CHEMICAL HERITAGE FOUNDATION

IVAN MAXWELL ROBINSON

Transcript of an Interview
Conducted by

James G. Traynham

at

Wilmington, Delaware

on

24 January 2001

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Ivan Maxwell Robinson

(Date) August 4, 2003

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IVAN MAXWELL ROBINSON

1920 Born in Lakeville, Nova Scotia, Canada on 26 May

Education

1940 B.Sc., chemistry, Acadia University
1941 B.Sc. (Hons), chemistry, Acadia University
1942 M.A, chemistry, University of Toronto
1949 Ph.D., chemistry, Purdue University

Professional Experience

Canadian Industries, Ltd.
1942-1943 Research Chemist
1945-1946 Research Chemist

E. I. DuPont de Nemours and Company, Inc.
1949-1952 Research Chemist
1952-1954 Supervisor
1954-1961 Section Manager
1961-1964 Manager, Technical Sales
1964-1973 Laboratory Director
1973-1975 Research Associate
1975-1981 Research Fellow

Indiana University, Bloomington
1981 Visiting Scientist

Honors

2000 Lavoisier Medal for Technical Achievement

ABSTRACT

Ivan Maxwell Robinson begins the interview with a discussion of his family life and education. He was born in the small village of Lakeville, Nova Scotia, where his father ran the general store, and his mother was a school teacher. Around sixth grade, Robinson's family moved to Kentville, Nova Scotia, where Maxwell Robinson attended junior high school and high school. After high school, Robinson earned his bachelor's degree in chemistry, with honors in 1941, from Acadia University. He obtained his master's degree in chemistry from the University of Toronto in 1942, and worked briefly for Canadian Industries Ltd. while studying. After a brief term of service in the Royal Canadian Air Force, Robinson returned to college, where he earned his Ph.D. in chemistry from Purdue University in 1949. Robinson was interviewed by numerous corporations while studying at Purdue University, and decided that DuPont was the best place to do research. Subsequently, he moved his family to Wilmington, Delaware, and joined DuPont as a bench chemist. Robinson worked initially in Frank Gresham's research group trying to make a polyimide from a monoamine. By 1952, he successfully made a high-molecular-weight polyimide from a long-chain diamine. In that same year, Robinson was made a supervisor at DuPont. Robinson's group is credited with numerous chemical innovations, such as coordination polymerization, and copolymers of ethylene-sulfur dioxide. Robinson retired from DuPont as a research chemist in 1981 and joined Indiana University as a visiting scientist. Moreover, Robinson has been teaching genealogy at the Academy of Lifelong Learning for over 10 years. In 2000, Robinson was awarded the Lavoisier Medal for Technical Achievement. Robinson concludes the interview with a discussion of Karl Ziegler's and Giulio Natta's work on propylene polymerization, and its relationship to his group's work at DuPont.

INTERVIEWER

James G. Traynham is a Professor of Chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

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INTERVIEWER: James G. Traynham

LOCATION: Wilmington, Delaware

DATE: 24 January 2001

TRAYNHAM: Dr. Robinson, I've read that you were born on 26 May 1920, in Nova Scotia, Canada. Tell me something about your parents and your childhood.

ROBINSON: I was born in a very small village called Lakeville, in Kings County, Nova Scotia, where my father ran the general store. It was a very short walk from his store to the schoolhouse. It was a two-room schoolhouse, with six grades in one room and six in another room. I look back on my early schooling as a fascinating experience. Being in the same classroom with six grades had an amazing advantage. It gives you a chance to review what you had the year before and to get a taste of what's coming up next year. My mother was a schoolteacher and instilled in my sister and me a strong attachment to a good education early on. Because of her influence and the school system we had at the time, I took the first four grades in two years. Around 1927 or 1928 my parents moved to a farm. Dad found that his generous nature did not make the general store a viable situation economically. There were too many needy people asking for free flour, sugar, kerosene or something. He decided to try farming. In those days we had no electricity and no telephone on the farm, but we didn't feel deprived.

Primarily because of my mother's influence we moved to the town of Kentville, Nova Scotia, when I was in about grade six. Kentville had a population of about five thousand people, which is about as many as it has today. In that town they had a much broader educational system since they covered the whole county. There was a grade school, a junior high school, and a high school. I went into grade six when we moved to that town and progressed up to junior high. I only have a vague recollection of that time. I was the smallest boy in every class because of my age and stature at the time. My parents kept me back one grade, probably grade seven or eight, because of either my age or the classes. Anyway, it was a relief to get slipped back a bit.

By high school we had one room to a grade, which was a big event. It was a wonderful high school. I have very fond memories of my chemistry teacher, Wallace [L.] Barteaux, and my math teacher particularly, Walter [L.] Bowers. My sister, who lives in Halifax [Nova Scotia], says that she has seen Walter Bowers in recent years and he still asks about me. I had a wonderful relationship with that teacher.

TRAYNHAM: How many were in your high school class?

ROBINSON: I just looked it up. I would guess there were about twenty-five.

TRAYNHAM: That's the number that was in my high school class.

ROBINSON: Is that right? Good for you. I just looked at my high school yearbook and read with glee, "Maxwell Ivan Robinson—all great men are not dead yet." The next year, I took up my studies at Acadia University. In those days, if you passed the exams at Kings County Academy with a certain average you did not have to take the provincial exams, which was a relief. Also, if you passed through grade twelve you could go into college as a sophomore, which was not a relief. I went into Acadia University as a "freshy-soph" to save money because it was the [Great] Depression and times were very difficult. One of my most traumatic experiences as a "freshy-soph" was taking Calculus II without ever taking Calculus I. It took a lot of catching up to get up to speed.

I had excellent chemistry professors at Acadia. My major organic professor was Professor Chester [W.] Small, a Newfoundlander, and an excellent professor.

TRAYNHAM: When you arrived at Acadia had you already chosen chemistry as your major, or did you decide that while you were at the University?

ROBINSON: I had decided in high school. In the ninth grade I had to write an essay called, "Who do you want to be?" Back then I said I wanted to be a medical doctor. Somewhere along the line I switched over to chemistry. By the time I went to University, I knew that chemistry was the path I wanted to take. I received a Bachelor's degree in chemistry in 1940, and then I stayed one more year to get a Bachelor's degree with honors in chemistry. That gave me four years at Acadia University.

TRAYNHAM: What was the distinction between the two degrees at Acadia at that time?

ROBINSON: For the honors program you had to take a certain number of courses in your major topic, which I had not had time to do in three years, and write a dissertation—mine was entitled, "The Construction and Use of a Photoelectric Colorimeter." In addition, I picked up a major in mathematics along the line. From Acadia I went to the University of Toronto in the fall of 1941. My major professor there was Professor George F. Wright, who was a strict taskmaster. I could

have some comments off the record about George F. Wright. He ran a micromanaged research group. It was a very rigorous situation.

TRAYNHAM: I remember reading papers by George F. Wright back in the fifties, but I didn't know that aspect of his research direction.

