CHEMICAL HERITAGE FOUNDATION

DANIEL W. FOX

Transcript of an Interview
Conducted by

Leonard W. Fine and George Wise

at

Pittsfield, Massachusetts

on

14 August 1986

(With Subsequent Corrections and Additions)

Daniel W.
Fox

JH
3/5/96

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 24 February 1988)

Daniel W. Fox, interview by Leonard W. Fine and George Wise in Pittsfield, Massachusetts, 14 August 1986 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript #0058).

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

DANIEL W. FOX

1923	Born in Johnstown, Pennsylvania on 14 May

Education

1949	B.S., chemistry, Lebanon Valley College
1951	M.S., chemistry, University of Oklahoma
1953	Ph.D., organic chemistry, University of Oklahoma

Professional Experience

1943-1946	Navigator, United States Air Force
	General Electric Company
1953-1956	Chemist, Turbine Laboratory
1956-1959	Specialist, Advanced Development, Chemical Development Department
1959-1962	Manager, Product Development, Chemical Development Operation
1962-1966	Manager, Research and Development, Chemical Development Operation
1966-1968	Consultant, Chemical Technology, Chemical Development Operation
1968-1970	Manager, Research Operation, Chemical Development Operation
1970-1979	Manager, Central Research, Plastics Division
1979-1988	Manager, Chemical Development, Plastics Technology Department

Honors

1976	Plastics Hall of Fame

ABSTRACT

Daniel Fox begins the interview with a discussion of his family background and early education. Fox and his sisters grew up in various parts of Pennsylvania. He credits his high school chemistry teacher with piquing his interest in science. After graduating from high school, he decided to attend Penn State University. He only spent a year there, however, before joining the Air Force and became a navigator during World War II. When he returned from his military service, Fox went to Lebanon Valley College, where he earned his B.S. in chemistry in 1949. He then went directly to graduate school at the University of Illinois, studying under "Speed" Marvel. There he was first introduced to polymer chemistry. Fox was married by this time, and he and his wife had difficulty finding housing in the Urbana area. So he decided to move to the University of Oklahoma. There he worked on nitration studies of diphenols and biphenyls, and received his Ph.D. in 1953. Though the Air Force wanted Fox to serve during the Korean War, he instead did research for the Atomic Energy Commission, synthesizing isocorsitrone. In 1953, Fox accepted a position at General Electric, and spent his entire career there. He worked on various projects, including polycarbonates, PPPO, PBT, and the development of Lexan and Ultem. Fox concludes the interview with his final thoughts on working at General Electric.

INTERVIEWERS

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

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

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INTERVIEWERS: Leonard W. Fine and George Wise

LOCATION: Pittsfield, Massachusetts

DATE: 14 August 1986

FINE: Dan, I would like to start by giving you an opportunity to talk about your early life and particularly anything that might have predisposed you to a career in science. Where are you from? What is your family background?

FOX: I lived in various parts of Pennsylvania. Probably the formative years for chemistry were in Middletown, Pennsylvania, which is a place where a nuclear plant sort of erupted. As a matter of fact, as a child I used to play on Three Mile Island when it was a pond. All of the children in the neighborhood had boats of some sort, or rafts, and we would camp and play on Three Mile Island.

It was my high school chemistry teacher who got me interested in science. He was a very enthusiastic chemistry teacher and he turned a lot of students on to chemistry. I really planned to make a career out of chemistry while I was in high school. After high school, I took a year off to work in order to get the money together to go to college, and then started at Penn State [Pennsylvania State University].

WISE: Were either of your parents in science?

FOX: Neither of my parents ever went to college.

FINE: Do you have any brothers and sisters?

FOX: I have two sisters. One is a school teacher, and the other was a nurse until she gave that up when she became a housewife.

FINE: Are they younger than you, or are you the oldest?

FOX: Actually I have a sister who’s older and a sister who’s younger. The sister who is younger than I is quite a bit younger, about ten or fifteen years or so.

FINE: So you are not really a legitimate middle child? [laughter]

FOX: No. I was the last child for ten or fifteen years. When I finally started at Penn State after working for a year, I was only in less than a year (two full semesters), and the War was going on in the Pacific and in Europe. I felt a desire to get out of school and serve the country, so I signed up for meteorology school. I got ambushed into the Field Artillery and finally got transferred into the Air Force again and ended up being a navigator. Ultimately, I spent about a year and a half in Saipan and made many trips there to Japan and back.

FINE: Were you there at the time of the nuclear business?

FOX: Yes. I was on Saipan and they were on Tinian. We used to watch them take off and go to Japan if we weren't flying. That particular mission at Hiroshima actually came at about the time we thought the war would probably end pretty soon anyway. It was a smashing end to the whole thing to have a bomb drop.

So I came back from Saipan and wanted to go back to school. At Penn State, they had a very overcrowded situation. As a matter of fact, they had one teacher lecturing to three rooms, with a personal appearance in one room and a speaker in the other two. Having come from the Army where I had a lot of people speaking to me over a loud speaker, I had no desire to go back to Penn State. [laughter] So I signed up at a little school nearby, Lebanon Valley College in Annville, Pennsylvania, and decided I would start school there and transfer eventually.

FINE: You mentioned that you were a chemist from the start. Was your high school teacher notable in any way as far as sending armies of kids on into chemistry?

FOX: Not really.

FINE: Was he a great teacher?

FOX: He was a good teacher. He let the students do pretty much what they wanted in the laboratory, and he encouraged them to work individually. He didn't stick with the manuals. He really provided an atmosphere where one could use imagination rather than follow directions.

FINE: Before high school, were you one of these Gilbert chemistry set kids?

FOX: No. I really wasn't. I did a fair amount of what I would call mechanical inventing. I did a little gadgetry, and things of this sort, but I didn't play with chemistry sets at home.

FINE: Was Penn State really radically different than Lebanon Valley at that point? All of those schools must have just been swamped with the veterans right after the war, and even the little schools must have been crowded.

FOX: They were crowded, but not nearly to the extent that Penn State was. There is one other thing. I tried to go to MIT. I took tests and filled out forms, and received a little encouragement. Then I got a form letter saying, "Due to the unprecedented return of former students, we will be unable to take you. You might try again next year."

Lebanon Valley was a little church school. It was twenty-five miles from home and it seemed like a good place to start. I might add that they did have a book shortage there. I had to share a German book with a young lady who was a biology student, and she later became my wife. That little vignette comes from Lebanon Valley College.

FINE: What kind of chemistry department did they have at Lebanon Valley in those days?

FOX: In those days, they only had a two-man department, but the gentleman who was running it was an outstanding teacher. He had worked in industry and came back to teach school. His name was Andrew Bender. Dr. Bender supplemented the school's chemistry budget by buying analytical equipment and other instrumentation from his personal funds. He literally lived and breathed chemistry, and he was the real inspiration for my going further in school.

FINE: Was he retired from industry?

FOX: I think he probably had at least twenty years or more of industry and then came back to teach.

FINE: Was he an "organicer"?

FOX: He was an organic chemist.

FINE: Who did he work for?

FOX: I really don't know. He sent a lot of students to a lot of schools. One that you might put down for the records here is Tom Fox, a well known polymer physical chemist. I think he ended up at the Mellon Institute, but he had worked for Rohm and Haas for a while. He is quoted in many of the classical physical polymer chemistry books.

WISE: He's not related to you at all?

FOX: He is not related. He lived about fifteen miles in the other direction. I had many interesting meetings with him at Gordon Conferences. There was another student whose name is [Elizabeth K.] Weisburger now. She was a Garvin Award winner and is doing cancer research at the Rockefeller Institute. Dave Sheets, who is Director of Research at Dow Chemical, was another one of Bender's students.

FINE: Where did he send all of these people to graduate school?

FOX: A lot of them went to Cincinnati. He had some very good ties at Cincinnati and sent a lot of students there.

FINE: At this point you got a bachelor's degree. Had you thought about going to work or had you thought all the time about going to graduate school?

FOX: I wanted to continue, and I went to the University of Illinois.

FINE: You didn't think about going back to Penn State and working for Frank Whitmore?

FOX: No. But I would say that Whitmore was an interesting character. I had a couple of meetings with him. In one of the meetings, when I left the school, he called me in and literally raised Cain. He had previously informed us that any chemistry or chemical engineering student that had been threatened by the draft should see him and he would take care of things. He said, "Nobody takes my students away from me." I didn't feel the urge to stay on so I didn't go to see him. He was very upset about that and he told me so. I sent my letter of resignation to him when I quit, and I think I had about two spelling errors and a misplaced comma. He wrote a letter and called that to my attention. [laughter] So I have some vivid memories of Frank Whitmore.

FINE: Was this when you were enlisting in the Army?

FOX: Yes. It was 1943, when I was enlisting in the Army, and leaving Penn State after completing my freshman year.

FINE: Was it commonplace to be able to keep science students out of the military at that time?

FOX: I don't think it was commonplace to keep science students away from the military at that time, but I think that Whitmore had sufficient interaction or action with enough people in the government or military that he literally could. He let this be known to his students. The only teaching contact I had with him was that he required all chemistry and chemical engineering students attend a lecture series which he gave personally. Every freshman had to go to his private lecture. He had a large lecture hall of three to four hundred people. They kept very strict attendance on who attended Whitmore's lecture, and if you didn't go, you were in trouble.

FINE: Whitmore was one of the power brokers in chemistry in those days. Actually, he had some pretty close ties with General Electric through the CR&D Center in Schenectady.

FOX: That's right.

FINE: So you went to Illinois?

FOX: I went to the University of Illinois, and had the opportunity of having several impressive instructors.

FINE: "Speed" [Carl S.] Marvel?

FOX: "Speed" Marvel taught Organic Chemistry 344 (or whatever it was). This was a typical organic chemistry course, but it was there I discovered that all molecules did and could have difunctionality. So instead of talking about simple esters, we talked about polyesters. Instead of amides, we'd talk about polyamides. He managed to turn everything into a polymer. It was very impressive. One of the more impressive, formative things that happened to me was having the opportunity to study chemistry under Marvel.

WISE: Was that the first time you had heard about polymers?

FOX: Almost. As a matter of fact, when I started at Penn State, I was signed up as a chemical engineer. But I had switched to chemistry at Lebanon Valley and then got interested in polymers very much at Illinois. I also had [R.C.] Fuson and he was quite impressive.

FINE: Roger Adams?

FOX: I didn’t have Roger Adams. I think he was retired at that point.

FINE: At least he was out of the mainstream.

FOX: There were several other teachers. [Worth H.] Rodebush was there, and [Herbert S.] Gutowsky was just getting started. [Donald R.] Martin was just getting started in inorganic chemistry. There were others, but the most impressive one was “Speed”.

FINE: When you came under Marvel’s wing at Illinois, was it clear to you that this was really an interesting area of organic chemistry and you were going to really look further into it?

FOX: It did. We had a lot of problems. There were some with course work, but not too much. I had gotten married when I left Lebanon Valley. I couldn’t find housing in Urbana so I went alone and left my wife at home. For a couple of months I lived in a field house. Finally I brought her out and we rented a trailer. It burned down in the middle of winter and in the middle of exams. We had just no end of calamities. To get student housing you had to be a resident of Illinois, and you had to be married and have two and one half children. That was the standard in order to get into student housing.

WISE: Even for graduate students?

FOX: It was impossible.

FINE: Because of the postwar bulge?

FOX: That’s right. So I started looking around and wrote to a number of other schools. I received a very interesting response from the University of Oklahoma at Norman, and at the end of the school year we packed up and went to Norman.

FINE: Was Marvel going to let you go?

FOX: I don’t think he really cared about me that much. I was just one of a large group of students. He did have a thing that amused me. The opening announcement in his class was, “Look at the man on your right. Look at the man on your left. One of them won’t be there at the end of the course.” He managed to make that happen, and I was that man. [laughter]

FINE: Do you remember any people from your Illinois days who became outstanding chemists, or was it just too early?

FOX: I would say that it was too early. I maintained some acquaintanceships for a number of years but none of the people became outstanding.

FINE: Have you kept up with Marvel over the years?

FOX: Only when I see him at meetings. Then I do see him and chat with him from time to time. He’s a grand old man.

FINE: Did he ever express regrets that you went to Oklahoma?

FOX: I don’t think so.

FINE: Does he even know that you were at Illinois?

FOX: I don’t even know that. I thanked him on at least a couple of occasions for his input to my career.

FINE: Was Marvel a great teacher and influencer of young people in chemistry?

FOX: Very much so. He put forth a lot of philosophy that I remember. For example, he told the story of a couple of graduate students having lunch in their little office. One of them ate an apple and threw the apple core out the window. The apple core started down and then turned back up. So he made a note in his notebook, “Threw apple core out the window, apple core went up,” and went back to reading his magazine. The other man threw his banana peel out the window. It started down and then it went up. But, he almost fell out of the window trying to see what was going on. Marvel said, “Both of those guys will probably get Ph.D.s, but the one will be a technician all of his life.” [laughter] I remember that. First you must have all the details, then examine the facts. He that doesn’t pursue the facts will be the technician, Ph.D. or not. That was just one of the stories, and he had quite a few of them.

FINE: Was Bill [William] Bailey at Illinois when you were there?

FOX: No. I don’t think so.

FINE: He probably preceded you by a few years. I think he is probably a couple of years older than you.

WISE: Did you do any industrial work at all during the summer?

FOX: No. The only industrial work I ever did was a year I spent in a forge shop at Bethlehem Steel.

FINE: How did you get to the steel industry?

FOX: The steel mill was in Pennsylvania right near home. I applied for the job and worked there for a year. I banked all my pay and started at Penn State. After the War, I had the G.I. Bill to work with.

FINE: So you hopped into your Model A Ford and took your wife to Oklahoma.

FOX: No. It was a 1939 LaSalle, which was a real classy, needle-pointed car. I had that car because I had actually operated a gas station while I was going to undergraduate school. One of the spoils of the gas station was a black, 1939 LaSalle convertible.

WISE: Did you keep up your mechanical interest and work on the car yourself?

FOX: Oh, yes.

WISE: Was that one of your continuing interests?

FOX: Yes.

