CHEMICAL HERITAGE FOUNDATION

EDGAR W. SPANAGEL

Transcript of an Interview
Conducted by

John K. Smith

in

Wilmington, Delaware

on

9 May 1997

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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EDGAR W. SPANAGEL

1905 Born in LeRoy, Wisconsin, on 25 July

Education

1928 B.A., chemistry, Lawrence College
1933 Ph.D., organic chemistry, McGill University

Professional Experience

1928-1930 Chemistry Instructor, Lawrence College

E. I. du Pont de Nemours & Co., Inc.
1933-1941 Research Chemist, Experimental Station, Wilmington, Delaware
1941-1943 Technical Superintendent, Nylon Plant, Seaford, Delaware
1943-1947 Research Manager, Nylon Research Lab, Wilmington, Delaware
1947-1950 Assistant Director, Cellophane Research, Buffalo, New York
1950-1958 Assistant Director of Production, Film Department, Wilmington, Delaware
1958-1965 Director of Manufacturing, Wilmington, Delaware
1965 Retired

ABSTRACT

This interview describes Dr. Edgar W. Spanagel's life, focusing on his contributions to nylon research at the DuPont Company. Spanagel was born on a dairy farm in Wisconsin, attended small schoolhouses, and graduated as salutatorian from Waupun High School in 1924. Although his father died when he was fourteen, leaving his mother alone with four children, Spanagel was able to save enough from after-school jobs to fund his first year of study at Lawrence College. At Lawrence, he took his first chemistry class and, after a successful semester, decided upon a chemistry major. Supporting himself with various jobs at night and in the summers, Spanagel completed college in four years and was elected to Phi Beta Kappa. He accepted a job teaching chemistry labs at Lawrence for two years and then applied for scholarships to graduate school. With the help of Stephen Darling, an organic chemistry professor at Lawrence, Spanagel secured a scholarship to McGill University, where he worked under Darling's former colleague, Charles F. H. Allen. Spanagel completed his dissertation on anhydroacetone benzil in 1933. At that point, Allen contacted Wallace Carothers, whom he had known at Harvard University, and Spanagel was interviewed at DuPont. He accepted a position as Research Chemist in Carothers' research group and began work on large ring compounds, first for use in perfumes and then making polymer. In 1934, Donald Coffman made polymer from aminocaproic acid, and soon, the research group focused on polyamide preparation and 66 polyamide. Spanagel worked with 66 salt, discovered by Wesley R. Peterson, and eventually introduced autoclaves to prevent the loss of diamine and maintain high molecular weights in polyamide production. To prevent discoloration of polymer, the group used silver-lined and then stainless steel autoclaves. After production was scaled up, Spanagel was moved to the semiworks for several months, solving equipment problems before returning to the research laboratory to develop a yarn size for use with full fashion knitting machines to produce women's stockings. His development of a boric-modified size for yarn was essential to stocking production at the Seaford nylon plant, where Spanagel later moved as plant technical superintendent. The latter part of his career was spent in management positions dealing primarily with nylon, cellophane and Mylar film. In the interview's closing section, Spanagel discusses his relationships with supervisors, Carothers and George Graves, colleague Paul Flory, and his views on research vs. management careers.

INTERVIEWER

John Kenly Smith, Jr. is Associate Professor of History at Lehigh University, where he has been a faculty member since 1987. He co-authored *Science and Corporate Strategy: DuPont R & D, 1902-1980*, published in 1988. He served with the DuPont R & D History Project from 1982 to 1986 and was Newcomen Fellow in Business History at Harvard Business School from 1986 to 1987. He received the Newcomen Prize in Business History for Best Book Published in America and is on the editorial board of American Chemical Society Books.

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INTERVIEWER: John K. Smith

LOCATION: Wilmington, Delaware

DATE: 9 May 1997

SMITH: Dr. Spanagel, let's just talk a little bit about your early origins and your family. Where you were born and when you were born? What kind of family did you come from?

SPANAGEL: All right. I was born in 1905 on a small dairy farm, on the east side of the Horicon Marsh, in Wisconsin.

SMITH: What marsh was that?

SPANAGEL: Horicon. That's the headwater of the Rock River, which ultimately runs into the Mississippi. When I was about seven years old, we moved to a small town, Waupun, Wisconsin, which is on the east side of this same marsh. My family finally consisted of one older sister and two younger brothers. My father died when I was fourteen years old.

SMITH: Was the family still farming?

SPANAGEL: No. We were in Waupun at that time, and unfortunately my mother was left with four small children from fourteen to two years old, and not much money. We were not a wealthy family.