ROBINSON: I just received from the University of Toronto one of their periodicals in which a student wrote up his recollections of his time with George F. Wright. I have it here if you care to read it. Anyway, it was a rigorous and uncomfortable environment for me. Fortunately, I was very successful in the research program there. It was during the [World] War [II] and we were working on explosives, which was interesting in a way. We were working on the nitration of hexamethylenetetramine to make RDX [cyclonite], which ended up being the propellant in the atom bomb. In addition, we were testing the brisance of RDX in an armory on the weekend. We used shaped charges that could blow a hole through a steel plate when the explosive detonated. We didn't know why we were doing it, but many, many years later we found out why. My major thesis was on the synthesis of methylene-dinitramine, which was very successful. In fact, it was considered for further developmental work. I was told not to talk about it, and I may be still under the same restrictions because I haven't talked about it. I have my thesis here (1).

From Toronto I was interviewed by Canadian Industries, Ltd. I went to work at their research lab in Beloeil, Quebec. I worked there until 1943 when I joined the Royal Canadian Air Force. I went into pilot training, which I never finished because by the time I got further along in the program they decided that the War was progressing favorably and they didn't need any more pilots. When I was on my honeymoon with Jean [Jeannee Robinson], the MPs [military police] greeted me as we got off the train in Nova Scotia with a telegram to report back at once. I ignored it for a couple of days till they sent another telegram saying I was to be reassigned to something other than pilot. I would be a navigator, flight engineer, bombardier, air gunner, or discharged. The latter was appealing, so we went back to Montreal [Quebec] for discharge. [laughter]

TRAYNHAM: You got discharged?

ROBINSON: I got discharged. Went back to CIL [Canadian Industries, Ltd.] for a short time and then I decided that if I was going to excel in research, I needed to go back to school.

TRAYNHAM: Do you remember what prompted that line of thought?

ROBINSON: I don't think it was any one situation. I was in a research lab in which there were Ph.D.'s, master's degree people, and bachelor's degree people, and I could tell that the Ph.D.'s had a better understanding and ability. I said to myself, "If I'm going to do this I'd better do it right," even though I knew going back to graduate school would be tough. In late 1945, I wrote to a number of universities that had ads in *C&E News* [*Chemical & Engineering News*]. With the big influx of GIs [general infantrymen] going back to school at that time, there was a big demand for graduate assistance. While I heard from several places, Purdue University said, "We'd like you to come and we can guarantee you housing." Housing was the thing that tipped my decision to go to Purdue. I went to Purdue in the fall of 1946, but two or three days before I was to leave they sent another telegram saying the housing was not ready.

I had to leave my wife and child behind with her folks in Montreal, which was traumatic for us, and head off to Purdue. It was an exciting time; I just relished it. I remember the first day in the big assembly hall vividly. They had crowded five-thousand students into it who were taking chemistry—chemistry was a required course for freshman engineers. There were about sixty graduate assistants and eight chemistry professors. They told us what we would do. Classes started at seven o'clock in the morning. A professor would lecture to eight hundred or so undergrads, and then another eight hundred, and so on. One day a week the graduate assistants would take over for two other lecturers and a lab, and explain what the professor said in his lecture. [laughter] That's what we did. It was a wonderful experience. We had to learn the chemistry that the professor was teaching. I look on that as a wonderful part of my education. I don't think it was planned that way, but being a graduate assistant had some real pluses.

My major professors at Purdue were Professors Henry [B.] Haas and E. T. [Earl T.] McBee. Henry Haas was head of the chemistry department, and part way through my time there he went to take a job in industry. Fortunately for me, I had a backup in Earl McBee, so I didn't have to switch major professors. That probably would have caused me to start another research program and take more time, which I was very reluctant to do. My major thesis was on the vapor-phase nitration of 1-1-1-trifluoropropane. I found some good chemistry there and had a satisfactory thesis. Jean typed my thesis for me. I've got to give a little talk here in Wilmington [Delaware] this month for the Lavoisier Award, and I'm going to acknowledge that Jean helped put me through graduate school at Purdue. She got a recognized but unaccredited degree Ph.T.—"putting husband through." At Purdue I was interviewed by a number of corporations and went to visit some of them. DuPont [E. I. DuPont de Nemours and Co., Inc.] in Wilmington looked like the best place to do research.

TRAYNHAM: Did you apply to any Canadian companies at that time, or did you decide to stay put?

ROBINSON: No, I did not. I should go back a back a little bit. I worked at Canadian Industries, Ltd., while I was in graduate school. It was jointly owned by ICI [Imperial Chemical

Industries, PLC] and DuPont. When I interviewed with DuPont, they said that there was a good chance they could give me credit for my CIL and [Royal Canadian] Air Force time, which was five years. That was an added incentive. We decided to come to Wilmington, Delaware in 1949. There was still a shortage of housing, but we did find a rental house in the suburbs. It was a tremendous blessing compared to the one room apartment we'd been living in for three years.

TRAYNHAM: Did Purdue ever fulfill its promise of housing?

ROBINSON: Yes, to a degree. It was not comfortable housing. About a month after I arrived at Purdue in September they said that because there were so many married graduate assistants they would open up the Purdue Memorial Union Building, which had a wing for visitors like a motel. It was a very majestic structure in the middle of the campus. We had one room per family. All of us graduate assistants with families crowded into that facility. We couldn't afford to eat in restaurants. Our motel room had a shower in which we set up a table, a hot plate, and a pressure cooker. That's what we subsisted on. There was no refrigerator. We bought food every day as we ate it. It was a tough experience, but we were all in the same boat. Other housing on campus was even worse; people were living in garages and unheated attics. We actually had heat and hot water, so we were semi-privileged in our one room.

TRAYNHAM: Then you got to Wilmington and found actual housing.

ROBINSON: We had a whole house but no furniture, at first. We sat on the stairs and slept on the floor for a while. It was a new life. I joined the ammonia department and was a bench chemist down in the 269 Building on the Brandywine River. It was about 100 yards from the [W. H.] Carothers Laboratory where Wallace [H.] Carothers made his tremendous contributions to the early polymer industry.

I should go back a little bit to 1939. When I was a senior chemistry major at Acadia when I went to the World's Fair in New York. I saw the new miracle, nylon fiber, for the first time in the DuPont Pavilion. The publicity there was that it was made from coal, air, and water. I said, "That's a lot of hype," because a lot of chemistry and engineering had been done and a strong commercial commitment had been made to bring it to reality. I think DuPont's statement at the time was that you could do some real miracles with chemistry and science, and that's what happened with nylon. I had no idea that ten years later I'd be working in the polymer field within 100 yards of the Carothers Laboratory.

I was at the right place, at the right time, with the right people. I was in the exploratory research section of the old ammonia department, which became the polychemicals department later that same year. I was working on chemicals while in the ammonia department, but the

emphasis switched rapidly to polymer chemistry. Frank [William Frank] Gresham was head of the exploratory-research section. His agenda was to find new polymers for our plastics business. At that time, we had major plastics and nylon resins, polyethylene resins, methacrylates, and Teflon. As a bench chemist in Gresham's group, I was assigned to look for a polyimide early on. We had a good business in the polyamide field with nylon resins, but the hexamethylene diamine portion of that polyamide was very expensive. We thought that if we could by any means make a polyimide from a monoamine it might result in very significant cost savings. I tried many different ways in the lab to make a high-molecular-weight polyimide from a monoamine. Most of those reactions ended up with too many side reactions, so I never made anything of significant molecular weight.

