CHEMICAL HERITAGE FOUNDATION

STEPHANIE L. KWOLEK

Transcript of an Interview
Conducted by

Bernadette Bensaude-Vincent

at

Wilmington, Delaware

on

21 March 1998

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is one in a series initiated by the Chemical Heritage Foundation on behalf of the Society of Chemical Industry (American Section). The series documents the personal perspectives of the Perkin and the Chemical Industry Award recipients and records the human dimensions of the growth of the chemical sciences and chemical process industries during the twentieth century.

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Oral History Program

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Dr. Stephanie L. Kwolek

(Date) 3/21/98

(Revised 25 March 1997)

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

STEPHANIE L. KWOLEK

1923	Born in New Kensington, Pennsylvania on 31 November

Education

1946	B.S., chemistry, Carnegie-Mellon University

Professional Experience

	E. I. du Pont de Nemours & Co., Inc.
1946-1959	Chemist
1959-1967	Research Chemist
1967-1974	Senior Research Chemist
1974-1986	Research Associate
1986-	Consultant
	National Academy of Sciences, Research Council
1986-	Consultant

Honors

1959	Publication Award, Delaware Section, American Chemical Society
1976	Howard N. Potts Medal, Franklin Institute of Philadelphia
1978	Award for Contributions to "Kevlar" (du Pont trademark for aramid fiber), American Society for Metals
1980	Chemical Pioneer Award, American Institute of Chemists
1980	Award for Creative Invention, American Chemical Society
1981	Honorary Doctor of Science degree, Worcester Polytechnic Institute
1983	Alumni Association Merit Award, Carnegie-Mellon University
1985	Engineering/Technology Award, Society of Plastics Engineers
1985	Polymer Processing Hall of Fame, University of Akron
1988	Harold DeWitt Smith Memorial Award, American Society of Testing Materials
1990	du Pont Honoree at the Bicentennial Celebration of the United States Patent and Copyright Laws
1995	Inducted member of the Inventor's Hall of Fame
1997	Perkin Medal, Society of Chemical Industry (American Section)

ABSTRACT

Stephanie Kwolek begins the interview with a discussion of her early career at DuPont. She joined DuPont in 1946, the same year she earned her B.S. in chemistry at Carnegie-Mellon University. Kwolek spent much of her time working on polymers, including aliphatic and aromatic polyamides. She discusses her level of independence in the laboratory, as well as her relationship with her supervisors. Kwolek began work with 1,4-B, and was able to get a high molecular weight polymer. It was eventually discovered that the polymer spun beautifully, and was quite strong. This polymer became Kevlar. Kwolek discusses industry competition, the testing and scale-up of Kevlar, and the problems of confidentiality. She further discusses the relationship between Kevlar and Paul Flory's theory of liquid polymer crystals. Kwolek concludes the interview with comments on her love of writing, her decision to leave DuPont, and the future of polymer research.

INTERVIEWER

Bernadette Bensaude-Vincent is a professor in the Department of Philosophy at Université Paris X. She holds a doctorate in philosophy from the Sorbonne, and is currently a fellow at the Dibner Institute of the Massachusetts Institute of Technology. Bensaude-Vincent is the author of numerous articles and books on the history of chemistry and physics, including *Eloge du mixte: matériaux nouveaux et philosophie ancienne*. In 1997, she received the Dexter Award for outstanding achievement in the history of chemistry.

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LOCATION: **Wilmington, Delaware**

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BENSAUDE-VINCENT: You must have loved chemistry, because you entered your profession with only a bachelor's degree. I'm really curious to know how you acquired the sort of background that you needed—all the experimental know-how about synthesizing and spinning—because you had to learn about spinning, as well.

KWOLEK: Well, first of all, I started working back in the 1940s. There was polymer chemistry being done—but it was with vinyl polymers rather than with condensation-type polymers, where you combine one or more intermediates rather than splitting out a small molecule. So there was very little of that work done. Even though I came in with a bachelor's degree, it was a learning process for all of us, whether you came in with a Ph.D. or with just a bachelor's degree.

We even wrote a number of books on condensation and polymerization. Since very little of this work was being done in the universities at that time, we did the type of research that universities do. We did a lot of basic fundamental research in polymers.

We had to find new ways to make these polymers. [Wallace H.] Carothers preceded us, but he used a melt polymerization method because nylon, which has a flexible molecular chain, also has a melting point somewhere around 250 degrees Centigrade. All he had to do was melt the intermediates together, heat them, and then remove—under vacuum—the alcohol or water or whatever it was.

When you cool that kind of polymer down, you get a solid or a plug, which you then break up. Then, you re-melt the stuff and force it through the tiny holes of a spinneret. Even spinning equipment can be very simple because all you need is the cylinder. You have a piston at the top, and at the bottom you screw in the spinneret, which is generally a metal plate with very tiny holes in it. With pressure applied to the piston, you force the melt out through those holes in the spinneret. Then you cool the molten fibers as they come out. They solidify and you wind up the filaments or fibers.

At that time, we were primarily interested in finding other ways to make these condensation polymers, whether they were plain nylon 6,6 or some other type of polymer. We thought there must be other ways of doing that—particularly low temperature ways of preparing these polymers. By that I mean some temperature somewhere between room temperature and 100 degrees Centigrade.

The Second World War had come to an end about that time. Some of the German patents had been released and someone—I think it was Emerson [L.] Wittbecker had seen some patents on the preparation of polyurethanes. I remember, I think he saw that—or something of the sort—because he then decided that he would start out making a particular kind of polyurethane. He succeeded, of course, and we all thought, "You must be able to make other polymers as well—polyesters." Actually, there may have been someone that had already done some work on polyesters; I don't remember.

There were many other classes of polymers—condensation-type polymers—that you certainly should have been able to prepare by these methods. There were a number of people assigned to these projects. You worked as little groups or something of that sort. Paul [Winthrop] Morgan, who was my manager at that time, decided that he would like to work with the polyamides, and these were either 6,6 or 6,10. The code 6 stands for hexamethyl diamine because it has six carbon atoms. The 10 stands for sebacic acid because there are ten carbons in that.

We started working with these relatively simple tractable polymers. We succeeded. We started out with dissolving the diamine in water and we used an acid chloride because you had to have something that reacted very rapidly. We had things like sebacyl chloride. That was dissolved in an organic solvent. You had to have an acid acceptor because you generated hydrogen chloride, so you used sodium hydroxide, or sodium carbonate, or something of that sort, and that was successful.

So we covered the field of aliphatic polyamides, and in the meantime other people were working on polyesters and all this sort of stuff. Then, of course, we thought that we would like to get into the field of aromatic polyamides. We started out with a half-and-half combination of aliphatic diamine and an aromatic acid chloride.

At that time, we also discovered that in order to get higher molecular weight polymer, you had to go to a totally organic system, rather than using water to dissolve one of the intermediates. That worked out very well and we made high molecular weight polymer. We had things like hexamethylenediamine with terephthalic acid, or with isothalic acid, or other combinations where you had a benzene ring and then an aliphatic group in between and so forth.

BENSAUDE-VINCENT: You said it worked very well, but how much time did it take until you came to this?

KWOLEK: It took a number of years, because this work started about 1950.