My schooling started on the farm. I went to a one-room schoolhouse, one teacher for all grades and all the children who came. Some grades had no children; some had one; some had two. That first school in Waupun was a two-room schoolhouse. It had two floors: the bottom floor and the upper floor. The bottom floor was the first and second grades, and the second floor was the third and fourth grades, and one teacher in each room. Beyond that, we usually had one teacher for one grade.

I finally graduated from Waupun High School in 1924, and then went to Lawrence College, at that time; it's now Lawrence University. I was there from 1924 to 1928, and stayed on for two years in the chemistry department, mostly handling the laboratory for the department. Then I went to McGill University.

SMITH: Let's just back up. I want to ask just a few questions about this. You were a chemist as an undergraduate?

SPANAGEL: Yes.

SMITH: At Lawrence. What did you study at Lawrence, and how did you end up going to college?

SPANAGEL: At Lawrence, my major was chemistry. As far as how did I wind up going to college, I was the first one of all my brothers and sisters and cousins. My mother had a sister and a brother, and they had children. My father had sisters and brothers, and they had children. None of them ever went to college. Now, when I went to Waupun High School, I was getting good grades. As a matter of fact, I was the salutatorian, second in the class. My teachers, of course, encouraged me to go to school, but I wondered how I could afford to.

While I was going to high school—well, even before going to high school—I took any job I could get. For example, I mowed lawns for two widows for several years, twenty-five cents for one lawn, thirty-five cents for the other. The last two years in high school, I worked before and after school in a grocery store, as well as Wednesday and Saturday nights, and Saturday all day and at night until eleven o'clock or so, when people finally stopped coming in. I got paid for that—not very much, as I remember, $6.50 a week.

Most of this money I was putting in the bank. At that time we didn't have much to spend, but by the time I got through high school, I had managed to save up several hundred dollars. So, with the encouragement I got from the teachers, and the fact that there really wasn't much I could do except work in a grocery store, I decided I'd try to go to college, and decided on Lawrence.

SMITH: Where is Lawrence College?

SPANAGEL: It's in Appleton, Wisconsin. It's now Lawrence University. It was rated a good school for Wisconsin and was fairly close to Waupun. When I got there, we registered, and the one who registered me was the chemistry professor, Dr. [Louis] Youtz.

SMITH: Hughes?

SPANAGEL: Y-O-U-T-Z.

SMITH: Oh, Youtz!

SPANAGEL: Yes. He asked me what I'd taken in high school, and I said, well, I'd taken physics. The high school didn't offer chemistry. So that was one of the subjects he advised me to take and as it turned out, I got a high mark in chemistry, so the next year I took it again, and took chemistry every year after that, and that became my major.

SMITH: You didn't have any family interest? You just discovered it in college?

SPANAGEL: That's right. Well, no one else had ever gone to college.

SMITH: I see.

SPANAGEL: My father had never gone to college; my mother hadn't gone to college. At that time people just didn't go to college. They were lucky to get through eighth grade.

SMITH: Right, right.

SPANAGEL: I had nobody except teachers to advise me, and they wanted me to go to college very much. So I decided to go—not without some rumbling from people saying I ought to stay home and try to earn money for my mother. Anyway, I went, and the first year of college I got something like a ninety-three or a ninety-four grade in chemistry. I kept on and went through the whole four years. I was selected for membership in Phi Beta Kappa at the end of the fourth year, so I had done fairly well in college. Then they offered me a job to stay on and teach for two years.

SMITH: Teaching the chemistry labs?

SPANAGEL: In the labs, yes. Which I did. But at the end of two years I recognized the fact that I couldn't stay there. At that time there was a lot of discussion about graduate schools where you could get scholarships.

SMITH: Now, this was 1929?

SPANAGEL: No, let's see.

SMITH: Where are we?

SPANAGEL: We're about 1930 now.

SMITH: So the Great Depression had already begun?

SPANAGEL: Yes, just started.

SMITH: Right.

SPANAGEL: The possibility of getting jobs was not very good, but with the money I earned in two years teaching college, I paid off all the money I'd borrowed to go to college and had saved some extra. I could consider graduate school.

We had a new professor in chemistry, Dr. Stephen Darling, who came to Lawrence. He'd graduated from Harvard, and he came to teach organic chemistry. He wrote to various colleges for me, to see if he could get a scholarship for me, and one of the places he wrote to was McGill University, because he knew Dr. C. F. H. Allen who was at Harvard at the same time. That was the first place that offered me a scholarship, so I went to McGill University and worked under Dr. C. F. H. Allen.

SMITH: Did you consider Illinois? I mean, they were the great magnet then in the Midwest with Roger Adams and [C. S.] Marvel.