TRAYNHAM: They would necessarily be branched polymers because of the hydrocarbon tail from the monoamine.

ROBINSON: Yes. It could be an aliphatic or an aromatic monoamine. I did make low-molecular-weight polymers, but nothing of any significance. I don't remember the details, but I decided to look slightly outside the box and not restrict my experiments to just monoamines. I had looked in the literature on stable imides, and I found that pyromellitic imide was a very stable chemical. Fortunately, pyromellitic anhydride was available from the oxidation of durene, so I tried to make a polymer from pyromellitic dianhydride and hexamethylene diamine. I ended up with an infusible mass, but at least it gave an infusible mass. I've just gone back to review my lab notebooks, and by 1952 I had made a high-molecular-weight polyimide from a long-chain diamine. It had some gel in it, so it wasn't completely linear. I modified the process and was made a good, high-molecular-weight polyimide for the first time. It had interesting properties. It had very high melting characteristics and good electrical properties. It was exciting. I got promoted to supervisor probably as a result of that breakthrough. Walt [Walter M.] Edwards was assigned to my group to continue the work on polyimides.

TRAYNHAM: Was the work you did on polyimides ever successfully commercialized?

ROBINSON: Yes.

TRAYNHAM: What was the polymer name?

ROBINSON: The definitive structure changed from my initial one, but polyimides are made by DuPont today with the trade names Kapton and Vespel.

[END OF TAPE, SIDE 1]

ROBINSON: The breakthrough in making attractive polyimides was brought to the attention of other departments, and they came to see how we made that polymer. In the polychemicals department, it was advanced to a stage where "Polymer E" was developed, probably for electrical. In the film department there was also a strong interest. We talked to a man by the name of Cy [Cyrus] Scroog, who took over the initiative to see what utility the polyimide structure would have for the film department's uses. The textile fibers department displayed some interest early on, but I don't think they ever made any significant developmental effort. After the initial success with polyimides, I went to the library and looked up other heterocyclic-ring structures—model compounds. I found stable ring structures in polybenzoxazols and polybenzimidazoles. I wrote it up as a potential project for the exploratory section, and we assigned Keith C. Brinker to the project.

Keith, who was in my group, made stable, plastic, high-molecular-weight polybenzimidazole, and polybenzoxazol for the first time. Keith and I made the initial polymers on each of those two classes. Incidentally, the chemical literature shows that those early patents were recognized as the first patents in the polyimide group. I went to a site on the Internet when I heard about this interview coming up. I typed "polyimides" in a patent search and I found that today there are 1,370 patents on polyimides.

TRAYNHAM: Is yours the first?

ROBINSON: Walt Edwards and I had the first. There are 73 patents on polybenzimidazoles and 46 patents on polybenzoxazols, and Keith Brinker and I have the first on both. That's recognized in the literature. Incidentally, there are 2,200 patents on polyamides, so there's a lot of patent work. May I move on to another topic?

TRAYNHAM: Please do.

ROBINSON: In the exploratory section I had from five to seven technical people reporting to me—it varied over time. Art [Arthur W.] Anderson had roughly the same number over that period of time. Anderson and I both reported to Frank Gresham, who tended to be a micromanager. He liked to know the details of what was going on. When I joined his group initially we had to write a daily report, which the technical men objected to highly, but Frank insisted. Frank relented a little bit in the polychemicals department, so we went to a weekly report system, to which the technical-bench guys still objected. It was the job of Art and I to make it palatable, which I think we did reasonably well, although some objected more than others. In retrospect, there was an advantage to the weekly report system. We got together for

staff meetings with Gresham once a week, in which we went through each of those weekly reports in great detail and discussed the successes and the failures. We had an intimate, steady look at the research progress over that time period. It was a lot of work but it kept us sharp.

TRAYNHAM: I'm sure it kept the bench chemists sharp too.

ROBINSON: Exactly right. They didn't like it, but that's right. We had a very productive group of young chemists. For the most part they were directly out of school and had never done anything in the polymer field; and probably never wanted to, because back in academia polymers were gooey messes that were unknown, undecipherable, and unwanted.

TRAYNHAM: Chemists made a lot of them but they went down the drain.

ROBINSON: Exactly. By 1954 I had a man in my group by the name of N. G. [Nicolas G.] Merckling. We were in the polyethylene business, and we knew technically but not commercially that better polyethylene could be made. In fact, Don [Donald] Pease in central research had shown that a more linear polyethylene was a stiffer material, but it required impractical conditions to make it. Art Anderson suggested putting a ring structure in the polyethylene chain to make it stiffer. He suggested norbornene as a candidate, which is the bicyclic-ring structure you get from cyclopentadiene and ethylene. It has one double bond. The rationale behind Art's suggestion was that a lot of free radical polymerizations don't proceed because the radical moves around to portions of the molecule where it prevents the chain-polymerization process. On the bicyclic-ring structure of norbornene, you can't really get a free radical in a bridgehead position. Therefore he suggested ethylene norbornene. It was Anderson's idea to assign Merckling, who was in my group, to that program. He tried many different, unsuccessful experiments to create a copolymer.

He came across an old patent, although I don't know the details as to how. The reason I remember that fifty years later is that we've been in a lot of patent litigation about that work, so I have had my mind refreshed agonizingly over the last thirty years. Merckling found an old patent by Max Fischer, who was working for BASF [Badische Anilin une Sodafabriken] at the time, in which he made a polymer from ethylene. The unusual thing about the Max Fischer catalyst was that it looked like a modified Freidel-Crafts catalyst on the surface because it was aluminum chloride [$AlCl_3$], titanium chloride [$TiCl_4$], and a metal that I think was sodium [Na]. Nick found that patent reference and decided to try the Max Fischer technology with ethylene norbornene. He made a very small amount of polymer, not really enough to do much with. I suggested to him that he leave out the norbornene and just try ethylene, and he made another small amount of polymer that, intriguingly, looked fairly stiff. In fact, I have that sample right here. Shortly after that Nick was able to make a small amount of polymer from propylene. I remember that part because I took that very small amount of polymer, heated it on the spatula,

pulled out a fiber, and said, "We've got the very first polypropylene in the world." That wasn't actually true but we thought it at the time.

It was an exciting time. We made many new polymers that we had not conceived of a few weeks before. We invited people from other departments to come into Gresham's office, and we sat down and reviewed what we had discovered. As the work progressed, Anderson made a major contribution—he conceived of the key portion of the catalyst as a reduced form of titanium. That became the key to making the catalyst. With that, we were able to make catalysts from many different elements of the periodic table. Soon there was enough interest in other departments for a periodic meeting with chemists from fibers, film, and elastomers.

I don't know the total number of chemists assigned to those programs at the time, but we did have a substantial number of people at the experimental station working on coordination polymerization, which is the name I coined for that type of polymerization. In fact, I got a little bit of heat about that at the first research review. We had conceived of a mechanism coordinating the π electrons on the carbon-carbon double bond with the reduced titanium [Ti]. After it activated that molecule, it swung around and formed a link to the titanium, letting another olefin double-bond get coordinated, activated, and swung around, and so on, to propagate it. Up to that time, the only ways to make a polymer were through free radical, anionic, or cationic polymerization. At the first research review for the entire polychemicals department, I said in my talk that we would offer a new kind of polymerization called coordination polymerization. It was not universally accepted because one of the research associates wrote a memo saying it was hogwash, and it couldn't be a new kind of polymerization—but it was.