BENSAUDE-VINCENT: Did you have a long time of exploration?

KWOLEK: We had plenty of time, there wasn't the pressure that there is now. Actually, the emphasis now is on the short-term.

BENSAUDE-VINCENT: Yes. That's what I have been told.

KWOLEK: It wasn't that. That was not the case then. We had long-term objectives, and so we just covered the field doing an awful lot of basic chemistry with no pressure applied. At that time, the laboratory director was Dr. [William] Hale Charch, who much improved cellophane. He was a very independent character and very much interested in basic chemistry. As I said, there was very little pressure and we had the opportunity to publish a lot of papers as well as books.

But anyway, to get back. We then progressed to a wholly aromatic polyamide; but in order to make sure that the polymers were tractable, generally, we selected something that had meta-orientation the way Nomex does.

So you still had flexibility in that molecular chain and the polymers were quite soluble. During the whole time that we were doing all this fundamental-type chemistry, we were also producing new fibers and new commercial products. We came out with Lycra spandex, which is a combination of polyurethane and a number of other things. We came out with Nomex, which was MPD—metaphenylene diamine and isothalic acid. We came out with Orlon acrylic fiber. As I said, we were producing.

BENSAUDE-VINCENT: You two were independent, but you were producing.

KWOLEK: That's right, we were producing. Then by the time 1964 came around, we had made hundreds of polymers and had devised many new ways of making polymers, but there was still one block. That was the very intractable para-aromatic polyamides, the ones that had the very rigid molecular chains from which Kevlar was finally made.

It was a great time to do polymer chemistry. Actually, a lot of people refer to it as the golden age of polymer chemistry because of the lack of pressure. Even if there was pressure, it generally didn't get down to our level. The laboratory director was a very independent person and somehow fought off the pressure had been placed by people higher up in the company.

Then I was assigned to do the work on these rigid rod-type polymers. At that time, Paul Morgan was writing a book and he was trying to get it finished (1).

BENSAUDE-VINCENT: You did most of the experimental work while he was writing his book?

KWOLEK: I was working in the laboratory on various assignments that came down the line. As a matter of fact, the assignment that I had been given was something that nobody else wanted to do. He was too busy writing his book. It wasn't until many years later that I found out that he was actually on a leave of absence to finish writing that book.

I had a tremendous amount of independence. Every once in a while, the research director, who was working downtown at that time, would come by and ask me what I was doing and why. I used to wonder why the heck he was bothering me.

At that time—when I was making those discoveries—the company was not enthusiastic about using sulfuric acid as a solvent, and he had requested that I not use sulfuric acid and look for other polymers. When I first started that work, I had two of the rod-like polymers in mind. One was 1,4-B, which is a poly-1,4-benzamide, and the other one was poly-paraphenylene terephthalamide, which is the formula of the present Kevlar. Anyway, he had requested that I not work with sulfuric acid.

Of course, I would get desperate every once in a while. I would dissolve the polymer in sulfuric acid. I had a hypodermic syringe and I would put the solution into that. I had a little bath, and I would squirt the polymer into the bath, and I had a windup at that end, and I would collect some fibers, and so forth. I used to think to myself, "I wonder if he's spying on me [laughter] to see if I'm working with sulfuric acid."

BENSAUDE-VINCENT: Why did he mind the sulfuric acid—because of the health risk?

KWOLEK: First of all, sulfuric acid is very expensive to work with. It's not that the intermediate is very expensive, but you have to have special equipment to handle it—all sorts of safety equipment. It's also expensive to dispose of.

BENSAUDE-VINCENT: All right.

KWOLEK: Eventually they got around to that in the plant. They use it, and everything is caged in and done under very safe conditions. I started out, actually, looking for organic solvents, but when I got desperate, I had to convince myself that there really was something there, even though I was not getting great properties with sulfuric acid. If I heat-treated that fiber then the modulus—its stiffness number—would go up to 400 or 500 grams per denier. So it was obvious to me that there was something here, even though I was having difficulty making it go.

One of the reasons that I started working with 1,4-B was because I thought it would be more tractable—easier to dissolve. The only problem there was that PBDT—the intermediates—were readily available. At one time, Morgan had made PBDT and if you could make that work, it would be the direction in which to go. I worked with 1,4-B—the only problem there was that it was described in the literature. Someone in South America or Spain had written an article on it. When I made it, first of all, I had to make all the intermediates.

BENSAUDE-VINCENT: You made it by yourself in the lab? You couldn't buy that substance?

KWOLEK: No, you could not buy it, so I had to make the intermediates, the par-aminobenzochloride hydrochloride. That was really a mess, because you have to use hydrogen chloride gas and cyanochloride. This was summertime, and it's very humid here.

BENSAUDE-VINCENT: Did you have any technicians working with you?

KWOLEK: Oh, yes. I had a technician who helped, but because it was such an early stage, I had to work with him to observe just what was going on. I had to devise new ways of doing things as we went along. I couldn't just give him a recipe and say, "Do this."

The thing that I found out in the literature—all the elemental analysis—was that carbon, hydrogen, and all this sort of stuff was quite variable from experiment to experiment. What I found out was that the intermediate hydrolyzed very easily. If I put it into the oven—this was a vacuum oven—it polymerized on me to a molecular weight polymer.

Eventually, I was able to work out the whole procedure so that you were able to get high molecular weight polymer; but the problem, then, was that I couldn't dissolve it. I spent quite a bit of time looking, because I had to have an organic solvent. At that time, Larry [Lawrence Forwood] Beste and some of the other people in the laboratory had found that these aromatic polyamides were soluble in amide solvents if you put in a salt like lithium chloride or calcium chloride. I went through a series of solvents and finally found that it was soluble in tetramethylurea with lithium chloride.

BENSAUDE-VINCENT: How many solvents did you try? Do you remember?

KWOLEK: How many times?

BENSAUDE-VINCENT: How many times, yes.

KWOLEK: Many, many times.

BENSAUDE-VINCENT: Was it six months or something?

KWOLEK: Oh, no. I started the work on a part-time basis. I think it was in June of 1964. Actually, I was working on two projects then.

BENSAUDE-VINCENT: At the same time?

KWOLEK: At the same time, yes. Totally unrelated. [laughter] By January of the next year, I had spun the polymer. Sometime, I think maybe in December, I had noticed that once it was dissolved in tetramethylurea, this polymer made an extremely odd solution. It was unlike any of the polymer solutions that we had, which are generally rather viscous and transparent. Here I had this watery type of solution, even though it was somewhere about 10-15 percent in concentration. It was opalescent when I stirred it. It was cloudy and you thought, "Well, this is one of the no-no's" as far as spinning is concerned because you would plug up those holes that were 1/1000th of an inch.

I decided that I would filter this through a very fine filter and the solution was cloudy on both sides of that funnel. Then I put some on a spatula, and I saw that it flowed very readily, and it was a cohesive type of flow as compared to water, which usually drips after a while.

In the laboratory, we had a man in charge of the spinning equipment, which was already set up. This is the type that they use in a plant except it's simpler. I told them that I wanted to spin that solution. We would spin together because the equipment was his responsibility. He refused because he looked at that thing and he said, "It's too thin. It's cloudy. It's going to plug up the holes" and all this sort of stuff. Then I would go to the lab and I would take some of the polymer and dissolve it in sulfuric acid in a very crude fashion. [laughter]

BENSAUDE-VINCENT: Did you feel discouraged at that moment?