SPANAGEL: Illinois and Minnesota were contacted. Illinois didn't offer me a scholarship. Later, I was offered a scholarship by Minnesota, but I'd already accepted the one at McGill, so I went to McGill. With this scholarship, I was able to get a Ph.D. in three years and still do the assistantship I had to do. When I got through at McGill with my Ph.D., the question was where to work.

SMITH: What did you do your dissertation on?

SPANAGEL: It was on anhydroacetone benzil. I have a paper I can give you, and it will show you what that was all about (1).

SMITH: Okay. What was Montreal like in that period, when you were there?

SPANAGEL: Well, McGill was considered, I think, the best school in Canada.

SMITH: I mean the city. You came from a small town.

SPANAGEL: Oh, yes.

SMITH: Montreal must have been a pretty exotic place?

SPANAGEL: I guess so. It's a big city. But I lived very close to the campus at McGill. Actually, I took an overnight train to get there from Wisconsin, and just started looking for places to stay. I ended up in a rooming house that boarded McGill students. For a while you could get meals there, too, but most of the time you couldn't. I got acquainted with the other people who lived in the boarding house. Most of them went to McGill.

SMITH: Yes.

SPANAGEL: Dr. Charles F. H. Allen was my professor, and I got along very well with him. I thought McGill was a good school, but how could I judge? I hadn't gone to many others.

SMITH: Right.

SPANAGEL: Of course it is true, at that time, Harvard, Illinois, Wisconsin, Minnesota, and a few other state universities were considered top place as far as the chemistry major was concerned—especially, for organic chemistry, Illinois and Harvard. That's where both Steve Darling and Dr. Allen got their degrees.

SMITH: At Harvard?

SPANAGEL: At Harvard. Dr. Allen knew Wallace [H.] Carothers well, because both of them were at Harvard at the same time.

SMITH: They had taught together at Harvard?

SPANAGEL: Yes. I don't know if Allen taught, but Carothers did.

SMITH: He might have been one of Carothers's students?

SPANAGEL: He might have, I don't know. But he knew him well.

SMITH: Well, Carothers was there from 1926 to 1928, I think, at Harvard.

SPANAGEL: That was just about the time Dr. Allen was getting his degree. Well, anyway, when I needed a job, Dr. Allen wrote to Carothers, and Carothers turned the letter over to [Arthur P.] Tanberg, who was a director at the Experimental Station. Tanberg asked me to come down for an interview, which I did right after I got my Ph.D. I came down for the interview, and then went back to Wisconsin. In a few days after I got home, I got a letter offering me a job for one hundred seventy-five dollars a month, and I accepted.

SMITH: When you came down here for the interview, what did you see, who did you talk to?

SPANAGEL: Dr. Tanberg, and let's see—Hamilton Bradshaw, and Wallace Carothers.

SMITH: And you talked to Carothers?

SPANAGEL: Yes. As a matter of fact, Carothers was the last one I talked to. He took me back to the railroad station.

SMITH: I want to know, what was your impression of the place? What was your feeling about the size of this outfit?

SPANAGEL: The Experimental Station, as you know, was several laboratory buildings. My impression was that it was a good place to do work, as far as I could tell. The truth of the matter is, at that time, there were Ph.D.s walking the streets, and we were ready to take any job that was being offered.

SMITH: So, you weren't being too picky.

SPANAGEL: I wasn't being too picky, right.

Now, is there anything else you'd like to know about my family? I covered it pretty fast. You could say a lot of things about it. "Was it difficult to go to college?" The answer is yes. You lived on as little as you could use. I think the first year of college, I spent five hundred dollars. I went to a small hotel and got a job waiting on tables. I got my food for that. That's all I got—a few tips, but my food, mostly, for that. Then I just had to pay for my room and tuition. So what I did was scrape and scrape to get through the first year, you see. I did that, and by that time, I went back and worked summers, and made money, and put it back, and had enough for the second year.

It was a year-to-year thing, all the time, except towards the end I did borrow some money to go the last year. I think the last year, I only spent something like eleven hundred dollars. When you consider that against what you have to start at today, you can hardly start on ten thousand dollars a year. Even Delaware, I think, costs a lot. I don't know what the figure is nowadays.

It was tough, but you got used to doing without. I did join a fraternity. It was cheaper to live there than anyplace else, because they immediately appointed me to work with the cook on getting meals—I helped serve them—so I got my meals for that. Now, this is after I stopped—the last two years—after I stopped working at the hotel. I got my meals free practically, for what I did, practically all four years I was in college. Then I had the two years afterwards in which I didn't get my meals free, but I was getting paid for that.

Well, the truth of the matter is, at that time, nobody was showing a lot of money. Everybody was a little bit broke, so a lot of the people I was going to school with were not spending a lot of money either. You had other people, so you didn't feel quite as lonely.