Remarkably, other laboratories in the US [United State of America] and Europe made essentially the same discovery at about the same time. We were shattered, of course, to find we would be in a multiparty interference on polyethylene and polypropylene and all the rest of those things. That went on for a long time. We did get the Canadian composition-matter patent on polypropylene, not that it did the company any good, because polypropylene is now a commodity, a multi-million pound-per-year plastic. DuPont carved out a specialty niche in the Tyvek polyethylene business, but polyethylene and polypropylene are commodity plastics these days made chiefly by companies in the raw material business.

TRAYNHAM: You alluded to the dispute over the patent. Did DuPont lose out on the patent dispute for polypropylene in the US?

ROBINSON: Yes, they did. The Phillips Petroleum Company ended up with the composition-matter patent on polypropylene.

TRAYNHAM: Was that because their documentation was dated earlier?

ROBINSON: I'm not sure. It could have been that their filing date was earlier. I do know that there was a lot of discussion about the quality of the records from Phillips Petroleum.

TRAYNHAM: Were they of good quality or deficient quality?

ROBINSON: Our legal people told us they were skeptical of the quality of Phillips's records. I can say something off the record about that later. I had a group of good people. I'd like to talk a little bit about those people.

TRAYNHAM: That would be great.

ROBINSON: Don [Donald H.] Payne was a very ingenious physical chemist in my group. He separated polypropylene into an anamorphous fraction and a highly crystalline fraction. That was the first time we saw a highly crystalline polypropylene. One of the things we did with that material was in film form. I remember that Herb [Herbert S.] Eleuterio and I took it to the film department and had it two-way oriented in their equipment. They said, "This stuff looks just like Mylar." It was tough and transparent; even in highly crystalline form it was still very transparent.

I have worked with so many people. In fact, I sat down recently and made a list of people I worked with as background for an award I'm getting later this spring. I came up with forty-one that I remember. I don't have time to talk about all of them now, but I would like to talk about some of the good things we did. Rudy [Rudolf B.] de Jong had a program on making ethylene SO_2 [sulfur-dioxide] copolymer. Up to that time he'd only been able to make a one-to-one copolymer, which was not a useable material. Rudy and I found a way to reduce the amount of SO_2 and made a very interesting copolymer of ethylene SO_2. I wonder what happened to that, because we had the patent on it (2). It was an interesting polymer, a good way to get rid of some SO_2 in a reasonable structure instead of putting it out in the environment.

TRAYNHAM: Was the SO_2 unit in the polymer chain?

ROBINSON: Exactly right. The key was to restrict the availability of SO_2 during the copolymerization process. Walter Gall, in my group, found a way to make good high-molecular-weight polyethylene glycol terepthalate by using pyromellitic dianhydride. The textile fibers department was very interested in that at one time, but I don't know what happened

to it. Don [Donald] McCane in my group found a way to dimerize perfluoropropylene into a perfluorodimethyl cyclobutane, which was of interest for its boiling point. I don't know what ever happened to that either. Don [Donald] Brener, in my group, worked on polyimide composites. I wonder what ever happened to those. I could go on.

I'm going to skip to 1958. Management decided that the chemical side of the polychemicals department's business had not been given enough attention. They spun off a group in the research division to work on the chemicals business of the polychemicals department. I was essentially separated from polymer chemistry for quite a long time. Our objective was to come up with new chemicals for the chemicals business. That project took a long time. I initiated a project to investigate the chemicals that we discarded at our plants. I did all the traveling. I made a trip to our departments, first to Belle [Works, Belle, West Virginia], then to Sabine [River Works, Orange, Texas], and then to Victoria [plant, Victoria, Texas]. At Belle I found that in the nylon intermediates business, particularly in the hexamethylene diamine area, we were indeed burning and throwing away by-products. The first one that I discovered was hexamethyleneimine, which was a cyclic by-product from hexamethylene diamine. It was a very simple process to collect it from the distillation-refining process, and we were able to rapidly commercialize that hexamethyleneimine product.

TRAYNHAM: Did you have a market for it?

ROBINSON: Yes. I recently looked up hexamethyleneimine and found that there were a bunch of patents on it too. It's amazing what you can do on the Internet these days right from your home computer. There were also bottoms from the hexamethylene diamine refining train. That was a high-molecular-weight amine product, which we found a market for without any process improvement. We first called it Belle diamine heel. It was called amine-248-B for two, D for four, and eight for heel. We found a market for it in asphalt compositions. The biggest by-product stream was NVR [non-volatile residue], the non-volatile residue from the adipic-acid process, which had a lot of hydroxycaproic acid in it. We had a program for a while, in which we tried to make ester plasticizers for polyvinyl chloride [PVC], but it never got off the ground. I think today the adipic-acid process has been refined enough so they don't make nearly as much by-product. There was a stage in the refining process where they made a mixture of dibasic in atypic acids, which have been commercialized as esters. I think there is some market for that.

Around 1959, top management decided to amalgamate the chemicals business into another department. A group of us from polychemicals got amalgamated with the Graselli department. Culturally, that was a tough transition for a lot of us. It was an entirely different way of doing research. Fortunately for me I had another promotion to become manager of development and service at Chestnut Run [Delaware]. I went there in 1960 or 1961. I had a long career there until 1974. It was revealing in a way. An awful lot of work has to be done to take an invention from the bench, to the development stage, to the market, and to market acceptance—it takes a long time. We did that very successfully with azobisisobutyronitrile,

which is now known as "Vazo." It was made in a batch process just for internal use at the Parkersburg [West Virginia] plant. I assigned Bill [William P.] Langstroff, a chemist, to examine that process, and he improved it remarkably. We got the plant to accept it, and then we had enough product for the development work. We had a great development guy named Norm [Norman P.] Rockwell, who basically took it to our districts single-handedly.

One of the problems with taking a product like that into the established chemical-sales organization was that nobody understood the technology, and you couldn't train salesmen to handle it easily. It was a very difficult transition. I'm sure it has occurred with other products too. I look back on my administrative time as a lab director at Chestnut Run as satisfying, but not rewarding. There were too many headaches and personnel problems. I did work with some very good people, and it gave me a better appreciation for the whole aspect of the chemicals business. But something was lacking. In 1973, I was offered the chance to go back to research.

[END OF TAPE, SIDE 2]

TRAYNHAM: Dr. Robinson, you were just commenting on your administrative work at Chestnut Run and your return to Wilmington.

ROBINSON: Yes. Chestnut Run is just outside of Wilmington, so it's within the environment. I went back to the Experimental Station as a research associate in the chemicals, dyes, and pigments department. There had been several amalgamations of the DuPont businesses. I've been in five departments, but I've been here in Wilmington the whole time.

TRAYNHAM: As a research fellow, did you have administrative duties or did you conduct research full time?

