CHEMICAL HERITAGE FOUNDATION

ALLAN S. HAY

Transcript of an Interview
Conducted by

Leonard W. Fine and George Wise

at

Schenectady, New York

on

24 July 1986

(With Subsequent Additions and Corrections)

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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(Revised 20 February 1989)

Allan S. Hay, interview by Leonard W. Fine and George Wise at Schenectady, New York, 24 July 1986 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0057).

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

ALLAN S. HAY

1929	Born in Edmonton, Alberta on 23 July

Education

1950	B.Sc., chemistry, University of Alberta
1952	M.Sc., chemistry, University of Alberta
1955	Ph.D., chemistry, University of Illinois

Professional Experience

1950-1952	Instructor, University of Alberta
	General Electric Company
1955-1968	Research Chemist
1968-1980	Manager, Chemical Laboratory, Research and Development Center
1980-	Research and Development Manager, Chemical Science and Engineering
1975-	Adjunct Professor, Polymer Science and Engineering Department, University of Massachusetts

Honors

1970	Fellow of the New York Academy of Sciences
1975	International Award in Plastics Science and Engineering, Society of Plastics Engineers
1977	Rauscher Memorial Lecturer, Society of Plastics Engineers
1981	Fellow of the Royal Society of London
1984	Achievement Award, Industrial Research Institute
1985	Carothers Award
1985	Chemical Pioneer Award, American Institute of Chemists

ABSTRACT

Allan S. Hay begins the interview with a description of his secondary and undergraduate education in Alberta. After briefly describing his graduate work at the University of Illinois and a summer job at Du Pont, he begins the story of his career at General Electric. There, after only a very short time, he was able to oxidize xylenol to synthesize PPO. Hay focuses on the practical applications as well as the chemical aspects of the progress that occurred in plastics research (including the developments of Noryl and Ultem) during his career as both a research chemist and a manager at G.E. He concludes with a bit of insight into what lies ahead in polymer research and development.

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 reserach, 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: Schenectady, New York

DATE: 24 July 1986

FINE: We've gathered here on a hot, humid day in Schenectady to talk with Dr. Hay about engineered plastics materials and to see if we can gather together some facts about his life and career. I hear you're going up to Alberta, your home turf, to give a talk or two.

HAY: In October.

FINE: Do you get back there very often?

HAY: No. My mother and two sisters still live there. More often than not my mother usually comes down to visit us. It works out to be more convenient that way. She's coming next week, as a matter of fact.

FINE: I guess we should begin at the beginning, your Canadian origins, your family, and perhaps something about what it was that predisposed you to chemistry. I have learned that you filled the house with smoke and fumes on more than one occasion. Were your parents scientists?

HAY: No. My father was an automobile mechanic. He was born in Scotland and emigrated to Canada in 1911 when he was about twenty years old. A few years later he ended up in the army in World War I, and he was unfortunate enough to end up with a lot of shrapnel in his legs. Over his lifetime he spent about two years on and off in the hospital as the shrapnel emerged. He never had the opportunity to go on to further his education. His younger brother was a little more fortunate and he ended up being head of an agricultural college in England. My mother was raised on a farm. So there was basically no scientific background in the family at all. I went to high school in Edmonton, Alberta, a relatively small high school of about three hundred people. Edmonton, at that time, had a population of about seventy-five thousand to one hundred thousand. The high school curriculum included two years of chemistry.

FINE: Advanced thinking.

HAY: It had a first course in the tenth grade and then one in the twelfth grade. Of course along with that you had various physics courses and math courses, etc. Actually, if you graduated from high school in Alberta at the time, you could enter second year university in most places because what they taught in high school included a curriculum where you could go at your own pace. You could take high school in three, four, or five years, depending on the individual. The courses were all the same, but you paced yourself with what you were able to accomplish. I was able to finish high school when I was sixteen and that is when I became interested in chemistry. I had a small laboratory in my basement and would make such things as thiokol rubber and things like that.

FINE: Classic old experiments.

HAY: That's right. Some of the standard stuff that creates a lot of odor; things like gunpowder that tend to be very unpredictable and go off when they shouldn't and create lots of smoke. I must say my parents were very tolerant. I didn't really have a good concept of where to go as far as a career was concerned. I had thought at that time, for no particular reason as I recall, that I would go into chemical engineering.

FINE: There was no question that you would be able to go on to college, of course.

HAY: That's correct.

FINE: Your parents had the economic means?

HAY: No. But I lived in Alberta and university tuition was only $180 a year. I could get there by a streetcar, which cost five cents, and I started working summers when I was fourteen. So I was able to work things out pretty well.

FINE: What was it about chemistry that attracted you at the age of thirteen and fourteen to acquire a laboratory in your home? The old Gilbert chemistry set, was that it?

HAY: I may have had one of those at one time, but then I would simply go to the drug stores, because the drug stores at that time stocked almost everything and you could get all sorts of stuff and mix it together. I can't recall, at this point, what the incentive was. At any rate I went over to talk to the people at the university, and the then chairman of the chemistry department interviewed me. His name was Osman J. Walker and he convinced me that I should take the honors course in chemistry. In the Canadian universities, you could get the standard bachelor's degree in three years. If you took an honors course it was a four-year course and the last two years were highly specialized and were almost all chemistry. This meant that by the time you reached your junior year you were essentially working full-time in the laboratory. At any rate I was talked into entering the honors chemistry course where the school year was a full year. You took a course for a full year; never did have semesters. So I took chemistry courses the first year.

FINE: A full year, but not summers?

HAY: That's right. Organic chemistry and the standard inorganic course that used to be taught, both of which had three-hour laboratories associated with them. That was pretty good basic chemistry in the first year. The reason that I became very, very interested in organic chemistry was because of that picture I have on the wall.

FINE: Was that [Reuben B.] Sandin?

HAY: That's Sandin. He was an inspiring teacher, by far the best teacher I ever had in my life, as far as inspiring people was concerned. People would sit in on his courses and come back in the second year, just to hear him give a course. Now that's pretty good for organic chemistry.

FINE: What was so special? He was a dynamic lecturer?

HAY: He was just an extraordinary lecturer; he would stop at times and he would just philosophise. At certain times of the year the sun would come through the window and shine on a particular spot, and he would take half of the hour in simply talking about things. He was just really a remarkable fellow. He is eighty-eight now and still working in the laboratory. He still has a laboratory at the university. At any rate, he was the strong personality in chemistry at the University of Alberta, and he basically took the honors chemistry group under his wing. I think there were nine people in the class, or something like that. He essentially considered this group of people to be his family.

FINE: He would have been in mid-career then, age forty-five or something like that at the time.

HAY: At that time they did not have a graduate school at the University of Alberta, other than you could get a master's degree. They did not have a Ph.D. program, so most people would then go directly to graduate school from there. Most of the honors chemistry students went to graduate school, a graduate school that Sandin would choose for them. He had developed a reputation such that if he wrote a letter off to a university, a fellowship would come by return mail. This was a pretty select group, a small group that had been picked for chemistry. I did not go on to graduate school right away because he also asked a few people——I shouldn't say a few, one a year——to stay on and work with him and do research for two years while also doing some teaching, handling the laboratories, and so forth. I stayed on and worked very closely with him for the two years after I got my bachelor's degree.

FINE: Who was Sandin, chemically? Where did he come from?

HAY: He worked for [Julius] Stieglitz in Chicago.

FINE: Oh, he worked for Stieglitz in Chicago.

HAY: He was a very creative individual. He does not have a large publication record because that really wasn't too possible. The number of students was very small and they had no Ph.D. program at the time.

FINE: Have a lot of chemists come from there?

HAY: Oh yes. You can go all over the country to major chemical companies and find people of my era. Even though there were only half a dozen a year, it adds up over the years and he probably directed the careers of, I would guess, a couple of hundred people. Many of them ended up in "in" places. Ted [Theodore L.] Cairns, for example, of Du Pont comes to mind. Who's the fellow who is head of Mobil research? Don [Donald D.] Phillips, for example. There were a number of people at Du Pont laboratories who were graduates from Alberta. I could add Ray [Raymond U.] Lemieux and Alex Nickon.

FINE: He seems to have inspired people to careers in industry and yet he was an inspiring teacher. Did you think of going academic at any point?

HAY: I did at some point, but then I decided I would much rather do research. He chose the University of Illinois for me for graduate school. He wrote a letter to Speed [Carl S.] Marvel, who was then the chairman of organic chemistry there, and by return mail a fellowship came. So I went to the University of Illinois.

FINE: Marvel had a racket. He didn't have to screen anybody.

HAY: I worked for Nelson [J.] Leonard at the University of Illinois. I was in a rather unusual position compared to most of the Illinois students in that I already had several years of research.

FINE: And publications.

HAY: And publications. So I was immediately able to start doing research and as a result I was only there two years.

FINE: Could we just go back one short step? Of the five or six people who were in your class at Alberta, did any of them turn out to be as famous as you?

HAY: No. I've lost track of most of them at this point, with the exception of one fellow who was on the staff at the University of Alberta who is one of my good friends; every time I go back there I see him. I mean I'm aware of where a couple of the others are, but you also keep track of the people a few years on either side. For example, Alex Nickon is an example of a graduate who went into academe, and Leon [E.] St. Pierre, who used to be here and is now at McGill. There are a number of others. There are a number who are in academe, actually, when you come right down to it.

FINE: Did most of the people end up going to the States to graduate school?

HAY: Yes. At that time there were really only two graduate schools in Canada. It was either Toronto or McGill. Sandin would send a few students to McGill for graduate studies. For some reason he did not like Toronto and so he never sent anyone to Toronto. As a result, there were about half a dozen schools in the U.S. that his people would go to regularly: Illinois, Wisconsin, Minnesota, Berkeley, and occasionally someone to

Harvard. St. Pierre went to Notre Dame and I think a couple of other people went to Notre Dame also. He had built up contacts at about half a dozen schools.

FINE: So you were really in a fertile environment for organic chemistry.

HAY: As I say, for the last two years of my bachelor's degree, I was basically working full-time in the laboratory and as well I worked summers doing research projects. So from the time I was eighteen I was essentially involved full-time in experimental chemistry.

FINE: Did any of this early work have anything to do with polymers?

HAY: No. Like most graduate organic chemists, I never knew what a polymer was until I ended up in industry. Well, I shouldn't say that. There was a polymer effort going on at the University of Illinois at that time with Speed Marvel, but people sort of turned their noses up at the polymer chemistry at that era, because they didn't have the tools available at that time to really allow a proper characterization of a product. Incidentally, that still holds today in chemistry departments even though the analytical tools are available for a full characterization; that is still a bias in the universities. You can take the top twenty chemistry departments and there is no polymer chemistry taught at all to speak of.

FINE: Things are actually getting worse, I think. Marvel was not replaced at Illinois, for example; I'm not sure when [William J.] Bailey retires whether Maryland will replace him with a polymer chemist; and so on.