SMITH: This was still in the twenties, though?

SPANAGEL: This was in the twenties, yes.

SMITH: Before the Depression?

SPANAGEL: Yes. Was it 1928 or 1929 that the Big Depression started?

SMITH: The fall of 1929.

I went to McGill from 1931 to 1933, during the Depression. They paid me enough so I just about could make it without borrowing any money to go to graduate school. I was through graduate school in 1933—about twenty-eight years old, and no money, so I would have accepted any job.

SMITH: Did you explore any other jobs, in either teaching or with other companies?

SPANAGEL: I was not too anxious to teach. I put an application in with a number of other companies, and some of them weren't interested. I did get another offer after I accepted the DuPont job. Of course I refused.

The question of how much money, one hundred seventy-five dollars a month, is interesting. I went home. I had a grandfather who was ninety years old, and he more or less followed my career. The conversation was something like this. "You have a job?" "Yeah." He said, "How much you going to get?" I said, "One hundred seventy-five dollars a month." He said, "My God! There isn't that much money in the world." [laughter]

He was raised on a farm all his life. He came over from Germany, and my grandmother came over from Germany too, and they were married here. Together they bought a farm, and they built it up. Later, their oldest son was running the farm, and they were living with him. But Grandfather died shortly after I came to work here. I thought it was interesting; he said, "There isn't that much money in the world." He used to have a hired man in the summer. He would get something like thirty, thirty-five dollars a month in the summertime, but in the wintertime the hired man just got his keep. They weren't used to what you'd call big wages and big salaries.

SMITH: What did you do with all your money? Did you have any money left?

SPANAGEL: Not much.

SMITH: All of a sudden you were a rich young man.

SPANAGEL: Not rich, but what at that time was a fair salary. I saved some every month.

SMITH: You saved it.

SPANAGEL: Three years later I got married.

Now, do you want to start on what I did with DuPont?

SMITH: Yes. Sure. Do you remember your first day at work? A lot of people tend to remember that day.

SPANAGEL: Yes, I think so, kind of vaguely. Actually, the first day I came to work, I waited in Tanberg's office for a couple of hours.

SMITH: A couple of hours!

SPANAGEL: Because this was the summertime, July. He had a place down on the shore. This was a Monday morning, and he didn't get back until a couple of hours late. [laughter] He immediately took me up to Carothers' office and turned me over to Carothers. Dr. Carothers explained the nature of his work and said that the problem he wanted me to work on concerned making large ring. When some polymer is heated under vacuum, it gives off a small amount of a material that has been identified as a large ring—but not very much, and it doesn't always do it.

Of course, I read what they'd been doing, and the kind of work they were doing, and what they were working with. I got together with a glassblower and he built equipment that would let me regulate the heat and the vacuum and other factors, trying to make a large ring. The reason for trying to make big rings was that [Hermann] Staudinger had found a large-ring compound that had a musk odor. They had a 17-member ring. Theoretically, if we could manufacture a 17-member ring, we should have a material with a musk odor. They'd had some indication that this was true with the little material they had made.

I made some polymer and put it in this equipment and applied heat and vacuum on it, and nothing much happened. Then one day I tried, all of a sudden I started to get a liquid. That was the ring compound. Checking through what was doing it, I found out the vacuum had to be very, very good—below a certain amount. When I knew that, I worked until I got the vacuum down, and it produced a ring compound every time.

At the time I was working with decamethalene carbonate polymer, the resulting ring only had 13 members. We next prepared a 14-carbon glycol and made the carbonate polymer from it. When this was converted to a ring, a 17-member ring was obtained.

We next visited the plant making perfume chemicals in New Brunswick, New Jersey.

[END OF TAPE, SIDE 1]

SMITH: Okay. What did the org chem people think about it?

SPANAGEL: They were interested and after some discussion decided that they wanted to carry on the development laboratory at the plant. I went up there for about a month to work with them to show them how to make the 14-carbon glycol, and how to make it into a ring. From there on they took the development over and put it on market as one of the perfume chemicals they had for sale.

SMITH: What were your impressions? Now, you're just a new guy on the block. What did you see around you? How did Carothers's group work? What was the atmosphere like at this time?

SPANAGEL: It was really not much different from a college atmosphere in many ways. I mean you talked to everybody, and they talked to you. We had meetings with Carothers. We'd talk about papers in the journals as well as things we were working on. But Carothers was very much a hands-off supervisor. Sometimes I wouldn't see him for three or four days.

SMITH: He'd just give you a project and say, "Here, go solve this problem."

SPANAGEL: He'd give you the project. Yes, yes. Pretty much so. If you had any problems, you could go and discuss it with him. But, pretty much, he was there to help you.