KWOLEK: Oh, yes. I felt discouraged. Yes. First of all, it was such a struggle all the way from the very beginning. There were so many problems that had to be solved. The fact that I eventually got very high-molecular-weight polymer was preceded by weeks of low-molecular-weight polymer.

He refused, and I went back to the lab. As I spun some from this hypodermic syringe, I saw that it was very crude. Generally when you spin, you also draw fibers, you stretch them. Of course, this was not drawn. But if I heat-treated it, I got this very high modulus. I tormented him for a while, and he finally conceded and said he would spin it. I think he probably thought, "Well, I'll give it a try and if it fails, then that's the end of that." We spun that 1,4-B solution and the fiber was very strong. It spun beautifully. Then I submitted the fiber to the physical test lab, because every fiber we make is tested for strength.

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KWOLEK: I also told the laboratory director, then. Of course, everybody got very excited and we realized that we had something that had commercial possibilities. One of the reasons for starting this work was the fact that we were looking for a reinforcing fiber for radial tires. There had been rumors that in the future, there would be gasoline shortages. If you could get a lightweight fiber that had very high strength and very high stiffness, then it would find many applications—particularly in airplanes, helicopters, and so forth—where having a lighter weight vehicle would save on energy.

BENSAUDE-VINCENT: Was there any connection with the space program, as well? Was there any demand from the space program?

KWOLEK: At that time, I don't think we were interested. I don't think there was any talk of a space program. It came later, of course, but at that time the only thing we thought about was automobiles, trucks, and airplanes.

While I was working, Dr. Joe [Joseph T.] Rivers had been searching for fibers that could be used to reinforce bullet-resistant vests. Somewhere at Experimental Station, there was a tunnel or something that had been used for testing explosives. He set up a lab there where he tested various fabrics that had been made and how well they resisted being shot at with a gun.

I remember the very first time. I had very little fiber that had been heat-treated and had the modulus around one thousand, but he took a small sample of that and tested it for resistance to the bullets. That was very exciting because he found that it was quite resistant.

In the meantime, I think the U.S. government had been doing some testing at their laboratory in Connecticut. Everybody, of course, became very excited. Generally, when you're looking for something and you've made a discovery, there are other people who are thinking the same thing.

BENSAUDE-VINCENT: Of course. [laughter] Were you aware of any competitors?

KWOLEK: At the time I started the work I was not, but later on someone apparently saw a patent that came out of Monsanto [Company], where they had made 1,4-B. They were not able to dissolve it, so they were not able to spin any fibers. I came along, and I guess I made a somewhat less-crystalline polymer, and so I was able to dissolve it.

BENSAUDE-VINCENT: Did you know about the patent when you started?

KWOLEK: Not when I started, but sometime in the early work, I heard of that patent. I think someone in the laboratory had heard of it, and that's probably what had gotten them started. We had been thinking about these very intractable aromatic polyamides for quite awhile, but I think suddenly there was tremendous stimulus when they had seen this particular patent.

BENSAUDE-VINCENT: Did you stop your other project? You told me that you had two projects.

KWOLEK: That's right. It's a long story. We did not. When I made that discovery, and there was the realization that these rigid rod polymers could be commercialized, then we—not I, but people above me—assigned groups of people to work on these two polymers. There was a group with Tom [Thomas I.] Bair. He worked directly under Morgan at that time because he had just gotten his Ph.D. He started the work on PVDT, which is the present-day product.

Apparently, there was a change of mind. I think they realized that it was going to be very difficult to find a solvent for PVDT. They decided that we could work with sulfuric acid, so he started working with sulfuric acid and PVDT.

In the meantime, there were other people who were assigned to find new solvents for these polymers. If you could possibly find an organic solvent, this was what you wanted. He started out, and then other people were assigned to work on 1,4-B, and they started scaling up spinning. We had one team working on PVDT and another team working for me. I think the reason was that the work had to get done as fast as possible and the patents had to be issued as fast as possible, because we realized that there would be other people thinking along similar lines.

1,4-B was being spun and eventually PVDT, but in the meantime there were many, many people who had gotten into the act, all of whom made contributions—some more or less. One of the reasons why we finally went with PVDT was because the intermediates were readily available. They were cheap and DuPont [E. I. du Pont de Nemours & Company] already had used terephthalic acid with Dacron polyester.

BENSAUDE-VINCENT: You had all the know-how for this?

KWOLEK: That's right. Exactly. With 1,4-B, you would have to make the intermediates—you would have to make the polymer. Then, of course, the two polymers had very good properties, but in some cases one property was better than the other. While we were working on 1,4-B, we were desperately trying to succeed with PVDT.

Things were going rather slowly and then along came Herb [Herbert] Blades, and he is mechanically gifted. He devised new spinning equipment. Instead of just doing a wet spinning, where you spin the fiber directly into water to remove the solvent, he decided to use the air gap method. Between the spinneret and the water bath, there was a small area of air, which helped because you could further stretch that fiber.

Actually, the air gap had been used initially by—I think—Monsanto. I believe there's a patent out on using that air gap method, but I think they used 6,6 nylon, which doesn't form a liquid crystalline solution. I think they were able to spin the polymer faster; but it didn't improve the properties. But here, where we had a liquid crystalline solution, this made a difference.

BENSAUDE-VINCENT: It made a difference in the properties?

KWOLEK: That's right. The other thing is that we were working with various people who were experimenting with sulfuric acid. They steadily increased the concentration of this solution. When it got to Herb Blades, he increased the concentration to 20 percent, and in the course of that he heated the solution. This helped tremendously; it improved the properties. Now you could spin the PVDT and get 20 grams per denier of tenacity immediately, without heat-treating it, but you still got the same modulus of four hundred and fifty.

It was still necessary to heat-treat the fiber if you wanted the 1,000 gram per denier modulus. Certainly for tires, you didn't have to heat treat. What you wanted was the slightly higher elongation, and a 450 grams per denier modulus was good enough, but you had the twenty tenacity right to start with. When it came to composites, where you wanted the 1,000 grams per denier modulus, you had to heat-treat the fiber.

Even at the very end, when we were doing plant testing of the fibers with the tire cord people in Akron, we were supplying both 1,4-B and PVDT. It was after that, when everything has been worked out, that we switched to PVDT.

It is a rather long story, but it was tremendously exciting because you were doing something that nobody else had done. For years, people had tried to spin these polymers—and actually they had been spun—but nobody had discovered that they formed liquid crystalline solutions. That was what made all the difference in the world. With just an isotropic, you got something like 2 grams per denier for tenacity and maybe 50 grams or so for the modulus. It was having that liquid crystalline solution where the molecules arranged themselves parallel to each other in these pockets. When you spin, all these little groups have a tendency to arrange themselves parallel to the long direction of the fiber, and so you end up, then, with all these very high properties.

This is particularly important because these fibers cannot be drawn the way that you would draw a flexible polymer chain unit like 6,6, where unless you draw the fiber, you don't really have much in the way of properties. As those molecules come through the spinneret, they are all tangled up and what you do is stretch that fiber somewhere near its softening point. What you are doing is aligning those molecules parallel to the long fiber axis. In the process of doing that, you are also crystallizing the fiber. As a result, you end up with improved tenacity and modulus and all the other favorable properties.