ROBINSON: I returned as a research associate and I was made a research fellow after I'd been there awhile. The only administrative duty that I had was my excellent laboratory technician, Larry Dworsky. I should say something about my lab technicians. When I began at DuPont, I had an excellent lab technician named Pat DeCampli, who was a gem. Larry Dworsky had been a lab technician for a long time, and was a very good, dependable experimentalist. Larry and I hit it off well together. There were several changes in his title, but when I was a research fellow it was the top non-technical assigned title.

There was another man there at the same time, Ralph [K.] Iyler, who was also a research fellow. Just as a coincidence, Ralph Iyler had passed through the same Canadian Industries laboratory at Beloeil that I did ten years later, and we both ended up in the same building at the DuPont Experimental Station years later, both as research fellows. One advantage of being a

research fellow was that you got an opportunity to work on what you want to do. The polymer business of that department was the soft segment for Lycra. It was a polymer made by polymerization of tetrahydrofuran. It looked like an area where someone with a polymer background could be of value, so I gravitated to work in that field. I enjoyed getting back to the bench and looking at what we could do to enhance our role in the soft segment of the Lycra business.

TRAYNHAM: What do you mean by soft segment?

ROBINSON: Lycra and other polymers derived in the same general nature are usually made from several different components. For example, the polyurethane technology requires a diisocyanate along with a short-chain, glycol-like, butane diol to make it a hard segment where the ring structures were joined by a very short segment. But then they need a flexible long chain, usually with a molecular weight of 1,000 to 5,000, which gives the soft segment portion of the polymer. Changing the ratio of those three components controlled the properties. It shows up today in things like scooter wheels for kids' scooters.

Returning to the specific needs for Lycra, at that time there were concerns about free radical stability to the resulting polymer, hydrolytic stability, and processing. We found that an ethylene-oxide THF [tetrahydrofuran] copolymer gave a much lower viscosity to the melt stage for spinning, and resulted in a much higher productivity for the established spinning plants.

There was quite a bit of interest at Waynesboro [Works, Waynesboro, Virginia] in improving their productivity with that copolymer. One of the problems we found early on was that it had a by-product component with a cyclic-ether structure. I did the initial laboratory work showing that they were many different ring sizes with different ethylene-oxide-to-THF compositions. We did a lot of work to control that undesired by-product in that polymer. Production was scaled up to make drum size quantities, but then I retired and I don't know what decisions were made on that polymer. I made many different variants to the soft segment structure. I have at least ten or fifteen patents on many different modifications to that PTMET [polytetramethylene ether glycol] backbone structure, including sulfur modified, carbonate modified, and blend modified. I don't know where that technology stands today. If there are needs in the market I think it would be good to go back and review the many variants to the initial polymer that I found. I had a great time and a very rewarding experience. I retired at the end of 1981.

TRAYNHAM: That was a little early for your age, wasn't it?

ROBINSON: Yes.

TRAYNHAM: Did you choose to enjoy the good life or was DuPont having cutbacks?

ROBINSON: I would say there was an atmosphere of cutback times in the DuPont Company right then. Jay [K.] Kochi at Indiana University offered me a chance to go there as a visiting scientist, which I did.

TRAYNHAM: Had you two become acquainted when he was a consultant at DuPont?

ROBINSON: Yes. Jay and I had a great relationship. I felt he was one of our best consultants. He's very intelligent. I went out to Indiana University, spent a semester in his lab, and I produced what I think is a very important work on alpha-omega-perflourodicarboxcylic acid, which we published and patented. I think I showed for the first time that all Teflon has dicarboxcylic acid end groups. Nobody has picked up on that technical revelation since then, which is disappointing. I know some things I would do if I were back in the lab. I would love to get in there and stir around a little bit. I could show some good things that could be done with that technology.

TRAYNHAM: Kochi didn't continue it?

ROBINSON: No. He did not. Shortly after I left he was offered a big grant to go to the University of Houston, and the grant process in academia takes a lot of time and energy away from more productive things. But it's going to relieve him from ever having to apply for grants again. I think that was the primary reason he went to Houston.

TRAYNHAM: You had a full and productive career at DuPont and retired at age sixty-one. You have alluded a few times to the fact that you achieved a patent position in certain work that you did, but you don't know what had happened with the patent afterwards. As a bench chemist producing that patentable work, did you feel content or frustrated with the company decision not to commercialize that patented work?

ROBINSON: It was always somewhat frustrating. But I realize now, having been through the development and technical sales end, that a lot of other things have to fit together to make a commercial success. It's not just a big invention that makes a commercial success, because a lot of other things have to fall into place. It is discouraging that we haven't found an easier way to capitalize on significant inventions. I know the DuPont Company has tried a number of what appear to be innovative ways of doing that: they've had a task force; they've set up a

development department; and they've established venture organizations. But none of those innovations made the pathway from discovery to commercial reality easier. It's a tricky procedure. I'm reminded a little bit of the Tyvek story. I haven't told you about the Tyvek story.

TRAYNHAM: No, you haven't. Please do.

ROBINSON: In the early days of high-density polyethylene in the mid 1950s, responsibility for the project was transferred from Gresham's group to Dan Strain's group in the polychemicals department. They had put up a small autoclave to make larger quantities for evaluation purposes. Someone from textile fibers came to Dan Strain and said they would love to have a sample of that polymer for their own evaluation as a fiber. Strain said, "We don't have enough for our own use," and was reluctant to give out any samples. A man in Strain's group by the name of Dick [Richard] Jones, who was working on the autoclave, had a wife who worked in the textile fibers department. The supervisors up there said, "Surely your husband can release a small sample for us to look at." Dick happened to grab the last portion of product that had come through the letdown portion of the autoclave which was were the solvent flash-evaporated. It had made a sponge-like mass of fibers. He just grabbed it and put it in his lunchbox and took it home to his wife and she took it into work. Pretty soon the textile fibers management descended on polychemicals research management, probably [Frank C.] McGrew and said, "Why haven't you told us about this wonderful new fiber invention you have?" Of course, McGrew didn't know anything about it. That was the serendipity involved there. Then the question was, "What do you do with this brand new mass of sponge-like fibers?" An amazing market developed for such a composition.

TRAYNHAM: Was the autoclave operator reprimanded for disobeying the work protocol?

ROBINSON: He might have been given a bonus—I would hope.

TRAYNHAM: That sounds like a story to encourage DuPont and other companies to make sure that both spouses are employed, rather than resisting it. [laughter]

ROBINSON: I was at the Experimental Station recently to go through dome of my early notebooks in preparation for this interview and a forthcoming award. I found, to my chagrin, that a lot of the memos I had written to file were destroyed, primarily for legal concerns.

TRAYNHAM: Do you mean the memos contained potentially unfavorable disclosures?

ROBINSON: I don't think they did. We had periodic meetings between the DuPont groups involving coordination polymerization; at least once a month for many months. I usually wrote up a memo to file on the highlights of those meetings. They're all gone. I think that would be a great record to have available. I do have one good record that I found in a notebook. Over the years I kept idea notebooks. I found a couple of them that are almost like diaries on the people I talked to and the suggestions that were made. They are the closest things to diaries of 1955 and 1956 that are still available.

TRAYNHAM: It sounds like a valuable bit of archival material. I hope they don't get lost.

ROBINSON: I plan to bring it to somebody's attention, because I don't think there is a comparable record after the destruction of the files. I'd love to go back and do some of the lab work on some of those suggestions, which still look pretty good to me. Another interesting thing I found going through my files is a group of chemists at the station who were involved in the polyethylene adventure.