Well, we handed over this particular compound to the org chem people, and taught them how to make it, and then they went ahead and commercialized it. Of course, it's one of these very expensive chemicals, but perfumes are expensive. I never really knew if it was a success or not, I mean <u>financially</u>.

SMITH: Right.

SPANAGEL: I came back to Wilmington and started working on other ring compounds. One question was, "If you get a ring with carbonates, why not with others? Maybe there's a catalyst?" We had a little laboratory storeroom that had a lot of chemicals, so I started picking them off of the shelves and going in and trying those as catalysts. It didn't take long before I found one that worked with ethylene sebacate.

I tried several chemicals as catalysts. Cobaltous chloride worked, but cobaltic chloride would not. Also, stannous chloride would work, but stannic chloride would not. I could never explain why this was so.

The new catalysts opened up new opportunities to make 17-member rings. For example, with ethylene glycol and brassilic acid, we could make a 17-member ring with material that is much easier to make than the 14-carbon glycol.

I then worked on other kinds of rings to see how many I can make. One of the dibasic acids that was available was terephthalic acid. It's benzene with two acid groups.

SMITH: Right.

SPANAGEL: I started making terephthalic acid polymers with something like a 10-carbon dibasic acid; I could make a ring for that, too. Then I said, "Well, I wonder how small they can be made." I made it with ethylene glycol, which we now know as the base for polyester fiber.

SMITH: Yes. I know what <u>that</u> is. <u>The</u> polyester.

SPANAGEL: Yes, <u>the</u> polyester.

SMITH: Dacron, DuPont calls it.

SPANAGEL: I could not make a ring out of it. I had the polymer out on the desk, and Carothers came in and looked at it. Then he got up and walked away, and he never talked about it again. I never understood that, but I wasn't smart enough to try to make a fiber myself. I was at that time looking at rings, so I wasn't very interested in it as a polymer for fiber.

SMITH: Was this before or after the big nylon breakthrough in May of 1934?

SPANAGEL: About the same time.

SMITH: This was before May of 1934, before [Donald D.] Coffman made the aminocaproic acid?

SPANAGEL: No, I don't think so. I worked in the same laboratory with Donald Coffman when he was making the amino acid and the first successful polyamide fiber. I saw Coffman when he got this little chunk of polymer, about the size of a Hershey Kiss, from the aminocaproic acid. He was trying to spin it through a hypodermic needle.

SMITH: I see.

SPANAGEL: The amount of thread he got was in yards—not in skeins—and all the testing had to be done with these small samples. This started the rush to find other polyamides that could be spun into a fiber.

SMITH: Right. But the polyester just sat there?

SPANAGEL: Oh, yes, the polyester from terephthalic acid was not carried any further at that time.

SMITH: And Carothers didn't ever think you could make a decent fiber from a polyester?

SPANAGEL: I don't know what Carothers thought, but you have to remember that the polyamide fiber made so far looked so promising that nobody was looking for another candidate.

SMITH: I see. So no one ever knew whether you were really the inventor of polyester?

SPANAGEL: Well, they knew I'd made the polymer.

SMITH: But you don't know whether you made a polymer that could have produced a fiber?

SPANAGEL: That's right.

SMITH: Okay.

SPANAGEL: A short time later, Dr. Carothers told me that I should stop the work on rings because he wanted me to work on polyamide preparation. In the work done so far, results were inconsistent: sometimes the polyamides prepared seemed to be too low in molecular weight to produce a good fiber. Various dibasic acids as well as diamines were being tried. Later Dr. Tanberg pointed out to me that we should concentrate on 66 polyamide, because that was the only one that was made from benzene—a good supply of raw material.

SMITH: Benzene.

SPANAGEL: Benzene, right.

SMITH: From coal at this point?

SPANAGEL: Coal, and oil.

Also, I should point out that about this time, Dr. Wesley R. Peterson found out that by mixing a diamine and a dibasic acid in methanol, a salt was formed that consisted of exactly equal amounts of acid and amine. This was an important discovery.

In my efforts to make polyamide, I started with the salt. The 66 salt was heated in a vessel, and water was formed and distilled off and finally a vacuum was applied to remove the last traces of water. The result was a viscous material which was the polymer. The results varied. Sometimes a fiber could be made from it and other times not, as we had started with a balance in amine and acid but didn't get a high molecular weight polymer. Something must be upsetting the balance between acid and amine. I checked the water that was produced in the reaction and sure enough, it was strongly basic. Some of the amine was being distilled off with the water.

So I talked to—I think it was still Carothers. We're now getting into the era when Carothers was going over to Europe, and George [D.] Graves was taking over, but I think this was still Carothers. Anyway, I talked to Carothers.