Well, with these rigid rod polymers, you can't draw them more than about 1 percent. You really can't do anything with them. You have to have everything lined up as it comes out of that spinneret. Even in heat-treating them, essentially what you're doing is perfecting the crystallinity and this improves the modulus and on some occasions it has also improved tenacity.

BENSAUDE-VINCENT: Were you working on more than the research phase?

KWOLEK: I was not involved in the scale-up.

BENSAUDE-VINCENT: Yes, but you followed it.

KWOLEK: Oh, yes. I followed everything, and we were constantly meeting together, and discussing problems—even in the development, making suggestions.

BENSAUDE-VINCENT: Yes. Did you go on site? Did you go yourself to look at what was going on?

KWOLEK: I was looking to see what was happening. You had your individual laboratory room, and then there was this great big central room in the laboratory where all the spinning was done. You could just walk out there and see what was being done and how the polymers were being dissolved.

BENSAUDE-VINCENT: Were you working on another subject by yourself?

KWOLEK: When I made that discovery, I dropped the other project and I continued working only on 1,4-B. At the same time, I was looking for other polymers, because we weren't sure what polymer we were going to commercialize. I started looking at co-polymers, and so we went through a lot of co-polymers. Not only me, but a lot of other people were assigned to work on these co-polymers, hoping that we could find something that had the very high strength and the high stiffness, which also had a higher elongation at break. That's what we were looking for.

BENSAUDE-VINCENT: But tell me, what was the other subject about, if you had two?

KWOLEK: Actually, it had something to do with dyes.

BENSAUDE-VINCENT: Nothing to do with polymer fibers?

KWOLEK: No. [laughter] Well, it did have to do with fibers, you know, dying, but it had nothing to do with the polymers that I was working with. That's right, yes; but I found that I liked working on two different things at one time.

BENSAUDE-VINCENT: When you are desperate on one— [laughter]

KWOLEK: Right, you can go to the other, and while you are working on the other, then suddenly something comes to your mind on how to solve the problems that you are having with the other one. [laughter] I think it's a good idea to work on two or more things at one time.

BENSAUDE-VINCENT: Would you say that it's a general rule for researchers always to have two?

KWOLEK: I think so. I think there are people who, even if they're working with one problem, are working on two different aspects of one problem, so that they can let one go for a while—the one that's frustrating them—and work on something else. It's always surprising how you're talking with someone else, and someone mentions something, and an idea suddenly comes to your mind on how you might solve your own problem.

BENSAUDE-VINCENT: It seems that you have sometimes been skeptical about your own results?

KWOLEK: I was, especially when, there at the beginning, you would get these fibers and they were so stiff that they would break when you pulled on them. The other thing is, we'd never worked with polymers that had such low elongation and that were so stiff.

We even had to devise new ways to measure their properties. When you measure tenacity, you clamp the fibers at two ends, and then you start stretching—except you have a machine that does this. What would happen was that the fiber would be so strong that it wouldn't break, but it would come out of the clamps.

BENSAUDE-VINCENT: It protested?

KWOLEK: Yes, so we had to devise clamps that would hold that fiber under any conditions. So, as I said, everybody got involved because it seemed that no matter where you turned, there were problems. [laughter]

It was a tremendous experience for everybody. It's surprising now—people are retiring, and they are recollecting some of the things in the lab, and they are talking about their experiences. They come back and they say that when that work was going on was the most exciting period of their professional life.

You could certainly understand why, because here you were, under such tremendous pressure, and there was this great secrecy. Even the reports that we wrote—the people in other labs didn't get to see them except the laboratory directors and with good reason. Apparently some of the information did get out—somebody had left the company and started talking. You can see why there was such tremendous pressure to get things done very rapidly.

BENSAUDE-VINCENT: Even during that period, you didn't go to the annual meetings of the polymer chemists to publish your results?

KWOLEK: We wrote monthly reports; but they were all—

BENSAUDE-VINCENT: They were in-house reports.

KWOLEK: In-house reports, that's right. The DuPont Company is made up of many different departments. Even at Experimental Station, you have a nylon department, a plastics department; I think there were something like ten different departments. None of our reports or communications were allowed out except if they were sealed, and then only the laboratory director was allowed to see them. Various special people were allowed to see them. You can see now why it was so exciting.

BENSAUDE-VINCENT: Does it mean that when we say "we", you mean only your lab, the room of your lab?

KWOLEK: The entire lab.

BENSAUDE-VINCENT: How did the information flow?

KWOLEK: I could talk directly to the laboratory director, and he in turn would talk to the research director, who at that time was located in the downtown office. Then, of course, there were other people that he talked with. We also had managers in the laboratory, and certainly they were informed, but I remember on the reports that were being written it said, "To be seen only by those who have a need to know." [laughter]

BENSAUDE-VINCENT: Everybody needed to know. [laughter]

KWOLEK: Right, everybody needed to know. I do remember when the first patent came out. I think it was on 1,4-B because that was the first one to come out. Shortly thereafter, a scientist from South Africa had seen that patent and I guess was traveling in the United States, and came to our lab and wanted to meet me. Of course, I did not meet him, but the news got around very rapidly once the patents came out. There's nothing that you could do about that. That's right. It worked out very well. Of course, I don't think that polymer would have been commercialized today, because it was terribly expensive to make, and you know the emphasis nowadays is on cheapness. Of course, there was the litigation, but as I said, the important thing is you have to be the first to manufacture something if you expect to make any money at all on a particular product.

The other fascinating thing was that we had these fibers. We took them to the tire people and they were not interested because they had steel wire and steel wire is cheaper. It was then being used to reinforce tires and it was cheaper. It meant that you didn't have to change machinery and you didn't have to learn techniques. Here we were with this great product.

BENSAUDE-VINCENT: Without a market.

KWOLEK: No market. [laughter] So then, of course, we got together a group of people again, chemists, and so forth. Their assignment was to look for new applications.

BENSAUDE-VINCENT: Did you work on this end-use area?

KWOLEK: I did not work on any of the end-use applications at all. My love was laboratory work and not end-use applications or any thing of that sort.

BENSAUDE-VINCENT: Up to now, you mainly emphasized the practical side of the fibers, but as far as I understand, the liquid crystalline solutions were also a tremendous theoretical discovery.

KWOLEK: It was a tremendous theoretical discovery, that's right. Up to the time that I made the discovery, we certainly were aware of liquid crystals. This was in nature; the tobacco mosaic virus is a liquid crystal. There's talk that the body functions partly because of liquid crystal formation, particularly the muscles or something of that sort. Then you had the very low molecular weight ones that were used in watches and thermometers. Some time before that, Paul [J.] Flory, the Nobel Prize winner, had written a book on polymers (2). In it, he did some theoretical calculations. He started out with polymers of only—this is theoretical—one molecular weight.

BENSAUDE-VINCENT: Weight?

KWOLEK: That's right. In his calculations, he found that as you increase the concentration of the polymer in a particular solvent with a certain value, you could get a separation into isotropic and non-isotropic liquid crystals. He apparently had. I've seen that since, but I wasn't even aware of it. None of us, actually, were aware of it at that time.

