THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

MALCOLM E. PRUITT

TRANSCRIPT

Transcript of an Interview
Conducted by

James J. Bohning

at

Midland, Michigan

on

15 January 1986

M.E.PRUITT

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 20 February 1989)

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

MALCOLM E. PRUITT

1915	Born in Hamilton, Texas on 15 October

Education

1941	B.S., chemistry, Abilene Christian University

Professional Experience

1936-1941	Hamilton County Public School, Texas
1941-1942	Clyde Independent Schools, Texas
	Dow Chemical Company, Texas Division
1942-1943	Control Chemist
1943-1944	Research Chemist, Gas Laboratory
1944-1946	Research Chemist, Organic Laboratory
1946-1948	Project Leader
1948-1951	Group Leader
1951-1954	Laboratory Group Leader
1954-1962	Director, Organic Products Development Laboratory
1962-1967	Director of Product Research
1967-1971	Director of Research and Development Dow Chemical USA
1971-1978	Director of Research & Development Dow Chemical Company
1975-1980	Vice-President
1978-1980	Corporate Director of Research and Development
	Council for Chemical Research
1980-1982	Chairman
1982-	Honorary Chairman

Honors

1972	Alumni Citation, Abilene Christian University
1973	Honorary Doctor of Science, Abilene Christian University
1978	Industrial Research Institute Medal
1981	Society of Research Adminstrators Award
1982	Commercial Development Association Award
1983	Outstanding Alumnus, Abilene Christian University
1985	First Recipient of Council for Chemical Research Award
1987	Earle Barnes Award for Leadership in Chemical Research Management, American Chemical Society

ABSTRACT

In this interview Mac Pruitt discusses his early bringing-up in a rural area of Texas and his struggles to complete undergraduate education during the Depression. After starting as a control chemist on the graveyard shift at the Dow facility in Freeport, Texas, Pruitt progresses through the laboratories and embarks on his extensive studies of the ionic polymerization of cyclic oxide monomers. The circumstances of his move into senior research management and his eventual transfer to Midland, Michigan, are embellished by Pruitt's reflections on R&D and on his initiatory role in the formation of the Chemical Industry Institute of Toxicology and the Council for Chemical Research.

INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry, and has been a member of the chemistry faculty at Wilkes College since 1959. He was chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences in 1988. He has been associated with the development and management of the oral history program at the Beckman Center since 1985, and was elected Chair of the Division of the History of Chemistry of the American Chemical Society for 1987.

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INTERVIEWER: James J. Bohning

LOCATION: Dow Chemical Company, Midland, Michigan

DATE: 15 January 1986

BOHNING: Dr. Pruitt, you were born on 15 October 1915 in Hamilton, Texas. Could you tell me something about your parents -- their names and occupations?

PRUITT: My father's name was Jesse F. Pruitt and my mother was Knoxie Stone Pruitt. They were farmers and I was the second of six children. We grew up on a farm just outside of Hamilton, Texas. I had very good parents. They were very strict but very good parents. They were good farmers and good Christian people. We weren't very rich by today's standards but we were considered the middle or elite of the community, although by today's standards we would probably be in the poverty level. [laughter] I had a good childhood. I had to do a lot of hard work but I didn't mind that. I have nothing but pleasant memories of my childhood and my parents. They both lived until they were in their eighties.

BOHNING: You mentioned that you were one of six. Could you tell me something about your brothers and sisters?

PRUITT: My older brother was a farmer and rancher in Hamilton County. I was next and then was my sister. Her name was Margaret Reese and she married a vice president of a pharmaceutical house. They lived in Fort Worth, Texas. The fourth was my brother, James Hubert, who was injured over Rabaul in a B-17 during Pacific War. His plane belly-landed on Burma Beach. His career was the grocery business. The next two were twins, Fay and Ray. Ray became a school superintendent in Eastland, Texas. Fay married a man named Bert Schrunk, who is a Gulf Oil distributor in Hamilton, Texas. They're all living, except Margaret who died this past summer from a brain tumor.

BOHNING: Did you receive your early schooling in Hamilton?

PRUITT: Yes. I received all of my elementary and high school education in Hamilton, Texas.

BOHNING: Were there any teachers or anyone else at that time in your life who had an influence on you?

PRUITT: Oh, yes. I can vaguely remember my first grade teacher, Mrs. Scroggins; she made a very good impression on me and that is what made me like school from then on. I had an English teacher all through high school named Miss Cook who crammed Chaucer down all of us. We all had to memorize Chaucer. She was an excellent English teacher. In fact, I don't know how old she is now. I think she's in her nineties but she still makes practically every high school reunion down there. She's one swell teacher.

In relation to what we're talking about, the one who probably had the greatest influence was my high school chemistry teacher, Cecil Nix. He was superintendent for the schools in Hamilton, but he was also teaching one course, chemistry. He is the one who inspired me to decide that I wanted to be a chemist. We didn't have a very large class. We did a lot of experiments together and a lot of talking. I liked him and he liked me. We didn't have too much equipment. We had to improvise a lot of it. But anyway, in that setting is where I decided that I was going to be a chemist and I never veered from it.

BOHNING: What about mathematics?

PRUITT: Yes. I had a good mathematics teacher, a fellow named John Sullivan. He was really a down-to-earth fellow. I was very impressed with him.

BOHNING: When did you decide to attend Abilene Christian [ACU]?

PRUITT: I graduated from high school in 1933, right in the depths of the Depression. It couldn't have been any worse. But I was determined to go to school and take chemistry and start being a chemist. My parents were farmers with very little money, and with four children younger than I was. I was valedictorian of my class and I got a $100 dollar scholarship. So I took the scholarship. My dad went downtown and bought me a twenty-five dollar suit and gave me twenty-five dollars. I was pretty sure that I wanted to go to ACU because we went to church and ACU was our church school. I also had some cousins living in Abilene that I could stay with. There were three boys, two cousins and myself. We roomed together in a little house. We did all of our cooking and all of the ironing of our shirts. After I went to school I never took another dime from my parents. They would offer it but I wouldn't take it. I made my way completely from there on.

Those were hard times. But, I think that a lot of people who grew up just like I did have turned out to be pretty solid citizens, because you had to make yourself go or you didn't.

BOHNING: How did you support yourself while you were there?

PRUITT: Of course, we had this house and we bought and cooked our own food. We had a milk goat that we got from somewhere and milked that. We lived on mush and a little bacon, and a few other things. We ironed our own clothes and washed our own clothes in the tub. For tuition, I worked for the school, and most of the time I worked in the Extension Office. That's where they send correspondence courses out. I ran that office--all of the mailing and the duplication--for a year's tuition.

In later years of college, I worked in a restaurant for my food and then I stayed in the dorms and worked in the Extension Office, which paid for most of my tuition and my room. In fact, I actually ran that place. I had an opportunity to make a fortune when I was in college because I had the main duplicating equipment for the university, and all of the teachers came to me to run off their final examinations. So I was privy to every final examination in the school. Boy, did I get bribe offers. I could have made a fortune selling those exams. [laughter]

BOHNING: You were at ACU for several years but you didn't finish straight away?

PRUITT: It took me eight years to get my B.S. degree. I went one full year on the scholarship and stayed in the house with my two cousins. Then I was completely out of money and came home that summer and worked in David Harleik's Dry Goods Store. I worked there all of the summer and fall. I saved up enough money so that I could go back for the spring semester. Then I went back to Harleik's and worked the summer and fall again and then went to another spring semester. So I had two years of college. In that day and time, you could get a temporary teacher's certificate with two years of college. I did that and that fall I went to teach in Lund Valley School in Hamilton County. I taught the seventh and eighth grades in this small rural school. It was a good little school. I worked there for two years. I was still paying back some debts I owed. That was also when they were giving allotments to the farmers for planting cotton and wheat. Well, in the summers, I measured cotton allotments for the county.

Then the third year, I became principal in this school and in that position I had to teach woodworking. I never had a woodworking tool in my life so I switched and went to school at North Texas State, which had woodworking. ACU did not have a course. I took chemistry and woodworking so I would be able to teach that when I went back in the fall. I went there for three summers, taking chemistry, physics and woodworking to be sure that I could teach woodworking. Then in the summer of 1941, I went back to ACU because I could get my degree quicker there. I took all of my final courses, which was a pretty heavy load, particularly all the chemistry I needed. I graduated in the summer of 1941 from ACU with a B.S. in chemistry.

BOHNING: I would like to ask you a little bit about your experiences at ACU and the chemistry department but before I do let me ask you about North Texas State since you took some chemistry there. What courses did you take and were there any faculty members who you remember?

PRUITT: The one that I remember the most was Dr. [Wallace N.] Masters, who people probably know. I think he has written a few textbooks (1). I think I took from him one course in organic chemistry. This old fellow, he was in his eighties when he was teaching this course, would stand there and spit all over the people in the first row of seats as he talked. But he was a very effective teacher and I really admired him. I took both qualitative and quantitative analysis. I also took physics there. I remember the physics teacher was a visiting professor from the University of Texas, but I can't remember his name. Maybe I took two courses in the physics department.

BOHNING: Was that course with Masters your first exposure to organic chemistry?

PRUITT: No. I don't know whether it was advanced organic or just a different course. I took my final course in the summer of 1941 from Dr. [Paul C.] Witt. I had about three different courses in organic. One of them might have been theory. Frankly, I don't even remember now.

BOHNING: You mentioned Dr. Witt. Let's go back to ACU.

PRUITT: Yes, he was the best teacher I ever had. He was the prime teacher. They only had two or three teachers. The whole thing was in a little wooden shack. It had a couple classrooms and some crude laboratories. Dr. Witt was one of the finest teachers that I had ever run into. Unbelievable. Simple and down to earth, but did he ever know chemistry in general but organic in particular, with a little leaning toward biochemistry. In fact, it's said that Dr. Witt got his Ph.D. from the University of Colorado and discovered sulfa drugs, only too late to get credit. Other people got credit for it but he had discovered the same thing on his own.

BOHNING: Did you do any research as an undergraduate?

PRUITT: Not really. We just did experiments. We would be assigned problems to do but I don't think that's research. It was more of just assigning a problem list. So it was mostly just testing and problem solving.

BOHNING: Were there any other students at ACU that you remember that went on to careers in chemistry?

PRUITT: There was one person in the chemistry department who worked for Dow. Sanford Thompson who worked for Dow as a chemist, but he got involved later in some other things like safety, inventory, and things of that nature. I don't know of any of the others who went to school with me who did great things in chemistry. I think most of them taking it never made a career out of it.

BOHNING: Had you heard anything about polymers before you left ACU?

PRUITT: No. I may have heard the word somewhere in a textbook but I knew nothing about polymers when I went to work for Dow. I hardly knew what the word meant.

BOHNING: When you left ACU you taught for another year?

PRUITT: Yes. I got my degree in 1941 and I did not know of any place to go for a chemistry job. I was really in debt. I was trying to work my way through all this, but they were still hard times. I got a job at Clyde, Texas as a science teacher and a football coach. I taught for one year. This was a very pleasant experience. I really enjoyed it.

BOHNING: It was around that time that Dow was expanding in Texas. How did you get into contact with Dow?

PRUITT: Sometime while I was teaching, I went back home to Hamilton to visit my parents. There was somebody there in Hamilton who told me that he had read in a paper that a new chemical company called Dow was coming to Texas. They were going to build a plant in Freeport, Texas. This was just a little conversation. I had never heard of them. I didn't know much about any chemical companies. I hardly knew what a chemist did.

