THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

ROY J. PLUNKETT

Transcripts of Interviews
Conducted by

James J. Bohning

in

New York City and Philadelphia

on

14 April and 27 May 1986

(With Subsequent Changes and Additions)

ROY PLUNKETT ORAL HISTORY ##0037 (copy for your records)

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ROY J. PLUNKETT

1910 Born in New Carlisle, Ohio on 26 June

Education

1932 A.B., chemistry, Manchester College
1933 M.Sc., chemistry, Ohio State University
1936 Ph.D., organic chemistry, Ohio State University

Profession Experience

E. I. du Pont de Nemours & Company, Inc.

1936-1939 Research chemist, Jackson Laboratories
1939-1945 Chief chemist, tetraethyl lead plant, Chambers Works
1945-1949 Superintendent, tetraethyl lead plant, Chambers Works
1949-1950 Superintendent, Ponsol colors, Chambers Works
1950-1951 Assistant manager, Chambers Works
1952-1953 Manager, chemical development, Organic Chemicals
1953-1960 Manager, plants technology, Organic Chemicals
1960-1970 Director of research, Freon products, Jackson Laboratories
1970-1975 Director of operations, Freon Products Division, Organic Chemicals

Honors

1951 John Scott Award, Franklin Institute, Philadelphia
1952 D.Sc., Manchester College
1968 D.Sc., Washington College
1969 Chemical Pioneer Award, American Institute of Chemists
1973 Plastics Hall of Fame
1977 D.Sc., Ohio State University
1985 National Inventor's Hall of Fame, U.S. Department of Commerce
1986 Moissan Award, France
1988 John W. Hyatt Award, Society of Plastics Engineers

ABSTRACT

Roy Plunkett starts this conversation with James Bohning by describing his family background. As the family were members of the Church of the Brethren it was natural for Plunkett to attend Manchester College, where Paul Flory was a close friend and near contemporary. Despite the rigors of the Depression, Roy Plunkett was able to complete his chemistry studies at Manchester College and to go to Ohio State University for graduate work in carbohydrate chemistry. An offer from Du Pont sent Plunkett to the Jackson laboratories and the refrigerants group where an early assignment was the synthesis of tetrafluoroethylene. Plunkett tells the story of the plugged gas cylinder and his recognition of the unusual properties of the polymer. However, soon after this, Plunkett was moved to tetraethyl lead manufacture; he details his work there and his later career in the Organic Chemicals Department. The interview ends with the recognition of his pioneer work with Teflon and the honors it has brought him. In a brief second interview, Roy Plunkett tells of his common religious background with Paul Flory, their student days at Manchester College and Ohio State University, and their contacts over the years. The conversation ends with further recollections of the circumstances of the initial discovery of tetrafluoroethylene polymerization.

INTERVIEWER

James J. Bohning holds the B.S., M.S., and Ph.D. degrees in chemistry, and has been a member of the chemistry faculty at Wilkes College since 1959. He was chair of the Chemistry Department for sixteen years, and was appointed chair of the Department of Earth and Environmental Sciences in 1988. He has been associated with the development and management of the oral history program at the Beckman Center since 1985, and was elected Chair of the Division of the History of Chemistry of the American Chemical Society for 1987.

TABLE OF CONTENTS

INTERVIEWEE: Roy J. Plunkett

INTERVIEWER: James J. Bohning

LOCATION: Marriott Marquis Hotel, New York City

DATE: 14 April 1986

BOHNING: You were born on June 26, 1910 in New Carlisle, Ohio. Can you tell me something about your father and your mother -- their names and their occupations?

PLUNKETT: My father was Joseph Henry Plunkett. He was born and raised in Franklin County, Virginia. He was the son of a mountaineer farmer. Before he went to Ohio in 1907 he met my mother, whose name was Elizabeth May Garst, of Roanoke, Virginia. Her father was both a farmer and a railroad man and they lived right outside of Roanoke. She went to visit my father in the summer of 1909 and while she was there they decided to elope to Kentucky and get married. That was the beginning of my parents' family life. I came along nine months later.

BOHNING: But they didn't stay in Kentucky?

PLUNKETT: That's right. They went back to Ohio. I was born in New Carlisle, Ohio, which is about fifteen or twenty miles northeast of Dayton, near Vandalia. My father was a farmer all of his life. He spent most of it in Ohio in Miami county. He did return to Virginia for a short period in the early 1920s but it didn't suit him and he went back to Ohio.

BOHNING: You were raised on a farm?

PLUNKETT: I was raised on a farm except for that year and a half when I was in Roanoke, Virginia. I graduated from Newton High School in Pleasant Hill, Ohio, in 1927. I was one of twenty-four in the graduating class.

BOHNING: I'd like to talk about the schools you attended but before I do, let me ask if you have any brothers or sisters?

PLUNKETT: I have three sisters and a brother. My brother is the youngest. He is nineteen years younger than I, and the three

sisters are spaced in-between.

BOHNING: As you went through school in Ohio, were there any teachers that had any particular influence on you?

PLUNKETT: Yes. For the first six or seven years, I went to a one-room schoolhouse. There were two or three different ones until about 1925, when my father and mother went back to Virginia for awhile. When I came back to Pleasant Hill I went into high school. I had a teacher there named William White. He was a math-science teacher and he had some influence on me. I remember one time that somebody asked me what I wanted to do. I said I wasn't sure what I wanted to do but I wanted to get my hands on the reins. There was another teacher by the name of Virden Thompson who I believe is still living in Pleasant Hill, Ohio. He was principal of the high school for a long number of years. He influenced me and my brother and sisters.

BOHNING: Did you have any chemistry when you were in high school?

PLUNKETT: I had very little chemistry in high school. This was a little country school, even though it was a consolidated school. They didn't get into chemistry very much, although Mr. White was interested in chemistry and I got a little interest from him. I don't remember it too well. At least he spurred my interest in chemistry.

BOHNING: What was it like growing up in a small country town in Ohio?

PLUNKETT: I was a young kid. My parents were Dunkards, Church of the Brethren Dunkards. I was raised pretty strictly in the teaching of the Dunkard Church. I was raised not to fight, turn the other cheek and so forth. I didn't get into much trouble in those days. [laughter]

BOHNING: You went to Manchester College in Indiana. How did that selection came about?

PLUNKETT: Manchester College is a church school of the Church of the Brethren. My family and I went to the Church of the Brethren in Pleasant Hill, Ohio. When I graduated from high school I was interested in going to college. It was the natural flow of things, to be interested in going to Manchester College.

BOHNING: In making your decision to go to college, had there been any influence from your family to do that, or was that something you did on your own?

PLUNKETT: I think the urge was as much mine as anybody's, but my mother definitely taught me to learn. There is one example I can tell about my mother. My younger sister came home one day and said to my mother, "So-and-so gets a dollar when she gets an 'A'." My mother said to her, "Well, you better worry about what you will get if you don't get an 'A'." [laughter]

BOHNING: Had you selected an area to concentrate in when you went to Manchester?

PLUNKETT: I had not. I think I didn't know enough at that point in time. I had enough to know that I wanted to start studying chemistry some, and I did start taking chemistry in Manchester. I was influenced a lot there. Dr. Carl W. Holl was a chemistry teacher and Dean of the College at Manchester when I was there. He had a Ph.D. from Ohio State. I had a lot of inspiration from him. I got on his staff in the college and did some laboratory assistantship work for him. He encouraged me and definitely had an influence in my going on to the university.

BOHNING: Can you describe the department at that time? How many people were in the chemistry department?

PLUNKETT: It was a small college. At that time, in the late 1920s and early 1930s, there was a total student population of five or six hundred. The chemistry department was pretty small. I would say there were only two on the staff of the chemistry department -- Dean Holl, and another man by the name of [Don] Martin. Martin graduated the year I was a freshman and went on the staff to teach as an assistant professor.

BOHNING: How many other students were there in chemistry?

PLUNKETT: If I think through the four years that I was there, there were probably eight or ten that were chemistry students. One of my friends who was at Manchester when I was there was later an outstanding chemistry student at Ohio State. He combined his schooling, doubling up summers and winters, and got through a year and a half ahead of me. He got his Ph.D. at Ohio State and as soon as I mention him, you're going to know him. He turned out to be a Nobel Laureate. That's Paul J. Flory.

BOHNING: Oh, yes. I should have made that connection earlier.

Did you have much interaction with Flory?

PLUNKETT: We had a lot of interaction. We started school at the same time. We were together essentially three years at Manchester. As I recall we roomed together with two other fellows at an outside boarding house for at least one year. We got into a lot of scrapes together as well.

BOHNING: What kind of scrapes, if I may ask?

PLUNKETT: Halloween pranks. [laughter]

BOHNING: That's very interesting. You said you roomed at a boarding house off campus. I know costs were quite different, but what kind of support did you have while you were going to school? How did you meet your expenses?

