiment. When they were attempting to make polyester fiber more dyeable, they put polyethylene glycol in it as a way of making the fiber less crystalline without dropping the melting point too much because melting point depression is a molar relationship, as based on Flory's theory (27).

Mark did something that nobody else had done. He made some polyethylene sebacate, which is basically a fluid polymer and also crystalline polyethylene terephthalate. He put them together in a flask as separate polymers, with an ester interchange catalyst, and watched what happened over time as the reaction proceeded. He took out samples and looked at them. He showed that it went from a fiber, all crystalline mixed with liquid, to a segmented product that had elastic characteristics, and then to something that was completely randomized and soft. He did that around 1950 (28). It was very interesting work.

FERGUSON: I think that some people read the patent literature, and some people read the scientific literature.

SCHROEDER: It's too bad that they don't get married. I think the average academic takes pride in deriding the patent literature as a bunch of junk. But in the polymer field, there's no question that the prime mover has been the industrial scientist making things, which the academics are later trying to explain.

FERGUSON: After the polyurethanes, what was the next polymeric project that you were involved in?

SCHROEDER: There were two. I don't remember which came first. I'll talk about the fluoropolymers first. Coincident with our work on urethane and polychloroprene we learned that the Air Force was rapidly moving in the direction of very high speed jet engines with high temperature requirements. Rubbers were needed for seals, O-rings, etc. but available products, even the silicones, didn't have the required thermal stability together with adequate solvent resistance. M. W. Kellogg Company had approached us for a license to a patent developed in Du Pont's Central Research which was for copolymers of vinylidene fluoride of a leathery character. It was Tom [Thomas A.] Ford's patent (29). Our old friend Butch [W. E.] Hanford, who worked with Ford at Du Pont on fluoropolymers, had moved to M. W. Kellogg so, before approving this request, I asked Bill [William B.] McCormack, who worked for me at the Jackson Laboratories, to examine the patent literature for clues to what Hanford might want this patent for.

As a result of our search, we found information suggesting Hanford was interested in polymers from chlorotrifluoroethylene which might to be rubbery. On learning that I felt we could do much better than that using our knowledge of fluorine chemistry. So I started up the project which led to Viton. Having had the experience during the 1940s at Central Research, when I made the rubbery trifluoromethyl substituted polyethylene (12), one of our prime goals was to look at hexafluoropropylene, which would put a pendant <u>trifluoromethyl</u> group into the vinylidene fluoride copolymers of Ford's. As a result of this, we very quickly discovered Vitons A and B and got the patents on it (30, 31).

FERGUSON: Was that the patent with John Pailthorp?

SCHROEDER: There were two patents here. Rexford's and the one with Pailthorp. We put Dean Rexford to work on the first copolymer. Dean found out how to copolymerize the hexafluoropropylene with vinylidene fluoride, which is a little difficult. So one patent was Rexford's (31). I put J. R. Pailthorp to work on making a more stable product by copolymerizing the two with tetrafluoroethylene as well. The first of these is Viton A and the other is Viton B. Both products, although a little leathery, turned out to be much better in a practical way than we had even expected (30). With the help of the Air Force, which took every scrap we could make, we had to get manufacturing very fast.

FERGUSON: This is fascinating to me, but of course I was there, and I was in it a little bit. Did Du Pont have any basic patents in the polyvinylidene fluoride homopolymer?

SCHROEDER: Yes. In roughly 1937, Roy Plunkett, in pursuing other objectives, made some tetrafluoroethylene and left it in the cylinder. It polymerized and he discovered polytetrafluoroethylene. As they say, luck favors the prepared mind. Roy and his helper, Jack Rebok, had the good sense to take a look into the cylinder that didn't deliver tetrafluoroethylene and discovered the white powder. With natural scientific curiosity, Roy had the good sense -- well you talked to Roy?

FERGUSON: As a matter of fact, Roy gave a nice talk on this at the AAAS meeting last spring. We have his oral history too (32). Jack Rebok thought he was the inventor, didn't he?

SCHROEDER: It's hard to say. There are stories here and I just don't know. Jack was an exceptionally good technician with a very healthy curiosity. It could well be that the curiosity with regard to that cylinder was more Rebok's than Plunkett's. The curiosity with regards to the properties of the dust were Plunkett's. So I would have called it a mix. It's a shame that Jack didn't get on the patent (33). I don't know, because I wasn't there.

FERGUSON: Why would Du Pont never get into making polyvinylidene fluoride?

SCHROEDER: That was over at the Experimental Station and Central Research. At the Central Research Department, [Melvin M.] Brubaker was interested in fluoroolefins. When Plunkett made his discovery, the Organic Chemicals Department chose to keep the development of tetrafluoroethylene polymer under its own wing. Central Research chose to pick up some other fluoroolefins and see what they could do with them. They started programs to synthesize vinylidene fluoride, vinyl fluoride and trifluoroethylene and to polymerize them. These studies didn't go along very fast until a few years later, when Harold W. Elley of the Organic Chemicals Department concluded that his department wasn't really good enough at polymerization to capitalize on the PTFE discovery. They didn't have enough understanding to handle the problem of polytetrafluoroethylene and to work it out.

Elley asked E. K. Bolton of the Central Research Department to take a look at this polytetrafluoroethylene and see if he could straighten out the polymerization so that it would be more replicable. The job was given to Bob [Robert M.] Joyce and Howard Young in Central Research. They worked out the polymerization of tetrafluoroethylene to polymer and also worked out means of making molded articles, basically by sintering (34). Joyce's fine work showed that the difficulties that had been experienced with the fluoroolefins were basically due to the fact that the fluoroolefin polymerizations were extraordinarily sensitive to termination, inhibition, telomerization.

The work basically elevated the standards for preparation and handling of these various fluoroolefins to a higher level. They made much purer monomers. The projects then moved into an area where they were making very pure monomers and polymerizing under very carefully controlled conditions. The job of studying the two monomers, vinyl fluoride and vinylidene fluoride, went to various people in Central Research. The man who did the vinylidene fluoride work was Tom Ford. Tom made vinylidene fluoride homopolymer and also vinylidene fluoride copolymers. He also made a number of other fluoroolefin polymers. In fact, Tom was just knocking out one fluoropolymer after another. It turned out that the properties of polyvinylfluoride and polyvinylidene fluoride were close. The physical properties of polyvinylfluoride were somewhat superior to polyvinylidene fluoride. The other properties were virtually indistinguishable. The products softened at such high temperatures that the question of solubility, the effect of the other fluorine, doesn't really come in. The very unusual electrical and electronic properties of polyvinylidene fluoride were never noticed. Nobody ever measured things like that then.

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SCHROEDER: Vinyl fluoride (VF) of is made from one mol of hydrogen fluoride and one of acetylene. Vinylidene fluoride (VF2) was then made by addition of two molecules of hydrogen fluoride to acetylene, then chlorination of the difluoroethane, and pyrolysis for elimination of hydrogen chloride. So it's obviously a lot more expensive than vinyl fluoride. Having seen that the physical properties of polyvinylfluoride were so good, the Chemical Department naturally chose to concentrate on vinyl fluoride.

The concentration on the properties of vinyl fluoride were almost coincident with the start of the war. It was quite impossible to maintain both. In a normal course of events, they probably would have pursued both. Because of the unusual properties of polyvinyl fluoride they were able to get a draft clearance for the chemist working on vinyl fluorine (VF) who was Fred Johnston. He was concerned with development of it. I'm not sure who first found it. It might have been Tom Ford. They concentrated on trying to make both moldable plastic and films and fibers. It wasn't realized until we were well into the development that polyvinyl fluoride (PVF) had a fatal flaw as a plastic. The temperature at which it molded cleanly was so close to the temperature of exothermic decomposition that very frequently, during molding of a polyvinyl fluoride plastic, the polymer would explode. We never did solve this problem with stabilizers of any sort. Then there were a number of other fluoropolymers of interest such as the TFE/olefin copolymers.

FERGUSON: They did get Tedlar film?

SCHROEDER: That came later. I'll finish off with this war part first. We gave up a number of other very interesting polymers discovered in the same period. John R. Roland discovered that the ethylene/tetrafluoroethylene copolymer was a fine plastic and that copolymers of propylene and tetrafluoroethylene were elastomers. I discovered that copolymers of ethylene and trifluoropropene were elastomeric. These were very interesting polymers but, with the war and the inability to get people away from wartime projects, we had to abandon them. Polyvinylfluoride for film we picked up during the 1950s at the time when I was Assistant Director of Jackson Lab. I picked up responsibility for fluorine research in Jackson Lab. We were looking around for new large outlets for our capability in fluorine chemistry other than propellants and refrigerants. We chose to take a look at polymerizable intermediates, which immediately brought up vinylidene fluoride and vinyl fluoride. When this request came from Hanford relative to giving us a license, we were singularly alert because we were already looking for things ourselves. It just didn't catch us fallow. We were really ready.