WISE: You were a hands-on type of person?

FOX: Yes. So I went to Oklahoma. They were trying to grow and they offered me a scholarship, which helped. Eventually they came up with a job for my wife who was a nurse, but was actually a medical technician. She ended up working at the infirmary as a med tech. Between the G.I. Bill, the scholarship (or whatever they called it), and my wife working, I was financially better off at that point than I probably will ever be in the future.

FINE: We all have that experience.

FOX: It's a very nice feeling. I had very few expenses and a reasonable income. The graduate school didn't have anyone working on polymers, so I went back to being an organic major with a chemical engineering minor.

FINE: You really didn't think much about polymer chemistry, one way or another, at that point. It was just incidental that Marvel was interested in polymers.

FOX: Yes.

FINE: Marvel has always said that he never thought of himself as anything other than an organic chemist, and that these polymers were just organic molecules.

FOX: That's right. They just happened to have two functionalities instead of a single functionality. Everything else is the same. He kept saying that.

FINE: Do you adhere to that philosophy to this day?

FOX: Absolutely.

FINE: You don't consider yourself anything more than a good old-fashioned organic chemist?

FOX: Well, I might argue that point.

FINE: Everybody thinks of you as a good old-fashioned polymer chemist.

FOX: I would say that I have different opinions of myself as far as that is concerned. I really am not a first class organic chemist. I'm not a typical polymer chemist from the polymer physics end since most polymer chemists are really physical chemists who do their work on polymers. I'd rather consider myself as an opportunist, able to get a few clues and jump to conclusions that frequently panned out. I would say that characterized my career, rather than my being a polymer chemist. I think I have an ability to recognize something that is valuable or worthwhile or has some applications before it's common knowledge. I have a good working knowledge of organic chemistry, a little understanding of engineering, and a little understanding of mechanical things.

FINE: That's probably good background for the kinds of things you've done, or maybe it is good background for any industrial chemist.

FOX: Quite possible.

FINE: The idea of an opportunist is intriguing.

FOX: I would classify myself as an opportunist. That has happened very frequently throughout my career.

[END OF TAPE, SIDE 1]

FINE: Let's go back to Oklahoma. What was the chemistry department like for a young graduate student at that time? Were there any notable figures?

FOX: There were very few notable personages in the chemistry department. I can recall one little anecdote. When I was looking for a project to work on for my thesis, I discussed the possibilities with all the professors. In all of my interviews I managed to include, “What kind of financial support do you have for this type of work?” One professor accused me of prostituting science before I became a scientist and I had to agree with him. I said that all this was very interesting to me but I would just as soon be interested in a subject that had some financial backing. Thankfully, that was the way I picked the project I worked on. The area of chemistry I selected was the nitration studies of diphenols and biphenyls. This work was sponsored by the Army Ordinance, which was interested in nitrated compounds.

FINE: Were you nitrating bisphenol-A and diphenols?

FOX: Diphenols, yes, but bisphenol-A, no. Actually, bisphenol-A was somewhat of a curiosity at that time. There really wasn’t any around, and nobody was thinking about it. I did a lot of nitration of phenols, chromatographically separated them, made the derivatives, and characterized them using all the classical techniques.

FINE: Synthetic organic chemistry?

FOX: Yes, it was synthetic organic chemistry.

WISE: Excuse my ignorance, but were these explosives? What was the purpose of these compounds?

FOX: Ultimately they could be explosives, but we were not nitrating far enough to make an explosive. What we were trying to do was determine the course of the reactions. Which spot does the nitro group hit first and second? If the solvents are changed and if the conditions are changed, how does the isomer distribution change?

FINE: This was the heyday of physical organic chemistry with ortho-para ratios and kinetics of substitution reactions.

FOX: That’s right. What were catalyst effects on rates and orientation, things of that sort.

WISE: So you were trying to learn the mechanism behind the reactions which the Army would later use to make explosives. Is that the idea?

FOX: I hate to put in the word explosives in here, but I guess most nitro compounds would have some ultimate application in explosives. The research I was doing involved trying to understand how to control the sites for nitration.

FINE: Who was your professor?

FOX: He was James C. Colbert. He was the advisor to the undergraduate students who were going to med school. He had leanings towards biochemistry. I managed to get through the regular graduate school, but a little incident happened about one semester away from getting my Ph.D. and finishing up. It was the Korean War at the time. The Air Force called me up and said they could use me. They thought I had done such a good job as a navigator in the Pacific, flying from Saipan to Japan, that they could use me either as an instructor or navigating planes to Korea. They wanted me in the Air Force immediately. I explained that I was a semester away from finishing my Ph.D., and asked if I could stay away that long. They said that they would let me stay to finish. So I was looking forward, not with a great deal of pleasure, to graduating and going back into the Air Force. There just happened to be a professor who had a lot of unused funding and Atomic Energy Commission supported projects. He asked me if I would like to stay on as a postdoctoral student. I assured him that I would really love to stay on as a postdoc student, providing he could talk to the Air Force. He said that he did not think there would be any trouble, because the Atomic Energy Commission had more influence than the Air Force. So he arranged it. He was a great man.

FINE: Who was that?

FOX: His name was Simon Wender.

FINE: Oh, Wender. Sure.

FOX: There are a couple of Wenders. I'm not sure you know that. He was a graduate of Emory University and he had a pretty outstanding man working with him. His name was Tereskell and he was also a biochemist.

It turned out that Dr. Wender had a lot of Atomic Energy Commission funds with which he had been working on antiradiation compounds. Specifically, they were following up, somewhat late, a lead of a Dr. [Albert] Szent-Gyorgyi, who had discovered what he called the

Vitamin-P complex. He had isolated this Vitamin-P type of material from Hungarian red peppers. (Being Hungarian, I guess it was appropriate.) This isolated material seemed to strengthen blood vessels. The classic example is that people who bruise easily have a deficiency of Vitamin-P complex. If he fed animals his Vitamin-P extract, it would strengthen the blood vessels. In the case of radiation damage to humans or to animals, the blood vessels lose their flexibility and rupture, and you die from internal hemorrhaging. The concept at the time was that if there was anything to Szent-Gyorgyi's thesis, then you ought to be able to isolate some plant pigments that could be fed to animals, strengthening their blood vessels and providing some immunization to radiation damage. That's what the Atomic Energy Commission was supporting.

At Oklahoma, there was a research institute which took over the postdoc work. There were a number of students who had been isolating all kind of plant pigments which were sent to the University of Michigan's medical school for evaluation. They were evaluated by shaving the tummies of rats or rabbits, and feeding them a diet of extracts. Later, a suction cup was put on their tummy and a vacuum pulled for a couple of minutes. The number of capillary ruptures per square inch was counted. On an animal where there was no treatment, let's say there were twenty ruptures. If it had eaten somebody's test extract, it might drop down to ten. That was in the right direction. What we were looking for was something that would drop it down to no ruptures. This was the type of work that we were doing at Oklahoma, in trying to isolate these extracts. A number of students had arrived at extracts of apricots and prunes that seemed to be very efficient. We were isolating them by a combination of grinding them up, stewing them in cold water, using ion-exchange to get rid of the sugar, and chromatographing them. At first, they were coming out in the order of milligrams per kilogram or multikilograms of starting materials. The isolated material that looked best of all was a chemical called isocorsitrone, which is a flavonoid compound. It is a flavonoid that contains a sucrose molecule on the side.

My job was to synthesize this molecule from scratch. If we could synthesize it, then we could have a larger supply. Also, if we could synthesize it, we could make a radiocarbon isotope so we could feed it to the animals and find out what happens to it. One of the fragments that I needed for the synthesis was guaiacol, which is ortho-methoxyphenol. There was no ortho-methoxyphenol around, but I learned that we had unlimited supply of guaiacol carbonate from the Navy pharmaceutical supply. It was used in humidifiers to generate a medicinal odor which one breathes. Supposedly it provides some respiratory relief from clogged up respiratory passages.

FINE: What is guaiacol carbonate?

FOX: It's actually diphenol carbonate with a couple of methoxy groups on it.

FINE: Can you make guaiacol carbonate without using phosgene? How do they make guaiacol carbonate?

FOX: Guaiacol comes from plant extracts. It's the same thing that vanillin is made from. I think they use phosgene to make the carbonate. In any event, I had an unlimited supply of guaiacol carbonate. I only needed about twenty grams of guaiacol so I assumed that the easy thing to do was to put a little bit of caustic in with this guaiacol carbonate and boil it for a few minutes. At the time, everyone knew that carbonates were very susceptible to hydrolysis. You could put a carbonate protecting group on something and do another reaction and take it back off easily. So that was common knowledge, that carbonates are very easy to tear apart with a little aqueous caustic. So I put the guaiacol carbonate in with my aqueous caustic and proceeded to boil it. It just steamed distilled away from the caustic unchanged. So I put a reflux condenser on and left it reflux for a day and it was still coming off with guaiacol carbonate unhydrolized. I had actually taken it home and left it on the kitchen stove so I could watch it over night, in case anything happened. (We were still in our apartment at that time.) I couldn't tear it apart conveniently. I know I could have if I used pressure and other conditions, but it was easier to order it from Eastman Kodak and wait for a week to get it, which is what I did. I tucked that one away in my memory and continued the synthesis. I got about two-thirds of the way through the procedure when my year of postdoctoral work was completed. The Air Force had lost interest in me, so I left and went looking for a job.

FINE: Did anything come of this anti-radiation business? Did you ever follow it up?

FOX: I think that we learned something from it. There was some value from it, but they never really got around to seriously treating people with it. Actually, I can give you medical advice. If you have a weak blood vessel system, it can be strengthened, very considerably, with these materials. The simplest remedy you can take is to eat apricots. They are very rich in this particular compound. Prunes are also very good, but they have some other physiological effects. [laughter] Nevertheless, they are in the same family, but the apricots are also rich in the compound.

One other thing we had learned (I don't know whether they are practicing it or not) was that you could also get a fair concentration of a homologue which has some effect from the outside covering of buckwheat grain. It is called rutin, and it is pretty easy to come by. You can get it for several dollars a pound. I don't remember exactly, but it isn't very expensive. It is not quite as effective as the substance we had found, but they did actually report that they had fed dogs a lot of this rutin for a week or two weeks, and then radiated them. About ninety percent of the dogs which had been dosed with the rutin came through and the ones that didn't have it, died. That is, nine out of ten died and nine out of ten lived. There was a difference between being treated with rutin or not being treated. There were stories, which I can't confirm, that they were giving people dosages of rutin and other similar things, prior to radiation treatment for cancer. I think that the technology has gotten so far now that they do not have these kinds of problems.

One of humorous things that came out of this study was that it turns out the reason that the blood vessels lose their flexibility almost has a parallel in a rubber hose laying out in the sun. When the heart pumps the vessels expand and contract. If you lose the flexibility, when the heart pumps you get varicose veins and things of that sort. It is possible to do a pretty good maintenance job on the body by eating foods and fruits rich in this Vitamin-P factor. It turns out that the loss of flexibility in the blood vessels is essentially an oxidation reaction. The flavonoids that we were after had everything that one would need in a plastic for an antioxidant. They have a ketone ring and a bunch of hydroxyl groups around two aromatic rings fused together. If I looked at the flavonoids now, I would say it probably is an experimental antioxidant. But in those days, I didn't know what an antioxidant was.

The other thing that is different is that these materials occur with or without sugars on them. Without the sugar, they pass right through the body undigested. With the wrong kind of sugar, they pass right through the body undigested. The key thing that separates a lot of them is that the ones that have sucrose or glucose units will be absorbed into the body, while the same compounds without those units will have no effect. The body has the ability to hydrolyze the sugars in the stomach; one of the reasons that they are not quite as effective as they might be is that maybe there is an adequate amount of the stomach acid tearing off the sugar and it is lost.

Radiation is an oxidation reaction. They found there were two things that were quite capable of providing equivalent results. One is a little bit of bisulphite. That has bad effects, but it will also protect from radiation. When small bugs are put in with a little bisulphite and irradiated, they don't die. But you can't experiment on humans with that. The other one is alcohol, which is preferentially oxidized in the body. This provides the perfect excuse to get drunk. All you need to do is assume that there might be an air raid or nuclear attack so you can drink. Unfortunately, when the alcohol gets oxidized it turns into formaldehyde and that isn't very good for you. Essentially any antioxidant tolerable in the body will amplify protection by a reasonable degree from radiation. I think there are some interesting things about the body when it gets old. It loses flexibility, it runs out of antioxidants, and it starts leaking internally from internal hemorrhage. The heart keeps pumping but the blood vessels can't expand and contract.

FINE: When you finished at Oklahoma you started looking for a job. Did you think about looking for a teaching job?

FOX: No.

FINE: You never wanted to play professor?

FOX: No. When I was a postdoc I was making the same amount of money as the professor who was directing the project. I was getting a grand total of seventy-five hundred dollars a year, and that's what he was getting.

FINE: Where did you interview?

FOX: I interviewed at DuPont, R.J. Reynolds, and GE. I selected R.J. Reynolds not because they had a hell of a product, which they didn't, but because they would have to pay an awful lot to get people to work for them.

FINE: Were you interested in Reynolds because of the biochemistry?

FOX: There was some tobacco biochemistry and I knew a little bit about it. But they were advertising because they had just built a new laboratory in Winston-Salem. So I looked at them, but I was really more turned on by DuPont. DuPont had some interesting propositions but they didn't interest me because of the management. It appeared that anyone I talked to couldn't do anything unless I had talked with their boss and I talked with their boss and then I had to get a vice president or president's approval to hire me. I was a bit leery of such a long chain of command.

WISE: Was this the Central Research Station?

FOX: I was at Richmond where they had a fiber place, at Deepwater where they manufactured chemicals, and also at a fabrics and finishes place in Philadelphia.

FINE: Polymers.

FOX: I really wasn't told that then. I met [Pier A.] Abetti at an ACS [American Chemical Society] meeting in Chicago. He told me all about the wonders of GE and I went to visit GE in Schenectady. It was at the large Steam Turbine laboratory. They had a great collection of people there. They were nice people. There was Abetti, a fellow by the name of [Abraham L.] Marshall, Andy Lekenec, George Browning, and their boss, Ed Flynn. They were all damn nice people. They made me an offer, but I had to take a three hundred dollar pay cut to go there.