I was teaching in Clyde and I decided that during the Christmas holidays I was going to go out and start looking. I decided to go first to Humble, now Exxon. (There is an interesting anecdote by Jim Mathis who was a senior vice president at Exxon. (2)) I went down to Baytown in Houston to apply as a chemist with Humble. They didn't really have anything for me as they really were looking for someone with a higher degree. I only had a B.S. and if they started you, they started you out as a roughneck where you worked in the lots and the fields. But they really didn't have anything for me. Then I remembered hearing about Dow which was about sixty miles down the coast. This was in the middle of the afternoon; I remember my dad was with me. I decided to go to Freeport to see what Dow had down there. I got there just before quitting time, around 4:30, and they quit at 5:00. I went into the employment office and they gave me a form to fill out, which I did. When I handed it to them they told me they would let me know if there's a job available. I said, "Ma'am, I came too far to just hand you a application form. I want to talk to somebody." "Well, it's too close to quitting time. I don't think we can get anybody." "Well, I insist." Finally, they sent me around to the personnel director, a fellow named Peters. I sat down and he looked my application over and said, "Well, the first thing I see wrong with you is that you taught school too long." [laughter] I thought that was kind of a stupid remark. Anyway, he said, "Okay, we'll file this and let you know." "No," I said, "I want to talk to somebody about a job." I insisted, and if I hadn't, I would never have gotten a job. I would have just walked off and that would have been it. Finally, he said, "Okay, I'll call Dr. Wright."

About the only thing they had in Freeport at that time was an analytical lab. Dr. E. R. Wright was the head of the analytical lab in the Freeport section. By now it was almost 5:00. He said, "He'll be coming out to go home in a few minutes and we'll just have you meet him in the clock room and you can talk to him there." I said, "Fine." So here I was expecting a big, dignified, well-dressed man. We all have these images. You know, Freeport is kind of warm even in the winter time. Well, here comes this little guy in short sleeves, and he had his little black lunch bucket. He came in and punched the clock like everybody else. We sat in a corner of the clock room and talked. But, he was a very nice man and a very smart man. We talked a bit. He liked the idea about me teaching school and he said, "Yes. I think we can use you. Dow is building a plant to produce magnesium for our military planes." This would be seven miles away. I don't know if you've ever been to Freeport.

BOHNING: No.

PRUITT: Dow Magnesium was to be seven miles away from the coast to get away from possible submarine attacks. They were building a big ditch to bring the seawater up to this plant called Dow Magnesium Corporation, owned by the government. He said, "Next summer we're going to open that up and we're going to need some people and we'd like to have you. When can you go to work?" I said, "I can't go to work until I finish my school year. I'm not going to run off and leave them. I'll be ready to go to work next June when school's out." Then I said, "You mean I'm all done?" He said, "Okay, I'll let you know next spring." June came and I got out of school. I went back to my parents because I was going to help them farm that summer if I didn't have anything else to do. I was out on a tractor plowing farmland when my wife came running out to the field and said, "You got a letter from Dow offering you a job." This was in June. I went home, packed my bags, and headed for Freeport. I started work on June 24, 1942.

BOHNING: What was your first position there? I have a note that says "magnesium shelf dryer." Is that correct?

PRUITT: I had probably the lowest position that a chemist can have in the company; as a control chemist on the graveyard shift. What is the shelf dryer? They take seawater and add lime to it to precipitate the magnesium hydroxide. This is done in big Dorr stirring tanks and the slush of $Mg(OH)_2$ settles to the bottom. Then they pump that out and hit it with a dilute solution of HCl to make magnesium chloride. Then they take the magnesium chloride and drive off most of the water in an evaporator. Then they take the semi-solid material and put it in a shelf dryer. It's a big thing which rotates the shelves in a blast of hot air. They dry it to the consistency they want for the magnesium cells, which is about seventy percent. My job was to determine when the shelf dryers produced feed with the proper amount of magnesium chloride. If the water content wasn't within a percent or so, it wouldn't work in the cells. My job was to analyze with a Volhard titration. These were all wet methods. They didn't have any of those fancy instruments of today. I did Volhard titrations on the graveyard shift. A lot of times the foreman didn't pay any attention to the analyses. Sometimes he would feel it and tell if it was ready to go in there better than I could. [laughter] And a lot of times he would even taste it. That was his method.

BOHNING: How did that method compare with your analyses?

PRUITT: It wasn't bad. They were pretty good. Some of these practical things like feel and eyesight are pretty accurate. Those old boys who ran the plants, it's amazing how much they know about the process using their own instincts.

BOHNING: Did you move to the analytical lab the next year?

PRUITT: Well, this was an extension of the analytical lab. I still worked under Dr. Wright. He ran all of these labs. I did that job in June and started training there. In August of the same year I moved over to this new plant that opened up, for which I was hired in the first place. I set up the lab in the shelf dryer. I actually set it up and got it running to do the analytical work for the shelf dryer. This was the first time that I ever ran into union jurisdiction. Of course, we didn't have much in the way of a laboratory. I needed a funnel rack. We didn't have one anywhere and I decided to just build one. There were scraps of lumber down there and I had a hammer and saw. So, I made a funnel rack. Some guy came along and said, "You can't do that." Well, I thought he meant that I didn't have the ability to build it. I said, "The heck I can't. Just watch me." [laughter]

Well, that was a union steward who was telling me that. I didn't know what a union steward was. I had never heard of the union. I was as green as a gourd. The superintendent came by and said, "You just upset the union steward. You're not supposed to be doing carpentry work." I said, "I'm not? Who ever heard of such a thing?" Anyway, I caused a semi-grievance right off the bat. That was my first run-in with the union.

I stayed there about two or three months and then they transferred me directly up to the main analytical lab in the Dow Magnesium Corporation, where we did all kinds of analyses. What we called adjusting liquors, magnesium hydroxide, cell feed, lime content, etc. Everything that was in the plant we ran. So I went up there and stayed about a year.

[END OF TAPE, SIDE 1]

PRUITT: Then one day they said, "Say, we would like to transfer you to the gas lab over at Plant A (the Main lab)." Now this is back to Dow Chemical. I liked the people whom I had been working with very much. We played touch football at noon. We were one great bunch of people, and I didn't much want to leave at that time. That was kind of stupid when I think about it now. But anyway, I said, "Okay." So the next day they transferred me to the gas lab. By that time Dow had built light hydrocarbon plants. Are you familiar with these, where they cracked LPG [liquefied petroleum gas] to make ethylene, propylene, butadiene, etc. In those days and times, they hardly had any way of analyzing material from the cracker. The only thing they had was Orsats [gas buret analyzers]. You know what Orsats are?

BOHNING: Yes.

PRUITT: Or Podbielniak columns. You know what they are?

BOHNING: Yes.

PRUITT: You do?

BOHNING: I remember seeing them. I never used one.

PRUITT: I was an expert at using the Podbielniak column. I could never duplicate my results. But that was the only way we had to analyze gases like ethane, propane, and methane as well as the cracked products from ethylene, propylene etc.. You take a sample and condense it with liquid nitrogen and distill them. Boy, is that an art! The columns freeze up and a little of everything else goes wrong. But I got to where I could really run those. I was good at that--as good as you could get. They would take the sample in a big five gallon jug and I think the sampling wasn't all that good either. We would pressure them up to maybe three or four pounds. We'd drain that out and condense it and distill it. I ran many a Podbielniak column during those days. I stayed there a year.

BOHNING: What kind of a person was Wright to work for? Did you have daily contact with him?

PRUITT: No. In this period that I'm talking to you about, there was a fellow named Ray Borup down at the gas lab. Dr. Wright was very non-communicative, very reserved, very quiet, but very thorough, and very good. I had a lot of respect for Dr. Wright but a lot of people didn't like him because he didn't communicate. He communicated almost entirely in Yes's and No's. I know now that Earle Barnes (he would play a great role in my life because he and I worked together from then on), was trying to set up a research organization in Texas, and he already had started. He didn't have too many people, about four or five, and he was looking for another man. He went to Dr. Wright to recommend somebody out of his group that would go into research. Dr. Wright undoubtedly had his eye on me to do that, but he wanted to be sure about me. He came down one day to the gas lab and got me off of the Podbielniak and said, "Say, I've got a paint vehicle here. I'd like you to analyze it. Tell me what's in this paint vehicle." He was just doing this to see how good I was. I took that and distilled it and did all kinds of things, Orsats and everything else we had then. I identified everything in it for him. That was good. He also had a bench up in the main wet methods lab where he did special samples--the oddball samples that came in. The next day he came up and said, "Say, I want you to come up and work with me." So I worked side-by-side with Doc Wright on all of these special samples. We never said a word. We would just sit there and work together with hardly ever a word.

I had been there for two weeks and one day I said to him, "Doc, am I going to stay here permanently or am I going to go back to the gas lab?" He didn't say a word. He just walked off and left me standing there. [laughter] See, he wasn't ready to answer me. We had those big slide rules for our calculations. A little later I was sitting there doing a lot of calculations and he slips in across the table--this was the way he communicated. He said to me, "How would you like to transfer to research?" Well, that's what I had been wanting all of the time. I said, "Well, I think so." He said, "Report to Bobby Wall, one of Earle Barnes' chief chemists, on Monday morning." He got up and walked off without another word. Well, that's good enough. That's all I needed to know, so I went to work in research.

BOHNING: Is that when you moved into the organic lab?

PRUITT: Yes. I really was working directly for Bobby Wall. He had about four or five people then. We started off on a polymer project. Dow in Midland was doing a lot of work on vinyl chloride-vinylidene chloride copolymerizations for Saran. Bobby Wall felt he had a better idea on how to do that. I was helping him set up and do a little polymerization. Nothing amounted to anything. Right off I could see that he didn't have anything. But I helped him to set up his equipment and do the experiments for him. That lasted for a short time and then I went on to something else. Bobby Wall went on to become a researcher for Monsanto. He was very good at instruments, very good at equipment but not too interested in carrying out the experiments. We would set up a good, fine apparatus and all of the instruments to go with it and then he would say, "Tear it down." "But, wait a minute, I would say. Let's run an experiment in this." [laughter] He was more interested in instruments and physics, and not in the chemical part. And, by the way, he was very good at it.

BOHNING: It's my understanding that when Dow first started in Texas, the research would be concentrated here in Midland.

PRUITT: Yes. We were not supposed to have any research in Texas. None whatever. That's right.

BOHNING: How did that slowly change?

PRUITT: They built the light hydrocarbon plant and then they built an alkylene oxide plant, then a glycol plant, and then they started to get into chlorinated solvents. They had to have some trouble shooting. Mr. Roush, Assistant General Manager of Organic Production, had hired Earle Barnes, who was in the special training course at Midland, to run the glycol plant. This was all in the middle of the war. My military classification kept changing from 1A to 2A and I was in and out of 2A, then back to 1A. The government kept demanding that those of us working on magnesium should be exempt. They just wouldn't let any of us who were working on magnesium go. They would get us a deferment and put us back, we would go into 1A, and they would come right back with another deferment. I never knew when I was going to go. But I guess it was a pretty essential occupation.

At that time synthetic rubber was needed very badly especially rubber for low temperature gaskets and similar applications. All of a sudden, Thiokol rubber became very important to the government. They wanted Dow to build a Thiokol rubber plant. So they pulled Earle Barnes off to start the building and the running of a Thiokol plant. We got this thing built and were training people when then they decided that they didn't want it. So, we never did start it up.

About that time, Mr. Roush, decided that he needed a troubleshooting group. He said, "Earle, you set up a troubleshooting research group." Nobody, I think, had any trouble with that. They could see what needed to be done. There was no real research, just plant problems. But we very quickly got moving into research. We got no research money from Midland. All of the money came out of Mr. Roush's production budget. Everything. We had to scramble and steal equipment from other places--anything we could do to get money to run on because we had no backing from Midland as far as research money was concerned. But it just slowly developed, and finally we became probably the more dominant research group in the company in the 1960s.

BOHNING: You did some early work on ethylene oxide polymerization. Now I'm quoting (3), "He (he being you) learned the hard way. He had many unscheduled explosions before he learned how to remove the heat buildup."

PRUITT: Where did you get this quote from?

BOHNING: From the Dow Research History (3).

PRUITT: Oh, you mean the book? Okay. Well, I was assigned right off by Earle to look into alkylene oxides, their polymerization and what we were going to do with them. Were we going to make ethers or glycols--what we were going to do with these things? Ethylene glycol's main use was for making anti-freeze. This development had really helped the military in the war effort; sometimes the propylene glycols were used as defoaming agents, but that's about all. The only person who knew anything about alkylene oxides was Staudinger over in Germany. I had read him and he didn't have much to say but he did know that alkalis like KOH or caustic soda would polymerize ethylene oxide.