Also by luck it had been found during the course of vinyl fluoride development that polyvinyl fluoride and polyvinylidene fluoride, when made into paints, were extraordinarily lightfast. This was an accident because over the war period, some panels which had been sent down to Hialeah in Florida had been forgotten. These panels had been sitting down there in Florida for almost ten years when somebody at the paint farm said, "Don't you want these panels?" They took these panels back and looked at them. Where most of the finishes used in the paints had chalked or embrittled, there were these polyvinylidene fluoride and polyvinyl fluoride finishes in pristine condition. There was nothing wrong with them.

FERGUSON: These were clear-coat finishes?

SCHROEDER: Clear coat and also pigmented -- just perfect. The reason for this is that both of these polymers are transparent to ultraviolet light. So the natural processes that generate radicals and decompose polymers don't occur. You don't get any energy where it counts. Then on top of that, there's no hydrogen in either polymer that's really hot, in the sense of being easily detachable by a free radical. Efforts began in our Central Research Department and in our Plastics Department to see if something could be done with these polymers. The final outcome of this was that the film started to look quite interesting. By now a Film Department had been formed, and they were looking for some films other than cellophane, on which their business had been formed, and polyethylene terephthalate which was moving slowly as the Mylar development. They were looking for some new films. As an ombudsman for fluorochemistry, I started promoting polyvinyl-fluoride around the company and got Film more than interested. In fact, we spontaneously undertook research work on our own on both of these products (VF and VF2) to get Film and also F & F [Fabrics and Finishes Department] more interested.

All of this ferment helped the development of Tedlar. It was a cooperative thing where they were looking for something new and we were pushing, trying to promote fluoromonomers for sale.

FERGUSON: Tedlar is still on the market?

SCHROEDER: Yes.

FERGUSON: Is it a successful product?

SCHROEDER: Yes.

FERGUSON: Did we license Pennwalt to the polyvinylidene fluoride film?

SCHROEDER: Yes, but the patent might have expired them. We licensed Kellogg to the Ford patent too. It would have been a dog in the manger kind of thing not to. It wouldn't have been good practice, especially when what they were trying to do was intended for an important military use. So we licensed and they developed Fluorel later. Then 3M bought that business from Kellogg.

FERGUSON: Let's now get into the Nordel polyolefins.

SCHROEDER: In the early 1950s, we were manufacturing polychloroprene, and looking at polyurethanes. Exxon had a product that annoyed us. That was butyl rubber. We thought a better hydrocarbon polymer could be made than butyl rubber. I asked Madison Hunt to put somebody on making a curable hydrocarbon polymer other than polyisobutylene, and incorporating a pendant rather than an enchained double bond. The only monomer that looked as though it had a chance was propylene, because it had been polymerized ionically. So we undertook a study. I think Al [Alfred C.] Haven was given the job. We started a study of copolymers of propylene with non-conjugated diolefins, trying to figure out how we could get the molecular weight up and how we could get a side chain unsaturated monomer in there.

Within a month or so after we got going coordination polymerization chemistry came in. Quite independent of Ziegler, Frank [William F.] Gresham and his group in the Polychemicals Department discovered coordination polymerization and its ability to polymerize olefins. So instead of working any further with the ionic propylene copolymers which are difficult, we quickly switched over to Gresham's coordination polymerization. Al Haven started making copolymers of ethylene and propylene with the long side-chain monomer we had devised for the other work. I think it was a methylheptadiene. This was spectacularly successful and we very quickly had a nice looking rubber. This became the first subject of the Gresham and Hunt patents on olefin terpolymers containing an unconjugated aliphatic diene (35). It was very hard in all this melange to figure out who really was the inventor. So we just took the representatives of the two groups, Gresham and Hunt, because there was an awful lot going on in Polychemicals and an awful lot going on in our laboratory. You have to be practical.

The polymer research was going so well that we soon decided build a small pilot plant. Our little pilot plant had controlled monomer and catalyst feeds and associated equipment. Since we were in the Hypalon business by then, we naturally felt it would be interesting to chlorosulfonate some of the ethylene-propylene polymers themselves instead of polyethylene to make a new Hypalon. Actually this yielded a beautiful elastomer. This analog of Hypalon was so much more like a rubber than Hypalon that it produced a very enthusiastic reaction on the part of our management in Wilmington. The result was that they forced us to stop the work we had been doing on the new hydrocarbon copolymer to concentrate on the new Hypalon analog.

FERGUSON: Let's go back and talk a bit about what Hypalon is and what its advantages were.

SCHROEDER: Hypalon synthetic elastomer was discovered by Ambrose McAlvey in the old Ammonia Department. His boss, Roger Williams, who later became the Research Vice President for Du Pont, wanted to make as many plastics as he possibly could and was jealous of the fact that the Organic Chemicals Department had Neoprene synthetic rubber. So he asked McAlvey, through appropriate channels, to make him a rubber. They fingered polyethylene as a basis because Central Research some years earlier had chlorosulfonated polyethylene using the Reid reaction, chlorine and SO_2 with light, and made a pretty good flexible leather substitute. So, Ambrose decided that he would take on polyethylene and this chlorosulfonation reaction first. When he chlorinated and chlorosulfonated polyethylene he made a product which contained about 25% chlorine and 1% of the SO_2Cl group, which turned out to be a not bad rubber, with excellent ozone resistance and light resistance; so they built a pilot plant. Unfortunately this Hypalon S-2 was a terrible rubber in terms of strength and particularly in its processing characteristics since it became very sticky and had little green strength.

The new chlorosulfonated ethylene-propylene analog that we made got around these difficulties. We were able to get much more chlorine into it so that the solvent resistance was better. The rubbery characteristics were better and the viscosity was higher. It processed more like an elastomer and less like a soft, soupy, melted plastic. In accordance with the orders from on high, we took off very quickly after this. We found that this beautiful rubber wasn't thermally stable because of the propylene content. It had too many tertiary chlorines and thus was subject to dehydrochlorination and subsequent oxidation with cleavage on long term exposure.

We recognized that this instability was caused by the substitution on the tertiary carbon. So we started gradually backing the propylene out of the structure and ended up with chlorosulfonated linear polyethylene -- a fine product which we soon introduced as Hypalon 4D. It processed well and with 35% chlorine had solvent resistance like Neoprene. I think that patent belongs to Paul R. Johnson and Mal [Malcolm A.] Smook (36). Mal is a good one to talk to if you want history on Hypalon. He was on the scene in Polychemicals and then with us when this happened. He was the one who told me about Williams telling McAlvey.

FERGUSON: This is a picture of more cooperation than I felt there was between the departments.

SCHROEDER: There isn't quite as much cooperation as you think here. The Ammonia Department (or it may have become the Polychemicals Department then) tried very hard, with Williams, to get a hold of this business and to build a plant to manufacture Hypalon. The entrenched forces of conservatism at Orchem, with a very nice Neoprene business by now, fought very hard against this brash newcomer who claimed Hypalon had all Neoprene's properties and heaven too. This led to an Executive Committee confrontation. As a result of this confrontation, Hypalon was taken away from Polychemicals and given to Organic Chemicals Department. So one department was robbed of something it was proud of, and the other department had something shoved at it that it didn't really want. Hypalon wasn't regarded with pleasure by the Organic Chemicals Department. My boss, Nick [Albert S.] Carter, a Neoprene pioneer, thought it was poison. In fact, he thought it was such poison that he looked at it and said, "Herman, you take it. I don't want to have anything to do with this. You assume responsibility for it." He was proud of Neoprene in which he had played a big part.

FERGUSON: My feeling was that some opportunities were missed over the years because the Plastics Department was only interested in something that was plastic.

SCHROEDER: They were. There's no question that you're right.

FERGUSON: The thermoplastic business probably came later than it should have.

SCHROEDER: I couldn't agree more. There were many examples of things that came up through the 1960s and 1970s, where we would have ideas in our department and I would have to veto them, as far as any extended work goes, solely on the basis that they weren't our business and were Plastics' concern or vice versa. Or I'd tell the fellows to do the work quietly and not to tell me about, because it was really doing something that our department couldn't be in. At the same time, the other department was singularly unsuggestable about the new ideas. You're dead right.

FERGUSON: Is there something more to be said about Nordel?

SCHROEDER: Well, there's a lot to be said about Nordel. You could spend hours talking about that development.

FERGUSON: Exxon came in with their dicyclopentadiene (DCPD) terpolymer. That was one we hadn't covered?