I pointed out that in trying to make polymer we were losing diamine, and perhaps if we made the polymer in a pressure vessel we could carry the reaction further along and prevent the loss of diamine. He agreed, and we worked with two engineers to build it. The first problem

was to find out what metal could be used. Our first tests indicated that only silver did not color the polymer.

SMITH: I'll bet the managers loved that.

SPANAGEL: The first autoclave we got was silver-lined. [telephone rings] It was silver-lined, but we got the autoclave, and I made polymer. The first polymer was fine, the second polymer fine—the whole problem was solved as far as making polymer was concerned. If you do it this way, you don't lose the amine, and you make high molecular weight. If you make a polymer with too high a molecular weight, you also get in trouble, because the melted polymer is so stiff it will not flow. This was solved by adding a trace of acetic acid.

Well, of course, after we found out how to make polymer, I became the source of polymer, not only to find out how to spin it into yarn, but to get yarns to see how good they really were.

SMITH: What size batches were you making?

SPANAGEL: Two pounds.

SMITH: Still in your silver autoclave?

SPANAGEL: Yes, but we soon learned that a certain stainless steel—I've forgotten the composition—would not color the polymer, at least after making a couple of batches. Stainless steel was used in the second two-pound autoclave and all autoclaves thereafter.

The next step was to build a larger autoclave.

SMITH: Right.

SPANAGEL: The next autoclave made about fifty pounds, but it took several months to build, and in the meantime I was making polymer for the development of spinning equipment. My next assignment was to the semiworks when it started on a three-shift basis.

SMITH: Running the semiworks?

SPANAGEL: Yes. The first piece of equipment in the semiworks was designed to make an adiponitrile from adipic acid.

SMITH: Okay.

SPANAGEL: The semiworks equipment got finished piecemeal. The first equipment to be ready was the equipment that made adiponitrile from adipic acid and ammonia. The molten adipic acid was pumped, along with ammonia, to nozzle into hot cylinders that contained a catalyst, but the catalyst was plugging up. My helper and I cleaned out the plugged catalyst and put new catalyst back to start again, but we started slowly, first with the ammonia, and finally pumping molten adipic but at a very slow rate. We were getting some adiponitrile, but as soon as we increased the adipic acid, the catalyst plugged again. The answer finally was not to add the adipic acid as a spray but as a vapor.

SMITH: Yes.

SPANAGEL: Equipment to make salt was next, but at that time we didn't have enough diamine to make a big batch in the semiworks. The larger fifty-pound autoclave to make polymer came later.

I worked on shift in the semiworks for several months, but it seemed like years.

My next assignment was back in the research laboratory. Experience in knitting stockings showed that we not only had to make a yarn, but had to make a yarn that could be knitted on a full fashion knitting machine; so my assignment was to find a size that could be put on the yarn that would work on a full fashion knitting machine. On this machine the stocking is knit flat and removed from the machine and sewed up the back. The material left on silk yarn by the silkworm leaves the fabric stiff enough so that there was no problem unrolling the fabric to sew the back seam. When nylon was taken off the knitting machine, it rolled up so tight that it was very difficult to unroll in order to sew the back seam, so my assignment was to find a material to put on nylon that would make it act like silk. None of the conventional sizes for yarn would work, so I started to look at new synthetic polymers that were water-soluble.

I found that polyacrylic acid polymer was water-soluble and seemed to help. At that time, we were testing the yarns on a single-head machine that was purchased to make knitting tests. Larger amounts of yarn with this size were made and tested in a knitting factory. Everything seemed to be going along very well, but after a few weeks we found that this size was corroding parts of the knitting machine. I had to start over again.

I found that polyvinyl alcohol seemed to work, but it stuck to the needles and knitting equipment. I was told that borax hardened the polymer, but it also was too sticky. I found that boric acid would harden the film I made when I added it to the solution, and when wetted was not as sticky as the borax-modified film. After many tests I finally found a boric acid modified size that worked, and this is the size that was added to the size for the yarn in the Seaford plant.

[END OF TAPE, SIDE 2]

SMITH: Well, you were talking about the sizing. I would like to stop a little bit here and talk about Carothers and your interaction with him, your impression of him.

SPANAGEL: Yes.

SMITH: I'd also like to talk about DuPont, [Elmer K.] Bolton, and the whole group that you were working for—and [Crawford H.] Greenewalt. This must have been a very exciting time.

SPANAGEL: Yes, it was.

SMITH: It must have been very saddened by the loss of Carothers.