So, I started doing this. My first experiment was in a big steam oven, with big wooden doors with a big latch on it. I was working first with ethylene oxide. I would take ethylene oxide, put it in a glass vial, chill it down and I would put in caustic or KOH and as well as other catalysts. Then I would seal them up, wrap them up in a cloth towel, put them in those ovens and see if they would thicken. Well, as time went on, I would look in there and I would shake them to see how thick they were getting. One day I went in there and I had about four or five of these big tubes. I shook them to see how thick they were getting. I decided that I would just move them up. Each higher shelf was a little hotter because of the steam set up. I wrapped them back up in their towel and put them on a higher shelf. I had just shut the door and had taken about five steps when the whole load of vials came apart, blowing the door open. Glass and towels went through my legs and back. It didn't hurt me but if that had happened just a few seconds sooner, it would have been right in my face.

I learned a lot about [cyclic] oxides very quickly--that you had to be careful with them. I went through this learning stage about how you have to polymerize those things as you feed them in. You just can't go and load up a big barrel or kettle full of this stuff because you'll blow it up. I did a lot of work on trying to make the butyl ether of propylene glycol with BF_3 on a continuous basis. This works very fast, but you never could get all of the fluorine out of the thing. Fluorine would cause corrosion. But that was in my early stages. This was just in the lab. Then I moved over to the pilot plant area. In fact, I moved into a big old warehouse.

BOHNING: The Butler buildings?

PRUITT: Yes. The Butler buildings. They didn't even have a concrete floor, only a dirt floor. My first lab I set up over there was in the rest room; it was the only place with a concrete floor. [laughter] Absolutely. It was a building about the size of this room, and it had two stalls at the end. I set up my lattice racks in them, but I couldn't stand that very long. Finally, I built another lab. I started to work all kinds of things, particularly trying to figure out what we were going to do with alkylene oxides. We developed the series of ethylene glycols, polyethylene glycol, polyethylene-propylene glycols using alkaline catalysts but only to fairly low molecular weights. What was always puzzling to me, particularly with ethylene oxide, was that we would make an E600, (that's code for ethylene oxide of 600 molecular weight, which we would determine by end-group analysis), but one time it would be liquid and another time it would be solid. How could this be? All this was in the beginning when nobody knew about molecular distribution, which we had no way of even measuring. I couldn't figure this out for the longest time. How one time I would make these things, I thought the same way, exactly the same way, and one of them would be a solid and one would be a liquid. But somehow, there was something that I did differently. I would change the distribution and would get a ethylene oxide polymer that would be solid.

BOHNING: How did you determine that it was the distribution that caused that?

PRUITT: I think I started trying to skim out the solids by temperature control and measuring the molecular weights. It was very crude, I know, but we were pretty sure that that was what it was. We didn't have any real good method of doing it, just some sort of an oddball separation scheme.

BOHNING: Were you doing this on a small scale or was this part of the pilot plant?

PRUITT: Well, it would be in five gallon kettles. I had several small kettles, one gallon, five gallon, ten gallon; little autoclaves that I was doing this work in. You would feed the oxide in as fast as it polymerized and be sure that you didn't blow up anything. I came very close to blowing up something several times. To feed the ethylene oxide we had these cylinders that could withstand quite a bit of pressure and they had soft plugs in case something happened. I got one one time that had some vinyl chloride in it. Well I steamed it, washed it, and steamed it and washed it. I knew I had it clean. Then I put ethylene oxide in it and used that to feed over into the kettle. After it had set there for about a week, somebody said, "Say, that cylinder out there in the pilot plant is kind of jumping around." [laughter] I dashed out there and sure enough the cylinder was just jumping all over the place. What it had done was blown its soft plug at the bottom and it was just absolutely having a fit. One of the things I learned was that those cylinders are zinc lined. There was enough zinc chloride left to start, slowly but surely, building temperature from the polymerization exotherm to the point where it got high enough and set off a chain reaction. It was at that point that the top soft plug blew and by that time the cylinder had enlarged three inches but it didn't blow up. That's where I learned that ionic chlorides would polymerize the heck out of oxides--very slowly but, with time, explosively.

BOHNING: You did a lot of other work on ionic polymerization. Was that the first?

PRUITT: That was just one learning lesson which led us to using ionic catalysts for polymerizing alkylene oxides.

BOHNING: Was it at this point that you had the first air conditioned building in the Texas division?

PRUITT: Yes. We decided to build a laboratory out of ricebrick. Somebody had come up with a new deal where they were taking rice hulls, mixing it with concrete, and making a brick that had good insulation qualities. It was experimental. I needed a laboratory, and I decided that we would just build a lab with no windows and use ricebrick and it would be cool. It is hot down Freeport, Texas. Then we went out and scrambled up an old heater. Normally you would put steam through it and it served as a heater. Well, instead of putting steam through it, I ran pipes across the street from one of the chlorinated solvent plants and tapped off the cooling brine from their refrigeration line. They had to have brine to run a lot of their low temperature distillations. I didn't tell anybody. I just tapped off their line and plugged it into my blower. I had the first air-conditioned lab. Everybody came in there. It was really nice. It was perfect. I could control that thing and everybody loved it. Finally the time came when the owner of the plant, Tom Brown, traced that line and he gave me thunder. Then I had to start paying him for the use of the brine. They put a meter on it. [laughter]

BOHNING: How long did it take him to find it?

PRUITT: Oh, a long time. Six months to a year. They began to suspect they were loosing brine somewhere and they couldn't figure out where. We did this one night when nobody was around. We did all kinds of things to stay in existence. We made midnight requisitions everywhere on the production people and from each other.

BOHNING: In 1946 you became project leader. That may already be the time that you're talking about, but I wanted to ask you about that. What were your responsibilities as project leader?

PRUITT: A project leader was really just the next step upward, where you were responsible for more than just your projects. You would have two, three or four researchers working for you. That way, you then can take on most of the projects or the ones you want done but you have somebody else doing them for you, or at least helping you. As a project leader I would probably have about four people helping me.

BOHNING: At that time you also went to Brooklyn Poly?

PRUITT: Yes. We set out to make a series of polyglycols. Good, solid, water-white polyglycols for anti-freeze and for other uses that would be stable and wouldn't have a bitter taste. Taste is one of the important things in polyglycols and for that matter in anti-freeze, particularly the polypropylene oxides that go into extracts, tobacco and foods. The aftertaste can be absolutely bitter. We worked on all that--to refine the quality of the glycols. Then we started to make the glycol derivatives, particularly for surfactants and detergents. That was our goal. To make good surfactants and detergents for de-emulsifiers, for the oil industry, even for washing powders. We were mainly aiming at industrial detergents.

I'm not sure of the sequence of these, but then we got involved in synthetic lubricants. We got to working with Standard Oil of California. At that time, like today, particularly up in Alaska and Canada, you could hardly run your car in the wintertime. You actually couldn't run it on oil lubricates. Most people ran them on kerosene. That's a very poor lubricating oil. Or they would take kerosene and put a little oil in it. It was a terrible situation. Standard Oil had a lot of business in Alaska and Canada and the high north, like Montana. They wanted a synthetic lubricant. So we started looking into this and we could make very good lubricant with a high viscosity index, but they weren't stable enough. You would run them for awhile and they would become acidic. So, we had to go to work on how to build these compounds and make them stable, and we did that. We got some very good looking stabilized synthetic lubricants. I made many a trip out to Standard Oil in Oakland working with those people. Finally, we got to the point that where we had one so good that they were ready to test market it in Alaska. We were all set. We were making the product and they were canning it. We were all set to test market this product in Alaska. Then, I went to the Gordon Conferences in New Hampshire. They had a conference on petroleum.

BOHNING: Approximately what year was that?

PRUITT: I really wish I knew. I would say 1953. Anyway, this fellow Hughes, from Sohio, came in and gave this big presentation on viscosity index improvers for oil. I remember this conference well. Many people thought false effects were showing up and that there was nothing to it. Others thought it seemed okay. Hughes was really defensive of it. Of course, I just sat and mainly listened. All of the experts in the oil business were divided over whether this meant anything or not. But it meant a heck of a lot, because it was real. Sohio really made a major breakthrough with viscosity index improvers for oil. Viscosity index improvers were certain polymers with a proper molecular weight that would be soluble in oil when hot but would come out of solution when cooled. This improved the viscosity index of the oil, thinner when cold and thicker when hot. The public started using them slowly at first but now have accepted them fully. So, that development put us out of the synthetic lubricants business.

Now, we could take a regular thin oil, thin enough that you could run it but you could thicken it to where it would run in high heat. That just eliminated us out of the synthetic lubricants business. If that hadn't have come along, we would have been big in that area. We had a good deal but it was really too costly and VI improved oils were so much cheaper.

In this period, Joe Baggett and I kept fiddling with the alkylene oxides. I just knew that somehow ionic compounds, salts and things, would work. We used BF_3 and we used zinc chloride. Zinc chloride is a very volatile catalyst, it was too potent. And then we tried ferric chloride. But even ferric chloride dropped into propylene oxide is too violent. So we started cooling it down to -20°C, and slowly dropping the ferric chloride and getting polymer formation. Well, that still wasn't very good but what we found out was that if we make this polymer, which is brown, and if we extracted the brown complex from the solution in acetone and dried it, we had a catalyst that was much more controllable. This was a complex of propylene oxide and ferric chloride, and could be used successfully to produce high molecular weight polypropylene oxide. So we slowly developed this complex by trial and error using kind of serendipity technique. Yes, this was the first time to our knowledge that organo-metallic catalysts were used for ionic polymerization.

BOHNING: I think you have a patent on this (4).

PRUITT: Yes, we have a patent on this catalyst. The way we would make the polymer most of the time would be to take a five or ten gallon rotating autoclave, and cool it down with acetone solution or some other coolant through a cooling jacket, and add the propylene oxide. We would then put in the required amount of ferric chloride complex and seal it off quickly. Then it would go through an exothermic period and a lot of people thought we were going to blow one of these up sometime. But, it went through this exothermic period and then it would just quit and polymerize at a controllable rate, and never blew up. It just went so far and that was it. We had it under good control. We got to where we knew exactly what this was going to be. So we could run a pretty size of batch of monomer because it would always come out the same.

[END OF TAPE, SIDE 2]

PRUITT: We made good high molecular weight polypropylene oxide, both isotactic, what we then called crystalline, and amorphous polymer. We would separate it by acetone solution into the crystalline high molecular weight and the amorphous. The melting point was only about seventy or eighty degrees for the crystalline part. What is the mechanism for this type of polymerization? As I moved into polymerization, I didn't know a thing about polymers. I was working strictly on observation. I had no knowledge about polymerization techniques. This is when I went to Brooklyn Polytechnic for a three week course because I felt that was the best place to go at that time. Turner Alfrey was the director of the school that year. Mark was there as were Overberger and Mesrobian. They were all good guys and I learned a lot. I learned what polymerization was all about. It has been my basis for polymer work ever since. They taught us a lot in that short period of time.

BOHNING: Did anyone else from Dow go at the same time?

PRUITT: I don't believe so. I believe I was the only one from Dow.

BOHNING: Were there other industrial people there?

PRUITT: Oh, yes. I don't really recall--there was a girl there who was very good in math. I don't recall now any of the other people who were there. I remember the professors well. I remember that Turner Alfrey, on the day that we finished, we had a big beer bust right there in the lab. He had a light cord tied up to see who could kick that cord as we kept raising it. We had a big time. I enjoyed that and I learned a lot. They were good in polymerization. That's how I met Mark and Turner and Overberger and Mesrobian.

When I came back I found that course helped me immensely, as far as understanding what was going on. We kept going with this polymer thing, and maybe a year or so after that, Turner came to Dow and became a very important polymer man in Midland, with Ray Boyer and others. I was still trying to figure out how to get the molecular weight up and improve the melting point and try to make it into a plastic, which it was really not suited for. You just can't get that melting point to improve, doesn't matter what you do. You can just keep carrying the molecular weight up, but the melting point doesn't go up.