TRAYNHAM: They each made a different contribution to the success of the project.

ROBINSON: Right.

TRAYNHAM: Do you have any comment you would like to make for the record about your assessment of DuPont's approach to chemistry and production? You moved around through several departments, with promotions as you went, and eventually returned as a research fellow doing a similar kind of work to what you had started off doing. Do you think that their approach to the organization of chemists is the most fruitful approach you can imagine, or could you make suggestions for change and improvement to the organizational structure?

ROBINSON: I have just a few comments. I think there are many situations in which people get promoted into a level where they don't belong or aren't comfortable in, and that's unfortunate. In my own case, I got separated from a mass of that initial discovery and went off on another road. Actually, the road I traveled was fine. I think DuPont treated me very well, but there was a lack of continuity. I would have liked to have kept in touch while I was in another area, which is disappointing. I feel slightly unfulfilled in that area. I don't think there's any easy answer. As I mentioned, DuPont has tried many different approaches to research and new product

development. There's no guaranteed "yellow brick road" that will be the way to achieve success all the time. It varies depending on the time and the invention and the situation.

TRAYNHAM: Your career certainly inspired your co-workers, inspired them to even invent a lengthy rhyming scheme at the time of your retirement. Among the papers I received was a copy of this rhyming summary of your career, which is an entertaining way to look back on it. You made reference a couple of times to a forthcoming award that you're to receive this spring. Would you tell me something about it and what brings it about in your twentieth year of retirement?

ROBINSON: The award is the Lavoisier Medal for Technical Achievement.

TRAYNHAM: Is it awarded by the DuPont Company?

ROBINSON: It's awarded by the DuPont Company as an annual award that started in 1990. Some of my coworkers like Gresham, Eleuterio, and Anderson have been previous awardees.

TRAYNHAM: Are all awardees also retirees at the time they get the award?

ROBINSON: I don't think so. A large number of people have received it, some of whom have passed on. It goes way back to [Charles M. A.] Stine and [Crawford H.] Greenewalt, and the early management people who were in charge of DuPont's research direction.

TRAYNHAM: What is the basis for the Lavoisier Award?

ROBINSON: Technical accomplishment. I really feel honored. I didn't do it all by myself. My teachers, my professors, and my associates should be given a piece of it at the same time. I can't stress enough how fortunate I've been coming from some pretty humble beginnings to this particular spot.

TRAYNHAM: What does the Lavoisier Award consist of? You mentioned a medal. Is there anything other than the medal?

ROBINSON: I've been to several of the annual ceremonies. They usually present a small videotaped interview of the person getting the award. They show an overview of his or her career—there's been at least one female, I think—and a short speech. There's also a site at the Experimental Station where a plaque is unveiled. But it is mostly recognition of technical accomplishment and significance to the DuPont Company.

TRAYNHAM: Is the award event held on site at DuPont?

ROBINSON: Yes. It's held downtown in the playhouse, and we can invite family and friends.

[END OF TAPE, SIDE 3]

TRAYNHAM: Dr. Robinson, I'm sure the Lavoisier Medal ceremony will be a gratifying and impressive occasion in the spring.

ROBINSON: I'm looking forward to it.

TRAYNHAM: What is the date?

ROBINSON: It will be on 26 April, here in Wilmington.

TRAYNHAM: According to my notes, you have been a participant in the Academy of Lifelong Learning under the auspices of the University of Delaware for quite a number of years. Tell us something about that.

ROBINSON: Back in the early 1980s a small core group of retired people in Wilmington got together and decided to make some use of the talents of the retired people in the area. Under the sponsorship of the University of Delaware, they formed a small group, got facilities in a building, and started offering a small variety of courses. It's now grown from that small group to approximately two thousand members, and we have about one hundred and forty volunteer instructors covering everything including art, music, language, history, social studies, and more recently, computer programming. For me in particular it's been a great chance to catch up a little bit on the fine arts side of academia. One of the problems of my scientific career is that there was such a heavy emphasis on technology that I didn't take any philosophy or any significant history or related topics.

TRAYNHAM: You mentioned you have about two thousand people involved. Is there any particular formality for getting involved in the Academy of Lifelong Learning or is it come-and-go according to the offerings each term?

ROBINSON: We have a registration process in January. Our classes start the first week in February and go on for the same semester schedule as the University. There's a registration fee for each semester. Instructors pay essentially the same fee. We have a multitude of talent in the retired community here and we're very fortunate to have people who enjoy getting together and keeping up with the world.

TRAYNHAM: You indicated earlier that you not only take courses but you're also a volunteer instructor. What course or subject matter are you teaching?

ROBINSON: I've been teaching genealogy for the past ten years. Initially I took on the job of tracing my ancestors primarily for my children. As immigrants to this country, Jean and I have a more tenuous hold on memories of the homeland, and I felt it would be worthwhile to let our children know what we can find out about the people and the times of our roots. Fortunately, there are records available today and they're becoming increasingly more available on the Internet, which allows one to go back dramatically in time to find out about the people who were our ancestors. I find it not a task but a very rewarding activity. I've generated three books, one on the Robinson family, one on the Lewis' of Prince Edward Island on my mother's side, and one just recently on my wife's side, the Andersons and McKays of Fife, Scotland. It's been a lot of work with a tremendous amount of satisfaction.

TRAYNHAM: I'm sure your children welcome this insight into their past.

ROBINSON: We've had a tremendous response. They can't get over how well our family has kept in touch over the years. In fact, we've had four family reunions since the 1980s. We now get together about sixty-five people from Canada and here. It is a challenging and rewarding activity for retirement.

TRAYNHAM: It's a personal avocation that you have brought into the teaching arena through the Academy of Lifelong Learning. Is there anything else that you've been teaching in the program?

ROBINSON: I give a couple of lectures a semester on how to do genealogy on the Internet, which has changed, of course. It's changing every year. It's amazing what's available.

TRAYNHAM: What courses are you primarily interested in taking?

ROBINSON: I've taken some wonderful courses, such as the "Life of George Washington." The man who gave that lecture is one of the best lecturers I've ever heard in my whole life.

TRAYNHAM: Was he a professional historian or did he develop an interest in it later, like you with genealogy?

ROBINSON: He's a retired technical person who's taken up history as his challenge. The other great courses I have taken are a series of medical lectures that go on each semester. A known expert in the field usually gives them, and we would have one hundred and fifty or two hundred people attending those lectures once a week. There are a great variety of challenges.

TRAYNHAM: You connected your genealogical interest to your family. Tell me something about your family. You have made reference to your wife Jean in our earlier discussion about your immigration to this country. Tell me something else about the family.

ROBINSON: I love to talk about my family. This past year I've generated an album for each of our children. I started with our number two son, who was going to be fifty in 2000. I said, "It would be a great fiftieth birthday present." Jean said, "If you're going to do it for Rick [Eric J. Robinson], you'll have to do it for the other three." So I have.

TRAYNHAM: You have four children?

ROBINSON: I have four children, and I've generated a memory book covering each of their lives to the present. In the centerpiece of that book I have a page with a condensed write-up on their grandparents. Let me just say a little bit about my parents.