SCHROEDER: This is very complex. Du Pont made the first curable rubbery polymers with linear diolefins, hexadiene, heptadiene, things like that. They got the basic patent (35). Stamatoff in the Polychemicals Department copolymerized dicyclopentadiene and norbornenes with ethylene. Herb [Herbert S.] Eleuterio discovered the olefin metathesis catalysts and polymerized those monomers and made the linear homopolymers of the cyclic olefins, like polypentenamer. The basic patents on those were Herb's (37). Following Stamatoff's work, we put a man to work on norbornene and dicyclopentadiene E/P copolymers. That was C. Wheaton Vaughan. He didn't really like the project. He just simulated some experiments and never did them. He told us they didn't work. A year or so later, I asked [Edward K.] Gladding to go back and take a look at that stuff. We polymerized ethylene and propylene with monomers like methylene norbornene, vinyl norbornene, norbornadiene and cyclopentadiene itself. These all worked and we have patents. There's a Gladding and Robinson, a Gladding and somebody else, and I have one (38).

We got patents on all of these combinations, but not dicyclopentadiene, because we had Vaughan's work that said it didn't work. In the middle of this, a foreign "early disclosure" patent came out to a couple of people who had worked in the Dunlop of Canada laboratory (39). I forget the names of all of them but one was Wilfred Cooper and the other had a Russian or Slavic name. They did a very nice piece of work on this and made some pretty good-looking dicyclopentadiene rubbers. We took a look at those and decided that they really weren't as good as the kind of product that we were making, so we chose not to go any further with them. But we also did enough to know that some of those other norbornene copolymers were good.

About 1960 or 1961, we built a pilot plant to make our ethylene/propylene/hexadiene terpolymer, which we called ECD-326, and gave several papers on it. Papers were given at the Gordon Conference and also at an ACS meeting. There were some papers by John J. Verbanc and some papers by Gladding (40). This development elicited great reactions of envy in the rubber industry. Exxon came in to see us and said that they would like to make one of them. They wanted to make the one that we were making but we said, "No, but we would be glad to license you to one of the other ones." So we licensed Exxon to the methylene norbornene copolymer patent which was very good. They were satisfied. We were in a position where refusing Exxon a license was very difficult, because they were one of the company's largest customers for tetraethyl lead. And we had excellent relationships with them. Earlier we had had a semi-partnership with them (Ethyl Corporation) on the manufacture of tetraethyl lead which had been dissolved many years before. They were responsible for sales and we were responsible for manufacture. It was an awful lot of business, plus good industrial relations, and refusing was almost impossible. So we gave them a license.

From Du Pont's standpoint, there was a very sad, almost comical occurrence. Gladding's chemists did the work on the bridged-ring strained olefins like ethylidene norbornene or methylene norbornene. We patented them all individually because of the Dunlop case mentioned above on dicyclopentadiene. Since we couldn't patent the whole class, we had to patent the individuals. Among those was a patent application on ethylidene norbornene and methylene norbornene. Gladding was the Division Head. The responsible patent lawyer was named Walter Steinbauer, who was not too sharp. Unbeknownst to me, the case that we filed was not the case that I had approved. Steinbauer persuaded Gladding that he could not cover ethylidene norbornene and methylene norbornene in the same case. Since Gladding had more information on methylene norbornene, the ethylidene norbornene was removed from the case. I never knew this until the damn patent was issued.

As a result of this, the most important monomer outside of the hexadiene type, and the one that's used by all of our competitors, was patent-free. The disclosure was in the patent, but there was no case.

FERGUSON: That may have been the tale that I heard.

SCHROEDER: We soon fired Steinbauer. He later went to work for 3M and then they fired him. He was an awful nice guy but highly incompetent. I don't know how much money he cost us. I don't know if you want that story.

FERGUSON: I'm delighted by all of these stories.

SCHROEDER: I want to add something on Viton successors. We discovered Viton and made many improvements on working with it over the course of years. It became quite apparent that as good as it was, it wasn't good enough for many of the uses that really were hitting us in an age where things were going off into space or going down five miles into the ground, where the temperatures and conditions were extreme. Starting as early as the late 1950s, we really went to work trying to create a better elastomer and pretty well run out of the normal possibilities outside of the fluorocarbon field. We exhausted the possibilities of exotic fluorocompounds and decided that we really had to get back to home territory and take a look at TFE copolymers and find out how to get them more flexible. Due to some excellent work in the Plastics Department in Gresham's group by Art [Arthur W.] Anderson, Herb Eleuterio, and others, the peroxidation of TFE to tetrafluoroethylene oxide and hexafluoropropylene to its corresponding oxide were discovered. This in turn led to some interesting chemistry in the synthesis of the perfluorovinyl ethers.

[END OF TAPE, SIDE 6]

SCHROEDER: Our friends in the Plastics Department found that if they copolymerized one of these perfluoroethers with tetrafluoroethylene (TFE) it gave them a good, stable, processable plastic. When we heard that we asked for the monomer and to see if we could make a decent elastomer, and they cooperated. [Frank C.] McGrew got me some perforce. He was very good about it. We just pursued that line and managed to copolymerize perfluoromethyl vinyl ether with TFE. It turned out to be the best of those ethers. It made a good looking rubber -- very stable -- that was utterly unvulcanizable. Then by dint of blood, sweat, and tears, and many years work, we finally found co-monomers that we could copolymerize that were adequate for curing purposes. The best was one that Dexter Pattison found. There were many that worked, but that was the best. It was a perfluorophenoxy-perfluoropropyl-perfluorovinyl ether. Not too good a name, but you get the idea. Using that, we were able to make vulcanizates which had stability all the way from the point at which they had flexibility, about -10°C, up to the point of their thermal decomposition, which was just about the same as Teflon, around 325°C.

The real problem we had here was not the rubber itself, but how to develop it. It was so expensive and hard to handle that we had to take the unusual course of deciding to market it ourselves as finished articles. The major market that we thought we had disappeared when Congress refused to support the SST any further. So we floated a little business in the laboratory. That was fun. Other than starting the project, my contribution was perceiving that the only way we could make money out of this was to make the articles ourselves so that we could sell on a value-in-use basis. It worked.

It's a fantastic rubber and led to a nice small business. The departmental vice president laughed at it but God bless my friend Charles Harrington. He said, "Let him do what he wants. Give him the money." David W. Holmes in Sales was furious. The sales people who were out in the field were furious, because we were competing with their customers but, they let us get started. Dave Holmes even gave us people like Ralph Graff to help us pull it together.

FERGUSON: You weren't really competing against conventional Viton customer.

SCHROEDER: Our customers thought we were and our salespeople thought we were. But this product was so much better than Viton that it wasn't. The real point was that our customers wanted to make all of the money. If there was any juice in this for us it had to be in selling the finished articles. There was no way we could justify all of the research and manufacturing expense to sell what would be, at the most, a few thousand pounds of material. You couldn't charge enough for it.   We could get maybe $100-300 per pound for it by selling it as a raw polymer. It had a value-in-use down in an oil well or in an SST of anywhere from $1000 to $10,000 dollars per pound. The following will make it very clear. Sixty thousand dollars worth of material saved them fifty million dollars in retrofitting the Titan rockets. We sold the Armed Services $60,000 worth of Kalrez.

FERGUSON: Is that right? That came back to you with information about how they used it?

SCHROEDER: Oh, yes, we knew where it was to go and, even told them how to process it for O-rings and seals. We knew they were using it for seals where Viton had failed. We had a special deal with the government where we would sell them the polymer for a reasonable price and let them use it through a defense contractor whom we helped. All the other applications we chose to go after ourselves. It was really funny to see how quickly the front office attitude changed when in the third or fourth year into this development, we made a million dollars profit out at the lab.

FERGUSON: Maybe that explained the change of philosophy that began to seep into Central Research at about that time. We were criticized for not having gotten involved enough in the marketplace and actually dealing with end-use customers.

That's probably part of your study committee's review of Central Research.

SCHROEDER: Yes, it could be. One very strong influential factor in Central Research that should have been even stronger was Monroe Sadler. In Central Research there was always a conflict between the theoretical scientist, [Theodore L.] Cairns, and the very savvy, practical guy, Sadler. Some things did happen partly as a result of this study committee. One of our recommendations was for the Development Department to get back to work on the Central Research Department to help them. In due course, they did merge the appropriate portions and put Sadler and his men back in Central Research. But I just don't know. The forces that produce change are hard to analyze. They're so complicated. You have to give Monroe an awful lot of credit for this.

FERGUSON: We'll pause here and set a later date to continue with this interview.

INTERVIEWEE: Herman E. Schroeder

INTERVIEWER: Raymond C. Ferguson

LOCATION: Greenville, Delaware

DATE: 12 January 1987

FERGUSON: We've discussed Adiprene, Hytrel, Viton, Nordel and Kalrez. I didn't say anything specifically about Vamac. Do you want to add anything about that?

SCHROEDER: I think most of what you need on Vamac you'll find in that Goodyear paper (24). It's a short summary. We had made the discovery that you could copolymerize ethylene with functional monomers like acrylic esters and acrylonitrile in the presence of a Lewis acid. We picked fluoroboric acid because it was recoverable by distillation. This gave us a beautiful alternating copolymer which had a combination of reasonable solvent resistance and good temperature and oxidation resistance. It was sort of a carefully engineered compromise to meet a perceived market gap.