FINE: Is that right? Isn't that interesting.

FOX: I don't want you to get the idea that money is that important, but it was a factor because they only offered seventy-two hundred dollars. The first thing that happened when I got there was, by pronouncement I was an expert in buccellation, which I didn't know anything about. I was an organic chemist. I guess they decided that they were going to make a polymer chemist out of me.

FINE: This was in 1952?

FOX: 1953.

FINE: So you were a postdoc for the better part of a year.

FOX: A full year. Actually, I had finished up my thesis in 1952 and had a Ph.D. in 1953 at the same time I finished up my postdoc.

FINE: You didn't interview at either the research lab or the plastics business in Schenectady.

FOX: No. Actually, there was no plastics business.

FINE: Well, there was the Pittsfield phenolics place.

FOX: It really wasn't. In 1951 they had just pulled the pieces together and called it the Chemical and Metallurgical Division. So I went to work in the Turbine lab in Schenectady, the Materials and Process Laboratory [M & P lab]. A couple of weeks after I landed there, my boss, Ed Flynn, told me that he had a policy of sending all of his new Ph.D.s out to the research laboratory for a little orientation before they began to work in earnest. Everybody in the Turbine lab was very much interested in a new magnet wire which was being sought at the research laboratory.

[END OF TAPE, SIDE 2]

FOX: They were serious in upgrading the capability of all electrical equipment. Up to that point, most of the equipment had a top thermo rating of 105º C, or Class A, and what they were interested in was a Class B wire insulation at 130º C. GE had literally pioneered the first Class A polymeric film insulation for wire which was a standard around the world. That was called

Formex. Consequently, what they were most interested in was making sure that the first Class B wire enamel was also GE's. So there was a great deal of pride riding on the fact that we had to win that one to get the first Class B wire enamel to be universally accepted. The M & P lab was doing a lot of the testing of the wire for electrical applications. They had a vested interest in the wire program, so they sent me out as their representative to join the team at the research laboratory. At the research laboratory they had been through several generations of materials which hadn't quite made it. When I arrived on the scene, the best thing they had was a glycerine glycol terephthalate polymer containing some drying oils, or triglycerides. This product was made by a man named Maynard C. Agens. He was a pretty forward thinking chemist in his own right. The problem they had with this material was that when they put it on wire, they applied multicoats and they'd run the wire up through a heated oven. They'd come back and put another coat on and put it through a heated oven again. The rate at which they'd run it through the ovens for a given size of wire was, let us say, thirty feet a minute. With the material they had when I arrived at the research laboratory, if they ran it at about ten feet a minute or seven feet a minute, they could attain all of the properties they wanted. But it would be at one-third to one-fourth the commercially acceptable speed. So the setting was, "What can we do to speed up the processing of this particular thing?" What they did literally was to establish patterns where each of the ingredients was varied, one at a time, over different ranges. Where one would go down, the other would go up. They set up all kinds of statistical experiments varying all of the ingredients. When it comes to the drying oil content, this presents a lot of opportunities because there are literally hundreds of different drying oils such as mineral oils, soybean oil, corn oil, olive oil and any kind of vegetable oil that you can think of that might work. Everything had to be cooked and equilibrated together. I was given a fairly large number of polymers to synthesize. These syntheses had two steps to them. First, you made the base polymer, then you equilibrated the oil, and then you put the two of them together and cooked them. It took about a day to make one of these, and I had about twenty to make.

FINE: Had you ever made a polymer before?

FOX: No. I learned how to do melt polymerization within a few days. I learned about catalysts and all the rest of the stuff in a couple of days.

WISE: Did you have some particular mentor?

FOX: Yes. My mentor was a man by the name of Frank Precopio. He was a Yale Ph.D., and was V.P. of what was Am Chemical, which is in Philadelphia. They made a variety of solvents but also made herbicides. They are now part of some outfit out in Minneapolis. He has a pretty good job. He was my mentor.

So I was faced with making twenty of these compounds, one a day. It was kind of boring after a while. The person who was doing the testing on that was Edith (Edie) Boldebuck.

She was responsible for the preliminary evaluation of everything. Instead of making each one of these materials individually and equilibrating the oil, the procedure was to take a trigylceride and put in glycol and equilibrate it to a digylceride. That was put in with glycol and glycerine and dimethylterephthalate and equilibrated again. I decided I would make a big batch of glycerine glycol terephthalate. If I had a big master batch, then all I needed to do was add the oils and cook them. That would cut down on a lot of steps. But, when I made this big batch of material without any oil in it, it looked pretty much like the products that we were eventually making with oil in it. So, essentially for the hell of it, I decided to submit a master batch without any oil.

FINE: Did you tell anybody?

FOX: I didn't tell anybody. Edie Boldebuck checked it and Ludwig came running to me and said, "This stuff is great. It's just what we want. What did you do?" That was the birth of the product called Alkanex wire enamel, which was almost immediately commercialized and still it is being made and sold.

FINE: Do you just get a thermal cross linking?

FOX: There is thermal cross linking or branching on the wire. Up until that point, all theory said that if you had a polyester that's cross-linked, it is brittle. Therefore, there must be some oil. This had some background in the original glyptal varnish resins and adhesives which we made. These essentially were glycerol phthalate and they were all brittle as the devil. They were extremely brittle. Then some GE people, whose names I can't think of now, discovered that if you took glyptal varnish and glyptal adhesive and put vegetable oil into it, you'd get an oil modified glyptal that was now tough and flexible. That became the basis for the the alkyd paint industry. GE claimed a little bit too much on that and we lost the whole thing.

FINE: You didn't get patent protection?

FOX: We claimed the whole thing. We owned the whole alkyd resin and the alkyd paint business, but somebody contested and found out that we had claimed something that didn't work and had the whole patent thrown out.

WISE: Without the patent protection you couldn't continue?

FOX: Without the patent protection we continued to make alkyd paints and maybe still do, I don't know. I think we sold our paint plant, someplace in western Massachusetts. We sold it and all the recipes to somebody, but we were a major factor in the alkyd paint business for a long time. In any event, the teaching had been quite heavy. In order to get flexibility and toughness, you needed oil. That was based on a lot of experience with a lot of our people. At the research laboratory, everybody was convinced that we needed oil and the fatty acids in there. My first material, which was incidentally called, DF-53 for Dan Fox-53rd experiment, was a product that became a commercial Alkanex. I subsequently made DF-54, DF-55, and DF-56, variations which, in my own opinion, were better. From a commercial standpoint they were happy with what they had so they never were commercialized. In any event, we had a product that met all of our requirements. One of the interesting things was that when a polymer was heated in a closed system (a so-called hermetically sealed system) up to 130º C or higher, there was always a little bit of moisture in it or a byproduct that comes from further condensation. We found that in sealed systems our beautiful Class B wire enamel suffered some degradation. Consequently, we were having a session of how we could improve the resistance to hydrolysis of this polyester, to make it even better. It's good enough, but how can we make it better?

WISE: Was this after you had taken the oil out?

FOX: This was after I got the oil out. I'd like to throw in one other name here. Probably the greatest person and inspiration that I worked with was a madman by the name of Abraham Lincoln Marshall. Abe Marshall was a madman from the standpoint that he ran everywhere in a hurry. He was totally safety unconscious. He made commitments to people when he shouldn't. He did all sorts of things but I liked him because he really had the courage of his convictions. He went out and sold this product, this Class B wire enamel, before we had it. He did a great job of selling it. He told everybody, "We will have it. We will deliver."

FINE: Do you mean before the DF-53?

FOX: Before the DF-53 was made, he sold it.

FINE: It wasn't a case where he just knew the experimental results.

FOX: No. He just had confidence that our guys could do it. At that time, our guys at the research lab had reached the maximum that was close to thirty professionals on this project.

WISE: So he sold it and then he put these thirty professionals on it?

FOX: They were already on it. He knew that they were going to win and he knew we were going to succeed. But he had actually sold the stuff to a lot of wire people and motor people. They were waiting for it so when we delivered it, it was expected. Incidentally, the patent on that is in the Precopio-Fox name (1). We had this meeting about what we could do to improve the hydrolytic stability, and that's when I remembered my guaiacol carbonate that I couldn't hydrolyze. My immediate thought was that if we could make a polymeric guaiacol carbonate we would have a polyester that wouldn't hydrolyze. But you can't make a polymer out of guaiacol. You can only make a dimer or trimer. What I needed was this guaiacol that had two guaiacols hooked together by a tail end, and then I could make a polymer out of them.

FINE: Of course, then it's not a polyester anymore, but it's going to be a polycarbonate.

FOX: As far as I'm concerned, a polycarbonate is a polyester. Our patent people argued successfully that they aren't. Any chemist would argue that a polyester is a polyester. In one case, it's a combination of a dihydroxy compound with an isoterepthalic acid or a carbonic acid. They're both carbon acids. I started looking for some biguaiacol. Again, there wasn't any available. I couldn't find any. There wasn't any available in the catalogues. Rather than look it up and try to figure out how to make it, I started looking for something to serve as a functional model. I tried hydroquinone using a plastic synthesis, which was an acid chloride, hydroquinone, and caustic (the Schotten-Baumann Reaction). If you try this in water you immediately get an insoluble precipitate and you can't melt it and you can't dissolve it. I tried resorcinol and I got the same sort of thing, but it was a little bit softer. The next question was, where can I get another dihydroxy compound? When checking, I found some bisphenol-A. I took it off the shelf and ran the same experiment and got a real low polymer. Then I conceived the idea of making my wire coating with kinds of esterification. Why don't I make the polycarbonate by trying esterification. I tried some bisphenol-A with some dimethyl or diethylcarbonate and it didn't work worth a damn. I tried a catalyst, and it didn't work. Then I got a hold of some diphenyl carbonate and put the two of them together and started heating. At a couple of hundred degrees C, I started getting a lot of phenol coming off and the reaction went very well.

FINE: You had a better leaving group.

FOX: I kept cooking it and reduced the pressure as I had with my wire coating. I used the same traps and everything else. I kept checking back and taking the phenol out. Eventually I raised the temperature. It was up well over 300º C. In fact, I was running out of thermometer. The stirrer kept stalling, and I kept twisting it. Finally it wouldn't stir anymore. The thermometer was about all used, so I cut the reaction, pulled the thermometer out and let it cool down. When I did pull the thermometer out, I did pull a long fiber of the polymer out with it. As it pulled down, it broke and shattered. I left it sit for the rest of the day. Don Sargent came in. He had worked for a couple of other companies besides GE and knew something about polymers. He was kind of impressed with the fiber, although I wasn't. One other interested bystander from the next lab, Art Bueche, came over and said, "What is it? It looks interesting. Will the fiber pull or draw?" I said, "What do you mean, draw?" And he told me, "Stretch." He played with the fiber a little bit and tried to propose that they wrap it around a mandrel and let it sit until morning. Chances are that it would shatter but if it didn't it might be interesting. So we wrapped it around a mandrel and let it sit until the next day, and it hadn't shattered. The next day we started trying to take the glass off the mass of stuff on the end of the stirrer. We could get some of the glass off, but we couldn't get all of the glass off because it was really stuck tightly. We laminated the flask and all of the stuff that had delaminated was stuck. We started trying to pound it with hammers to crack the glass off. We couldn't crack the glass off. We just pulverized it. No matter how hard we hit it, it would just bounce and rebounce. So then we started hitting it on the floor and did all sorts of things with it. That was the original glob that sat there.

Some interesting things happened then. The regular DF-53 was being pushed real hard to go to market. They needed more experiments done and slight variations and formulations so everyone was really still hung up trying to get that thing going. I was told that I really had to leave the polycarbonates sit for a while until we got the wire enamel out the door. So it sat for about a month or so.

FINE: When you say "leave it aside," was that because most people were really aware that here was something that was worth exploring?

FOX: I would say that everybody said we would get back to work on it after we got this wire enamel, which was a much more critical problem. Incidentally, some of the people who were involved in that effort on the Alkanex were pretty well known people. One that I had consulted with on solubility was Tracy Hall. Bruno Zimm, Glenn Russell, and Charlie Price were also consulting with us on it.

FINE: Was Russell still at GE when you were there?

FOX: Frank Mayo was there, and I tried to get advice from him, which was subsequently useful on the polycarbonates. They had pretty much a "Who's Who" in the field of polymers there at the time. Art Bueche was there and [Harold A.] Dewhurst was there. Another person who was interesting was Leon St. Pierre. Bob Lukes had just come in. Actually, I think St. Pierre had been to see Marcus. He was one of Charlie Price's students who was working on polyethylene glycol carbowax polymers.

FINE: Were you beginning to get a feel for yourself as really interested in polymer chemistry?

FOX: I would say that I became interested in polymer chemistry during this time. That whole course of the research lasted about six months. It was a very, very good six months, at least for the people I rubbed elbows with and the things that I learned. I learned a lot that I'm still applying to some of my associations.

FINE: You're describing it as a course. Was it really a course, or was it time and experience?

FOX: No. There was no course. It was time and experience. The man who sent me there, Ed Flynn, really sent me there with the purpose in mind that the function of the M & P laboratory was to provide material consultation to the electrical components. In order to provide consultations, you had to know something. One of the best things to know, when you don't know all the facts, is to know people who do know. That was the reason to go to the research laboratory, to become acquainted with all of these people. Then, when somebody asked me a question, I could always say, "I'm tied up now. Can I call you back in a short while?" Then you hang up and call somebody else and find out what the answers would be and then call him back. It's all a consultation. It was really an orientation type program. A lot of people, like Zabeth, did a tour of the research laboratory. A few others did tours of the research laboratory and this was a good basis for an orientation period in, in my case, polymers.

FINE: Did you learn a lot about physical methods while you were there? Did you know what a glass transition was?

FOX: No. Very little. I learned about glass transition temperatures and P.M. [phase modulation] later.

FINE: So you were really still playing organic chemist?