BENSAUDE-VINCENT: So you did not read the book before you started?

KWOLEK: No.

BENSAUDE-VINCENT: That's so incredible.

KWOLEK: No, we didn't; you see how much luck it was here. [laughter]

BENSAUDE-VINCENT: It was incredible.

KWOLEK: That's right, it was incredible. But at that time, none of us were even thinking of a liquid crystal.

BENSAUDE-VINCENT: So you started that work only relying on the catch of the lab?

[END OF TAPE, SIDE 2]

KWOLEK: I think at that time, Nomex was getting 100 grams per denier modulus and so we thought, "Well, it's going to be a stiffer molecule. We will get something stiffer." Then I made this solution—we'd never seen anything like it. Ordinarily, someone would have seen that and probably dumped it down the sink, but for some reason I looked at that thing and I saw it flowing from a spatula. No matter how unusual it looked, I thought it had possibilities even though I didn't know what I had at that time.

BENSAUDE-VINCENT: You said "for some reason." Could you try to formulate a reason?

KWOLEK: At that time, no. None of us did, as a matter of fact. It wasn't until I had been working for a few weeks, and Paul Antal came in. He was newly hired; he had been teaching at one of the universities down in Florida. He looked at that solution and he said, "That's a liquid crystalline solution."

Then of course, John [R.] Shaefgen, who had worked with Flory at one time—I think at the Goodyear [Tire and Rubber Company] laboratory—remembered that Flory had written a book on polymers, and he thought he remembered something about liquid crystals. We decided that we would get in touch with Paul Flory. We signed a contract for secrecy with him and he came. He was flabbergasted when he saw that, because even when he had done his theoretical work, he didn't have a real polymer in mind. [laughter]

BENSAUDE-VINCENT: Of course. Right. Does that mean that you had no time to go through the literature until then?

KWOLEK: No. I was so busy with all the other things, trying to get the fiber and the solution spun. Without spinning it and testing the fiber's properties, I had nothing. Everything was dependent on the fiber properties.

BENSAUDE-VINCENT: Yes.

KWOLEK: That's it. If I didn't get the high modulus, it would have just been like any other fiber, and we'd made hundreds of them. Then, of course, Paul Flory came with his book and educated us all.

BENSAUDE-VINCENT: You mean that the theory came afterwards? [laughter]

KWOLEK: Yes, the theory came afterwards. Even he had a hard time believing that they finally found his theory in a real practical experiment.

BENSAUDE-VINCENT: It must have been different.

KWOLEK: Yes.

BENSAUDE-VINCENT: What about your relations with the academic community of polymer chemists? You attended the meetings.

KWOLEK: Oh, I went.

BENSAUDE-VINCENT: You published some very well-known papers.

KWOLEK: Well, I had a lot of interaction with university people.

BENSAUDE-VINCENT: Did you consult with them?

KWOLEK: No, not as consultants. It wasn't until later, but we ended up educating most of them. Then we had polymer chemists that we used as consultants. There was Dr. [Carl S.]

Marvel, and there were a number of other consultants that we used, but none of them were into this type of work. A lot of them were working with either heterocyclics or vinyl polymers, things like that. Actually, the book that Paul Morgan had written on low temperature polymerization was used.

BENSAUDE-VINCENT: In universities?

KWOLEK: That's right, on how to make these polymers.

BENSAUDE-VINCENT: You can find it all over the world now.

KWOLEK: That's right. I'm sure of that. Then, once the patents came out, we were able to talk. I did a lot of lecturing, and of course the university people came to us. There has been a tremendous amount of interaction.

BENSAUDE-VINCENT: Who decided when you could publish and in which journal you could publish?

KWOLEK: We held *Macromolecules*, the journal, in very high esteem. I think that's probably one of the reasons why we went there first. Then, of course, there was the *Journal of Polymer Science*; but we did like the *Macromolecules* journal above all.

BENSAUDE-VINCENT: For what reasons?

KWOLEK: The papers were, we thought, more scientific and the work was more reliable. That was the primary reason why we went there.

BENSAUDE-VINCENT: How long did you have to wait before publishing? Did you have to ask your manager and the lab director?

KWOLEK: You wrote a paper and then it got circulated in your building, then in the various departments of the DuPont Company, and everybody criticized it. Primarily, it was criticized to see if there is any information in that might damage other departments or even your own department.

BENSAUDE-VINCENT: So it could take time?

KWOLEK: It took time before a paper got out, and of course, at times there were things that you had to remove. But it was quite a while before we started publishing. Actually, we could talk about things long before we could publish them.

BENSAUDE-VINCENT: You mean talk outside?

KWOLEK: Yes. Give talks or lectures and things like that outside and then eventually put them in writing.

BENSAUDE-VINCENT: In some previous interviews, you mentioned that you were fond of writing.

KWOLEK: Oh, I am.

BENSAUDE-VINCENT: Do you remember any moment when you first thought writing was part of your process of discovery? I mean, that formulating the results helped you to go forwards? Why did you like writing?

KWOLEK: I would say that I liked writing from childhood. I did a lot of things. I didn't start out to be a chemist. I was going to be a fashion designer, and that's what I did as a child. I spent hours drawing, and so forth.

BENSAUDE-VINCENT: That's not writing! [laughter]

KWOLEK: I know it wasn't writing, but I do remember writing poetry while I was a child. I do remember that, although I have nothing to show. I don't know what happened to it. Of course, I always loved science, regardless.

I think I probably inherited some of that from my father, who was very much interested in biology and plant life. I remember going walking in the woods with him as a child. We lived on the outskirts of town, so there was lots of farmland and wooded area. We spent a lot of time in the wooded area; sometimes my brother was with us. He discussed and identified plants, trees, animals, and bugs.

Then, of course, we had friends who had small farms, so I remember spending an awful lot of time at the farms as children, particularly where there were creeks, digging up crabs, all this sort of stuff that children generally don't do. This is what we did. My father also did a lot of grafting of trees, so I think some of my scientific inclination comes from him. That's right.

BENSAUDE-VINCENT: Writing a chemical paper is something extremely gratifying and it's different from writing poetry.

KWOLEK: You're right. It's very different. That's right.

BENSAUDE-VINCENT: But did you like writing your papers, or did you suffer when you had to write?

KWOLEK: Oh, no. I liked writing papers. I have always liked writing. I think this is probably why I admire authors and poets. It isn't just that they have creative minds, but the fact they are able to express themselves so well.

BENSAUDE-VINCENT: You mean that writing a paper was completely different from your laboratory notebook that you kept daily.

KWOLEK: No, it's not very different from my writing.

BENSAUDE-VINCENT: Because I really admire the writing of your papers. They're extremely—

KWOLEK: Everything's organized.

BENSAUDE-VINCENT: Yes, it's very organized, very clear, right to the point.

KWOLEK: That's right. When we were having the litigation with Esso, the attorneys that were on their side were extremely insulting to us, and they were to me as well, but one thing that they had to admit was that my writing was beautiful. That everything was very clear and concise, very readable and so forth. I used to laugh, because I used to be crushed at some of the things that they said, but it always brought me great joy that they had to admit that I was a good writer.