TRAYNHAM: Please do. Give us their names as well.

ROBINSON: I have another opposite page on Jean's parents, but let me just focus on my dad and mother. Charles Burnham Robinson, 1892-1973, and Dora Fanny Lewis, 1892-1983. Both were born in Kings County, Nova Scotia. They were married August 15, 1919. They settled first in Lakeville, Kings County, where they ran the general store. Then they moved to a farm, and finally to Kentville, where my father established a thriving car business. Charles Burnham Robinson was always known as "Bernie," after his middle name. He was working in his uncle's store in Lakeville when he enlisted in the Royal Canadian Army in 1915. He was part of the 85th battalion, which was a Scottish Highland regiment that wore kilts, interestingly enough. He was in the 219th Division during the War. He served overseas for the entire War and was wounded twice. After being discharged in 1919, he married Dora Lewis and took over the Lakeville store. He eventually opened his own car business. His cheerful and outgoing manner made him a popular businessman. He was a generous man, a soft touch for many of those in need. He was an outdoorsman and loved hunting and fishing with his dogs.

I would like to add a little bit about my mother. Dora was part of a strong Baptist heritage. Her early life as a teacher in rural schools and the Kentville school set the stage for a lifelong interest in education. She was able to attend her normal school 70th class reunion. Her many activities included church choir, the Women's Christian Temperance Union, and the Order of the Eastern Star, which is Masonic. One of her special interests was drama. She organized and produced amateur plays. She was a strong admirer of the British royal family and flew the British flag long after Canada had its own flag. In her time there was a significant awareness of class distinctions. That was a legacy of sorts from the British. Dora moved with the upper class in town. Her good friend Ethel Kinsman was the Mayor. She loved driving her car and transporting friends to events. Even in later years in her eighties she was teaching children to read. Today we call it mentoring. That's a taste of the book I've given to our kids. It covers their activities but also relatives and families and friends. I had a great time doing that.

TRAYNHAM: I bet so. It's a delightful gift for your children.

ROBINSON: My father actually went to work on an island farm off the coast of Maine when he was a teenager. My mother was one of four children—two boys and two girls. Both of the girls had one year in college at Acadia University, which was remarkable for those times. I'm sure that was part of the reason that they were always pushing me for more education. I should tell you one story, which I didn't hear about till years later. When I graduated from high school, the bank manager came to my parents and said, "We'd like to offer Max a job in the bank." That was in the height of Depression and we had no money. I remember putting cardboard in my shoes because the soles were so worn out. I would have jumped at that job. They said to the bank manager, "No, thank you. We want him to go to college." It was a critical point.

TRAYNHAM: Do you have any siblings?

ROBINSON: Yes. I have a sister. She also went to Acadia University a couple of years after I did, and she was secretary to the president of the University until she got married. My education was a critical stage in my life with my parents. Of course, they made the right decision for me, but I had a whole bunch of jobs to earn a little money. I mowed lawns. I caddied at the golf course. While I was in college, I was a bellhop for the Canadian Pacific Railway System, which was a great summer job. I got paid thirty-three and one-third cents a day, but I got room and board and tips. I could save roughly two hundred dollars by working in the summer. Tuition was five hundred dollars, so it was a big chunk towards tuition. One of the things I did to earn money was a little unusual. The hotels were four-star hotels and people who came had a lot of money. They paid a hundred dollars a day, which was ridiculous in those days, with me getting thirty-three and a third cents a day. But the hotels didn't have radios. I'd gone to the World's Fair in New York [New York] and I stopped over in Boston [Massachusetts] on the way back. In Boston, I went to a pawnshop and picked up a radio, typewriter, and suitcase for about thirty-five dollars. I had that radio with me. During the summer I would rent my radio out for a dollar a night in the hotel. That radio helped me through college.

TRAYNHAM: Your radio was worth more than your work. [laughter]

ROBINSON: Right. Isn't that amazing? Yes. The guests would come into the hotel and ask the clerk for a radio. He would say, "One of the bellhops has a radio." We talked earlier about the family genealogy books and here is my latest on Jean's parents, Janis Reed Anderson and Jessie Jarvis Mackay. I put the book together on my computer. I downloaded those tartans off of the Internet. Of course, there are great software programs now for genealogy.

TRAYNHAM: Impressive. Tell me something about your four children. What are they doing?

ROBINSON: I have four children. Our oldest boy, Ken [Kenneth M. Robinson], worked for the DuPont Company in the textile fibers department, and ended up in the composites area of the textile fibers business. They sold that portion of the business to another company, and Ken was given the opportunity of early retirement and he took it. He's fifty-four and retired. He's an outdoorsman. He spends all of his time on nature trails and he kayaks, bikes, and maintains the park trails. His life is outdoors and that's what he does. He was married and divorced and has two daughters who live in this area, so we have grandchildren close by. Our number two son, Rick, turned fifty this year. He's married and lives in Richmond [Virginia]. He's a teacher and he's right where he ought to be. The kids love him. He teaches social studies and he's a PE [physical education] teacher.

TRAYNHAM: Does he teach in a high school?

ROBINSON: Right now he teaches middle school, but he coaches soccer, both girls and boys, for both middle and high school. He has a wonderful relationship with the kids. He says the girls' mothers particularly will call and say, "What can I do to get Susie on the soccer team? Can we train?" He says the prestige thing of getting on the soccer team is almost out of hand. Rick is a wonderful guy. He's very much like his grandfather, very personable. He has friends everywhere who maintain contact with him. His best friend is out in Denver [Colorado] and they talk together once a week. Rick is sentimental but a wonderful kid.

Third child is Barbara [J. Harra], and she is a gem. As I said, assembling these pictorial reminders of years past has been a good chance to relive some of those moments vicariously and watch with wonder as she grew to the wonderful, fun-loving, and caring woman, wife, and mother she is. We couldn't have asked for a more wonderful daughter. She's a teacher. She loves to teach. She left teaching while her two boys were young and stayed out of teaching until the boys got into late junior high or high school. One of those boys is in college right now in Elon, North Carolina. The other boy is in high school. Barbara is a caring, wonderful daughter. She's a gem.

Our number four child, John [B. Robinson], is the youngest at forty-two, and he lives in Richmond. He studied business in college and went to work for a big paper company. He worked there for quite a while and then decided he could do better in his own business. He's in financial planning and he's doing very well with a wife and three kids. He's a Boy Scout[s of America] leader and he's active in the Baptist Church, which my mother would love. We go to a Presbyterian Church. We've been very blessed with our kids and we've got seven grandchildren.

TRAYNHAM: Two sons ended up in the same city, in Richmond?

ROBINSON: Yes, probably because of Rick. He was a bachelor for a while and he had a house. Rick said to our neighbor's boy, Charlie [Charles Pell], "Come and you can find a job in Richmond. Come and live free in my house." Ricky was the sort of kid where everything was a game. He would make a game out of who would wash the dishes. Charlie told me, "I always end up washing the dishes." [laughter] Then our youngest son went to Richmond after college, lived with Rick, and got a job down there.

TRAYNHAM: This has been a very interesting account of your life and times. Would you like to add anything to make sure the record is complete? Is there anything that you left out?

ROBINSON: I'm sure there are things I left out.