PLUNKETT: I only had a few sources. I had direct help from my father, what little bit he could do. I had help from the college and I had part time work. I did janitorial work. I did laboratory assistant work in the chemistry department and I did work downtown in a clothing store on weekends. Then in addition, I got some help, backed by my father, in loans from a banker back in my hometown.

BOHNING: You have already mentioned Professor Holl's influence on you. As you finished your four years at Manchester had you thought about trying to seek a job at that point or had you already decided to go on to graduate school?

PLUNKETT: I graduated from Manchester College in the spring of 1932. I don't know whether you remember that time or not but that was the absolute bottom of the Depression. I could not find any work tied in with chemistry at that time. At the urging of Dr. Holl, I wrote to practically all of the universities in the country. I wrote to one hundred and twenty-five universities, trying to get some assistance to get on their staff to go to school that fall. One of my very good friends was able to get in at Ohio State. There was only one who could get in, and he made the grade at Ohio State and I didn't. He was supported by Dr. Holl.

Well, it looked pretty hopeless to me. When I graduated in the spring of 1932, I owed the college a little bit of money and Dr. Holl said, "You stay on for six weeks for the first term of summer school and do some work for me and we'll wipe that out." I did that. I went home and helped my dad on the farm. What was I going to do? I couldn't find anything.

This friend who had gotten on the staff of the university was from a nearby town and he was going. We started to pool our resources and he said, "I'll help you out a little if you can get over there and get started." My dad didn't have anything. We were as poor as church mice. But he said, "I have a friend where we used to live that owes me fifteen bucks. If you can talk him out of that fifteen bucks, you can have it." I talked it out of him and I went to the university with that fifteen bucks in my pocket. And that's all I had. My friend loaned me a few bucks to help me pay the first fees I had to pay. I washed dishes three times a day for my meals. I got some help from one of the chemistry professors to do some laboratory work in connection with a practice teaching school nearby. I wormed my way into the university and eked my way through the first year and got on the staff for the second year.

BOHNING: When you say you got on the staff for your second year, does that mean you started to do some teaching?

PLUNKETT: I was a laboratory assistant.

BOHNING: What kind of courses did you take when you first started there?

PLUNKETT: I took nothing but chemistry essentially, except for some required things. I took German and French to meet my language requirements. I took advanced organic chemistry, advanced quantitative chemistry, and thermodynamics.

BOHNING: Do you recall the professors you had?

PLUNKETT: Yes. The head of the chemistry department was Dr. William Lloyd Evans. He was quite renowned in carbohydrate chemistry. I worked on him to take me on as a protégé. I had it in the back of my mind that this would help get me on the staff. So I went to work for Evans doing research in carbohydrate chemistry, and did get on the staff. I also did my thesis work in carbohydrate chemistry.

BOHNING: Did you continue your thesis work with Evans?

PLUNKETT: Yes I did. Another man on the staff was Melville Wolfrom. He was also a carbohydrate chemist and he was sort of an inspiration because he was sort of a renegade, one that you looked up to.

BOHNING: In what respect was he a renegade?

PLUNKETT: He was sort of rough-and-ready and gruff.

BOHNING: Not in keeping with the older school.

PLUNKETT: That's right, not in keeping with the older school. I remember Edward Mack. He was a physical chemist and I got inspiration from him. I made the remark that "all you have to do is listen to what that guy says. If you remember what he says you can get an "A" in his class." [laughter] He was good. I remember another physical chemist, Herrick Johnston. He taught thermodynamics. He would start out at the beginning of the class period way around in one corner and would write and talk, write and talk, and go around the room and cover the blackboard full of equations. If you didn't get them all down and remember them you didn't get anything out of his course. [laughter]

BOHNING: I was going to ask you earlier about your math background. You've now mentioned physical chemistry twice. What kind of math experience did you have?

PLUNKETT: I didn't have anything much in high school but algebra and geometry. In college I got advanced algebra and calculus. I didn't get any advanced mathematics there. I got some when I got into thermodynamics at the University.

BOHNING: You mentioned Paul Flory before. Did you interact with him at Ohio State?

PLUNKETT: Yes. He had been there a year and a half before I got there and was pretty well established. We used to socialize together and go to meetings together and things like that. We spent some summer-time activity together. His father was a Brethren minister and at the time lived in Elgin, Illinois. We visited back and forth. I remember one time while I was at the university we went to the Chicago World's Fair in the summer of 1933.

BOHNING: Are there any other student colleagues besides Flory that you remember?

PLUNKETT: I remember Homer Wilson, who was the one who loaned me a little bit of money to help me get started in my first year of graduate school. He was a pretty smart fellow. He went through and got his degree and worked for Rohm and Haas. But in the

meantime he had fallen in love with a Brethren girl and they got married. He left chemistry and went into missionary work. I lost track of him for a long time after that.

BOHNING: I realize this was still the Depression but what were your career goals while you were a student at Ohio State? Were you looking beyond that point?

PLUNKETT: Yes. It wasn't long after I got to Ohio State that I heard of the Du Pont Company. One of the ways that I heard of the company was through the fellowship Du Pont established at Columbus. As a matter of fact, Paul Flory had that fellowship for one year. We were there wondering if anybody was going to talk to us. I got to the Fall of 1935, and I was in my fourth year at the university. I had already taken my preliminary examinations and was doing my research work. I got a letter in November of 1935 wanting to know if I would come to Wilmington, Delaware to talk to some of the Du Pont people at Jackson Laboratory. The king wants to talk! [laughter] So I came to Wilmington on the train and went down to the Du Pont building to meet with a gentleman by the name of Ivan Gubelmann. He was one of the top men in the Organic Chemicals Department. I met with him and he told me how to get across the river to Jackson Laboratory. I went to Jackson Laboratory and there I met with the head of the laboratory Bill [William S.] Calcott. He had some tie-in with the neoprene work. I spent the whole day at Jackson Laboratory, talking with the people there. At the end of the day Calcott offered me a job. Well, it was a lot different in those days than it was some twenty or thirty years later when guys were getting eight to ten job offers. I had a job offer, and I said YES! [laughter]

BOHNING: I can appreciate that. Now you still had to go back and finish up.

PLUNKETT: I still had to back to Columbus and finish up. But I had a job! What do you know, the next week after I got back to the university I got another letter from Du Pont at Richmond, Virginia! [laughter]

BOHNING: My goodness.

PLUNKETT: But I had already accepted the job at Jackson Laboratory.

BOHNING: What was the Virginia offer?

PLUNKETT: The Virginia offer was in textile fibers. That was a rayon plant in Virginia. They had some nylon later on in the area and I guess they have Lycra there now. It was textile fibers. I get nostalgic about it, as you see already. I usually made up my mind in a hurry and if Du Pont's willing to talk to me, I wanted to go with them.

BOHNING: Had Flory gone down to Du Pont then?

PLUNKETT: Flory was already at Du Pont. He was at the Experimental Station, the forerunner of the Central Research Department. I think he was already working with Dr. Wallace H. Carothers.

BOHNING: Did you ever meet Carothers?

PLUNKETT: I met Carothers, but that's all. I never had any working relationship with him. I went to Jackson Laboratory. I had social contact with Flory afterwards because he met and married a Philadelphia girl and I had married a Columbus girl and we had a social life together.

BOHNING: What was your first assignment when you got to Du Pont?

PLUNKETT: When I got to Du Pont I was assigned to work in the fluorochemical field. I've got a story that ties in with what I'm going to talk about on Wednesday. Would you like to hear it?

BOHNING: Yes.

PLUNKETT: I'll give you a little bit of the story of how Teflon came about. I'm going to say Wednesday afternoon that I'm proud of my participation in this development, proud of the company with whom I've worked, proud of what has happened, and most of all I'm proud of the benefit to mankind from this original invention. The discovery of PTFE [polytetrafluoroethylene] has been variously described as an example of serendipity, a lucky accident, or a flash of genius. Perhaps all three were involved. But regardless, all are agreed as to the results of that discovery. It has revolutionized the plastic industry and led it into rigorous applications not otherwise possible.

I want to tell you a little bit about some of the things that had to occur before the discovery of PTFE could take place. In 1851 a U.S. patent was issued to Dr. John Gorrie for "an appliance for the artificial production of ice in tropical climates" (1). This can be considered to be a forerunner of the

household refrigerator. Next, the Belgian chemist [Frederick] Swarts did a lot of work on the substitution of fluorine for chlorine in organic molecules in the 1890s (2). He learned how to make chlorofluoro derivatives of carbon tetrachloride, chloroform, and similar compounds. They were essentially laboratory curiosities until the 1920s. By that time the household refrigerator had been developed to the point that it resembled today's machines. But there was one very serious drawback, and that was the refrigerants. They used such things as ethylene, sulfur dioxide, ammonia, and methyl chloride which were flammable or otherwise noxious materials. The refrigeration machines used in those days were not nearly as good as the machines of today. They would leak and discharge flammable and noxious gases, much to the consternation of the housewife.

