THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

JAMES BURTON NICHOLS

Transcript of an Interview
Conducted by

Raymond C. Ferguson

in

Wilmington, Delaware

on

14 and 16 January 1986

ARNOLD AND MABEL BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

Oral History Program

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Oral History Program

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J. Burton Nichols

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(Revised 6 February 1986)

JAMES BURTON NICHOLS

1902	Born in Danbury, Connecticut on 5 February

Education

1923	B.S., chemistry, Cornell University
1924	M.S., chemistry, University of Wisconsin
1927	Ph.D., physical chemistry, University of Wisconsin

Professional Experience

	E. I. du Pont de Nemours & Company, Inc.
1927-1942	Research chemist
1942-1953	Head, physics section
1953-1966	Supervisor, physics and analytical division

ABSTRACT

Born in Danbury, Connecticut, Burton Nichols was only a few months old when his father died and his mother then found employment in the local industry so as to support Nichols and his sister. Encouraged by the high school superintendent, Nichols won scholarships to help him through his undergraduate studies of chemistry at Cornell, where he completed a senior research project with Wilder D. Bancroft. At Bancroft's urging, Burton Nichols met Svedberg, then on his way to Wisconsin on sabbatical leave, and followed him to Madison. His introduction to sedimentation techniques was by construction of a pioneer optical centrifuge and its use in pigment characterization. Fellowships enabled the newly-married Nichols and his bride to go to Uppsala where he contributed to the early development of the ultracentrifuge. Recollections of this period are followed by an account of his arrival at the Du Pont Experimental Station to work in Kraemer's group, starting with the application of ultracentrifugal techniques to industrial problems. During his long career at Du Pont, Nichols was involved in the evolution of new instruments and polymer characterization. The interview concludes with Nichols recalling colleagues, Du Pont management and organization as well as his professional society activities.

INTERVIEWER

Raymond C. Ferguson obtained his degrees in chemistry from Iowa State University (B.S., M.S.) and Harvard University (Ph.D.). He worked in research divisions of the Organic Chemicals, Elastomer Chemicals, and Central Research Departments of Du Pont, principally in molecular spectroscopy, organic structure analysis, and polymer characterization. Currently he is affiliated with CONDUX, Inc., a consulting association of ex-Du Pont professionals.

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INTERVIEWER: Raymond C. Ferguson

LOCATION: Bellevue Manor, Wilmington, Delaware

DATE: 14 January 1986

FERGUSON: Your wife calls you Burton. Do you prefer Burton or Burt?

NICHOLS: Burt.

FERGUSON: Burt. I thought that's what you were known as at work.

NICHOLS: I started out as "Nick" in Wisconsin.

FERGUSON: Is that right?

NICHOLS: Burt is the preferred one.

FERGUSON: I know you were born in 1902, but I don't have a record of where.

NICHOLS: In Danbury, Connecticut.

FERGUSON: Can you tell me something about your parents?

NICHOLS: Well, my father, who was a farmer, died when I was four months old, leaving my mother with a small daughter and me. My grandmother took care of me and my mother supported us. I went through high school and was able to get a tuition scholarship from Cornell. At the time this was a munificent rate of one hundred dollars a term.

FERGUSON: This was the scholarship or the tuition?

NICHOLS: The scholarship covered the tuition.

FERGUSON: What about your living expenses?

NICHOLS: Oh, they might have run six hundred or so. I don't remember now.

FERGUSON: What did you do to meet your expenses?

NICHOLS: I did some dishwashing, and worked at a soda fountain from ten to twelve at night and to about one on Saturday nights.

FERGUSON: This was in Ithaca?

NICHOLS: Yes.

FERGUSON: What was your mother's education and background?

NICHOLS: Well, I think just a housewife, except that to support us she did a couple of things. One was in the hatting industry. She trimmed hats, since that was the main industry in Danbury at that time. Then she worked with the Rogers Silverplating Company, who had a plant just a few doors from us. It was very convenient. One time when I was quite young, let's say five, I got my head stuck in the exit of the chicken coop (we were allowed chickens in the city at that time). Since she was so close to us, she came home and got someone to free me.

FERGUSON: What was your father's name?

NICHOLS: James Henry.

FERGUSON: And your mother?

NICHOLS: Martha Barnum.

FERGUSON: That was her family name, Barnum?

NICHOLS: No, Wildman.

FERGUSON: An earlier draft of your biography said they had English and Welsh forebears.

NICHOLS: She was on the Welsh side and my father's mother was on the English side.

FERGUSON: Did she have a high school education?

NICHOLS: Yes, I think she had that much. I just don't know.

FERGUSON: Was it family influence or school influence that got you interested in science?

NICHOLS: I really was heading toward a career in civil engineering at Yale since that was the popular thing for the boys in those days. They were building bridges and all sorts of things. I'm sure glad that I didn't go into that field when the Depression came along.

FERGUSON: What was Danbury, Connecticut like in those days. Was it an industrial town?

NICHOLS: Oh yes, and it was one of the first stopping places for the immigrants. The Irish came over of course. And later the Italians and...

FERGUSON: Polish?

NICHOLS: Well, Polish to some extent. The Portuguese came too, but the Irish were the really hard workers. They dug the ditches and they did all sorts of things. They made their way.

FERGUSON: I assume you went to public schools from kindergarten through high school.

NICHOLS: Yes.

FERGUSON: How large was your high school?

NICHOLS: Oh, we had a graduating class of a hundred. So, it was in the general order of four to five hundred.

FERGUSON: In your interview with John Smith (1), you said that your school superintendent, Borst, wished you to go to Cornell instead of Yale. Had you already gotten an interest in chemistry from high school?

NICHOLS: Yes. I took that in my senior year.

FERGUSON: What was taught in high school chemistry in those days?

NICHOLS: Oh, I suppose a little bit of analytical chemistry; we probably made a few compounds.

FERGUSON: Do you remember a laboratory?

NICHOLS: We had a laboratory, so we could make titrations. I remember that much, but not too much more.

FERGUSON: Was there work on combustion or Priestley theory or gas laws?

NICHOLS: Oh, the atom was indivisible, a hard ball in those days. I was in high school 1915 to 1919 and the recent work hadn't crept into the textbooks by that time.

FERGUSON: I was in high school in the 1930s when I finally heard about the electron. I think it was Anderson's work that was publicized in the popular press.

NICHOLS: Anderson or Chadwick or somebody.

FERGUSON: What effect did World War I have on you and your family?

NICHOLS: No particular effect. I was in high school during that. We all had war gardens, sold war bonds. We had shortages, of course. In the winter when there was no coal to speak of, we high school kids went out and chopped wood. I had a three-acre wood lot from my father's family. We would chop dead chestnut trees, and oak and so forth. We had wood for ourselves and we probably had wood to sell.

FERGUSON: Turning to your time at Cornell, you said there were about forty people with you in the chemistry program.

NICHOLS: Yes. That was our class.

FERGUSON: How many were women?

NICHOLS: At most I would say half a dozen. I remember one, Hildegarde Payer. She was from Tupper Lake up in the Adirondacks. I thought she was French Canadian, but she may have been Chilean.

FERGUSON: How many were on the chemistry faculty?

NICHOLS: Possibly twenty, but that's only a guess. Louis M. Dennis was still the king. He was called "the King". He was chairman of the department. I worked under Wilder D. Bancroft.

FERGUSON: What was your goal and expectation from the bachelor of chemistry degree?

NICHOLS: Oh, I hadn't got very far in expectations. It was with Bancroft that I had my first introduction to a high polymer. I didn't know it was that when I worked on the molecular weight of nitrocellulose.

FERGUSON: In fact, you wrote a paper under your own name on that work (2). Was this a senior research requirement or something you undertook for special credit?

NICHOLS: No, that was senior research. The seniors were either allowed or supposed to have a small project at that time.

FERGUSON: What was the motivation behind studying nitrocellulose?

NICHOLS: It looked like something that I could do, I guess. [laughter]

FERGUSON: That clearly was an exposure to a high polymer, but the term wasn't used in those days.

NICHOLS: All I could determine was that, if it had a molecular weight, it was quite high.

FERGUSON: What subjects were required in the undergraduate chemistry degree?

NICHOLS: This bachelor of chemistry degree was essentially a chemical engineering degree. That was before the days of Dusty [Fred H.] Rhodes. In fact, he arrived about halfway through my course and brought a breath of fresh air, because he came directly from the Barrett Company, where he was chief chemist. He introduced problem courses. He would tell about how they worked out solutions to problems in the plant, tracing the inventory of materials as they went through, and catching a place where the leak was and all sorts of practical things.

FERGUSON: Were the other faculty strictly academic types, with more classical traditions?

NICHOLS: Yes. They hadn't got to the stage where everybody was consulting with some chemical concern or other.

FERGUSON: That didn't come along until quite a bit later, did it?

NICHOLS: It must have been, probably, in the 1930s.

FERGUSON: I looked at the first Du Pont chemical department organization charts. There were four consultants in 1925. One was Emmet Reid. I guess you knew this.

NICHOLS: Yes. He was a perennial.

FERGUSON: I can't remember who the others were.

NICHOLS: Adams could have been one.

FERGUSON: Roger Adams, I think, hadn't come on board yet in 1925.

NICHOLS: Carl S. [Speed] Marvel had not come at that time.

FERGUSON: Did you have the breakdown of subjects like organic, inorganic, and analytical at Cornell?

NICHOLS: Yes, we had courses in organic chemistry. If you weren't good enough to exempt the final, you were in trouble because William R. [Uncle Billy] Orndorff gave pretty stiff finals.

FERGUSON: This was in inorganic?

NICHOLS: No, organic. We made preparations and such things.

FERGUSON: Was there a physical chemistry course?

NICHOLS: Yes.

FERGUSON: Do you remember who taught that?

NICHOLS: Thomas R. Briggs. Inorganic chemistry was taught by Professor Arthur W. Brown. He gave the introductory freshman chemistry course, too. I took microscopy. Emile M. Chamot was still active at that point.

FERGUSON: Oh, Chamot was there?

NICHOLS: Yes, he was really the one that introduced the subject to the country.

FERGUSON: Was he an analytical chemist?

NICHOLS: I don't know. His prize student was Clyde W. Mason. After I got through and was going to Wisconsin, Mason suggested that I start a course out there. I was flattered, but I didn't want to get tied up that way.

FERGUSON: He was asking you to teach microscopy?

NICHOLS: Yes, he wanted me to organize a course at Wisconsin.

FERGUSON: Was Mason a professor?

NICHOLS: He was an assistant professor at that time. I had an elementary spectroscopy course. That was with Jacob Papish. We used a flame for elemental analysis.

FERGUSON: Was it just visual observation, or did you use some sort of dispersive element to get a spectrum?

NICHOLS: We must have had a prism spectroscope.

FERGUSON: In your work on nitrocellulose with Bancroft, were you, or anybody else there at that time, aware of Staudinger's interest and ideas about polymers?

NICHOLS: That was too early. That was in 1922, let's say. So, Staudinger may have been just starting that line of work.

FERGUSON: There wasn't, as far as you know, anything in the literature?

NICHOLS: At least it didn't percolate down to the undergraduate level.

FERGUSON: Did you have a lot of journals to read or a lot of reading to do as an undergraduate?

NICHOLS: I don't recall. We didn't have many journals in those days. The American Chemical Society series had not separated too many yet. We had the Journal of Physical Chemistry, which Bancroft ran, and the Journal of the American Chemical Society. I can't recall whether Industrial and Engineering Chemistry had started at that time. [Volume 1 in 1909.]

FERGUSON: I suppose that a graduate student at Cornell would likely have more exposure to the foreign journals.

NICHOLS: Yes. We had a fair library there. We were in the remains of a chemistry building which had burned two or three years before I got there. Just the lower story remained. We took our qualitative chemistry in the attic of Rockefeller Hall, which was the Physics building. There was a large group of Arts students taking this as an introductory course. When they all got working with hydrogen sulfide, the air just got so blue you couldn't see across the room. Well, there were one or two cases where somebody got too much hydrogen sulfide and dropped, but no fatalities.

FERGUSON: When I was in school, in the 1940s, the attitude toward safety at colleges and universities was still pretty cavalier.

NICHOLS: Oh, sure. I don't think they knew the word safety. I remember one occasion in organic lab. I was making ether and distilling it with a Bunsen burner underneath. For some reason I decided that I'd better shut off that burner and take it away. The balloon flask cracked almost immediately, but I didn't actually get a fire.

FERGUSON: You weren't getting ether vapors out in the room?

NICHOLS: No, but the main vessel cracked and let it out. So I'd have had a pretty bad fire.

FERGUSON: What was Bancroft's field?

NICHOLS: Colloid chemistry.

FERGUSON: You said he was a real character. What did you mean?

NICHOLS: It's a little hard to say. Let's say he didn't think in straight lines. I'd judge that he was probably more like an English chemist. He'd had a broader education than most chemistry professors at the time. That showed in his writings. He always had something interesting in his Journal of Physical Chemistry that made better reading, rather than just scientific knowledge.

FERGUSON: Was there anybody else at Cornell that particularly influenced your future career?

NICHOLS: The professor I had for calculus, Walter B. Carver. He somehow instilled in me a way of mathematical thinking. It did not go as far as I would like. I didn't get any farther than differential equations. I went on and took a couple of math courses at Wisconsin. Also, the head of the engineering department, Dexter S. Kimball, gave a course in industrial organization which was very interesting. He was a good lecturer. I have his book (3).