TRAYNHAM: I see from the record that during your twenty years of retirement you've been called back into service by DuPont a number of times to be a witness in patent infringement cases. You have been given accolades from the supervisors at DuPont for the efficacy of your witness testimony in those cases. As you look back on your career, could you choose any one thing that was the most satisfying to you?

ROBINSON: It's hard to select one thing, but the joy of discovery is hard to match. It's terrific. One thing that I have secretly enjoyed in late retirement is people who I haven't seen for a long time saying to me, "You were my mentor." I didn't mean to be their mentor, and I didn't realize it at the time. I've had people come and say, "The reason I stayed with the DuPont Company is because of you." You can't buy that sort of stuff.

TRAYNHAM: No, you certainly can't.

ROBINSON: One thing that I'd like to bring to your attention, Jim, if you haven't seen it, is this book that I only just recently received. I'd heard about it, but I have not had a chance to read it (3).

TRAYNHAM: It's entitled, *Science and Corporate Strategy*.

ROBINSON: It covers the DuPont Company up until 1980. I have not had a chance to digest that book, but it contains revelations to me about some of the management's background decisions on corporate planning, some successful and some not so successful.

TRAYNHAM: Was that a book commissioned by DuPont or was it written by persons who were totally outside the company and got interested in it?

ROBINSON: They were commissioned by DuPont. The two authors are David [A.] Hounshell and John Kenley Smith [Jr.], who covers DuPont R&D [Research & Development] from1902 to 1980.

TRAYNHAM: Do you appear in the book?

ROBINSON: I do, yes. Interestingly, they have here my full name, Ivan Maxwell Robinson. It covers primarily the period in Gresham's group. I will read one little section of it. I mentioned in our earlier discussions, Jim, that the discovery of coordination polymerization occurred in many places at the same time. Here they discuss the background. "In September 1954, DuPont learned that Karl Ziegler, director of the Max Planck Institute for Carbon Research in Mülheim, West Germany, had discovered catalysts similar to those DuPont was using and was offering to discuss his linear polyethylene process for $50,000 in advance (4)."

TRAYNHAM: In what year did you and DuPont had developed the coordination catalyst?

ROBINSON: 1954.

TRAYNHAM: You discovered it in 1954 and Ziegler discovered it in 1954 also.

ROBINSON: I'm not exactly sure of the date. Actually, the intriguing thing is that [Giulio] Natta in Italy learned about the catalysis from Ziegler but went on to investigate propylene polymerization. I'm not going to go into a lot of detail, but here in the book it says, "DuPont's chemists had a right to feel disappointed if not somewhat bitter, because they had made the same discovery that had earned someone else fame and fortune," because Ziegler and Natta got the Nobel Prize. "In any event, the Polychem chemists led by Gresham, Anderson, and Robinson had made outstanding contributions of Nobel Prize caliber, but the extremely competitive nature of polymer R&D prevented them or DuPont from receiving widespread recognition or monetary rewards for this work (5)."

[END OF TAPE, SIDE 4]

ROBINSON: Ziegler and Natta published their early findings. In *Nature* magazine they published some of the intriguing leads without adequately defining just what their catalyst system was, which was unheard of for those times (6). They got the publicity. Understandably, DuPont, like any other corporation, was reluctant to let us disclose what we had done. That was true on the polyimide story too. A lot of people were able to publish later when we did not. Publication policy is a dilemma for big corporations. I don't think there's an easy answer. For the chemists, of course, there's a carryover from the academic tradition of "publish or perish," which still prevails. When you go from university to business there are the commercial aspects of those problems of disclosure.

TRAYNHAM: Do you think that there was a shift in the corporate attitude toward publication that enabled [Charles J.] Pedersen to become known for his crown-ether work at DuPont and receive the Nobel Prize? It was some years after your work on the coordination polymerization.

ROBINSON: I don't know. I do know there was a tremendous difference in the culture of DuPont departments. Some departments were much more willing for the researchers to publish their work than were other departments. It depended on the management of the department. Along with Warren [J.] Brehm, I had generated three papers for publication on polyimides, which are in this stack of documents here. Management would not release that information, so we didn't get publicity.

TRAYNHAM: Maybe it was an indication that your management sensed more profit potential out of the work that you were doing than the management of other companies did.

ROBINSON: Right. Why take a chance?

TRAYNHAM: What else do you want to have included here?

ROBINSON: The other thing I just wanted to point out is in the latter stages in my career as a research associate the top management would send a cover letter whenever a patent was issued in my name expressing their hopes that my continued exploratory patent work would be of benefit to their business. Here's one from my general manager, [E. P.] Blanchard—Doc Blanchard—saying, "Congratulations for a contribution to the well-being of the department. It is much appreciated." That's pretty unusual. That didn't go on early in my career, but near the end of my career management was paying attention to acknowledging some of the technical contributions.

TRAYNHAM: One intriguing aspect I would like to ask about—it has nothing to do with your career—is one of the early documents that you showed me. It was a list of your high school classmates, and it had your name as Maxwell Ivan Robinson. Yet now it's Ivan Maxwell Robinson. Was that an error at the high school level or did you deliberately invert your name? You were known as Max all the time.

ROBINSON: I was known as Max, and they probably just assumed my first name was Maxwell. On one of my university diplomas it says Maxwell Ivan Robinson too. It's always been a problem because my Ph.D. diploma says Dr. Ivan M. Robinson. It's not a big problem.

TRAYNHAM: Your family always calls you Max.

ROBINSON: Yes, they always did.

TRAYNHAM: Thank you very much for this lengthy and detailed account of your DuPont career. It will become a valuable part of the archives at the Chemical Heritage Foundation.

ROBINSON: Thank you, Jim. I've enjoyed the morning. It's been nice reviewing all this history.

[END OF TAPE, SIDE 5]

[END OF INTERVIEW]

NOTES

1. Ivan M. Robinson, “An Investigation of Methylenedinitramine,” University of Toronto, May 1942.

2. Rudolf Bauke de Jong, Ivan Maxwell Robinson, “Copolymers of Ethylene and Sulfur dioxide,” U.S. Patent #2, 943,077. Issued 28 June 1960.

3. David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (New York: Cambridge University Press, 1988).

4. David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (New York: Cambridge University Press, 1988), p. 493.

5. David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: Du Pont R&D, 1902-1980* (New York: Cambridge University Press, 1988), p. 497.

6. For example: Karl Ziegler, “Alumino-organic Syntheses in the Field of Olefinic Hydrocarbons,” *Angew. Chem.* 64(1952): 323-329; Karl Ziegler, “Linked Polymerization of Ethylene and its Homologs,” Brennstoff-Chem. 35(1954): 321-325; Giulio Natta, Piero Pino, Georgio Mazzanti, Paolo Corradini, and Umberto Giannini, “Synthesis and Properties of Highly Polymerized Crystals of Some ά-olefins Having Branched Chains,” *Atti accad. Nazl. Lincei, Rend., classe sci. fis., mat. E nat.* 19(1955): 397-403; Giulio Natta, Paolo Corradini, and Gino Dall’Asta, “Structure of the Crystalline Poly(propylene oxide) Chain,” *Atti accad. Nazl. Lincei, Rend., classe sci. fis., mat. E nat.* 20(1956): 408-413

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