FOX: Yes, I was playing organic chemist. When I went back down to the Turbine Lab after my six month stint I kept playing with the polycarbonates on the side. My boss let me play. I got involved in some other things. I got involved in more varnishes and insulations and things of that sort. I got into binding resins and developed some pretty useful furfuryl alcohol core-binding resins. All of it was in polymers.

WISE: So you didn't mind that you had to leave the polymers and go back to the M & P laboratory?

FOX: No. I would have to say that I was a little bit glad to get away from it.

WISE: Why do you say that?

FOX: Because they didn't do anything in a hurry at the research laboratory. It upset me. I had some personal things. One was DF-53. Bill Gillen was running the evaluation down at the main plant. I sent some of that stuff down to him and he proclaimed it as being excellent but the viscosity was too low. We needed to increase the viscosity a bit so it wouldn't run so much. So I said, "Yes." That was on a Friday afternoon when I heard that. I spent all of Friday afternoon and Friday night making the damn stuff and worked it up on Saturday. He told me to get the viscosity checked, so I turned the thing into the analytical section for analysis of the viscosity. They told me they only ran viscosities on Tuesdays, so I waited until Tuesday. I said, "What did you get?" They said, "We only run six samples a week and yours was the seventh, so we'll get it next Tuesday." At that time I was ready to leave. It was not an efficient operation. I was not cut out for the leisurely pace that people led around the place. They had to hurry up and get their lunch before noon so they could see the new movie. I wasn't used to some of the things and I liked the pace better where you were more of an individual down at the Turbine Lab.

WISE: That's very interesting. You were more of an individual down at the Turbine Lab than you were in research.

FOX: I could call my own plays. I eventually ended up acting as personal consultants to people, providing information; whereas the research laboratory was a little bit more leisurely.

WISE: Too academic?

FOX: Yes, it was too academic. Incidentally, one other guy who was a key man in the wire coating was a fellow by the name of John or Jack Elliott. He ended up at Locktite for a long time.

FINE: Did Al [Alphonse] Pechukus figure into this at all?

FOX: Al came in later. I'm going to get to that. After I went back to the Turbine Lab, Al Pechukus was the manager of R & D here for the newly created chemical development operations in Pittsfield. The chemical development operation was created for the express purpose of studying new chemical businesses. At the time, they had a project going on some cellulosic isolation process.

[END OF TAPE, SIDE 3]

FOX: They were looking for some new projects. Alphonse Pechukus wandered in to the research laboratory in Schenectady and was shown what was left on the mallet. It was demonstrated for him that he could pound it and do all sorts of things to it. He said, “What is it?” We said, “A polycarbonate.” His first response was, “It can’t be.” “Why can’t it be?” “Because all polycarbonates are brittle. They’re all brittle. I know because I spent the last six years with Pittsburgh Plate Glass where there is an awful lot of aliphatic polycarbonate work,” he said.

WISE: Why do you think no one did it?

FOX: I can give you a couple of reasons. One is that everybody knew that carbonates were easily hydrolyzed and therefore they wouldn’t be the basis for a decent polymer. The other reason is that bisphenol-A was not an item of commerce until about two years before that when [V. M.] Castan and Ringley discovered epoxy resins. They needed a bisphenol for it, so they made bisphenol. By the same token, if you looked in the literature prior to 1950, which I did, there were cases where terepthalic acid and isophthalic acid were considered rare chemicals of no commercial application. The polyester business in the States is six and a half billion pounds a year. But they had been uncovered or discovered by [J. Rex] Whinfield and [James T.] Dickson in Europe (3). [Wallace Hume] Carothers had bypassed them. But they discovered them during the war and they were closed down from a secrecy standpoint. They weren’t even available until after World War II. It was 1946 or 1947 before they were even talked about, so the ignorance was pretty general. I’d been taught in organic chemistry that carbonates could be used as a temporary blocking agent because they were easily hydrolyzed. I think everybody else too, was and if I hadn’t tangled with the guaiacol carbonate, I would still think that. There was no good reason to consider the use of a carbonate in bisphenol-A. In the case of Bayer’s situation, they claim they did it by foresight and planning, but I sort of doubt it. [laughter]

FINE: Mylar and Dacron came out at about the same time so it is unlikely.

FOX: Well, I’ll tell you what spurred our whole wire project. That was the availability and appreciation of the fact that ethylene glycol terephthalate was a hell of a good polymer with great insulation properties. It was a thermoplastic which had crystalline problems. You couldn’t top it with crystalline, and it had too low a softening if it weren’t crystalline. It just wasn’t of any use for wire coating. But that served as the basis for our looking at that material.

Up to that point almost all attempts at high temperature varnishes were based on phthalic anhydride which was terrible because whenever it gets hot, it cracks and sublimes. It has no thermal stability, but that was the only common acid that was used in insulation. The availability of terephthalic acid and isophthalic acid as a consequence of the 1941-1942 discovery of Whinfield and Dickson made the whole thing possible. We actually took Mylar film which we were using for electrical insulation, and boiled it up in phenol, dissolved it in the cresol, and tried to use it for varnish. It was not very soluble and it was just terrible. Then the question was, could it be modified to break up the crystallinity and make it soluble. Then it could be cross-linked everybody knows that cross-linked. Polyesters are brittle, so we'll put some oil in, and that was the evolution of the whole thing.

FINE: I see. That aspect of it is really interesting. I would have almost thought that the oil part of it was the up front part and then you found you didn't need the drying oils afterwards.

FOX: Prior to the Alkanex we had made a product which we called Silicanese. They were trying to substitute silicon oils for the normal drying oils. Here they had a product that was reasonably thermally stable in that it wouldn't evaporate or go away at a high temperature, but it had little to no mechanical properties. So they threw out the silicone oil modifications and went to the natural oils.

Incidentally, we were not alone there. Dow Corning had an almost identical product to GE's product which contained silicone oil. They have patents on that. As they found out, their product was slow curing and didn't have much in the line of properties. They kept reducing the amount of silicone modifications until they got down to an extremely low silicone content. What they were really doing was waving the silicone oil over our Alkanex and calling it their product. I think we sued them. We stopped them from playing around the neighborhood backyard. They too approached a similar product by a backward way.

I might add from a personal standpoint, that the Alkanex ended up being fought in patent courts all around the world. I had the opportunity as a new employee to essentially be commuting to Europe for a while, which was pretty rare in those days. I've been in court in England on a number of occasions. I've been in patent court in France. I've been in court in Stockholm. I've been in court in Germany. For about a year or so I was courting, if you will. [laughter] It's kind of an exciting thing to go argue in court. One of the famous ones was in Stockholm where the court hearing was conducted in Swedish. They were opposing a German company who was speaking in German. Our French subsidiary, Thompson-Houston, were arguing in French. I was trying to speak in English. It was just like the Tower of Babel. Fortunately, they had a couple of very capable patent representatives who could handle anything. It was really amazing. That was just an interesting sidelight.

But coming back, the person in charge of the development here was a guy by the name of Bill Cass. He left the company and established Owens Corning Fiberglass Research Laboratory at Granville. Then he left there and went to Northeastern University. I think he retired as a professor at Northeastern. He was a hell of a nice guy. Another guy who was a principal creative individual here on the spot was Eugene Goldberg. He is at Florida State now. He had a long and very excellent career, mostly at Xerox. He retired down there and the last I know he is doing research on polymers which are going to kidney stones and gallstones. For the most part they are cross-linked polyamids. He was the brightest person we had here at that time.

WISE: These people all worked for the chemical development operation?

FOX: Yes. Cass had the early chemistry part.

FINE: This would have been under Pechukus?

FOX: Pechukus had the development. Pechukus ended up shortly thereafter becoming a corporate consultant down in New York City. Another interesting person who welcomed me on the job as my boss's boss was Bob Kriegel. He told me how much I would like it here, and about two months later I made a fifty cent contribution to his farewell party because he left. He was a boss ahead of my boss. I think Pechukus moved on to corporate consulting shortly after he brought the project over here. They had it going more than a year by the time I came here. I guess I came here in January of 1956 or December of 1955.

WISE: How did you come? Did Pechukus go down and recruit you out of the Turbine Lab?

FOX: Yes. Actually, Bill Cass recruited me. He had built up a team and I was independently playing around with polycarbonates over there and they were playing in earnest here in Pittsfield. We had several communication trips back and forth and he asked me if I would like to come. I decided to come and follow the polycarbonates.

FINE: You ended up in Pittsfield in 1956. Who would have been Pechukus' boss? Rueben Gutoff?

FOX: No. About that time Cass left because he didn't get the job that he thought he should and [Albert] Schubert got the job.

WISE: Was he running the whole chemical development operation?

FOX: Eugene Schubert was running the chemical development operation.

WISE: What was your job then?

FOX: I was a specialist in advanced development until Schubert left and Gutoff took over. That's when I became manager. But I spent a lot of time in those days making copolymers with static properties.

WISE: So you weren't directly in the main line of commercializing.

FOX: No.

FINE: At this point had a decision been made to commercialize this?

FOX: Yes. A decision was made to try to commercialize it but there were a lot of problems. They started the development here initially by trying to duplicate my melt polymerizations, and they failed. Primarily, it was almost totally a mechanical failure. They could not handle the viscosities and temperature and reduced pressure. The classic was they did twist a three-quarter- inch shaft off of a one or two gallon reactor. They had no horsepower, but they had a big paddle stirrer in a kettle. The stuff was viscous as hell and they kept twisting and twisting and the shaft broke off. You don't stir highly viscous materials in big kettles with anchor stirrers, but there wasn't anybody here who knew anything about stirring high viscosity material or handling high temperature or anything else. One particularly interesting story I like to tell is that they needed high vacuums, they needed a lot of surface generation, and they needed high temperature. So they went over to silicone to use in a vacuum roll system, which is a couple of rolls that turn in a vacuum and generate a lot of surface. They put a batch of polyethylene into that and started running it and the whole damn thing froze up. They couldn't get it open, and they couldn't get it apart. It was kind of expensive equipment. It took them weeks to soak it loose and get it apart so that it would be operable again. They literally made themselves *persona non grata* in the silicone business. Anyhow, they failed mechanically. Then we started looking for other ways of doing this.

FINE: Were they still doing this by transesterification?

FOX: They tried and that was a total failure. It was probably Gene Goldberg who found that instead of putting caustic in water and trying to polymerize it, you could put monomer into pyridine and after you make polymer all you need to do is get rid of pyridine hydrochloride and get rid of all the stench. That's the way they were going for a while.

FINE: That was really the first step.

FOX: Then they found you could use methylene chloride as a dilutent.

FINE: So now you have an interfacial polymerization.

FOX: What came next was to put lime in with the methylene chloride and pyridine and leave it, eventually leaving the pyridine out. You use lime to get the chlorine. We actually built our first plant, a five-million-pound plant, using lime as an acid getter. Then we became aware of the interfacial polymerization and ended up licensing that complex. But we went through quite a few steps there. Incidentally, I discovered the use of pyridine over in Schenectady, and so I started making some polymers there. I took it to our patent attorney and our patent attorney was also the Pittsfield patent attorney. He informed that I was a little bit late because that's what they were doing over in Pittsfield as of last week or something. But it wasn't until we had a process going that we could make good consistent polymers that we could really begin to learn something about the product. I would say that I had a tough product when I made it. It turned out to be probably about five tenths, but it had gelling. I found out later that I made that polymer rather readily with no catalyst. In checking off every bit of BPA you could attain at the time it had a tenth to a half of a percent of lime and all sorts of other stuff in it. All the DPC had that garbage in it. I hadn't thought about purifying anything ahead of time. Subsequently I purified some BPA, and purified some diphenylcarbonate, put them together, and ran a melt polymerization. There was no reaction.

FINE: That is really instructive.

FOX: So, fortuitously we had the catalyst. It is of interest to note many years later, we were actually running a melt facility and the reaction was a little bit slow. One guy happened to be eating lunch and he had one of these little packs of salt for hamburgers. He said, "Maybe a little salt would help." He sprinkled a little salt in and the reaction took off. So a little salt helps when you're doing a transesterification process, if you understand why it works. The reason it works is that contrary to common teaching, salt reacts with phenol at high temperatures to produce HCl and sodium phenate. But if you had asked me when I was going to school, I would have said that salt is inert unless you put acid in. Phenol is strong enough to crack at high temperatures. This is one of the bad things about any kind of metal. If you have any sodium chloride you can destroy your polymer.

I was actively making polymers then and so was everybody else. We were aiming at who could make the highest intrinsic viscosity polymer. It was a bit of a challenge to see the best colors because everything was dark brown to begin with. The best color was a long time in coming but the high intrinsic viscosity was relatively easy to make. When they took the stuff to market they found out that there was no equipment anywhere to process it. There was no mechanical equipment capable of high enough temperature and horsepower to handle very high-viscosity polycarbonates. Then we started backing up. We found out we could back all the way down to almost four-tenths of the intrinsic viscosity and still have most properties. We settled a little bit above that, five-tenths for most of our products.

Another thing that happened was when we measured the melt viscosity over a period of time, it would either climb or fall away or disappear and lose its viscosity through degradation. A lot of this turned out to be due to impurities in the bisphenol-A. We actually paid Shell Chemical two dollars and fifty cents a pound for making pure bisphenol-A at a time when that was an outrageous price. We bought quite a few tens of thousands of pounds of bisphenol-A at two dollars and fifty cents a pound. You could buy the impure compound for twenty cents or twenty-five cents a pound, but it was necessary to have the pure compound. About 1956, they had reached a point where they were talking about going commercial and they were going to announce the product. So we picked the Gordon Conference to announce it. Before the paper was actually delivered at the Gordon Conference, I think [Herman] Schnell, who was a wire inventor, announced the same product.

FINE: At that same Gordon Conference?

FOX: No. They made the announcement in a German publication (2). That sort of shook the place up a bit.

[END OF TAPE, SIDE 4]

FINE: I think this is probably a good point to talk a little bit about the patent questions that arose with Bayer. I guess outside of GE and Bayer and within the industry, it is not generally known that GE paid licensing fees and had arrangements with Bayer all these years, and that the case was in conflict in the courts for a long time. Can you fill us in on what the basis for this litigation was and what the general arguments were.