FERGUSON: I'd like to move on to The Svedberg. I noted that in one of your patents he lists himself as Theodor Svedberg (4).

NICHOLS: He was born Theodor, but he adopted The very early in life.

FERGUSON: I'd often wondered whether he did this because of the English meaning of "the". [laughter]

NICHOLS: I don't think so. I never asked him point blank.

FERGUSON: How did you come to meet him?

NICHOLS: I finished in three and a half years at Cornell. I had had three years of French and German during high school. They gave me credit for my German, so I came with some credits already, and I took fairly heavy schedules. At the beginning of my senior year, I found out I could finish at mid-year. Bancroft told me in December, I think it was, that this young Swedish professor was coming to spend a term at Wisconsin and was stopping at Rochester to give a lecture at Eastman Kodak. He said, "You'd better go up and see him. Maybe you'd want to go out there for a while."

So I did, early in January. I was very much impressed, because I had expected to see one of these staid, bewhiskered European professors, fairly plump. Instead, here was this very slender, boyish, enthusiastic person. He looked like a very good man to work for or with. He was about thirty-eight.

FERGUSON: Yes, and he would have been professor of physical chemistry at Uppsala for about ten or eleven years.

NICHOLS: Oh, yes. He was one of the youngest professors at the time of his appointment.

FERGUSON: He already had an established reputation, didn't he? I was wondering why he came to Wisconsin.

NICHOLS: Because Professor J. Howard Mathews, the head of the department, had the idea that he would like to establish a colloid chemistry course at Wisconsin, and even an institute for colloid chemistry. Mathews asked Svedberg to come and teach in the spring and summer. There were lectures and laboratory work. There was a group of twelve to fifteeen who came for the spring term and took the various projects that he had to offer. Some of them brought a project with them, such as Dick [Richard E.] Bradfield, who was a well-known soil chemist at Rolla, Missouri. He later went to Cornell and became very well known. In fact, there is now a building named for him at Cornell.

FERGUSON: Were these people mostly already established or were they graduate students?

NICHOLS: Well some of them were just fresh out of graduate school. Alfred J. Stamm, for example, came from Caltech and I think he had just graduated. He had his Ph.D. at that time. He later went on to the Forest Products Laboratory at Madison and became one of the foremost wood chemists, specializing in the structure of wood. I don't think he went into the chemical aspect so much. After he retired from Forest Products he went down to Raleigh, North Carolina, and became a professor there. He died just this last year. In fact, most of my friends that I knew at that time have died.

FERGUSON: In the biography Arne Tiselius and Stig Claesson wrote on Svedberg that appeared in Annual Reviews of Physical Chemistry, they stated that Svedberg's invitation to Madison came at a very opportune time and that this was a turning point in his career (5). Did you sense that?

NICHOLS: I think so, because the enthusiasm he showed was really something.

FERGUSON: I gathered that research funding and conditions in Sweden immediately after World War I were pretty bad.

NICHOLS: Well, it was pretty insular. Each professor sort of made do with what he had.

FERGUSON: Then, you enrolled immediately as a graduate student at Wisconsin?

NICHOLS: Yes. I made my decision immediately, probably around the middle of January. By the ninth of February I was out there and had selected my problem.

FERGUSON: Had you considered graduate school elsewhere?

NICHOLS: No, I hadn't really. I'd had some talks with Eastman Kodak that I might go there. In fact, when I went up to meet Svedberg, I made a couple of contacts. But he was the deciding factor for me. Of course, the salaries were very high in those days. People with B.S. chemistry undergraduate degrees got $125 a month. That was stable for several years. Ph.D. chemists got $200.

FERGUSON: These would be industrial salaries, such as at Kodak?

NICHOLS: Yes, and it was approximately the same at Du Pont.

FERGUSON: How about financial support at Wisconsin?

NICHOLS: Let me go back. After my first year [at Cornell], I applied for a Knickerbocker bursary. It was set up by a prominent man in New York. I was successful in getting that. It amounted to about nine hundred a year. I don't think I had to continue late night work behind the soda fountain. I could make my way. I had joined the Alpha Chi Sigma by that time, also.

FERGUSON: Was that a social fraternity?

NICHOLS: No, it's chemical. So my expenses were not high, because it was a cooperative house.

FERGUSON: Was there also an Alpha Chi Sigma in Wisconsin?

NICHOLS: Yes. I went there immediately, so I was right in the middle of things.

FERGUSON: Did this Knickerbocker bursary continue when you went to Wisconsin?

NICHOLS: It was for my completion of the four years and they allowed me to take the final term portion along with me to Wisconsin, so that really tided me over. By that time, I was a lab assistant and I think I was getting fifty dollars a month, which kept me going.

FERGUSON: It was obvious to me that as soon as you got there, you must have started on your optical centrifuge, because you had a paper with Svedberg published in December of 1923 (6). Did you start out immediately?

NICHOLS: Yes.

FERGUSON: Did you have course work going on at the same time?

NICHOLS: Oh yes, the regular graduate schedule. There were some good men at Wisconsin. Professor Victor Lenher, Sam Lenher's father, was professor of inorganic chemistry. He'd done a lot of work with some of the elements. He was a graduate of a European university, so he was quite a proud man. Sam, his son, got some of his bearing from him.

FERGUSON: Can you think of some others?

NICHOLS: Well, Professor Farrington Daniels had come just about a year or so before I got there. I was associated with him. We took the first course in chemical thermodynamics from the book by Lewis and Randall (7). This was the first work on thermodynamics that was understandable by an ordinary person. It was full of problems to solve. We worked from the manuscript while Daniels was reviewing it, so we had the benefit from it even before it was even published. That really opened our eyes to what a good foundation thermodynamics would be in the study of reactions.

FERGUSON: Wisconsin had some outstanding analytical chemists, but that may have been after your time.

NICHOLS: Yes, it was well after. John E. Willard was one.

FERGUSON: Who was the organic man there?

NICHOLS: That was Homer B. Adkins. He was a good man. All of our Wisconsin people that came to Du Pont had taken their organic with him, just as those that came from Illinois took it from either Roger Adams or Carl Marvel.

FERGUSON: How did you and Svedberg work together on this optical centrifuge?

NICHOLS: He let me pretty much develop my own ideas, which were pretty poor, as you might expect for a young graduate without any particular background. My mechanical background was not very good. I'd had a number of courses in engineering at Cornell. I didn't get into too many problems, but if he had worked on it himself, it would have been a much more polished sort of thing.

FERGUSON: Was he an accomplished machinist?

NICHOLS: He knew pretty much what he wanted.

FERGUSON: You started with a Dumore high speed motor. Was this an electric drive?

NICHOLS: Yes. It was a high speed electric grinding motor. It had an extension shaft and carried a small grinding wheel. For that purpose, it would go up to twenty thousand [rpm]. Then I put this heavy head on there, and I never got more than about two thousand.

FERGUSON: Was the shaft vertically mounted in a standard centrifuge design?

NICHOLS: No. You could hold the Dumore motor alone in your hand and get around to the various surfaces. I mounted mine in a heavy block. I actually put sandbags around the outside, because I was expecting a higher speed.

FERGUSON: You cut slots through the head?

NICHOLS: It had a brass cylinder, with slots for viewing direct light. Also, you could look at different angles to get scattered light for colloidal material.

FERGUSON: I gather you did some pigments.

NICHOLS: Yes, one of the people that came when Svedberg was there was Henrietta Liebe, from one of the pigment companies. I think it was Acme. (It probably is no longer in existence.) She brought a series of samples from various manufacturers. She wanted comparisons, so I ran those for her.

FERGUSON: What was her background?

NICHOLS: I think she had a Ph.D.

FERGUSON: It sounds like she might have been German.

NICHOLS: Yes, it could well be.

FERGUSON: Was she from a German university?

NICHOLS: I don't know. Anyway, this was a good choice, because since they were relatively coarse, I had to use a high viscosity medium. I dispersed them in 95% glycerin, so the convection was not as bad as it would have been in aqueous suspension. There were a lot of things wrong with the first design which showed up and enabled Svedberg to design what he thought would be a workable centrifuge on his way by ship back to Sweden.

FERGUSON: Did you have optical microscopy for particle size determination?

NICHOLS: Yes, and ultramicroscopy. Many nights I stayed up until twelve or one o'clock counting fields of colloidal gold particles in the ultramicroscope.

FERGUSON: Did you do that work at Wisconsin?

NICHOLS: Yes. I found I could get about the same result with my eyes opened or closed, because the essential thing was to dilute to a point where you could count at a glance no more than five particles. They were streaming by, or slowly streaming by, depending on the stillness of the atmosphere. I had a metronome, so I'd count each second or so.

FERGUSON: Was this optical microscopy?

NICHOLS: Yes, but this was the ultramicroscope so we saw the light scattered by the small particles.

FERGUSON: What particle size range -- three hundred micrometers?

NICHOLS: No, they'd be down somewhere in the range of twenty to fifty. The so-called Faraday sol, which would just give a blur in the ultramicroscope, would be down close to three. Of course, with that heavy density they were easy to centrifuge. The first working ultracentrifuge was developed in Sweden. Herman Rinde, the man that was associated with Svedberg, had done a number of years work on gold sols for his doctorate.

FERGUSON: Did you make your gold sol samples?

NICHOLS: Yes, because they had a pretty short life. They started to grow or floc.

FERGUSON: What did Svedberg require of you?

NICHOLS: When I was in Sweden?

FERGUSON: Let's talk about Wisconsin first.

NICHOLS: I was his lecture assistant, so I had things to prepare during the summer session.

FERGUSON: He lectured on colloids?

NICHOLS: Yes. He had a number of demonstrations. One was the preparation of colloidal metals by the spark technique.

[END OF TAPE, SIDE 1]

NICHOLS: That chair you are sitting on is a family heirloom, about 200 years old.

FERGUSON: I'll be a little more careful!

NICHOLS: It's quite sturdy. That painting on the wall was done in 1790. That was my great-grandfather.

FERGUSON: What was his name?

NICHOLS: Ebenezer.

FERGUSON: So the English side of your family has been in this country since ...

NICHOLS: We came over in 1634. My English ancestor was one of the nine families that founded Stratford, Connecticut, part of which is now Bridgeport.

FERGUSON: We didn't have any of this information in your biography.

NICHOLS: No. I've spent several years, off and on, working up the earlier part of the family. I've never done more than the Nichols family.

FERGUSON: You people that live in the East have a great advantage. You can trace things back much more successfully than those of us who ended up in the West.

NICHOLS: Well, I go back to about 1500. The first ancestor of record was born around 1500 and he was a brewer in London. On one of my trips to England, I walked the street where he lived. Of course, in the meantime, it had been subjected to the great fire of 1666, which covered what was then most of London, but is now just a very small part of London. Then it was flattened again in the air raids. It's now filled with foreign banks. It's about a one block street. [laughter]

FERGUSON: To get back: we were in the middle of your tour at Wisconsin. You were going to add something about Svedberg.

NICHOLS: He was a great botanist and he took some time out from his work to make a collection of the local wildflowers. He had little social life. He was married at that time to his second wife. He ended up, I think, with five.

FERGUSON: Five wives?

NICHOLS: Yes. The first one was a professional woman. She was a doctor. They agreed to disagree because each one was working on their own careers. The next one, the one we knew in Sweden and Wisconsin, was a homebody. I think there were three or four children by her, and a couple by the first wife. The third one was sort of a -- nobody liked her.

FERGUSON: What happened to the second wife?

NICHOLS: He divorced her because I guess he was attracted to this younger woman. That ended up again in divorce. Then, right after the war, he met the editor of one of the good Swedish craft magazines. They hit it off pretty well. They were compatible. She stayed with him and took care of him until he died in 1971. She's still alive.

Well, there is one thing I did want to interject here. When it appeared that I should go over to Sweden, I applied for an American Scandinavian Foundation scholarship. The first year I was an alternate. Then the next year, I got it. At that time, my wife was just graduating from Wisconsin in applied arts. We wanted to get married, and I wanted to take her over with me. Her parents said she should have a year of teaching first before she got married. I said, "Well, if she doesn't go with me, I won't go either." [laughter] That solved the problem.

FERGUSON: You had met her at Wisconsin?

NICHOLS: Yes. She was the first girl, I guess, that I met there.

FERGUSON: And what was applied arts?

NICHOLS: Well, it's sort of industrial arts. It's work in metals and wood; some painting, drawing, various industrial arts.

FERGUSON: When did she graduate?

NICHOLS: 1925.

FERGUSON: You were there from February 1923 to the summer of 1925?

NICHOLS: Until July 15th.

FERGUSON: Then you were married in the summer of 1925?

NICHOLS: Yes. We were married in Madison.

FERGUSON: Before she graduated?

NICHOLS: No, she graduated in June and we were married in the middle of July.

FERGUSON: What was her family background?

NICHOLS: Her father was Danish and her mother Norwegian. Therefore we had to go to Sweden.

FERGUSON: Did she speak the languages?

NICHOLS: She didn't really speak it then, but she could recognize it.

FERGUSON: How about you?

NICHOLS: Well, my background was entirely different, so she twitted me most of the time that my pronunciation was terrible. She could get as close as a non-native could get to the language.

FERGUSON: So your trip to Sweden, by boat, was essentially a honeymoon trip?

NICHOLS: Oh, yes.

FERGUSON: What were the accommodations?