I invited Turner Alfrey to come down to Texas one time. I said, "Come down here and tell me what I have to do". Well I was disappointed in the result because he could make very few suggestions to help the problem. It turned out that I was in a whole new field that nobody knew anything about. I didn't realize that. Turner was a vinyl polymerization man. This was ionic polymerization in glycols. He couldn't offer any better suggestion than I had because he didn't know any value for this field. He didn't have any experience.

I could see over the years why that was true. We were tackling completely new ground. Then, Charlie Price started picking up and doing the same kind of work. He had been playing around with KOH and propylene oxide, making mostly liquid oligomers. He was really intrigued with our idea that you could make solids. He did a lot of work himself on this. We had a lot of correspondence but I never met Charlie Price till I saw him in Japan in 1965. We had a lot of phone conversations and letters. He had a lot of good ideas. We worked together a good bit. Then a fellow named [Junji] Furukawa over at Kyoto University picked up on this. He became a very good friend of mine. He came to the United States and I went over to see him a few times. He worked in this field an awful lot. It turns out it's better as a rubber, if it's done right. If Dow had been a rubber company, and understood rubber, I'm not so sure that propylene oxide polymers wouldn't be a good rubber today, before all these other new rubbers caught on. You see, if a product ever gets established, it usually does well. With the proper work, which has never really been done on propylene oxide because it turns out now it's too high priced compared to some of the other monomers, that it would have made a very stable low temperature rubber and so forth. But, I was trying to make a plastic out of it. We believe this was the first work on ionic polymerization.

In 1965, the Japanese had a big IUPAC meeting on ionic polymerization at Kyoto University. Furukawa and those people were handling it, I guess. I was invited. Only three industrial people were invited--me, a man from Du Pont, Otto Vogl, and a man from Hercules, what was his name-- yes, Edwin J. Vandenberg. We were the only three industrial people in the world who were invited. At that meeting, they had all these fancy names I had been reading in books and articles from around the world. They were from Russia, England, Canada, Japan and Israel. They held meetings for two days. I sat there quietly and listened. I got a little disillusioned. They would bring up problems and I said to myself if that's a problem, they could solve it in a few hours if they just would do the right experiments. It looked like that the main goal was to have something to discuss.

At the end, Charlie Price got up to summarize the conference. I was sitting back there with Vogl. Charlie said, "We've been going on for two days now and the father of ionic polymerization is here with us but we haven't heard from him". And I said to myself, I wonder who that is. [laughter] Then he said, "It's Malcolm Pruitt of Dow Chemical Company". I never dreamed he was speaking about me. They were giving me credit for having done the first ionic polymerizations through this work which, by patent, I guess was right. He was very complimentary and I thought, well that's why I was invited. I was wondering why I was invited with this room full of these expert academic people.

BOHNING: Would you say that's the difference between an industrial chemist and an academic person? The academics seem to talk about it while you just went ahead and did the experiment.

PRUITT: Yes. A little. However, they are considering the fundamental aspects of an area rather then producing a product which industry tends to do. Discussing fundamental theory is a major thrust of the academic people. As they went through their reasoning I knew about some of the things they were talking about, because I had resolved earlier in my career that my goal in life, (this was before Dow started pushing me in other directions) that I was going to become a world expert in organo-metallic compounds and ionic polymerization. About the second year I was in research, I decided to go down to the University of Houston to get my master's degree. I talked to Earle about it and he agreed. Three of us drove to Houston twice a week to start taking a masters in chemistry. Well, they were just setting up their masters program. They had just started and they had one woman who was running the organic part. She was teaching these courses at night and it was just the three of us and her. They set it up especially for us. We were breaking in their masters program, and this woman, who was an excellent teacher and chemist, was absolutely scared to death of us. Here were three big industrial chemists and she thought that we probably knew twice as much as she did, which of course wasn't true. But all she would do was say, "Read chapters so and so". Sometimes she wouldn't even come to class. I think she got to the point where she was absolutely scared to death to come and meet with us. It would embarrass her. The class was advanced organic chemistry.

BOHNING: Do you remember her name?

PRUITT: No, I don't, but I can see her face.

BOHNING: What about the text you were reading?

PRUITT: I think the text we were using was Gilman (5). I went back to Earle and said, "I think I can learn more by just concentrating on my job and studying on my own when I need to know something, rather than going to that school. At least, until they get themselves set up." So I never did go back.

BOHNING: Did the others continue?

PRUITT: No. They quit too. But the University of Houston now has a nice M.S. and Ph.D. program. But in the early stages there just wasn't enough there to do much with. I did take physical chemistry down there under Dr. [George W.] Drake. He was good. I retook physical chemistry and that helped me because I was working so much with gases and physical properties. I didn't get a very good course in physical chemistry in ACU or in North Texas. I was working so much with gases and all of these conditions that physical chemistry was essential. So I took a course in that from Drake, who had been there for years teaching this topic. He was really good. That really helped me. But we learned hardly anything out of the organic course, except for what we just read for ourselves.

BOHNING: I have some other notes here of things that happened around this time. Looking through your patents, I was struck by the fact that you have two patents on plant growth media (6). How did that come about?

PRUITT: When I was over at plant A in this semi-plant setting, most of my work was in alkylene oxides of some sort. Either polymerization, or derivatives, or lubricants and so forth. We had one patent where we made the triols of glycerin (7), which is a very vital product. It is the guts of the urethane business today. Of course, at that time we didn't know anything about the urethanes. We were trying to make it into a good detergent or surfactant. Then we moved over into plant B into a new research center and I had a new building and a new lab and became director of research of the organic products development lab. I began to take on quite a few things. Out in the field, some of the sales people had noticed that urethanes were taking off in Europe and they were also sure they would be needed in this country. Other people in the U.S. were also looking at them. But there wasn't anything much happening yet. I looked into it and said, "Okay, if nobody else from Dow was involved, I'm going to become the urethane man in Dow and I'm going to develop that technology for our company." So I went to work real hard with my people, setting up one group on the polyol side and one group on the urethane side, developing polyurethane. We came up with all kinds of urethanes, catalysts, and polyols, and all the related technologies. Of course, the first thing we shot after was flexible foam--making polyols to make flexible foams for furniture, bedding, and so forth.

As we progressed, Carbide was coming up right with us, and maybe one or two others, all competing. We were right there from the first, and maybe we were the first. We had a good product, a good system, and we had been big in polyols for a long time. So, my group put Dow into the urethane business. Joe Baggett, who worked closely with me for years, had certain quality of serendipity that allowed him to observe things that usually other people would ignore. Between us we were able to develop a metal complex from iron chloride and propylene oxide that was a new and unique catalyst to polymerize alkylene oxides.

Then I got intrigued with the idea of flexible urethane foams as a growing medium. I worked on that on the side along with Joe Baggett and Mel Handley. So a rather unique fall out from the urethane technology was Nutrifoam.

I don't know quite what got me on to this. I got a patent on this idea and then I assigned Joe Baggett to spend more time with me and had Mel Handley to look after the plant growing. We found out that we could put nutrients in urethane prepolymers, then foam to shape and this would produce the ideal growing media. It was a heck of a good idea except you're trying to replace dirt, which is pretty hard to do. [laughter] But we began to experiment. We wanted permanency, where the nutrient would stay a long time. If you just put in a soluble salt, in no time it's gone. We started putting in nutrient salt that would slowly leach out and would last for years. Ideal for slow growing houseplants. We could use this foamed-in-place nutrient and it would furnish proper fertilizer for three years. All you had to do was properly water them. That's all. And they would grow fine. It was great if you knew how to do it. In fact, I have a Christmas cactus in my house today that stayed in one of those foam pots for fourteen years without anything except water. And then it began to play out and I had to put dirt around it. They were especially good for orchids. Orchids prefer a dry airy medium such as bark. We actually got a florist up in East Texas to set up a big greenhouse and he was very successful in growing orchids in this medium. Then a company in Houston went into business and set up a production line to make a line of nutrifoam pots. But, they just couldn't make enough money out of them. People really liked to grow things in dirt. So we decided that there just wasn't enough business there. This company went with us a good while, but they finally pulled away. We called it Nutrifoam. It's a good idea. We had a greenhouse out there just full of everything under the sun. They were just beautiful. But it was not really a good commercial deal.

Now with my career in Texas, I was trying all kind of far-out things like that because it's interesting and you never know when one of them might click. However, if there's a reason why it's just not going to be commercially successful you can't keep spending more money on it.

BOHNING: Is that one of the reasons why you moved away from ionic polymerization them? You did some of the early work, but you never really published it that much or pursued it within the company. Is that true?

PRUITT: Yes. We never published anything outside the company. I don't think I ever wrote a paper outside of the company. That's the last thing I wanted to do was to tell someone else what we were doing.

BOHNING: I understand. I guess what I'm after is the fact that you did not continue the ionic polymerization work. You moved on to other areas.

PRUITT: Yes, I had to. The company was broadening me as a manager and I began to be involved in all kinds of areas. As I worked with the propylene oxide polymer, I just couldn't get it to perform as a plastic material. We had nobody in Dow who could look at it as an elastomer. I wasn't very experienced with elastomers either. At the time no use was visualized for polypropylene oxide, so I dropped it reluctantly. However I continued to come back to the polymer every time I had an idea of what to do with it.

At one point we got a joint research venture with Goodyear in where they would try to put it into a speciality rubber. But by that time, they had already developed the ethylene/ propylene tri-rubbers and a lot of other rubbers like polyisoprene. They had put so much money into them that there was just no way for this product to get proper attention. They decided that they just couldn't afford it, but I thought for a time they were really going to put it over because they had a pilot plant, working with us very closely on making propylene oxide rubber for specialities. But you can just spend so much money on so many projects and the others were so far along. The other rubbers are probably cheaper now because propylene oxide is pretty highly priced at present.

In the end of our research, I was trying to figure out how to get that molecular weight up, change the molecular weight, raise the melting point. The big problem was molecular weight. How in the world do you measure molecular weight and particularly molecular weight distribution. At that time around (1945-1950) the only way to measure molecular weight distribution was with sand filters or centrifuges. None of these would give us any help. So I got a man from Basic Research named John Moore to tackle the problem of how we could measure molecular weight of a mixture of amorphous and crystalline polypropylene oxide and particular measure its molecular weight distribution. So, I said to John Moore, "You figure out a way to determine the molecular weight of polymers and their molecular distributions." He went to work on this and he has a pretty creative mind. He started out at first trying to see if he could do it by measuring end groups. We could get up to one hundred, two hundred, three hundred molecular weight, and, also there was no way you could determine the distribution anyway. Well, it looked pretty discouraging, but somehow or another, he stumbled on the idea of using beads to separate the various polymer sizes. I guess you know what I'm leading to if you're a polymer man at all.

It began to look better and better. You could take a very dilute solution of polymer, put it through a bead column, and come out with a molecular weight and a molecular distribution. John had invented gel permeation chromatography. Probably the best analytical tool for polymer research that has ever been developed. GPC puts research in polymer development on a very fast track. All companies took to it. Are you familiar with GPC?

BOHNING: To a certain extent.

PRUITT: If you're not acquainted with GPC, you ought to be. It is absolutely unbelievable. What it can tell you about polymers to correlate with properties. But even with this tool we couldn't figure out any way to get the melting point up on polypropylene oxide. By this time, we had gotten involved in polyethylenes. We had bought the ICI process, and had built a plant. At first I wasn't backing up the existing product lines. That was done by the manufacturing groups. But, I was involved in trying to find new copolymers of ethylene, things such as the ethylene-acrylic acid copolymers, which is just now beginning to be a very big business.