HAY: It's kind of an unusual situation. So I worked for Nelson Leonard on a natural products project (1).

FINE: I noticed the publications that you did with Sandin (2). It almost seemed obvious that you would go to work for somebody like Nelson Leonard and continue that sort of research. Is that why you chose to work for Leonard?

HAY: That was Sandin's choice for me.

FINE: That too.

HAY: Not exactly. Sandin chose the University of Illinois but one chose the professor when there. There had been a lot of people who had gone from Alberta to Illinois, by then, and people followed after me. For example, two years later a fellow whom I had known quite well at Alberta, Bob [Robert J.] Crawford, went down and he is now back teaching at the University of Alberta. When I graduated from the University of Illinois, I began looking for a position in Canada but found that essentially there was no research being done there. You really didn't have much choice but to go down to the U.S.

FINE: Were you looking for natural products? Pharmaceuticals?

HAY: No, not really. I was just interested in organic chemistry and I could have ended up in almost any area. I interviewed with a fairly large number of companies all around. The reason I chose General Electric was that they told me that I could do whatever I wanted.

FINE: Who told you that?

HAY: Jack [John R.] Elliott. He was also a University of Illinois graduate. When they came through they essentially told me I could do whatever I pleased.

FINE: Actually one of the things that I find not even surprising anymore is that our Ph.D. candidates have to be dragged kicking and screaming to interview G.E.; they have no idea that G.E. offers careers for organic chemists.

HAY: I did not sign up to talk to G.E. What happened was that Elliott came out on an interview trip and he talked to Nelson Leonard and Leonard asked me if I would talk to him. That was the only reason I ended up interviewing at General Electric because I had absolutely no idea...

FINE: Nothing has changed.

HAY: I had no idea that they were interested in doing any chemistry.

FINE: You had a summer at Du Pont before that. Could you have gone back there?

HAY: Yes.

FINE: Was the problem that they didn't offer you the same amount of freedom?

HAY: That's right. With General Electric it just seemed like an unusual situation.

FINE: You were at Du Pont in 1953; you really did get out in the heyday of the chemical industry. What kinds of things did you do that summer you spent with Du Pont?

HAY: This was in the very early stages of the major program that Du Pont Central Research had on cyanocarbon chemistry.

FINE: Oh. Tetracyanoethylene...

HAY: Tetracyanoethylene had just been discovered about a year or so before, maybe 1951, so I was working on TCNE derivatives. During a summer you don't accomplish an enormous amount in a couple of months' time, but it was a very interesting experience and it at least gave me an idea of what an industrial laboratory would be like to work in. I spent a very pleasant summer with four other Illinois students. We rented a house for the summer. We all worked in the same place. It was a very enjoyable summer going out to the beach in New Jersey and so forth.

FINE: Actually, this is probably jumping a little bit ahead of the game, but TCNE chemistry, that kind of chemistry, probably leads to quinone chemistry and antioxidant chemistry and a whole range of the things that you eventually got into.

HAY: The work that I did there was, as I recall, things like the reactions of TCNE with dimethylaniline, which gives you substitution in the four position, and this was one of the materials that they tried to exploit as a dye. I did a lot of degradative studies on that. It's interesting, of course, when you look at an area like that. I don't know how many man-years they spent on that project over the next ten years, but an enormous number. I don't think there are any commercial products to speak of from that massive amount of chemistry that was...

FINE: Interesting scientifically because of the electron flow through the molecules.

HAY: Of course, the original incentive was that it would polymerize because it would be similar to tetrafluoroethylene with those electronegative groups. It was really a surprise to find that one of the cyano groups would displace so easily.

FINE: That would have been the summer of your first year at Illinois?

HAY: That's right.

FINE: Nelson Leonard was willing to let you go do that.

HAY: Yes. It was fairly common, at least with Du Pont. Du Pont had a pipeline to Illinois because, of course, they had Roger Adams and Speed Marvel who consulted for them. Speed still consults for them. In fact, I remember him saying that he had spent eighteen months in Hotel Du Pont in Wilmington in the sixty years that he had been consulting for them, or something like that.

FINE: Poor guy.

HAY: So there was kind of a special arrangement with Du Pont. They encouraged students to go there for the summer. I went there for the one summer. It isn't really that common now, is it?

FINE: I would say at this point you'd be putting yourself in jeopardy with your research sponsor if you walked in one day and said, "By the way, chief, I'm going to work in industry for the summer."

HAY: I think they truly felt that it was an integral part of a person's education to be able to spend one summer at an industrial laboratory.

FINE: I would think that that would be invaluable and you are the beneficiary. Did it have any influence on your ultimately going to work in industry?

HAY: Probably.

FINE: How about your summer at NRC [National Research Council]? Did that provide any long-term benefits to your career?

HAY: That was the summer before I went to Illinois. It was also an interesting experience because it gave me the chance to learn this marvelous new technique, infrared. It is hard to imagine today, with the analytical tools that we now have, that during the period when I was an undergraduate, chemistry was being done about the same way it had been about forty years ago, with very little change. When you ran an experiment, when you isolated something, you did all your analyses, all of the carbon and hydrogen analyses, all your own halogen analyses, your Kjeldahl nitrogen analyses, so you were very careful to have pure samples. The only spectroscopic technique that we had available was ultraviolet, with one of these little instruments that would give you one point at a time and then you'd have to plot your own graph. Infrared was something that we had heard of that was coming along. I worked with R. Norman Jones at the NRC who was a pioneer, primarily interested in infrared.

FINE: That's interesting because gas chromatography was coming along, so by the time you got to Illinois...

HAY: That was before G.C.

FINE: By just a year or two.

HAY: It was not available and NMR was still to come. Research was done rather differently then than it is today.

FINE: When you were at Illinois, it was probably one of the preeminent chemistry departments in the country.

HAY: It was.

FINE: I guess [Elias J.] Corey was there at that time. Roger Adams was still active and Speed Marvel.

HAY: [Reynold C.] Fuson and [Harold R.] Snyder. It was clearly one of the very top organic departments in the world. It was a very, very large department and at that time half of the department was organic. Now, the breakdown is more normal between the various disciplines, but then it was highly oriented towards organic chemistry.

FINE: Were you aware at that time that you were in this hotbed of organic chemistry?

HAY: Well, I certainly was aware of the names because Snyder and Fuson and Adams and Marvel were all really the big names.

FINE: Yes. Snyder and Fuson wrote the textbook (3).

[END OF TAPE, SIDE 1]

FINE: Good old organic chemistry. Was that the feeling that you got there or was polymers something that you sensed was going on?

HAY: Polymers was something that was attached to the work that Marvel was doing. I don't think that most chemists thought that it was very exciting, again, for the reason the products of the reactions were, to a large extent, uncharacterizable at that time. A number of the things that were done in those days had vile odors attached to them, mercaptans as transfer agents for vinyl polymerizations and so forth. I think he was sort of separate, in some ways, from the rest of the group because essentially all of his work was polymer oriented by then.

FINE: The majority of the graduate students would just look askance at going to work for Speed?

HAY: Many of them, yes. That's right.

FINE: So Jack Elliott comes along interviewing graduate students and he sold you a bill of goods.

HAY: He invited me out for a trip and I managed to squeeze it into the itinerary that one sets up when one goes to half a dozen different places. When I came here, as I say, I was just attracted by the possibility of going someplace and doing what I felt like doing. Obviously, as a summer student at Du Pont, I didn't really learn too much about the system, but it did seem to me that things there were reasonably well controlled, as far as the areas that people worked in. I'm not sure that I was very knowledgeable on the subject; I don't really know what the facts are.

FINE: When Elliott talked to you at Illinois, was he able to describe organic research to you that was exciting?

HAY: I think it was probably more the feeling that they had a commitment from the powers that be to put a substantial effort into trying to develop new chemical businesses. As you know, their chemical business was pretty small at that time. I guess it was just appealing to be able to go to a place where it looked like they clearly had the resources available and were interested in getting into new businesses.

FINE: So you were really banking on an attitude for the future rather than anything about silicone chemistry or phenolics and alkyd resins which really would not have been so exciting at that time.

HAY: I can't even recall the interview at this point.

FINE: You interviewed here at Schenectady?

HAY: Yes.

FINE: Did you even know Pittsfield [Massachusetts] existed?

HAY: No.

FINE: They kept that a quiet secret. So you finished up your thesis, which was essentially, in many respects, a continuation of the kind of work you had done as an undergraduate, good old organic chemistry, and you got here. What was your first project?

HAY: They essentially shook my hand and said, "There's the laboratory. Go talk to people." I had a group leader at that time, a fellow by the name of Don [Donald E.] Sargent, but I was encouraged to go and talk to people to see if I could come up with some topics of what would be interesting ideas to work on. One of the things that I had worked on at the University of Illinois, actually the major thing, was doing oxidation of nitrogen heterocyclics with mercuric acetate as the oxidizing agent. That's kind of an interesting thing to run because mercury is so heavy that the amount of reagent that you use substantially exceeds the weight of the material that you are oxidizing. On a weight basis at least, it is hard to think of a more inefficient way to run a reaction. But I was sort of interested in oxidation. Just in talking with the people, I learned, of course, what businesses the company was in. They were just in the last stages of developing Alkanex wire enamel at that time, which had been a very major project at the Center. This is a polyester of terephthalic acid and ethylene glycol with some trifunctional agent in it for crosslinking. Of course, the company at that time was also in the alkyd resin business, which was also based on phthalic acids. It looked like an interesting area might be to try and develop some chemistry that would be useful for oxidizing xylenes to phthalic acids. If you are going to do that, it seemed that the simplest way would be to do it with oxygen.

So I started to look for experiments to see if I could find a way to oxidize the xylenes. It didn't take too long before I came up with an interesting process in which you had a cobalt catalyzed reaction initiated with ozone to raise the cobalt to the trivalent state, after which it would be self-sustaining. That worked quite well except, interestingly enough, it would only work for the meta and para isomers. It wouldn't work with the ortho, which we figured out was because once you've oxidized to some orthophthalic acid, it would chelate with the cobalt and deactivate it. We then started looking at ways of getting around that deactivation and looked for acids with different pKs to see how it would be affected. One that we put in, a stronger acid, was bromoacetic acid, and we were rather astonished to find that the reaction just took off by itself. We soon found that the reactive catalyst was in fact a cobalt acetate bromide, and this turned out to be a very efficient catalyst for the oxidation of xylenes to the phthalic acids. This generated quite a bit of interest and our chemical engineering group got involved and we started scaling it up to the bench scale just to see how good the reaction was, and it looked very good. That was when our alkyd resin plant burned down in downtown Schenectady. I think that was in March of 1956.

FINE: So you had been here less than a year?

HAY: Yes.