NICHOLS: This was the Swedish-American line, and they gave the Fellows privileges. They gave us second class accommodations, but at the third class rate. Our cost was $125 apiece for about fourteen days.

FERGUSON: Marvelous.

NICHOLS: Yes, that's what I thought.

FERGUSON: Eleanor's father was a farmer?

NICHOLS: No, he was a machine designer with the Gisholt Machine Company in Madison. He worked right up until his death, practically. He could come and go as he wanted in the last years, but he was still active.

FERGUSON: Was she living at home then and going to school at Madison?

NICHOLS: Yes. Her mother would stuff me on a Sunday dinner, which I usually had with them, so much so that I could hardly get up. [laughter] She wanted to see me eat. Anyway we got along all right.

FERGUSON: Your interview with Smith covered some of the general aspects of your two years at Uppsala, I think more on the social side than on the scientific side. Were the courses, the lectures, and the seminars all in Swedish?

NICHOLS: Yes. Well, I took almost no courses. I usually attended Svedberg's lectures, which were often on the subject for which the Nobel committee was going to award that year's

Nobel Prize. So we had an idea. In one year for instance, the course was on enzymes, so that meant certain people would be nominated.

FERGUSON: Did he select these topics to help himself understand the field?

NICHOLS: Yes.

FERGUSON: Was he on the committee?

NICHOLS: Yes. He would then have this practically first-hand knowledge of the field, because by the time you have prepared a series of lectures you know something about a subject.

FERGUSON: Was he a good lecturer?

NICHOLS: Yes. He lectured in English at Madison.

FERGUSON: Did he have good English?

NICHOLS: Oh, yes. Very good.

FERGUSON: In the scientific literature at that time, German was most important, wasn't it?

NICHOLS: Yes, but at the time we got over there, the Swedish people were changing to English as their second language. They would have nothing of German if it was possible.

FERGUSON: You suggested in your other interview that you and Eleanor were in demand because people wanted to practice their English.

NICHOLS: Oh, yes. We were also the envy of young students. They had a passbook system for alcoholic beverages. They were state controlled. You had to present this book. A young student could get only one liter of hard liquor a month, whereas a married couple could get two, so we were looked up to. [laughter]

FERGUSON: How did Eleanor occupy her time during your stay?

NICHOLS: She did some painting. She painted several of the picturesque university buildings. There's an old castle on the hill which was quite picturesque, and other things. We very shortly became acquainted with some medical people. We had a series of friends who occupied our spare time. She had tea for the young students, that is, Svedberg's students. She brought tea and bakelser pastries, these nice rich confections, cookies, and that sort of thing.

FERGUSON: What were the similarities and differences between the Universities of Uppsala and Wisconsin?

NICHOLS: Well, the students at Uppsala were much more mature, because after they had gone through the intermediate schools they were already equivalent to about a second year American college level. They were accustomed to independent work. I don't think it was all force-fed. They had to dig it out, as it was in many of the European universities. They didn't have the tutorial system that they had in the English colleges. Anyway, they were pretty well-rounded people by the time they got to the university. So that struck me. They weren't as frivolous as some of our American students, but they developed some of our traits in later years.

FERGUSON: I assume the professors did have lectures?

NICHOLS: Oh, yes. They had general university courses, and the various chemistry courses. If the students were specializing in chemistry, they had to attend those lectures.

FERGUSON: Had an oil-bearing centrifuge, or at least a low-speed centrifuge, been built there before you got to Uppsala?

NICHOLS: Yes. The low speed ultracentrifuge was built in 1924 and was functioning well.

FERGUSON: What was the speed?

NICHOLS: It was not over ten thousand rpm. It was in the five to ten thousand range. It was sufficient to centrifuge the smallest particles of gold sol -- the Faraday sol.

FERGUSON: Did you work on centrifuge design while you were at Uppsala?

NICHOLS: No.

FERGUSON: Did you go immediately into the work on egg albumin?

NICHOLS: That was my main one. I worked on egg albumin as the prime project and then on hemoglobin. With the sedimentation equilibrium centrifuge, you could only say that there was a good indication that it was a single molecule, or monomolecular. The oil-turbine centrifuge was built to give much better resolution and shorter time; when you had to run two or three days for a determination with the low-speed centrifuge, your output wasn't very high. You could get all the information you needed in two or three hours with the high-speed centrifuge.

FERGUSON: Your status there was essentially as a postdoctoral?

NICHOLS: Yes, even though I didn't have my doctorate. The second year I had a fellowship from the Rockefeller International Education Board. It was given usually only to postdoctorates, but Svedberg's recommendation was sufficient to give me the status. Then I was in luxury, because the Scandinavian-American fellowship was only a thousand dollars, from which I had to pay passage for two people both ways, if I had gone back home after that first year. The second one was two hundred dollars a month.

FERGUSON: It doesn't sound like much now, but you could get along quite comfortably on that?

NICHOLS: Oh, yes. We had the so-called matsals, the equivalent to a public dining room, where you could buy your meals by the month. So, at a hotel nearby, we had breakfast and dinner for eighty kronor a month apiece. A krona at that time was worth twenty-five cents, so it was twenty dollars a month for two meals. It was a buffet table, so you could go back and eat as much as you felt like. Eleanor gained about twenty pounds in two years.

FERGUSON: I was thinking that there was a little hazard to that.

NICHOLS: Yes, there was a hazard. Well, the fancy cakes also contributed.

FERGUSON: When you were at Uppsala, was the concept of the molecular nature of polymers being developed or accepted?

NICHOLS: Yes.

FERGUSON: Was that a topic of discussion in your seminars?

NICHOLS: Well, just previous to my getting over there, Robin Fåhraeus ran a sedimentation equilibrium on carbon monoxide hemoglobin and found to his surprise that it was apparently a single species -- not a range, or not indeterminant, as people had thought up until that time. They thought all proteins were just conglomerates. The uniformity was very soon corroborated by some refined osmotic pressure work at Cambridge by G. S. Adair. He came out with the same result, around sixty-seven thousand, which was four times the weight from the iron content.

FERGUSON: Was there communication between Svedberg, or other people at the Institute, and Staudinger?

NICHOLS: I don't know of any such, but there could have been. I don't know when Staudinger started his work, but it was in the early 1920s, I'm sure. It could have been just about the same time. It [polymerization] may not have gone more than a few additions in his first publication. He just demonstrated that you could add on one monomer unit at a time.

FERGUSON: You're saying Staudinger's contribution was really in the making of synthetic polymer molecules?

NICHOLS: Yes. Then he got around somewhat to characterization. Staudinger's equation dealt with the viscosity of the polymer in solution. It was a pretty simple sort of thing, but it gave a general idea.

FERGUSON: But you weren't aware of this work at Uppsala?

NICHOLS: No. Not until I got to Wilmington.

FERGUSON: I see. How well did you know the Nobel laureate at Uppsala -- [Manne Karl] Siegbahn?

NICHOLS: I got to know Siegbahn quite well because his institute was next door. I also met Svante Arrhenius, just once before he died. It was at a formal dinner in Stockholm. When he found out I was working with Svedberg he said, "Oh, he's a nice young fellow." At that time, Arrhenius was probably in his eighties. His liter a day of aquavit had not killed him. There was a

Swedish club called the one-liter club, and he belonged to that.

FERGUSON: Aquavit is hard liquor?

NICHOLS: Yes. It runs about eighty proof or so. At one of their meetings, one of the members got up to the podium to make a speech. He took out a liter bottle and drank it down. That was his speech. [laughter]

FERGUSON: What was Siegbahn's field?

NICHOLS: He was head of the physics institute. He developed a number of instruments. One was a microphotometer, which we used in Wilmington to convert the photographic record to a curve. This was used to establish the relationship between concentration and density.

FERGUSON: There really wasn't much instrumentation in those days, though.

NICHOLS: Not too much. Siegbahn's main field was x-ray. He developed this instrument as a sideline for the photometry of his x-ray plates.

FERGUSON: There was another Siegbahn.

NICHOLS: That was his son, Kai.

FERGUSON: It was the son who developed photoelectron spectroscopy. Paul Bierstedt worked on that a few years ago, probably after you left.

NICHOLS: Yes. I didn't know about it. This characterizes the first few molecular layers on the surface.

FERGUSON: You said earlier that the group at Uppsala and science in Sweden was more or less isolated from the rest of Europe. Did you have much interaction with the European and British scientists?

NICHOLS: I don't remember much. Of course, Svedberg on occasion gave lectures elsewhere, but a lot of that came later. I think it was fairly insular.

FERGUSON: Were there plenty of journals and papers to read there or was scientific discovery communicated more directly in correspondence?

NICHOLS: No. We had a number of the English journals. Nature and the Biochemical Journal. Also the Proceedings of the Royal Society. Svedberg's interests gradually became almost entirely biochemical. The journals that were immediately available were more in that general field. People came from England to work on the centrifuge. There were one or two that built ultra-centifuges, or had Svedberg build one for them. They could then continue work on medical problems and medical analyses.

FERGUSON: Did you return to the University of Wisconsin?

NICHOLS: We got off the boat and came right down to Wilmington, and had about a month or less just to get established. Then I went out to Wisconsin and I had my doctoral exam.

FERGUSON: So you had really essentially finished at Wisconsin except for your thesis?

NICHOLS: Yes. I had written a good bit of my doctoral dissertation on the west coast of Sweden, propped against a rock on one of the islands off the coast. We spent a month there. I would write it down and then Eleanor would type it. By the time I left there I had it pretty much done.

FERGUSON: When was this? You went on a tour of Europe in April of 1927.

NICHOLS: Yes. Just the month of April. Then we had about a month on the west coast in the summertime.

FERGUSON: The preceding summer?

NICHOLS: No, the same summer.

FERGUSON: Oh, you came back to Sweden then.

NICHOLS: Yes. The universities closed down in the summer, because the Swedes like the sun so much. They don't vegetate entirely, but they go off where they can get as much sun as possible.

FERGUSON: So you could write almost twenty-four hours a day if you wanted to.

NICHOLS: Well, it wasn't quite that easy. It got, not actually dark, but twilight at around ten. At two it started to get light. On a hill, you could read a newspaper anytime of the night. It was really flat there. It was sixty degrees north.

FERGUSON: You were due to arrive at the Du Pont Chemical Department in November 1927?

NICHOLS: That's right. Du Pont had me on sort of half-time for the three months of the fall in Sweden, while the Swedes were building one of their lower speed ultracentrifuges. I waited and brought back some of the parts, which I wanted to keep under my own supervision.

FERGUSON: Did Du Pont buy the centrifuge from Svedberg's institute?

NICHOLS: Yes. They bought three of them altogether. Two low speed centrifuges, and, in 1935, the oil-turbine ultracentrifuge.

FERGUSON: Was this Elmer Kraemer's idea?

NICHOLS: Yes.

FERGUSON: Had you known him at Wisconsin?

NICHOLS: Yes. He had earlier spent some time in Uppsala at Svedberg's lab. I'm not sure whether it was before his degree or after. He preceded me at Du Pont by a year, at the time when Dr. Stine was just formulating his project for fundamental research. I came with the blessing of Elmer Kraemer, never having been interviewed. They just offered me the job.

FERGUSON: According to what you wrote before, you were offered jobs by Du Pont and Eastman Kodak, and you were also negotiating with Swarthmore. What tipped the choice to Du Pont?

NICHOLS: Elmer Kraemer was there and I would have the centrifuge. At Swarthmore I would have been professor of colloid chemistry. I hoped that I would get an ultracentrifuge, but I don't think there was anything immediately offered.

FERGUSON: Was an ultracentrifuge a pretty key apparatus for colloid chemistry studies at that time?

NICHOLS: It was working out very well.

FERGUSON: Kraemer's field was colloid chemistry, wasn't it?

NICHOLS: Yes. He was excellent. He had probably the best command of the literature of anyone in the field. He wrote a chapter in H. S. Taylor's treatise on physical chemistry (8). He gave a course in colloid chemistry down at Delaware, of which I have a manuscript. Then he got over into the biochemical field when he left Du Pont. He was associated with the Biochemical Research Foundation that had their laboratories down at Newark, by the University. Then he actually went back to Sweden for some work just before the war and had to leave precipitously.

FERGUSON: You were in Kraemer's group from about 1927 to about 1938, when he left.

NICHOLS: I think somewhere along the line I got over to Dr. Patterson's group.

FERGUSON: The organization charts I looked at, which were certainly not complete, listed you in Patterson's group in January or February of 1939. It may have been earlier than that.

NICHOLS: It's possible it may have been that late.

FERGUSON: I think you expected to form a physics group.

NICHOLS: No, that was later. That was during the war. Dr. Arthur W. Kenney was head of the physics group. When he went up to Boston or to MIT for a war assignment I was asked to take over. That was my physics assignment.

FERGUSON: Before that there was a Taylor who had a physical group back in the early 1930s. Why weren't you in his group?

NICHOLS: I don't recall.

FERGUSON: There was a G. B. Taylor, a physical chemist from Princeton, as early as 1925.

NICHOLS: Yes.

FERGUSON: He seemed to run a small physical group in 1925, continuing on for some years.

NICHOLS: Yes, he was there when I got there. He also spent a summer out at Madison. I met him there first.

FERGUSON: Was this Guy B. Taylor?

NICHOLS: Guy Taylor. I thought that he mostly worked by himself. Maybe not.

FERGUSON: At most he had one or two people working with him.