I was just telling our new research director yesterday, "Don't ever let sales business people and the manufacturing people get involved in backing up your products without research input because your technology will slowly decline." We ran into such trouble with our products that we were losing almost two million dollars a month on polyethylenes. This was way back then. So the business managers had decided to drop the business. Well, a manufacturing manager named Dave Rooke in Texas and I decided to take a shot. "I'll take the research and Rooke will take the manufacturing." We started trimming the product lines out and if I hadn't had GPC I would have never had made it. But I'm telling you, you put a polymer in that GPC machine and in an hour or two, it will plot out the molecular weight distribution. If you get the same molecular distribution curve, you've got exactly the same polymer, every time. So we could take a competitive Carbide product, say, and get the molecular distribution. Where they had a good product and were eating us up with it, we could quickly duplicate that and make a similar product, or one that was even better, in no time; with the guidance of this instrument. We improved our product line almost overnight. They thought we had performed miracles, which in a way we had. We got the polyethylene line back on track and made a highly profitable product line out of polyethylene. Much of that was due to GPC. GPC was unbelievable and I think that everybody in the polymer business knows about it by now.

We could get very little attention to this instrument in Midland. I came up to Midland and pleaded with them. We were already doing great things with polyethylenes. I even tried to interest the sales department to use it in their advertising to show how good we were with polymers. One day I told Ray Boyer and Ray Boundy, "If you don't think this thing is very important, I'm going to sell it because there are people who are going to be on our back and are going to invent this and Dow is going to lose out." They said, "Well, fine." I did not look back. I went home and I got our local lawyer at Dow and we sold this technology to Waters Associates. Are you familiar with Waters?

BOHNING: Yes.

PRUITT: It was a small upstart research company. This fellow Waters was just beginning to develop a business. It was the best thing we could have done in research, because he was willing to put a lot of time and money into it, compared to a larger company like Foxboro. To them it would have been just another product. Anyway, we went to work with Waters and started developing GPC and before no time we had this all over the industry. John Moore, was the inventor; he was the one to develop the idea and technology. He was in demand to give talks all over the world, mainly to the analytical type folks. Of course he did a lot of it to help Waters promote the product.

[END OF TAPE, SIDE 3]

PRUITT: Dow didn't get much credit for GPC because we let Waters take the front and ever since then, whenever I go out somewhere and we talk about polymers, I say, "Do you know about GPC?" and they say, "Oh, yes." I said, "Do you know when it was invented?" "Well, no." So we have never gotten proper credit nor did John Moore get proper credit for inventing it. It wasn't long ago that a couple of people from Bell Labs called Ray Boyer. It was just about a month ago. They said, "Where's John Moore? We don't think he got enough credit for his developing GPC. He didn't get any awards or anything for inventing this." They had been looking in the Chromatography Awards in C&E News, hearing about this person and that person. So Ray called me and got me in contact with them and I said, "Absolutely. I think he never received the proper credit, nor did Dow get credit for this." John Moore is now retired and living in Florida. I think that GPC is probably the greatest invention to help fully develop a polymer line. The reason it came into being is because, if you want to invent something you should have a real need and a demand that it be filled. We have more inventions, particularly in instruments, because there's a real need and you tell your people that you have to have an answer. Then they know exactly what they're shooting for. Somewhere, if you have a bunch of smart people, you will get the answer.

For instance, we were doing a lot of work on rigid urethane foam for insulation. All the work in Midland had been done on polystyrene foam. Well, I got to working in Texas on polyurethanes. The important property for foam is to measure the K factor--the insulation quality. The instrument we had, which was developed, I guess, in Midland or somewhere, took twenty-four hours to get a K factor on a piece of foam. When I ran into that I said, "That's absolutely antiquated. We have to beat that." I told my people, "I want a machine that I can take a piece of foam, stick it in a slot, and it will read off what the K factor is, just like that. I want a machine like that."

They went to work and they almost invented it. They came up with a machine that would read it off in ten minutes. I assume that is still the standard today. But, we invented more instruments in Dow because of need. A person who's trying to come up with some kind of instrument working at an instrument company in just an open room has a difficult problem. But when you have a need, it really directs your effort. I believe most instruments are invented that way.

BOHNING: You also did some work on rocket propellants.

PRUITT: Yes. We got into a few government contracts. We had a fellow who worked for me, kind of indirectly. His name was Wallace McMichaels. He came from a government plant and development lab. He was in this arsenal up at McGregor, Texas working on propellants for Phillips. He then came to work for Dow. So he kind of plugged in the idea that since, polypropylene oxide has a lot of oxygen in it and burns easily, it would make an excellent binder for a rocket. We did a lot of work on it but we never did know enough about it. Frankly, the company didn't show any interest. We would develop things and we had no way to follow through. If you don't have a way of moving on a project sometimes they just die. I'm not so sure that we couldn't have done a lot more good but there was just a lot of other things with higher priority in the company than this.

We never did get too far with it. I regret that sometimes. A lot of things we could have let up on and done better but there were just so many things coming at you. They throw the whole polyethylene research at you, and you have to deliver. Some of these unique urethanes uses sometimes got put by the wayside. I'm sure if somebody took enough time out to find really good uses for polypropylene oxide they would find them. But, we just didn't have a way of getting to that.

BOHNING: I want to come back to that point later. At this point, I have some names of people that you were associated with in Texas. You've already discussed a few of them but let me mention them and see if you have any other comments you wanted to make. Earle Barnes especially, because you worked closely with him for a long period of time.

PRUITT: Oh, yes. I transferred from Dr. Wright's to Earle Barnes's research group and I have since then essentially worked with Earle for most of my career. I've temporarily reported to other people but Earle and I have been side by side ever since. We're two of the best of friends. We trust each other implicitly. Earle Barnes probably, in my opinion, is the best executive director Dow ever had or will have. He's truly remarkable. He's genuinely interested in technology and very creative himself. His people skills are outstanding. He has the motivating power to get people to do new things. I guess the most fortunate thing that ever happened to me at Dow was the fact that I worked with Earle Barnes because he trusted me, he believed in everything I wanted to do and he was behind me. That's the reason why I went off and did a lot of screwy things.

Some of them worked. Some of them didn't. So many other bosses would have said cut out that nonsense and get on with whatever <u>they</u> liked.

Earle and I talk even now. In fact, I was talking to him yesterday. He called me from Wyoming. He was an excellent scientist and manager of people and great inspiration to me.

BOHNING: What about Walter Roush?

PRUITT: Walter Roush was a major manager of manufacturing in Texas, under whom Earle worked for the longest time. We were the organic section of the Texas Division. He was a fine man and hard. He wanted you to do right and he expected you to do right. But a top-notch guy. I liked him. He eventually became head of the technology center for all manufacturing for Dow. He's been retired for a bit. He held a good, hard, steady line. Everybody called him Papa Roush. [laughter]

BOHNING: Could you tell us about A. P. Beutel?

PRUITT: A. P. Beutel went down and ran the Texas Division. He was an unbelievable man. Of course, he put the first plant in Freeport because that was the best place to put the magnesium production from sea water. Then he went into the petrochemical business at Freeport. That was the place to do it in the south. If it hadn't been for his ability to fight and get his way with the board, they would have put a lot of those plants up North. If they had, there would probably never have been the present Dow Chemical Company. It probably would have gone under or had much less growth. This was all due to the cheap raw materials available in Texas at the time.

He put all of the large petrochemicals in Texas. He set the tone for the Texas division people--their aggressiveness, their risk taking, their ability to fight and to do everything you're big enough to do. He did that all of his life. Even in his middle seventies, he still showed interest in the research of the Texas division. He stayed at Midland, mostly running their government affairs. When he came to Texas, he never failed to give me a call, "Come over and tell me what's new in research." He was always thinking about the future.

When Dr. Beutel's era passed, that's what I call in Dow or in the chemical industry generally, the end of the age of the patriarchs. We had very creative people like Herbert Dow who started these companies. There were a lot of them around. The Fords and Herbert Dow and so forth. Then as they faded out, they developed a bunch of strong men under them. Men like Strosacker here at Dow, Beutel, and a whole bunch of people who ran their section of the company like they were king, more or less. They were risk takers, they were pioneers and they were tough. The business people didn't dominate them. They did what they told them to do. The business people weren't running the company. They were running the company. And of course, these patriarchs have all died out. Now, we've swung around to the young, business people who dominate Dow's approach to things. I don't mean that in a derogatory manner. I mean they've taken over and they're running the companies. Now, Dow has held out longer on this than most companies and they are being swung that way pretty strongly now. Most companies have gone that way: Carbide, and Du Pont, etc.

There are phases that a company seems to go through. Dow has been very fortunate. Because of the above background, Dow Chemical has been dominated primarily by research and production. They held the power in the company. That means we have had strong manufacturing and strong research and strong development of new products. When you swing around the other way--like Carbide; Carbide has gone from one of the toughest research competitors that we ever had in the 1950s and 1960s to almost a has-been because they've decimated the R&D and their manufacturing technology. That's the reason they are having trouble in certain places. I hope Dow doesn't go that way. I was talking to the new director yesterday and he said he's not going to let it happen. I think that he just might be able to do it.

BOHNING: Did you know E. C. Britton?

PRUITT: Yes. I knew him well.

BOHNING: Did he come down to Texas?

PRUITT: Yes. In fact, he was one of my best buddies. I worked very closely with his laboratory. I knew him well. When they were building a new research center in Freeport, we had this old group of buildings over in plant A but we were going to build this new research center in plant B, away from the Gulf a little bit. I don't remember all of the factors now, but they were just going to put part of us over there. I didn't have a laboratory there. Doc Britton came down one day, visiting and looking around. In looking through the plans for the new research, he said, "Where is Mac Pruitt's laboratory? He's not over here." The next day, I was called up, "Get in here and plan your laboratory." So, he was my buddy. [laughter] I got me a brand new laboratory out of it. Dr. Britton was quite a guy. He was a storyteller, inventive, had a million ideas and held many, many patents. They don't have them like that anymore.

BOHNING: The last name I have here is C. M. Shigley.

PRUITT: Yes. Shigley was in the inorganic side of the Texas division. His research department was called the inorganic group and it backed up the magnesium and everything we had in the way of inorganics. He ran that for years. Actually, one time, he was the plant manager for Dow Magnesium Corporation. Then he moved back over to research; then he came up to Midland when Dr. Beutel took over government affairs department for Dow. He was a good friend of Dr. Beutel's. He became a part of the government affairs department with Dr. Beutel. He was a very nice gentleman and quite talented. But he and Earle were kind of in competition with each other for Dr. Beutel's favor because Earle ran all of the organic and Shigley ran the inorganic side. Earle never became the full director of research for Texas. When Earle became general manager of the Texas division and then moved to Midland, Shigley moved, and then Levi Leathers became the director of research. It was the first time we ever had a full director of research in Texas. Then I followed Levi and was director of research of all of the Texas operations.

BOHNING: Was that in 1965?

PRUITT: 1967.

BOHNING: What was the relationship between Dow Texas and Midland during the period between 1965 and 1971, before you moved to Midland?

PRUITT: It was good. We did a lot of good research in the 1960s particularly in new products, urethanes, epoxies, all kinds of copolymers of ethylene--I could just name dozens of them. During this same period the Midland location had two different laboratory groups. The Independent laboratories for basic research and the Michigan Division laboratories. For many years the independent labs had been the only labs and the one who had developed most of the products for Michigan Division. But now they were the independent labs and many felt they should go with the trend in the 1950s and become more academic in purpose and projects. More highly scientific. While others in the labs felt they should continue to back the old products lines.

At the same time the Michigan laboratory had been relegated almost to quality control labs, so these two laboratory groups were not as effective as they should have been. Very little new product development took place. This was the late 1950s and early 1960s.

Texas was at its peak because we had the freedom to take on whatever we felt was good for Dow. This freedom was in large measure due to Dr. Beutel and Earle Barnes. I was the new product director while Levi Leathers and H. H. McClure had the process departments. Sales and TS&D at Midland turned to us for helping them develop new products. We worked very closely with them. So the 1960s was a very fruitful period for the Texas Division research. Dow started a business team concept in the company, and I was on one of the two first business teams that they had up here. We worked very closely with the business people in technical service.