FOX: I can very simply state the case as I saw it. We were in total ignorance of their work until Dr. Schnell's paper was published in *Angewandte Chemie* (2). This shook us up. Our management immediately contacted Bayer and informed them that we too were working in the same area as they and that it would take a long time for the courts to decide who had priority. In the meantime, we were reluctant to continue a development program not knowing whether or not we would be able to continue. We presumed that they felt the same. Therefore, it would make sense for us to agree that if they prevailed, they would license us and if we prevailed, we would license them. We would cross license any part of the process with some sort of financial compensation to the person who held the patents. We would not exchange any information or any technology. That is the arrangement which we established and essentially have maintained until today. We have, from time to time, taken advantage of our ability to license various segments of the technology which they developed and we have, likewise, given them license to operate some of our technology, with, as I understand it, a payment or fee or an appropriate summation of credits and debits to each other.

WISE: What about the division of markets? Was there any kind of agreement that they would stay out of the United States if you stayed out of Germany?

FOX: There was one slight agreement with respect to who could operate where. If they prevailed, they would not permit us to come to Europe under the initial license patent. That is the only restriction as far as I know.

WISE: So there was no restriction which kept them out of the United States.

FOX: There was no restriction which kept them out of the United States.

FINE: Of course, both companies were marketing in both places.

FOX: On both continents. As a matter of fact, our polymer plastics marketing in Europe was nonexistent at the time.

FINE: Was this in 1958?

FOX: I would say roughly 1956.

WISE: That was immediately after you read the paper.

FOX: It was very close, within a year of their public announcement. I'm not sure of the exact date, but I think it was 1956. Within a year we had a working agreement.

WISE: Did you both agree not to license any third parties with the technology?

FOX: No. You can't do that. We can't. Perhaps they could.

FINE: That would be illegal exclusion.

FOX: At least I think it is. I'm speaking as a layman. Our legal people are very much concerned about us abiding by the laws.

FINE: Did you feel that this project was moving along at a rapid pace at the time? You talk about the first discovery. In 1956 here's Bayer and GE talking about cross-licensing agreements. There must be people in high places who recognize commercial potential here in both companies. Things seem to be happening pretty quickly. Was that the feeling you had at the time?

FOX: In retrospect, I would think that way. But at the time I really hadn't any idea or concept of how businesses operate.

FINE: Of course the silicones business, in a sense, had established the whole concept of marketing in support of research, or even beginning to drive research. In effect you had to go out there and create markets for your materials.

FOX: I think there were a few things that happened at GE. Originally all of our chemistry was applied to solving internal problems and internal applications. Everyone of our manufacturing sites had literally established a small, back-room chemical facility to concoct or brew special formulations of varnish, plastics, etc. We had dozens of these things scattered around. A case in point was here in Pittsfield where we started making phenolic varnish and phenolic resins to serve ourselves. I'm not sure of the date, but I think it was about 1951 when somebody with great foresight decided to pull all of the chemical operations together and called it the Chemical and Metallurgical Division. This then gave us a much better base from which to operate. But this combination was really in its infancy, at the time that polycarbonate resin born.

FINE: Who would that have been that pulled everything together?

FOX: It was [Owen] Schmitt.

WISE: Jay Jeffries and Schmitt and Bob Gibson were the names.

FOX: I don’t know whether Jay Jeffries pulled it together, but he was instrumental. Schmitt was an organization man who stirred things up and scared everybody in the area, but he was very instrumental. The first division General Manager was Bob Gibson, and his background was in advertising. The gentleman knew nothing about chemistry. Jeffries, who was a key man, a vice president, was actually a metallurgist and my kind of guy. He was extremely creative and inventive and just had a fabulous career. Bear in mind that this was all pretty new here and I don’t think anybody really knew how to act in this particular case with the Bayer arrangements.

WISE: Who was really running the polycarbonate project then?

FOX: That was Gene Schubert.

WISE: He was running the chemical development operation.

FOX: The chemical development operation had responsibility for Lexan up until the point where it literally was transferred to what was then the chemical materials department. That transfer only occurred about the time we were putting up a plant in Mt. Vernon.

WISE: Had this crowded out the other things such as the radiation plastic process?

FOX: That was still going on and that was a commercial product. We weren’t doing any research. We were just operating a radiation facility and radiating tonnage quantities of polyethylene film which was being marketed through our insulation of materials business.

WISE: So the whole technical effort was focused on polycarbonates.

FOX: All technical effort was focused on polycarbonates. There were a couple of miscellaneous side projects. This was all chemical development operations. Our method of support was that all of the existing departments of the Chemical and Metallurgical Division had to make contributions to the development operation on the basis of their sales and profitability. One of the champions at the time, was Charlie Reed, who was a department manager. We had a molding business which we subsequently sold to Pave, which is now part of Hercules. We had our laminated products business in Coshocton which we still operate. We had a chemical materials business which operated the phenolic business, and we had the alkyd polyester business, etc., in Schenectady which was part of that. We had the silicones and we also had the Metallurgical Division that was primarily making carbeloid. This was one of Jay Jeffries' additions to the business.

WISE: Is it fair to say that besides the carbeloids and silicones that the others weren't making anything that everyone else was making, or were they just sort of not very unique or proprietary business?

FOX: They were making laminates, taking resins and putting them on paper and pressing it. They were making decorative things, but it really wasn't chemical. It was a mechanical processing of materials and the molding operation was processing materials. I might add that the molding operation was key to GE in those days because there was very little talent and/or facilities to process plastics. There were no garage operators and we had new plastics that were available. I think the one that gave them the most problems was nylon. Nylon was one of the first crystalline polymers they tried and its problem was that when it melts it turns to very low viscosity, like water. Ordinarily, when you make molds you can leave gaps and things of this sort and mold them, but when you start molding nylon you must have precise fitting molds and these things weren't available. So we had our own mold-making facilities here. We had to develop molding equipment to do it and we had a need to learn to process plastics and that's what we did. But as the industry became more mature, more people could supply this processing technology, and we sold out. We could not justify staying in a business where everybody could get in if they had a little capital and a little place to work.

This was in service with the polycarbonates because they were higher melting and more viscous than anything else that was on the market. There was nothing like it. Consequently the equipment that was available was not designed to cope with the mechanical requirements. We literally had a situation where we had a product but no customers that could utilize it. We had to train and teach people how to mold this material, and how to change their molding machines to accommodate it. We had to do a real evangelistic kind of program here educating the industry. I maintain that this is one of GE plastic's major contributions. We paved the way for the high performance, high temperature plastics which are common today.

FINE: Was that not true with silicones? Was it polycarbonate that really moved forward this whole idea of training and educating, providing all of this vast network of support for the user?

FOX: I think that the silicone people had the equipment to handle silicones. There really wasn't much difference in the early days between the silicone applications and the rubber industry. Rubber processing, rubber handling, and this general compounding was quite similar.

In polymers, there was a lot of thermoset processing equipment. There was a capability of molding nylon. There was a capability of molding the polystyrenes and to some extent polyethylene. But that was about the extent of the processing. They were all relatively lower temperature, lower viscosity materials.

FINE: It was very different from the polycarbonates.

FOX: It was tough finding the equipment. As I mentioned in one talk I gave, we actually took calrod units and taped them on or wrapped them on extruder barrels in order to get the temperature up. We had to change the tooling. For one thing, if you use the nylon tools, ours didn't shrink so therefore you couldn't get it out of the molds. There weren't a lot of other molds we could adapt so we had to get special molds. It was a pretty tough thing, trying to get this to market, and GE deserves a lot of credit, along with I presume Mobay or Bayer who had the same problems in Europe. Maybe they were a little more sophisticated.

FINE: Did you talk to each other about these problems?

FOX: No. We operated under a very, very tight blanket. I didn't even get to meet Herman Schnell until six or seven years later. They didn't want me talking to him and they didn't want him talking to me. This was a pioneering job in establishing an ability to process high temperature materials. Later, Union Carbide's polysulfone came along, and that was a tough material to process. But at least people had already wrestled with the polycarbonates and they could handle that one. Our own PPO came along and that was very tough to process but we had some experience with handling tough-to-process materials. At this time, one of the things that Gene Goldberg had made was some polyester carbonates. These things were higher temperature processing, higher viscosity than the Lexan polycarbonate and we couldn't do anything with them except cast film. But this was a pretty interesting product. It took twenty years before polyester carbonates hit the market as an item of commerce.

FINE: These are polyester carbonates from another acid chloride than phosgene.

FOX: When I'm saying polyester carbonate, I'm thinking primarily of isophthalate, terephthalate, BPA iso-terephthalte carbonate, and the polyarylates in general. One of the first happened about six or seven years ago. Allied Chemical announced Copec, but they never went to market with it. More recently, we went to market with our high heat Lexan. Bayer has announced they have some. Dow has come in very late in the polycarbonates and they have announced they have some polyester carbonates. But at the time we were trying to learn to process polycarbonates, the processability of these higher heat materials would have been impossible.

WISE: So this product engineering work was really crucial customer relations.

FOX: You had to provide somebody to go hold the customer's hand or have the customer in and hold his hand and show him how to mold the stuff, show him how to modify his equipment and molds in order to mold. This literally was, for GE, the birth of Tech Services. You have to provide a technical service or your customer can't use your product.

FINE: Was this the point where you began to go out and show people that here were materials that could be used in lieu of existing materials?

FOX: Well, it had to be the education of the marketplace. In this case, we made some guesses as to where the material would eventually be used and a couple of them were right. [laughter]

WISE: Which were some of the right and wrong guesses?

FOX: We thought we were going to have a great future in electrical film and electrical insulation because that's really the place we started. As a matter of fact, we didn't have anything happen in electrical film that was useful for fifteen years or more. Now, electrical film and sheet are quite useful. One of the reasons for sheet being delayed was that the original color of the stuff they made was pretty dark brown. Despite the fact that it would make good windows from a non-breakable standpoint, people did not like to look out of brown windows. So that market was held up by lack of color.

The first application that we had was a colored disc on somebody's electrical range. (It may have been Kelvinator.) They had a little disc on the stove that had a pie cut in it where one was blue, one was red, one was white, and this one was green. You could turn it and the light behind it showed and you could see what setting it had. This was sitting on top of the range, right in the back instrument panel. They had been making this thing out of acrylic for a long time. But the design got a little bit snugger or the temperatures got a little bit higher, and the acrylic started melting. They needed something that was transparent and they didn't like the idea of getting a glass and dyeing it or staining it or painting it or tinting it. So they bought some material. I think we sold them four hundred pounds for about two dollars a half a pound. That was an early market.

Another early application was wind lights on supersonic jets. As airplanes fly faster, the air friction heats things up on the skin and the acrylic was no longer useful because it would shrivel up when the airplane would go fast. So they tolerated our off-white color and made wind lights for supersonic jets. There was another thing that we tried. At the time transistor radios were popular. Television and the movies were showing dropping radios off of airplanes and off of ladders. They would drop down to the ground and they didn't break. That looked like a great product for us so we made out pretty well in making cases for little transistor radios that you could drop off ladders and from helicopters. The only thing was that once you dropped them, the insides weren't working. [laughter] The outside was perfectly unscathed. Another very early application that we lucked into was the birth of a peek-a-boo coffee maker. It had a little sight tube to tell you how high the coffee was or how high the water was. They didn't want a glass tube in there in case it got jarred or hit or banged or something. So we found an application as a sight tube on coffee pots. We were really scrounging for places where we had a unique fit.

FINE: What color was the polycarbonate at this point? Was it kind of amber?

FOX: Amber.

FINE: At what point did you have optical quality polycarbonate?

FOX: It definitely wasn't until we switched over to interfacial polymerization. I'm guessing now, but I think our sheet business started about 1965.

FINE: That was really late in the game.

FOX: At that point, we had a product whose color looked pretty respectable.

FINE: Did the color come from thermal degradation of phenols to quinones? Was it the typical kind of chemistry problem you have with the phenols and temperature?

FOX: It comes from a lot of things. A lot of them are miscellaneous impurities in the polymer. They either cause the color or catalyze the polymerization of the color. A typical example here is a trace of iron anywhere in the system. You know the reaction of iron with phenol. It's a typical color test for phenol, using ferric chloride.

FINE: It gives beautiful red colors.

FOX: With any phenol you get all kinds of browns, reds, purples, etc. It only takes parts per million of iron to do a job like that, and in the beginning all of our bisphenol-A and all of our raw materials had iron in them. You learned that you have to get the iron out. And, a little bit of chlorine in there will chew up a metal machinery and generate its own iron chloride. So a little chlorine is bad. Then, if you just take a little plain phenol, the phenoxide ion will go black very rapidly with oxygen. You get all sorts of things. So it's a complex mixture of all sorts of things that cause color and this is the type of thing that you get better at with experience. You could say there's probably not a year that goes by that we don't ratchet up just a little bit better in color.

FINE: Is it the equal of Plexiglas?

FOX: Acrylic? No! It's a little bit more yellow than acrylic and we do have a very small amount of haze in the polymer. So it is not quite the equivalent of acrylic, but it is better than most glass, which is interesting.

FINE: Was there a lot of research on the chemistry causing the color?

FOX: There was and has been. There was a lot in the beginning. I would say that it definitely has tapered off a bit, but there are still some continuing professional people whose objective in life is to get the color out of it.

WISE: You were saying that you were scrounging for places where you had a unique fit. Was there one big hit or breakthrough where you got a big volume use that suddenly made this take off?

[END OF TAPE, SIDE 5]

FOX: We did get some early electrical motor parts, spools, bodies, wrapping coils, and things of that sort. We had a reasonable size hit for a while in a cast film for photographic aerial photography. This was despite the fact the color was not that good. This was when the "spy in the sky" activity was going on and they were interested in photographic film with which you could take a series of pictures and run them through developing and then cut them out and splice them. One of the problems with most film was that it absorbed moisture and expanded and contracted and shrunk a little bit during the processing. There was no certainty that everything would match to scale. It was discovered that polycarbonate film was essentially free of all of these liabilities. Ansco film was actually a pretty sizable customer for the polycarbonate, which they dissolved in solvent and cast as an aerial film. That was an application.