NICHOLS: He could have had the Rumanian, Victor Cofman. He was a real character. When I was in Sweden, somebody talked about this curious man at University College who had these wild ideas about curved space for colloids. When I heard that this candidate, who was then out at Penn State, was coming to work for Du Pont, I assumed it must be Victor Cofman. I don't think I even knew he was in this country.

FERGUSON: How long were you directly involved with ultracentrifuge work?

NICHOLS: From the time I got there to roughly 1940.

FERGUSON: Did you have assistants to run it for you after you got it started?

NICHOLS: Yes. Emerson Bailey. The work went down fairly fast after around 1940. There were special things like developing instrumentation. We had the idea that we ought to be able to get rid of photographic plates, but there had to be developments in the photocell.

FERGUSON: You were looking for improvements in the ultracentrifuge detection system?

NICHOLS: Yes, to shorten the times. Bailey had that assignment. During the 1930s, we were putting order in the research on the hiding properties of the white pigments. We were measuring the physical properties of white pigments of varying refractive index. We finally developed a 'master' curve from which we could get useful data. The optical distribution curve contained implicitly the variation of extinction coefficient with particale size. Bailey developed what he called a teeter-totter board (actually a product integrator). It was a device with which the data obtained from the experimental runs could, by manipulation of 10 segments of the curve, balance a one hundred gram weight-average extinction coefficient, giving the true weight distribution curve of the sample.

FERGUSON: You were doing this graphically?

NICHOLS: Well, this was actually by brute force at the start. Later we were able to get the data pretty fast from the spectral transmission (9).

FERGUSON: This was using visible light?

NICHOLS: Yes, 0.4 to 2.0 microns; the visible and near infrared. During a good part of the 1930s the TiO_2 development was in progress. We had quite a bit to do with that.

FERGUSON: I infer that during this period you were not really much directly involved in polymer work?

NICHOLS: That's right. The real involvement came later, as far as I was concerned. The cellulose work was done mostly by Dr. William D. Lansing.

[END OF TAPE, SIDE 2]

INTERVIEWEE: James Burton Nichols

INTERVIEWER: Raymond C. Ferguson

LOCATION: Bellevue Manor, Wilmington, Delaware

DATE: 16 January 1986

FERGUSON: In our earlier session two days ago, we had just begun to discuss your contributions to polymer science after your arrival at Du Pont. What work stands out in your mind with respect to your particular contributions to polymer science?

NICHOLS: If you consider cellulosic materials and derivatives as natural polymers, my work with the ultracentrifuge complemented Lansing's original work. We set up relationships between viscosity and molecular weight so that they could have a quick method of characterizing their materials.

My work on the cellulosics started in the 1930s. Cellulose and cellophane were difficult to dissolve. Lansing had worked out an apparatus for dissolving cellulose in cuprammonium solvent under nitrogen to exclude oxygen. He was able to get very good reproducible results. I continued his work, mostly with the oil-turbine ultracentrifuge which was set up in 1935. There we got a somewhat better idea of the uniformity of the material, whether it had a narrow or broad distribution of molecular weight.

I went from the centrifuge work to general characterization. To follow that line along, I visited the various plants producing cellulosic products such as cellophane, rayon and acetate, to get an idea of their problems.

FERGUSON: At the time you came, Du Pont was already in the polymers business because they'd acquired Fairfield Rubber Company in 1916.

NICHOLS: And the French technology for cellophane in 1923.

FERGUSON: Were these plants you were visiting acquired?

NICHOLS: No. Du Pont built them. They just got the technology from the French. The Tonawonda plant outside of Buffalo was the Du Pont Rayon Company. It was, I think, a subsidiary of the company for a while. There I met the pioneers, especially of cellophane. I got into the cellophane part more than the rayon.

I knew most of the people at Buffalo in supervisory positions.

FERGUSON: Was this somebody like William Hale Charch?

NICHOLS: Yes, Hale Charch and Preston Hoff.

NICHOLS: Also, the brother of our Bradshaw, Henry.

FERGUSON: Henry was at Buffalo?

NICHOLS: Yes. There were a number of people I knew. I worked into the program on broader characterization of cellophane, and spent a couple of years working on dry-cast cellophane. By supporting the polymer on a drum and subjecting it to heat and rapid airflow, I obtained a product with equal strengths in the two dimensions. There was not the strong machine direction and weak transverse direction that the ordinary cellophane had.

FERGUSON: This was the basis of your two patents (10). Explain dry-casting. Is it similar to melt casting?

NICHOLS: No, it just decomposed the viscose back to cellulose. Viscose was cellulose xanthate. That was the compound that was made in the preparation. It was spread in a thin layer on a uniform surface and then subjected to heat. You had to have a considerable flow of hot air going over it, so that the surface would set quickly, and not contract to give a hazy surface. Under those conditions I got relatively clear film. It wasn't brilliant, but you could read through it, all right.

FERGUSON: Was there some drawing involved after it cooled?

NICHOLS: No.

FERGUSON: It was just cast and used that way.

NICHOLS: The strength went up from about two to as high as fifteen.

FERGUSON: Pounds per square inch or was some other tensile test used?

NICHOLS: It was a test that was used. You cut little samples out and tore them in the Instron tensile machine.

FERGUSON: After your work in the Chemical Department here, was this work continued on up at Buffalo?

NICHOLS: That's right. They took it through a small semiworks to demonstrate the problems. Two things stopped it. The second world war, and W. Hale Charch.

FERGUSON: You knew Hale Charch then?

NICHOLS: Oh, very well.

FERGUSON: As I recall, he had basic patents on waterproofing cellophane (11), which sort of made the cigarette packaging industry.

NICHOLS: Yes, he was the fair-haired boy up there.

FERGUSON: That was before or after you worked with him?

NICHOLS: It was during that time.

FERGUSON: He stopped your development?

NICHOLS: He didn't see much prospect for it. It was tricky, and I think he saw the synthetic polymers coming along. Toward the start of the war they started to get some synthetic polymer films. I'm not sure just where that came in. Anyway, it was easy to get perfectly clear, high-strength synthetic polymer films when they finally were developed.

FERGUSON: Was ICI into polyethylene work that early?

NICHOLS: They must have started before the war. ICI developed the high-pressure polymerization method.

FERGUSON: High pressure polyethylene?

NICHOLS: Yes. I'm sure that was before the war.

FERGUSON: Developed in England, I assume.

NICHOLS: Yes.

FERGUSON: We were talking about Hale Charch. You said he was a fair-haired boy. I was under the impression he was highly regarded and was a very competent man.

NICHOLS: Oh, yes.

FERGUSON: My wife worked for him in Textile Fibers Pioneering here at the Experimental Station. She and everybody I knew there was very impressed with him.

NICHOLS: Yes. He took care of his people. He would stand up for them under most any circumstance and give them a fair shake. During the early days he had sort of turned around cellophane and made it an all-purpose material. He, at one time, was disappearing from Buffalo around Thursday evening and getting back Tuesday morning. They traced what he was doing. He was going down to Mexico and visiting a senorita. [laughter] He was colorful.

FERGUSON: It seems to me his wife was not a Mexican senorita though.

NICHOLS: I don't think he was married at that time.

FERGUSON: I think he married a German lady.

NICHOLS: Could well be. I probably met her.

FERGUSON: I think I met Mrs. Charch once. He didn't come to the parties I went to, so I never really got to know him.

NICHOLS: He had a nice house out on Route 100. Unfortunately it burned. Didn't his wife die?

FERGUSON: I don't know. You mean after he died or before?

NICHOLS: Before, I thought.

FERGUSON: Did you work with him when he got to the Experimental Station?

NICHOLS: No. I was into other things. I had some contact with him, but not as an associate. Then I got into the broader characterization of polymers as they started to develop. I had to set up osmotic pressure. The neoprene work came along too. I was very closely associated with Walt [Walter E.] Mochel in his work.

FERGUSON: Was Walt synthesizing or characterizing the neoprene, or both?

NICHOLS: I think he was doing both.

FERGUSON: Membrane osmometry was one of the things I knew you were involved in. It was a significant development and a contribution to polymer science?

NICHOLS: Yes. When the work blossomed, we couldn't rely on one or two cells. We had a big aquarium that held close to a thousand gallons -- that may be an overestimate. There must have been twenty or thirty units, which kept us rather busy. These were simple osmometers -- a tube with a membrane on the bottom. We measured the height of the column.

FERGUSON: With a graduated capillary?

NICHOLS: No. We had a movable measuring instrument.

FERGUSON: Oh, a cathetometer.

NICHOLS: Cathetometer. We practically wore it out, working it up and down. [laughter]

FERGUSON: Bill Remington tried to pass an old cathetometer off on me when I first came here. I said, "What's it for? Who would want it?" [laughter] From where did the development of the membrane osmometry come?

NICHOLS: Oh, that went a long ways back.

FERGUSON: It wasn't a new invention.

NICHOLS: No, it was just a means of simplifying matters. Harold Spurlin of Hercules did work on osmometry and some of the original work was done in Denmark by Sörensen back in the 1920s.

FERGUSON: Did you just pick up information from the literature, or was that a part of a physical chemistry course in your graduate school days.

NICHOLS: No, we didn't have anything like that in graduate school. We needed the number average [molecular weight] from the osmometer, and the weight average from the centrifuge or later from the light scattering technique. I was into the latter pretty deeply. In fact, with Freddie Baum and Henry Aughey as designers and builders, we probably had the best equipment in the country for light scattering. Our equipment is now at the Smithsonian Institute.

FERGUSON: Didn't Peter Debye really start the light scattering for polymers?

NICHOLS: Yes. I knew that in the Rayleigh scattering equation there was this volume term -- the square of the volume -- but he used that as a starting point to develop a good light scattering method.

FERGUSON: He was at Cornell by this time?

NICHOLS: He was at Cornell. He came over just before the war, maybe in 1938, when he gave the Baker lectures there. In the war effort he was asked to set up a quick characterization for the synthetic rubbers that were being developed. His apparatus was fairly crude, but it did the job. I went back and forth frequently. Not more than once a month, but I went to Cornell quite a bit. They built one of their units for us, which was crude by Henry's standards.

FERGUSON: Henry Aughey's standards?

NICHOLS: Yes, and Freddie Baum's, especially the optical part. Baum and Aughey really developed it into a fine piece of equipment.

FERGUSON: You did angular and concentration dependence extrapolations to zero?

NICHOLS: Yes. Debye and Mark were closely associated on this problem. Marks's people, especially Paul Doty, developed an instrument. It may have been the same as Debye's; I don't know. Anyway, I gave him a sample of cellulose acetate, of which I had a good supply, and which I had characterized pretty well by the ultracentrifuge and the osmotic pressure. I knew the number average was around thirty-eight thousand, and the weight average should be at least in the seventies. When they got their first results at Brooklyn Poly they got only about forty thousand on the sample. I knew something was wrong. Debye looked at these results and said, "Oh, you have to take into account the angular dependence. You have to extrapolate to zero angle." That brought the results into agreement.

FERGUSON: When I got acquainted with light scattering the Zimm plots were used.

NICHOLS: Yes. [Bruno] Zimm was also from Brooklyn Polytech.

FERGUSON: Was Zimm at Brooklyn Poly with Paul Doty?

NICHOLS: Yes. They had quite a crew there. The Zimm plot was a plot both of angle and concentration. It was a good net.

FERGUSON: Yes. You had sort of a distorted grid.

NICHOLS: You had a double extrapolation.

FERGUSON: Of concentration and angles?

NICHOLS: That's right, concentration and angles. You had both things to contend with. Flory and others had demonstrated that a single concentration didn't mean too much.

FERGUSON: Yes. You probably used solution viscosity measurements, at least for a crude characterization of whether you'd made a polymer or not. Did you have charge of the viscosity work?

NICHOLS: Yes. We had a big tank which would hold a number of viscometers.

FERGUSON: Twenty gallon tank or something like that?

NICHOLS: It was probably bigger than that, could have been fifty.

FERGUSON: How big a crew did you have doing molecular weights 1930s?

NICHOLS: Well, Beverly Price was my assistant.

FERGUSON: Beverly Price didn't come that early, did she?

NICHOLS: She came in 1945, just after the war. Emerson Bailey was still there, I think. He left somewhere in that range and went first to Franklin Institute and then out to Denver to the Martin Marietta plant. He was killed when he started to come out onto a main road and a truck without brakes hit him.

FERGUSON: Emerson Bailey worked with you from almost from the time you came?

NICHOLS: A lot of the pigment work was done with him. We developed a master curve so that we could get out data for any white pigment of known refractive index.

FERGUSON: Did you publish that?

NICHOLS: Yes, there was one paper in Industrial and Engineering Chemistry (9). I think it's under his name only. I don't know what year it was.

FERGUSON: Was there more you wanted to say about the molecular weight methods that you used or developed?

NICHOLS: That was roughly it. The light scattering work carried on for some years.

FERGUSON: It was still going on in the 1950s when I arrived and thereafter, I think.

NICHOLS: I had that. During the war I was interim head of the little physics division. The exciting development of all these instruments was during that time. Henry Aughey and Freddie Baum developed a very fine Raman apparatus. We'd call it crude now. They did this with consultation with Professor David H. Rank at

Penn State. He was a good infrared man.

FERGUSON: Who developed this at Du Pont? Or was this something you purchased?

NICHOLS: No, this was Aughey and Baum, because we needed some Raman spectra. On the ordinary infrared, that was Joe Downing. He went out to Michigan and was there for, say, six months while an instrument was being built for him.