[END OF TAPE, SIDE 1]

PLUNKETT: "Boss" [Charles F.] Kettering of General Motors fame was very much interested in the refrigerator business and was concerned about the lack of adequate refrigerants. He called upon Tom Midgley, Albert Henne, and Robert McNary to look for a safe refrigerant, one that would be colorless, odorless, tasteless, non-toxic, and non-flammable. These gentlemen then made a study of all the known compounds that might fit the bill, from a physical constant standpoint, as refrigerants and tabulated them and plotted them in accordance with the atomic table. Their deliberations led them to postulate that a fluoride or a fluorocarbon might be suitable. They were not aware, of course, that all fluorine compounds were poisonous. At least, that's what most people thought in those days. So they decided to repeat some of Swarts' work and actually did make some CCl_2F_2 and found that its physical properties were such that it might be useful.

Midgley reported at an ACS meeting in Atlanta in 1930 that they had tested CCl_2F_2 and that it had promise as a refrigerant. They demonstrated it and said it could be safe -- you could smell it and you could stick a match in it. Midgley reported later when he was being awarded the Perkin Medal that there might have been some fortuitous circumstances in their work. He called up a supply house and ordered five one-ounce bottles of antimony trifluoride, the entire country's supply as far as he knew. They used one bottle of antimony trifluoride and and produced some CCl_2F_2. They put it into a bell jar with a guinea pig and the guinea pig just lapped it up and said, "Boy, this is fine." They were elated that they had made something and it looked like it might be worthwhile. Later they used another bottle of antimony trifluoride and repeated the experiments. They did everything as before but this time the guinea pig kicked up its heels and died. They were dismayed. On further examination they found that of the five bottles of antimony trifluoride Midgley had originally gotten, one of them was anhydrous and the rest had water of crystallization. Of course, when they reacted that with HF, they

got some phosgene-like materials, and that was the cause of their problem. If they had not chosen the anhydrous bottle first they might have given it up as a bad hunch. That was the start.

The people at Du Pont carried on and developed manufacturing processes for a total of five fluorochloro chemicals over the first five years of the 1930s and marketed them through Kinetic Chemicals Incorporated, a joint venture of Du Pont and General Motors.

BOHNING: Do you know anything about the origin of that name, Kinetic Chemicals?

PLUNKETT: I don't know anything specific about how they chose the word kinetic. No, I don't, and I guess I was too naïve to even ask the question myself in those days. [laughter]

BOHNING: How long did that company last?

PLUNKETT: That company lasted for fifteen to twenty years, essentially the life of the original commercial patent for refrigerant 12, as Freon refrigerant. When it was dissolved, Du Pont bought out the General Motors interest and Du Pont continued to manufacture refrigerants.

BOHNING: Now this was before you joined Du Pont? Who did you report to when you first arrived?

PLUNKETT: I was assigned to work in the fluorocarbon division. Frederick B. Downing was the division head and Anthony F. Benning was group leader of the group to which I was directly assigned. The first couple of years that I was at Du Pont I did whatever jobs came along working on fluorochemical compounds. Early in 1938 a crisis arose because of the proprietary ownership by Frigidaire of $CF_2Cl\text{-}CF_2Cl$ (refrigerant 114) which they would not permit to be sold to any competing refrigerator manufacturers. Other refrigerator manufacturers were asking Du Pont to do something. "Find us a way to get something we can use in competition with this material." I was assigned to work on the problem of getting another refrigerant. I and my associates decided to make $CF_2Cl\text{-}CF_2H$, a compound which we thought would have some similar refrigerant properties to Freon 114 which was already being produced.

I set out on a proposition to make this material. Up until that time, tetrafluoroethylene [TFE] was almost a laboratory curiosity. It had been reported as being made a number of times in the early literature, but the circumstances were not too clear cut. In 1933 [Otto] Ruff had reported making TFE by decomposing

CF_4 in an electric arc (3). He took the crude TFE that was produced and reacted it with bromine to get the dibromotetra-fluoroethane compound. Then he pulled the bromine out with zinc in ethanol and got a purified TFE. He characterized the compound and determined some of the physical properties.

A year or so later Henne reported pulling Cl_2 out of tetrafluorodichloroethane in a similar manner (4). I decided to try to make the compound tetrafluorochloroethane by making some TFE and then adding HCl to it. I chose to make the TFE by pulling the chlorine out of the tetrafluorodichloroethane. I was ambitious and decided to make one hundred pounds of the material. I did set up the apparatus and did convert the material to about one hundred pounds of TFE and then purified it in the laboratory by distillation. I stored it in small cylinders in a dry ice chamber and set it aside to use later to make the proposed refrigerant.

BOHNING: Why did you select one hundred pounds? Was there any reason you made such a large amount of it at the beginning?

PLUNKETT: We wanted to have enough to evaluate it as a refrigerant and to evaluate it thoroughly toxicologically for its effect on animals. So it just seemed like a round number.

BOHNING: You stored this in small cylinders?

PLUNKETT: Yes.

BOHNING: You must have had a huge collection of cylinders.

PLUNKETT: Yes, I had quite a few. I set the limit at one to two pounds per cylinder so there were about fifty cylinders. I scrounged bug-bomb type cylinders and small cylinders of all kinds. I rinsed them out and used them to store my TFE.

BOHNING: They were all kept at low temperatures?

PLUNKETT: Yes, at dry ice temperature.

BOHNING: What happened on April 6, 1938?

PLUNKETT: After having made the TFE, I then had to set up a laboratory apparatus to add HCl to the TFE. I would put the cylinder of TFE on a scale and vaporize the TFE through a

metering device into a electric-heated carbon-bed catalyst tube reactor. At the same time I was leading from another system a stream of HCl which I generated. That was the way I was making the tetrafluorochloroethane.

On the morning of April 6, 1938, after having selected one of the cylinders we had been using previously, my helper said, "Hey Doc, did we use all of the gas in this cylinder?" And I said, "No, I don't think so." He said, "Well, nothing's coming out. I don't know what the heck is wrong." On further examination it was quite evident that there was material in the cylinder but there was no gas pressure.

BOHNING: That was from the weight of the cylinder?

PLUNKETT: Right. From the weight, there was still material in the cylinder but there was no pressure and the valve was completely open. No gas would come out. On further checking, we actually opened up the cylinder and let a wire down through the valve to be sure there was no gas pressure. Later, we took the valve out of the cylinder and shook out some powder which proved to be the first evidence of a polymer of TFE.

BOHNING: How did you conclude that polymerization had taken place? Were you aware of polymerization as a chemical phenomenon?

PLUNKETT: I was familiar with the fact that monomers do polymerize and you make polymers out of polymerizing monomers. I was not aware that TFE would polymerize. As a matter of fact, I think the teaching of that time would be that no fully halogenated ethylene molecule would polymerize. This was like a proverbial bumblebee. It didn't know it couldn't polymerize and went right ahead and did so. Somebody asked me in the past, "What was your reaction?" My reaction was, "Well, we have to start over now. Particularly if all of this other stuff has polymerized like this."

BOHNING: You had made a large amount of HCl addition product before this particular incident happened. So this wasn't the first cylinder that you used.

PLUNKETT: This wasn't the first one. How many cylinders we had used is a hazy memory now. I don't know how many we had. I know we had made a fair amount of the proposed refrigerant and we still had a lot of cylinders of TFE left.

BOHNING: You also mentioned your helper. What was his name?

PLUNKETT: His name was Jack Rebok. He was a local, southern New Jersey boy from Paulsboro, New Jersey who came to work with Du Pont after he finished high school.

BOHNING: Did he have any reaction when this occurred?

PLUNKETT: His reaction was, "What the hell is going on, Doc?" And he thought the same thing I did. "Well, we're going to have to start all over. We're going to have to make this stuff."

BOHNING: When did you realize that that white powder had unusual properties?

PLUNKETT: Not right away because all I could see was the powder. I soon started to examine some of it to see whether I could dissolve it in something. I couldn't find anything in the laboratory that would dissolve it at all in any shape or form. It didn't react with anything. There were no chemical reactions that I could determine so those facts themselves said that there was something unusual about this material.

BOHNING: Did you do that the same day?

PLUNKETT: I did some of it the same day.

BOHNING: Did you eventually work your way through the rest of the cylinders to see what they contained?

PLUNKETT: Well, I don't remember how many cylinders were cut open but there was a substantial number.

BOHNING: Certainly more than one.

PLUNKETT: Yes.

BOHNING: Probably even more than two or three?

PLUNKETT: Right.

BOHNING: In terms of the patents you've had, the Teflon patent didn't come until 1941 and the others came in 1946 and later.

PLUNKETT: Right. After having found out that the TFE does polymerize, one of the things I set about to do soon thereafter was to make TFE polymerize. I did set up some experiments in which TFE was put into glass tubes without any designated catalyst, with potential catalysts, and with a solvent. Under all conditions I did get some polymerization. Those studies provided the basis for the patent claims on the original patent of PTFE as a composition of matter and how to produce it (5).