FINE: Just one thing. Was there a group of people working on this, or were you in the lab by yourself?

HAY: I was by myself.

FINE: So this was all your own thing. Elliott was your boss?

HAY: Yes. He was the branch manager, at that point. As I recall, Don Sargent was a group leader at that time.

FINE: A Ph.D. chemist.

HAY: Yes.

FINE: Jack Elliott is a Ph.D. chemist, as well.

HAY: Yes. University of Illinois; he worked for Speed Marvel.

FINE: I see. It's the "old boy" system at work.

HAY: Absolutely. After the plant burned down, we proceeded on the project for a while, but the company was less than enthusiastic about developing a process for phthalic acids because later on in the year or the next year, I don't remember which, they sold the business to Archer Daniels Midland and got out of the business completely. At that point, they didn't really have any interest in backward integrating. They did a study of the process over in our chemical department in Pittsfield and they came up with a projection that terephthalic acid would only be a specialty chemical in the reasonable future and on that basis the project was killed. I still have a copy of that report; it's kind of interesting to look at. The whole thing got killed at that point and we didn't file the patents the way we should have. Then another surprise came along because in a patent issued to Scientific Design, then it was [Alfred] Saffer I think, claimed a process with a bromine assisted oxidation of xylene to terephthalic acid. The differences in the processes were that whereas the process that we had developed took place at essentially atmospheric pressure and 120°C, and it gave essentially quantitative yields of terephthalic acid, the Scientific Design process took place under pressure at about 190°C for a ninety percent yield. But it ended up with them having the basic patents on the bromine assisted process, even though I feel there was a substantial difference from ours. The problem was that we did not really do the job that we should have in filing the patents. For example, we never filed overseas patents. As a matter of fact, that resulted in changing the whole process for filing overseas patents; subsequently the Center was always consulted on whether something should be filed overseas. That [oxidation] process became the basis for most of the production of terephthalic acid worldwide today. It was sold to Amoco and this is the process that Amoco uses today. I still don't think they have quite the right process, but that's by the way.

FINE: I don't know how you deal with decision making and hindsight, but there are so many instances where you can point out something like that where, but for having made just a modestly different decision, you would have had a business of great proportion. The polymer industry is full of examples of that. All the people who passed up the polyethylene licenses when [Karl] Ziegler came over here, for example.

HAY: The future is pretty difficult to predict, isn't it? Anyhow, we were rapidly getting out of that project and we continued on ahead looking at different substrates that we could oxidize. Some chemical engineers were added and there was another fellow working in the laboratory with me, John Eustance, and then there was another fellow, [Harry S.] Blanchard, who was brought onto the project. He worked on some of the mechanistic aspects of the process. But after these decisions, the effort was rapidly de-escalated and we started looking around for other things to work on. At this point I was still very interested in catalytic oxidations, as I really felt it was a theme worth pursuing. I still had no interest in polymer chemistry per se, because I hadn't really been involved in any polymer chemistry except that the products of this process potentially would go into the polymer businesses that the company had at the time.

FINE: Were you very much aware of the polycarbonate work that was starting in G.E.?

HAY: Not too much. I had talked to some of the people over at Pittsfield because they at one time were considering scaling up the terephthalic acid process. So I was certainly aware of it. Of course I knew they were in the phenolic resin business and that was why I read the thing that triggered off the new area I got into, a Russian paper which reported a very simple catalytic oxidation of aniline to azobenzene. This process was rather remarkable in that it went at room temperature and gave the product in some very high yields simply by taking a pyridine solution, adding some cuprous chloride as a catalyst and bubbling oxygen through it. The traditional way of making azo compounds is quite a bit more complicated than that. I did a little work then just to see whether or not there were any diamines available that might make a polymer. I knew that much about polymers. In retrospect the materials that were chosen were such that, even if they went to a polymer, it would be so highly crystalline and insoluble that you wouldn't be able to get much out of it. Actually there was a fair amount of work in subsequent years, particularly in the U.S.S.R., and a lot of papers came out, but nothing, not surprisingly, ever came out of it. As we were in the phenolic resin business and we were getting into the polycarbonate business, the basis for which was bisphenol, I was really interested in the possibility of taking a system such as the one for oxidation of aniline and seeing if you could run a catalytic oxidation and get a product. I probably was thinking most of the time of getting bisphenol out of it, which would be a raw material for the emerging polycarbonate effort. Immediately I started to see what kind of phenols one could oxidize. The simplest one, phenol itself, gives absolutely no success.

FINE: You get all these quinones, these terrible things when you oxidize phenols.

HAY: Yes. You get all sorts of things. In retrospect, in the oxidation of phenol, almost anything that happens gives a product that will oxidize easier than the starting material so you automatically end up with a very complex mess. To simplify the reaction, the simplest thing would seem to be to look at various substituted phenols to eliminate the number of reaction possibilities. A hindered phenol like 2,6-di*tert*butylphenol carbon-carbon coupled to the four position; of course this chemistry was well known in the literature by then. There had been a fair amount of work on that and similar systems, so we looked at materials like the naphthols. With *beta*naphthol, you'd get some carbon-carbon coupling. We went through a number of experiments, and then it turned out that one of the samples in the stockroom was a bottle of 2,6-dimethylphenol and we tried that reaction. We were a little astonished to see the reaction stay nice and green, but in a few minutes the reaction became very viscous. Again, I really knew next to nothing about polymers at the time, but I guess I knew enough to know that something unusual had happened. I remember isolating the material by dumping the reaction mixture into methanol and washing it and ending up with a product that would dissolve in chloroform and, on evaporation, would produce a film. I remember taking it down and going into Elliott's office and showing him this. He was having a meeting then; I guess nobody knew I was working in this oxidation area. Of course, nobody believed the reaction because it was unprecedented as a polymerization.

FINE: It wasn't a condensation and it wasn't an addition. What had you done?

HAY: It didn't take very long to get the analyses, such as were available at that time. We didn't have any capabilities for doing any molecular weight determinations, for example. But its properties showed that it was a reasonably high molecular weight.

FINE: From the viscosity.

HAY: That's right.

FINE: Was this whole thing in one day? It wasn't something that you had to take weeks to set up or anything.

HAY: That's right. In fact, just a morning.

FINE: What were the reaction conditions? You just dissolve this xylenol...

HAY: All you do is take pyridine, add a little bit of cuprous chloride to it, put in the 2,6-xylenol, stir it and bubble oxygen through it and in twenty minutes you have a polymer. It's hard to think of anything easier.

FINE: At room temperature.

HAY: At room temperature. The reaction was exothermic so it would warm up to 50° or 60°. Clearly something was happening because it was a very exothermic reaction.

FINE: Have you demonstrated this subsequently, how easy it is to make a polymer?

HAY: Actually it is used in a couple of schools. The University of Massachusetts, for example, has it in their laboratory manual for students. It's one that should be used, because it's so easy. It's one that should be used in graduate laboratories.

FINE: I knew there had to be a better way of doing these things. I've been looking for polymer demonstrations to put into our organic synthesis laboratories. They're very hard to find, experiments that are relatively easy to do.

HAY: You don't have to worry about purifying things and you don't have to have it anhydrous. You don't have to have it in the absence of oxygen, because you're using the oxygen as a reagent. It's hard to imagine a more foolproof polymerization experiment.

FINE: When you wrote this up in your lab notebook, did you make some profound or prophetic comment about it?

HAY: I have no idea. I'd have to look. I certainly could look that up.

FINE: You did think it was a big deal right away, didn't you?

HAY: I guess I don't really know. It was certainly unusual; I didn't know enough about polymers to know whether or not it was a big deal. Most people thought it was kind of an interesting reaction, but you have to remember the chemical background for most of the old-timers around here was in wire enamels. Their first thought was, "If it's not any good on wire, it's not any good for anything." That was really what people would tell you. You'd show them something and the first thing they'd say was, "Is it any good on wire?"

FINE: I guess that's a throwback to Edisonian research. You had been here a year.

HAY: No. That was August of 1956, so that was about a year and a half. I came in March of 1955.

FINE: How long was it until somebody decided that it was really worth exploring? Was it a day, a week, or a month? Were you really on your own to decide to follow this up? You could have conceivably set this aside and gone back to your basic idea, or you could have said, "This is really interesting."

HAY: People were very interested because it was a new polymerization reaction and a very novel one.

FINE: You showed them the film right away?

HAY: I can't recall. That was one of the first things that we did. It was really the simplest way to demonstrate that you had something. There was quite a bit of interest in it, and we started to make somewhat larger quantities so that we could get a better feel for it and get some idea of the material properties. We had no really good facilities around; we couldn't even measure an intrinsic viscosity. Some people in the polymer physics area had some equipment. Getting a molecular weight determination was a big deal at that time, because you had to get an osmotic molecular weight and that might take you weeks or, with light scattering, it might take you even longer. And after you got them you couldn't rely on the results. You really relied on viscosity and elemental analysis and infrared. That was really about all we had at the time. The other thing you must remember is that 2,6-dimethylphenol was not really an item of commerce. The sample we had was from Shell Oil material and was really a by-product of the coal tar industry and was relatively impure. At that time, it was a relatively expensive raw material. In relatively short order, people heard about this reaction in Pittsfield but there really wasn't too much interest because they were very busy. They had a case of indigestion just developing the polycarbonate business at that time.

[END OF TAPE, SIDE 2]

HAY: I can't remember exactly how many people were involved, but the project was expanded. We had some people trying to make larger quantities of the material for evaluation.

FINE: This was still under Elliott's direction, wasn't it?

HAY: Yes, it was. I began to look at enlarging the reaction to see what happens to other phenols.

FINE: Without really having a feel for the properties of the polymer yet.

HAY: That's right.

FINE: It was just a polymer and therefore...

HAY: Yes, but it was in a relatively short time that we got some feel for some of the properties for the 2,6-dimethylphenol polymer. It was relatively easy to get the heat distortion temperature and the glass transition temperature but then, we had these parallel efforts in scaling up the material, scoping the reaction and then, shortly afterwards, also had some people that began to look at the mechanism of polymerization. No one had absolutely any idea of why this polymerization took place, and it really took a number of years to get a good idea and a good feel for the mechanism of the polymerization. Two or three people worked on that for quite a long period of time.

FINE: If PPO had not been clearly promising, based on the properties of the polymer, would a study of the mechanism have followed as a consequence of the atmosphere and attitudes up here in Schenectady?

HAY: I guess that's hard to say. I think that it helped that the first experiment gave high molecular weight linear polymers. If we had a low molecular weight material, it probably would have gotten thrown away. It would have been unusual but people in the literature had done oxidations of 2,6-dimethylphenol with other oxidizing agents and had gotten mostly carbon-carbon coupling products plus some low molecular weight by-products, which they discarded, but in retrospect were probably oligomers of maybe ten units, well below the point with any polymeric properties.