FERGUSON: Who was at Michigan then, doing that?

NICHOLS: It was Professor H. M. Randall. We had it built there. It was installed in the "dungeon," which had been an underground rifle range.

FERGUSON: Oh, the ballistics fort.

NICHOLS: It was 40 building. During the war, we found the method was too slow. It was just an energy record, so it was photographic. It was a spark-printed record. There would be, say, a forty-five degree slope as the background and you'd have to pick off the location of the bands. It was much too slow for the work on the fluorides during the war. This was on the atomic energy project.

FERGUSON: You were using infrared to analyze uranium hexafluoride and that sort of thing?

NICHOLS: Yes. Joe Downing developed a crude compensator. It subtracted the background.

FERGUSON: Was this double-beam IR, or modulated?

NICHOLS: I think it was double beam. Brown Instrument got in on it with him and they had a pretty satisfactory instrument for doing gas samples. The detector was rather poor. I've forgotten what it was.

FERGUSON: I think they were thermocouples since they didn't have lead selenide then.

NICHOLS: Thermocouples, yes. Then we got word through Professor

August H. Pfund of Johns Hopkins that there was a development out at General Motors Laboratory near Detroit. We had to get clearance from the Manhattan Project, General Groves or General Nichols, to visit the laboratory and see it. This was the thermistor, which gave a much greater sensitivity. So that was installed with the Brown spectrometer. That helped a lot.

FERGUSON: This infrared spectrometer was a laboratory-built predecessor of the commercial spectrometers?

NICHOLS: Yes.

FERGUSON: You got a Perkin-Elmer model 12 somewhere in there. Was that after the war?

NICHOLS: Yes, we worked with Perkin-Elmer later. We also worked with Perkin-Elmer on the development of the x-ray spectrometer. Up until that time it had to be photographic. We wanted to put it on a chart and get a nice clean record. We worked with one of the men there. Henry and Freddie Baum rigged up an old quadrant so they could scan -- go through the angles that they wanted.

FERGUSON: Did they use a photodensitometer to record the spectrum?

NICHOLS: Yes, with a photomultiplier for greater sensitivity. There were pretty sharp bands. It was a relief to me, to be able to pick off peaks from a chart rather than from a photographic record.

FERGUSON: Was this x-ray emission for elemental analysis?

NICHOLS: No. This was x-ray diffraction. We were working with samples from metals research at Newport. These were new metals that they were working with for various purposes, especially pure silicon. There were a number of those alloys.

FERGUSON: Was this x-ray diffraction for crystal structure and identification?

NICHOLS: Yes. We didn't get into the more esoteric forms which came later.

FERGUSON: Did you get involved in x-ray fluorescence or elemental analysis?

NICHOLS: I did not. That came later. I believe that O. E. Schupp had that.

FERGUSON: You had an emission spectrograph at the Experimental Station when I arrived. Was that used during the war or did that come along later?

NICHOLS: That was there for a long time. That was Robert Berndt. When Bill [William D.] Phillips came, we got into NMR.

FERGUSON: That would have been about 1954.

NICHOLS: I had nothing to do with that.

FERGUSON: You said infrared was gotten initially for the Manhattan Project work?

NICHOLS: Well, we didn't get it originally for the war work. We got it for characterizing the various organic messes that the chemists made.

FERGUSON: When did this work start then?

NICHOLS: Downing came around 1938.

FERGUSON: Let's switch to the neoprene development, because that was the first major polymeric product to come out of Carothers' research.

NICHOLS: That was the first ultracentrifuge work we did on synthetic polymers. We were studying the emulsions. We did quite a bit of emulsion work. That recalls that we also did quite a bit of work on the emulsions they prepared at Buffalo. That was for delustering rayon. They called them delusterants, and they were put on from an emulsion. We had to compare emulsions and the properties they gave. The first samples we had of neoprene or Duprene, as it was first called, were in the emulsion form. They gave a tremendously uniform sedimentation in the centrifuge.

FERGUSON: As I recall the, micelle sizes of polymerization emulsions were usually uniform.

NICHOLS: Unusually uniform for an unfractionated material.

FERGUSON: This was an aqueous emulsion?

NICHOLS: Yes, that was our first emulsion polymer.

FERGUSON: Arnold Collins was the inventor on the patent?

NICHOLS: I don't think Carothers was on the patent. Arnold Collins was at the Station for a few years.

FERGUSON: He then went to Orchem?

NICHOLS: He went over to Orchem.

FERGUSON: Arnold told me at one time about his discovery. He was very modest about the whole thing. It sounded as if his ideas and his instructions had come from Carothers.

NICHOLS: Well, it's quite possible that they had fruitful discussions, because Carothers would have known of Nieuwland's work out at Notre Dame.

FERGUSON: Father Nieuwland was actually at the Chemical Department once, wasn't he?.

NICHOLS: He was a consultant, wasn't he?

[END OF TAPE, SIDE 3]

FERGUSON: Yes, Father Nieuwland was a consultant. But I saw him listed at one time on the chemical department roll, not designated as a consultant. It's possible he might have spent a summer there.

NICHOLS: Could be.

FERGUSON: It was about 1928, I recall.

NICHOLS: It would have to be in that area.

FERGUSON: His field was acetylene chemistry, wasn't it?

NICHOLS: Yes.

FERGUSON: Did you have any personal contact with Nieuwland?

NICHOLS: No, except one or two explosions they had.

FERGUSON: At the Experimental Station?

NICHOLS: Yes. From the divinyl acetylene work. DVA was a bad actor. I recall the divinyl acetylene polymer would pyrolyze spontaneously. If you had any copper around it would go off in a hurry.

FERGUSON: Monovinylacetylene is extremely hazardous, as well.

NICHOLS: Yes. DVA made a gummy mess. There was one explosion, probably with neoprene materials, in a hood. When it was through, the duct, instead of being rectangular, was round.

FERGUSON: This was with Collins or somebody that was working with chloroprene synthesis?

NICHOLS: I think so.

FERGUSON: Eventually the Louisville plant blew up.

NICHOLS: It also blew up their big semi-works over across the river [at Deepwater, NJ].

FERGUSON: Oh. I remember Nick Carter's retirement party, and they were showing pictures of his accomplishments. One was of a building in flames. It was an in-house joke. Everybody except the young people knew what this was about. [laughter] That actually did happen at Chambers Works then?

NICHOLS: Yes. It was a sizeable building that went up. They traced it, I think, to a bit of copper in one of the lines.

FERGUSON: You mentioned in your Smith interview that Svedberg and the Swedes were involved in neoprene during and after World War II.

NICHOLS: Yes, because they were desperate for rubber-like material. There was enough information in the patent literature that Svedberg felt it was their best bet. They had some minor explosions. I never dug into that to see whether there was much damage.

FERGUSON: Some of the people at Jackson Lab in Chambers Works were quite resentful that we had given neoprene technology to the Russians during World War II. Were you aware of that?

NICHOLS: I didn't know that.

FERGUSON: I'm not sure that they're competitors with us in any way. The big competitors are the Germans and the Japanese. Let's get back to the development of neoprene.

NICHOLS: We had much characterization, as I had mentioned before, in Walt Mochel's project.

FERGUSON: I thought Mochel came later, about 1937 or 1938.

NICHOLS: It would be in that range.

FERGUSON: You co-authored papers with Mochel and Mighton on molecular weight distribution of neoprene GN (12).

NICHOLS: Yes. They were comparing different forms.

FERGUSON: You did both solution viscosity and osmotic pressure?

NICHOLS: Osmotic pressure and probably light scattering later. We did a lot of light scattering work on neoprene and other rubbers that were made later. Adiprene was one. There was a three-component one.

FERGUSON: Are you thinking of the ethylene-propene-diene rubber? It's a hydrocarbon polymer called Nordel.

NICHOLS: Yes, we did a good amount of work on that.

FERGUSON: Was this done by light scattering?

NICHOLS: Yes, mostly by light scattering.

FERGUSON: Beverly Price was doing the light scattering for you. They had a light scattering setup over at Jackson Lab when I came, but for some reason, no one was using it.

NICHOLS: I don't recall that.

FERGUSON: When did the gel permeation chromatography come around?

NICHOLS: That was in the late 1960s. I never got into that. Don Bly did that. He came somewhere in that time.

FERGUSON: Don came to the Central Research Department in the late sixties. Joe Downing actually ended his career in the GPC lab.

NICHOLS: Bly had to be there at least a year before I retired, which would make it around 1966.

FERGUSON: That's right. Don came from Textile Fibers shortly after I arrived in 1964. He came into Patterson's group about 1965, and brought the gel permeation chromatography with him. Gas phase chromatography, too?

NICHOLS: No.

FERGUSON: You, or at least the people working for you, did some infrared characterization of polychloroprene. John Maynard worked with Doris [Huck] Hahn.

NICHOLS: Yeah, that's right.

FERGUSON: She was in the infrared lab and did some work with Maynard on the structure of neoprene.

NICHOLS: I guess it was about 1953 when I joined the analytical division. I had charge of most of the instruments. I got the infrared laboratory, and ultraviolet, with Ellen Wallace and Charlie Matthews. I don't think I had the mass spec. I think Bill Taylor may have had that. That went through sort of a low period, then it came up, and it's a prime mover now.

FERGUSON: You mean the mass spec work?

NICHOLS: Yes. When these instruments started to become computerized, that took all the drudgery away.

FERGUSON: Yes, it did.

NICHOLS: I had NMR with Harlan Foster. Bill Phillips started that work when he came. When he moved on to other things it came over to analytical, and Harlan Foster took it.

FERGUSON: Bill Phillips and the people of that time were real pioneers, and, as you well know, certainly revolutionized the characterization of organic compounds.

NICHOLS: There were many years that our company's research was far ahead of any other industrial place as innovators. Then other people learned how to innovate.

FERGUSON: After a certain point neoprene was transferred to the rubber division of the Organic Chemicals Department. Who were the key people over there, or did you have any association with them?

NICHOLS: I didn't have much, but I knew most of them.

FERGUSON: How about Bill [Oliver M.] Hayden?

NICHOLS: I've known him now for something like sixty years, practically from the time that I came. He's ninety-two now, I think. He had a mild stroke but he came out of that.

FERGUSON: He was, as I recall, a rubber chemist, natural rubber?

NICHOLS: Yes, he started out that way.

FERGUSON: He ended up as technical director?

NICHOLS: That's right.

FERGUSON: Was Herman Schroeder involved in neoprene?

NICHOLS: Yes, I think he was involved after he left the Station, probably from downtown.

FERGUSON: I'm not sure he ever went downtown then, unless that was on his way to Jackson Laboratory. He was only in the Chemical Department about two years.

NICHOLS: Some were sales people. I probably knew a dozen or so of the people engaged in the neoprene work.

FERGUSON: Tony Carter?

NICHOLS: No, "Nick" Carter.

FERGUSON: It was "Nick" Carter that I was thinking of.

NICHOLS: He was a bright boy.

FERGUSON: He became Director of Research for Elastomer Chemicals Department when it was split out from Organic Chemicals. Art Stevenson?

NICHOLS: I did not know Stevenson as well. There was "Cabby" [Carl] Bartle and Al Northam. There was a whole crew of them.

FERGUSON: I want to get on to nylon.

NICHOLS: Well, the oil-turbine ultracentrifuge was, in a sense, bought to help out with the nylon work. Actually, the solvents were so esoteric that we never did very much with it. We could handle them, but if we used a mixed solvent, that introduced troubles, because then the refractive index difference was really unknown. You didn't know, in these active solvents, how much was attached to the polymer and how that changed the overall picture. That was especially true of one of the last polymers that went through. That was Kevlar, the one that is very strong.

FERGUSON: Oh, you're thinking of the linear polyaramide.
NICHOLS: Yes. With a simple measurement we got a refractive index difference of around 0.4, which was practically impossible. The solvents we had to use were sulfuric acid, methyl sulfuric acid and so forth. We had an unknown solvent and an unknown polymer. Essentially that was unsolvable.

FERGUSON: Let's go back to the polyamide research that Carothers was doing. Paul J. Flory came. Wasn't this when he developed his condensation polymerization theory?

NICHOLS: Yes.

FERGUSON: He checked the degree of polymerization and the distribution experimentally, didn't he? Were you involved in the molecular weight measurements?

NICHOLS: For some of the simpler materials he worked with, there was an end-group titration method. We did a little of that. The first sizeable polymer was with hydroxydecanoic acid. Kraemer and Lansing got out a paper on that (13).

FERGUSON: Was this with hexamethylene diamine?

NICHOLS: No, hydroxydecanoic acid.

FERGUSON: Was this an amide polymer or a polyester?

NICHOLS: It was a polyester, because Carothers was working on the esters first. He didn't find anything interesting.

FERGUSON: Would I be right in saying that was probably unfortunate, because they just didn't happen to draw fibers and post-treat them? Polyesters really are nothing unless you spin them and draw them properly, is that right?

NICHOLS: Yes. At least with the compounds that he made, he found nothing interesting.

FERGUSON: You mention in your Smith interview that you and Carothers were in a luncheon group at the Experimental Station.

NICHOLS: Yes, that went on for many years.

FERGUSON: Was there a lot of discussion about the work, or was this more social?