We were quite enthusiastic about it until we ran some cost calculations. I had been worried about the complicated process. To make that product, we would have had to invest about eighty million dollars in plants to make fifty million pounds. There was no way we were going to make a gamble like that on that kind of a product -- considering that it had to be of moderate price. I remember that I then exerted what you might call executive privilege. I told the men to knock it off. "Let's find a practical way to make this polymer rather than make such a fancy thing. Let's go make it down in one of the Texas plants where they know how to copolymerize ethylene, even if it isn't so perfect." That produced wails from my colleagues Burt [Burton C.] Anderson and Rudy [Rudolph] Pariser, but faced with a lack of funds, they had to do it. In the high pressure reactors of Sabine works, we very quickly found Vamac which was practically as good as the beautiful, alternating polymer. It just wasn't as nice scientifically.

FERGUSON: You brought up a point that I have been thinking about. It seemed to me that over the years I saw lots of cases where it was extremely hard to terminate a research project even though it wasn't going anywhere.

SCHROEDER: Oh, it's terrible. It's difficult for everyone.

FERGUSON: Who finally has to decide this?

SCHROEDER: That depends: management, research management, even the chemist; it varies. If I had the good sense to see it, I usually had to do it myself, because it's very hard to separate a scientist from his child. You can understand that.

FERGUSON: Oh, yes.

SCHROEDER: I stopped a number of them. Pete [E. Peter] Goffinet was pursuing two very interesting processes for making Nordel. One was a new solution process with evaporative cooling, which we now use. The other was potentially cheaper but much more of a gamble, a suspension process. We just didn't have the money, energy or people to pursue both on the scale that would ensure getting the thing done. I told Pete that we had to pick one of them. He thrashed and fumed and kicked. Finally, when I couldn't get him to make up his mind, I brought him into the office and said, "Pete, pick a process. If you don't pick it, I will!" Then he picked it. You have to do things like that.

FERGUSON: Was that the slurry process?

SCHROEDER: Yes, the slurry process was the one that we killed. We just didn't have enough manpower or the money to do both. It's like going to the store and wanting to buy two suits when you have only money for one. There were a lot of cases like that.

There was a another fascinating one with Rudy Pariser, which just shows the psychology of the situation. There was a project he was pursuing that obviously was going around in circles. I won't identify the chemist because it's immaterial. But the fellow was on the head of a pin and he wasn't going to get off of it. Rudy loved the project but I said, "Rudy, we have to stop it. It's not getting us anywhere." After much "Sturm und Dräng" I told Rudy that we were going to knock out the funds. He said, "Okay, we'll stop it." Then I did something that was inspired. I said, "Rudy, write me a little letter summarizing this discussion." So Rudy wrote me the letter and we theoretically stopped the project. I went back to my office. Three months later I was out there again and there was this damn project going along at full steam and getting nowhere at the same rate. So I said, "Rudy, we agreed to stop that one." He said, "No we didn't. You approved it last time." So I had my files with me and whipped this letter out and showed it to him. He looked at it and was absolutely stunned and said, "I never wrote it." It just shows you how you get fixed on these things.

FERGUSON: I can remember that Rudy got extremely frustrated one time about Chestnut Run never making up their mind about testing a product candidate. Does that ring a bell with you?

SCHROEDER: It was often difficult to get them to accept a new product. The real reason is in their psychology. They test products that we market and decide whether they are good enough. They also look at external products and try to compare them with ours. Their general approach to things is to try to show that competitive products aren't adequate. That's just the way they're geared -- to try to make the plant make better stuff, or try to show the outside competitor is not as good as the old homemade stuff. When you send them something from the laboratory, the way they go at it is, "What's wrong with it?" When they're faced with a situation where the product is different but better, it presents them with an agonizing decision so they can't make up their minds.

FERGUSON: You have a patent on a cordless vehicle tire (41). Did this patent ever go anywhere?

SCHROEDER: No. Actually, there were two of them (42). One on Hytrel and one on urethanes, each with a Nordel tread. In each case, those were defensive patents to protect ourselves. We were engaged in a couple of secrecy agreements with other companies, where they were pursuing technologies using our products. These were thrown into the pot as ways of being sure that specific coverage would be devoted to the use of our products as we wanted them to be used in tires. The company with whom we were engaged in the secrecy agreement would have had free access to these patents in any case. It was just defensive.

FERGUSON: Would you like to talk about your consulting career?

SCHROEDER: I'd like to talk about the research organization and the philosophy of running research a bit. We can do that after or later.

FERGUSON: Let's do it now.

SCHROEDER: As I ambled through the Chemical, now called Central Research, Department, I saw the way research was run, some of which I didn't like. It was too empirical and too imitative of hot stuff that was going on outside. Then I saw the way I was handled in Jackson Laboratory, and what worked and what didn't work. When I got to Jackson Laboratory I was left alone. They just told me to do what I want, pretty much, as long as I took care of the project. That was very good. What I developed was a strong conviction that to run a research organization the looser you can leave the reins, the better. Of course you have to control things to be sure that the train goes in the direction that you want it to go. It depends upon the character of all the individuals who are doing the work. You just forget about the really able ones, you direct the ones who need direction, and you get rid of the ones who need too damn much direction. You have to because you don't have the time to spend with them.

As a result of this, when we set up our new department, the Elastomer Chemicals Department, we tried to set things up so there was a minimum number of layers between the chemist doing the work and us in Wilmington, so that we had a chance to listen directly to what the research man thought he was going to accomplish. I had been in a world where my bosses were always making the promises about what I was going to accomplish. This often had no connection with reality. They didn't understand the science or they didn't really understand what you could accomplish in a given time.

We set up an organization where the chemists were writing down what they could accomplish and the division heads could say whatever they wanted to say separately. But, we had on paper what the chemist thought *he* could do. This, of course, caused the supervisors to gnash their teeth, because they lost some kind of control that they wanted, but it gave us a much better feel for what we were going to accomplish. It stimulated the scientists because they had a hell of a lot more personal responsibility for what they were doing. On top of that, we really encouraged them to spend a significant amount of their time in freewheeling, trying to find new things to do, and really getting to think.

We also set up the maximum number of contacts, reviews and the like with the scientists that we could tolerate, so that they again had the chance to talk about what they were doing, and we could talk to them directly. We did all we could to stimulate them to accomplishment by getting them to publish and trying to get them to patent things. This is why I set up those special lunches for the men who published each year. I had a similar thing for patents. Since patents were really what we were supposed to get, I set up a fancy dinner for those who got patents each year and got the top management people in and said, "Look, these are our heroes. These are the fellows who got us the patents." I used to tell them about the people who did the publishing. The result of all this is a much greater feeling of esprit de corps, interaction between the scientists and the management and feelings of great participation. So we had hellishly good morale compared to most of the other research divisions.

The other effect of this is that we were extraordinarily productive. By any count that the people in the patent-legal part of the company kept, we were knocking out patents per man at a rate of three to four times that of most of the other research divisions. I'm violently against the kind of regimented society that is presently in place at Du Pont, (except possibly in Central Research), where all of your goals are so carefully set that each fellow has no time [for his own initiatives].

FERGUSON: Even there there's an element of research-by-objective. It's interesting that you sense that in the same way that I saw it developing in the 1970s and 1980s. The Plastics Department seemed to be the role model for the company, rather than the Elastomers. I think that was beginning to happen perhaps even before you retired. In my view, they weren't contributing as much in the way of new innovations.

SCHROEDER: No, they weren't at all.

FERGUSON: I think a lot of what happened at the Du Pont Plastics Department may have been competitive pressures. It was a commodities type of environment.

SCHROEDER: Actually, it's quite complex. It's partly in the character of their management and partly it comes from historical development. A conflict grew up in Plastics after John Beekley retired as Research Director and Frank McGrew became Research Director. McGrew was a very capable man but he was very arrogant. He did what he damn well pleased, and he completely ignored his management. He made all kinds of promises that his organization couldn't come through with. He overblew his accomplishments and he overspent his budget prodigiously. So it was a case of excess. He got into a bit of a to-do with Roy Schuyler when Roy was head of one of the businesses. He got to treating Roy like a pile of dirt. All of a sudden, one morning, Roy was his boss. Within a week McGrew was on his way to Geneva. They trumped up a job for him.

This built up a kind of spirit in that department to kick the hell out of the Research Division. I'm sorry to tell you all of this but it gives you a feel for the atmosphere.

FERGUSON: No. I always knew there were office politics and personality conflicts. How about some of the research leaders of other departments? Did you know Hale Charch well?

SCHROEDER: I knew Hale pretty well. He was a very able fellow. Hale is going to be covered in good detail in that book that Hounshell is putting out (6). It's silly to waste time on it because he's more than covered in there.

What you have with the research directors is the same sort of thing that you get with any group of people. If you get enough, there are good ones and poor ones, and you get some semblance of a distribution.