FINE: You'd need what, one hundred units before you'd start seeing typical polymeric behavior?

HAY: Probably. At fifty you'd probably start seeing something, but you'd have to get closer to one hundred before you had close to the optimum properties. The project went on for a number of years, exploring it as a new polymerization method. Maybe that answers your question. The project was continued without knowing whether there was any commercial possibility, and without getting really too much encouragement from the chemical business. I also then began going back to see whether indeed you could polymerize phenol. In analogy to enzymatic oxidations, one way would be to change the catalyst so that the catalyst actually physically blocks the ortho positions, so we began looking at different catalyst structures. It had some success. We were able to make high molecular weight polymers from phenol.

FINE: You were using complex amines?

HAY: But we were never able to make linear polymers. We'd always wind up with a substantial amount of branching, a high molecular polymer, but highly branched and without any useful properties at all. That is still considered a major carrot because that polymer has now been made by a different means. People have made the high molecular weight linear polymer by condensation of parabromophenol and have shown that it is a material of interesting properties. It has properties similar to Mylar, except that it would be very hydrolytically stable, because it's an ether.

FINE: Is this Carbide chemistry?

HAY: No, this was done at Du Pont and at 3M.

WISE: Why is the linear polymer better than a branched one?

HAY: To have the properties you have to have entanglement of the chains. With highly branched material, then it's just a blob of material in a solution so you get no entanglement. That's probably still an interesting carrot because the problem with the product from parabromophenol is that it is an interesting material but the cost would be much too high. You have to make sodium salts and you have to have high temperatures and expensive solvents for fairly long reaction times. Whereas if you could do it by direct oxidation of phenol, you would have a real winner, even today.

FINE: Can you spin a fiber out of PPO?

HAY: Yes.

FINE: This was the heyday of the fiber business.

HAY: Why don't I come back to that part of it, once we get to the commercialization. Anyhow, the whole thing went on for about four years before there was any interest at all from our chemical department in Pittsfield.

FINE: Was that because of the polycarbonates?

HAY: That's correct. At that time the polycarbonate business was on its way and their chemical development operations, which was an entrepreneurial group that was formed to develop new products, began to get interested in what their next product would be. So they decided to have a new look at the polymer from 2,6-dimethylphenol. We had given up, at that point. Earlier they had suggested that if you can oxidize phenol or orthocresol, or something that was commercially available, you've really got something. But at that point we had essentially given up on being able to make linear high molecular weight polymers from those phenols that were commercially available. So then they decided to look again at the 2,6-dimethylphenol polymerization and they started to scale it up in Pittsfield.

FINE: What about your own career at this point? You had been at G.E. for five years. Was Elliott still your boss?

HAY: Yes.

FINE: And Sargent, was he still in the picture?

HAY: No, he had gone at that point. He went to work for 3M.

FINE: Had you advanced to the point of being the equivalent of Sargent, let's say?

HAY: No. We didn't have any managers at that point. We had a group that just worked together.

FINE: It sounds like a good arrangement. Fewer chiefs and more Indians.

WISE: Were you able to publish under Elliott?

HAY: The first publication was in 1959, which was before they started working on the commercial development in Pittsfield (4). The project was three years old at that time. We had our patents well filed, so we were able to convince people that we should put in a communication to the editor of JACS [Journal of the American Chemical Society], which we did.

FINE: Did that generate a lot of interest?

HAY: Quite a bit. It is interesting that it brought out of the woodwork some other work that was going on, or had been going on, in other places. For example, we mentioned Leon St. Pierre earlier. Leon St. Pierre had worked for Charlie [Charles C.] Price when Charlie was at Notre Dame. It turned out that Price had been working on the polymerization of 4-bromo-2,6-dimethylphenol and had been able to obtain low molecular weight polymers and he still had a project going. I guess it was a bit of a surprise to him.

FINE: Did Leon work on that?

HAY: No. Leon worked on polypropylene oxide when he worked with Charlie Price.

FINE: When Leon was here, was he involved in polymers at all?

HAY: He was involved in polymers, yes. He got involved in silicones. He ended up going more in the physical chemistry direction rather than the synthetic direction. I guess he knew Charlie Price was working on this. So it got a fair amount of people interested and I think it also got a few people in industry involved; there was an effort started in this area and, as things go in the patent area, we did end up with an interference at one point with the work that Price was doing, which he had assigned to U.S. Rubber. I think it was U.S. Rubber that he had sold it to. When we went to Du Pont we were able to show that we had made high molecular weight polymer and they hadn't; we preceded them anyhow.

FINE: You were a lot more sensitive to patent matters at this point because of the terephthalic acid business?

HAY: Yes. I had had an indoctrination in patents, and at that point I was reading the Official Gazette.

FINE: How about the polycarbonate patents? Were you also aware of difficulties in the offing with the Bayer and the G.E. patents?

HAY: Not in any great detail, but certainly I was aware of it. Anyhow, they started putting effort into it in Pittsfield, and then in relatively short order we were able to make fairly substantial quantities of the material.

FINE: When you say "they" started making efforts in Pittsfield,...

HAY: In 1960, yes.

FINE: ...who was that?

HAY: The manager of the chemical development operations at that time was Reuben Gutoff, and I guess Dan [Daniel W.] Fox was working for him. Dan had a critical part in bringing the material to his attention.

FINE: Is it a process development effort that is going on at this point?

HAY: Initially it was simply a scale-up to make larger quantities.

FINE: For evaluation.

HAY: For evaluation. To see if you could mold it or do anything with it. Then, I can't remember the exact date, they also hired [John F.] Jack Welch.

FINE: Who is "they"?

HAY: Gutoff and Fox. I don't know who did it actually.

FINE: Everybody claims they hired Jack.

HAY: Yes. I don't know who actually gave him the offer letter. I've heard a number of people claim it. Maybe you'd better ask Welch.

FINE: Welch was part of this Illinois networking, as well.

HAY: Yes. I was on the recruiting team that interviewed him when he was looking for a job. It would have been the recruiting season of the fall of 1959 because he was getting out in mid-1960, something like that I think. As far as I could see he had basically decided that he wanted to work in Massachusetts because that was where he had been raised. There were not too many choices. I think he picked General Electric...

FINE: Because of geography? Isn't that something.

HAY: It might be interesting to ask him that. We also started looking at other types of materials which could be oxidized, and later on we had oxidation projects with various substituted acetylenes and spent a lot of effort in that area. That project also got to the point in the late 1960s where we had quite a large effort here. It got to the pilot stage to make these materials as precursors for carbon fibers. The materials were of interest because the basic polymer has the empirical formula $C_{10}H_4$, which means it contains 96.75% carbon, and yet you can make a transparent film or fiber from it. Clearly then, you don't have very far to go to convert it to pure carbon since it is almost there. One of the potential advantages appeared to be that, since you had a very small weight loss in going to the carbon fiber, you should be able to do it faster. The conventional method with something like polyacrylonitrile requires a very long time to go from the polymer fiber to the graphite fiber, and is therefore not readily amenable to a continuous fiber spinning graphitization process. They felt that even though the raw materials would be more expensive, the yields would be twice as high because you lose hardly any weight and you might have significant advantages in the materials processing. We were able to make graphite fibers that were equivalent to the best that were being made by any method then. But we could not get any interest from chemical business to pursue this project, so it was stopped.

FINE: Did you have any inkling that the polyacetylenes were conducting polymers at that time?

HAY: No, because these ones aren't. They're completely different types of structures. When you polymerize acetylene you really end up with alternating double and single bonds. We're talking about oxidative coupling, which gives you a completely different type of molecule. The ones we were particularly interested in weren't conjugated either. You had to have the unconjugated ones to have solubility. That was another major project that came out of the oxidative coupling work. In retrospect, I think it was a good decision not to pursue the carbon fibers, because even today it is not a large business and nobody is making any money on it as far as I can see. It is one of these businesses that the Japanese have targeted to take over. And their industry I think is one hundred percent over capacity in the world for carbon fibers, and the Japanese are still building plants.

FINE: Getting back to the 1960 period, were you directly involved in the commercialization process and getting it over to Pittsfield?

HAY: Yes, as far as talking to them about the right way to run it, going back and forth with them. I don't know when it was that it actually got to the point where they thought it was interesting, but then they set up a special polymer products operation, as I think it was called. They soon got to the point where PPO looked to be a very interesting material because of the high glass transition temperature and the hydrolytic stability. That was the one thing that distinguished it from polycarbonate, the hydrolytic stability. It had very good electrical properties in addition.

But soon the real barrier became monomer availability. They scoured the world for xylenol and ended up being able to get impure xylenol. Then the challenge was to develop a purification method. If you had to go that route, one, there was a limited amount of the monomer available from coal tar, and two, purification could add substantially to the cost. So a program was initiated to try to develop a synthesis for the monomer, and the key person who was brought in on that was Steve [Stephen B.] Hamilton. He looked at a number of different methods of making it, and there were two methods that looked attractive, one based on cyclohexanone and formaldehyde, and the second, which turned out to be the more attractive, was from phenol and methanol. With the right catalyst, a magnesium oxide catalyst, you could simply take those two and pass them over a hot tube containing the catalyst and end with a high yield of 2,6-dimethylphenol.

FINE: Was this a G.E. process?

HAY: Yes.

FINE: From Schenectady?

HAY: Yes. So what had been a relatively unavailable material all of a sudden became very available, and from very inexpensive raw materials.

FINE: This was the 1960s?

HAY: This was 1962, I would guess.

FINE: Was this something you thought could be done and was just a question of finding the process? It wasn't a surprise.

HAY: No. I think there was a feeling; there had been a lot of work done on the alkylation of phenols.

FINE: The ortho-alkylation business?

HAY: Of course that was with olefins, but there had been work done on alkylation with alcohols and certainly work with alkylation by methanol. I think the surprise was that you could get such a high degree of selectivity for the 2,6 product. That was a very pleasant surprise and one that removed a major barrier to the commercialization of the product.

FINE: At this point had you ever been asked, or thought about, moving out of Schenectady over to Pittsfield and getting into the management of product development and marketing? You know, to go the typical route?

HAY: No.

FINE: Nobody ever tried to pressure you into doing that?

HAY: I'm sure I had been asked and had been quite explicit that I had no interest in that.

FINE: Was there ever any thought to having a research support group over there in a formal way?

HAY: They did. That was where the chemical development operation was.

FINE: So Fox, in a sense, was doing work in that group?

HAY: Yes, there was a lot of back and forth. We've always had very close interactions with the groups.

WISE: That was less true with the polycarbonates, though, wasn't it?

HAY: The polycarbonates were an exception. Fox discovered that in the laboratory here, and there was not really much done beyond a couple of experiments because they could not generate any interest in the management in Schenectady. It was really only because of a key manager in Pittsfield in the chemical department, Al [Alphonse] Pechukas, who had had a background in aliphatic polycarbonate chemistry at PPG. I guess Fox brought this to his attention and he became very interested and championed its development. There was essentially no interaction between Schenectady and Pittsfield on that project.