BOHNING: But the other processes came later. You filed for a patent in 1939 and it was granted in early 1941. You also had patents on the preparation of TFE and on the CHF_2-$CClF_2$, but they came after the war (6).

PLUNKETT: Right. They came afterwards because they got caught in the Patent Office secrecy order to cover up all the work on fluorine chemistry.

BOHNING: This probably comes a little bit later but I wanted to ask it now. You refer to it as PTFE. When did the name Teflon come into existence?

PLUNKETT: The Teflon name for PTFE resin was coined in 1944.

BOHNING: Oh, that much later. Were commercial uses coming into being at that time?

PLUNKETT: That was still 1944 and we were still under wartime secrecy. Du Pont had made some evaluations of potential uses. Maybe we should back up a little bit. The next question was, "What are you going to do with this stuff?" I didn't know much about what to do. I didn't know anything about polymer chemistry itself but I was fortunate to be with the Du Pont Company who had a lot of chemists and engineers doing work in polymer fields. So we asked some of our associates in the Central Research Department to take some of this powder and see if they could mold it in some way and characterize it a bit. They did, using powder metallurgical techniques. They compressed the powder under pressure and temperature in a mold, and did do some molding. By examination of that we started to get some physical properties. This led of course to examinations and postulations of what could be done with it, what it would take to manufacture it, and what it was going to cost. Those deliberations at that point in time weren't too encouraging. This stuff was going to cost so much that nobody was ever going to buy it.

BOHNING: Were those decisions made by marketing people?

PLUNKETT: I don't know whether the marketing people had gotten into it at that time or not. There were probably deliberations only by research people at that time. But, then we came to the time when the country formed the Manhattan Project. The Manhattan Project people began to study ways and means to separate uranium isotopes and found out that they wanted to make fluorides of uranium and try to separate them that way. They recognized that they were going to need a lot of materials. Du Pont reviewed this with some of the Manhattan Project people. When General [Leslie] Groves, who was director of the Manhattan Project, heard about this new plastic material, he said, "That sounds like something we're going to need. You had better get a hold of it and develop it or we're going to take it away from you." That was about the way it was. [laughter]

And so with that encouragement, and with the interest of Du Pont people, work was stepped up on the fundamental studies and to develop some techniques that could at least be a basis for a pilot plant. In the early 1940s there actually were pilot plants set up to make TFE, to polymerize TFE, and to mold and fabricate PTFE. All three separate strings were coming together. This work was going on while they were making stuff. I think one of the Du Pont men was talking this morning about Du Pont's experience in fluorochemicals and said all of it that was made during the whole war period wasn't very much. But a lot of it got used by the military and by the Manhattan Project. I'm sure that Du Pont had opportunities to evaluate it for some of their own uses at that time.

There is one point I can relate to you. At the Chambers Works they had a dye process that involved nitrations with seventy percent fuming nitric acid. This mixture had to be pumped and it was a very corrosive operation. The pump failed at least once a week and had to be torn down and rebuilt. One of the early tests of Teflon was to pack this pump and valves with Teflon packing. It ran so well that six months later, as I remember the story, it was decided to tear down the pump assembly to see how it had stood up. It was torn down and everything was still working alright. So there had been some cases where it might be too expensive, but here was a case in point where it was cheap at any price.

BOHNING: Does your original laboratory notebook still exist?

PLUNKETT: My original notebook which records my observations of the day that PTFE was discovered does exist. It's in the hands of Du Pont and recently some of the Du Pont European people made some copies of this two page spread and blew them up and asked me to sign numerous copies of them. If you talk to Lana Kirch maybe you could get a copy.

BOHNING: We'll have to see if we can get a hold of one. That

would be excellent to have (7).

There are a number of things that are curious to me after this event occurred in April of 1938. Your involvement changed rather quickly.

PLUNKETT: My involvement changed within a very short period of time. This had nothing to do with what I wanted to do or what I might not want to do or what somebody else wanted to do. There was another fellow that was working on tetraethyl lead research and was getting a promotion. They took him out of doing direct laboratory work to be assistant director of the laboratory. They needed somebody to pursue that tetraethyl lead work. I was chosen to do that. So within months after the discovery I was transferred into doing tetraethyl lead work. I know it was within months because on January 1 of 1939 I was transferred from the laboratory to the tetraethyl lead manufacturing organization as chief chemist.

BOHNING: What was your position before that?

PLUNKETT: Research chemist.

BOHNING: In the Jackson Laboratories?

PLUNKETT: Yes. I was a research chemist assigned to fluorine and a research chemist assigned to lead and then I got transferred from the research organization to the manufacturing organization in tetraethyl lead.

BOHNING: You were with the tetraethyl lead plant from 1939 to 1949?

PLUNKETT: Right. In 1939 when I was transferred to the tetraethyl lead organization the use of antiknock compounds were expanding rapidly and continued to do so for the next year or two.

[END OF TAPE, SIDE 2]

PLUNKETT: Then along came Pearl Harbor and the U.S. involvement in World War II. One of the first things that happened was that the government stopped the manufacture of automobiles. Stopping the manufacture of automobiles stopped the demand for tetraethyl lead. We in the operations group that was making tetraethyl lead were faced with what to do. What we did was to convert our

facilities from making tetraethyl lead to making one of the synthetic rubbers. We developed a process for making synthetic rubber using our tetraethyl equipment. We just got to the point where we had a conversion and were starting to make synthetic rubber when Roosevelt said we're going to build millions of airplanes -- warplanes! Then all hell broke loose. [laughter] We had to get back into making tetraethyl lead! [laughter]

BOHNING: How long were you involved with synthetic rubber before you moved back?

PLUNKETT: About a year or a year and a half.

BOHNING: What was your function in that process?

PLUNKETT: In the tetraethyl lead organization I was the chief chemist. We were just all given the job of converting to rubber manufacturing, and I kept the same position.

BOHNING: Were you actually involved in the manufacturing process?

PLUNKETT: I was involved in developing the manufacturing process and the conversion process. Actually we were working with one of the Thiokol rubbers. We met with the Thiokol people and got their process information which we tried to transmit to fit the kind of apparatus we had.

BOHNING: I notice you have three patents on tetraethyl lead (8).

PLUNKETT: Yes, I have some patents on tetraethyl lead. They were on methods of improving the yield of tetraethyl lead in the manufacturing process during the reaction of ethyl chloride with lead monosodium alloy.

BOHNING: It's not clear in my mind what you did after 1945.

PLUNKETT: In 1945, which was approaching the end of the war period, we had an organizational upheaval in the tetraethyl lead groups. The head man (the superintendent) and the second head man (the chief supervisor) were removed. They picked up Plunkett and said, "You're boss." So I got promoted to being superintendent of the tetraethyl lead area in early 1945. I had that position until 1949 at which time I was transferred to a similar position. I was made the superintendent of the plant

producing vat dyes.

BOHNING: It was the Ponsol colors area. What does the word Ponsol mean?.

PLUNKETT: Ponsol was a part of a trademark for vat dyes.

BOHNING: So in that 1945-1949 period, I can imagine TEL [tetraethyl lead] became very important again.

PLUNKETT: TEL became very important again. Until then, Du Pont and Ethyl Corporation had a manufacturing and service agreement type of arrangement. Then in the period of time we're talking about Du Pont and Ethyl Corporation split. Du Pont went out to manufacture and sell tetraethyl lead on their own and Ethyl Corporation went out to manufacture and sell tetraethyl lead on their own. I was deeply involved in working out the details of that schism because we had a manufacturing plant at Chambers Works and one that we were operating for Ethyl Corporation in Baton Rouge, Louisiana. I spent half of my time on the train going back and forth to Baton Rouge for several years.

BOHNING: Now with the dye area...

PLUNKETT: I was transferred in the latter part of 1949 into this dye operation as superintendent over the top of everyone else who was there. I was a bad boy, and I was resented. I only stayed there nine months. In June of 1950 I got promoted to assistant plant manager of the Chambers Works. All of the resentment that I felt when I moved into that dye business was gone by that time.

BOHNING: Do you think that resentment was in part due to your association with Teflon?

PLUNKETT: At that point in time it wasn't that clear that I was associated with the development of Teflon. It hadn't yet become too widely known. The first public recognition that I received as inventor of Teflon was in 1951 when I was awarded the John Scott Medal by the city of Philadelphia. That was in 1951 but I didn't receive the medal until June of 1952. Du Pont had several hundred people come to a banquet at a country club in Penns Grove, New Jersey. I was presented with the John Scott Medal and they had a big to-do. Professor [Hubert N.] Alyea from Princeton came down and talked about it. He eulogized my work. That was the first public recognition of many. Also, it was the first time that anybody in this country saw any cooking utensils lined with Teflon. They made muffin tins lined with Teflon and gave

them out as favors to all the attendees of this banquet. Some of those muffin tins are still in existence. I have one of them at home.