FERGUSON: Do you want to talk about some of your Du Pont colleagues?

[END OF TAPE, SIDE 7]

FERGUSON: I have some people that I happen to know that I wanted to ask you about, but it should really be your choice of who is important.

SCHROEDER: Go ahead. It's all right.

FERGUSON: [Nick] Carter.

SCHROEDER: Carter was a close personal friend of mine. He was very good, solid, well-organized, not an imaginative kind of fellow, but very capable, and had very good judgment.

FERGUSON: Wasn't he was in the Neoprene development program?

SCHROEDER: Yes. Nick was a fine chemist. He was responsible for the development of the synthesis of monovinyl acetylene which Father [Julius A.] Nieuwland had never had. In a sense, Dr. Arnold Collins snatched Neoprene right out from under Carter's nose. Nick was fiddling around with the chemistry of monovinyl acetylene. It may have been Elmer Bolton's responsibility, telling Carothers and Collins to go after it. But Nick was a good chemist, extremely well-organized, very effective and imbued with the same general philosophy that I tried to expound, getting a maximum done by the individual chemist, hopefully with the minimum direction.

FERGUSON: Harold Elley.

SCHROEDER: Elley was our research director when I worked in Orchem from 1945 to 1956 when he retired, and he had been director for awhile before that. He was very highly respected by his colleagues and very popular with his people. Harold was a gentle soul. I don't know if he was a good chemist or not. I did believe that he was a bit too subservient to his management. I never had any feeling for Harold from the technical sense. His practical sense was pretty good. He was unusually able to listen to his people and then to come up with a rather excellent, balanced judgment.

Very typical of Harold is something he did in connection with the Teflon development. Teflon had been discovered by Roy Plunkett and the people at Jackson Laboratory set out to try to get control of the polymerization that had occurred spontaneously for Plunkett. They had no experience with that kind of polymerization or anything other than a polychloroprene or analogous polymerization, and no conception of how sensitive the polymerization of a fluoroolefin was to chain terminators and all kinds of other things.

But in any case, despite much work they never got anywhere. Harold took a look at that situation after about a year and a half, and he saw what was going on. On his own, he decided he was going to ask Central Research to take on the project. He went over to Elmer Bolton and told him that he wanted him to see what he could make of it. His judgment said, "The hell with my pride, let's do this right."

FERGUSON: That's not really the territorial imperative that you see so often.

SCHROEDER: No. Harold's a good man.

FERGUSON: George Holbrook.

SCHROEDER: George was a very unusual man -- and highly individualistic. George was bright and vigorous. He had a Ph.D. in chemical engineering and was technically quite able, but George was a man of different ideas. George was one who would see in a situation things that other people didn't see. He was a highly original character, very good in handling people, a star sort of character. I think he was a bit frustrated when he got up on the Executive Committee because George was a nonconformist and he was surrounded by a pack of conformists.

FERGUSON: Maybe you heard Holbrook's comment to Rudy Pariser. Holbrook said that he had expected that as he rose in the company, he would find that people were smarter, and that proved not to be true.

SCHROEDER: No. You've got the same distribution all the way up. That's what I was trying to imply when I said that about the research directors. It applies to people on the committee, vice presidents, etc.

FERGUSON: There were some real personalities at Jackson

Laboratory and I'd like to know if you have a comment on them. They may not be significant people. What about Fritjof Zwilgmeyer?

SCHROEDER: Fritjof was extraordinary and I don't think it's appropriate to comment. Fritjof was a seething mass of ideas. He didn't have an idea of what was good or bad, but a seething mass of ideas, and a hell of a lot of fun to work with.

FERGUSON: Was Vic [Viktor] Weinmayr in the same category?

SCHROEDER: Nowhere near as many ideas but there were a lot, and a very capable man. Vic had nimble fingers. He could make things happen and accomplished a great deal.

FERGUSON: Do you have any other colleagues that you would like to mention?

SCHROEDER: I'll come back to that. Let me think that over. You want to talk about consultants. We talked a little about Fieser, didn't we?

FERGUSON: Yes. We also discussed John D. Roberts but we didn't get that on tape.

SCHROEDER: I had hired Roberts for us because I was unable to get Paul Bartlett because Paul had a commitment with PPG that he thought was a manner of honor. He suggested Roberts so I went up to MIT, talked with Roberts and got him to visit us. Roberts was a very interesting consultant and an extremely good man, very bright. He was good for the management because they were more used to taking the kind of character that Jack was, which was sort of abrasive. He was very difficult with the scientists, because he had a bit of a tendency to try to put people down. Unless you were a vigorous infighter, you didn't like Jack. I like Jack very much, but you had to be hardened and calloused and tough to handle that guy.

FERGUSON: The first time I encountered him, I think if Bill Remington hadn't been in the room, I'd have been stomped on and sent down the drain because he was very blunt.

SCHROEDER: He's ferocious.

FERGUSON: How about Melvin Calvin?

SCHROEDER: Oh, Calvin was magnificent. Calvin was one of my very favorite consultants. I loved him, and the people loved him. Calvin had the ability to stimulate people, to talk with them about their problems, to evoke from them the ideas they had, and to get them to think. In most cases, they would solve their own problems. He greeted what he got from them with enthusiasm even if it was junk. Then he would gently straighten it out. So he was extremely popular. We could have used four times as much time as Calvin would ever give us. The only reason the consultantship was terminated was because Monsanto put him on their board of directors and he had to withdraw.

FERGUSON: I see. I understood that he had got terribly busy.

SCHROEDER: Yes. It was a direct conflict.

FERGUSON: Did you have anything to do with hiring "Buck" [Lockhart B.] Rogers?

SCHROEDER: No. [David N.] Hume and Rogers were both from MIT, and I had nothing to do with hiring them. They were both interesting consultants. Rogers was by far the better. I'm not sure that he was better scientifically, but in the interrelationship with people in the Analytical Division and elsewhere, Rogers was ten times as good as Hume.

FERGUSON: I agree with that entirely. He was inspiring and extremely competent.

SCHROEDER: It got to the point where Rogers and Hume got split. We unfortunately rolled a seven and got Hume, which in due course, we had to terminate.

FERGUSON: I think we mentioned Wilfried Heller earlier.

SCHROEDER: Carter hired Heller.

FERGUSON: You might say something about him. He's not as well-known scientifically.

SCHROEDER: You might say that Heller was limited in scope but really good at somethings. Heller was a fine consultant for specific problems that dealt with polymerization, the behavior of dispersions, the physical chemistry of polymerization. As far as I know those people who needed him liked him very much.

FERGUSON: I think Fred [Frederick T.] Wall was one of the most impressive consultants that I had.

SCHROEDER: I used Fred a little bit when I was in Central Research. Not much, but I liked Fred. Of the consultants they had at Central Research, one I used was Roger Adams. Roger used to come and see me because I was one of the few who was practicing the sort of organic chemistry that Roger liked. So he would always come around whether I asked for him or not. We always enjoyed it. I always liked him. Roger was magnificent. You don't need any more on him. Carl S. Marvel was very good. I didn't use Marvel much, not because I didn't like him, I just didn't have occasion to. I did use Fred Wall and liked Jack [J. R.] Johnson. I used Emmet Reid. That was mainly because Reed needed somebody to use him, and I was playing for awhile with some sulfur chemistry about which Reid knew a fair amount. That was to help the management take care of Reid. Who else did I have?

FERGUSON: "Buzz" [Ralph N.] Adams from Kansas.

SCHROEDER: I don't know him but he was quite good by repute.

FERGUSON: He was the only consultant I ever had that would go back to his lab or go to the library, look up something, and write you a little letter on what you had consulted on. He was usually helpful.

SCHROEDER: I never used him but the reports we got from our people were that he was very fine. The one I used several times myself when I was in Central Research was Richard Lord from MIT. He was very good, enthusiastic, and a lot of fun. I didn't have all that much to do with infrared, but every once in awhile there was a problem and I would get a hold of him. He was good.

FERGUSON: I used him and enjoyed working with him. He got to be a personal friend.

SCHROEDER: I used Paul Flory a little but not much. I also used Herman Mark a little. These people are well-known so you don't need any information on them.

FERGUSON: Let's go on to your subsequent career in consulting. How did you get hooked up with the Metropolitan Museum of Art?

SCHROEDER: That was Harold Kwart, a professor of chemistry at the University of Delaware, who did that for me. The Metropolitan was trying to hire a chemist to run their conservation work. Harold came to see me one day when I was working with Du Pont, and asked me if I could advise them. I said, "But Harold, why would a chemist want to work for the Metropolitan Museum? They can't offer a good chemist any kind of future." I explained this to Harold and he said, "They never thought of it that way and neither did I. But would you mind talking to them about that?" I said, "No. I'm going to retire in a few months. When I'm retired, I'll go up there with you and we'll talk to them."