I'm not sure where we made our big breakthrough and big application. Another application that was kind of early, and again based on toughness, was odometer hubs for rental vehicles. These were just little mileage indicators on the hub that had a transparent cover so you could check how many miles a vehicle had gone. For a while, we had pretty interesting applications for this because it had to stand up under stones, and had to be able to maintain transparency and weatherability. We capitalized on this. The odometer hub was a little thing that looked like a small pie plate. We ran contests in places like Detroit and offered a genuine twenty-five dollar Defense Bond if anybody could break it. We gave out hundreds of these things. Every time somebody would come back with a broken one, we would get a lot of free publicity in the papers talking about how so and so won a Defense Bond by breaking this unbreakable plastic. That worked pretty well. We got a lot of free advertising out of it. Some people had some very interesting ways of breaking it. As a result of this low cost advertising, we ended up with a lot of people who found some useful applications for the product.

We had some early hits here and there in the automotive industry. People looked quite early at tail lights but we couldn't beat the acrylic on cost. We did get into after markets and into commercial vehicles. We got into tail lights because if you break a tail light on a car, the insurance company fixes it. If you break a tail light on a commercial vehicle, you fix it. The tail light that costs two dollars out of acrylic might cost four dollars out of polycarbonate, but the replacement cost is something like twenty dollars. It is quite easy to sell in this kind of market. We did get special applications for non-breakable tail lights and lighting covers.

Another early one was a reasonably good product for street lights. I like to tell the story that Central Park had street lights that had an average life of probably a couple of days. We put in some polycarbonate light covers and they lasted for years. They just didn't break. They had bullet holes in them, and they turned some raunchy brown that didn't let much light through, but they were still intact. [laughter] That was one of the early applications for the polycarbonates.

FINE: McDonald's Golden Arches.

FOX: Right now we have a lot of outdoor signage.

WISE: Were they pretty much on schedule as far as becoming profitable?

FOX: It made out pretty well. They built a five-million-pound plant and a lot of people questioned, "What the hell are we going to do with five million pounds of the stuff?" But by the time we were up and running, they had almost all of that five million pounds sold or committed and we started thinking about expansion.

FINE: This was in Mt. Vernon?

FOX: This was in Mt. Vernon. Essentially we've been in an expansion mode ever since.

WISE: Was it around 1960 when that came on stream?

FOX: It was 1960. That was in the middle of a cornfield and we leased the adjoining field to farmers who were growing corn. There was corn all around the place and we owned acres and acres. The question was what would we do with all this land? Subsequently we have found that that land has appreciated very greatly beyond our corn fields and we are buying back acres at prices that we had probably paid for the whole site in the first place.

WISE: Were you still using the phosgene in large quantities in a plant like this? Was this something new and difficult? Was it a problem?

FOX: Yes. It did then and still does pose a hazard. The thing that we had going for us was that phosgene was in large use by a number of other companies before we got involved. I don't know whether it is in place here or not but just in case you're not aware, Mobay, which was another manufacturer in the States before Dow entered, was really a joint venture of Monsanto and Bayer Chemical Company in Germany. Bayer happened to be the world's leading manufacturer of polyurethane resins. Polyurethanes are based on phosgene. Bayer wanted to enter the U.S. market and, as a German company, at the time would have had significant problems. So they formed a joint venture with Monsanto Chemical to manufacture and sell polyurethanes in this country. They have had phosgene capability to make polyurethane here in this country. At a somewhat later date, our government decided that they should break this up and forced them to separate. Mobay then became a fully owned subsidiary of Bayer and Monsanto was out of it. But from the very beginning, Mobay did have phosgene capability and experience. They were knowledgeable.

We acquired, not from Mobay but from anybody who had constructed phosgene handling facilities, the knowledge that they gained from the industrial use. This was not a totally frightening undertaking because we had good advice. I would say that we have been tightening up our whole phosgene handling ever since, investing an awful lot of money in trying to make the stuff doubly safe and then triply safe and quadruply safe. We do all of our handling, for example, in a room within a room with all kinds of exhaust systems which will neutralize this material. We actually operate under positive pressure to keep the stuff from migrating out. It has to stay in. There are all kinds of safeguards. This has been gradually tightened. In our case we have the EPA [Environmental Protection Agency], and OSHA

[Occupational Safety and Health Administration]. In Europe, the European codes are even tighter.

WISE: Has there ever been a serious problem?

FOX: We have had one or two small discharges. To my knowledge, I don't think GE has advertised it, but I think we probably had a "casualty" from phosgene.

WISE: This didn't tend to cause anyone to have second thoughts about going into the business? You had to develop this capability, which was so different from anything else.

FOX: I don't really think it did. You can always talk carbonyl chlorides.

FINE: That's like magnetic resonance. What might be more important here is the question of phenol itself. GE suddenly becomes the leading manufacturer of phenol, putting up a world scale, four hundred million plus pounds per year plant in Mt. Vernon. The phenol, of course, is critical to the quality of the polycarbonates and also to independence of supply. That must have been a significant decision.

FOX: It's an interesting story. We did have a phenol plant here in Pittsfield during World War II. We were using a chlorobenzene hydrolysis using the Dow process. We operated it successfully. We built it in order to provide ourselves with phenol which was required to make our phenol formaldehyde resin. During the war there was a severe shortage of phenol so we actually made our own to supply our own uses. I think that our plant was less than the most efficient and I have been told that our cost of production of phenol was equivalent to, or slightly higher than, the market price of phenol, so there was no economic incentive to operate it. But we did operate it because there was no commercial phenol. So as soon as the war was over, we stopped running our phenol plant. A skeleton still remains out back there. Our only need for phenol at the time was for phenolic resins.

We started the polycarbonates with a five million pound plant. We only needed, at the most, five million pounds of phenol for that and maybe twenty million to forty million pounds for the phenolic resins business. This is considerably less than an economically sized plant. The economically sized plant at the time was maybe fifty million pounds for the minimum sized plant. We needed about twenty-five million or thirty million so we started looking at ways to get into the phenol plant for our business. As soon as we could grow a market or use it a little bit more we would go into the business. We got our applications up to maybe forty million or thereabouts. Only ten million short and we started letting it be known that we were going to go into the phenol business. The price of phenol dropped a penny or two pennies and it was no longer an economical thing to do. We took the penny or two pennies and sat there. Our volume built up a little bit more and we were at the original economical size, but now the economical size for a phenol plant was one hundred million pounds. So we only had half a plant to go and the people who were selling us phenol were now operating out of one hundred million pound plants. So we waited a little while and then PPO came along and we needed some phenol for that. We brought up our own market and we got close to one hundred million pounds, and now the economical size was up to two hundred million pounds. But we let the industry know that we were coming in the business and they cut the price for us, temporarily at least, which removed the incentive for us to make our own. We pulled that game for a long time.

Finally, (I don't know the exact date of this) we bit the bullet and decided that we were going to have a world-class, economical plant. At that time it turned out to be in excess of four hundred million pounds, maybe closer to five hundred million pounds of plastic, which was a lot more than we needed at the time. But we built the plant and had it operating. Finally we had control of our major raw material, not only the economics of it but also the quality.

The same thing happened in bisphenol-A. In bisphenol-A there were a few merchant producers but nobody produced polymer grade BPA. In order to get polymer grade BPA we had to pay premiums. We had to do some purification of our own and all sorts of other things, and at best, it was marginal. We literally hunted around the world for the best quality BPA we could get and bought it from everywhere. But this was no way to run a business.

Eventually we decided we had to make our own so we went into the bisphenol-A business. Again, we were sizable when we started, but we've outgrown the original plant a number of times. Right now we're in the process of bringing on still a larger capacity with more sophisticated processing. I want to give you a little sidelight on the bisphenol-A. Bisphenol's major application is in epoxy resins. When you make bisphenol-A, you make about eighty to ninety percent polycarbonate grade material and you make ten to twenty percent isomeric mixtures which really aren't any good at all to make polycarbonates, but are perfectly fine for making epoxy resins. We were asking for somebody to take the bisphenol-A, cut out the heart and give it to us, and then go with the rest of it to epoxy resins. They would do that for a premium but really their business was supplying the epoxy resins business. This is one of the cases, incidentally, where Dow right now will have an advantage. They make epoxies or sell to the epoxy business and they have gotten into the polycarbonate business. Now they can skim off the quality materials and use that for polycarbonates and the rest of it can go into epoxy. To get around this, the only way we could handle it practically was to stop making this ten to twenty percent isomer mixture. We have spent an awful lot of time and effort in tightening up the process so we don't make as much of the undesirable by-products. This has been the subject of a lot of research. We have a totally new process coming on stream.

WISE: You also tried to go into the epoxy business a little while ago, but that didn't work out.

FOX: We tried to get into the epoxy business but we had some bad press and we stayed away from it. We have been in and out of the epoxy business quite a few times. We are a large consumer of epoxy resins in our laminating product business and they actually formulate epoxys. Incidentally, they can use the poorer purity material to some extent, but the amount they need is small compared to the amount we would generate. Epoxys are good business, but GE has elected not to get involved.

WISE: Let's go back to your own career for a minute. You mentioned that you went on to be Advanced Development Manager for CDO just at the time when Lexan was on its feet and PPO was coming along.

FOX: It was just the time that Lexan was going commercial. When they went commercial they took all the people with them and they stripped us of all of our equipment. I was captain of the ship without one as far as chemistry was concerned. I had an engineering partner who was the captain of an engineering operation without any engineering facilities and very few people. My job was to go find some new business opportunities. Prior to leaving Schenectady, I had developed an interest in aromatic polyethers. I had actually tried to make some aromatic polyethers using the para-chlorophenol route of self-condensation.

FINE: This interest goes back to guaiacol days.

FOX: No. I was interested in something that wouldn't hydrolyze. Polyethers wouldn't hydrolyze and there wasn't a polyether polymer on the market. I got attached to that and started experimenting. I had laboriously made dimers, trimers, and tetramers by one step reactions and then I coupled them together and made very high molecular weight 1,4-polyphenylene oxide. I discovered it was a crystalline product. At this time I knew about Tg. It had a low Tg—and not a particularly high Tm. In many respects, it is very comparable to PET [polyethylene terephthalate] but at a little bit higher temperature on the Tg.

FINE: You had done all this before the oxidative polymerization reaction was first run?

FOX: Yes. I was experimenting. I needed an autoclave to make my polymers. I was going to put my para-chlorophenoxide salt in an autoclave and I didn't have any in Schenectady. I knew there was some out at the research laboratory so I called Art Beuche and asked if he could get me into the autoclaves out there to run the experiment. I talked to him about it and he agreed to get me in. I ran a couple but they were complete fiascoes. All they would do was char. That reaction doesn't go. It isn't a controlled one. I was intrigued with that.

It was 1956 when Al Hay invented PPO. I learned about it and immediately got intrigued with it. That was the product I wanted. All I needed to do was have him straighten it out so they left the methyl groups off and made the product I wanted, polyphenylene oxide. I started supporting that here. I had three chemists, but I wanted more.

They had a tight situation here and I couldn't have any more people. But I knew that our direct energy conversion operation had a project making fuel cells for space applications. They needed an ion exchange resin for their fuel cells. I figured that PPO would make a very good ion exchange resin as it could be chlorinated or quaternized or sulfonated. So I touched base with the man who was running that project and told him I could give him some good chemistry if he could give me some financial support. He agreed to underwrite a couple of chemists. So I went looking for chemists to use up his money and work on his project and make ion exchange resins out of PPO. One was Bob Shenian, and another was Ken Gorman. I got this project going and we were trying to make PPO and we did make PPO here.

WISE: Was this the very first application of PPO in Pittsfield?

FOX: That's what I was going for. That's the guise under which I was able to get a little funding to expand the program. But we were trying to make a molding compound out of it and we had made a fair amount of polymer. Every time we tried to mold the polymer that we were making, it would gel. We had a capillary plastomer here where we would force the polymer back and forth through an orifice under pressure. We could follow the time it took to go back and forth and watch the viscosity. PPO started like this. The viscosity just kept going out of sight and it would set up to gel and wouldn't process. We did a lot of work in trying to clean it up and purify it. The purer we got it, the better it became. One of the things that was quite obvious with all the material we had was that it contained a lot of residual copper salts. Copper salts are the best oxidation promoter catalyst we could get. It also had a lot of amine in it that we couldn't get out. If we dissolved it, reprecipitated it, and we extracted it, the cleaner we got it the more resistant it was to premature gellation turning to charcoal. We had a real program going trying to purify it. We were working with the research laboratory on that.

At the time, we had very little support from our management on this because we already had a thermoplastic that we didn't know what to do with and that was difficult to process. Here we were trying to give them another one, and they said, "No way." That's one of the reasons I had to go outside for funding. Edie Boldebuck had checked this stuff and found out you could put it in a sealed tube and heat it at 150° C for weeks without any effect. I found that the Army Medical Corps was advertising for people to work on plastics which could be made into medical and surgical equipment that could be autoclaved. Again, an autoclave. So I went down to see the people at Fort Totten. I spent the day selling and ended up getting a contract to develop PPO as a polymer to make medical equipment for surgical applications.

[END OF TAPE, SIDE 6]

FINE: So you really had to go out and get funding because of the polycarbonate.

FOX: There were very few people around who were interested in supporting another phenol plastic molding compound, with one exception. The exception was Charlie Reed, who was, at this point, in metallurgical operations. But Charlie said, “What would Dow or DuPont do if they had something like this? They would put an army on it. What’s wrong with you guys? If you won’t pay your share, you can double or triple my assessment because I think this is a thing that needs support. This is the kind of chemistry we ought to do.” So he literally was the man who sang for our supper and kept it going with the other narrow-minded individuals running the other departments who were more interested in how quickly they could get a payback on the plant we already had before we did anything else new.

FINE: Did you ever try and convince Al Hay that he should come over here and get into development?

FOX: Yes, I did. I talked to him once but at the time he was working for Jack Elliott. Jack Elliott was about to leave and he had that position in mind rather than coming over here. At one time he was all set to go to our insulation materials department because they were going to go in a big way into polyesters, and Al had just invented a route to oxidation of xylenes through terephthalic and isophthalic acids. They had proffered a job and were telling him what a great opportunity it was. Then the plant burned down. There was an explosion and it burned down, so we went out of the business. Yes, I talked with Al a couple of times, but he had a home where he was. He had everything he wanted.