BOHNING: In talking about this as the first public recognition that you had, how did the company react? It must have taken some time before the full impact of what you had done was recognized.

PLUNKETT: I think it did take quite awhile. Of course, it didn't amount to anything financially to Du Pont until probably 1950. Maybe later because during its early stages it was costing them a hell of a lot more to produce it than they were getting for it. Even though they were selling it for twenty-five dollars a pound or something like that and even though little gasket companies like the U.S. Gasket Company in Camden, New Jersey had the Teflon delivered to their bank vault in Camden. If they wanted to use some, they had to go over to the bank to get it. [laughter] That's an absolute true story.

BOHNING: Do you think the company eventually recognized you properly?

PLUNKETT: Oh the company recognized me. There's no question in [Edward G.] Jefferson's mind who's responsible for Teflon. I hadn't seen Ed for several years until after I retired down at Corpus Christi in 1983, which was the tenth anniversary of the set-up of the Corpus Christi plant. Ed was down there to give a talk. I had seen him just shortly before the talk. Ed used the occasion to recognize me and eulogize me right there on the spot.

BOHNING: That's wonderful.

PLUNKETT: Last year when I got notification that I had been nominated for the Inventor's Hall of Fame, I got a call from the senior vice president of the Central Research Department. He called and told me that Ed [Jefferson] had asked him to call me. A day or two later I got a letter from Ed telling me about about this. This was in early December of 1984. In early January of 1985, I got a call from the Public Affairs Department. Lois was home and I was on the golf course. They told her, "Well, have him call. How would he like to have a vacation in Delaware?" She said, "What, in January?" [laughter]

I did go that next week and they had been commissioned to see that I was properly recognized. I came up there and spent a whole day talking to a group of people and spent a day in the photo shop and things like that. When it came to the time of the presentation of the award, the inductees were feted at a banquet on Saturday night and the award presentation took place on Sunday

afternoon.

I was called to Wilmington several days ahead of this presentation and they had the public relations group from Philadelphia to come and spend almost a day interviewing me. Then I spent more time with the photographers. Then I went to Washington. There was a public relations outfit down there to set up interviews with me from all of the TV, radio, newspapers, and magazines. I was in Washington from Friday until Tuesday evening and I was constantly in a Mercedes limo being pushed from one place to the other. Du Pont had a press breakfast on Tuesday morning following the presentation of the award, to which they had invited a lot of people to come. It was a big press breakfast they had set up in my honor. They had a big banner up, "A New Name in the Hall of Fame," and they had several of the Du Pont people including the head of the Central Research Department. They had somebody from Bell Laboratories and the engineer in charge of restoring the Statue of Liberty, and several others. I got up and said a few words.

On Monday morning there was an article in the Washington Post and it seemed the White House saw this article in the Washington Post which was about me and some others, but this story was mainly about me. The President was having a luncheon on Tuesday of one hundred scientists and engineers and he said, "Get these guys here." So I had lunch with the president on Tuesday.

But on Sunday, after I got the award, the first thing I did was to get in an airplane and fly to New York to appear on the Today show on Monday morning and be interviewed by Jane Pauley and fly back to Washington before noon. I think they recognized me.

BOHNING: The reason behind that original question was that the company itself took awhile to realize the impact of the work.

PLUNKETT: Oh, yes. There was no question that I was a god, because if you have a list of the Teflon patents, it's the most widely patented material the Du Pont company ever had by far. Through the years, I was continually being called up to sign a patentapplication. Every time it was patented in a new country, I had to sign another patent application. There was no question about who made the discovery. Whether I was naïve or whatever -- I made the discovery and I knew what I had chemically. I didn't know all it was valued for but I had made the discovery. I say sometimes when I talk to high school kids -- I've done that a number of times -- one of the things you go to school for is to learn how to think. You have to have a prepared mind when you get into situations like this. And I think I had a prepared mind.

BOHNING: I am intrigued by the fact that you worked very quickly on the properties and were able to determine exactly how unusual this material was. The next step then was to perfect the production of the polymer.

PLUNKETT: It was to demonstrate that I could make something.

BOHNING: So then you had a way of doing it routinely.

PLUNKETT: At least I had a basis for a patent. I don't know if I had a basis for a routine. Some other people got in to do that. But I did immediately do something to try to find out what it was.

BOHNING: Let's go back to 1940.

PLUNKETT: The Manhattan District work gave the impetus to develop the processes for making and handling and fabricating Teflon. That work was successful and they found a lot of military applications and numerous applications in the chemical processes in connection with the Manhattan Project. One of the first military uses was in the nose cone for proximity fuses on proximity bombs. It would transmit the radio waves for awhile. That was one of the first uses. But it got a lot of uses, even in the early days, in electrical and electronic applications. Of course those are the greatest users today. The tele-communications field is the single greatest application of Teflon even today. At the end of World War II, patent secrecy was lifted, and at the end of 1946, there were publications and patents granted. There were about thirty patents granted in 1946 related to Teflon materials.

BOHNING: Were these all Du Pont patents?

PLUNKETT: I think that I'm talking only about Du Pont now. No, I doubt that there were because Du Pont had apparently gotten some other people working on it by that time. It's one of the things about it because there's been many chemists and engineers and entrepreneurs that have contributed to the development of Teflon and PTFE and all of its uses. But in 1946, the Du Pont Experimental Station people put out a paper in which they described some of the work done during World War II (9). The Plastics Department people put out a paper in Industrial and Engineering Chemistry in which they described the processing applications (10). There had to be lots of inventions just to learn how to handle the damn stuff. One of the things was a ram extrusion press in which you feed the powder in through a bin and compress it in a heated zone and come out with a rod or tube or

something. It was processing Teflon under controlled conditions. They would come out with little rods to big tubes to great big batches of the stuff. Obviously, it took an awful lot of work and people to develop it.

BOHNING: Who were some of the people that worked in the area after you left it, that picked up where you left off?

PLUNKETT: Butch [William E.] Hanford and Bob [Robert M.] Joyce were two people that did work in the early 1940s in the Central Research Department and they published the 1946 paper (9).

BOHNING:: Was their work mostly characterization?

PLUNKETT: Their work was process work for methods of polymerization. They developed the basis for the emulsion polymerization process and they characterized the polymer. They did some of the early characterization work. Then in the Plastics Department, there was Malcolm Renfrew.

BOHNING: I talked to him just yesterday.

PLUNKETT: You did?

BOHNING: Yes. He's here for the ACS meeting.

PLUNKETT: Yes. He and [Ernest E.] Lewis were both at Arlington at the time and they did a lot of the work in connection with developing techniques of handling the polymer.

BOHNING: I didn't realize Malcolm was involved. What years would that have been?

PLUNKETT: The early 1940s, because the paper was published in 1946 (10). It was after the secrecy was lifted. Then of course the process for making TFE from the pyrolysis of $CHClF_2$. That was developed and perfected by the people at Jackson Laboratory, [Anthony F.] Benning and [Robert C.] McHarness (11).

BOHNING: Did you keep up with what was going on in Teflon while you were over in TEL?

PLUNKETT: I didn't keep up as much as I might have because I was

not in on the secrecy of the Manhattan Project work. I knew the Manhattan Project was going on because we had the pilot plants there in the Chambers Works, right next door. I was aware that the work was going on but I was not part of it. I was one of the first guys that was called after the bomb was dropped in 1945. I got a call from one of my bosses who said that they just dropped a bomb on Hiroshima.

I have a couple of paragraphs in my talk which indicate that from stove tops to outer space, from electronics to ecology, from architecture to the Statue of Liberty, Teflon touches everyone of us some way almost every day and it is an integral part of all of the things that makes the machinery of commerce work today.

Then I want to add one more thing at the end. It's been exciting to be a part of this thing, to have a part in it and to know that it's been a benefit to real people, not just to imaginary people but real people that I know. Several years ago, I attended a dance near Wilmington and a doctor friend of mine said, "Roy, I want you to meet somebody here. See that gentleman out here dancing. He's here dancing only because he's wearing a Teflon aorta which I installed." So I add to that, that over the years I've experienced or heard of many instances like that and it makes me realize that I've been more than just helpful.

BOHNING: Absolutely.

[END OF TAPE, SIDE 3]

BOHNING: How do you view today what occurred back on April 6, 1938? You essentially answered your reaction to that. I'm sure you couldn't imagine at that time what was going to come in the future but you evidently had some idea that this was something very important that was going to develop.

PLUNKETT: In a certain period of time I realized that it had potential and I've indicated that there were times when there were doubts over the economics of it. That certainly has been proven out again and again.

BOHNING: Did you have trouble convincing anyone of what had occurred then?

PLUNKETT: No I don't think I had any problem convincing anybody of what had taken place. I knew what had taken place and I think they knew.