About two days after I retired, I was up at the Metropolitan talking with the people at the Museum and explaining to them why only a dunderhead of a chemist would work for them. They took me around, showed me the place and then they left and came back and said, "Would you like to do it?" I said, "I don't want to work at being a chemist. I'm not really capable of being a chemist anymore. I'm a dinosaur, having been out of the lab for so long." Then they offered me a job of trying to set up an organization for them and hiring some people. I said, "Well, I really don't want to work full-time, but I would be willing to consult." So I made a deal with them to spend on the average two days a week per year. I had it renewable after a year, because I think when you hire a consultant, it shouldn't be renewed except by mutual consent. I've had some experience in trying to get rid of ones that didn't work. I was convinced that both sides ought to have an escape clause so that it wouldn't be embarrassing.

I set out to work to set up a scientific organization for the Metropolitan. As I took a look at what was going on in the outside world of art with scientific people and visited some universities such as the New York University School of Fine Arts, the State University of New York at Cooperstown, and some organizations in Europe. I got a pretty good feel as to what they needed. But then as I talked to the Metropolitan I found that they really didn't want to support the kind of thing that they said they wanted to support despite the fact that my proposal was very modest. Really, all they wanted to do was say they had me and use me for show. So I hired them a sort of a peon, a nice young chemist who was very interested in art. As soon as I could, I quit. But in the course of that interesting year's work, I got into some work with the Smithsonian and with Winterthur on scientific advisory committees. Those places were much more responsive and much more objective and sensible about the use of science for art conservation and restoration purposes.

FERGUSON: I had some really enjoyable interactions with Vic [Victor F.] Hanson at Winterthur.

SCHROEDER: Oh, yes. He was lovely.

FERGUSON: He was doing interesting things and had enthusiasm. I used to take visiting European scientists around.

SCHROEDER: He really enjoyed playing with his x-ray fluorescence of elements to see what was in the various artifacts that the Winterthur possessed. He was a very good man.

FERGUSON: Yes, he was. Did he ever tell you the story about the lead chromate in the U.S. currency?

SCHROEDER: Yes. That was fascinating.

FERGUSON: I guess that never got in print.

SCHROEDER: No. I don't think so.

FERGUSON: He couldn't get any of the papers to pick it up. I don't know whether he backed off from writing it up or not.

SCHROEDER: He had written it, but I don't know whether he published it. He sent me a copy of what he put together. I'll look around and see if I have it. If I do, I'll give it to you.

FERGUSON: I was on a train with him, going down to a meeting in Washington and he told me that whole story. I thought it was hilarious.

SCHROEDER: I also got into some outside consulting first; for Du Pont, then for others. This came at me kind of like rain. I've done some for a couple of companies on patent cases or suits. I've also done some general advising on business situations and on polymer chemistry. This was for the Department of Commerce and for companies like Borg-Warner, Allis-Chalmers, Chicago Rawhide [CR Industries] and Exxon. I've found that many other companies just wanted to use me as a way to get at Du Pont technology, so I couldn't touch them. Exxon was extraordinarily clean. They wanted me for a special situation. Basically, they wanted information to tell them whether they should go into a particular business, like buying a company. They could not have been more ethical in this.

FERGUSON: Do you think that the companies you dealt with were in general ethical?

SCHROEDER: Yes. The ones I dealt with were all fine. I filtered them. You can sort of tell when they go at you, whether they're after boudoir-type information or whether it's clean information that they want.

FERGUSON: How did you get involved with the school board in Chester County, Pennsylvania?

SCHROEDER: I made a terrible mistake. I went to a PTA meeting and it was a terrible mess. As they were going along, I made a very critical comment. They thanked me very much and after the school board meeting was over, they came over to me and said, "We have made you a member of our executive committee." I did that for about a month and then said, "I have to get out of this." And they said, "We want to run you for the school board." This was in Chester County. "There's a character we have to get off of the school board because he won't let us make any progress." This was interesting because it was Chris Sanderson, who was a local hero for his attempt to preserve the culture of fifty years ago. He ran square dances and had a local museum. He was the local character, but his idea of education was as it was given in 1880.

Anyway, there were enough people in that district by then who were not farmers and local Chester county people so that after a very heated campaign in which I did nothing, I got elected.

FERGUSON: How did you find the time with your four children and all your other activities?

SCHROEDER: Oh, it wasn't too bad. It was an evening thing. It's interesting. I was on the school board for six years and then didn't want to run again. I learned by then that the system had so much inertia, that there wasn't much point in fiddling with it. I also learned that there was no way you could get those schools improved to the point where they were good enough for my family. So I took my four kids out of the school system and sent them to Tower Hill School. That was a valuable thing. Whether you think it's proper to send kids to public school or not, you do get concerned about your own kids. You want them to get a decent education. You have it pretty good in the district where you are in Delaware.

FERGUSON: Yes, but we took our daughter and finally put her into Tower Hill. E. Bright Wilson went through the same thing with his kids. It's interesting, because it's a philosophically difficult thing to do, but he just couldn't tolerate the Cambridge city schools.

SCHROEDER: That's what happened apparently with my parents and me. That's how I got into that private school. You just conclude that your kid won't get what you want him to get. Even if you want him in a public school, you won't get it.

FERGUSON: Are there any of your professional society activities that you want to talk about? I know that you're a member of several societies, but I don't know if you were ever really active as an officer.

SCHROEDER: I was an alternative councilor with the local section of the Delaware ACS. I didn't do much other than what was required. I really wasn't too interested and didn't have time to spend on that kind of activity. I did get fairly active in the Institute of Synthetic Rubber Producers. I ran their research committee for about eight years. I basically tried to get them organized. That was worthwhile from the time standpoint because we were supporting academic research, trying to get a coherent approach to certain technical problems that were common throughout the industry, measuring standards, reporting standards, and that sort of thing.

I also worked quite actively in the Industrial Research Institute [IRI] which was an association of industrial research directors. I worked on their award committee, setting up an award which still persists for technical excellence rather than management excellence. I was on their academic liaison group for quite some time and ran a couple of meetings for them. That was worthwhile because it got me to meet research directors of other companies in which I was interested. You get to know these people much better if you work on committees with them. This is true of the Institute of Synthetic Rubber and also of the Industrial Research Institute. You get to know them as friends and the way they think, whereas if you just see them in meetings, they are just people.

FERGUSON: In general, do you find that these other research directors in companies have significantly different research philosophies?

SCHROEDER: Oh, yes. Some of them are startlingly different. There was a small group of people who were really interested in research, people like [Norman] Bruce Hannay of Bell Labs and Art [Arthur M.] Beuche of GE. That varied. You know the companies. Those people really understood research and tried to do a good job. In many other cases, it was primarily development, the technical side of commercial greed. They were really caught in their businesses. They were slaves to their businesses. You could count on ten fingers the ones in the IRI that were really interested in research. In the Institute of Synthetic Rubber Producers, there were maybe three, including Du Pont. One was

Bayer.

FERGUSON: You've spent a lot of time for the Harvard Club and University. You were on the Visiting Committee at Harvard?

SCHROEDER: Yes, for three terms. Actually, before that, when I worked in Orchem as Assistant Director, we had a vigorous recruiting policy. We were trying to be as independent from Central Personnel as possible. The way we did it was to send emissaries out to universities to get acquainted with them, not to hire. I used to go out and give talks about our research. My schools were Harvard, Yale, MIT, Cornell, Princeton, Columbia, Wisconsin and, when Carter retired, Stanford, Washington, Berkeley, Caltech, and UCLA. They were good schools. The real purpose is to let these schools know the character of your research by describing programs and results. It's up to them to judge whether it's good or bad. Of course, you try to get to know the people, the professors and deans. If they know the character of your research and they know you, then you could talk to them about a man who happened to work with them, when you are reviewing papers about candidates. If you have a referral describing Joe Doe, you want to know what Joe is really like. It helps in hiring a good staff. Also, it avoids misfits so that you don't have any unpleasant experiences.

FERGUSON: Does the Visiting Committee have an impact on faculty selections and so on in the chemistry department at Harvard?

SCHROEDER: Not much.

[END OF TAPE, SIDE 8]

SCHROEDER: We'd meet once a year. It was a group of both academic and industrial people who were regarded as suitable for the purpose by Harvard. They would give us some statistics on the department and talk about the character of their work and we would react. We were supposed to say everything was fine and hunky-dory, but they're pretty smart up there, as you well know. They didn't take adverse comments too kindly. I remember I got on one theme in which I think I did have a little effect. At the time I was active on the committee, Harvard had what was very probably the New York Yankees of chemistry faculties. They were a superb group. It was very hard to find anything anywhere near comparable. I knew all these fellows well. I knew how old they were, and I kept telling them, "Now look, you have to do yourself some real recruiting." A couple of my colleagues like Bill Schlichter at Bell Labs were on that group and they said the same thing. It was very hard to get these professors to realize that they were mortal and would be retiring.