NICHOLS: It was mostly social. Well, the composition was something like this: Guy Taylor and his wife-to-be, who was the librarian; Carothers; Henry Aughey; perhaps Freddie Baum. Lee Williams and me. Beverly Price came in later. Helen Carothers, who was Helen Sweetman at the time, came in. She was daughter of the comptroller of the station, whatever his title was. There were some others. We'd each buy food for a month. We could have it delivered to the Station. This was a time when Hearne's Market was in full bloom and would deliver anywhere.

FERGUSON: I gather that you knew Carothers well.

NICHOLS: Oh, yes.

FERGUSON: Did you have serious discussion or interaction with him on the theoretical side?

NICHOLS: Not very much. No.

FERGUSON: You said you first heard about Staudinger's work when you got to Du Pont. Was this through Carothers?

NICHOLS: Yes.

FERGUSON: Were Carothers and Staudinger in correspondence?

NICHOLS: They might have well have been. In early years he had a house out on Kennett Pike with three other men.

FERGUSON: You did cover that in the Smith interview (1). May we include the transcript of the Smith interview in your file at BCHOC?

NICHOLS: Oh, sure. I remember that I have a right to use it.

FERGUSON: I cut you off on these stories about Carothers because I wanted to get to Paul Flory. Did you interact quite a bit with Flory?

NICHOLS: Yes. He was such a prepossessing young man too. He also had a nice wife.

FERGUSON: He was impressive to you in those days?

NICHOLS: Oh, yes. That reminds me that, to get better viscosity data on nylon, Guy B. Taylor, probably with interaction with Paul, developed a high-temperature viscosity method for molten nylon.

FERGUSON: A melt viscosity method?

NICHOLS: Yes.

FERGUSON: On the bulk polymer?

NICHOLS: Yes. That gave Paul some data that were useful. I'd forgotten about that melt viscometer.

FERGUSON: It's still called that and it's one of the standard tests in the plastics industry. There are standard ASTM melt index tests. They're not really very good, but they're so well accepted in the trade that you have to run them.

NICHOLS: It gives an indication.

FERGUSON: It's not a good molecular weight method in the absolute sense, but it's a useful, quick method. Flory did some work on thermal stability of various polyamides, 6-6; 6-10; and 6-12; and so on. [In a nylon the first digit is for the number of chain carbon atoms in the diamine moiety, the second for the diacid.]

NICHOLS: I don't remember that.

FERGUSON: This was a pyrolysis method. I had occasion to look back at his reports from this period.

NICHOLS: He did it the hard way. At that point, it [thermal analysis] was pretty crude.

FERGUSON: It was very crude. He pyrolyzed the polyamides and isolated some of the products, but wasn't able to identify much of anything except carbon dioxide and ammonia. The reason for mentioning it is to draw out your feelings. It turns out that nylon 6-6, from a thermal stability point of view, was not the best choice. Where you aware of that?

NICHOLS: No. Was nylon 6 better?

FERGUSON: 6 was later developed by somebody else. It also had thermal degradation problems. I brought this up because I read some reports of yours that dealt with color in the 6-6 polymer.

NICHOLS: Oh, yes. I did some interim work there. I was making some crude dye experiments and development of color on heating. It was sort of a miscellaneous group of things.

FERGUSON: At that time Buffalo was producing nylon flake for spinning. Was that what was going on? There were problems with quality control, weren't there?

NICHOLS: I worked through the Carothers lab [at the Experimental Station]. I never went outside of the lab for that color study.

FERGUSON: Was this a dyeing study?

NICHOLS: No.

FERGUSON: The hypotheses was that there were monomer impurities or something was affecting the quality of the product. You worked maybe six or eight months on that?

NICHOLS: Not more than that. This was an interim thing.

FERGUSON: What about your reports? I think you were writing either bi-weekly or monthly reports that were quite detailed.

NICHOLS: We started out with a weekly summary. In the late twenties it got around to monthly reports. Eventually it was a three-month report.

FERGUSON: Were these the only reports you wrote?

NICHOLS: Except individual letters. I did much work for other people, outside of the Station.

FERGUSON: These would be internal memoranda that reported the specific studies?

NICHOLS: Yes. I did a lot of work for the Newark, New Jersey plant on the colored pigments.

FERGUSON: This writing of a weekly or monthly report implied rather close supervision. Was that the way it was in the early days under Tanberg?

NICHOLS: Yes.

FERGUSON: Did this change later on?

NICHOLS: The weekly reports were dropped by the time that we cut back from the Saturday morning work. Saturday morning was probably the time when we collected our thoughts for the week.

FERGUSON: I guess this was the period [1925] in which Stine was setting up fundamental research.

NICHOLS: Yes.

FERGUSON: In your earlier interview you said that that was cut back quite significantly during the Depression.

NICHOLS: Yes, but they kept their full staff all through the Depression. They got maybe six hundred dollar projects, and some of them just literature projects, but they kept us going.

FERGUSON: Did they cut salaries or anything?

NICHOLS: The first one was a work cut, but the actual salary rate was not cut. The second one was a straight ten percent. They never went back to the Saturday work. Well, informally, we worked on Saturdays during the war.

FERGUSON: "Ding" Bell, who eventually became a Director of Research in the Film Department, remarked to me that the World

War II period at Du Pont was the most exciting of his life. He worked something like twelve to fourteen hours a day, six or seven days a week, and loved it. Did you have that kind of pressure at the Experimental Station?

NICHOLS: It was usual for quite a crew to come in at night and work, especially in our group, when Henry Aughey and a couple of the others were in the midst of some development. They would come in most any night. During the war, we had this spectroscopic analysis of the impurities in uranium samples coming over from Chambers Works. Henry had discussed means of increasing the sensitivity with the Bureau of Standards. These levels had never been attempted by spectroscopy before. They might be only a thousandth of what were common. On occasion Sam Lenher would come over and look over the results.

FERGUSON: Was Sam Lenher in the Manhattan Project?

NICHOLS: He was associated with it. He was still across the river [at Chambers Works] at that time. The Chambers Works was very active in the Manhattan Project at that time, too.

FERGUSON: We could discuss the state of polymer science and technology at Du Pont when you arrived, and the growth of the polymer business at Du Pont. I would like to touch a bit on the Du Pont staff and consultants you knew. We could talk a bit about Du Pont research and research management and your personal views on the current state of polymer science and professionalism. Which of these would you like to tackle first, or any of them?

NICHOLS: I think consultants that I knew might be good to start with.

FERGUSON: H. S. Taylor was a consultant. Was this the physical chemist from Princeton?

NICHOLS: Yes.

FERGUSON: Did you know him?

NICHOLS: I knew him. I didn't have much contact with him. Elmer O. Kraemer had much more contact. In fact, Kraemer wrote a considerable chapter in Taylor's 2nd. edition of a two-volume treatise on physical chemistry (8).

FERGUSON: Do you want to start off with the ones you think of?

NICHOLS: Well, one of the first that they had was August Pfund, from Johns Hopkins. He was an infrared man and general physicist. Kenney and Aughey were on a monthly schedule with him. He always had a fund of stories. He was getting along in years, that is, to us. He was probably in his sixties.

FERGUSON: Wasn't Prof. John D. Strong, the grating man, at Johns Hopkins?

NICHOLS: I don't think he consulted with us, although he made gratings for our Raman spectrometer. It was only Emmet Reid and Dr. Pfund. Dr. Pfund always had a pipe with him. I had very close contact with Fred Wall.

FERGUSON: He was at Illinois?

NICHOLS: Yes, Illinois. He was a physical chemist. Herman Mark came down at least once a month. In fact, if you include the various departments that he consulted for, he was probably down there once a week.

FERGUSON: Was this as early as the mid-1930s?

NICHOLS: I don't think he came over quite that soon. The Company got him a place when he escaped from Austria. He went first to Hawkesbury, I believe, to the paper company up in Canada. Then, the Du Pont arranged to have him come down as a professor at Brooklyn Poly.

FERGUSON: You mean that Du Pont had a hand in getting Mark to this country and getting him established?

NICHOLS: Oh, yes. Sure. Mark had quite a crew there at Brooklyn Poly. They would have weekly Saturday morning seminars. I attended a good many of them.

FERGUSON: You mean you went up to Brooklyn to attend the seminars?

NICHOLS: Yes. There was Isodore Fankuchen, a good X-ray man. Walter Stockmayer was there. He went up to Dartmouth. I think he's still at Dartmouth.

FERGUSON: Right, and is still a Du Pont consultant. There was a man who went out to Michigan. I think he's head of a department out there now.

NICHOLS: Probably you are thinking of Charles Overberger. Bryce Crawford is at Minnesota. We had dealings with him. It was Henry Aughey or Joe Downing. I saw Carl Marvel, not every time, but I always enjoyed talking with him. I saw Roger Adams less often. And a Northwestern man who used to come, an analytical man.

FERGUSON: John Wheeler was a consultant, wasn't he?

NICHOLS: Oh, yes. He was delightful. He was the most unassuming man you can imagine. With all that he accomplished in the esoteric field of the atom, he could just outline a thing in his head. We couldn't follow him. At least I couldn't. He and Debye had somewhat the same characteristics. They could visualize a problem and the possible answer, how to attack it.

FERGUSON: You commented in the Smith interview that Debye had a wonderful ability of exposition, to make it seem simple.

NICHOLS: Yes.

FERGUSON: I consulted with Wheeler on occasion, but it seemed to me a little strange that you had this high-powered physicist as a Du Pont consultant, when most of the work was really chemistry. Did he contribute to your work?

NICHOLS: Oh, yes. During the war especially.

FERGUSON: Look down the list of people that were your associates at Du Pont. Are there are any that you particularly want to mention?

NICHOLS: Theodore Baker was the distillation expert. He stayed on as a consultant for many years after he retired. He died when he was ninety or so. I think he was Norwegian. He could quote from the sagas. He'd made quite a study of the Norse sagas and could read the old Norse. He was a well-rounded man.

FERGUSON: Was it the custom to spend some time with the consultants, and entertain them when they came to town?

NICHOLS: Oh, yes. That was especially true of Marvel, because with his birding knowledge. They just swarmed around him to come, or to stay a day late, so that they could make a birding tour.

FERGUSON: How about Paul Flory?

NICHOLS: Did he ever consult?

FERGUSON: Yes. I think that's part of the story. I'm trying to see if you know anything about it. It was my understanding that Flory left Du Pont within a year after Carothers died, with some dissatisfaction or hard feeling. Were you aware of that?

NICHOLS: There was something in the air. I never knew just what it was. Knowing his subsequent career, I suspect that he didn't feel that he had quite the possibilities that he might have elsewhere.

FERGUSON: I believe he went first to Goodyear out at Akron and then to..

NICHOLS: Goodyear and then Esso.

FERGUSON: Oh, another company?

NICHOLS: I think he went to Esso.

FERGUSON: Then he went to Cornell, then Mellon Institute.

NICHOLS: Yes.

FERGUSON: And then Stanford.

NICHOLS: In Stanford he found a happy home. [laughter]

FERGUSON: Well, I think he did well at Cornell.

NICHOLS: Oh, yes. I'm sure he did.

FERGUSON: I was on the wrong side, or let's say the other side, of a couple of scientific controversies with Flory. I found him rather abrasive. Did you have any contact with professionally, or as a consultant?

NICHOLS: Was he a consultant?

FERGUSON: Oh, yes. He consulted for the Textile Fibers Department. I don't know whether it was while he was still at Cornell or whether he started later.

NICHOLS: I didn't know that.

FERGUSON: He consulted at Textile Fibers Department but, as I recall, he very seldom consulted at Central Research, until the last five years, maybe, when he was pumping Howard Starkweather and other people in our department, for what we knew. I had heard from other people that knew him that he was very gentlemanly and very objective about his own work -- very self-critical.

NICHOLS: I can't tell you. I saw him so infrequently after he left.

FERGUSON: What about Crawford Greenewalt. Would you say he was a competent engineer?

NICHOLS: Oh, yes. Very much so. There were several incidents that might bear repeating. One of his projects at the Station was a little semi-works process for methyl mercaptan or something like that. The gentle breezes would spread the odor around pretty well. When the people working at the Station would go home, their wives would avoid them, because the odor settled in the hair. One Sunday morning this little plant burned down and there was a sigh of relief. [laughter] I think that was the end of the work on mercaptans.

FERGUSON: So he worked as an engineer on process studies for several years before he began moving up to management?

NICHOLS: Yes. He and Dr. Kenney were very close friends. Kenney lived out beyond Hockessin, at Stoney Batter. He had this house on a side hill in the valley there, a very nice place. There was a wood there too. Frequently several of us would go out there on a Saturday and cut wood -- cut down, thin out trees, take out dead wood and so forth. On occasion, Greeny and his wife Margareta and Julian Hill and another man would come out and have a quartet of instrumental music. They'd play out in the open very nicely.

FERGUSON: Greenewalt's wife played?

NICHOLS: Yes. I don't remember what. Julian Hill played the violin. I don't remember the actual composition now.

[END OF TAPE, SIDE 4]

FERGUSON: Is Julian Hill still well?

NICHOLS: As far as I know. He's sort of crippled though, because he had infantile paralysis which left him with one shriveled leg.

FERGUSON: Yes, I remember him.

NICHOLS: But he did skate for a number of years.

FERGUSON: Yes, I used to skate with him over at the Wilmington Skating Club. You did too.

NICHOLS: Yes, sure. He still stayed with the skating club up until about five years ago. He had a bad fall in his back yard and that hurt him some more.