BENSAUDE-VINCENT: So writing is part of the scientific research for you?

KWOLEK: That's right. Writing is very important when you do research because everything you do—particularly if you make a discovery—is scrutinized a million times, so writing becomes very important.

BENSAUDE-VINCENT: Would you say that your writing was part of your success in the discovery of Kevlar?

KWOLEK: I think so. My writing indicates some type of orderliness in thinking, which is a help, in addition to creativity; but you have to have a certain type of orderliness when it comes to thinking.

BENSAUDE-VINCENT: Are there any other physiological buttons, or even moral qualities, that you would consider part of your scientific creativity?

KWOLEK: I think honesty is very important in your work, and in the way you think, also. I consider that to be a very important factor; and particularly when you're working in a commercial laboratory, you have to be discriminating as to what you work on. One of the objectives, of course, is that the company is there to make money and they expect you to make money, no matter how you do it. You have to keep in mind that you don't work indefinitely on something that has a very good chance of failing. At some point, you have to decide when you have to drop the subject. It may be painful at times, but I think this is important.

BENSAUDE-VINCENT: Did you decide by yourself, or did people decide for you?

KWOLEK: Generally, the chemist decides himself before other people decide. Now, some people never decide, but I think after a while, you've got to admit to yourself that, no matter what you've done, this isn't working out and you just can't go indefinitely doing something.

BENSAUDE-VINCENT: All right. Towards the end of your career at DuPont, you probably experienced a dramatic change of research policy in the company, and from what you said, I guess that you were not completely happy by this time?

KWOLEK: Actually, the dramatic changes did not occur until after I left; but it was already in the wind.

BENSAUDE-VINCENT: By the early 1970s?

KWOLEK: It was about the middle 1980s or something like that when we began to sense that there were going to be drastic changes ahead. I left, and I think that was a factor in my leaving. I loved research so much that I thought I was going to find it painful, and so I decided then that I would change careers and do other things. But the new era was already obvious to people and it certainly accelerated very, very much after I left. I really had found it very depressing, and I knew how depressing it was to the chemists who were working because I keep in touch with them.

I act as a mentor to young women who are scientists and there was a time when you would be approached and asked, "What do you think I ought to work on?" Now it's, "What do you think I should be doing concerning my future? Do you think I should be studying this or that? Do you think I should be applying for another job?" and all that sort of stuff. So there has been a drastic change in thinking among scientists, and sometimes I wonder, "How can you possibly think or be creative when you have this tremendous stress and you're constantly worried about whether you're going to have a job?" These are problems that I never encountered.

BENSAUDE-VINCENT: Even during the 1970s? Because the change of policies started in the early days of the 1970s.

KWOLEK: There were times. We go through cycles.

BENSAUDE-VINCENT: What about you? You didn't get any pressure?

KWOLEK: I never felt any pressure, no.

BENSAUDE-VINCENT: You were free of that?

KWOLEK: That's right.

BENSAUDE-VINCENT: Because of your reputation?

KWOLEK: Possibly because of the type of work I did and because of the kind of people I was surrounded by: Paul Morgan was very much interested in the more basic type of research. I think back on the men that I worked with. They were a fabulous group of very creative people, even those who left and went to work for other companies. I remember that at other times, if anybody left that lab, they were immediately grabbed up by some other company because the laboratory had such a tremendous reputation. But then I also know of men who left, Wayne [X.] Hill who went to work in Phillips [Petroleum Company]—I think it was Phillips—and became laboratory research director, something high up. There were a number of other men, all of whom occupied very high positions in other companies.

Then, of course, there were a number of books that came out of the laboratory, and so you realize that there are just so many hours per day. You've got to sleep during that time, so it meant some of that writing had to be done during working hours, and yet you had that freedom. There was all that mental stimulation and a genuine interest in research among the men that I worked with; so there was a lot of exchange of information. Then, of course, there was always excitement because one person or another had made some sort of discovery.

BENSAUDE-VINCENT: Yes.

KWOLEK: Whereas now the tendency is more towards working on and improving old products. Now, how long can this go on? I don't know. I don't think it can go on forever.

BENSAUDE-VINCENT: Producing something new can't come out from this line of research? Improving processes, improving existing formulas?

KWOLEK: Well, as I said, you can—

BENSAUDE-VINCENT: Let's have some examples.

KWOLEK: It works, for a while. Certainly you can improve products and it can be very stimulating, but after a while, it just seems to me that there has to be something that is altogether new.

BENSAUDE-VINCENT: For something very new you need a long-term project and autonomy?

KWOLEK: It's either going to have to be a new product or a new way of doing something. It doesn't necessarily have to be a new polymer; but there have to be discoveries—what I'd call <u>scientific</u> discoveries. Certainly, it seems that there isn't as much emphasis on chemistry nowadays as there is on electronics and information, but I still consider chemistry to be fundamental to just about all discoveries. I don't think you can drop chemistry at any point. There have to be chemists—they may not be polymer chemists, but there are going to be other types of chemists—who are going to have to make very basic discoveries. They are going to drive other discoveries.

BENSAUDE-VINCENT: What kind of basic discoveries? New processes or new molecules?

KWOLEK: It could be, that's right, processing new molecules or it could be new discoveries in the medical field.

BENSAUDE-VINCENT: Okay.

KWOLEK: That's right. Even in electronics, you've got chemistry entering into that with particular products.

BENSAUDE-VINCENT: But in that case, there would be new materials adapted to a specific function.

KWOLEK: Yes, but—

BENSAUDE-VINCENT: Your own discovery—this strikes me very much—was a product without any prior function or any prior destination.

KWOLEK: You know, a lot of discoveries have been made because people were not specifically looking for something, and I found that over and over again. A lot of discoveries are purely accidental. Then someone else sees that and thinks, "Well, this might be practical for something else," or he connects that with something that he is doing. I don't think it has to be a particular product; but it can be something in mathematics, actually, or that starts people thinking in a new way. Or even when you think about all the space products and things that have from space discoveries. But then, there new ways of doing things.

BENSAUDE-VINCENT: So you would not recommend that research be too targeted?

KWOLEK: I think there should be both. It should be a combination of research. I mean, some things that do not have a particular objective in mind and then others that do.

BENSAUDE-VINCENT: You had a specific objective. But it was your own objective and not the corporate objective.

KWOLEK: That's right. There were always objectives in industry, but then somewhere along the line something happens, and you end up making something unrelated. What you want, then, is to have an open attitude on the part of people who decide where the money goes and what you can or cannot do. You've got to have people with open minds in research, even though you have specific assignments and objectives.

BENSAUDE-VINCENT: So the 1960s were something like a golden age.

KWOLEK: That's right, the golden age. That doesn't exist at the present time.

BENSAUDE-VINCENT: In retrospect, did you—

KWOLEK: Well, you know, especially in polymer chemistry or any chemical fields, industries go through cycles. You have a great period, then you have to cut back, and then again another great period and so forth. I'm hoping that this is one of those cutbacks now and that we're going to come back again.

BENSAUDE-VINCENT: Do you think that is something that you can come at from new targets? I mean, anti-pollution, the problem of toxicity, environmental problems. You don't think that this can help to find new materials?