Another interesting thing was that my job here was censoring any research lab publications that were relevant as far as plastics was concerned. He had invented this material in 1956 and I had sat on quite a few attempts to publish it. He slipped one by in *Physical Review* that I didn’t bother to check. That one got published and that was the first disclosure that got out on his work. My missing his publication on PPO was the thing that got us into Europe.

Incidentally, the top man at this time was Reuben Gutoff and he proceeded to set up a new organization. No. I’m just a little ahead of myself. Reuben was in charge, but I was still doing the advanced development. A man by the name of Copeland was doing the engineering. Marketing had hired a man by the name of Bob Finholt, who was a Ph.D. from Louisville. He’s dead now.

Finholt was a dreamer and we owe a lot to him. He was another one of those people who would sell something he didn’t have and then would go back and tell the troops, “You have to deliver. You’re committed.” He really had a vivid imagination that went way beyond the realm of reality many times. But it was the type of thing we needed. One of the wild things that he did, among other things, was somewhere around this time. We were all aware that in order to keep our money coming we had to have "show and tells" for our department managers who supported us. One of the things I needed was some PPO parts. We couldn't mold parts worth a damn, and I remembered that Edie Boldebuck had mixed some low molecular weight polystyrene with PPO and used it for another application. So I mixed some with our PPO and found out that it made a nice transparent polymer. If you put enough polystyrene in, you could process it, except the parts were extremely brittle. So we used to mold PPO styrene parts and show them to our audience for "show and tell" but never let them touch them or get their hands on them for fear they would break. [laughter]

At one time, Al Gilbert did get one and broke it. It was kind of embarrassing. He was there as our guest, listening to our song and dance. We knew that the PPO was compatible. Finholt sold the exclusive rights to Rexall or Rexall-Dart to try to find some useful applications of combinations of polystyrene and PPO. He gave them a year of exclusive rights for fifty to sixty thousand dollars. They screwed around for a year and didn't accomplish anything. Their right lapsed and we got it back.

WISE: That's a good story.

FOX: He was also doing it with the Warren Paper Company. He wanted to see whether they would pay fifty or sixty thousand dollars for the right to see whether there was any paper connected applications of PPO. That one didn't go through. But this was trying to raise money to keep the damn project going.

FINE: Couldn't you get any support from Bueche?

FOX: Bueche and company derived all of their support from the same people who were giving us a support problem. We paid dues or taxes to the research lab so the people we were trying to get money from were paying to support us and were also paying to support Bueche and company. So there wasn't any possibility there. We really did some scrounging and I think it took the guys with guts like Finholt to scrape up money to keep the thing going.

FINE: Was John Welch in the picture at this time?

FOX: He came in 1960. We had this process to the point where we were making it in two-liter glass reactors. We were a little bit concerned about pumping oxygen into hot toluene.

FINE: It's like a carburetor.

FOX: We wanted to get to metal and we wanted to scale up. We didn't have any facility to go into but I had a room out back which had been a high pressure laboratory. We wanted to get that thing going and we needed an engineer. Welch came looking for a job. He had been invited in by the chemical materials people who ran the polycarbonate business. He had not been invited by me. They told me that I could talk to him after they were through. They kept him until about 4:30. Roy Moody was one of the group who was stalling and trying to sell him on coming to work in polycarbonates. At 4:30 they gave him to me and told me that I could talk to him, but he had to catch a 5:30 train back to Urbana. So I had the hour and then I asked him if he would mind missing the train so I could talk to him. Mike Goodman gave him a real sales pitch and he decided to work for us on PPO. He was the first engineer we had on the premises to work on PPO.

Another guy that came in almost at the same time was Al Gowan. Alistair Gowan was from Edinburgh and went on to a postdoc at British Columbia. He was a polymer physical chemist. I managed to get him. They both came in the summer of 1960, right after graduation. Gowan turned out to work his way up to run the PPO business for a while. Then he became our second guy in Europe and ended up being V.P. in Europe for a while. Eventually he came back and ran the metallurgical business for a while and then worked his way out of the company. He is now with John Brown Equipment. He was a very sharp guy.

As a sidelight, he and Welch were scratch golfers when they arrived here and they took on all the local people. Welch set up a little pilot plant. The first reactor was literally a modified copper kettle drum. That was the time when they were doing metal spinning and they made a copper kettle drum with a flange on top of it. We put a plate on top of it, and some bushings, and that became my first PPO facility. We ran into a real major stumbling block when we found out that if we took all the dimethylphenol in the world, if we could get it, (it was scattered at about one or two percent content in the coal tar), we wouldn't have enough to run a plant. So we had to have a PPO process. Gutoff did a good job of pushing on the research laboratory to develop a PPO monomer process.

FINE: That was another Hay project.

FOX: Steve Hamilton was the one who did that eventually, but he was pushing on Hay and Elliott. Elliott was still the boss in getting things going. We started a project of our own and we went down to Princeton and hired Princeton Research Associates which was a spin-off from an outfit called Petrotex. Carroll Wolff was the key man there. We hired him to get on with the process. We had three processes going simultaneously. Steve Hamilton pulled the plum out, and found the particular catalyst in methanol and phenol that could be made into monomer. We ended up with a pile of that here in Pittsfield. We scaled up a little miniature plant that Welch built. It was making hundreds of pounds of polymer. It moved along pretty well.

Finally, it was set up as a separate business, with Jack Welch as the manager of engineering and Finholt the manager of marketing. I think Gowan may have taken the technology. But somewhere in the line Shenian got in and Shenian ran the R & D function of PPO for a while.

WISE: So you made the product announcement about PPO in 1963. Were you aware around this time that it had these oxidation problems?

FOX: Oh, definitely. But I named it. I called it PPO—polyphenylene oxide, not polyxylene oxide. If I had thought we were going to stay with dimethylphenol, I would have called it PXO. But I was sure as hell that somebody was going to be smart enough to figure out how to make my old PPO by oxidizing phenol. A fair amount of effort went into trying to do that, unsuccessfully. For a while we tried to compromise and make para-methylphenol from ortho-cresol. But we weren't able to make it. I had my heart set on PPO. That's what I wanted. I'd say that I didn't do too much in the line of making a product out of it other than putting the styrene with it. One contribution made almost by accident, but it was a major contribution and then it later became controversial, was made by Eric Sizack. Eric Sizack has the basic patent for impact modified PPO Noryl (4).

FINE: The rubber modified?

FOX: Yes, the rubber modified.

WISE: That's what cured the brittleness problem that you talked about earlier.

FOX: I didn't know anything about impact modification and nobody else around our whole system knew anything about it. If anybody down at the Research Laboratory knew anything about it, they sure wouldn't tell us about it. Everyone was aware that we could mold and fabricate the stuff and it was brittle. What we were doing was buying crystal styrene and mixing with it. Edie Boldebuck had much earlier tried to modify the melt viscosity of low molecular weight styrene by putting a little PPO in it. She claimed that composition of matter and got a patent on it. So we had a composition of matter patent on PPO styrene but it really isn't worth a damn thing. The only one that's worth anything is the impact modified material.

WISE: How was it impact modified?

FOX: We were buying regular crystal styrene. She suggested trying impact styrene. Impact styrene has a small amount of rubber particles put into it for the express purpose of making it nonbrittle. If you take impact styrene that contains a little bit of rubber to make it tough, the same rubber helps make the combination tough. Now if you put more rubber into the combination, you get a tougher product. He literally started a project to brand it.

I think Finholt commissioned Ralph Hagen and company at the major science appliance laboratory to study blends of PPO. They made ABS blends and ASA blends and SAN blends. They blended almost any kind of polymer around. They did a lot of work on blending and they helped perfect this Noryl as we know it.

WISE: That was after Sizack?

FOX: Sizack was the first one who happened to put in rubber modified polystyrene. He essentially made rubber modified Noryl which was the thing that we really ended up selling.

WISE: Was he was working for the PPO project?

FOX: He was working for the PPO project. He was in a marketing function at the time and he was reporting to Finholt. Finholt was a wild and crazy Kraut, if you will, or an Austrian, whatever he was. He came up to Pittsfield from Monsanto but he came by way of Bridgeport, where our headquarters were at the time. He came by way of Bridgeport and the operation was here. I was told to keep an eye on him and help him. When he needed some variacs for some reason he took all the variacs off somebody's set-up that they had on a major program. He took over the extruder when somebody had it scheduled and they had to clean it up to use. All I was doing was fighting his battles to keep him out of trouble.

WISE: He made this big a contribution?

FOX: He made a very key, critical discovery.

WISE: What was the state of the project between the time you introduced the new product and this modification? Was there a kind of a crisis situation in that you had a product and it really didn't have any markets?

FOX: No. I think everybody was going on the faith that we could sell all the products.

FINE: That seems to have been a philosophy that pervades all of these things. Of course, that's gutsy, to make that kind of investment. But then you had a history of success with polycarbonates.

FOX: We thought that we would solve the problem.

FINE: Does that attitude go all the way back to silicones?

FOX: I don't know too much about the silicone background, but from everything I know GE really did a great pioneering job in taking a little bit of technology and building it into a super technological position in the silicones. Our boo-boo there was that we were much stronger technologists than we were marketers, and we neglected the markets and polished the technology. Dow Corning went after the markets first and the technology second. Eventually the marketing first, technology second scheme prevailed. I think that this is one of the things that we learned pretty quickly here. Marketing is very, very critical. Technology can and will come along, but without the markets, nothing will happen. There is very good reason for us to pride ourselves and talk about our technology-based marketing growth. This holds up quite a bit.

WISE: Just one last point to get this clear for the record. Jack Welch was not really involved in this product definition. He was involved in the chemical engineering and the product part.

FOX: Yes, but as you see on some of the write-ups, Jack Welch was never involved in one phase of anything. He was always involved in all phases.

WISE: He was here when the key developments were occurring in going from crystalline polystyrene to hips.

FOX: Yes. At one time, Jack had been operating an injection molding machine which I like to say was tiller operated. It had a wheel, and you pulled it to doing injection molding. Jack had been in the crystal styrene business—crystal styrene PPO. Jack was well aware of it, but I would give credit to Finholt, the wild marketing guy, for pushing applications. He hired Finholt and he later hired another guy for the express purpose of what could be put into this thing to modify it.

WISE: Was that Gutoff he hired?

FOX: No, Finholt. That was the guy who was marketing. Ultimately Finholt reported to Gutoff at this point.

WISE: So Welch then became the manager afterwards.

FOX: Welch, as I say, was interested and knowledgeable in the chemistry. He was very much in the engineering. He stuck his nose very early into the marketing and was very much involved in marketing. I would say that he made major contributions in all phases. There is no taking that away from him.

There is another vignette. For the record, I don't know whether he likes it or not, but one of my favorite stories is that he did resign from GE. He found out that a classmate got a higher salary increase at the research laboratory than he got here. Gutoff wouldn't let us match his peer's salary, so he resigned. We had a little farewell party and then he came back.

FINE: Why did he come back?

FOX: Because Gutoff literally spent the night selling him on coming back.

FINE: Why did he let him go in the first place?

FOX: Welch was working for a boss who was a little bit weaker...no, there's no question about it, he was quite a bit weaker than Welch. Welch couldn't stand anybody weaker than him around and he didn't like it at all. Then he found out that a man by the name of Gettner at the research laboratory, who had graduated from Illinois with him, had gotten two hundred dollars more on his annual appraisal than Welch got. Welch told his boss, Copeland, the weak one, "Gettner and I were classmates at school. He's bright but I'm a helluva lot smarter than he ever was. If he's worth two hundred or five hundred dollars more to the research laboratory than I am, I quit." His boss took this story to Gutoff and explained it to him. I was there when he explained it. "Welch says if we don't at least match what Gettner is getting in salary, he quits. Is it all right if I match it?" Gutoff said, "Hell, no. Let him quit. Nobody is going to tell us how to run our business."

So, the time came about two or three weeks later and we were having a staff meeting here. Gutoff was up from Bridgeport and there was Finholt, Copeland, and myself. We were all sitting around with our finance guy, and Copeland asked to be excused. He was called out. He went to say good-bye to Welch, because it was his last day. He came back in and said that he had just said good-bye to Welch and asked if Gutoff cared to say good-bye to him. Since it was his last day, they were having a party that night. So Reuben said, "I'll talk to him. I'll be right back." He left about four o'clock. He didn't come back. We sat until 5:00, 5:30, and he didn't come back. Finally we said, "The hell with it," and we broke up our meeting. He had taken Jack down to one of the local joints and they sat until pretty late at night and talked. Jack sheepishly came back after that and said he decided not to go.

FINE: So he never really left.

FOX: He never left. He had resigned and he had run out his notice, but he came back on. I would say that within about two months, he was running the organization his former boss did.

FINE: That must have been quite an evening session they had there. You wonder what went on in that session.

FOX: I think there must have been some commitments made to him about the future. But he stayed on and has made out pretty well ever since. He didn't stay anybody's reportee or underling very long. He had a pretty good path up here. From the very beginning it was quite obvious that he had a helluva lot more on the ball than did Reuben. Reuben was a smoother talker but Jack dealt in facts. Charlie Reed turned out to be one of his champions.

WISE: Let me get into one more detailed question. You sent me that nice Harvard Business School case study which Jack Welch wrote that ended with the four options being presented for PPO. Either you go full speed with it as a business, there were two intermediate ones, or you drop it all together. Am I correct in assuming that it was the top option that was adopted?

FOX: Yes.

WISE: I just wanted to make sure.

FOX: That was an interesting story that Jack wrote.

WISE: Do you feel that was a pretty accurate story?

FOX: Oh, yes.

WISE: Do you think that was a fairly accurate story so that we can use that as a source?

FOX: I have no objections to your using it as a source but I don't know whether Jack objects to your using it as a source.

WISE: But you don't find anything that might be fictionalized?

FOX: I don't see any fiction in it. It's a very good story.