FERGUSON: They almost seem immortal.

SCHROEDER: They were so damn good and so intolerant of some very fine people that went through there and got thrown out. They made a number of distinguished mistakes. Now of course, the world is difficult, because people just don't automatically jump when somebody says, "Go to Harvard." There are too many other choices and too many other good environments.

FERGUSON: I think plant safety and disasters is certainly a growing concern. I know you at least knew about it and were involved in some significant events at Du Pont.

SCHROEDER: I found the basic Du Pont attitude toward safety always excellent. The one we had inculcated into us at the laboratory, particularly at Jackson Lab (which was more dangerous than Central Research because of the kinds of materials they fiddled with) was that you do not take calculated risks. You don't take risks. You fix the situation so that they are no safety risks. The other attitude that I found, that was singularly good, was that safety was a line organizational responsibility and that the individual laboratory and plant manager were responsible. In details such as the way the laboratory is, the way the plant is, whether things are clean, the general spirit was pretty good. However, individual performance varied very greatly from plant to plant. The situation that I thought was most interesting was the one that occurred in the 1960s at Louisville, Kentucky. Do you want to talk about that?

FERGUSON: I'd like to.

SCHROEDER: This brought out a point with regard to safety that we had in what you might call our safety equation, but the constant next to it wasn't great enough. What it amounts to is not the safety of individual operations, like how you brush your teeth, but <u>process</u> safety on the whole which is important. We didn't factor in properly some of the elements of process safety. The 1965 incident at Louisville, in which I think fourteen people were killed, points this out. Our process for making chloroprene originally was known to be dangerous and we took steps to handle this. We knew that acetylene was explosive. So in the old process, we dimerized acetylene to monovinyl acetylene (MVA), separated the monovinyl acetylene from acetylene and divinyl acetylene behind barricade of stone walls. It was a fort. The Chambers Works was built that way and the Louisville plant was built that way. The compression of acetylene was done in very small stages in Nash pumps which are basically water pumps, where acetylene is pretty safe.

In the 1950s, with the need to make more and more Neoprene, the business was exploding. It was growing like the dickens. We got interested in more economical processes, and made the first step which you might call over the edge of the cliff. We switched from these reciprocal Nash pumps, which were safe, to blowers or rotary pumps. Roots centrifugal blowers. This took the pressure up substantially. As my friend Carter said when this particular process was developed on the plant, "There's going to be trouble." Carter was very conscious of process safety, having grown up with all the little explosions that they had with acetylene handling.

FERGUSON: They did have explosions at Chambers Works?

SCHROEDER: Oh, yes. They had lots of little explosions. After the beginning they knew what to do about them, and they treated them safely. We went to these Root blowers and that process was put in at Montague, Michigan, when they opened up the new Montague Works. It was then put in at Maydown over in Ireland and also put in over in Japan, when we opened up the Japanese plant.

Out of all of this, we had developed a core of knowledge that said that we had to be very, very careful handling acetylene and monovinyl acetylene, but once we had them and ran the monovinyl acetylene into the plant to make chloroprene, we were pretty safe. So these extreme provisions came up only to the point where you shot monovinyl acetylene into a chloroprene plant.

We thought from our studies of explosivity, which had been pretty careful, that acetylene was the dangerous actor. Gaseous monovinyl acetylene could be exploded with an explosive squib but the pipes were designed to contain that energy which was small. Our experiments showed that it would not be possible to set liquid monovinyl acetylene off under the conditions in our plant even if the gas exploded. You couldn't develop enough energy. We thought there was no situation where there would be enough energy in gas to set off the liquid monovinyl acetylene. Though explosive in the vapor phase, there was not enough weight of gas to damage the equipment. We were quite unsuccessful in setting off explosions of monovinyl acetylene in the liquid phase except with excessive explosive charges. Thus we thought once we had monovinyl acetylene as liquid we were safe and we knew chloroprene was pretty safe. These are dangerous chemicals but from experience we knew how to handle them. We knew them. So we thought it was like handling HCl or liquid ammonia.

During the startup and operation of the plants in Montague and Maydown, we had two explosions which killed people. We thought those were traced back to acetylene explosions. We took some provisions to correct what we thought was the trouble but it turned out that we didn't understand those explosions. Meanwhile the Neoprene business kept growing and the Plants Technical group developed a process that replaced the Root blowers which operated at 50 to 100 pounds. This was done to improve productivity and the rate of reaction in the chloroprene process where the monovinyl acetylene was brought into contact with hydrogen chloride in the presence of a catalyst. They suggested we switch from the Root blowers to Swedish rotary screw compressors which rotate at 300 rpm, up an order of magnitude from the 100 psi rate of the Roots blowers.

My friend Carter didn't like that process but we couldn't do anything since the Plants Technical Section was in the manufacturing division. Then in 1958 or 1959, we got responsibility for the Plants Technical Section. Brel [Charles B.] McCoy straightened out that situation. George Holbrook had liked the Plants Technical Section separate from the Research Division. He liked to stir things up. Brel McCoy liked order and he didn't like waste. So we got the Plants Technical Section and with it the job of completing the new process. By then that process had gained so much momentum, because it looked so good and was so economical and would save us so much money, that we finished off the work. We actually thought after going through a very exceptionally careful review of the hazard, instigated by my friend Carter, who was singularly conscious of this, that the process was okay. I'm now talking about the process of taking monovinyl acetylene and making chloroprene. We had completed ran appropriate explosion test with squibs. We analyzed the equipment for strain.

Everything looked all right and we put in a plant with Allis-Chalmer's American version of the Swedish screw compressor. The single chloroprene line that we put in with the big screw compressor ran very well and was capable of making something like a 180 million pounds of chloroprene. It was a dream. It worked like a charm. We got higher yields. We could run at lower conversions and get less dichlorobutene. We had very high process efficiency. It was beautiful. All of our problems with making chloroprene seemed to have disappeared.

One day, there was a little signal down at the plant of some trouble with the compressor. It seemed to be not running quite so fast and laboring a bit. The plant management and Manufacturing Division chose to keep working, because they thought that they could straighten that out on the run. I don't know the exact details here.

FERGUSON: Did they have the compressor instrumented for monitoring?

SCHROEDER: Oh, yes. This was a very sophisticated compressor.

We had indications of speed, temperature, power consumption, etc. whatever was needed.

FERGUSON: It was monitored from a central control room?

SCHROEDER: Yes. I don't remember the detail but there were some signs of a little trouble and they sent out some people to look at the plant. It wasn't red lights flashing or anything like that. It wasn't a stop situation, just symptoms. Then it blew and it really blew. It just blew up that whole plant. It blew out the lines. The explosion propagated throughout the whole plant that manufactured MVA. It went backwards and killed twelve people initially and then I think two more died. It was so bad that one fellow, whom I knew quite well, they didn't find for a day. Finally they got him and when they found him, he had been blown so that he was wrapped around a pipe, just like lagging, about two inches thick. It was terrible!

I got the job of running the group to find out what had really happened. I used Art [Arthur F.] Myers, who is a very sharp engineer, full of good questions, to run the inquest group down at Louisville. I got Bob Schultz, who was a good engineer up at Montague, into Wilmington, and appointed him as a supervisor of process safety. This was the first one that the company had. The job that we gave Schultz was to ensure the safety of our processes, starting with this one, so that this kind of thing wouldn't happen again. The job we gave to Myers was to find out what the hell had really happened. We also started some new explosivity tests.

To make a long story short, we found out that monovinyl acetylene polymer had built up in the very complex seals in the screw compressor. The seal has all kinds of convolutions and little passages. It's extremely complex and sophisticated. Polymer had built up in the seals and pushed the seals against the face of the rotor. This is why the compressor was laboring a bit. The velocity had gone down. Running around at 3000 rpm, slowing down to about 2000, the compressor rotor, working against the seal face, built up a hell of a lot of heat. That heat was the ignition source. That's the first thing we had to find -- the ignition source.

The explosion had then propagated in the vapor phase of MVA through the lines. That in itself would have done no serious damage. It would have blown a gasket or safety disc here or there. All of the lines were built to withstand a vapor MVA explosion. The explosion without doing any particular damage, blew a seal here and there, but propagated through the vapor in the big condenser. The big condenser really was a large still with a top hat. You run the synthesis of chloroprene at relatively modest conversion to avoid adding two moles of HCl to one of monovinyl acetylene and thus synthesizing things you don't want. In the big still, you separate chloroprene from unreacted monovinyl acetylene, which then goes back into the compressor.

In the top hat of the big still, which is about ten feet across, was a seething mixture of monovinyl acetylene vapor and monovinyl acetylene liquid droplets. In such a situation, we later learned, that although monovinyl acetylene vapor does not liberate enough energy to set off monovinyl acetylene liquid when the liquid is at the bottom and the vapor is at the top, in the situation where the monovinyl acetylene liquid is dispersed as a mist in the vapor, it can be set off. We simulated this later. When it's set off the monovinyl acetylene liquid has in it about six-tenths of the energy of TNT. So, we just blew the ass out of that still. That gave us a new explosion which propagated down the liquid lines and blew out everything. That's what killed all the people.