KWOLEK: Oh, definitely. I think that all sorts of discoveries can always come up with some new material or some new technique of doing things and so forth. I think that is definitely possible. Yes.

BENSAUDE-VINCENT: For the future.

KWOLEK: For the future, that's right. Certainly, I have confidence that there are going to be many discoveries in the future. I just think we're going through a revolution right now. As painful as it is, I think that eventually things will get straightened out. I hope that someone wakes up and decides that the stock market isn't the only thing in this world to determine what kind of work will be done.

BENSAUDE-VINCENT: Did you ever have the feeling that you were working out of stock market pressure?

KWOLEK: Never. [laughter] I never even thought about the stock market. No. But I think about the stock market a lot now, and I'm sure the chemists nowadays probably religiously follow the stock market.

BENSAUDE-VINCENT: Do you think that nowadays, it would be possible for a chemist to start without a Ph.D. like you did? I mean, was it because polymer chemistry was at a starting point at that moment? Is it possible for a chemist to learn the job at the bench just like you did?

KWOLEK: I think it would be very difficult.

BENSAUDE-VINCENT: Too difficult?

KWOLEK: I think so. Because when I started, there really wasn't very much knowledge about polymers, so I came in with a bachelor's, but with an open mind. With all those facilities available to me and books, and everything else, I just sort of absorbed things like a sponge [laughter] and went on. I was just surrounded by these very bright people and so I learned from them, and of course later they learned from me. As I said, initially I learned from them; but nowadays you could never start with a bachelor's degree in research. You would be a technician. But even with a Ph.D., I notice that a lot of the people that are coming in have done post-grad work before they come in. The companies are being very highly selective.

When I started in Buffalo, New York, in 1946, Dr. Hale Charch—who, as I said, was the inventor of moisture-proof cellophane—was a very unusual person. He was a character, actually. I don't think those things mattered very much to him. I remember he showed me around, and then we came to his office. We were talking. He said to me, "I'll let you know in two weeks whether we will hire you."

With great boldness—I would never do it now—I said to him, "I wonder if you could possibly tell me sooner because there is another company that wants me to decide whether I should come and work for them." So he called in his secretary, and he dictated the letter to me while I was sitting there, and offered me the job. Well, you know, that doesn't happen today. [laughter] But it did happen then. He was just a very unusual person and very protective of his chemists and his professional people.

BENSAUDE-VINCENT: Because he had the program, right?

KWOLEK: That's right.

[END OF TAPE, SIDE 3]

KWOLEK: Unlike most managers, he spent his time going from laboratory to laboratory, talking to the chemists, asking them what they were doing and why they were doing it. He was a different breed of scientist. We all loved him, naturally. The people he worked with speak very highly of him.

BENSAUDE-VINCENT: He died in 1958?

KWOLEK: Yes, he died about then. That's right.

BENSAUDE-VINCENT: Did you have the same kind of good relationship with his successors?

KWOLEK: Yes, but there wasn't the tremendous personal interaction.

BENSAUDE-VINCENT: That you had with him?

KWOLEK: From then on, that's right, although you still had reviews. You talked to them, and frequently you felt free to talk to anyone you wanted to, but there wasn't that personal type of interaction.

BENSAUDE-VINCENT: It was important, you think?

KWOLEK: I think it was very important.

BENSAUDE-VINCENT: There were fewer of you at that time, and afterwards, there were many more people in the lab.

KWOLEK: That's right, there were fewer people. Then we moved down to Wilmington, Delaware, to the new laboratory that had been built for us, and the number of people was greatly expanded, but even here he maintained that personal contact for as long as he lived. That's right.

BENSAUDE-VINCENT: Do you think there is an ideal size of laboratory, to have this kind of good relationship with the director?

KWOLEK: I think you can have a good relationship whatever number there is, because usually you have the laboratory divided into groups. You have an analytical group, you have a physical group, you have an organic group, and so forth. You can relate to each group and go from one group to another. So I don't think it's very difficult to maintain contact with your employees.

BENSAUDE-VINCENT: But you don't have the coverage of each part of the laboratory.

KWOLEK: Yes, but you don't have to cover every group every day. So you can assign different days to talk to people of each group.

BENSAUDE-VINCENT: Do you think that you also learned something from your collaborators, even from the technicians you have been working with?

KWOLEK: I think technicians are extremely valuable. I know how frustrating it can be if you don't have a good technician, but if you have a good, conscientious technician—I just worked right with them.

BENSAUDE-VINCENT: Did you train them all?

KWOLEK: I trained them, that's right. Sometimes they had already been working for someone else, but it may have been on an altogether different project. So what you've got to do is, you've got to train them in the kind of work that you're doing. A lot of these people who are technicians are very intelligent people. They may not have a college degree, but they are intelligent people. It's very easy to work with them.

BENSAUDE-VINCENT: Were they men or women?

KWOLEK: They were men. I always had a man, yes. It's only been recently, I guess maybe within the last ten or fifteen years. At the time when I left, they had women working as technicians in addition to men. But you generally had female technicians in the analytical lab or in the physical test lab. You had male supervisors, at least in all the ones that I knew. Actually, there were very few women chemists; most of them didn't stay. Certainly during the time that I worked, women would get married and leave. It isn't like now, where women stay on. If you have children, you left and took care of your children. It's a different world now. I frequently found myself one of maybe two or three.

BENSAUDE-VINCENT: You mainly had a completely male environment then?

KWOLEK: It was primarily a male environment, except in the analytical lab or the physical test lab. Generally, those who worked doing organic chemistry were men. It was a very pleasant experience. I enjoyed it very much.

BENSAUDE-VINCENT: I understand that. [laughter] I'm afraid that we have exhausted you.

KWOLEK: What time is it? I've done a lot of talking. I'm amazed that I have been talking and talking and talking here.

BENSAUDE-VINCENT: About the life of the laboratory in the industrial lab. You were given clear assignments, you never worked on your own.

KWOLEK: No, I did not. I think you got to work on your own when you were on a laboratory director level or something of that sort.

BENSAUDE-VINCENT: Even when you became a senior researcher?

KWOLEK: No, generally there were assignments. There was a broad objective, that's it. Within that, then, you decided what you would be doing, how and so forth. Yes.

BENSAUDE-VINCENT: So you were able to choose the solvent that you wanted to use?

KWOLEK: Oh, yes, I did all that on my own, that's right, which polymers I worked on. I could make my own decisions.

BENSAUDE-VINCENT: You just had to report at the weekly meetings?

KWOLEK: I had to report weekly and monthly. There were weekly reports and monthly reports. Generally, you were reporting positive developments, but if it was all negative, you just didn't bother to say.

BENSAUDE-VINCENT: Does that mean that you perceived the weekly meetings as something like an evaluation of your work, or more as a kind of free discussion?

KWOLEK: It was free discussion. The manager would get his group together, and then everybody would talk. You would give a very short talk about what you had been doing or what you had found. You just went around the circle. Some people might have some ideas, and say, "Why don't you try this or do that?"

Then, I think about twice a year, you would have what is called a "research review," in which you would go to the conference room and give a half-hour talk or forty-five-minute talk, in which you used charts and slides. You would then present the results of your assignment—of your project—and what you had found and try and tie it all together. Depending on what you had found, there would be decisions made higher up as to whether you should continue or not along that line of work.