That's the reason we were able to develop so well because we were just hand in glove and there wasn't anybody in the top management bothering either of us. We just did our thing and if we were on something that wasn't any good, we just buried it and didn't tell anybody. That's the ideal way to do product research. If you carry all the decisions about new products too high in the company, you have trouble because the top management cannot afford to fail. Guys like me down in the trenches, in the labs, we could fail all day long as long as we could bury them; nobody knew how to get in there and figure out what we were doing. Just as long as we were successful enough on most projects. We worked hand in glove with the product departments. We didn't blame anybody. We protected each other and we developed the products. We had people like John Donalds and that kind of Business Manager helping us.

So, we came up with all kinds of new products and processes in the sixties. The relationship between Texas and the business and TS&D was excellent. I would say the relationship between the central research and Texas was good but still with a little jealousy between the two groups. This was one reason why we couldn't get GPC accepted. We were the young upstarts.

BOHNING: Is this when you started the work on the polycarbonate resins?

PRUITT: We had gotten bisphenol A and all the raw materials for polycarbonates. In about 1965 I decided that Dow ought to be in the polycarbonates now, somehow, somewhere. It was tough because Bayer had all the patents and GE was there, both good. So I went to work trying to buy the technology from Bayer. I made a trip to Germany and went to their plastics people. They said you're crazy. The market is not going to grow and you don't need to be in there. We're not going to sell you anything.

I came home one time when the president of Bayer was in Freeport visiting Dr. Beutel. I went to Earle's office and said, "Earle, now's the time. Let's sell this guy on licensing us the technology." [laughter] We talked to him and he said, "Fine. Sounds like a good idea to me." I said, "Gee, we've got it made." But, when he went home, he went back to the same people I went to--no soap. So, if we were going to get into the polycarbonate business, we'd have to develop our own technology and get around all the patents.

We slowly went to work in the lab, just one or two people playing around, trying out, doing this, doing that. We had no backing from anybody and we didn't want any backing. We began to learn things and do things and find out things and slowly we developed our own technology, one hundred percent within Dow.

BOHNING: It was in 1971 that you had moved on to Midland. How did that move come about?

PRUITT: Julius Johnson was director of research. He had taken Ray Boundy's place. But management they wanted Julius to move out of that job and take over the worldwide pharmaceutical business. By that time Earle Barnes was president of Dow USA. We had Dow USA and Dow Europe areas. Earle wanted me to come and take Julius's place. I'm not so sure about some of the board members. They probably wanted somebody up in Midland, I'm sure. Earle really had the choice because he was president. He called me and said, "I'd like for you to come up here." One thing I had said was that I would never move to Midland, Michigan. That's the last place where I would go. [laughter] I had been coming up here twice a month for years to all of these business teams. That's a burden--all of that traveling. You've just been through it. Traveling up here is worse than living up here. Anyway, I didn't like the idea of leaving my home. But Earle said, "How about coming up temporarily for about six months." Well, I couldn't say no to that but I knew what he was doing to me. [laughter] He said, "I'll rent you a house and furnish it for that six month period."

So, I moved up here in this place they had picked out for me. I believe I moved up here in May or June. But I knew I was hooked when I moved. You rationalize that well, maybe I'll get to go back. [laughter] Frankly, I began to like it immediately. The challenge of the job and what was going on and I knew that if I didn't take it, somebody else would and they might not do as well as I thought I could do. In September, I became permanent and took the job. So Earle snuck up on me and got me up here. I was fifty-seven when I moved up here. And that was another thing. I thought,"Why would they want someone who is fifty-seven?" Today they wouldn't do that. They wouldn't even think about doing that. But I think it's wise to take people who have been there. I knew this company like a book and probably knew research about as good or better than anybody in the company. Earle knew that. The feeling today is that a good man can do anything anywhere you put him, and the younger the better. This is good and bad. If a guy hasn't earned his spurs, it demoralizes a lot of people. You don't want to make too many moves where you put people who haven't earned their spurs. Well, I had earned mine. I had been there a long time, done a lot of good, developed many products, been involved in everything up at headquarters for years, business people and all. So I wasn't a stranger to hardly anybody.

[END OF TAPE, SIDE 4]

BOHNING: Recently I came across the following quote from Chemical Business(8): "At Dow, the new emphasis on commercial development looks very different from its old approach to new products. In the past, researchers would have done most of the early commercial development themselves, isolated from the realities of the marketplace. They would then have to twist the arms of Dow's different divisions to adopt a project and pay for further development. In other words, they were creating something new and then looking for a place for it. Now, Dow pushes researchers to modify existing products to supply whatever the customer wants." In other words, listening to the marketplace and then going back to R&D.

PRUITT: Was this a recent quote?

BOHNING: Yes, in Chemical Business.

PRUITT: What you're saying there, if I heard it right, is kind of contradictory. I was going to say that it's right and wrong. It depends on who you talk to at Dow, what answer you'll get about anything. I think, frankly, Dow has done an excellent job of developing new technology. It has been from the inside most of the time. To me, that's a way to stay solid and sound. When business and financial people get control of a company, they don't know very much about technology. They want to make their mark. How do they make it? Financially. So they start making it by acquisition. And this is what Dow is starting to do right now.

Second, the business man is not willing to take a risk. He wants a brand new product badly but he's not willing to take one that will take him five years to get it. He wants it to sell ten million pounds tomorrow. That's just natural. So what do they do? Acquisition. Sometimes you can do okay with acquisition. Most of the times, you lose your shirt. Carbide lost their shirt. They went acquisition. They destroyed their research and went acquisition. Most companies lose out by going acquisition rather than staying with their own technology and being creative. Your researchers will give up if you're seesawing them up and down. We have done that, but not as much as other people.

The last thing that you said there was that we develop off of our existing products. That's what Dow has been the best at in all this time. If we once get in a business we are tough. I've always said that being a good second a lot of times is better than being a first, particularly if there is plenty of room for innovation. I would never let my people go into a new area like polycarbonates unless we offered our own share of technology. We're offering our part of it and not just taking somebody else's ground. That's where you start from. And that's the way you get around the patents. But that's not where you end up. You end up by contributing your share of the technology otherwise, you ought not to be in there. I ought to reread this because it kind of sent me in different directions. In today's world the big commodity products have got it pretty rough--a lot of them have. The fact that the Arabs and others are going to take that cheap oil and gas over there and make polyethylenes and a lot of other stuff makes for pretty rough competition. The chemical industry people in this country are scrambling to get higher value products, more diversification which I think is good. However big volume chemicals will be Dow's bread and butter for a long time.

Dow's goals today and what they're trying to do is correct. There's no doubt about that. I assume the way they're going at it is all right. I don't know all the details of what their plans are exactly but it's no doubt that the present management feel the business people should run the company. There are a lot of ways to do things. All I say is if they do, do not let the research and the manufacturing technology get too dominated by the business people. If you do, you'll lose your shirt. Do not lose your technology. It has to have a strong voice, and research and production has had a strong voice at Dow. Maybe they have to lose that a little bit because of all the government regulations and all of the environmental problems and a lot of other things; manufacturing in Dow can't go off and do their own thing by themselves anymore.

The sophistication of the products themselves demands a very close tie with all functions. That's okay. But, if they will maintain very strong R&D function in the company, where they can have their say so and they get their input, then I think it's okay. I don't know whether I've answered your question.

BOHNING: Yes, that's fine. I wanted to get your response to that. And I guess that really leads me to some of the things that you did as research director here between 1971 and 1980. There were a number of things that were quite innovative. Probably your colleagues in similar positions at other companies were not thinking along the lines that you were. For instance, you helped developed the product stewardship concept.

PRUITT: Yes, I did help develop that. But before that, what got me in that frame was, back in 1969 when I still was in Texas--we had a big explosion in Texas that killed a lot of people. It was because of a mix-up about a product being manufactured at Midland and then sent to the Texas manufacturing people. Ray Boundy, the Company R&D director and Earle Barnes, the manager of Texas Division, told me, "We want you to create a program of some kind that will never let that kind of thing happen again. We just have to have some way to prevent this from happening." So, I was assigned that task for the company. I went to work at that and created what we call the Reactive Chemicals Program [RCP]. I got a lot of people together and we developed the RCP program to help prevent explosions. We go through a lot of rules and regulations about handling chemicals and everybody who is the owner of the chemical, like a manufacturing plant or a laboratory, is responsible to see that this don't happen. He has to go through all of these check lists. We have a manager of reactive chemicals in the company today who watches after the program.

I was a manager of this program all of the time, but when I and became director of research in Texas, then I gave the program to Safety. They kind of let it drop. Dow had another rash of explosions that killed some people. Earle came to me and said, "Look, I want you to take that back and never turn it loose, it's yours." And I put strength back in the program and we haven't had any problems since. A fellow named Doug Roush runs that now. Then I came up to in Midland and I began to run into all of these environmental concerns, worries and things about what's going to happen to this chemical, and what's going to happen to that product? So, I set up the product steward system where every product has a steward assigned to it. If anything happens to anybody with that line of products, then that steward is directly responsible to me. If we got in trouble, I'd call him on the carpet and say, "How come you let that happen." Every product has a steward. Generally the manager of that set of products is the steward. He sees to it that all of the data is obtained, handles all the problems, know who we are going to sell it to, and everything about it. It works. Boy does it work; because they know that I have it all written down on a computer--who is the steward for these products. They worry about those products [laughter] because the minute we get in trouble, the first one I call is the steward. "Come over here and tell me what happened." I mean they don't like to come.

Doug Roush is also the director of product stewardship for the company. We try to keep this concept in high visibility in the company. I think it's a very effective system. Management backs the program all the way.

BOHNING: That brings up another interesting point. What were safety considerations like in the early days in Texas?

PRUITT: Well, they probably weren't very good. I don't remember them being bad because I think the people in Dow were always pretty safety conscious. But, we didn't have much of a program. We kept having a lot of lost-time accidents. Du Pont was always so good, and Dow's record looked pretty bad by comparison. Finally, we just said to ourselves, "Look, we're going to stop this nonsense." That was the top management, Beutel and other people talking. We just put in a program where everybody is responsible for safety--everybody. Now we're as good as Du Pont.

But, it's been a hard, slow build-up. I don't remember it ever being real bad in Texas. We would think it was probably pretty bad today because I pulled more stupid stunts myself personally that probably would have gotten me fired today. I can remember one time when we were making diethers. You use sodium metal. And we would take toluene and put in big hunks of sodium and melt it. Then we started putting in the polyols to make the sodium salt of the polyols and then add some methyl chloride to make the diether. One time, we had about a 250 gallon kettle of this toluene-sodium soup stirring and ready to make some diethers and the steam went off on the kettle. We couldn't get any steam, and it solidified in there. So, now what in the world do you do? You can hardly melt such a chunk. Then the steam went back on, but trying to melt through three or four feet of gunked up, solid sodium-toluene. What do we do? We decided that we would just pull it out. We got a winch and pulled the whole glob out and laid it on a tarp on the deck and started chipping it off with axes. Can you believe this? And, of all things, it began to rain. We got a tarp pulled over the top of it so it wouldn't rain on it. We chopped enough stuff off of it to get that thing back in there. Just think, that could have burnt us to a crisp. One flash and we would have been gone.

We used to get our pipes stopped up with sodium. What did we do? We would be up on top of the structure and we would just throw it out in the drainage ditch that was out there. It would sit there for a moment, and then, boom, up it would go. I pulled so many things in my time. But, for every one of those today, you would probably have to go through paperwork and study before you could do any of that.

BOHNING: At the same time that you were looking at this Product Stewardship, you started the CIIT, which I guess you originally dubbed the American Institute of Industrial Toxicology. You had already established a toxicology lab here. How long had that been in existence?

PRUITT: Oh, a long time. Don Irish had established the first tox lab in 1934, worrying about the toxicology of chemicals long before the environmentalists had ever thought about the problem. Of course, it wasn't that all sophisticated and all that good because of the state of the technology but they had at least started a program. Two years ago we celebrated the <u>fiftieth</u> anniversary of our tox lab. And, we have been the leader in industrial toxicology, leading everyone else in the world. You can't believe how long and complicated a tox lab is. You might want to see our tox lab sometime. That is a big place and they are tops. Many people don't want to trust our data, but we've got the best data in the world.