FERGUSON: Well, to do some really big name dropping, did you ever have any contact with Pierre S. du Pont?

NICHOLS: Yes, in several ways. In fact, I knew all three of the brothers, Irénée, Lammot, and Pierre du Pont through entertaining Svedberg. That was the first contact. Irénée took us on a boat trip in his yacht down the Chesapeake, in the 1930s. Somewhere near the Sasafras outlet we pulled aboard a youngster who had got out too far and needed help. So we pulled him in.

FERGUSON: The du Ponts were sufficiently impressed with Svedberg that they wanted to entertain him?

NICHOLS: Yes.

FERGUSON: How many visits did he make?

NICHOLS: Perhaps half a dozen. I think that was the only contact I had with Lammot du Pont. It could have been also that in the very early days, when we had to report to the top management, we made one or two reports on our work as fundamental research projects.

FERGUSON: Paul Arthur told me one time that Irénée du Pont kept close tabs on the Chemical Department research even after he retired as president. He read all the reports. Particularly, he watched the reports pertaining to inorganic pigments and minerals very closely. Maybe he didn't follow your work that closely.

NICHOLS: I don't know. Anyway, when Svedberg was here, we also were invited to Granogue for the fireworks on fourth of July. For a number of years afterwards we got invitations, until they decided they'd had enough of us.

FERGUSON: How about Walter Carpenter, Jr?

NICHOLS: Well, I have to get around to Pierre.

FERGUSON: Oh, yes.

NICHOLS: Of course, Longwood was a place that Svedberg would especially like to visit because of his botanical interests. At the time when the Brandywiners were young, and when his wife was still alive, Mr. du Pont took great interest in the Brandywiners. If the season was right, he'd bring baskets of peaches and set them out for us to munch on. At the end of the final show he would have the whole group, which sometimes ran up to the order of one hundred, to a party in the open space in front of the house. He'd set up a bar, and I don't know how some of the younsters got home. Maybe they were cautious about indulging, but I doubt it. It was a very handsome buffet. So he was very kind to us. But then, now you want Walter Carpenter.

FERGUSON: Yes.

NICHOLS: I had several contacts with him. You see, he was a Cornell graduate and gave X millions to the university, especially for the engineering department. For some years he would invite the Cornell Club of Delaware out to his farm for a picnic. He would come to an annual meeting on occasion. He had considerable interest in the local club, so I knew him through the Cornell club. He was a very good man.

FERGUSON: He was put in charge of Development around 1918. Tom Aston, who was in the Development Department back in the 1960s, wrote a history of the Du Pont Company which was based primarily on what Walter Carpenter's ideas had been, and what they had set up. The interesting thing about that history was he didn't mention Carothers or nylon or neoprene. [laughter] I was horrified!

NICHOLS: Those were too mundane!

FERGUSON: I could never convince Tom that he'd missed something.

NICHOLS: Who was it?

FERGUSON: A fellow named Tom Aston who was head of the Development Department downtown in the late 1960s. I think he was trying to justify the Development Department and some of the modern changes.

This might be a good point to discuss the Du Pont research philosophy and research management. Can you briefly say how that developed and changed over the years from the time you came?

NICHOLS: Well, there were several changes. When I first came there was no general indication as to what one had to work on -- you picked your problem. Somehow I got into pigments. My first years were devoted mainly to white pigments and colored pigments and dyes -- the colloidal dyes, not the substantive ones.

Then the first restriction came during the Depression, when Dr. Bolton took over. He was known for wanting to be able to punch the cash register from the work that was in progress. Under the circumstances, it was not as restrictive as you might have thought it would be, since they kept their whole staff.

Then, of course, during the war, everybody was busy enough so that they didn't have time to think. They just did what they could. After the war under Greenewalt, blue sky research was in order. Then, I guess, there was sort of handwriting on the wall. Things were not going as well on the cash register as they might. I'm not very clear about that. Then the extended Experimental Station developed so as to bring the departments closer together.

FERGUSON: There was a big expansion of the Experimental Station in 1948, I think.

NICHOLS: Yes.

FERGUSON: Was that an increase in research staff or was that just collecting everything into Wilmington?

NICHOLS: Well, that was bringing in the basic research from the other departments, so that there could be more interchange.

FERGUSON: It was my recollection that the operating departments still kept their independent labs, by and large, until almost the 1970s or 1980s.

NICHOLS: Yes, sure.

FERGUSON: Was there a belt tightening after the Greenewalt era or during the Greenewalt era? Let's see, Greenewalt was president until 1962.

NICHOLS: 1962. Was the first expansion of the station in 1951?

FERGUSON: I think it was in 1948. Maybe it was completed later. Buildings like Elastomers were added in 1953. That was a new department then, of course.

NICHOLS: I think that some of the young chemists were branching out to such an extent that there didn't seem to be any real future [for the company] in what they were doing.

FERGUSON: I worked in an operating department in those days. I assure you that that was the opinion of some about Central Research, at least.

NICHOLS: Yes. I think that probably developed to such an extent that they did have some restrictions put on.

FERGUSON: Let's go back to your professional society activities. Early on, you were involved in the Division of High Polymer Physics of the American Physical Society?

NICHOLS: Yes, I was there through its inception and I served as its chairman for a year. At the same time, I was on the steering committee or board, whatever it was, of the Polymer Chemistry Division of the ACS.

FERGUSON: You were a member of the American Physical Society from your Cornell days, or your early Du Pont days?

NICHOLS: No, I joined during the war because so many of my contacts were with physicists.

FERGUSON: Did you say you were in the organizing group of the Division of High Polymer Physics?

NICHOLS: Yes.

FERGUSON: Who was mainly responsible for that?

NICHOLS: Mark was.

FERGUSON: Herman Mark?

NICHOLS: Yes. He was probably the main organizer.

FERGUSON: Did you continue to be active with them after your chairmanship?

NICHOLS: Oh, for some years. Much earlier, I was at the organizing meeting in Evanston of the Electron Microscope Society. That was around 1940. So I knew all the work and the good scientists in that group. I'm not sure whether I was ever a member of the colloid group or not.

FERGUSON: There was a cellulose division, or were cellulose and colloids together at one time?

NICHOLS: I don't remember. Then I was involved in the Gibson Island conferences, which they were called for some years, until it became much bigger.

FERGUSON: Were these the forerunner of the Gordon Research Conferences?

NICHOLS: Yes. These are now the Gordon conferences.

FERGUSON: They started off at Gibson Island. That was where?

NICHOLS: That's in the Chesapeake, just outside of Annapolis. The group, and I don't remember who the main organizer was, wanted a good isolated spot for a series of meetings and seminars during the summer. It turned out that this Gibson Island, which was owned by a group of Baltimore and Annapolis people as a country club, was in bad straits because of the war. They couldn't get to it and that sort of thing. So they agreed to build a conference building away from the main building, where they would still be functioning somewhat. We had week-long conferences there. We had the use of the bar in the main building and that was all. They had two drinks: planter's punch and mint julep. One time that I was down there it was the 4th of July. Along about ten or eleven o'clock we decided it would be a good time to sing a couple of Christmas carols. So, Neil Gordon comes down in his bathrobe, with his eyes flashing and says, "Get thee hence! Get home!" [laughter]

FERGUSON: Who was Gordon?

NICHOLS: Neil Gordon from John Hopkins was the organizer and head of the conference. When we got to the meeting place at nine o'clock next morning he gave us a lecture for about a half hour on etiquette and behavior and so forth. [laughter]

FERGUSON: Sociability was a large part of the Gordon conferences when I was involved.

NICHOLS: Oh, yes. So much of the exchange of ideas happened during those social hours. Work was discussed there which would appear as published articles maybe a year hence.

FERGUSON: Yes. You were also involved in the Society for Applied Spectroscopy?

NICHOLS: Oh, yes. I was. At that particular conference I was at the ASXRED (American Society for X-Ray & Electron Diffraction) meeting because we had a little electron diffraction going with our electron microscope.

FERGUSON: Who did the electron microscope in your days?

NICHOLS: Well, Henry Aughey did this. He got it in working order, but then Carl Willoughby took it over. Henry always had to get Willoughby out of trouble. He was not really mechanically or instrumentally inclined.

FERGUSON: My wife was an electron microscopist in Textile Fibers. This was Angela Wierzbowski. I came to the conclusion that electron microscopists are a very temperamental bunch. [laughter] Is that your feeling?

NICHOLS: It could be. Yes.

FERGUSON: They never trusted each other, among other things.

NICHOLS: Well, there's so many things that could go wrong. So many artifacts.

FERGUSON: Well, I'd say it's a very esoteric business, although it's important.

NICHOLS: Even the size of particles could change if they got too concentrated a beam on them.

FERGUSON: Were you involved with the organization of the Society for Applied Spectroscopy? I know you were very supportive.

NICHOLS: I was just a member.

FERGUSON: I can recall you used to bring your harem with you, Ellen Wallace and Naomi 'Pete' Schlichter and sometimes Kay Looney, to the SAS meetings.

NICHOLS: Oh, yes.

FERGUSON: Was this your way of encouraging them to engage in or be more aware of their profession? What's your feeling about the professionalism, particularly with respect to participation in professional societies and attending meetings, as it was back when you started, and has changed over the years?

NICHOLS: You never know what contacts you'll make. A casual word dropped -- we ran into this problem and it worked out this way -- might just solve a problem for someone. In an instrumental society there are so many possibilities. Of course, if you were deeply involved in organic chemistry, the same thing applies.

FERGUSON: Yes.

NICHOLS: But an outsider going to an organic meeting wouldn't necessarily pick up very much.

FERGUSON: With respect to the Delaware Section of the American Chemical Society, which has been one of the most successful, I've heard reports that they had very good attendance, back in the 1930s, and during and after World War II.

NICHOLS: Things started to go down after the war. In the 1930s, it was really flourishing.

FERGUSON: The local section of the ACS has had its problems in recent years, except for maybe a few really outstanding things, such as the Carothers Award lectures. Do you have a feeling for why that is so?

NICHOLS: Oh, I think that probably it's a matter of so many different ways of spending your time. That is, right after the war they were accustomed to a fairly austere sort of living -- not being able to go off weekends very much, and TV was just coming up. In fact, I forget when the first real sets came. The first TV that I saw was at the World's Fair of 1940. I don't think they became general until around 1955?

FERGUSON: About then.

NICHOLS: That makes quite a bit of difference when you have to weigh a good program against a possible dull lecture. [laughter]

FERGUSON: What is your feeling about the proliferation of specialized societies?

NICHOLS: Well, I think it's probably a step in the right direction, because, in any given year, there's a limited number of people that could be invited to a general meeting who would be able to draw the whole spectrum of chemists.

FERGUSON: Let's turn to a quick rundown of some of your associates at Du Pont that you'd like to mention.

NICHOLS: Well, Sam Lenher would be a good one to start with. He was the son of a prestigious professor at Wisconsin. I had him as a student in a lab where I was the assistant. He was going through, I think, physical chemistry. That was my first contact with him. Then he graduated and we both went over to Europe. He spent some time in London working on a doctorate, and then went to Germany and worked with Max Bodenstein. I met him in Berlin one time. We had dinner together. Then we both came to Du Pont. He very soon figured that he'd better get into more practical things, rather than pure research.

FERGUSON: Was he ambitious in a business sense?

NICHOLS: Yes. He could see that his progress would be much greater that way. On one occasion we would take walks together. It was possible to walk around in Alapocas Woods. There was just a dirt road at that time. There was just one problem that I thought might come up and that would be centrifuging something at a fairly high temperature, say a hundred degrees. He said, "Well, why don't you just go ahead and develop such a thing." Because I didn't have any immediate use for it, I didn't follow through on his advice. Kistiakowski was there too.

FERGUSON: Oh, yes. George Kistiakowsky.

NICHOLS: Just for a summer or something like that.

FERGUSON: Now this was before he went to Harvard?

NICHOLS: Before he went to Harvard.

FERGUSON: So he'd have been coming into this country from Berlin? Didn't he finish at the University of Berlin?

NICHOLS: I don't know what his background was.

FERGUSON: Do you remember where he came from, arriving at du Pont?

NICHOLS: No.

FERGUSON: Did you know him, or work with him?

NICHOLS: No, I just knew him, that was all. I didn't have any real contact with him. He worked with Guy Taylor.

Victor Cofman was a very odd man. I don't know that I mentioned him.

FERGUSON: You mentioned that he had some strange ideas. Where was he from?

NICHOLS: Romania. At one time he was at University College in London with [Frederick G.] Donnan.

FERGUSON: There were two Cofmans. One was Cofman and wasn't the other Donald Coffman?

NICHOLS: Yes. He was from the mid-west.

FERGUSON: He made some important contribution in nylon development, didn't he?

NICHOLS: Yes, I don't remember what it was at the moment.

FERGUSON: It seemed to me he had a patent on the drawing or processing of the fibers.

NICHOLS: Could well be. Of course, Julian Hill was the first one to draw the polymer. Hood Worthington was just the man for instrumentation on this new process of hot melt spinning.

FERGUSON: Was he the one that was involved in the melt rheometer?

NICHOLS: No, that was Guy Taylor.

FERGUSON: Now how about Howard Starkweather, Sr.?

NICHOLS: He was in the Rubber Division.

FERGUSON: He went on to Organic Chemicals, Rubber Chemicals Division.

NICHOLS: I knew him more socially, actually, than scientifically, so I can't make any particular comment.

FERGUSON: He ended up at a fairly high level in Orchem.