FINE: Was Pechukas the one who really forced the issue on polycarbonates? The reason I'm so amazed is because his son [Philip Pechukas] is the chairman of the chemistry department at Columbia.

HAY: I didn't know that. Is Al still alive?

FINE: Yes. He's in his early seventies, something like that.

HAY: I think as you go through the chain of events, he certainly is one of the very key individuals as far as the polymer business at General Electric is concerned.

FINE: I don't think Phil knows that. I'll have to tell him.

HAY: He was managing a group over there, and he clearly was the one who championed the polycarbonates and started them off on their profitable road.

WISE: It was really your work that began the tradition of close collaboration between Pittsfield and Schenectady, which pretty much continues uninterrupted until this day.

HAY: That's right.

WISE: Another thing we heard, I guess in talking to Charlie [Charles E.] Reed and also from some other sources, was that there was some difference of opinion in Pittsfield between the people who wanted to keep all the resources on polycarbonates and those who didn't. Did you get involved in that?

HAY: Yes, there were discussions on that. The polycarbonate people wondered why we needed another material having comparable properties to polycarbonate. Of course the reason they wanted another material was they had a group chartered to come up with another product. PPO just happened to be the most likely candidate to pursue at that time.

FINE: Were there financial constraints imposed on the advancement of PPO because of the moneys required to get polycarbonates into the marketplace in a big way?

HAY: No question about it. They had no interest in looking at anything else other than polycarbonates, and rightly so. They didn't have the resources to take on two simultaneously. It was only when the one was well on its way that they were able to pick up another.

FINE: What kind of role did Jack Welch play in this period between 1960 and 1962?

HAY: Well, he rapidly advanced in management during that period. Since he was, right from the beginning, on the chemical engineering side, he was part of the commercialization of the product. As he got more involved in management he took on more aspects of the commercialization.

FINE: Did people then regard these polymers as engineering plastics with special properties as they do nowadays? At that time, there was only one engineering plastic, nylon.

HAY: I think the feeling was that there would be a place for high performance materials that had properties beyond the commodity materials. There was a lot of speculation on what products they would go into, almost all of which were wrong. And of course, as you know, the real success of the business depended on the really great marketing effort.

[END OF TAPE, SIDE 3]

FINE: By the 1970s, with PPO, polycarbonate, and nylon on the market, there is a whole genre of engineering plastics.

HAY: Probably at that time you would have had difficulty finding anybody who could have projected that the business would be the size that it is today. I think that they were viewed only as specialty products, perhaps ultimately of tens of millions of pounds, not the hundreds of millions or the billions of pounds that they will be. There is a difference in scale; people just didn't dream of the business being that size.

Anyhow, we'll come back to the question that you had about fibers. We had published that paper and then we gave some papers at an IUPAC meeting. I can't remember the year but it was in Montreal; it might have been 1960 or 1961 (5, 6). The Dutch company AKU became interested in the chemistry after having seen the publication, and they sent over one of their fellows to that meeting, Jan Bussink. He introduced himself and we talked afterwards. One thing led to another and a year or so later a joint venture was set up with AKU. At that time they set up a laboratory in Arnhem and began working on it [polyphenylene oxide]. Their interest in the material was potentially as a fiber. They had a lot of very creative chemists in Arnhem and we had parallel programs looking at mechanisms and we would have meetings with them periodically to discuss the chemistry. They made a lot of significant contributions to the chemistry of oxidative coupling polymerization.

FINE: This would be the late 1960s?

HAY: I guess the middle to late 1960s. American Enka is a subsidiary of AKU; AKU is now called AKZO. They have a plant down in North Carolina where they set up spinning facilities. General Electric knew absolutely nothing about the fiber business, so this was a potential opportunity to get into a new business.

FINE: Can you spin polycarbonate into a decent fiber?

HAY: You can spin it into a fiber, but it is not terribly interesting because it is soluble in so many things and it does not have the hydrolytic stability. They were interested in PPO I guess principally because of the hydrolytic stability, stable to a strong base so you could wash it. You might have a little difficulty washing polycarbonate because it would disappear in the washing machine. However, we couldn't understand why they were interested in PPO because it is not a crystalline material. It's amorphous and is soluble in dry cleaning solvents and similar liquids. When I asked they said, "Well, that's okay. There's a place for this in the white goods market." They put a rather large effort into development. Depending on how it was spun and subsequently woven, they could end up with a fabric that felt like wool or like silk, with good appearance and handle, and it looked like a very attractive material. A couple of little problems. It turned yellow in sunlight, which is a little bit of a problem. They didn't seem to be worried at all about its solubility in things like dry cleaning solvents because of this special market. The thing they overlooked was that it is impossible to manufacture the material in a textile plant because the machinery is covered in oils and greases which get splattered onto the cloth. The fabric has to be washed with a dry cleaning solvent. You couldn't manufacture the material with existing technology for making cloth, because of that problem. So that sort of died.

In the interim we had, as I said before, given up trying to make linear polymers from phenol, which would have been attractive because they would have much better oxidative stability than PPO itself. Instead we turned our attention to trying to make a completely aromatic polymer. We worked on replacing the methyl groups with phenyl groups making 2,6-diphenylphenol. After quite a bit of effort we not only developed a facile synthesis for the monomer but also an excellent polymerization. It turns out that you can make the monomer in a two-step reaction from cyclohexanone. Simply self-condense cyclohexanone to the trimer, dicyclohexenylcyclohexanone, and then dehydrogenate it and you have 2,6-diphenylphenol. That process went into a pilot plant in Pittsfield and we started working with that material. It looked very interesting for a number of reasons. One, it was thermal oxidatively stable because it is an all aromatic system. But in contrast to the polymer from 2,6-dimethylphenol, it likes to crystallize and if you heat it above the glass transition temperature, which is about 235°C, it crystallizes and then it melts at 480°C. The problem with that was you couldn't consider using it as a molding compound, which was the only business that General Electric knew, because there is no way you can mold or extrude or otherwise process it if it crystallizes on you. With a melting point of 480°C, it would essentially set up in the process equipment. Because of that we did not pursue it then. If we were doing it today we probably would have modified the polymer to make it moldable, because potentially it is a relatively inexpensive molding compound with some interesting properties. For example, the electrical properties are superb and it has one of the lowest dissipation factors of any polymer known; since its glass transition temperature is 235°C, its electrical properties remain constant over a very wide range. As you made the material it was amorphous and was in solution, so you could either cast films or spin fibers out of it and then post-crystallize it. We did not really have the capability to develop a film business and, although there was a fair amount of work done in Pittsfield, it never got to the point where it was really pushed as a commercial business.

FINE: This was what was called P3O.

HAY: That's right. So when the PPO [fiber] project went down the drain, the people in the Netherlands picked up P3O and began to develop it as a fiber. They were interested in it as an industrial fiber. One of the major application areas that they were looking for was as a cloth that would be used as electrical insulation in the EHV underground cable, again because of the superb electrical properties that were constant over a very wide temperature range and its thermal properties. They proceeded with this and got to the point where they had a plant in Amsterdam that would manufacture about twenty-five tons a year for evaluation. Then in the general malaise that hit the fiber industry in Europe several years ago, all ventures were killed essentially and the project died at that point. Prior to that, of course, what had been a joint company between AKU and General Electric had been dissolved and G.E. bought out AKU. I don't remember what year it became one hundred percent G.E. owned and P3O was licensed to AKZO for commercial development.

FINE: Is that still the case?

HAY: I'm not sure. Nothing is going on with this material now, as far as I know.

FINE: Was it an interesting material as a fabric, do you remember?

HAY: Yes. But again, it was mostly interesting then as an industrial fabric because of the high temperature and solvent resistance and the electrical properties. I think, knowing what we know today as far as handling materials is concerned, that is one that we probably would have done differently.

FINE: Well, there are not too many engineering plastics that are fibers, other than nylon.

HAY: That's right. Anyhow, coming back to PPO, they rapidly got to the point where they were commercializing the material, formed the polymer products operation, and built a plant in Selkirk, a huge ten-million-pound-a-year plant.

FINE: By 1965 standards.

HAY: Right. They took quite a gamble on building that. Then of course the real problem arose with PPO in the marketplace, because manufactured products kept failing.

FINE: They built the plant in Selkirk before the development of a polystyrene blend?

HAY: It was on the way. They had a semi-works, I forget what the scale was, in Pittsfield. So a lot of the work was product development, most of it done out of the new material made in Pittsfield. I can't remember what the exact timing was when the plant came on stream.

WISE: But they had committed to it just on the expectation of PPO alone.

HAY: Yes. Absolutely.

WISE: Even knowing the weaknesses. That was pretty well known from the beginning, wasn't it?

HAY: Yes. It was overlooked or glossed over. You know, ask any chemist about the oxidative stability of this polymer and he would say, "Not too good," especially when you look at the end group. It looks like a big antioxidant, essentially.

FINE: That's what everybody was interested in in those days.

HAY: But it did have the high glass transition temperature and hydrolytic stability, and that gave you molded materials with really excellent dimensional stability. The problem was that if you took it out to a commercial molder and if he didn't use tender loving care in molding, which none of them do, you ended up with a lot of oxidative degradation of the material and obviously what went in one end of the molding machine, did not come out as the same product at the other end because it was degraded. All along there had been a substantial amount of effort going on, principally in Pittsfield, in blending PPO with a variety of other materials. We also had a project going on in the laboratory here; what was that recording tape material called?

FINE: Mylar?

HAY: No. It was a special recording tape.

WISE: Oh, thermoplastic recording.

HAY: Yes. Thermoplastic recording was being developed in the Center here. This required plastic tape, and on top of that you had to have a thin layer of a thermoplastic material with a glass transition temperature low enough so that when hit by an electron beam, there would be a deformation of the film. Those deformations could be run off, and with the appropriate optics you could get a color picture. It was a means of recording things; what was required was a thermoplastic that had a very specific softening range. Edith Boldebuck was working on this in the same laboratory, and she found that a ternary mixture of PPO, polystyrene and diphenylsilicone was completely miscible and that by taking appropriate ratios of these she could get something with exactly the softening range needed for this application. In the same set of experiments she found that PPO and polystyrene were miscible.

WISE: Was that work done here?

HAY: Yes. Not so long ago I found an old note that she had written; she had actually tried to get people interested in this blend of PPO and polystyrene, with not too much success. But at any rate, they were faced with a crisis then within the PPO area.

WISE: What do you mean "a crisis?" Was that because the market would not grow to large enough proportions?