BOHNING: How did your colleagues react to that when they heard

the news?

PLUNKETT: It's too hazy in my mind. I don't know what happened there. It took quite awhile before there was anything significant.

BOHNING: We left off at 1952 when you moved to the Organic Chemicals Department.

PLUNKETT: Oh, yes. In 1950 I became assistant manager of the Chambers Works. I spent a year and a half in that position. In January of 1952 I was transferred to Wilmington as manager of the plants chemical section for the Organic Chemicals Department. I had charge of the chemical development on all of the organic chemicals department plants. A little bit later I became assistant manager of the plants technical group which put engineering and chemistry together and later I was manager of the plants technical organization. That lasted until about 1960 when the Organic Chemicals Department had a major change in organization. They divided the businesses into several business groups. One of them was the Freon business group. There was a business director. I became director of research for the Freon Products Division in 1960. I spent ten years in that position.

BOHNING: You were almost coming back to where you had been at the beginning.

PLUNKETT: Right. During that period from 1960 to 1970, my staff and I developed some new processes for large scale production and integration of the manufacturing of Freon products and formed the basis for establishing the plant in Corpus Christi, Texas. In 1970 I was moved out of research and was given the job of director of operations. The main job was to get that plant into being.

BOHNING: That explains why you're in Texas now. [laughter]

PLUNKETT: That explains how I got to Texas. [laughter]

BOHNING: I had wondered about that. I remember people talking about your retiring to Florida. I wondered how you had arrived at Texas.

PLUNKETT: I probably would have ended up in Florida if it hadn't been for Texas. I fell in love with Texas when I became acquainted with the people. Of course I was in Wilmington all of

the time. I was not personally down there, but I was in charge of the plant. I went back and forth to Texas once a month for five years and I got kind of familiar with how to get there.

BOHNING: I have read somewhere that you are quite a collector of antiques.

PLUNKETT: I did do quite a bit of collecting of antiques back in the 1950s and earlier. I did a lot of restoration. My previous wife and I did collect antiques. I did furniture restoration and bought an old house. It was one of the taverns on the King's Highway in southern New Jersey. I spent a little time there. Lois and I have some antiques now but we're not doing as much as I did earlier.

BOHNING: I also understand that you have two sons.

PLUNKETT: Yes. Michael will be forty-five this fall. He is a technical representative for Du Pont's carpet fibers and works out of the Charlotte office for the Textile Fibers Department. He apparently is a very much respected person in his field. At least he's still being sent to a lot of meetings and is pretty effective in the carpet fiber sales business.

My other son, Patrick, will be forty-two this fall. He is sort of a renegade. He was always pretty lazy but pretty smart. He came back from Vietnam in 1969 and said, "Dad, I'm going to be a ski bum for a year." And that lasted about eight years. I guess he was a pretty good ski bum. He's located in Washington, D.C. now and is a computer software consultant. He and two or three other fellows have formed a little company which they call Internet. The name almost tells you what it is. They're doing network studies for connecting computers. He just recently got married and is looking forward to raising a family.

BOHNING: One of the reasons I asked a question like that is curiosity about whether they may have followed you scientifically.

PLUNKETT: Neither one of them did. Michael came closer. He got a degree in mechanical engineering but somewhere along the line the magic left him. They had an interest in the early days when they were small. "Daddy, that's what I want to do. I want to become a chemist." In the early days Patrick had a little chemistry set. In 1955 when the boys were about fourteen and eleven, and I was away in Trinidad, they blew up that chemistry set. Both of them went into the hospital. The younger one had a piece of shrapnel that went through his forearm and severed off the artery. It's still severed. My other son, who was closer to

the set, got burns on both eyes but he didn't loose any sight. So, I don't know whether that pushed them away from science or not.

BOHNING: It's amazing that there are a number of people with established careers in polymer science whose children never went into chemistry.

Is there anything else that you would like to add at this point that we haven't talked about? We've primarily concentrated on the early period.

PLUNKETT: I think that that pretty well covers it. I can relate to you a couple of instances. I mentioned earlier that during the years I've gotten a lot of honors. Do you have one of my bio write-ups?

BOHNING: Yes, I have some information, but it's back in Philadelphia.

PLUNKETT: Did that come from Du Pont?

BOHNING: Yes.

PLUNKETT: I've received a whole array of various types of honors -- Modern Pioneer in American Industry, Modern Pioneer in the Institute of Chemists. They're all recorded in this bio-script. I've gotten three honorary D.Sc. degrees. The first one I got from Manchester College in 1952. The second one I got from Washington College in Maryland in 1968. The last one I got from Ohio State in 1977. And after that one, until I got the National Inventor's Hall of Fame, I thought I had arrived. [laughter] Somebody said to me at the time of my induction into the Inventor's Hall of Fame, "What do you think about this?" I said, "Well, I don't know. I'm riding on cloud nine but I wonder what the hell a little old country boy from Ohio is doing in this crowd."

BOHNING: Well, there were a lot of people with similar backgrounds. Paul Flory is one example. Harold Urey was a country boy from Indiana.

PLUNKETT: Jane Pauley said, "Well, Edison was a country boy." [laughter] When she shook hands with me when I finished my interview with her she said, "I think Edison is in good company."

BOHNING: When did that interview take place?

PLUNKETT: February 11 in 1985.

BOHNING: We might see if we can get a hold of a copy.

PLUNKETT: I think Lana Kirch may be able to get you a tape of that. A public relations firm in Philadelphia were the ones who arranged it.

BOHNING: I guess that brings us to a close. Again, I would like to thank you very much for taking the time to chat with me this afternoon.

PLUNKETT: I'm happy to cooperate, Jim. It's important to do these things.

[END OF TAPE, SIDE 4]

INTERVIEWEE: Roy J. Plunkett

INTERVIEWER: James J. Bohning

LOCATION: Beckman Center for the History of Chemistry, Philadelphia

DATE: 27 May 1986

BOHNING: Dr. Plunkett, I want to thank you again for agreeing to spend some time with me this morning. In reviewing the transcript of the talk we had in New York, one of the areas I want to go back and perhaps get a little more information on is your association with Paul Flory. When did you first meet Paul Flory?

PLUNKETT: I first met Paul Flory when we entered Manchester College in the fall of 1927.

BOHNING: I believe you mentioned that his parents and your parents knew each other.

PLUNKETT: That is not true. My family were members of the Church of the Brethren and Paul's father was a minister in the Church of the Brethren. Paul's uncle was also a minister in the Church of the Brethren and he was the minister for a period of time at the church that my family attended in Ohio.

BOHNING: But you never met him before you went to Manchester?

PLUNKETT: No. The other Flory had a son who went to Manchester at the same time. So Paul and his first cousin were at Manchester at the same time.

BOHNING: Do you remember the names of the cousin and the other minister?

PLUNKETT: The cousin's name was John. I don't remember his father's name.

BOHNING: I think you said that you and Flory also roomed together when you were at Manchester.

PLUNKETT: I think that Paul and I roomed together in the third year that I was at Manchester. He and I and two other fellows roomed together. We had the upstairs of a nearby house that was a certified rooming house for the college. We roomed together that year which was the last year that he was at the college. It was my third year. He got ahead by going to summer school and finished up the following summer. He then went on to the university. I was out what would have been my fourth year because my father was ill and I had to run the farm.

BOHNING: I see. Let me go back to your first meeting with Flory. I assume you took the same classes together. Did you both come in as chemistry majors?

PLUNKETT: When we came in to the college I don't know that we decided that we were going to be chemistry majors. But it so happened that we did essentially take the same classes the first year and the second year.

BOHNING: Did you socialize at all during that time or was your contact mainly through classes?

PLUNKETT: It was a small college. The student body was not more than about six hundred at that time. That means the freshmen class was probably two hundred. It drifted down a bit toward the senior class so that you pretty well got to know everybody. Yes, we did socialize in college functions.

BOHNING: What about during the summers when you were back home?

PLUNKETT: He lived in Illinois and I lived in Ohio but I remember one summer. This was after college because we both went together to the World's Fair in Chicago. I believe it was the summer of 1933. That was after we had both graduated.

BOHNING: Could you tell me something about Flory in terms of how you remember him as a student? What he was like as a student in the classes you attended?

PLUNKETT: I always recognized Paul as being a very smart fellow. He knew what he was doing, where he was going, and he was always at the lead of any group that he was associated with. I tried to keep up with him but I couldn't quite do that. He not only exhibited that he always wanted to be at the top -- he was always there. I'm sure that he always had the urge to be there. It was always my goal. I didn't like to be second best. I never was always best, but I didn't like to be second best. I think Paul

inspired everybody he worked and associated with. He was recognized as a leader in whatever way. This sometimes carried over to mischief too. So on mischief nights such as Halloween, he would get into mischief and he was a good leader for mischief too. [laughter]

BOHNING: Are there any specific examples that you care to share?