NICHOLS: You'd mentioned...

FERGUSON: Oh, Tanberg. I was just going to check with you to see whether the story I'd heard is true. The tale I heard was that Tanberg and Henry du Pont were drinking buddies and that this was the way Tanberg got informal, or behind the scenes, approval for a lot of his ideas in the days that he was director. Did you know that?

NICHOLS: No.

FERGUSON: Do you believe that?

NICHOLS: It could be. He was sort of a staid gentleman, though.

FERGUSON: Tanberg was?

NICHOLS: Yes.

FERGUSON: Well, maybe it was somebody else. I heard that Henry du Pont, who was on the executive committee, would meet every afternoon at the tavern down along the Brandywine.

NICHOLS: Oh, it must have been Hagy's.

FERGUSON: Yes, probably.

NICHOLS: The Brandywiners rehearsed at Brecks mill and the tavern was only a quarter of a mile away, so we'd congregate there afterwards. I remember we were giving the show, "Merry England" and in that they have a masque in which I was an Egyptian dancer. Several of us that were in that got up on the tables and went through a few motions. [laughter] We hadn't had more than one beer, I'm sure.

FERGUSON: Well, I wouldn't have pictured that! You always seemed so very staid and sort of formal to me when I first met you at du Pont. Well, maybe the Tanberg story is not true.

NICHOLS: Tanberg struck fear in the eyes and heads of applicants when he went out to interview them.

FERGUSON: That would be consistent with what I've heard also. Is it true that he was the boss and he did the hiring and firing and ran a pretty tight ship?

NICHOLS: Yes, that's right.

FERGUSON: How about Stine?

NICHOLS: He was the chemical director at that time.

FERGUSON: Was it his idea to do the fundamental research?

NICHOLS: As far as I know it was Stine's. I never heard another name. I knew him in various ways. You haven't mentioned Cole Coolidge. Cole Coolidge, especially, I liked. Some people didn't like him too well, but he seemed to take a special liking to me. I think it was probably his say that gave me the three month trip to England just after the war in 1945. I went through all the ICI laboratories, and university labs and institutions in England. Whoever was the first choice couldn't make it, so I took over. Coolidge was the best friend I had on the steering committee at that point.

FERGUSON: I see.

NICHOLS: I was shocked, of course, as everybody else was when he died. That would have around 1953.

FERGUSON: I don't have any knowledge of that.

NICHOLS: It was close to that.

FERGUSON: Let me ask about Jerry Berchet. Didn't he have some important inventions?

NICHOLS: Well, he did a lot of the early work with Carothers on the polymers. He's mentioned very favorably when that early work is gone over.

FERGUSON: Did he do the early work on Delrin, or the Delrin forerunner?

NICHOLS: I sort of doubt it, because he went into patents. I forget whether it was before the war. It was somewhere in that range.

FERGUSON: I'm curious about that. Both Berchet and Arnold Collins, who had at least had some distinguished accomplishments in synthesis, ended up in the patent division. Was that their desire, or did they get burned out?

NICHOLS: Well, it's hard to say. Perhaps their writing was good enough to show that they had real promise in developing a proper patent procedure. A properly written patent means a lot.

FERGUSON: I hadn't looked at it that way. I think we always sort of looked down on the patent division and information division, or information systems people, and perhaps unfairly. The jobs were not glamorous.

NICHOLS: No, they weren't. It was hard work, because so much of it amounted to literature research, and patent literature especially. It'd be about as dull reading as you could imagine.

FERGUSON: We are back to 1928 as far as these particular people are concerned. I don't know that there are any others.

NICHOLS: Well, there is Dr. Elmer K. Bolton, of course. I had quite close contact with him. Also Dr. Ernest Benger, who came down from Buffalo at the same time as the main expansion took place, when the textile fibers [department] came down.

FERGUSON: Now there was a Benger Lab down at Waynesboro. Is that the man?

NICHOLS: It's the same person. Yes, he started up at Buffalo in rayon. I knew him up there. Whether he actually was down at Waynesboro or not, I don't know. Anyway he was a good man.

FERGUSON: Did he do some outstanding polymer research? I guess since he was in rayon division, he was a polymer man of sorts.

NICHOLS: Well, he was present during the development of Dacron, Orlon and that group. He had a hand in that. I was going to comment on Bolton. It was during the time that I was acting head of the little physics group that we wanted some equipment. It could have been the mass spec. My timing is not very good, but anyway we wanted something that cost about twenty-five thousand, which was a fair amount of money in those days. Bolton remarked, "Well, I'm glad that you're not asking for something around a hundred thousand."

FERGUSON: He could stomach twenty-five thousand but.... Well, of course, he'd have to go up to another level of management to get approval for fifty thousand or a hundred thousand?

NICHOLS: I would think it might well be.

FERGUSON: Where did he go after he left Chemical Department?

NICHOLS: He retired from there as chemical director.

FERGUSON: Then Salzberg came in?

NICHOLS: I'm trying to think. I think there was Salzberg and then McQueen.

FERGUSON: Right, that was the succession after Salzberg.

NICHOLS: After that I've lost track.

FERGUSON: Paul Salzberg was the organic chemist from Illinois, I believe.

NICHOLS: Yes.

FERGUSON: Did he work in polymers at all or was he mostly just in organic chemistry?

NICHOLS: I don't recall what his field was.

FERGUSON: David M. McQueen was a physical chemist.

NICHOLS: Yes, and a good Canadian. I don't know that he ever was naturalized.

FERGUSON: Oh, he's still in Delaware, raising horses, I think.

NICHOLS: Oh no, he died several years back.

FERGUSON: Oh, McQueen died. I'm sorry, I'd forgotten that.

NICHOLS: He was raising horses though.

FERGUSON: Here are some lists from 1927, which was the year you arrived. Some of these people you've already mentioned.

NICHOLS: George Schwartz, I forget what his field was, but it became safety. He was in charge of safety and instituted some of the things which helped us have good safety records.

FERGUSON: Merlin M. [Bru] Brubaker is a name that rings a bell with me.

NICHOLS: Yeah, Brubaker was head of the Chemical Department. I think Salzberg was his assistant. I think that Bru refused to step up, so Salzberg took it.

FERGUSON: You worked with Gordon D. Patterson for many years.

NICHOLS: Yes, I worked with him for many years. He had the pigment work at the Station, along with Cliff Sloan, who did a lot of the developmental work on the TiO_2 process. I was quite close to him.

FERGUSON: Was Patterson organic or inorganic chemistry?

NICHOLS: He wasn't organic, I'm pretty sure. He came from Meadville and graduated from Alleheny College. Then he went to Ohio State for his graduate work. I'm pretty sure he didn't have an organic background. It may have been physical chemistry.

FERGUSON: I inferred this, perhaps from the title of the research group he had earlier. This was in a short summary report by Tanberg in August of 1927, in which he described briefly what the various groups were doing. The ones that seemed to be polymer related were: Don Coffman (nitrocellulose solutions and films); John Iliff (paints); John L. Keats (Duco undercoats); Harold Barrett (colored auto top coats); the Patterson group; Charles O. Bostwick and Lewers (Duco undercoats and quick drying enamels, and a variety of paints containing nitrocellulose); and Shive and Jim [James H.] Werntz (paints).

Do you remember those projects?

NICHOLS: I remember all of those people. Bostwick was a playboy. He didn't stay with the company very long. Bill Lewers, I guess was competent. I don't remember much about him. Shive was eventually at Philadelphia. And Jim Werntz, of course, was at the Station for a good many years. He died some years back.

[END OF TAPE SIDE 5]

FERGUSON: The organization charts were very cryptic; they had names without initials, or they had only initials. It's interesting that all of those things have been removed from Lavoisier Library. The only place I could find any of them was over at Hagley in the Eleutherian Mills library.

NICHOLS: They burned so much of it.

FERGUSON: It was after you left that they got into a company-ordered records destruction schedule, in which all these records -- organization charts, personnel records, and so on -- were destroyed on a regular schedule. A lot of these materials that would be of interest to historians and librarians weren't even sent over to the hall of records. The hall of records, in fact, has a records destruction schedule. So, this, perhaps, reiterates the value that any of the old papers you kept, or notebooks, or research notes might be of interest to Hagley.

Did you have any correspondence or letters from Svedberg that you kept?

NICHOLS: Oh, yes. I have a number of letters.

FERGUSON: Have you any plans of what to do with them.

NICHOLS: Well, it's quite possible that when you get through with this project you might like to go over some of this stuff that I have here.

FERGUSON: It's not that I wouldn't be interested, but I wouldn't be the person to do this. I think that if you are interested, you could talk to John Smith at Hagley and offer them to him. If not, the BCHOC group up at the University of Pennsylvania might like to have them. They could catalog them and you wouldn't even have to organize them. I'll be seeing John Smith. Would you like me to contact him or do you want to?

NICHOLS: No, I think I'd rather contact him myself because I'm still not sure some of this material should go outside of the company.

FERGUSON: There are some things which you might need to screen yourself for confidentiality.

NICHOLS: Yes. I don't think there is anything that I really have now, after all this elapsed time, but in earlier years it might have caused some eyebrows to raise.

FERGUSON: John Smith and David Hounshell have some understanding with the company with respect to this, so that would probably be a safe place to send it.

NICHOLS: Yes, I'd think so. I have thrown away a fair amount of correspondence. For a number of years I kept correspondence that Elmer Kraemer had, or really, ideas that he had. As I went through them I gradually threw away things. I still have some. I don't know whether that would be of any interest to you. [pause] I think probably not.

FERGUSON: You did a biography of Elmer Kraemer for a journal, didn't you?

NICHOLS: Yes, that was just a general biography (14).

FERGUSON: Somebody going back in Du Pont history might want this material that you have saved.

NICHOLS: This is mainly interdepartmental correspondence.

FERGUSON: I thought this was a good opportunity to call this to your attention. I think the historians would really be interested in any letters from Svedberg. Are there any pictures?

NICHOLS: Yes. I'm sure there's nothing there that would affect the company.

FERGUSON: Did you keep the photographs. They used to take photographs of the personnel of 228 building [Experimental Station] almost annually. Did you keep any of those?

NICHOLS: I have several, but not a complete set.

FERGUSON: Those would be of particular interest to Smith.

NICHOLS: Surely they have a file of those.

FERGUSON: After you retired Bob Joyce was doing a history of the Chemical Department and Central Research. He was trying to collect all of that kind of material that he could find. I don't know whether he turned that over to Smith or not. [Further discussion of how to offer documents to the archives.]

I wanted to get a reference to the book that had the photograph of the portrait of your great-grandfather Ebenezer Nichols.

NICHOLS: It is called <u>We Crown Them All: An Illustrated History of Danbury, Connecticut</u>, by William E. Devlin (15).

FERGUSON: It's a beautiful book. Autographed by Devlin, I see.

NICHOLS: It's published in California, I think.

FERGUSON: Sponsored by the Danbury Historical Association.

NICHOLS: Windsor Publications. Funny, I don't see an address.

FERGUSON: 1984. We don't really need the address.

NICHOLS: Well, I'm pretty sure it's California.

FERGUSON: Do you think it might be a limited edition?

NICHOLS: Well, to a certain extent, yes.

FERGUSON: Well, Burt, for myself and for BCHOC I really want to thank you for this interview.

[END OF TAPE, SIDE 6]

[The following information was supplied by Nr. Nichols after the formal recording was stopped. He has requested that this be added to the record.]

First he mentioned the foreign students who were at Uppsala with him. One was Neil B. Lewis, an Australian who had a doctorate from Oxford. After Uppsala he went back to Australia and became the Eastman Kodak representative there.

There was a Rumanian who had attended University College and knew Victor Cofman there, and that's how Nichols first came to hear of him. The Rumanian, Eugene Chirnoaga, went back to Bucharest and became a professor of physical chemistry. He was liquidated by the Nazis during the war.

There was a Japanese chemist named Tominosuke Katsurai, who just showed up at Uppsala. He called Arne Tiselius and said he wanted to come over to the university. Tiselius asked him, "How long are you going to be here, so that I can get a return ticket for you?" and he said, "I hope two years, if you'll have me." Apparently that was the first contact they had with him but he did stay on.

Arne Tiselius became probably the best friend of Nichols and his wife over there, right from the start. Burt says he was obviously of Nobel Prize quality right from the beginning. They had no doubt that he was going places from the way he tackled his assigned project of electrophoresis.

In further conversation Eleanor Nichols came in and remarked that she had done mechanical drawings of Svedberg's centrifuges for him. So she too had some part in the experience there.

NOTES

1. Interview of J. B. Nichols by J. K. Smith, 14 July 1983. See BCHOC Oral History file #0034.

2. J. B. Nichols, "Nitrocellulose and Camphor," Journal of Physical Chemistry, 28 (1924): 769-771.

3. D. S. Kimball, Principles of Industrial Organization (New York: McGraw-Hill, 1919).

4. T. Svedberg and J. B. Nichols, "Analytical Centrifuge," U.S. Patent 1,648,369, issued 8 November 1927 (application filed 10 September 1923).

5. A. Tiselius and S. Claesson, "The Svedberg and Fifty Years of Physical Chemistry in Sweden," Annual Reviews of Physical Chemistry 18 (1967); 1-8.

6. T. Svedberg and J. B. Nichols, "Determination of Size and Distribution of Size of Particles by Centrifugation," Journal of the American Chemical Society 45 (1923): 2910-2917.

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