HAY: Yes, that's correct. With the failures they were having in the marketplace, there was a question of whether they would be able to sell any reasonable amount of material. Certainly ten million pounds was coming on stream, and they wanted some expectation that they could sell that. It was a crisis and there was a lot of work going on; a lot of it was in Pittsfield, and they did demonstrate that PPO and polystyrene were miscible. Then they got the people in the Plastics Application Center (it wasn't called that at the time, but the plastics laboratory) in our major appliance business in Louisville involved. After it was shown that PPO and polystyrene were miscible, one of the problems was the material wasn't very tough because styrene is very brittle. You had to be able to toughen it to have a viable product. That was worked on in Louisville, and basically they came up with a rubber toughened PPO/polystyrene blend.

FINE: Ten percent rubber.

HAY: Which became Noryl.

FINE: So that was a Louisville development.

HAY: The rubber toughening, done under contract from plastics.

WISE: We asked Charlie Reed, and he couldn't remember if there ever was a meeting where people sat down and said, "This is the thing we're going to go with." Was this the type of thing where there had to be a decision made and somebody had to be the champion of it, or was it just a gradual transition?

HAY: I don't know. I guess Welch would be the best person to ask. As you say, these things just sort of evolve.

FINE: Is Welch going to be a good one to talk to?

HAY: I don't know.

FINE: Is he going to remember? You know, there is so much folklore that has come down.

HAY: There is a lot of folklore. For example, there's this story that Roland [W.] Schmitt keeps repeating and the genesis of which was when I was trying to get Welch interested in P3O. In fact, they did get interested in it and a large effort was mounted in Pittsfield because it looked like a very exciting material. Had the gods been kinder, and if the material had melted at 350° instead of 480°, or something like that, it would be a commercial product today and might very well have displaced PPO.

WISE: But, just from your point of view, you don't see one individual person as the champion of Noryl.

HAY: There were so many different inputs that I have a hard time pointing to anybody in particular. If you talk to any ten people on that subject, you'd probably get a different answer. The patent was issued to Eric Cizek (7), who is no longer with the company, but if you talk to people like Pop [Popkin] Shenian, he will say, "Well, we just put his name on it because he was convenient."

FINE: I've heard him say that.

HAY: Oh, you have? Another person you might talk to is Bob Anderson, who has now for many years been in the transformer laboratory in Pittsfield. He was in the very early stages of that effort too, so it would be interesting to get his input. He would be another person to get an opinion from on who made the key decision. I've always assumed that Jack Welch made that.

WISE: But there was about a six-month period of crisis in management there before it became apparent that this was the new one.

HAY: And Welch was clearly in charge at that point.

WISE: He was?

HAY: Yes.

FINE: So there is a central focus here. Because as I see it somebody has got to say, "Let's put some money into Louisville and have them do some studies for us to toughen the material." The polystyrene discovery takes place here. There's got to be input...

HAY: But I'm not even sure that they knew about that discovery here. That was in a completely different context and I'm not even sure that the people in Louisville were necessarily aware.

WISE: Another name that comes in is Bob Finholt.

HAY: Yes. He's dead now. He was managing the polymer products operation for a while. He actually has his name on a couple of patents on the blending of PPO with a variety of things.

FINE: He was in Pittsfield and Welch was in Pittsfield.

HAY: Welch reported to him, and then Finholt went down to a job in Fairfield and I think Welch took over at that point.

FINE: That's what Charlie Reed had said, that he had, in essence, put him in at that point.

HAY: He was clearly in the key decision making process for Noryl. I would assume that he must have made the decision to go ahead.

FINE: There was no moment of discovery. There were a lot of different things going on to try to solve a number of problems. It all came together in such a way that they got off the hook in the plant.

HAY: There was parallel work on the stabilization of PPO, a lot of work going on to see if the problem with the basic material could be solved. It is interesting that now we have turned full circle and are coming back and saying, "Well, really we put too much emphasis on the polystyrene blend; we should really be putting more emphasis on PPO." Now we have two major blends based on PPO. We have a PPO/nylon blend, which was developed in BOZ, and now we have a PPO/polyester blend that looks very exciting, which came out late last year.

FINE: This is GTX.

HAY: GTX is the nylon blend. The other one doesn't have a name yet.

WISE: But you never were able to stabilize the PPO. That proved to be a dead end.

HAY: We were never able to stabilize it to the extent that you could process it at very high temperatures without degradation. That's a disadvantage of having something with such a high glass transition temperature; you have to process it at extremely high temperatures. Noryl did two things. It lowered the processing temperature rather significantly so oxidation problems became less significant and it was moldable. At the same time, it lowered the cost rather significantly because the differential between PPO and polystyrene was very substantial. It is much less today than it was at that time.

FINE: I'm not sure whether this is an aside or whether we have to explore this, but in competition with Noryl there are products that are a lot cheaper, like ABS, and a lot of the success driving the businesses has been the ability to market science and technology to the customers. As a research manager, to what extent were you involved in all of that? You chose not to leave research.

HAY: I was at the bench until 1968, so I did not have any management responsibility until then. The business was well on its way before I had any responsibility for it. The person who really put the major effort into PPO at the time that they were having problems was Art [Arthur M.] Bueche. Art Bueche marshalled all of the forces on the fifth floor, essentially, to really push the product and make sure that it was going to be commercial.

FINE: So at that point Welch is in charge of the development work going on in Pittsfield and Bueche is here in Schenectady.

HAY: He was their department manager at the time. We mustn't forget Reuben Gutoff either; Jack Welch reported to Reuben Gutoff and Reuben was a super salesman.

FINE: So Gutoff was in Pittsfield for a long time, really over the whole lifetime of this project.

WISE: How did you decide in 1968, after being at the bench all this time, to go into management?

HAY: I had two people working for me and I had absolutely no interest in being in management. I guess I decided at that point that, in the directions we were going, perhaps I could do a better job than anybody else in guiding the effort. We went through a lot of very exciting developments with these various materials that we tried to commercialize, as well as PPO, and there was hardly a dull moment during all of that. I guess that I felt that I was the best qualified.

[END OF TAPE, SIDE 4]

HAY: I guess I reached the point where just having a couple of assistants working directly for me was not enough to keep you really fully occupied.

FINE: Did you really break free completely at that point?

HAY: I went directly from the bench to a laboratory manager, so I skipped a couple of layers of management there, which was a little unusual.

FINE: At this point there were no group leaders?

HAY: There were unit managers and branch managers, and I went directly from the bench to managing a laboratory. That was kind of a change. Obviously, I was still maintaining an interest in chemistry and shortly afterwards I acquired an assistant. For a number of years up until four or five years ago, Bernice Boulette worked directly for me in the laboratory, even while I was laboratory manager. We came up with a new catalyst for PPO, the one that is used now in the plant, and discovered another class of polymers called polyformals.

FINE: Which catalyst is this?

HAY: The di*tert*butylethylene diamine catalyst. The polyformals never ended up being commercialized, although they may still at some point. Potentially they are in about the same price class as polycarbonates. You make them directly from bisphenols and methylene chloride, and they are basically the same as polycarbonate except that instead of the carbonyl you have a methylene in the chain. What you end up with is a polymer with a 40° lower glass transition temperature compared to polycarbonate. The polymer from BPA [bisphenol A] has a glass transition temperature around 100°C. Again, if nature had given it another 10°, it probably would be a commercial product today because it is very stable oxidatively, obviously has hydrolytic stability, is very stable towards strong bases, and is very tough material. It has many of the properties of polycarbonate with some significant differences. Sometime when we have a synthesis for a bisphenol other than BPA that is inexpensive, the polyformal will be a prime candidate.

FINE: With a material like this, did it ever get out of the Schenectady plant environment?

HAY: It never got much beyond the multi-pound stage.

FINE: It seems, in the history of the whole thing, that silicones set the stage, that the mind was prepared for polycarbonates and to move into polymer chemistry in a big way. With PPO certainly people should be looking for all kinds of things and, of course, it's harder and harder to find things. So when you come up with something like this...

HAY: Yes. But the only thing that would have driven it would have been a substantially higher temperature material. That required having a suitable bisphenol available. It's hard to put resources into developing something that would be an offset for acrylic, for example, but at a substantially higher price. There would be a market niche for it because, for example, we have a major effort in trying to improve polycarbonates to be used as compact disc material. The reason you use polycarbonate in that application rather than acrylic has nothing to do with toughness; it is moisture sensitivity, because the acrylic happens to absorb a lot of moisture and polycarbonate doesn't. Yet the other problem you have with polycarbonate for this application is that it is very difficult to mold. So we're always compromising on the molecular weight of the material to get it to mold. This polyformal material would probably be an ideal compact disc material, but you're not going to develop a new molecule for just one application.

FINE: Let me just ask you something, getting back to the academic elements in this oxidative polymerization. The mechanistic studies and presumably a lot of other support research must have been going on all these years and probably still is. I would assume that there must have been a lot of interest, if not in publications, if those showed up largely in the patent literature, at least when you go to an ACS meeting you run into Glenn Russell or Frank Mayo or Leon St. Pierre. Has there been a great collegial interaction between G.E. because of this unique, major discovery in oxidative polymerization?

HAY: No. We published quite a bit in this area and I'm sure that our publications could number almost a hundred. We have a substantial number of publications and a lot of talks have been given. Most of the effort in academia has been on the physical property end. For instance, there has been a lot of work looking at blends and finding reasons why PPO and polystyrene are compatible. The reason that there hasn't been too much on the polymer synthetic end is that there isn't very much polymer synthetic work going on in universities. Very, very little. I am an adjunct professor at the University of Massachusetts. I'm not very active at the moment, but I did have a student over there. He got his Ph.D. with me and he did work on some substituted diphenylphenol polymers, trying to lower the melting point so that we would get a product of interest. We had some success with that. There is a lot of work going on in industry in this area. Some of the major companies are coming into the PPO area now, several in Japan and several in Europe since the patents ran out a couple of years ago. We had it pretty well tied up for a long time with the patents.

WISE: If the interest were there on industry's part, maybe the way to solve the problem of this malaise in universities for having faculties with polymer chemists, in contrast to the material science or polymer physics departments, is to provide support. Obviously, if there are funds for graduate students, then professors are going to think about working in an area where there is support.

HAY: Well, that's certainly possible. I guess there are barriers in chemistry departments to getting into it, because...

FINE: The patent question; proof of ownership.

HAY: No. I think it is because the professor has to adopt new analytical techniques and he wouldn't have the equipment in a typical chemistry department. A typical chemistry department wouldn't have facilities available, so that would mean either acquiring that equipment himself or having an active collaboration with some other part of the university. As you know, every chemical engineering department in the country has a significant effort in polymers.

FINE: Even the electrical engineering departments do.

HAY: That's true, whereas the chemistry departments don't have anything at all.

FINE: I can vouch for that from our own point of view. Columbia probably would not hire a lonely polymer chemist. They would feel that they would have to hire two or three and so have a nucleus of a group which could then justify the purchasing of expensive equipment. So it doesn't happen.