SPANAGEL: Oh, yes. Of course, I was not working with Carothers that long. I put down some dates here. I started with the company in 1933, and all this work on the large rings, I was through with that in 1935. Then, in 1935 about—late 1934, 1935—I was trying to make the polymer. I started that when I was working with Carothers. He's the one who put me on the job. I was making polymers when Carothers went over to England. Then they took all the nylon people and put them over with the rayon department, and I then reported to George Graves. So, you see, there was a period where I didn't know whom I was reporting to. [laughter]

The work on sizing was done after I was transferred to the rayon department. Then I reported to George Graves. I didn't really get to know Carothers very well. All my dealings with Carothers were very good. He came, and we'd talk about the work. Sometimes I wouldn't see him for a week, and when I was on a trip longer times than that. I remember one time he came in and said he was on his way to report to the chemical department steering committee and he didn't have much to talk about. I showed a bottle of a large-ring compound I had made, and he seemed very pleased.

That's the kind of fellow he was. He was nice to work for, but I didn't really get to know him very well. The only personal contact I had with him, and I had it with him very early, that's the time when we went up to New Brunswick. We came back on the train. It was late, and I was living in a bachelor house. He asked about dinner. He told me he had a girl working for him and asked would I have dinner with him, so I went over. We had a drink, had dinner and talked. I thought he was very pleasant and we spent a pleasant evening together.

SMITH: Where was he living then? Was that in Arden?

SPANAGEL: No. He was living in an apartment.

SMITH: Wawaset.

SPANAGEL: Wawaset, yes.

SMITH: So who fixed the dinner?

SPANAGEL: He had a girl come in every day to fix dinner for him.

SMITH: He did?

SPANAGEL: Yes.

SMITH: Wow!

SPANAGEL: Well, you know, girls worked cheap in those days.

SMITH: I know, and men didn't cook, yes. [laughter] That's interesting.

SPANAGEL: Then, we got into this nylon group, and of course they brought quite a few people into the nylon group. It wasn't long then, there was one time there, they had a Modern Pioneers celebration. Did you hear about that?

SMITH: Oh, yes, in Seaford.

SPANAGEL: No.

SMITH: Or was that up here?

SPANAGEL: No, this was nationwide. They had regional meetings. We went to Philadelphia with nylon. That was the product DuPont had picked out. There were about ten people. We all ended up in New York. Nylon was the only product that had more than one person. We had about a dozen people—oh, maybe about eight or ten or something like that, and I was one of those. We went up to New York. Carothers had killed himself, and therefore George Graves was sitting at the big table up above. We were at a table down below. We were all introduced. This was for our work in nylon, and there were quite a few people there. Windy [Winfield W.] Heckert was there.

SMITH: What year was this?

SPANAGEL: Well, I'm trying to think.

SMITH: Well, approximately.

SPANAGEL: Oh, about 1939 or 1938. It had to be in that time frame. I've never heard any more about it since then. At that time, we just had radio, so they broadcast it all by radio. There was no television. I remember I told my family out in Wisconsin that we were going to be on, and they listened to that. That was kind of exciting.

There were a lot of things like that going on. Then of course, just being into the development of a product that showed so much promise, everyone around was so enthusiastic. It was a wonderful feeling. It was wonderful to do. [phone rings; brief interruption]

Now, let's see. We're at what point now?

SMITH: Well, they were going to build a plant in Seaford.

SPANAGEL: Shortly after the plant started, I was asked to go to the plant as a plant technical superintendent. Of course, you never turn down a job, a promotion.

SMITH: So you moved to Seaford?

SPANAGEL: Yes, I moved to Seaford. I was there three years, from 1940 to 1943. Then I was brought back to the Experimental Station as assistant director of nylon research, and I was working for Windy Heckert. He was the laboratory director of nylon research, and I was his assistant. I worked there three years until 1947, and then was asked to go up to Buffalo to be the head of the cellophane research laboratory. I didn't know a darned thing about cellophane.

SMITH: In that period with nylon, what sorts of things were you working on at Seaford? Were there any big problems?

SPANAGEL: When I went to Seaford, I went as a supervisor and had technical and engineering people reporting to me. The technical people were primarily concerned with difficulties encountered in all the operations and with testing incoming materials as well as the yarn that was being shipped.

I remember a problem that we faced shortly after I got there. The filaments from the spinning machine were flying against each other and causing breaks. The problem was corrected by a simple change in the cleaning schedule for the screen on the air chimney used to cool the filaments.

Another example was, the nylon salesmen were reporting incidents of dying unevenness showing up in stockings. Examination of the drawing machines showed some oscillation in the gears.

SMITH: Did you like being an engineer? Because you went from being a chemist to being more like an engineer, I guess. Did you like that kind of problem solving?