Anyway, for the reason behind the CIIT. I was now in R&D headquarters and we had a lot of problems but the one that really got to us was vinyl chloride. They found five people with cancers and then they accused the industry of covering it up--and everything under the sun about vinyl chloride. None of this was true. At reasonable levels vinyl chloride is a real safe compound. I could almost drink vinyl chloride. I've been around vinyl chloride so much myself. When we were developing vinyl chloride monomer, my job was to determine if it was okay for polymerization. I would obtain samples of vinyl chloride, then would put them in a citrate bottles or even a coke bottle, weigh out so much and then cap it. If it weighed too much, you would just let it vaporize out into the room until we got down to the right amount, and then we would cap it. I think a little vinyl chloride is good for you. I think it prevents cancer. I'm serious. I told my toxicology guys that and they thought I was crazy.

Anyway, these people that died, had been down into these polymer kettles, all this PVC slime all over them, and just saturated them with vinyl chloride and everything else, like catalysts, etc. Sure, no wonder they had cancer. But in a normal exposure... Anyway, do you remember when all of this hit the fan? It was terrible. This was a very vital product to us. I sat and listened to them, and I got so disgusted. I said to my people, "Now, we can't just go test everything. We've got a good tox lab here but a lot of people don't even have a tox lab; very few do have tox labs and we have just a limited number of toxicologists. What we need to do is join up together in the chemical industry on this thing and create a good, impartial, solid way of getting the tests done together." I called a meeting on that and most didn't think that we could ever accomplish such an idea. They thought it would just be too much from a legal standpoint and all kinds of other reasons. Well, I said, "Just throw away all of those negative ideas and go do it." We did and today we have, I think, one of the finest toxicology labs and it is really respected by environmentalists and labor and government and everybody else. All these types of people say it's the finest thing that the chemical industry has ever done. And, I've been real happy with all the effort that everybody's put in to do this.

BOHNING: You called the original meeting here?

PRUITT: Yes, I called it here in Midland in 1974. I just called up seven of my buddies at Monsanto, Du Pont, Stauffer, Union Carbide, Exxon, and Air Products. I invited them out here and presented the idea to them. I found an outside lawyer named Milt Wessel, who is a gem. He's not an average lawyer. He was a big factor in our success. He sat with us to be sure that we didn't do anything wrong, illegal, or say anything, or plot anything. He's been with us ever since. He still is the legal counsel for CIIT--a genuine believer in the concept, creative in his thinking, figures ways to make things workable and legal.

Anyway, we pulled it off but there were many times when several companies were doing everything in their power to block it. But when we finally swung around and got everybody convinced, then everybody piled in and did their part. So many of us got to know each other and the problem of the environment that we now have a network of people throughout the chemical industry. You can call everybody up and they know what it's all about. The smaller companies can go to the Institute and get their answers now. We're getting data quickly. We purposely called it the Chemical Industry Institute of Toxicology. And people say, "Take out that word industry." No soap. We want people to know it's industry. We're going to show you that we can do that and build credibility at the same time. And we have. That it can be operated independently of any chemical company. What the independent board decides at CIIT is what counts. A company CEO? He might listen, sure. But he doesn't have more say so in what's happening in CIIT than anybody else. CIIT's influence helps maintain a very scientific, impartial attitude. We declared at every meeting that all results would be made public and that we were going to build the most respected toxicology institute in the world. It's fast becoming that.

BOHNING: Were there other organizations like this outside the United States at that time?

PRUITT: Not like this. They formed a thing called BIBRA [British Industrial Biological Research Association] in England. It was different, although it had some of the same things about it. Dr. Leon Golberg, the person we picked, for our president had been associated with that and knew something about it.

But the main criteria of success is picking the right people to run it. If you get the wrong guy, you're got a problem. We got a gem in Dr. Golberg. Later he left and a fellow named Bob Neil is running it now. At the beginning, we sure needed Golberg. He was well versed in that area. He's a fighter for truth. He fights very smoothly, though, and gentlemanly. He really put the concept into practice. It took a lot of work on his part as well as other staff and the dedicated members of the founding committee and the board to follow. A lot of effort by good people and concerned companies.

BOHNING: What was the nature of the opposition you encountered at the beginning?

PRUITT: Well, some companies just didn't believe in the concept and also asked if we really needed it. This was in the beginning, but as discussions continued everyone agreed to proceed.

BOHNING: I believe you also started Cooperative Research, where you funded projects at large and small colleges.

PRUITT: Yes. I started back in 1976 or 1977, or maybe before that. Anyway, I set up a university academic section in my staff. Their job was to try and find out all of the good research projects going on in the universities. We combed the universities hard and picked up a lot of information, by the way. This was the seed germination of the CCR thing. Frankly, I had a pretty poor opinion of academic people. I thought they (particularly chemistry departments) were living in the dark ages. I think some of them are, but not many. We have to help each other as we develop new science and technology. Back in the 1920s and 1930s, with Herbert Dow and other people like that, practically all of the work was done in cooperation with universities. They came out of the universities and they went back and worked with the universities. They were just one big happy family. I got to checking and looking and reading about this. Then I heard about big symposiums in Washington stating that we were losing our lead in technology in this country. The Japanese are getting most of the patents, and we're generally losing out. Then some of them would say it was because the universities and industries were not cooperating. That probably was one of the main problems. Then I said to myself, "If that's true, then we ought to do something about it." But just holding a big symposium and inviting the biggest names around that you can find as speakers; going to Washington and conducting a big meeting in a big auditorium, where you talk about the problem and publish the proceedings; and then you go home. What good does that do? Nothing, practically.

So I said, "We're going to have a meeting and we're going to bring the parties that can do something about it together." And then we said, "Who are the parties?" Well, we can't bite off the whole United States but we can bite off the chemical industry. Who then are the parties in the chemical industry that can do something about this, if it is a problem. We decided the parties were the heads of chemistry and chemical engineering departments and the heads of research at the chemical companies and related chemical companies. Okay, so we will call a meeting and bring those people together.

BOHNING: Did you tell them what the reason was when you called the meeting?

PRUITT: In general terms, yes; but we thought that the way to really get the parties together was to meet and talk something in common, technology. So we had a big technology symposium. I ought to show you a copy of the meetings program. We covered a wide variety of technologies. We tried to select the leading edge technologies affecting the chemical industry and then bring in the world's leading expert in that area as a speaker. We had fair success as the program shows. We had Dr. Baltimore, a Nobel prize winner, come in and speak on biotechnology. We had the industrial people and academic people to discuss technology and from there move to the problem of how together we can improve the development of new chemistry and technologies. At the end of the conference, the last half day, we brought up the question, "Okay, have we got a problem between us; and if so what is the problem?" When we brought this question up we aroused pent-up emotions from all quarters. Everybody seem to have a problem. Academia couldn't figure out particularly what the conference was attempting to do, and believed industry was up to something. The chemistry departments particularly had their own caucus with a spokesman who was going to get up and speak. It was pretty interesting. But my proposal at the end was, "Okay, we've haggled this around and we have a lot of things on the table, but the only way you can solve this is to have a follow through. You can't just come up here and waffle this around and go home. That would be worse than ever." So I said, "I am going to appoint a task force to take what has been said at this meeting and go digest it. We're going to have a meeting in a year from now with a different host, and we're going to make new proposals, and we're going to talk about them." I proposed that we should form some mechanism of continuing our discussions and working together. I called for a possible institute and I didn't know for sure what it ought to look like, or how it should operate. That made some people nervous. They were afraid this was going to interfere with their present funding from industry. Many of the heads of the chemistry departments were very leery of the whole idea at first, but as time went on many of the early doubters later became leaders in the CCR organization.

Anyway, that just shows what happens when the parties start talking to each other. You can't believe the trust that has developed now. We can go into our annual meeting and you can do most anything. Nobody raises an eyebrow. Before you could hardly say anything. "Now what do you mean? What are you fixing to do to us?" The main problem we have now is penetrating deeply enough into the chemistry and chemical engineering departments, as well as the industrial research organizations. We need to figure out a way to get deeper than the heads and get on down to all of the professors as well as to the bench scientists in industry. Slowly, we are doing that.

This has been about the most rewarding experience that I have gone through. It's been frustrating at times, but to see the change in attitudes, the trust and communications improving, has been very rewarding.

[END OF TAPE, SIDE 5]

PRUITT: I, along with other industrial people, didn't appreciate the problems that the professors really had with their funding, or their problem of money raising for their research. They're all out there on their own. If you don't raise any money, you just don't get anywhere, and that's a problem. I've learned to appreciate a lot of things and so have a lot of people in the industry and so have the academic people. You ought to attend one of these annual meetings. They are great. Everyone just loves them. It is a place to talk, compare notes, catch up and personally learn to know people. We make it a point to pay the professors' way because we know they haven't got that kind of money in their budget. So every professor that comes is paid by CCR. I paid their way for the first conference on my R&D budget. That first meeting cost me two hundred and fifty thousand dollars. I took it right out of my research budget. Several of the industrial people asked, "How did you get approval from your management to spend that kind of money"? I said, "Do you want me to be honest? I didn't ask them." [laughter] The first time Paul Oreffice knew I was doing this was when I asked him, "How about being my keynote speaker?" [laughter] It was fine with him, and he agreed to be the speaker. Management at Dow was very much behind the whole process.

BOHNING: When you accepted the SRA Award (9), you described how important it was for R&D to sell itself to the top management. You were obviously very successful at doing that.

PRUITT: You just have to keep them involved. Most people are afraid to communicate with top management for fear of rejection, and that they don't want to hear about R&D, that they are not interested. At Dow they are interested. You have to keep the information flowing. For instance, most people spend a month preparing the R&D budget for the next year. I never wasted a minute on a budget proposal. We had what we called the happy hour at the board. At every board meeting I had a happy hour. At every board meeting I went in for thirty minutes and gave a research presentation to the board. I would take anything from where we needed to ask them for some money to the most exploratory research project out there where I would tell them, "You may never hear of this again, but it is surely interesting." I would literally have to spend hours with these speakers training them to make a good presentation. If you just take them in front of the board, well first of all, they would probably just fall dead from fright. I would spend hours with these people listening to them. The first time they would come into my room, shaking and reading. I would said, "Okay, I'm going to let you read that one more time and then you're going to throw your notes away." "Oh, I can't." "Yes, you can. You're going to speak right off the cuff. You're not going to have one note." By the time they got there, they were just marvelous. You can't believe what you can do with an amateur speaker. I had the board members say, "How in the world do you get all of these good speakers?" I said, "Well, it comes pretty easily." I never did tell them all I did. But they loved that. Maybe one or two didn't. But the board in general looked forward to their board meetings. You know, wrestling around with capital numbers and all of the routine matters is boring. But some good technology...puts spice into their lives.

BOHNING: You also commented in the SRA address that you received the award because of the efforts of a lot of other people. Were there any people that we haven't mentioned that you might want to conclude this part.

PRUITT: I don't know that I ought to mention anybody's name in particular. I think I had one of the finest staffs in the country. Good staff doesn't just happen. The general rules by which we picked people to lead R&D were: (i) Leave a person in one place long enough for him to achieve something. Success raises the self esteem and confidence of a person as well as building trust in him by others; (ii) Never promote a person unless he or she has earned their spurs. Then everyone knows he must pay his way if he is to progress. It also raises morale in the troops, knowing that the person who works hard and accomplishes something will receive his reward. I would often say to my directors and staff when we were assembled together, "In this room, I believe, is assembled the finest technical managing group of people anywhere in the world. You may not appreciate that, but don't downgrade yourself because you are". And they were. As far as I'm concerned you couldn't beat that group of people anywhere in the world, in what they could do. I could ask for anything under the sun and they would deliver it. Of course, in some other field they couldn't but in the fields we were working in, they could. That's the people I'm talking about. They delivered. In the 1970s, we had more problems than you could shake a stick at. Energy costs, environmental problems, capital problems--you just name them and we had them. We had to deliver miracles nearly everyday to stay in business, and we did that. As their leader I would get much of the credit for everything they did. For example, I get a lot of credit for CIIT, but just think of all the people that put in all of the time and effort to make that thing work. I'm not a detail man. In those meetings I don't worry about the details because they are worrying so much about them that I'm just keeping them going --keeping them from getting bogged down too far in the details. We had some great people. In everything I've been in we had unbelievable talents.