HAY: The PPO mechanism is certainly a very interesting piece of chemistry. Then you take the whole polyetherimide field, where a lot of fascinating chemistry is involved.

FINE: It's good organic chemistry.

HAY: I would venture to say that if you took the top five chemistry departments in the United States, you would have a hard time finding an organic chemist there who even knew those existed.

FINE: Joe [Joseph G.] Wirth was at Columbia last September and he talked about work that was ten years old; people sat there and said, "Gee, what wonderful chemistry."

WISE: The other half of the question: do you have academic people who consult here?

HAY: Yes, a few. Charlie [Charles G.] Overberger has consulted with us for many years.

FINE: How about Glenn Russell? You must know Russell pretty well.

HAY: Yes. I remember him. He was here, of course, when I arrived. We overlapped for four or five years, but his interests were really quite different from mine. He is really a physical organic chemist and is interested in the mechanisms of small molecules. He wasn't terribly interested in getting involved in polymers. The other organic man, Sam [Samuel] Danishefsky, consults with us.

FINE: Sam does? Natural products Sam?

HAY: He's been here for about ten years. We have a great time in discussions with Sam. Sam doesn't have much feeling for polymer chemistry, I must say, and he doesn't have much feeling for industrial chemistry other than pharmaceuticals, which is a completely different ball game as you well know. But our chemists like to talk chemistry to Sam and they have a great relationship with him.

FINE: That's interesting that you would do that.

HAY: It's a stimulus to the people here. I think that Sam is very naive about what is commercially interesting and what isn't, and that's all right. We can take care of that.

FINE: It's really an amazing relationship. It's quite a surprise.

HAY: Sam enjoys it. He has kept it up for ten years, and he must be among the top five natural products chemists in the country.

FINE: Top two. Gilbert Stork and Sam Danishefsky.

HAY: It depends on whether you consider [Elias J.] Corey part of that or not.

FINE: Well, that's interesting. Professional relationships are always intriguing and I can never quite tell what light they shed. Have you stayed close to Charlie Price?

HAY: Charlie hasn't done any research for a long time, but we did have contact with him years ago.

FINE: What about Bailey?

HAY: No. I know Bailey but I've never really had any close contact. I don't know how you get around it, but because of the current situation it is tough to find anybody from academia who knows very much that is relevant to what we're doing. We're the experts in the areas that we are involved in.

WISE: That raises the question about the next generation. The responsibility then is clearly yours to train your successors.

HAY: Yes, and we have lots of very talented younger people around at the present time.

WISE: If I could clear up one thing that you're saying. There didn't seem to be much of a disadvantage for people like yourself or Dan Fox who came here without knowing much about polymers, because analytical techniques were simple. But now you're saying it would help much more to know and to have had more training in polymers in college.

HAY: Let's put it this way. It seems to be a sad lack, when you look at the chemical business and the role that plastics play in both the chemical business and in the world at large, that polymers barely get mentioned in a chemical curriculum. That really doesn't make sense and, in fact, it means that people don't get turned on because they don't know it exists.

FINE: Of course even Joe Wirth, who came later in this whole thing, probably would still fall into the old school, just a classic organic chemist doing classical organic chemistry. We ought to talk about Ultem. It's new history, but it is really a further example of good organic chemistry turning out polymeric products which have unusual properties. I just came into this the last five or six years with my experience with G.E., and of course Ultem was the "in" thing. In fact, when I first heard about it, it was Ultrex. I met Joe about 1978 or 1979. He was in Pittsfield and it sounded like classic organic chemistry—nitro displacement reactions, anhydride chemistry. Was this another example of a young guy out of graduate school, give him some rope and let him hang himself?

HAY: Yes. Essentially we always are happy to maintain an exploratory effort on almost anything as long as novel ideas are being pursued. You know there are a lot of fashionable areas that come along like conducting polymers and so forth, but I don't get terribly excited about them if the only ideas are to repeat what somebody else is doing, to put another methyl group on the molecule or something like that. If you are doing something that is on the leading edge, great; maintain it. Here was a case where there was some literature that indicated that you could displace a nitro group. Thus if you could design a bifunctional molecule you had the potential for making a polymer. So we had this exploratory effort going on; I guess it started in 1968. It was a modest effort with Joe and an assistant, and he looked at a number of molecules and found that if you designed the right molecule with an activating group on it, you indeed could make high molecular weight polymers. A number of the molecules looked at, for example, would be a benzene ring with a cyano group and two nitro groups on it. The cyano group would be enough to activate the two nitros so you could displace them. A whole bunch of polymers were made, all very interesting but there was nothing that was worth pursuing commercially. In fact we were almost ready to close the project down. You know, a go-ahead chemist can make several new polymers every day for his whole life, if he wishes. If you're just turning a crank and making different modifications there are enough permutations and combinations to go forever. I recall there were abstracts of papers from one of the Welch Foundation symposia and there was one abstract; I don't remember what school it was from, but it was from some small community college or small school somewhere. They had done a displacement with a base, on a nitro compound, nitrophthalimide.

FINE: Nitrophthalic anhydride, I think.

HAY: At any rate, it clearly looked like there was the possibility of having something with commercial potential. And it was very soon demonstrated that you could do a displacement in high yield on the nitro imide, and then it was a matter of trying out the various permutations and combinations.

FINE: There really is a big difference at this point, even though somebody like Joe comes in and is given free rein with his first research project...

HAY: This was not his first research project. He had been on a couple of others.

FINE: But this is clearly polymer chemistry to start out with. You're not just studying nucleophilic displacement reactions.

HAY: That's right. We were looking for something to lead to a goal. That is correct.

FINE: Whereas in 1955, you were looking at new chemistry.

HAY: Then, of course, Joe left. He was only here in the very early stages of Ultem. In fact, I don't think Joe ever made Ultem. I'm quite sure he didn't.

FINE: I've seen Howie [Howard M.] Relles's name on a lot of papers. Subsequently, Williams, Donahue...

HAY: There were a raft of people. [Tohru] Takekoshi is one of the key guys in developing the present process that we used. I'm quite sure that Joe never made the molecule that we call Ultem. It was the displacement reaction that he worked on. The key was the imide system which came out of this obscure publication then zeroing in on what would be a commercial molecule. It was quite clear that Joe had a commercial material when he put some bisphenol in. Very rapidly one concludes that bisphenol A should be looked at and there is only a limited number of diamines. Once you start looking at those and come up with something, you have to ask what is one really looking for? If you are going to commercialize something, you want it to be a step beyond currently manufactured materials.

WISE: You were guiding them in the general marketing viewpoint pretty early in the process?

HAY: From the standpoint of the desired property profile basically we would aim at something close to the limit of commercial processing. That was really the guideline. Polyimides were well known, of course, but you can't process them in the mill.

FINE: So you make fibers out of them.

HAY: Or film.

FINE: You don't mold them.

HAY: Our system produces very interesting materials in which you can adjust chain flexibility to have something which you can melt process. Of course, there is the question of how high you can go. You can go as high as can conveniently be processed with the current equipment. Now we're going higher by making copolymers and so forth that can be handled at a higher temperature. Ultem is clearly a family of materials. We went through all the various stages until you narrow in on a particular molecule which would have an interesting set of properties. Then we were told, "Well, you'll never be able to make the stuff [on a commercial scale]." Of course, with the process initially used to get the evaluation quantities of the material, that was true. It was not a practical process——expensive solvents and so forth. After showing that the material looked like it might be interesting, we then had to develop a process and that was the major effort, a lot of people looking at many different syntheses and eventually coming up with a lot of very creative chemistry for the various steps, for example in that exchange reaction. Then at this time people say, "Okay. It has interesting properties." But everybody asks, "What on earth are you going to use it for?" To a large extent, the same things happened with polycarbonate and PPO, and you really need faith that the plastics business is going in the direction of higher and higher performance. You have to make the assumption that there is going to be a place for a material with a higher performance capability. Anyway, we had a material with interesting properties. We showed we could make it, but I remember the critics saying, "Well, it's so complicated you're never going to be able to manufacture it in the plant."

FINE: These are people in chemistry?

HAY: That's right. It is very unusual chemistry and we had a lot of people going around saying, "You're never going to make that stuff." Dan Fox was one of the strongest critics, for example. He went around telling people we'd never be able to make it.

FINE: Pro-PPO, pro-polycarbonate, anti-polyetherimide.

HAY: Completely anti-etherimide. He would tell people that you just can't make it. As you know, the process operates extremely well although it is a very sophisticated piece of chemistry. Anyway, you demonstrate that you can make it, you demonstrate that the process can be utilized, then you even had people saying, "Well, you've got the stuff, but you will never mold it." When I was going through the appropriation requests, Jack Welch was one who said that.

FINE: Of all people.

HAY: That's right. He actually commissioned a molding trial to convince himself that it could be molded. One of the beautiful things about Ultem is the way it molds; it's a fantastic material from the standpoint of moldability. That's one of the big pluses for the material, a high temperature material from which you can make precision moldings. Then, of course, when you got all that out of the way it comes down to, "Well, you'll never be able to sell the stuff." I think that they're well on the way to demonstrating its salability, and as a matter of fact I don't think it's going to be too long before they start building another plant. But it was a long odyssey, starting with the original exploratory experiments in 1968 and then, this year, having the first major plant on stream.

WISE: Is it true that the reason it was once called X-76 was because they thought it would be ready by 1976?

HAY: I think commercial introduction was planned for 1976.

FINE: It was actually 1979 or 1980 or something like that.

HAY: I don't remember the year. I think it was 1981, but it might have been 1983; I can't remember. I lose track of dates.

FINE: But each of these developments do take about twice as long as people expect.

HAY: Part of the problem was that it took us a long time to get Pittsfield involved because they had a contingent over there saying, "You're never going to be able to make this stuff." In fact, we got to the point where we couldn't get things moving and so we got Art Bueche to help us out. I forget what year this was. It turned out to be one snowy day, a day when they had a record snowfall in Pittsfield, and Art agreed to go over with us to talk to Don [Donald E.] Debacher, who was then division general manager. We all got into the limousine, managed to get through all the snowdrifts and get into Pittsfield, and we got there before most of the people in Pittsfield did because they didn't think anybody would come to work in such bad weather like that, whereas we had driven all the way over from Schenectady. His [A. M. Bueche] going over and having the discussion there was the thing that started the commercial development of Ultem, because they had not been anxious to do any work on it at all until we got Art to put pressure on them.

WISE: This was while Art was still running the place here?

HAY: Yes. They agreed to set up an effort on Ultem at that point.

FINE: That would have been in the mid-1970s?

HAY: It was probably 1975, I would guess.

FINE: Would Charlie Reed have been involved in that at all?

HAY: No. He was out. Don Debacher was the division general manager at the plant. I guess it was 1975. Al [Alfred R.] Gilbert, Howie Relles, Art Bueche and I went over there.