SPANAGEL: The real difference was in being a supervisor. You don't do the work, but the people reporting to you do. Ideas of what should be done with any problems come from them and you and maybe other people, so the emphasis has to be on getting the problem solved and not who did it. Actually I liked research, but being a supervisor has its compensations.

SMITH: I see.

SPANAGEL: I next was returned to nylon research at the Experimental Station in 1943, for three years, as manager of nylon research, and then I went to the Yerkes Research Lab in Buffalo, New York, as assistant director of the laboratory. During my stay at Buffalo, the laboratory developed a continuous process for making cellulose xanthate, which is used to make cellophane. It also developed a process for making Mylar film. Mylar film is made from the polyester terephthalate that makes Dacron fiber.

SMITH: Yes, the polyester.

SPANAGEL: In 1950, I was transferred back to Wilmington as assistant manager of film production for the film department, and in 1952 became assistant director of production, and in 1957 director of production. That about covers it, except the last six months or so before I retired, I did some liaison work in the start up of a new Mylar plant in Luxembourg.

SMITH: Very good. Let's go back to the nylon development just a minute. What do you consider your most important contributions?

SPANAGEL: Oh, let me see. I suppose finding out how to make polymer consistently, because without that knowledge, we couldn't even consider a commercial operation. My other contribution of finding a size was important only because without it, we didn't have a yarn that would work on a full fashion knitting machine, and women's hosiery was the first prime market we were looking at.

[END OF TAPE, SIDE 3]

SMITH: Good. These were important contributions.

SPANAGEL: Yes, but I would like to think that what I did every day was also important.

SMITH: Right. Maybe we can talk a little about the actual work and supervising of people. Which did you like?

SPANAGEL: If you carry out a chemical reaction the same way every time, you get the same result every time.

SMITH: With people, it doesn't always work that way.

SPANAGEL: Right. I used to think that to actually do the work in the laboratory was more satisfying. I think Carothers also thought that way, because he had a laboratory of his own right next to his office, although he didn't do much actual laboratory work. I accepted a supervisory job as soon as I was offered one, in spite of the fact that this involved a move for my family to the small country town of Seaford. I guess my preference for laboratory work was not very strong.

Carothers and Paul [J.] Flory were working diligently on finding out how long the chains in the polymer were. Carothers didn't seem to be very interested in the semi-works being developed for testing nylon plant processes.

SMITH: Tell me a little bit about Paul Flory.

SPANAGEL: Paul Flory joined Carothers' group about a year or so after I did. He developed a formula for predicting the lengths of the chain in the polymer, and this helped in understanding what the polymer was actually like. After Carothers died, Flory seemed to lose interest in the kind of work he was given to do, and he left DuPont. After various jobs in companies and schools, he finally ended up at Stanford University, teaching and doing research. Much of his work was in polymers, and he got the Nobel Prize for this work.

SMITH: The Nobel Prize in chemistry.

SPANAGEL: Yes. I always thought that if Carothers had lived, he might well have gotten the Nobel Prize.

SMITH: Yes, he probably would have. Did you know Paul Flory well?

SPANAGEL: I knew him very well the first few years he was in Wilmington, because we both lived in the same bachelor house. This was a very nice place for a new employee to live, because the house held six people, and we all had complete use of it. The lady who managed it lived next door and she had a girl come in and clean the rooms, make our beds, and prepare an evening dinner. All six of us ate dinner together. We also prepared our own breakfast and had lunch at work. It was a pleasant and easy way to live, and you spent plenty of time with your housemates and got to know them very well.

SMITH: I can see how you got well acquainted.

SPANAGEL: Both Flory and I got married in a couple of years and soon both of us moved out of Wilmington. Then we met only at odd times, like special Chemical Society meetings. The last time I talked to Paul Flory was after a section Paul gave at the Chemical Society meeting in Wilmington. That was years ago.

SMITH: Did Carothers and Flory get along well?

SPANAGEL: Yes. I have every reason to believe that Carothers thought highly of Flory. Paul Flory told a number of people that Carothers gave him a lot of good ideas about polymers. Unfortunately, Paul died several years ago.

SMITH: After Carothers died, who carried on his work on fundamental research?

SPANAGEL: I don't know, but I have a feeling that when Carothers died, most of the fundamental work stopped. You have to remember that a lot of questions about long-chain polymers had been answered.

SMITH: Right. Well, we have talked about a number of things, and I want to thank you for spending this time with me today.

[END OF TAPE, SIDE 4]

[END OF INTERVIEW]

NOTES

1. C. F. H. Allen and E. W. Spanagel, “The Reactions of Anhydracetonebenzil. II.” *Journal of the American Chemical Society* 55 (1933): 3773.

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