The accuracy of our experiments attempting to set off liquid monovinyl acetylene from the vapor was vindicated by the fact that the explosion propagated right down the gas line to the big storage tank where we stored liquid monovinyl acetylene. We stored it below O°C, maybe -10°C, but it was cold. The explosion propagated down to the tank, blew the vapor in the big tank, but did not set off the liquid. That was a hell of an experiment, because there was about 50,000 gallons of monovinyl acetylene in that tank. It would have really done something to that end of Louisville if that baby had gone off. We know the explosion got that far because the tank dimensions had changed very slightly. Thus you could see the results of the explosion.

While this was going on, we figured we had to dilute the stream so that this sort of thing would happen again. I made the suggestion that proved to take care of it. It was to dilute the MVA with butane. The idea was to have in there a harmless molecule that would dilute the monovinyl acetylene so that it would be much harder to set off. Obviously, in a complex process, you want something that has physical constants as close to monovinyl acetylene as possible, so that it would go around the plant with the monovinyl acetylene, in effect as a tracer. It's not at all profound. I just suggested that we use butane because it has practically the same boiling point.

We ran some experiments with butane and found that with about 20-25 mol % of butane you could not set off the vapor except with extreme energy, and it was impossible to set off the liquid. So as quickly as possible in our other plants in Japan and Ireland we put butane in, and never had any trouble. We really went over those plants with a fine-tooth comb looking for any spots where there might be a chance of having pure monovinyl acetylene and vapor plus mist. So, we made the process safe. But of course, having had that experience, even with the process sanitized with the butane, we redoubled our efforts on the butadiene route, which was then a bit more expensive due to the low cost of acetylene, and got our lovely new butadiene process.

FERGUSON: Was the butadiene process economically advantageous?

SCHROEDER: Very! We had a nomograph that basically related the cost of chloroprene from butadiene or from acetylene, based on the relative costs of acetylene and butadiene. We knew the crossover point, and at that time we had a contract with National Carbide, where we had a price on acetylene of about eight cents, whereas butadiene at that time was about seven or eight cents, and at the same price, the acetylene was better. It turned out that National Carbide wasn't really able to maintain that price. They had to increase it, and they did. It was sort of a force majeure situation. The price of acetylene went up, and by the time we had the process worked out, it was sound to go to butadiene. Also, our new butadiene-to-chloroprene synthesis used a new phase-transfer catalyst for the dehydrochlorination. That increased the yield by about ten percent over the old direct use of caustic. So we came out like gangbusters. Is that enough?

FERGUSON: Yes. Would you like to talk about MOCA? You wrote a paper on that (43).

SCHROEDER: Sure. That was interesting. We manufactured polyether urethanes which were cured with an amine, methylene bis-_ortho_-chloroaniline. That's where MOCA comes from -- from formaldehyde and _ortho_-chloroaniline. That gave with the liquid polyether urethane extraordinarily good properties in hard urethanes for gears and industrial tires and things like that. It got such good properties out of the polyether urethanes that we hurt Bayer who was marketing polyester urethanes.

[END OF TAPE, SIDE 9]

SCHROEDER: Bayer then leaked out rumors that MOCA was carcinogenic. Our own tests had indicated to us that MOCA was okay. We had run some limited rat or mouse tests and it seemed to be all right. We had a core of knowledge that said that although dichlorobenzidene was carcinogenic, as most of the benzidenes were, diaminodiphenylmethane didn't seem to be, and chlorinated products of those types seemed to be okay. So we had figured that we were all right. When Bayer leaked out this information, I wrote to them and said that I would like to talk with them about this, because we didn't want to put out a substance that was carcinogenic without knowing about it and at least handling it properly -- if it were carcinogenic.

So Bayer agreed to see me and I trotted over to Germany in I think 1965 to talk to their research director, by the name of Hans Holtschmidt, and also with their toxicology people. They showed me tests which indicated that if you fed MOCA to rats using a liver damaging diet, the rats got cancer. They had nothing else other than that. Our Haskell people said that they thought that the diet itself was what caused the cancer. The question would best be answered by running MOCA tests on rats with a normal diet. Really, you couldn't be certain about rats or mice, in any case, because so many of the strains develop cancer almost spontaneously. What we really needed were some dog tests.

So we started out on some rat tests with normal diets and with the Bayer diet. We also did long-term dog tests which take many years and treated them differently from the way we had before. We upped the doses of the MOCA and in due course, we did confirm that rats would get cancer when fed a normal diet if the level of MOCA was increased, whereas with a normal diet and a lower level, which would cause a cancer in the rat that had the energy deficient diet, you wouldn't get the cancer.

The dog tests kept looking all right. The dogs went on and on and on and they seemed to be okay. But then, as the dogs were nearing the end of their lives, they started to look ill, so we started to sacrifice them and we found that, indeed, the dogs got some cancer, but nothing like what the rats did. So MOCA was potentially dangerous. There is a much better correspondence between dogs and humans than there is between rats or mice and humans. Still, we had no indication that we'd been in any trouble with MOCA. We had no sign of any trouble from the people who had made it. We had made it carefully in the plant, so that the exposures were minimized. But in the early days, in the 1950s when we were making MOCA in our semiworks, it's possible that the people got some exposures. We looked over all of those people and there was no sign of any trouble.

With a substance that was so important to a business, and with no sign that anybody had been hurt by it, and with every effort made to maintain cleanliness in the plant, we were advised by our legal people and by [John A.] Zapp (Director of Haskell Laboratory) that there was no reason why we shouldn't continue to make the stuff. We started to undertake efforts to be sure our customers used it safely. Later, MOCA was placed on that list of fourteen substances which were supposed to be carcinogenic. MOCA was the only one on that list where there really was a question at that time, as far as humans were concerned.

In any case, we then sent appropriate warnings out to our people again and continued to manufacture the stuff. Finally, we decided that even if the substance was harmless, as far as people were concerned, the risk of getting sued on the basis of its presence on the list and in a rather litigious society was too great. So we got out of that business.

FERGUSON: At the present point, can Du Pont now enter some of the businesses or operations that they got into earlier?

SCHROEDER: No.

FERGUSON: What would we lose?

SCHROEDER: Well, you couldn't move as fast. You'd have to get over so many institutional barriers and regulations. For instance, look at how we got into the manufacture of chloroprene rubber. We had a pilot plant operating in the Chambers Work nineteen months after Arnold Collins first took that rubbery polymer out of the column. We had Viton made in a semiworks within three and a half years of its discovery and commercially within five years of its discovery. Nylon was discovered in 1934 and I think the Seaford Plant was announced in 1939. Things like that. For substances with unknown risk, such things are now inconceivable. There's the EPA, the effluent problem, the toxicity problem. You just couldn't go through all the necessary tests in that kind of time.

We always ran toxicity tests on the substances we were going to make. Starting way back, shortly after Haskell Laboratory was established in the 1930s, we knew about the acute toxicity, the toxicity orally, the toxicity on skin, and that kind of thing. People ran patch tests and limited animal tests. They always got things like LD_{50} values [LD_{50} is that dosage which produces 50% mortality]. We did no long-term tests except in cases where there was reasonable suspicion. So some of these insidious affects could have been missed. In the case of nylon, I knew, as I started to work at the Central Chemical Department, that hexamethylene diamine was dangerous. Of course, it's caustic, and we knew that it had effects on blood pressure, but we handled it appropriately. The only sorts of things that we treated with extreme care were those that were extremely toxic. These were substances like phosgene, HCN, or things that were suspected of being carcinogenic like some of the aromatic amines, beta-naphthylamine, and benzidene. They were almost proscribed.

The battery of hurdles that you must get over to get a new product going these days is such that it's bound to inhibit. I think that in many cases it's excessive. It really depends upon how new a substance is, and what category you're in.

FERGUSON: How's this going to impact the future of the polymer business?

SCHROEDER: I don't think it will have an enormous effect on the future of the polymer business. Chances are that the big polymer discoveries, the high volume ones, have been made. There will be many new polymers made, but they will be refinements, special things from new techniques like group transfer polymerization. We will learn how to control molecular weight and learn how to better copolymerize various monomers. But the basic monomers with which you operate are the outcome of reactions on certain raw materials. The raw materials aren't cheap. There will be fancy polymers made, of course -- things like conductive polymers, piezoelectric polymers, photopolymers, you name it. Those developments will all be slowed down, but I don't see any big events which create the major effluent and toxicity problems. I think the chances of getting new ones are so slight and the development cost of getting a really new polymer is so great. The incentives are all on blending things and making modifications of what you already have.

FERGUSON: Thank you very much.

[END OF TAPE, SIDE 10]

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