BENSAUDE-VINCENT: Did it happen that you were assigned to stop in your project?

KWOLEK: I'm trying to remember now whether I was or not. Frequently, you had just about covered everything that you wanted to cover by the time your period was up. You had more or less formed your own opinion as to whether something was going to come out of that or not, but I don't recall specifically being told to drop a project. After a period of six months, or three or four months or whatever it was, you generally could tell which way the wind was blowing and whether it was worthwhile continuing along those lines or not.

A lot of it was exploratory-type research and if nothing looked very good coming out of that, then you just dropped it or were told to get into something else. There were always available projects: the manager had a whole list of things that we could work, and so you frequently had a choice.

BENSAUDE-VINCENT: How did you make your choice?

KWOLEK: Generally, I would see the list, or else I would see in the work I had been doing that there was a possibility that something may come out of an idea, or something that I had observed, and then we could go in that direction. If I saw nothing new there at all, then it was possible to do something altogether new.

BENSAUDE-VINCENT: But you had to argue about that, you had to convince your manager and your director, so how did you do it?

KWOLEK: Generally you did that on a one-to-one basis in talking to him, telling him what you had seen, what you thought the potential was.

BENSAUDE-VINCENT: I won't go further.

KWOLEK: Okay. [laughter]

BENSAUDE-VINCENT: We could end up exhausted. I could have one or two hours at home. [laughter] Thank you very much. You are very, very kind and I really appreciate meeting you.

KWOLEK: You are most welcome. I certainly enjoyed it. I hope this comes out better than my first one that I just about rewrote.

BENSAUDE-VINCENT: Oh, dear.

KWOLEK: I think this one's better, particularly since I had some time to think about these things. I don't mean just with the notice that you gave, but over the years since I retired. With all the talks that I have given, I have been able to get things better organized in my mind.

Particularly when you've been away from something, I think you look at things more objectively.

BENSAUDE-VINCENT: Yes, you have distance then. So you could write a history now, by yourself, if you like writing.

KWOLEK: Yes, I've been told to do that by people who have knew me and said, "You know, you ought to give your version of what happened and so forth."

BENSAUDE-VINCENT: All the personal factors, all the excitement and moments of discouragement are crucial for understanding. Simply how daily life goes on in the lab.

KWOLEK: That's right, yes. It's something to consider since I do like to write and I do a tremendous amount of writing. One of the reasons why I haven't is because I get all the letters from across the country from kids who are writing on women inventors, and I am it, or else they call, or they come with their parents. [laughter] Or people who want autographs and all this sort of stuff.

Then you get involved in all these committees, whether you want to or not. As a result, I neglect myself and instead spend my time doing all these things. After my brother had the heart attack last September, I sort of changed my ideas, also, and I've decided life is very transient here. Since we're all in our seventies now, I better start thinking about mortality. [laughter]

I have started collecting all my papers and so forth. There are millions of them over forty years. I've even gotten around to getting rid of clothes. For years, I kept saying I'm going to lose some weight and I'm going to get back into those old clothes that are hanging in that cupboard, all in excellent condition, but I decided if something happened to me, I could never possibly ask him to go through all these things. That's what I am doing now. I'm going through things: papers, clothes, all sorts of things.

BENSAUDE-VINCENT: Did you keep your laboratory notebooks?

KWOLEK: You don't get to keep them and you are not allowed to take any of the papers that are strictly about what you did there, or your suggestions. A lot of my papers are my university contacts. Some of them are permissions to write papers and all that sort of stuff. I could take those with me, but not the notebooks. The files are the property of the company and so you don't get to take that with you.

It's actually right. No matter how much you would like to have them. They are the ones who paid your salary and they feel that they own these things.

BENSAUDE-VINCENT: They really owned these.

KWOLEK: That's right, exactly.

BENSAUDE-VINCENT: How do you feel about becoming a kind of icon, a kind of mythical, major female inventor?

KWOLEK: You're right, an icon. Well, I'll tell you. It really hasn't made any difference for me except that it's made me more busy. [laughter] That's essentially what it has done to me. Sometimes I feel sort of embarrassed by the whole thing; but as I said, it's certainly been nice interrelating with all these people and there's certainly a lot of benefits. All these banquets—

BENSAUDE-VINCENT: Talks.

KWOLEK: That's right, and meeting other people. Particularly, like this thing that's coming up now. You don't get to meet actors yourself—not that I've ever been enamored of actors, but this is a Shakespearian actor—and famous writers. Saul Bellows and so forth. It gives you an opportunity to meet people. Even going to Washington. I didn't vote for the President [William Jefferson Clinton] and I don't admire him; but still, it was rather unique going and talking to him and to various men in Congress, the heads of the Patent Office, and of course presidents of companies and people like that. It's very interesting, talking to these people and getting their ideas. It certainly expands you horizons, mentally. It's very stimulating and mentally you don't have the chance to get old. Your body may get old, but your mind doesn't get old.

BENSAUDE-VINCENT: This is its misfortune.

KWOLEK: You're right.

BENSAUDE-VINCENT: Have you been in Europe?

KWOLEK: Yes, I have been in Europe. I've been to Cambridge [University] and Oxford [University]. I gave talks. I certainly like interrelating with scientists from other countries. I have met some fabulous scientists and had a chance to talk with them. It's been very interesting. I've never been able to figure why anybody would want to go to Israel, because it's all desert. The two weeks that I was there, I became so lonely because it was all sand, except on the farms where they were growing things.

BENSAUDE-VINCENT: Were you at the Weizmann Institute?

KWOLEK: No, this was at the university there.

BENSAUDE-VINCENT: For what?

KWOLEK: It was a macromolecular symposium. Actually, I even got to speak to some of the DuPont customers there. It's been very interesting, because I've got the chance to do things that I would never otherwise have been able to do, going to these meetings and giving talks. You meet a lot of characters, also. [laughter] But it's been fun, it really has been.

BENSAUDE-VINCENT: You would do it again?

KWOLEK: Oh, I would do it again. Yes, very definitely.

BENSAUDE-VINCENT: From the very beginning?

KWOLEK: From the very beginning, I would do it again. I might do things somewhat differently, but I would still do it. Maybe I would have been more efficient or something like that. But then, you never know, because as you continue to work, you accumulate knowledge with years and then of course you can do different things that you could not do when you first started out.

BENSAUDE-VINCENT: Certainly. So you have a big capital of knowledge now?

KWOLEK: That's right.

BENSAUDE-VINCENT: Are you working as a consultant?

KWOLEK: I am. I'm not doing as much now. I didn't work as a consultant outside the company. I did only within the company. Everybody tried to get me as a consultant and the reason they wanted to was to get as much knowledge as they could about Kevlar. I felt that was not my job. That was part of the DuPont Company knowledge and it should stay there, so I did not consult outside the company. Instead, I've lectured at universities and so forth. But all my consulting was within the company.

[END OF TAPE, SIDE 4]

[END OF INTERVIEW]

NOTES

1. Paul W. Morgan, *Condensation Polymers: By Interfacial and Solution Methods* (New York: Interscience Publishers, 1965).

2. Paul J. Flory, *Principles of Polymer Chemistry* (Ithaca: Cornell University Press, 1953).

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