[END OF TAPE, SIDE 7]

FINE: I want to get off on a different track just for a little bit. My early training in polymer chemistry in graduate school was in an environment that included Bill Bailey. Polycarbonates had just come into place and polyphenylene oxide was something which was purely an academic kind of interest. We never had much of an experience beyond, "Here is the newest class of polymers that is being studied." Was there much interaction with the academic community? This was hot stuff. There were Gordon Conferences going on. There were scientific meetings. You had consultants.

FOX: With PPO?

FINE: Or on polycarbonates.

FOX: We did have some interaction from the beginning on polycarbonates with Dick Stein.

FINE: He is at the University of Massachusetts.

FOX: Dick Stein probably had the earliest samples of polycarbonates and he knew about them. He's been thoroughly up to date on what's gone on in almost all of our polymers. I hired his first Ph.D. candidate when I was still in Schenectady, a man by the name of John Keen.

FINE: You picked up Shenian?

FOX: Shenian and Keen were classmates at U. Mass. When I needed a couple of people to work on PPO I asked Keen if he knew anybody and he recommended Shenian. So that's the reason I got Shenian. I got Foreman from Orange, Texas where DuPont had sent him from West Virginia. We had his pal Don Jafers with us and Don Berdop. We picked up quite a few people. But our contact at U. Mass was Stein. We had some contact with Brooklyn Poly but not a whole lot.

FINE: Was it Charles Overberger?

FOX: Yes. Overberger and Bill Cass were buddies. We talked with a number of people there. You name them, we talked with them. Harry Gaylord had just left and there was Murray Goodman, who went out to La Jolla. He was a very good friend. In the polycarbonates we got a little interaction with Vivian Stannet, who was at Cornell at the time. He measured crystallinity, permeability and things of that sort for us. But for the most part there really wasn't too much interaction with academics.

FINE: There was not much of a discussion going on about PPO. Polystyrene was certainly a unique discovery and the polymer physics people were interested. Was there anything much going on there?

FOX: Unfortunately, no. Frankly, I used to tear my hair out at the lack of interest and consideration that we got from our corporate research lab. One of the classic cases that bothered me was Fraser Price, who was a great crystallographer. He spent his whole career studying carbowax and polyethylene which I used to say we don't make, use, or give a damn about. I couldn't get him to look at the crystallinity of our polymers. But, you could publish about carbowax, but you couldn't publish about new products. That's one of the things that bothered me there.

There were a few people at the research laboratory who were involved and capable. Mel Schultz was one of them. Tom Legrand came a little bit later. He was interested. George Gaines did some work for us. But for the most part, we didn't get a great deal of support from a polymer physical end. We had a fantastic amount of support from the synthetic end. That was because Al always supported us well from the synthetic end and that's good.

I mentioned before that I let that paper get by me. It was picked up by some chemists at AKU [Algemene Kunstzijde Unie] in the Netherlands. They had a program going with their high voltage people there. They have a name for it. They were interested in high voltage cable. From an analytical standpoint, or from an actual standpoint, styrene was nearly a perfect material, but it did not have high enough heat. They were pressing on AKU to develop a product like styrene with higher heat. Al's little message got out and general physics went for it. A couple of bright people there immediately jumped on it. Based on its making PPO, they started evaluating it, measuring it, characterizing it and doing a whole lot of work with it. They had actually made high voltage cable sections and testing and things of this sort based on wrapped film of PPO. We ended up picking up a fair amount of useful information from them and they were working very closely with a Dutch association. A couple of polymer chemists from Utrecht were coming in and consulting with them, so they probably did more basic, chemistry than we did at the time. We have since gotten very much involved, but in the early days most of our program was synthetic.

FINE: There were a couple of things I was saving until the end, but I suppose I ought to throw them in here. I'm interested in why GE got into the polyester business, the serendipitous element of adding polyester to polycarbonate, and how GE got into the composite business. Finally, at the end, we ought to just touch lightly on your views of the Ultem business and GE's getting into a really complicated kind of new industry there.

FOX: When PPO graduated, there was a big question of, "What do I do next?" One of the things that our marketing people kept telling us was that now we had two amorphous polymers. They had great thermal stability and everything else, but they dissolved in everything. What we needed was a crystalline polymer. Myself and a marketing type literally took off around the world trying to acquire a crystalline polymer. We also had a major program going on from the synthetic standpoint, trying to create a crystal polymer. We did make a few things that were pretty interesting, but they weren't quite everything we wanted. We narrowed it down to PET, which had all the properties we wanted, but it was not processable. Polyformaldehyde was a good possibility and we visited Europe and had propositions from several people to get into the polyformaldehyde business but there were a few things that I didn't like about it. The key obstacle there was that DuPont didn't want anyone else in the business and they had some patents out on it. So that was sort of barred.

At the time, the nylon business required too much integration. You had to make your own monomer. You had to make your own diamines and diacids and everything else, and that was not the type of thing we could hope to get in. So we were literally screening things in all directions trying to find something that we could buy, to get in a hurry.

Then Celanese came out with a product called XP-71. The man who was manager of R & D for our Lexan business for quite a few years was Paul Burcheska. He left and went to Celanese. Celanese ended up making this XP-71 product which seemed to have all the properties we wanted. It had very fast crystallization. We tried to analyze it and tear it apart. It was PBT [polybutylene terephthalate]. My boss at the time was Jack Welch, and his question to me was, "Fox, why the hell can't we make that stuff?" I said, "Not a reason in the world as far as patents because the patents ran out with Whinfield and Dickson." He said, "Then what the hell are you doing? Go make some."

So we went to make some. And this was where Pim Boman came in. He was the key man in making PBT and we made several pounds laboriously in the laboratory. We tested it and it was great. We were having a little conference of war in Jack's staff meeting. What the hell are we all waiting for? Why don't we commercialize that? We'd need a kettle or a reactor to start making larger quantities first. So he said, "Go buy one." He turned to Gary Whitehouse, who was the engineering manager at the time, and asked him, "Where can you get a kettle or where can you get a reactor?" Gary said "I'll check." He left and he called Wang Reactor. They said they just happened to have a forty gallon kettle that somebody had elected not to take. It could be delivered immediately but it cost two hundred thousand dollars. Jack said, "Go buy it. I'll get the signature." So we bought the kettle, had it in here in a matter of less than a month, and we were scaling up PBT.

We made it and it looked good. Jack was the one who immediately said, "I don't want this business if we can't get control of the raw materials." The key raw material was butanediol and that started a long, long research program to make or generate butanediol. We went straight through that. The thing that made it so easy was that we were calling on the customers to selling them Lexan and Noryl, and where they needed solvent resistance we had it. So we had completed our market basket and it went very fast. Actually, we came in late. Subsequently, six or more U.S. companies came in, and almost all dropped out. We ended up number one in the market. That was the PBT.

FINE: That was a good market.

FOX: It fit perfectly. What else was there?

FINE: Ultem.

FOX: Ultem is an interesting story. It was around for a long, long time. It was beautiful chemistry but totally impractical commercially. But the research laboratory had an awful lot of effort going on. They kept plugging it. We sort of kept giving it more or less oral commendation, but we really didn't get very much involved because what it had was at least six different distinct chemical steps. In each one of them you might get a ninety percent yield and if you take ninety times ninety times ninety, you end up with about a five percent overall yield. You waste an awful lot of chemicals and an awful lot of time and equipment. It just didn't look like it had a chance for commercial success. To show good faith, I did hire a person and kept him working on it for a year or so, but my heart wasn't in it.

FINE: Were you essentially against it?

FOX: I wasn't against it from a chemistry standpoint, but I know I was against it as far as it being a worthwhile venture for GE plastics. I think they sort of sensed this over in the research laboratory.

FINE: In retrospect with the Mt. Vernon facility, how do you feel about that?

FOX: Better.

WISE: Better. What is the status on Ultem? We always play it up at the Research and Development Center.

FOX: It could be billed as a great success. The market is building a lot faster than I ever thought possible and the technology has become greatly simplified. Right now we are sold out of our large pile from our semi-works facility and we are in the process of commissioning a production facility. We have a few good applications for it and if they continue to look good, we'll sell that one out fast. I guess it is destined to be a very good success.

WISE: Is it on the level of the Lexan and Noryl?

FOX: This material is selling for three or four times the price. Therefore the volumes are going to be smaller. It is going to be profitable but I don't think it is going to be on the same world scale as polycarbonates or PPO. It is competing for markets where there are some reasonable competitors. In fact, there are some damn good competitors. Fortunately, they are all in the same price range so you are competing at a higher level. I have no doubt that Ultem is going to be a good or maybe better than good, but I don't know whether it is going to be a great business. I think all parties involved have done an outstanding job. I'm sort of in the position where I wasn't one of the initial champions, and maybe I'm a bit biased in not calling it a howling success. I will give them a lot of credit for doing a great job and I now see where it has some markets where it can justify its costs. Some of these markets are quite large, printed circuits being one of them.

There is a great deal of enthusiasm in our company. I don't quite share that totally, but it is a very good product for the applications they are working on. I think sooner or later some lower cost products will probably take the large volume portion of these markets. There will still be a sizable market, but it won't be like PET coke bottles that are now in the seven hundred million or eight hundred million pounds after six years, and still going up fast. Of course the difference there is the price, which is around sixty cents a pound, and we're talking around five or six dollars a pound. It is going to be restrictive.

Back in 1975, we were looking for some other name or some other way to designate this and we had a party at the Guest House with Hal Lewis of the Research Laboratory. I think

Bueche was there, and Roy Moody and a few others. Our Bicentennial was coming up so we decided to call it X-76. This would be the experimental resin seventy-six. We thought we might get around to introducing it by then but it took us an extra six years. There's many a slip between mouth and lip, or something like that. I don't think there's any question that Bueche played a key role here, along with Jack. Rumor has it that Bueche told Jack, "We've given you people a lot of business over a long period of time. We think Ultem is a great product. I think you owe us one." And suddenly we started an all out cooperative development program. I would say that it has been a very worthwhile relationship because the people from the research lab literally came over and lived with us and we lived with them. It has been everything you could expect or hope for in a working arrangement.

FINE: There may be an interesting way to tie this whole thing into a loop. The laboratory in Schenectady was really built in the aftermath of Edison and by Steinmet in the early years. In a unique way, there aren't very many laboratories like that. In a sense you have a unique relationship between research and supportive development, and then marketing driving research. You can't point to DuPont, or anybody else that I can see, and find the same kind of working relationship that you've been describing back and forth here as we've gone from one product to the next product over your whole career. Do you feel that this is a unique kind of enterprise that GE has?

FOX: I think this is something that GE has over all of our competitors. There is an absolute open door between all technical people. I would say that the managerial ladders and things of that sort are pretty insignificant when you get to a technical community. I don't have to ask my boss if I may go talk to somebody over in Schenectady. If I want to call, I'll call Owen Schmitt today and talk with him. I was never without that ability. I think that is one of the things that is unique about GE. We do not have chains of command. This is one of the things that disturbed me when I went for an interview with DuPont. Each man had to check with his boss who had to check with his boss who had to check with his boss to get final approval to say I'd like to have you come to work. This bothered me, having to get through intermediates in trying to make things happen. We have done a great job here in making things happen. There is no question about it.

The other thing I would say if you want to characterize GE Plastics is that somebody had the foresight many years ago (and I think it was Bob Gibson), to start a chemical development operation whose sole purpose in life was to start new businesses. This concept, the idea of having a chemical development function, is not different. But all the chemical development functions I've encountered are staff functions with maybe a marketing man or somebody else in charge. In order to make anything happen, they must go and interrupt somebody else who has a job that he is being measured on and say, "Would you drop your job and do some work for me? Would you do this or would you do that?" Nobody will ever do that on a voluntary basis. So all they are is a staff function with no muscle. GE Chemical Development Operations was put together with a manager, a manager of R & D, a manager of engineering, and a finance man. We have every function and every one of the functions has people who can make things happen. They can buy equipment and establish equipment and do whatever the hell they need to do to make things happen. This makes a difference.

WISE: That no longer exists though, does it?

FOX: Well, we sort of lost it, but not completely. What Joe has is a good remnant of it. He has me and right now I'm working on new polymers and new products. I have an engineering counterpart, McGinnis, who is downstairs. He is able to work on the engineering part of the problems. We have all of the technical facilities of the General Electric Company that we can work on. We have some funds and we are supporting work at Louisville. We are contributing to work where we can call the plays at the Research Laboratory over and above our assessment. We can go outside and hire outside companies. We can operate this as a unit with literally Joe having that as the chemical development operation.

WISE: I don't know if you have read that *Search For Excellence* book which has that absurd little paragraph about the history of Lexan (5). It says that it is a total bootleg operation. They made it sound like you were carrying this thing out in secret. That's total fiction, isn't it? In addition to getting all the names wrong, they also got all the facts wrong.

FOX: A lot of it was fiction. It was nice they spelled my name right.

FINE: You are now thirty-three years into your career here. As you look back on it, what gives you the greatest pleasure?

FOX: The thing that gives me the greatest pleasure is to contemplate all the people who are working because I started something.

FINE: Nicely said.

FOX: And that's kind of exciting.

[END OF TAPE, SIDE 8]

[END OF INTERVIEW]

NOTES

1. D. W. Fox and F. M. Precopio, “Polyesters from Terephthalic Acid, Ethylene Glycol and a Higher Polyfunctional Alcohol,” U.S. Patent 2,936,296, issued 10 May 1960.

2. H. Schnell, “Polycarbonates, a Group of New Types of Thermoplasts: Preparation and Properties of Aromatic Esters of Carbonic Acid,” *Angewandete Chemie*, 6¸ (1956): 633-640.

3. J. R. Whinfield and J. T. Dickson, “Fibers from Glycol Terephthalate Polymers,” British Patent 578,079 issued 14 January 1946.

4. E. P. Cizek, “Blend of a Poly(phenylene ether) and a Styrene Resin,” U.S. Patent 3,383,435, issued 14 May 1968.

5. T. J. Peters and R. H. Waterman, *In Search of Excellence: Lessons from America’s Best-Run Companies*, (New York: Harper and Row, 1982).

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