PLUNKETT: I'll share one. One Halloween, we found an old horse-drawn milk wagon that was a mile or so from the college. Paul and I and a few other fellows -- I don't specifically remember who they were -- decided that we were going to get that milk wagon and take it into the auditorium and set it up on the stage. And we did that. Halloween night we went to bed and set our alarms for about one o'clock, got up, went out and got the milk wagon, and pushed it up to the administration building. We got the side doors open to the auditorium, took the wheels off the wagon, carried the body into the auditorium, put it up on stage, and then put the wheels back on. Then we went home and went back to bed. [laughter]

Part of our glee was taken out of it though because when we got up the next morning, the president had found it, got somebody else to take it out and we couldn't tell anybody. [laughter]

BOHNING: How did the faculty at Manchester respond to him?

PLUNKETT: I think by and large he got A's in practically all of his classes -- not all of them. The dean of the college and the head of the chemistry department responded to him very much because as he got interested in chemistry, the dean encouraged him to try a few things. He had him doing experiments, I believe, at the beginning of his third year. He just inspired everybody. He was proper. He was not a braggart but he also wasn't too modest. We knew he was there. I remember one time when I gave a talk at Cornell. I was talking to him afterwards and he said, "Don't be so damn modest." [laughter]

BOHNING: Let me back up a moment. Had he focused on chemistry early or do you know if he was considering other possibilities at Manchester?

PLUNKETT: I'm not sure. He finished up his third year at Manchester in 1930. He had enough to qualify for his degree so that he went to university in the fall of 1930. By that time he had certainly settled on chemistry. He then went right straight through at Ohio State and I think he got his Ph.D. in 1934.

BOHNING: When he went to Ohio State you were still at Manchester.

PLUNKETT: I was still at Manchester but I had two things happen to me. He got out a year early and I lost a year because of taking care of my family. So, it was two years later when I got through. I got out in 1932 and I went to Ohio State in the fall of 1932. Paul was pretty well established at that time in doing his doctorate research. I don't know what year it was but he was a Du Pont fellow at Ohio State.

BOHNING: Did you have any contact with him during that two year period?

PLUNKETT: Yes. We occasionally met at social affairs. He was a member of the Gamma Alpha society and so was I. I think we both lived at the house part of the time.

BOHNING: What about the time that he was at Ohio State and you were not? That two year period between 1930 and 1932.

PLUNKETT: I don't remember specifically about there being any contact because usually the summer periods evolved from the winter and whatever we did in the summer got planned in the winter. So, I don't recall much activity during that period of time. Anyway, I was pretty busy running my dad's farm for about fifteen months.

BOHNING: You said you lived in the Gamma Alpha house together.

PLUNKETT: Yes, we lived in the Gamma Alpha house and we sometimes went to parties that involved girls. I got married after he was gone. I believe that I lived in the Gamma Alpha house and he did too during the last year that we were both there.

BOHNING: Did you keep in contact with him over the years?

PLUNKETT: Yes. He came to Wilmington when he went to work with Du Pont in late 1934. I came to Wilmington to work at Du Pont in 1936. During the time that we were both there, we frequently met on social occasions. We didn't have any direct connection workwise. He was working with Carothers in the Experimental Station. I was working in Jackson Laboratory and the twain seldom met so that I didn't see him directly at work. But we did meet at parties. I forget just when he and Emily got married but

it was sometime during that period of time. We would have some visits house to house. He left Du Pont and went to Exxon first, I believe. Then he went to one of the rubber companies. He went to the University of Cincinnati to teach for awhile. He had another industrial connection and then he went to Cornell. He then went from Cornell to Mellon Institute and from there to Stanford. Those were essentially the movements that he made in his career.

I kept in touch every year all the time at Christmas time. During that whole period of time there was at least an exchange at Christmas time. I would quite frequently write him a note if I saw something that had been recognized of his and occasionally got one from him. Several years ago he gave the Franklin Medal talk at the Franklin Institute. When he came to give that talk I received an invitation at the suggestion of Dr. Flory.

BOHNING: I think you also said that you were at the Priestley House in 1974.

PLUNKETT: Yes, when he gave the Priestley Award lecture out at Priestley's home. Actually he gave the address at a place nearby, but received the award at the house. There were quite a few of us who drove up there that were former associates. Don Martin, who had been a student of Holl's and a teacher at Manchester, was at the time located in the Wilmington area and associated with Hercules. He and I and one or two others and I believe his son-in-law was with us. His daughter married a man who turned out to be a chemist and worked for Hercules. Right now I can't think of his name. But we went there and I think we spent the night. It was a late night program.

BOHNING: Yes. The dinner was late in the evening. Did you have a chance to talk to him then?

PLUNKETT: Yes, but not much. He was pretty busy. I had a chance to chat with him and congratulate him. I saw him most often for a period of years after he went to Stanford. I would frequently in my capacity as a research director at Jackson Laboratory have the opportunity to visit several of the California schools. Usually when I visited Stanford, I would have dinner with Paul and Emily and sometimes stay the night with them. So we would keep in contact that way.

In the fall of 1981, Washington College in Chestertown, Maryland had a two hundredth anniversary and they had a day called "Two Hundred Years of Chemistry." They invited a number of people there. Flory was invited and I was invited. Part of that came about because one of the prominent members of the Board of Governors of Washington College was a man by the name of Phillip Wingate who retired as a Du Pont vice president. He was

a graduate of Washington College and knew both Paul and me as associates because he started with Du Pont at about the same period of time that we did. He had earlier been instrumental in seeing that I got an honorary doctorate from Washington College. So he helped put together the invitation list for this two hundred year anniversary. We got to visit with Paul and Emily and had a special dinner at the house of the president of the college. That's the last time that Paul and Emily and Lois and I were together.

BOHNING: I think that information is very helpful in filling in some more of your association with Paul Flory. What I would like to do now is move to 1938 to the Teflon work and ask you a few additional questions. Let me ask you about Jack Rebok who was your lab assistant. How long had he been with the company?

PLUNKETT: All I can say, Jim, is several years. I don't know know that I ever knew exactly when he started work. He was a top notch technician when I got there in 1936.

BOHNING: What was his background?

PLUNKETT: He was a local born and raised from Paulsboro, New Jersey. He lived at Paulsboro when he retired and I think he still lives there. Paulsboro is just across the river down there. Whatever training he got was mostly by osmosis with people he worked with.

BOHNING: Was he assigned to you specifically?

PLUNKETT: At the time he was my helper on this project.

BOHNING: What kind of scientific reading were you doing at that time?

PLUNKETT: At that time I received the Journal of the American Chemical Society. I received Industrial Engineering Chemistry. I received Science and of course the ACS news magazine. I read those pretty regularly in those early days. I didn't read them as much in later years. I would read special things that came along but I don't think there's anything else that I can say that I read regularly. Well, for awhile I read the English magazine Nature that was similar to Science. I was reading that at the time. Other than that, my reading was what I was looking up in the library that was connected with my work.

BOHNING: Did you select the target molecule that you were trying to make as a possible refrigerant?

PLUNKETT: I don't know that I did it alone. I certainly participated in the decision because we wanted to get as close to the properties of the dichlorotetrafluoroethane as we could. The boiling points were not far apart. Therefore, the pressures and so forth would be about the same. We saw it was going to be easy to make.

BOHNING: The addition of HCl to TFE was already established in the literature.

PLUNKETT: I think the addition of HCl to TFE had already been done. There certainly had been things added across the TFE double bond. I don't remember any patent related to the other one. So it must have been done. The extraction of the chlorine had been demonstrated by Henne a year or two before (4), and it just seemed logical.

BOHNING: The route to TFE was also in the literature.

PLUNKETT: Yes.

BOHNING: But it was not done in the scale that you were working with.

PLUNKETT: I have made the statement a number of times, but I don't know that I could quantify it completely. I said until that time it was available in a few grams at a time. Sometimes less than that.

BOHNING: I'm still intrigued by the quantity that you made. How long did it take you to make the hundred pounds?

PLUNKETT: Well, Jim, I don't know now. I think I said someplace that I was assigned this problem in 1938. The Teflon discovery came on April sixth so it got done in that period of time. [laughter] What I had to decide was what kind of apparatus to use. I used a fairly large volume. I don't know whether I did it all at once or not but I ran the Freon 114 into the alcohol-zinc solution. I presume it had some temperature involved. The gas evolved and was collected in a cylinder. It was cooled sufficiently to get it to condense. Then I took that cylinder of gas and distilled it in the laboratory. That's how I got the material I stored in the little bottles.

BOHNING: How did you maintain this at a low temperature? You said you had to keep it in dry ice. That's a large quantity to keep at such a low temperature.

PLUNKETT: We did quite a bit of work with low temperature materials in the refrigeration business. We had a laboratory that had a good size storage box that was kept cool all the time with carbon dioxide. So I kept it in the storage locker.