BOHNING: What are the major changes that you have seen in the chemical industry in your career?

PRUITT: Well, I have a speech on that (10). I ought to give it to you sometime. I can go through eras, but I haven't the time to do that. The chemical industry is a maturing industry. In the years that I was growing up with it, it was a developing industry and now although still growing but is in a more mature state. That makes a difference. When I first worked for Dow, Dow had $78 million in sales. When I quit, they had sales of $11.5 billion. In fact, I had four times the research budget that we had in sales at the time that I went to work for them.

So, I was in a real growing time with the Dow Chemical Company. From here out, you won't see that kind of growth. It's a matter of taking a mature industry and fitting in what you can and to handle all of the problems facing it. However, the chemical industry is still a high technology operation and still very exciting. New technologies are emerging, anyone of which can have a sizable impact on the business. As synthetic materials replace natural products such as metals, wood, and cement and this will happen more as the properties of plastics, resins, fibers and ceramics improve. Many companies are fleeing from the word "chemical" but in the long term I think the public's attitude toward chemicals will change and those who take pride in it will gain in the long run.

The big change has been all of the things that have impacted the industry from the outside, starting in the 1970s with energy, environment, and regulations of every such size and description. Years ago, when we built a pilot plant we didn't ask anybody anything. We just went ahead and built a pilot plant. We have a plant down in Freeport that we called the instant epoxy plant. We had to build a different kind of plant quickly. I built that plant, and from the start until the time when we turned out the first product was only ninety days. They still call it the instant epoxy plant. Ninety days. And that plant produces six million pounds a month. It's a pretty big plant, although not as big as some of the larger ones. There is no way that you could do that today. It would take you at least a year, maybe two. This is good in many ways; now one has to be sure the environmental factors are taken of before a construction starts.

I guess the other thing is that in the chemical industry, and particularly Dow, because of all of these legalities and the regulations and other outside impacts, the business people have become more and more dominant. Maybe it's a necessity. Frankly, I think it's bad in some ways and good in others. Dow has always had technology dominate the company and I think that is changing rapidly unless we can swing it back. But you do have to honor all of this other stuff. You would be in trouble tomorrow if you didn't.

Another big change is energy cost. The chemical industry of the 1940s, 1950s, and 1960s was built on cheap energy, now it is one of the great expenses you have to consider in all processes. If you don't adapt to that, you're out of business. Cheap energy will never return. In reality, that is probably a good thing because we are fast running out of resources in this world, and somewhere down the road, we aren't going to have any resources if we don't start preserving them. We must all think of conservation and it has been working well. The government should have listened. There was a committee back in 1967 who predicted such energy shortages and that we shouldn't have had oil depletion allowance to provide cheap energy so long. If we hadn't had the oil depletion allowance, energy would have been higher years back and people would have been saving energy way earlier. But they didn't do it. They started giving depletion allowances to keep the price of oil down, and that was a false economy. Therefore, people just didn't put capital in plants to properly conserve energy, including Dow. I remember when I first went to Freeport in the 1940s, you could drive from Freeport to Houston with your headlights off because of all the gas flares between there and Houston that came out of the oil wells. But you don't do anything like that anymore. I know from pictures that they're still doing it in Libya. They have these big flares going, burning up all of that gas.

BOHNING: I remember those as a kid growing up in Cleveland. Standard Oil had a big plant there and that area was always well lighted at night. My last question looks toward the future. What do you think is in store for the chemical industry over the next years? What opportunities are there for young chemists coming to work at Dow?

PRUITT: I have a speech on that too (11). In 1979 and 1980 I was talking about the eighties and beyond. A lot of people believe that now the opportunities are diminishing and in a lot of respects that's true. A lot of chemistry has been found. All of the important monomers have been found. But I believe the opportunity for innovation is just as strong or stronger than it's ever been. We have more problems to solve--energy, pollution, process--and the people who solve them are the ones who are going to win. For example, now there is a process for making paper that eliminates caustic. It requires one-third of the capital, and there is no pollution. It uses a solvent like acetic acid or ethyl lactate, or something like that. You put the pulp in there and heat it up, it all goes into solution. Then you change the temperature and it forms two layers. Then the lignin goes off, and the pulp goes this way and that's it. In the 1970s it was just unbelievable what we could do to save energy when we put our minds to it. If we hadn't, we would have been out of business. That's going to continue. We have to continue to run cleaner plants and make less pollution, with lower energy costs. And everybody is depending on this new technology.

This is what the Council for Chemical Research [CCR] is doing. I consider CCR as a defender of chemical science. That's what it is. University and industry are doing it together. The chemical industry is not properly understood and at present has a terrible image. Washington's knowledge is mainly what they read in the papers and see on TV. Most of government exploratory research is spent on physics and defense. What we have to do is convince the people that everything depends on chemistry. The Pimentel Report is trying to do just that (12). I don't care what you get into, most of the problems are solved through chemistry. Even when there are applications--making a resistor or integrated circuit or something--the basics come from chemistry. We have to convince the people that we need more money for chemical research. We even have to convince the people involved in it that they have a real future and really need to get with it. Many chemistry departments really do live in the past. They are working on some little old deal they've been working on for the last fifty years because they can get funds for it, instead of risking a little bit to get out there and get involved in some new technologies. Some of them want to live a lifetime on one little area. Let me tell you what bothers me, and I don't know how to solve this. Some of the best researchers out in academe are not very good money raisers. And some less creative researchers are the best money raisers. Well, that's kind of true at Dow. Some good, talkative researcher may not be very good in the lab, but he gets along pretty well. Then there's some kind of an oddball guy that's a very good inventor, but he gets pushed by the wayside. I always try to find out about these people.

It's a shame that a professor has to spend so much of time raising money. And, he probably has to modify his research in ways he prefers not to do in order to get a contract. Some of the most creative people we have probably would like to spend their time being creative rather than running around trying to prepare a contract that will persuade someone to give them money. I don't know how to change that. I haven't figured it out yet. I wish I knew how. This is a reason that CCR is giving unattached money to the departments. We tell them, "You put it where you want to put it, and we hope you put it on some good, creative, young person that hasn't much money but has good ideas." I do think that a lot of the professors have broadened their views, and CCR is trying to help do that. There's an awful lot of good ones out there. But some of them still have a pretty hard opinion of industry and profits. I tell them, "What in the heck do you think this money is that you're getting? That's profits. What's the difference between you going out and getting a contract and us having to go out and make a buck off a product?" However, a meeting of minds and an improved communication is taking place in CCR. Both sides, industry and academia are becoming much more cooperative. Neither are violating their principles, they're just being understanding and cooperative. I have great hopes. If we could get everybody to do this, we would be better off. I just hope that everybody, including industry, continues to support this.

I believe the future is bright indeed for the chemical industry. I believe the public and the press are beginning to separate the bad things that have happens from chemicals (which are rather isolated) from the main stream of chemical production. This industry is one of the safest, most responsible, with management of highest integrity than any I know. Most of the bad stories have proven to be false and only in small isolated cases has the environment been affected. Asbestos is bad but we have all known that since fifty years ago, particularly if you smoked. Getting workers to wear masks was almost impossible. I believe the word 'chemical' will one day be back in good graces and most will appreciate the importance of chemicals to the welfare and technology leadership of our country. The industry must help by having the readiness of data available to all who need it, particularly the press. Maybe we should sit up data banks in key universities scattered across the country.

Then there are so many opportunities in new technology. There are new fields being discovered every day by both universities and by industrial exploratory research. Many of these will develop into large broad product lines and some highly sophisticated specialty products. We also have the job of modified existing products to get super properties such as from all our plastics. We have just begun to realize that regular plastics can give such properties if the polymer chain is put together right. For instance, the high modulus and high strength polyethylene fibers or the very high impact polystyrene. We badly need a very high temperature resin for composites that is not brittle. The chemist of the 21st century will have more challenges than he can conquer. He will be look up to rather than be downgraded. The problem will be finding enough chemists to do the job. That will be crucial the rest of this century. Then young people will start flocking back to the profession when they become more popular and the compensation begins to rise properly.

I would love to be involved in chemical research in the 21st century.

BOHNING: Do you have anything else that you would like to add?

PRUITT: Probably, if I could think of it.

BOHNING: Then I would like to thank you very much for spending this time and sharing your experiences.

PRUITT: I enjoyed it. There's nothing I like to talk about better than research and technology and chemical industry. I'm still quite involved. I'm on the phone constantly with somebody, arranging something or trying to do something. Yesterday I was trying to put together a deal in the energy area between the University of Texas and one of the Dow people.

I'm slowly beginning to pull away. I couldn't have wished for a better career. It was absolutely fun all of the way. It has a lot to do with the people you report to. Earle Barnes was my mainstay--you already know that. I couldn't wait to get to work and hardly ever took any vacation. Of course I traveled so much that I didn't need much of a vacation. I couldn't have had a better career. I enjoyed it and I relished it. I think I have accomplished some things that I'm proud of. People have been generous in saying I have. So I look back, pretty relaxed and pretty happy with my life. There are several causes that I would like to tackle yet, but I won't go into those right now. You have to have a lot of resources at your hand to do some of these things and if you don't have that, you just can't get it done. As you get older, your energy level starts to drop and I can't go at it like I used to. I used to work from daylight to midnight.

BOHNING: Thank you again.

PRUITT: You have been very nice and easy to talk to.

NOTES

1. W. N. Masters and L. P. Floyd, High School Chemistry and W. N. Masters and T. A. Willard, High School Chemistry Manual (Dallas: Southern Publishing, 1928).

2. J. F. Mathis, "CIIT: The Jewel of the Industry," Chemical Industry Institute of Toxicology Activities, July, August and September issues 1985. BCHOC Oral History file #0047

3. R. S. Karpiuk, Dow Research Pioneers: Recollections (Midland, Michigan: Dow Chemical Company, 1984) p. 520

4. Malcolm E. Pruitt and Joseph M. Baggett, "Catalysts for the Polymerization of Olefin Oxides," U.S. Patent 2,706,181, issued 12 April 1955 (application filed 5 June 1952).

5. H. Gilman, Organic Chemistry: An Advanced Treatise (New York: Wiley, Vols I & II, 1938, III & IV, 1953).

6. Malcolm E. Pruitt, "Plant Growth Medium," U.S. Patent 2,988,441, issued 13 June 1961 (application filed 18 February 1959. Malcolm E. Pruitt and Joseph M. Baggett, "Nutrient Plant Matrix," U.S. Patent 3,373,009, issued 12 March 1968 (application filed 30 July 1964).

7. Malcolm E. Pruitt and William A. Rogers, "Polyoxyalkylene Glycol Ethers of Glycerine," U.S. Patent 2,831,034, issued 15 April 1958 (application filed 7 December 1953).

8. A. S. Brown, "Commercial Development Grabs the Spotlight Again," Chemical Business 7(10) 1985: 23-26.

9. M. E. Pruitt, acceptance speech, Society of Research Administrators Award, 6 October 1981. BCHOC Oral History file #0039

10. M. E. Pruitt, "A Pearl Without Price. I.R.I Honor Lecture," Research Management, (Sept. 1978): 38-41.

11. M. E. Pruitt and W. E. Brown, "Polymer Futures: A Look Toward the Year 2000," 30th Anniversary Celebration, Society of Polymer Science, Japan. (18 October 1982, Nagoya, Japan) BCHOC Oral History file #0039

12. G. Pimentel, Opportunities in Chemistry, National Research Council Report (Washington D.C.: National Academy Press, 1985).

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