FINE: The chemists. Of course Fox is a chemist. Why would Fox have gotten so narrow in his views after all the chemical successes?

HAY: I'm not going to attempt to answer that.

WISE: A couple of things that I'm interested in. G.E., because of these engineering plastics, has established itself as a major force in an area of chemistry, but it is not well known that this kind of activity goes on. You essentially are training your own people, in your own image. In effect you are independent of the rest of the chemical community, of necessity. Do you have open relationships with your competitors? Do you talk to the Du Pont people? Are there benefits in talking to the Celanese people?

HAY: Very little technical interaction. I know the key people at central research in Du Pont, for example, but they are essentially in completely different chemistry than we are. There is no specific technical interaction except for polyesters. But there is not much chemistry in polyesters.

FINE: It's all physical properties. It's either nylon or it's polyester.

HAY: Or polyacetal, and we're not in the acetal business and not likely to be.

FINE: Dow is now going into the polycarbonate business, and presumably...

HAY: I know some of the people there but there is not really any technical exchange or any reason for it.

WISE: As opposed to the electronics type of business, it's a matter of staking out the issues on which you are the world's authority and then trying, of course, to translate that into a product with the required properties. Without patents, for example, it would be impossible to go into this business.

HAY: If you discover something in electronics, the world knows about it the next day. It is quite clear that if we didn't have the 150 patents on Ultem, for example, we wouldn't have a plant. Who's going to put the kind of money in a major investment like that, unless you have the proprietary position? We have a number of things going on in the laboratory now that I think are going to revolutionize the composites business, for example. I can't talk about that.

WISE: That's another interesting aspect. You couldn't have a guy in his garage going into this business.

HAY: That's right.

WISE: It's high investment and patent protectable. I guess what I'm getting down to is not just that you're smarter than the other R&D managers around here, but it also has to do with the nature of the business.

FINE: But patent protection is something that has changed, as it takes a decade to develop a product like Ultem. So it really depends on your establishing a market as well, to the point where your competitors look at your established market and decide that they don't want to go up against G.E. in this field. Perhaps the composite area is an area which would not need the huge investment in chemical development. In a sense there is an opportunity for the Edisonian type of approach.

HAY: To a certain extent. But we are talking about advanced composites, such as very high performance graphite fiber filled materials, the super high performance materials that would go into airframes and the aerospace business——at the moment principally military applications, plus sports equipment——where the ultra-high performance of a highly oriented graphite filament in a polymeric matrix is really needed.

FINE: The baseball bat.

HAY: The baseball bat to some extent, except that they do not need quite that high performance for that. Of course the major needs are in the military. The problems you have there are that it is not going to develop into a major business until you get some new chemistry. The reason for that is quite simple. If you look at the typical advanced composites that are manufactured at the present time, the cure cycle for these things is horrendous. First to have the ultra-high performance, the finished piece has to be completely void free. For example, let's take a polyimide composite. You take this in the precursor form, the amic acid form before being cured to the polyimide, in a solvent and then impregnate the graphite fibers and strip off most of the solvent. So you have this thin layer in which the fibers are highly oriented. You have to lay these out——depending on what properties you want, you have fibers oriented in different directions——and then you have to secure them into the final shape. But since you have to have something that is completely void free, you have to do this under a vacuum and gradually increase the temperature so that you not only remove all the last traces of solvent but also the molecules that are formed in the final condensation to give the imide. Gradually increasing the temperature and doing the cure under pressure, the whole cycle may take eight or ten hours, or even ten to twelve hours, for the final fabrication. Not only do you have materials that are rather expensive from a raw material standpoint, the processing costs are overpowering. In addition, in most cases the yields are horrible because it is an uncontrollable process. So you need some new chemistry.

WISE: So you're saying it isn't finding a whole new polymer, it's processing the polymer system.

HAY: As long as you have to depend on the existing chemistry, you are stuck with those processing times. You may make certain improvements by automating the whole thing and you'll cut the costs of the process, but you're still stuck with that lousy chemistry.

FINE: But if everybody is going to go into composites or advanced composites as an obvious place to invest, in the near future you might imagine that there will be companies other than G.E. working in the same field.

HAY: We're not in the business and there are people that are. Celanese is in the business, ICI is, and BASF. They are the three big players in the U.S. now.

FINE: So you might have to talk to your colleagues.

HAY: No. We'll have to come up with better chemistry.

WISE: This would be a matter of G.E. having a proprietary niche, as opposed to just going into a business that other people are in.

HAY: Yes. We're not going to go into a business that other people are in.

WISE: This again differentiates this from electronics, where it's very rare for someone to come up with a really different niche than anyone else.

FINE: But G.E. did go into the polyester business, which was a competitive area.

HAY: Yes, but the product line fit a niche and they had the marketing in place.

[END OF TAPE, SIDE 5]

FINE: So that was different, but it fit a need. And the polyester business has been very successful. I guess the reason for my question about whether you talk to your colleagues at Du Pont is that in an academic environment you really do benefit from sharing information, even though you may give up something. If somebody gets a good idea and scoops you on something, there is your idea published in somebody else's paper. But on the average, you get more out of sharing than not.

HAY: I understand, but since the patent position is so important in areas like this, we're not going to say anything until we have the patents all in place. We're starting to talk about Ultem now, and publish on it.

WISE: Something I always wondered about is, how does it affect the bench chemist himself or herself, who has to take a delay of three or four years or sometimes even longer?

HAY: The biggest barrier we have to publication is the inertia of people to write papers.

WISE: That's interesting.

HAY: That's the biggest barrier that we have. In some cases there may very well be a delay for a couple of years. I was able to publish fifty papers when I was active, and I was certainly working in proprietary areas.

FINE: It's just as true in academic circles.

HAY: Some people we practically have to beat over the head to write up their work, let alone go to the final pains of getting something written up in a quality necessary for external publication. I don't want to name any names, but we had some really outstanding people who could have had a significant external reputation, if we could get them to publish. We have some other people, who are great. One of our outstanding polymer chemists is Jim [James V.] Crivello. Jim also has an interest in publishing. He not only has about eighty patents to his name, but he must have eighty publications also.

FINE: I think we should wind things down here. I guess the philosophical question is whether you still consider yourself, after thirty years, a plain old organic chemist, or do you now think of yourself as a polymer chemist?

HAY: Yes, I think of myself as an organic chemist. Every week I read the Official Gazette, and I read Derwent patent abstracts for both chemistry and polymers.

FINE: Do you still read JOC [Journal of Organic Chemistry]?

HAY: I read JOC. I have about thirty journals cross my desk. I look at things in a different way. Some people look at a piece of new chemistry and say, "Why did that work?" I look at it and say, "What can I do with it?"

FINE: Yes. You're an industrial organic chemist at this point. When you look at this whole field which is now clearly defined and named, engineering plastics, do you see it as an identifiable entity in the whole field of polymer chemistry? Do you really see the future as being more and more in the direction of materials with unique properties and high performance?

HAY: Yes. And some of the materials that we now look on as being specialty materials are going to be relatively large volume materials in the future.

FINE: Polycarbonate could almost be called a commodity plastic in that sense. And polypropylene seems to be emerging into higher performance areas. ABS used to be just a junk polymer and now people are pushing the properties of things like that.

HAY: In the Ultem, polyetherimide family of chemistry, we're going to be able to come out with a whole range of materials and basically we'll tailor the molecule for specific applications.

FINE: By changing the backbone?

HAY: Yes. The strategy is to capitalize on the chemistry, utilizing, as much as possible, the same equipment but with different molecular structures to make the modifications.

FINE: We'll have to get you down to Columbia to try and excite some of our young people to careers in polymer chemistry. We'll have to get you to come down and talk to the undergraduates. I had one of your people do that, Dan Brunelle. We have an undergraduate seminar, when these kids are in the second half of their first year. They are young in the game and it is really exciting to watch them when they listen to what the real world is like.

HAY: Dan is doing some revolutionary work at the present time. In about two years he may be able to talk about it.

FINE: He talked about PCB chemistry. I asked him to do that. When Wirth came down to talk on engineering plastics, I told him it would be the usual seminar format, the faculty and all the graduate students. I told him to bring along some show and tell stuff. As I guessed, the graduate students are totally unsophisticated and the faculty too. He brought some bumper parts and a disc from a compact disc and a whole variety of other things. It was just amazing. Chemists shouldn't be amazed about these things, but here's a place like Columbia and these graduate students that come from Berkeley and Harvard and Illinois and every other darn place.

WISE: Talking about this fact that polymers never really caught on as an academic specialty, do you find any difference in your ability to recruit people?

HAY: I think that in some cases there may be some reluctance from people when they are interviewed on campus, but in most schools we have at least a couple of professors who have a pretty good idea. At this point we have a lot of graduates from the various schools. I think we're considered to be one of the better laboratories in the country for a chemist to work for.

FINE: Probably one of the best.

HAY: We don't have any problem in convincing them that there is chemistry going on once they get here.

FINE: That's for sure.

HAY: I mean on an interview trip.

WISE: You don't seem to have much trouble finding the ones who want to go on to operations and keeping that pipeline going.

HAY: And if someone is really dedicated to research and is capable, we'd love to have him make his career here. But I think starting in a central laboratory is an ideal place for a person because they can make that career choice more or less at their leisure.

WISE: I don't understand why the other laboratories, at the R&D Center, haven't picked up on that.

HAY: It's been astonishingly successful because when you look at our chemical business, you not only have Joe Wirth, in plastics, as a graduate of R&D. The manager of Noryl, Howie Relles, is a graduate of this place; the manager of Lexan, Clay [Clayton B.] Quinn, is a graduate of this place; the manager of R&D in Ultem, Dan [Daniel E.] Floryan, also; and you could move into silicones and find the same thing. The manager of central research in silicones is a graduate of this place; we have a microelectronic materials manager who is a graduate, Tom Swiger; and you can go on and on and on. Take the Plastics Application Center in Louisville, Gary Mellinger is a graduate of this place. It just makes an extraordinary difference in relations to have those people and obviously the easiest way to transfer technology is to be able to communicate it to capable people. When people transfer from here they look at it as a promotion, which it is, and it is a direction which they chose. Obviously there is more management opportunity, than there is here. It is viewed as a positive move. I think that probably part of the problem in other areas is that people view it as a banishment.

FINE: Kicked up and out.

HAY: But clearly they are successful. The more capable the people are who you are working with on the other end, the easier it is going to be to transfer technology.

FINE: Was Fox ever here?

HAY: He was here but only on a temporary assignment. Polycarbonate was discovered here while he was here. He was with a wire enamel project.

FINE: Was Welch ever here?

HAY: No.

FINE: Shenian?

HAY: No. They were in Pittsfield.

FINE: It's an exciting story, it really is.

[END OF TAPE, SIDE 6]

NOTES

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