BOHNING: We've already covered the events of April sixth pretty thoroughly. When did you decide to try and reproduce the polymerization? You started to look at properties pretty quickly after you found the powder. But there's one other thing I would like to ask you before that -- the decision to cut the tank open. I found that that's not on the transcript of our previous conversation. We only talked about taking the valve off.

PLUNKETT: For years, I've said that after we didn't get enough out by scraping and dumping it out, we decided to cut the tank open. I have been asked if it was that day and I've got to say I don't remember exactly but it was very close. We wanted to find out what happened to the weight. So I did cut it open but there was nothing mentioned of that on the sixth or the eighth. I haven't re-examined that. A notebook might reveal it but I don't know right now.

BOHNING: Did you ever go back and make the original potential refrigerant that you had set out to make?

PLUNKETT: I had made quite a bit of it. I don't remember now how much I made afterwards. I'm sure that I did make some more because at the time we were in the process of exposing guinea pigs to constant conditions of this new compound mixed with air. It was really to see what the effects would be. The guinea pigs were sacrificed after some periods of exposure and judged on whether there had been any detrimental effects.

[END OF TAPE, SIDE 1]

BOHNING: Did that compound ever become a real refrigerant?

PLUNKETT: Yes, it did. It never reached commercialization. The impetus, as you recall, is that the Frigidaire Corporation had proprietary rights to the other molecule. Such people as General Electric wanted to use it but Frigidaire wouldn't let them use it. So they came to us to get a substitute. Frigidaire was claiming some special properties and advantages for the 114. As

time went on, they apparently didn't maintain that advantage position and it got de-emphasized so that the others didn't have the desire to compete. Of course, Freon 12, or refrigerant 12 [CCl_2F_2], became the one of choice for the household refrigerator almost exclusively and it still is today.

BOHNING: Were your activities after April sixth divided? Were you looking at some of the properties of the polymer and also at the refrigerant?

PLUNKETT: I'm sure they were. I recall that it was within a few months that I got a completely new assignment. In that period of time, whatever I did was joint. I don't know exactly when I got this other assignment. I think it might have been as late as August. But in January of the next year I was transferred to the manufacturing organization in the tetraethyl lead business.

BOHNING: You did some work on trying to repeat the polymerization.

PLUNKETT: Yes. I ran a number of experiments in which I sealed TFE gas into pressure tubes and stored it under various conditions with or without a specific catalyst and with a specific catalyst in a solvent. I found that under those broad conditions, I could get polymerization. I never did have a chance to do enough of it to get any good at specifying the conditions. That was developed by the people at the Experimental Station.

BOHNING: Did you have any specific reaction when you were transferred out of that one position to the TEL plant?

PLUNKETT: No. My feeling has always been, Jim, that I was there and I accepted Du Pont as the place to work and if Du Pont thinks it's good for me, then it must be good for me and so I'll do it. I never seriously questioned not doing what they asked me to do.

BOHNING: At that point there was no realization of what this material really was in terms of its commercial potential.

PLUNKETT: Yes. There was no realization of what Teflon might amount to.

BOHNING: As the development of Teflon continued later on, were you contacted at all?

PLUNKETT: I had contacts not in the technical sense but in the legal sense because patents were being applied for and the original patent was issued in 1941. But then it was also patented in quite a few countries. Every time they put one in a different country I had to sign a piece of paper.

BOHNING: In terms of the patent process, once you made your observations on April sixth, this patent was applied on July first.

PLUNKETT: July 1, 1939.

BOHNING: So it was a year later. Would you as the person in the laboratory initiate the patent process?

PLUNKETT: Yes. Any researcher could propose a patent on anything that he had done. The researcher had to make the original search as to whether it was patentable or not. He had to make a scientific judgment as to whether it could be patented and make a proposal for it to be patented.

BOHNING: You did that then?

PLUNKETT: As far as I know.

BOHNING: Is that lag of about a year normal for patent applications to take place?

PLUNKETT: They can vary all over the map. It takes a long time to get the chemists and the lawyer to agree on what they're going to say. [laughter] It's just like all other legal aspects. You get something to the lawyer and it may be a month or two months later before something comes back. That's not unusual. I kept in touch in a way by knowing what was going on and knowing people who were doing the work. I was also aware, in a minor way, of the Manhattan District work that was going on at the site. Have you ever met Blaine McKusick?

BOHNING: No.

PLUNKETT: Blaine McKusick was a research director at the Du Pont Experimental Station when I was with Du Pont. He was the first one that made a contact with me last year to give the talk that I gave yesterday at the AAAS meeting. He's taken over leadership of the AAAS chemistry division from Rustum Roy, who I believe is

now the chairman. I saw him yesterday afternoon and he asked me, "Did the Manhattan people ever get you into the process?" I said, "If they did, I didn't know it."

In the meantime, I was pretty well ensconced in the manufacturing of tetraethyl lead and it was a pretty important field. When I expressed an interest at one point in time of possibly doing more by offering my services to the country, somebody said, "Well, if you do, you'll be right back here in a private's uniform." [laughter] So that discouraged me from doing that.

BOHNING: You did some of those early experiments to cause the polymerization. Do you know when the first large scale polymerization started to take place? Was that the work of Joyce and Hanford (9)?

PLUNKETT: It grew out of what Joyce and Hanford did but they were doing laboratory size things -- bench-scale stuff. The people at Jackson Laboratory, Benning and McHarness and the group that I had been associated with, they developed a larger scale process for making TFE and developed the pyrolysis of Freon 22 (8). Then the group at Arlington did the polymerization scale-up work. They were in the Plastics Department of Du Pont.

The decision was made that they were going to let the Plastics Department develop the material. This came in the early 1940s. The first large scale pilot plant didn't get built until after the war. There were pilot plants of different sizes. I don't know what to call large scale. But if you want to talk pound quantities at a time, a few pounds or something like that, I'm sure that took place in 1941, 1942 or somewhere in there.

BOHNING: What was the catalyst of choice in the polymerization eventually?

PLUNKETT: I believe that the first commercial catalyst was persulfate. Aqueous polymerization.

BOHNING: I think I've asked the questions that I needed to tie up the loose ends with. Is there anything else that you would like to add at this point?

PLUNKETT: I can't think of anything right now. I've been happy to participate with you in going over these things. You've tried to jar my memory and sometimes it won't jar very well.

BOHNING: I appreciate all of the time that you spent with us and

your willingness to take more time this morning. Thank you very much.

[END OF TAPE, SIDE 2]

NOTES

1. John Gorrie, "An Appliance for the Artificial Production of Ice in Tropical Climates," U.S. Patent 1851

2. F. Swarts, Bulletin de l'Académie Royale de Belgique, 24 (1892): 474-. idem., ibid., 29 (1895) 874-.

3. O. Ruff and O. Bretschneider, "Preparation of Hexafluoroethane and Tetrafluorethylene from Carbon Tetrafluoride," Zeitschrift für Anorganische und Allegemeine Chemie, 210 (1933): 173-183.

4. E. G. Locke, W. R. Brode and A. L. Henne, "Fluorochloro-ethanes and Fluorochloroethylenes," Journal of the American Chemical Society, 56 (1934): 1726-1728.

5. Roy J. Plunkett, "Tetrafluoroethylene Polymers," U.S. Patent 2,230,654, issued 4 February 1941 (application filed 1 July 1939).

6. Anthony F. Benning, Frederick B. Downing and Roy J. Plunkett, "Preparation of Terafluoroethylene," U. S. Patent 2,401, 897, issued 11 June 1946 (application filed 4 April 1940).

7. Framed and autographed reproduction of Plunkett's research notebook for April 6 and April 8, 1938 is in the Beckman Center Collection. [Courtesy of E. I. du Pont de Nemours & Company, Inc.]

8. Willis J. Clem and Roy J. Plunkett, "Manufacturing Tetraethyl Lead," U.S. Patent 2,464,398, issued 15 March 1949 (application filed 12 March 1946). Plunkett, "Manufacturing Tetraethyl Lead," U.S. Patent 2,477,465 issued 26 July 1949 (application filed 21 March 1946). Clem and Plunkett, "Manufacturing Tetraethyl Lead," U.S. Patent 2,515,821, issued 18 July 1950 (application dated 30 November 1946).

9. W. E. Hanford and R. M. Joyce, "Polytetrafluoroethylene," Journal of the American Chemical Society, 68 (1946): 2082-2085.

10. M. M. Renfrew and E. E. Lewis, "Polytetrafluoroethylene. A New Heat-Resistant, Chemically-Inert Plastic," Industrial and Engineering Chemistry, 38 (1946): 870-877.

11. J. D. Park, A. F. Benning, F. B. Downing, J. F. Laucius and R. C. McHarness, "Synthesis of Tetrafluoroethylene. Pyrolysis of Monochlorodifluoromethane," Industrial and Engineering Chemistry, 39 (1947): 354-358.

INDEX